On the Persistence of the China Shock*

David Autor† David Dorn‡ Gordon Hanson§

September 2020

Abstract

In this paper, we build on analysis in Autor et al. (2013) and Acemoglu et al. (2016) on how greater import competition from China has affected U.S. local labor markets. By extending the time period under study, expanding the set of outcomes considered, and incorporating insights from recent literature on shift-share research designs and the connection between reduced-form and general-equilibrium impacts, we evaluate the persistence of the China trade shock, the relative and absolute effects of greater trade exposure on local economic well-being, and the robustness of the results to alternative specifications, outcome measures, and sample periods. We first document that the China trade shock plateaued in 2010, which allows us to examine its effects for nearly a decade past its culmination. Adverse impacts of greater import competition on manufacturing employment, employment-population ratios, and income per capita in more trade-exposed U.S. commuting zones are still present out to 2016 to 2018. Reductions in population headcounts, which indicate net out-migration, register only for workers 25-39 years old and do not materialize until 10 years after shock initiation. Although more negatively affected regions see larger increases in the uptake of government transfers, these transfers replace only a small fraction of the loss in personal income and primarily take the form of increased Social Security and Medicare benefits, indicating that over the longer run workers adjust to negative local labor demand shocks in part through movements into retirement and onto disability insurance rolls. The results are consistent with the exit from work being a primary means of adjustment to trade-induced negative labor demand shocks and with prolonged adverse outcomes in the U.S. local labor markets most exposed to China's rise.

Keywords: import competition, local labor markets, manufacturing decline, job loss.
JEL Classifications: E24, F14, F16, J23, J31, L60, O47, R12, R23.

*We thank Peter Bruno, Rebecca Jackson, Sophia Mo, and Savannah Noray for excellent research assistance.
†MIT Department of Economics and NBER. E-mail: dautor@mit.edu
‡University of Zurich and CEPR. E-mail: david.dorn@econ.uzh.ch
§Harvard Kennedy School and NBER. E-mail: gordon_hanson@hks.harvard.edu

1 Introduction

Recent literature on how import competition affects local labor markets has transformed our understanding of economic adjustment to globalization. Since David Ricardo, economists have appreciated that freer trade creates both winners and losers.[1] Yet, until recently, our sense was that losses from trade, especially for workers, were likely to be diffuse. 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-skilled labor.[2] Even though furniture factories in Martinsville, Virginia, may shut their doors, the pain felt by former assembly-line workers in the region would be no different than the losses suffered by those in equivalently skilled service-sector jobs in Houston or Miami.[3] We have since learned that losses from trade are regionally concentrated and long lasting.[4]

Much of the research that elucidates how 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. Its rapid opening to trade and investment disrupted global markets. Economies producing the labor-intensive goods that China had begun exporting experienced a massive increase in global competitive supply, while those producing the raw materials and other inputs demanded by China saw a commensurately large increase in global demand. Recent work exploits the speed, scale, and concentrated impacts of China's reform-driven export growth to gain insight into the labor market consequences of globalization. Labor markets more exposed to import competition from China experienced more plant closures; larger declines in manufacturing employment, earnings for low-wage workers, employment-population ratios, housing prices, and tax revenues; and larger increases in the uptake of government transfers, the fractions of children living in single-headed

[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. See also Maneschi (2008).

[2] Labor mobility across industries and regions is core to the Heckscher-Ohlin model, which trade economists used to analyze the labor market impacts of trade shocks for much of the 20th century. See, e.g., Feenstra (2015).

[3] The manufacturing employment-to-population ratio in Martinsville fell by 29.0 percentage points between 1991 and 2015 (41.4% to 12.4%), the largest decline for any U.S. commuting zone. In 1990, its two largest industries were knit outwear (SIC 2253) and furniture and fixtures (SIC 2599) at 25.1% and 24.2% of manufacturing employment, respectively. Both industries saw very large increases in import penetration from China.

[4] The seminal work of Jacobson et al. (1993) found scarring effects from job loss due to mass layoffs in the form of lower long-run earnings. These impacts are more severe when the job loss occurs during a recession (Davis and von Wachter, 2011). Mass plant closures due to import competition are likely to trigger the equivalent of a local recession. Although earlier work found that greater 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.

households and in poverty, and mortality related to drug and alcohol abuse.[5]

In this paper, we re-examine the local labor market impacts of exposure to increased foreign competition in order to address questions related to which market adjustment mechanisms are operative, how long impacts persist, how reduced-form empirical findings connect to conclusions about welfare, and how to perform statistical inference on the impact of trade shocks when using regional economies as laboratories for analysis. These issues arise in a vibrant literature that interprets, extends, and refines the original China trade shock research.

Among the most striking findings in the literature is that regions suffering larger trade-induced reductions in manufacturing employment saw neither larger net reductions in labor supply—due, e.g., to out-migration—nor larger net absorption of workers by non-manufacturing sectors, thus accounting for the observed declines in employment-population ratios.[6] We explore the persistence of these and other impacts through 2018, using the now-standard concept of commuting zones for local labor markets (Tolbert and Sizer, 1996; Dorn, 2009). The China trade shock—defined as the country's period of reform-induced export growth—leveled off by 2010. By this point, China's phase of transitional growth had ended, and the government had begun to roll back some reforms (Brandt and Lim, 2019; Lardy, 2019). Although the China trade shock did not unwind, it did plateau. This plateauing lets us examine the duration of the shock for nearly a decade after its full expression.

We find that in more trade-exposed regions negative impacts on manufacturing employment continue to intensify well beyond the peak of the China trade shock. Negative impacts on the overall employment-population ratio hit their peaks approximately 10 years after the shock commences and exhibit little dampening 16 to 18 years after shock imitation. Negative impacts of trade shocks on local labor supply do materialize, but only for workers ages 25 to 39 and only 10 years after the shock began. These findings are robust to a wide range of alternative specifications. Trade shock

[5]Research on the China trade shock has addressed its impacts on regional and industry employment (Autor et al., 2013; Pierce and Schott, 2016); plant closure (Bernard et al., 2006; Acemoglu et al., 2016; Asquith et al., 2019); worker earnings (Autor et al., 2014; Chetverikov et al., 2016); government transfers (Autor et al., 2013; Autor et al., 2014); housing prices and tax revenues (Feler and Senses, 2017); marriage, fertility, and household poverty (Autor et al., 2019); population headcounts and migration (Greenland et al., 2019); and mortality (Autor et al., 2019; Pierce and Schott, 2020). Other work explores how the China trade shock affected consumer prices (Bai and Stumpner, 2019; Jaravel and Sager, 2019), U.S. exports (Feenstra et al., 2019), and high school completion rates (Greenland and Lopresti, 2016); and how the impacts of trade exposure are conditioned by the state of the local labor market (Austin et al., 2016), industry product cycles (Eriksson et al., 2019), and state-level labor regulations (Chan, 2019). See Autor et al. (2016) and Redding (2020) for discussions of the literature.

[6]The absence of a local labor supply response to the China trade shock is consistent with findings on the null impact of generic Bartik (1991) labor demand shocks on local population headcounts for less-skilled workers (e.g., Bound and Holzer, 2000; Diamond, 2016; Notowidigdo, 2020).

impacts thus appear to be long-lasting and to entail adjustments that suppress participation in work. Increased joblessness is evident in larger declines in personal income in more trade-impacted regions, where these income losses are minimally offset by the increased uptake of government transfers (primarily of Social Security and Medicare benefits).

The estimation approach taken in much of the China trade shock literature is differences-in-differences. Analogous to recent empirical literature in labor and macroeconomics, we estimate shock impacts by comparing changes in outcomes in more-trade-exposed commuting zones (CZs) to changes in outcomes in less-trade-exposed CZs.[7] Absent additional assumptions or model structure, this strategy identifies the *relative* impact of the trade shock—e.g., whether manufacturing employment fell by more in more-trade-exposed CZs—but not the *absolute* impact of the shock—e.g., whether the trade shock reduced total manufacturing employment locally or nationally (Heckman et al., 1998; Helpman, 2018). Although relative impacts are informative about the redistributional consequences of trade shocks, they may or may not be indicative of net changes in outcomes of interest.

What assumptions need to hold for relative impacts to be comparable to absolute impacts? Consider a hypothetical case in which each local labor market is small—in that changes in demand or supply in any one market have imperceptible effects on wages and prices in all other markets—and labor is geographically immobile. Here, the full impact of shocks is absorbed locally. In Martinsville, Virginia, for instance, an adverse labor demand shock to furniture manufacturing would release workers into the local economy, which would affect local wages, prices, and employment in other sectors, but have no discernible impact outside of the region. No leakage is of course an extreme assumption. Labor is at least partially mobile geographically and spillovers between regions do exist (Monte et al., 2018; Greenland et al., 2019). The relevant question for empirical research is how close a given national context is to the no-spillover benchmark. Abundant research in labor economics indicates that regional labor supply is largely unresponsive to adverse regional labor demand shocks, especially for low-skilled workers,[8] while recent work in quantitative spatial economics suggests that spillovers between regions due to trade shocks are small (Dix-Carneiro and Kovak, 2017; Adao et al., 2019a). Because the employment-population ratio is a barometer of local economic well-being (Amior and Manning, 2018), depressive effects of trade shocks on employment rates suggest that workers in

[7] Relatedly, recent work in macroeconomics addresses how one aggregates shock impacts estimated for regional economies to the national level (Chodorow-Reich, 2019, 2020; Guren et al., 2020).

[8] See discussion of the literature in Notowidigdo (2020).

impacted regions have suffered welfare losses and that these losses were persistent.

To estimate welfare impacts of the China trade shock structurally, recent literature use reduced-form empirical results to discipline the calibration of quantitative trade models (Galle et al., 2017; Caliendo et al., 2019; Rodríguez-Clare et al., 2019; Kim and Vogel, 2020), thus permitting counterfactual analysis. These exercises show that, whereas the aggregate welfare gains from China's market opening for the U.S. are positive, they are small, such that many *relatively* adversely effected regions suffered substantial *absolute* welfare declines. In Caliendo et al. (2019), the aggregate impact of trade shocks on manufacturing employment is moderately smaller than that implied by Autor et al. (2013); Acemoglu et al. (2016), indicating that the employment impacts identified by differences-in-differences roughly approximate absolute impacts in quantitative models. Several recent papers derive sufficient statistics for changes in local welfare. We compare these results to those obtained using a simpler, but more-restrictive, approach using employment-population ratios.

Most empirical analysis of the China trade shock uses a Bartik (1991) approach to estimate impacts of greater trade exposure. Changes in regional outcomes are regressed on a shift-share variable, in which the shifts are national changes in industry import competition and the shares capture the importance of an industry in regional employment (where the shift-share variable itself is instrumented by another shift-share variable, based on foreign industry trade shocks). Recent literature formalizes the basis for identification and inference in a Bartik setting. Borusyak et al. (2018) treat identification as based on exogeneity of the shifts.[9] The impact coefficient identified in a Bartik regression is equivalent to that obtained by regressing the industry level outcome on the industry level shift (weighting each industry by its share in national employment), which dictates how to adjust standard errors in the regional shift-share regression. Adao et al. (2019b) show further that the resulting confidence intervals for the impact coefficient may be asymmetric. 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 clarify the exclusion restrictions, how one evaluates the validity of these restrictions, and how to cluster standard errors.

A final consideration regards the normative implications of adverse impacts of the China trade shock on labor markets in the U.S. and other countries. Few economists interpret our empirical results as justifying greater trade protection. As expected, quantitative models indicate that U.S.

[9] Goldsmith-Pinkham et al. (2020) propose an alternative approach based on exogeneity of the shares.

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. This failure is hardly a surprise. The U.S. does little to protect workers from mass-layoff events such as the China trade shock. Although Trade Adjustment Assistance delivers benefits to individual workers (Hyman, 2018), the program was too small to have mattered in the aggregate for workers who lost their jobs because of import competition in the 1990s and early 2000s (Baicker and Rehavi, 2004). Recent literature suggests that fostering employment growth in regions with low employment-population ratios, such as through place-based policies, may help workers hurt by persistent negative local labor demand shocks (Austin et al., 2016; Bartik, 2020). There is a glaring hole in the literature regarding the use of placed-based policies, or other interventions, to help those exposed to the adverse impacts of globalization. Policy failures in this domain are far from innocuous.[10] The success of populist politicians in the U.S. and Europe has been stronger in regions in which trade-induced job loss has been greater (Colantone and Stanig, 2018a,b; Autor et al., 2020; Rodrik, 2020). Absent meaningful efforts to redress losses from globalization, there may be little reason to expect renewed political support for freer trade.

In Section 2, we present evidence on the timing of the China trade shock. In section 3, we examine the persistence and robustness of trade shock impacts in U.S. commuting zones for a wide range of labor-market outcomes. In section 4, we discuss the connection between reduced-form empirical results and changes in welfare. By way of conclusion, in Section 5, we discuss what empirical analysis of the China trade shock does and does not imply about the gains from trade and trade policy.

2 The Timing of the China Shock

In this section, we review the origins and timing of China's dramatic export boom, and the variation in this growth across manufacturing subsectors. The China trade shock has progressed through well-defined periods of initiation, acceleration, and stabilization.

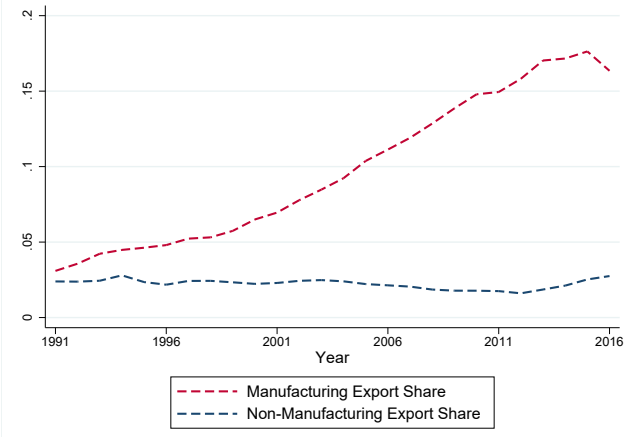
[10] On the general question of optimal spatial policies in spatial economic models with spillovers, see Fajgelbaum and Gaubert (2020) and Gaubert et al. (2020).

2.1 China's Rise as a Manufacturing Export Powerhouse

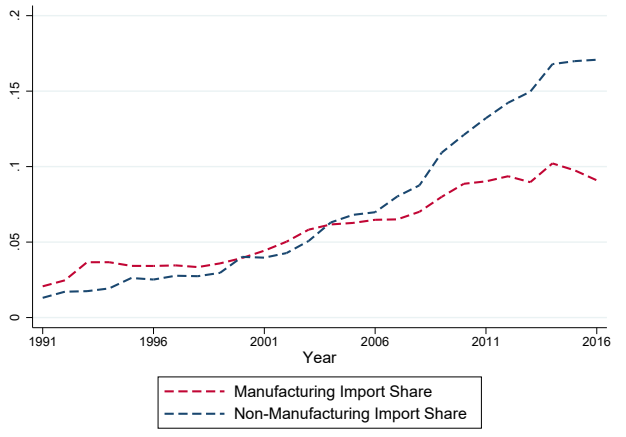
As seen in Figure 1a, China's share of world manufacturing exports rose from 3.1% in 1991 to 17.7% in 2015, before dipping to 16.5% in 2016.[11] The export boom—which has been celebrated as turning China into the world's factory[12]—has been very much a manufacturing phenomenon. China's share of world exports of non-manufactured goods, also seen in Figure 1a, has changed little over time, averaging just 2.2% between 1991 and 2016 and showing no discernible trend. This divergence in export growth between sectors reflects China's powerful comparative advantage in manufacturing—and labor-intensive manufacturing in particular—which derives from the country's initial abundant supply of labor relative to its supplies of agricultural land, natural resources, and physical capital (Wood, 1995, 2018). Once China began to relax restrictions on foreign trade and investment, first through the expansion of special enterprise zones initiated by Deng Xiaoping (Naughton, 2007) and later through an economy-wide liberalization associated with the country's accession to the World Trade Organization (WTO) in 2001 (Lardy, 2019), it became freer to exercise its latent prowess. The surge in manufacturing exports generated a corresponding increase in the import of raw materials used in industrial production, which pushed China's share of world non-manufacturing imports from 1.2% in 1991 to 15.6% in 2016 (Figure 1b).[13]

Figure 1: China Share of World Trade by Major Sector

(a) China Share of World Exports



(b) China Share of World Imports



Note: Data are from UN Comtrade, SITC Revision 2.

[11] See Appendix A for details on the data we use in the analysis in this section.

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

[13] China's share of world manufacturing imports also rose over time, from 2.1% in 1991 to 9.3% in 2016, driven in part by growing demand for inputs it was unable to produce for itself and in part by the expansion of global production chains with assembly operations in the country (Koopman et al., 2012; Kee and Tang, 2016).

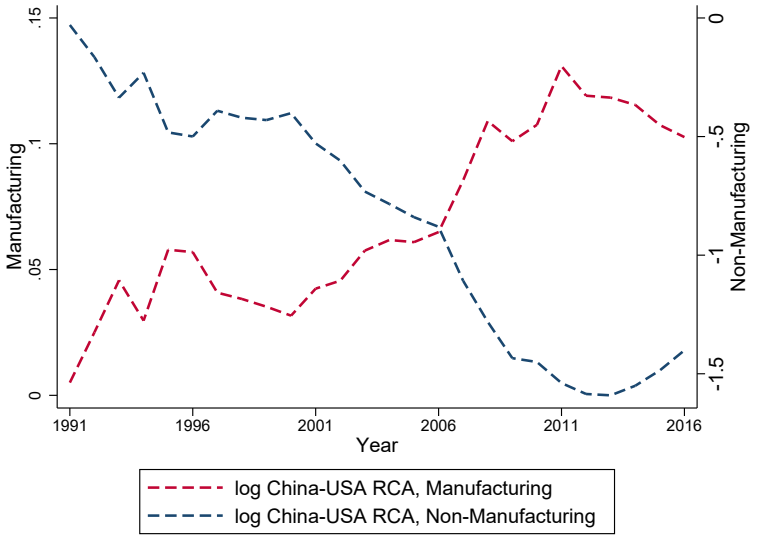
Our concept of the China shock period is that spanning the country's reform-driven transition from an inward-oriented and heavily centrally planned economy to a comparatively open and market-oriented one in which the majority of production occurred in privately owned firms. We conventionally date the China trade shock as commencing in 1992, when Deng expanded his "reform and opening" agenda to include foreign trade and investment (Naughton, 2007; Vogel, 2011). China's market transition unleashed dramatic improvements in productivity resulting from a spatial and sectoral reallocation of resources (Hsieh and Klenow, 2009; Brandt et al., 2013), the expansion of the private sector at the expense of inefficient state-owned enterprises (Song et al., 2011; Khandelwal et al., 2013; Hsieh and Song, 2015; Bai et al., 2017), new inflows of foreign capital and imported intermediate inputs (Yu, 2010; Brandt and Morrow, 2017), a reorientation of manufacturing to export assembly plants producing at the behest of multinational firms (Feenstra and Hanson, 2005; Brandt et al., 2017a), and the rapid adoption of previously banned foreign technologies (Hu and Jefferson, 2009; Wei et al., 2017).[14] Over 1998 to 2007, TFP growth in Chinese manufacturing progressed at the stunning annual average rate of 8.0% (Brandt et al., 2012). The 1992-2007 export surge thus appears to be an episode in which China's trade growth was driven more by productivity enhancements resulting from its marketing opening than by events in the rest of the world.

Because much of China's post-Deng manufacturing growth was transitional, it was destined to be finite-lived (Song et al., 2011). Once reforms had been enacted and a substantial share of the labor force had moved from the countryside to cities and from state-owned to private firms, easy growth was no longer attainable (Brandt and Lim, 2019). Between 2010 and 2013, Brandt et al. (2017b) estimate that TFP growth in Chinese manufacturing was negative, in stark contrast to earlier years. Changes in the political economy of reform in China may have further hampered the manufacturing sector. In 2008, President Hu Jintao gave (low productivity) state-owned enterprises renewed prominence in industrial planning (Naughton, 2016), a shift reinforced by President Xi Jinping in 2012 (Lardy, 2019). Although the share of state-owned enterprises in industrial employment declined from 62.0% in 1992 to 13.5% in 2013 (Naughton, 2017), these enterprises continued to absorb 38.0% of bank lending in the latter year. With Xi's re-prioritization of the state sector in national development, its

[14]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 contributing to an obvious sector bias in this growth. Any undervaluation would have worked against the observed increase in China's share of world imports. See Cheung et al. (2007) for a skeptical view of the undervaluation of the yuan.

share of bank lending rose to 51.3% by 2016.[15] The expiration of the productivity-growth miracle and the rollback of pro-market reforms appear to have slowed manufacturing growth after 2010. Whereas the differential in annual manufacturing export growth between China and the rest of the world was 8.2 percentage points over 1991 to 2001 and 8.4 percentage points over 2001 to 2010, it was just 0.7 percentage points over the 2010 to 2016 period.[16] Although after 2010 China did not lose ground in global markets, it was no longer gaining much ground, either.

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



Note: Data are from UN Comtrade, SITC Revision 2.

China's post-2010 slowdown is also apparent when we compare its trade performance to the U.S., which is of particular relevance for defining the trade shock facing U.S. manufacturing. 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. In the Eaton and Kortum (2002) model of Ricardian comparative advantage, RCA values summarize differences in fundamental productivity between a country and the rest of the world when comparing one sector to the average of all other sectors (Hanson et al., 2015).[17] In Figure 2, we show log China RCA minus log U.S. RCA for

[15] See Nicholas Lardy, "China's Private Firms Continue to Struggle," *China Economic Watch*, June 4, 2019 (https://www.piie.com/blogs/china-economic-watch/chinas-private-firms-continue-struggle).

[16] We calculate these differential growth rates using the annualized log changes in China's share of world manufacturing exports over 1991 to 2001 ($0.082 = \ln(0.070/0.031)/10$), 2001 to 2010 ($0.084 = \ln(0.149/0.070)/9$), and 2010 to 2016 ($0.017 = \ln(0.165/0.149)/6$).

[17] RCA values also account for the relative geographic advantage of a country in a sector due to its gravity-based

both manufacturing and non-manufacturing exports. China's manufacturing RCA vis-á-vis the U.S. rises in the early 1990s and then strongly after its WTO accession in 2001, reflecting the dramatic expansion in China's manufacturing capacity and its exceptional productivity growth in the sector during the period. After 2010, however, the China-U.S. relative RCA exhibits no trend.

By expressing China's comparative advantage in manufacturing with respect to the U.S. in terms of log RCA values, we evaluate the log difference in the two countries' exports in the sector relative to the log difference in the two countries' exports of all goods. China began with a modest comparative advantage in manufacturing over the U.S. in 1991—as indicated by a China-U.S. manufacturing versus all goods export differential of just 2.0 percentage points in that year. It reached its peak advantage in 2011—when its manufacturing-versus-all-goods export differential over the U.S. registered 18.1 percentage points—before diminishing modestly in the ensuing five years. China's RCA in non-manufacturing compared to the U.S. exhibits roughly the inverse pattern, dropping steadily through the 1990s and 2000s and then stabilizing at a strongly negative level after 2011. China's comparative disadvantage in non-manufacturing relative to the U.S. reflects the success of U.S. firms in exporting agricultural products, certain mineral products, and (more recently) oil and gas to the rest of the world, versus market forces in China having worked to concentrate factors of production in manufacturing, at the expense of other sectors.

2.2 Cross-Sector Variation in China's Export Surge

Movements in China's RCA for overall manufacturing mask variation in export performance across subsectors, which we explore in Appendix Figures A1-A4. These figures show China-U.S. relative log RCA values in the left column and China's share of world exports in the right column. They cover the 54 three-digit SITC Rev. 2 products for which during the 1991 to 2016 period, China's log RCA achieves a maximum value of at least 0.5, which indicates a ratio of China's share of world exports of a product to China's share of world exports of all goods of 1.65.[18] These strong comparative advantage products for China accounted for an average of 82.7% of the country's manufacturing

access to global markets. In practice, the correlation between RCA values and estimates of Eaton-Kortum fundamental productivity values from the gravity model (where these estimates are the exporter-sector-year fixed effects in the regression) are in the range of 0.70 to 0.80, depending on the years and sectors considered (Hanson et al., 2015).

[18]There are 237 three-digit SITC Rev. 2 products in total and 160 products that are manufactures. We exclude a handful of products in which China briefly had a high RCA and then moved into comparative disadvantage territory: SITC Rev. 2 products 014 (meat products), 111 (non-alcoholic beverages), 585 (resins and other plastic materials), 786 (trailers and other non-motorized vehicles), and 897 (jewelry made from precious materials).

exports over the period, while accounting for 41.4% and 29.5% of world and U.S manufacturing exports, respectively.[19] By choosing sectors based on China's RCA *relative to the world as a whole* and then presenting China's RCA *relative to the U.S.*, we display the relative productivity advantage of China over the U.S. in the goods for which China's comparative advantage is universal, rather than U.S. specific.[20] These are precisely the sectors that the Autor et al. (2013) identification strategy uses to isolate exogenous variation in the China trade shock.

In a first group of products, shown in Figure A1, China achieves its maximum RCA over the U.S. before 1997. These products, which include labor-intensive apparel and textiles (e.g., women's outerwear, underwear, handbags, leather goods, carpets) in Figures A1a and A1b, and simple manufactures (e.g., dried foods, explosives and fireworks, cement, radios, assorted plastic goods) in Figures A1c and A1d, are ones in which China already had a strong comparative in 1992.[21] In these goods, the China-U.S. relative RCA is either stable or declines modestly over time. Yet, because China's overall export capacity was rapidly expanding, its share of world exports in these products continued to grow, even though its RCAs were not. For each of the six just-mentioned apparel and textile products, China's world export share exceeded 35% by 2006, before stabilizing over the following decade. For simple manufactures, shown in Figures A1c and A1d, the China-U.S. relative RCA declines from high initial values over time, while its shares of world exports in these goods are roughly stable (except for pig iron). As China diversified its exports away from the simple products shown in Figure A1, its revealed comparative advantage in these products diminished, even while its share of world exports for these goods remained stable.

A second group of products includes those in which China reached its peak RCA between 1997 and 2002, shown in Figure A2. This group is comprised of moderately more sophisticated goods than those shown in Figure A1, including electrical power machinery; heating, lighting, and plumbing fixtures; bicycles, motorcycles, and scooters; and cutlery. These products also register high initial RCAs relative to the U.S. in the early 1990s, which rise into the late 1990s before plateauing and then declining modestly in ensuing years. Again, even as the RCAs for these products stabilized or

[19] The other manufacturing products are ones for which log China's RCA over 1991 to 2016 is persistently negative, indicating comparative disadvantage, or approximately zero, indicating neither advantage or disadvantage.

[20] Although we select products based on China's RCA relative to the entire world, we organize the products into four groups by the timing of when they reach their maximum RCA relative to the U.S.

[21] In handbags, underwear, women's outwear, woven fabric, and yarn, the China-U.S. log relative RCA exceeded a value of 3 in the early 1990s, indicating an initial differential of China-U.S. exports in the product relative to China-U.S. exports of all manufactures of over 300 log points.

diminished, China's expanding overall export capacity caused its shares of their world exports to continue to grow. In footwear, household equipment, and lighting and plumbing fixtures, China's share of world exports reached or exceeded 40%, in toys and games it exceeded 50%, and in pottery it exceeded 60%. These exports shares flatten by the end of the time period.

A third group of products, shown in Figure A3, includes more sophisticated exports, such as computers, computer equipment, recording equipment, and TVs. China's RCA in these goods peaked between 2002 and 2007. For computers and computer equipment, China began with a strong comparative disadvantage, as indicated by large negative relative RCAs in the early 1990s. Its RCA for these goods rose sharply over time, and its share of world exports ultimately exceeded 50% for computers and 30% for computer equipment. Not all goods with late peaking RCAs are advanced manufactures. China did not achieve its peak RCA until after 2002 in men's outwear, knitted underwear, and embroidered items, products which are labor-intensive. This pattern highlights the uneven evolution of China's comparative advantage. Although the general pattern is for China to achieve its peak RCA early for less-sophisticated products (e.g., women's outerwear) and late for more-sophisticated products (e.g., computers), there are exceptions to this rule. Once China achieved comparative advantage in a sector, its rapid overall economic growth ensured that it maintained a dominant position in world exports for the good, even if its revealed comparative advantage diminished. In just two cases in Figures A1-A3, pig iron and fur skins, do we see goods in which China lost a substantial share of world exports, among those in which it registered a strong RCA sometime after 1991. For the most products, once the China shock hit it did not subside.

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

Summary. Our examination of China's export growth yields three conclusions. First, the China trade shock, which was initiated by the country's internal economic reforms, appears to have stopped intensifying after 2010. It was likely brought to an end by the combined effects of China having completed its transitional period of post-reform growth and the partial reversal of reforms by Presidents

Hu and Xi. Second, most of the goods in which China had an initially or eventually strong revealed comparative advantage relative to the U.S. were ones in which it captured a large share of world exports. Impacted U.S. sectors were thus highly exposed to the China trade shock, as were other countries specialized in these industries. Third, there is substantial product-level variation in the timing and intensity of the China trade shock. In some goods, most of which appear to be relatively less sophisticated, China's trade advantage manifested itself early. In other products, China's export strength came late, where this group includes sophisticated products, such as computers and cellphones, and some less sophisticated ones, such as certain textile fabrics and non-metallic minerals. In the large majority of products, the trade shock appears to be strongly persistent. Once China acquired a large share of world exports, it has tended to hold onto it.

3 Empirical Results

In this section, we revisit regressions akin to those estimated in Autor et al. (2013) and Acemoglu et al. (2016), which identify the effect of greater import competition from China on changes in labor market outcomes for U.S. commuting zones. With China's export surge having peaked in 2010 and stabilized thereafter, we are in position to evaluate the persistence of its impacts for a time period that extends nearly a decade beyond its culmination. We also discuss results from a vibrant literature on identification and inference when using Bartik (1991) estimators, which has been inspired in part by research on the China trade shock.

We focus on three sets of outcomes. The first are ratios of manufacturing employment, non-manufacturing employment, total employment, and unemployment to the working-age population (i.e., ages 18 to 64). We measure employment using County Business Patterns (CBP) and the BEA Regional Economic Information System (REIS), unemployment using Local Area Unemployment Statistics (LAUS), and population using the National Vital Statistics System, where we aggregate data from the county to the CZ level. Trade shock impacts on these outcomes reveal the direct consequences on manufacturing employment, and the magnitude of labor reallocations to unemployment, non-employment, and other sectors. A second set of outcomes are changes in log population headcounts, overall and by age and gender subgroups, which reveal whether greater exposure to import competition led to larger reductions in labor supply (e.g., due to out-migration). A final

set of outcomes are per capita changes in personal income, labor compensation, and government transfers (overall and by program type), which reveal the impact of trade shocks on average incomes and whether the uptake of government transfers offset income losses due to greater trade exposure. We measure personal income, wage and salary compensation, and government transfers using county-level data from the REIS and the Social Security Administration, aggregated up to CZs.

3.1 Empirical Specification

3.1.1 Baseline Specification

We examine changes in exposure to import competition from China for the 722 CZs on the U.S. mainland.[22] As in Acemoglu et al. (2016), our measure of trade exposure is the average change in Chinese import penetration across industries, weighted by industry shares in initial CZ employment:

$$\Delta IP_{i\tau}^{cu} = \sum_{j} s_{ij90} \frac{L_{ij90}}{L_{i90}} \Delta IP_{j\tau}^{cu}, \tag{1}$$

where $\Delta IP_{j\tau}^{cu} = \Delta M_{j\tau}^{cu}/(Y_{j91} + M_{j91} - X_{j91})$ is the growth of Chinese import penetration in the U.S. for industry j for time interval τ and $s_{ij90} \equiv L_{ij90}/L_{i90}$ is the share of industry j in CZ i's total employment in 1990. We compute import penetration as the growth in U.S. imports from China, $\Delta M_{j\tau}^{cu}$, divided by initial absorption (U.S. industry shipments plus net imports, $Y_{j91} + M_{j91} - X_{j91}$) in the base year 1991.[23] Differences in $\Delta IP_{i\tau}^{cu}$ across CZs stem from variation in local industry employment in 1990, which arises from differential specialization in manufacturing versus non-manufacturing and in import-intensive industries within manufacturing. As shown in Appendix Table XXX, the decadalized population-weighted-average change in import penetration was 0.77 percentage points (σ=0.49) over 1991 to 2000 and 1.28 percentage points (σ=0.83) over 2000 to 2012, for an average decadalized change of 1.03 percentage points (σ=0.59) over 1991 to 2012.

Motivated by the local projections approach of Jordà (2005), we assess the evolution of trade-shock impacts by estimating period-by-period models of stacked time differences of the form,

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

[22] Rising trade with China is responsible for nearly all of the expansion in U.S. imports from low-income countries that occurred in the 1990s and 2000s (Pierce and Schott, 2016).

[23] We measure imports using HS trade data from UN Comtrade, harmonized to 4-digit SIC industries, and industry shipments using the NBER manufacturing productivity database (Autor et al., 2013; Autor et al., 2016).

where ΔY_{it+h} is the change in an outcome for CZ i between the initial period t (1991 or 2000) and later year $t+h$. For $h=1$, we have stacked time differences for 1991-1992 and 2000-2001, for $h=2$ we have stacked differences for 1991-1993 and 2000-2002, and so forth. To avoid stacking time differences in which the first period overlaps with the second, for $h=10,\ldots,18$, we truncate the end of the first time period in 2000 (e.g., for $h=10$, we stack time differences for 1991-2000 and 2000-2010, and for $h=11$, we stack differences for 1991-2000 and 2000-2011, and so on).[24] For $h<10$, we estimate the trade shock impact on outcome periods that are less than complete, which reveals how long it takes for the full impact of the shock to become manifest. For $h>10$, we estimate the shock impact on outcome periods that extend beyond the culmination of the shock, which reveals whether impacts attenuate as the time period lengthens.[25]

The control vector $\mathbf{X}'_{it}$ contains time trends for U.S. Census Divisions, a dummy for the second time period, and start-of-period CZ-level covariates. The covariates include the initial share of CZ employment in manufacturing, which allows us to focus on variation in trade exposure arising from within-manufacturing differences in industry mix; initial CZ specialization in occupations according to their routine-task intensity and offshorability (Autor and Dorn, 2013; Goos et al., 2014), which absorbs variation in outcomes associated with exposure to automation and non-China-specific globalization; and the CZ fractions of working-age women who are employed, the foreign born in the total population, and those with a college degree in the total population, which absorb variation in outcomes related to demographic factors. Including the initial manufacturing employment share as a control is equivalent to including the sum of initial CZ industry employment shares in (1). To allow for the possibility that unobserved industry shocks affect CZ labor-market outcomes via these employment shares (Adao et al., 2019b), we interact the initial manufacturing share with a period dummy (Borusyak et al., 2018). All regressions apply weights equal to the CZ share of the national population in the initial year and cluster standard errors at the state level.

We measure trade shocks for the sequential periods 1991 to 2000 and 2000 to 2012. We choose 1991 as the base period because China's shift toward export-led development begins in 1992, 2000 as the mid point because China's accession to the WTO in 2001 intensifies and redirects its export

[24] To make outcome variables comparable over the two time periods, we scale the value in first period by $h/9$.

[25] By excluding lagged values of the dependent variable in (2), we do not allow for dynamics in adjustment. We estimate the long-run impact of trade exposure on outcomes of interest, where we vary the definition of the long run by varying h. When examining changes in log population headcounts, we also report results in which we control for long lags on population growth (Greenland et al., 2019).

growth, and 2012 as the end period because it post-dates both the plateauing of the China trade shock in 2010 and the substantial volatility in global trade during 2009 and 2010 associated with the Global Financial Crisis. We explore specifications using alternative start points, mid points, and end points for the trade shock. We focus on U.S. imports from China and not U.S exports to China because the former dwarf the latter and because our instrumentation strategy is less well-suited to isolating exogenous variation in the growth of U.S. exports to China.[26] Autor et al. (2013) show that results for relevant outcomes are little changed when replacing growth in U.S. imports with growth in U.S. net imports. In related work, Feenstra et al. (2019) estimate zero impact of growth in U.S. exports to China on employment in U.S. manufacturing industries.

3.1.2 Causal Identification

A concern for identifying the causal impact of import exposure on labor-market outcomes in (2) is that U.S. imports may change both because of shocks to U.S. product demand and shocks to foreign product supply, where the former may be correlated with the residual, e_{it+h}. To identify the supply-driven component of U.S. imports from China, we follow Autor et al. (2013) and Acemoglu et al. (2016) and instrument U.S. import-exposure, $\Delta IP^{cu}_{i\tau}$, using non-U.S. exposure, $\Delta IP^{co}_{i\tau}$, which we construct using data on industry-level growth of Chinese exports to eight other developed countries:[27]

$$\Delta IP^{co}_{i\tau} = \sum_j s_{ij80} \Delta IP^{co}_{j\tau}. \tag{3}$$

This expression differs from (1) by using realized imports from China in other high-income markets ($\Delta M^{co}_{j\tau}$) in place of U.S. imports from China ($\Delta M^{cu}_{j\tau}$), the 3-year lag on industry absorption ($Y_{j88} + M_{j88} - X_{j88}$) in place of its value in 1991, and the 10-year lag on CZ industry employment shares, $s_{ij80} \equiv L_{ij80}/L_{i80}$, in place of their values for 1990.[28]

In their evaluation of Bartik (1991) estimators, Borusyak et al. (2018) derive sufficient conditions for causal identification in Autor et al. (2013), Acemoglu et al. (2016), and related contexts. Applying their framework, for the instrument, $\Delta IP^{co}_{i\tau}$, to be orthogonal to the residual, e_{it+h}, in (3), the following condition must hold: $\sum_j s_j \Delta IP^{co}_{j\tau} \phi_j \to 0$, where $s_j \equiv \mathbb{E}[s_{ij80}]$ is the expected employment share

[26] The 1991-2000 increase in U.S. manufacturing imports from China ($107bn) is 6.4 times the increase in U.S. manufacturing exports to China ($17bn); the 2000-2012 increase in U.S. manufacturing imports from China ($292bn) is 4.1 times the increase in U.S. manufacturing exports to China ($71bn), for values in 2015 USD.

[27] 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.

[28] The use of lagged values helps reduce bias associated with simultaneity and measurement error.

of industry j and $\phi_j \equiv \mathbb{E}\left[s_{ij80}e_{it+h}\right]/\mathbb{E}\left[s_{ij80}\right]$ is the exposure-weighted average of unobserved shocks for industry j.[29] This condition says that the large-sample covariance between industry-level shocks and unobservables must be zero. Because of the Bartik 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. Note that the orthogonality condition is satisfied under exogeneity of the employment shares, $\mathbb{E}\left[s_{ij80}e_{it+h}\right] = 0$, which is the basis for identification in Bartik applications derived by Goldsmith-Pinkham et al. (2020). However, share exogeneity is not necessary for identification. Without it, identification is still achieved with industry-level orthogonality. Because in our context the shocks are reforms in China that unleashed industry-level export growth, it seems more natural to base identification on the orthogonality of industry-level shocks than on the exogeneity of CZ employment shares. To check for industry-level orthogonality, Borusyak et al. (2018) recommend regressing current shocks on past outcomes, which are likely correlated with current residuals. Autor et al. (2013) and Acemoglu et al. (2016) perform such validation exercises for CZs and industries and fail to reject industry-level orthogonality. Using seven validation tests, Borusyak et al. (2018) fail to reject the joint-null of industry-level orthogonality in the Autor et al. (2013) analysis at conventional significance levels.

Borusyak et al. (2018) show further that by applying the appropriate weights to a specification as in (2), one can recover the estimated impacts of the trade shock on national industry employment. Alternatively, one can simply run industry-level regressions directly, as in Acemoglu et al. (2016) and Pierce and Schott (2016). Our interest, however, is not simply in the narrow impact of trade exposure on industry-level employment but rather in the full consequences of trade shocks for local labor markets, which reveal the margins along which adjustment occurs. We thus maintain CZs as the unit of analysis. Adao et al. (2019b) evaluate confidence intervals for shift-share estimators, when the residual itself has a shift-share structure (e.g., where unobserved industry shocks are transmitted to CZs via industry employment weights). The corrected shift-share IV standard errors that they derive, when applied to the Autor et al. (2013) analysis, widen confidence intervals asymmetrically to include substantially more-negative impacts of trade shocks on manufacturing employment (with no

[29] Borusyak et al. (2018) show that the orthogonality is satisfied under the assumptions that industry shocks are mean independent of unobserved shocks ($\mathbb{E}\left[\Delta IP^{co}_{i\tau}|\phi_j\right] = \mu$ for all j) and the industry shocks themselves are mean independent ($\mathbb{E}\left[(\Delta IP^{co}_{j\tau} - \mu)(\Delta IP^{co}_{k\tau} - \mu)|\phi_j, \phi_k\right] = 0$ for all j and $k \neq j$ and $\sum_j s_j^2 \to 0$). When using panel data and including time dummies in the regression, mean independence of shocks is defined in terms of deviations from period-specific means rather than from time-invariant means.

change in statistical significance). By clustering standard errors at the state level, we are consistent with Adao et al. (2019b), as long common specialization patterns across CZs within states are the source of correlated errors. This assumption is more restrictive than that underlying the general standard-error correction formula of Adao et al. (2019b). 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 than theirs.

3.2 Persistence of the China Trade Shock

3.2.1 Employment Outcomes

We begin the analysis with the impact of the China trade shock on CZ manufacturing employment, measured by its share of the CZ working-age population. Figure 3a displays 2SLS estimates of impact coefficients for trade exposure from the stacked-time-difference specification in (2), when using (3) to instrument for (1). The graph shows results for 16 separate regressions, where the horizontal axis organizes specifications according to the time difference being considered, beginning with stacked differences for 1991-1992 and 2000-2001 ($h = 1$) and extending out to time differences for 1991-2000 and 2000-2016 ($h = 16$).[30] Vertical bars show 95% confidence intervals for the coefficient estimates.[31] Structured in this manner, Figure 3 allows us to assess the cumulative impact of greater trade exposure on outcomes at progressively longer time horizons.

The negative effect of greater trade exposure on CZ manufacturing employment builds quickly. The impact coefficient reaches -1.38 (t-value$= -5.06$) eight years after shock initiation and remains at -1.33 (t-value$= -3.35$) 10 years after shock initiation, by which point in the second time period the China trade shock has approximately reached is full expression. Rather than diminishing over time, the adverse impact of greater trade exposure increases in intensity, reaching -1.97 (t-value$=$ -5.23) 16 years after shock initiation. For $h \geq 10$, any change in the coefficient estimate as h increases comes from changes in outcomes in the second time period. It is thus clear that as one moves

[30] For time differences of $h < 10$, the first-period and second-period time differences are the same length (e.g., 1991-1992 and 2000-2001, 1991-1993 and 2000-2002, and so forth). For time differences of $h \geq 10$, the first-period time difference is held constant at 1991-2000 (to avoid extending into the second period), whereas the second-period time difference varies from 2000-2010 ($h = 10$) to 2000-2016 ($h = 16$). To keep the dependent variable in the first and second time periods in the same scale, we multiply that for the first time period by $h/9$ for all $h \geq 10$.

[31] Because of concerns over endogeneity in initial CZ industry employment shares, Borusyak et al. (2018) recommend replacing the initial-period CZ share of employment in manufacturing in (2), with its 10-year lag (and interacting this variable with a period dummy). We implement this specification in Appendix Figure A5. For all outcome variables, coefficient estimates for this specification are very similar to those in Figure 3.

later into the 2000s the depressive effect of greater import competition on manufacturing employment becomes stronger, even though the trade shock itself is no longer intensifying. Given the population-weighted average increase in import penetration over 2000-2012 of 1.28 percentage points, the implied cumulative reduction in the manufacturing employment share over 2000-2016 is -2.52 ($= -1.97 \times 1.28$) percentage points, relative to the aggregate 2000-2016 change in the manufacturing employment share of 3.86 percentage points. Alternatively, when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the latter would have a reduction in its manufacturing employment share that is 1.89 ($= -1.97 \times [1.69 - .73]$) percentage points larger than in the former over 2000 to 2016. This compares to the 25^{th}–75^{th} percentile differential change in the manufacturing-employment-share of -2.33 ($= -4.82 + 2.49$) percentage points for the same period. These dynamically accumulating negative impacts of trade exposure on local manufacturing employment for the U.S. are consistent with what Dix-Carneiro and Kovak (2017) find for Brazil.

Workers losing their jobs in manufacturing may move into other sectors, onto unemployment roles, out of work entirely, or to another commuting zone.[32] Figures 3b-d examine which of these adjustment mechanisms are operative. In Figure 3b, we consider the impact of greater exposure to import competition on the CZ share of the working-age population employed in non-manufacturing industries. For all time differences, impact coefficient estimates are negative, but do not reach statistical significance. Although the results provide inconclusive support for negative spillovers from local manufacturing employment declines to local non-manufacturing employment, they show no evidence that non-manufacturing sectors helped absorb workers released from manufacturing due to the China trade shock.[33] Other margins must be accounting for this absorption.

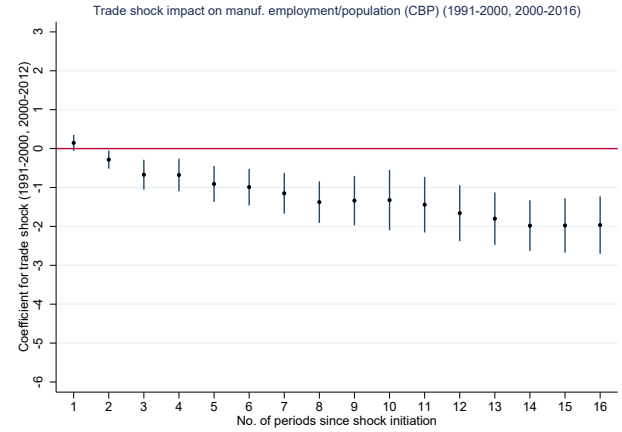
[32] This set of outcomes is not exhaustive. Workers exiting manufacturing employment may also end up incarcerated, enlisted into military service, homeless, or deceased (outcomes we observe imperfectly in the data).

[33] Bloom et al. (2019) report evidence that CZs experiencing greater import competition do see increases in non-manufacturing employment. They suggest that the discrepancy between their results and those in Autor et al. (2013) arises from differences in the time periods under analysis. We explore this possibility in Appendix Figures A15-A23 and find little evidence that it is the case. Instead, their results may depend on measuring employment in log terms, rather than as the share of the working-age population. Because there are persistent regional trends in population growth that long pre-date the China trade shock (Greenland et al., 2019), using log employment as the outcome variable risks confounding the impacts of the China trade shock on employment with these underlying trends. By using the ratio of employment to population, we effectively neutralize these trends (since they operate on both employment and population) and thereby capture the long-run impact of trade exposure on employment allocations.

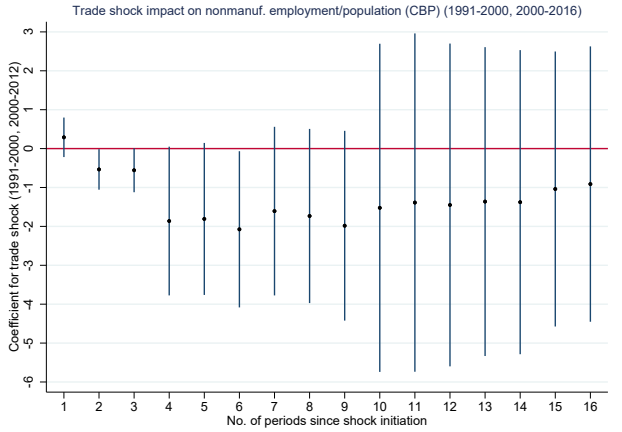
Figure 3: Trade Shock Impact on Employment, Stacked First Differences

(trade shock periods: 1991-2000 and 2000-2012)

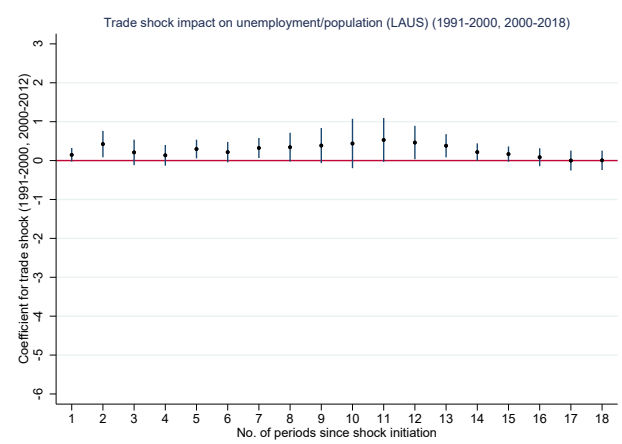
(a) Manuf. emp./Working-age pop.



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



(c) Unemployment/Working-age pop.



(d) Total emp./Working-age pop.



Note: Panels (a)-(d) report 2SLS stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012) in (1), instrumented by (3); and control variables include initial-period CZ employment (share of employment in manufacturing, including its interaction with a dummy for the second period; employment specialization in routine-task-intensive and offshorable occupations; share of women who are employed) and CZ demographic conditions (shares of the college educated and the foreign born in the population), Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state. The working-age population is individuals ages 18 to 64 years old.

In Figure 3c, we examine the impact of greater trade exposure on unemployment, defined as the share of those unemployed (as enumerated in the LAUS) in the working-age population. Impact coefficients are positive, indicating that CZs more exposed to the China trade shock experienced larger increases in unemployment. However, they attain statistical significance in just five of the 16 time periods. Impacts reach their peak 12 years after shock initiation, with a coefficient of 0.46

(t-value= 2.11). This result implies that when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a 0.44 ($= 0.46 \times [1.69 - .73]$) percentage-point larger increase in the share of the working-age population that is unemployed over 2000 to 2012. The positive impacts of greater import competition attenuate over time, dropping to 0.09 (t-value= 0.73) 16 years after shock initiation and to 0.01 (t-value= 0.05) 18 years beyond shock initiation. Unsurprisingly, in the long run movements into unemployment appear to play little role in absorbing workers who have lost their jobs in manufacturing.

A third margin of adjustment is non-employment, which sums the unemployed and those not in the labor force, and is measured by the share of the working-age population without a job.[34] In Figure 3d, we see that CZs more exposed to the China trade shock experienced larger reductions in their employment-population ratios. The magnitude of these impacts are comparable to those for manufacturing employment shares: they intensify up to 12 years beyond shock initiation and remain at these magnitudes out to 18 years beyond shock initiation. At 16 years after shock initiation, the impact coefficient is -1.93 (t-value= -2.33), which is very similar to the coefficient of -1.97 for manufacturing employment at that horizon. At 18 years beyond shock initiation, the impact coefficient is -2.21 (t-value= -2.31). 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.

The specifications we estimate are for stacked first differences. The advantage of this approach is that it increases power by allowing us to observe adjustment to trade shocks in successive time periods, and thereby control for confounds—such as regional exposure to automation, opportunities for offshoring, and demographic shifts—which are present in both time periods. Alternatively, one may wish to model the China trade shock as a singular event, such that we should evaluate results over a single long time difference. We show such results in Appendix Figure A7, in which we re-estimate (2) as a single time change, either for 1991 to 2016/2018 (for a trade shock defined over 1991 to 2012) or for 2000 to 2016/2018 (for a trade shock defined over 2000 to 2012). The results are

[34] We measure total employment using the REIS, rather than the CBP, because the BEA makes adjustments in county level employment that reduce measurement error. Because the REIS definitions of manufacturing and non-manufacturing are not consistent over time, we cannot use this source for sectoral employment levels, and so use the CBP. In Appendix Figure A6, we report results for the employment-population ratio for four alternative total employment measures, based on total REIS employment, REIS wage and salary employment, total CBP employment, and total LAUS employment. Although precision in the coefficient estimates varies across outcome measures, we see consistent evidence of long-run negative impacts of greater trade exposure on the employment-population ratio.

qualitatively consistent with those in Figure 3. However, we appear to be under-powered in single long time differences, such that coefficient estimates are sometimes no longer as precise.

Across employment outcomes, two features of labor-market adjustment are salient. One is the accumulation of impacts. As the China trade shock builds, the negative impact on manufacturing employment and total employment intensifies. A second is the absence of attenuation effects. The is no recovery in manufacturing or total employment for six to eight years after the shock reaches full potency. Among the surprising results in Autor et al. (2013) is that the impacts of the China Shock on manufacturing employment and non-participation in work are present at the decadal or near-decadal frequency, which we may consider the medium run. Extending the data out to 2018, we see that the decrease in employment-population ratios are highly persistent. To a first approximation, more-trade-exposed CZs suffered a permanent reduction in the size of their manufacturing labor force, with the bulk of this decline translating into a long-run increase in joblessness.

3.2.2 Spillovers between Regions

Trade shocks hitting one commuting zone may be transmitted to other commuting zones via changes in regional flows of goods and labor. We next evaluate evidence of cross-region spillovers of trade shocks. First, we consider indirect evidence of such spillovers by expanding our specification to account for a CZ's exposure to trade shocks in other CZs; second, we consider direct evidence of such spillovers by examining the impact of trade shocks on the local supply of labor.

Gravity-based Trade Shocks To evaluate the possibility that changes in labor-market outcomes in one region affect outcomes in other regions, Adao et al. (2019a) build a spatial general equilibrium trade model that captures such spillovers explicitly and generates reduced-form equilibrium conditions that have a shift-share structure. They show that if national industries are subject to exogenous shocks, such as through reform-induced improvements in Chinese manufacturing productivity, employment and wages in regional economies will be affected through two channels. One is through changes in local industry revenue, which is captured by changes in import penetration in (2). If greater import competition reduces revenues for local industries, there will be downward pressures on local wages and employment. 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, via the gravity structure of trade. For a given CZ, shocks to other CZs will matter more the larger and the closer are these other markets. 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).[35] We incorporate their approach by estimating the following extended version of equation (2):

$$\Delta Y_{it+h} = \alpha_t + \beta_{1h}\Delta IP^{cu}_{i\tau} + \beta_{2h}\sum_k z_{ikt}\Delta IP^{cu}_{k\tau} + \mathbf{X}'_{it}\beta_2 + e_{it+h}. \tag{4}$$

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,

$$z_{ikt} = \frac{L_{kt}D_{ik}^{-\delta}}{\sum_h L_{ht}D_{ih}^{-\delta}}. \tag{5}$$

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 five.[36] Appendix Figure A8 reports the results. The impacts of own-CZ trade shocks, shown in column A of Figure A8, are very similar in magnitude and significance to those in Figure 3. The impacts of gravity-based trade shocks, shown in column B of Figure A8, are small and imprecisely estimated for the manufacturing employment-population ratio (for all time horizons) and the total employment-population ratio (for all but one time horizon). Evidence of gravity-based spillovers thus appears to be weak.

Changes in Local Labor Supply The impact of greater import competition on changes in CZ log population headcounts summarize the net effect of trade shocks on local labor supply. Our results for these outcomes, shown in Figures 4a-d, indicate that greater exposure to import competition has effectively zero impact on the changes in the total CZ population or the CZ population of working-age adults, out to 18 years beyond shock initiation. These results are consistent with a large literature which finds that the local supply of less-educated workers—who constitute a substantial majority

[35] A second 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, it will raise real incomes, which will expand the demand for goods. There will be an own-region effect, in which local demand for goods changes, 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. Coefficients on these consumption terms are quantitatively small and highly imprecisely estimated. These null effects may indicate that consumption channel effects are common across regions and therefore absorbed into the constant term.

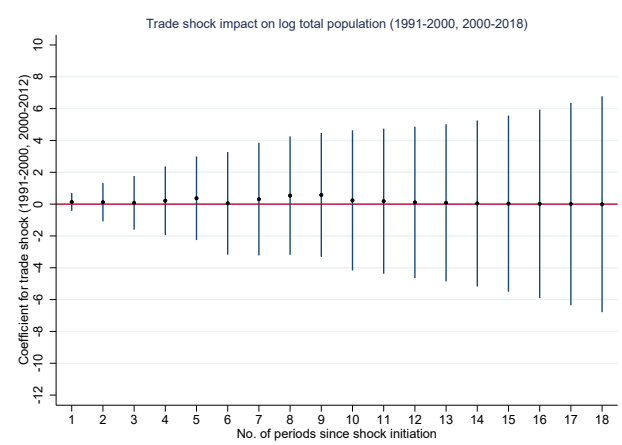
[36] We instrument for $\sum_k z_{ikt}\Delta IP^{cu}_{k\tau}$ by applying the gravity weights in (5) to the trade shock in (3).

of the manufacturing labor force—is minimally responsive to negative shocks to local labor demand (e.g., Bound and Holzer, 2000; Diamond, 2016; Notowidigdo, 2020).

Figure 4: Trade Shock Impact on Log Population Head Counts, Stacked First Differences

(trade shock periods: 1991-2000 and 2000-2012)

(a) Total population



(b) Population, ages 18-64



(c) Males, ages 18-64



(d) Females, ages 18-64



Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Labor supply responses to negative local labor demand shocks may differ by worker age. Younger workers may be more responsive to shocks and more mobile across regions, because they are less likely to be homeowners, are less likely to have formed families, are less established in their communities, or perceive a longer time horizon over which to reap the benefits of migration. In Appendix

Figures A10a and A10b, we examine the responsiveness of population headcounts to greater import exposure separately for workers 18-39 and 40-64 years old. Whereas impacts on older workers are mildly positive and on younger workers are mildly negative, neither set of coefficients comes close to statistical significance. In Figures A10c-f, we further disaggregate younger workers, now by gender and into subgroups for those 18-24 and 25-39 years old. Here, we see negative impacts of greater trade exposure on population headcounts for workers 25-39 years old, but not for workers 18-24 years old. Among the former group, negative impacts are mildly larger for men than for women, though none of these impacts quite reaches statistical significance. These results are broadly consistent with Autor et al. (2019), whose findings indicate that part of the differentially larger trade-induced reduction in population headcounts for young men may come from larger increases in their mortality, especially related to drug and alcohol abuse (Pierce and Schott, 2020).

The results in Figure A10 provide suggestive evidence that over longer horizons greater trade exposure leads to reductions in the population of young workers. Greenland et al. (2019) argue that the approach in Autor et al. (2013) for evaluating the impact of trade shocks on labor supply fails to account for dynamics in CZ population growth. There are strong secular trends in population growth across U.S. regions, which began well before 1990 (Blanchard and Katz, 1992). To account for such trends, in Appendix Figure A11 we re-estimate the regressions in Figure A10 and include as controls the log change in CZ population over 1970 to 1990 and its interaction with a period dummy. We take this approach, in lieu of using the lagged dependent variable as a control, to avoid introducing serial correlation into the specification. With time-varying controls for historical population growth, the impact of greater trade exposure has a significant negative impact on the change in log population headcounts, but only for workers 25 to 39 years old and only 10 years after shock initiation. The coefficient estimate for men 25-39 years old of -6.45 for $h = 18$ indicates that when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a -5.23 ($-6.45 \times [1.69 - .73]$) percentage-point larger decrease in headcounts for this age group over the 2000 to 2018 period. For comparison, the 25^{th}– 75^{th} percentile difference in population growth across CZs for males ages 25 to 39 over 2000 to 2018 was 20.44 ($= 19.19 + 1.25$) percentage points.[37] Cross-CZ spillovers via net migration thus appear to be modest and concentrated on narrow age groups.

[37]The greater mobility of the foreign-born, who account for relatively large share of younger workers, may be contributing to the greater labor-supply responsiveness for this age group (Cadena and Kovak, 2016).

3.2.3 Personal Income, Labor Earning,s and Government Transfers

Adverse impacts of trade shocks on local labor demand may reduce earnings, especially among lower-wage workers. Chetverikov et al. (2016) and Autor et al. (2019) find that greater exposure to import competition from China caused larger reductions in earnings in the bottom three to four deciles of CZ earning distributions, where declines are larger for men than for women. In order to assess changes in average well-being, we examine the impact of the China trade shock on personal income, labor compensation, and government transfers per capita. The results appear in Figure 5, where we report income and transfers relative to the total population of a commuting zone, given that non-working individuals may earn income from non-labor sources and receive government transfers of various kinds. In Figures A12-A14, we repeat the analysis using log income and transfers per capita as the outcomes, so as to compare responses in elasticity terms across income concepts.

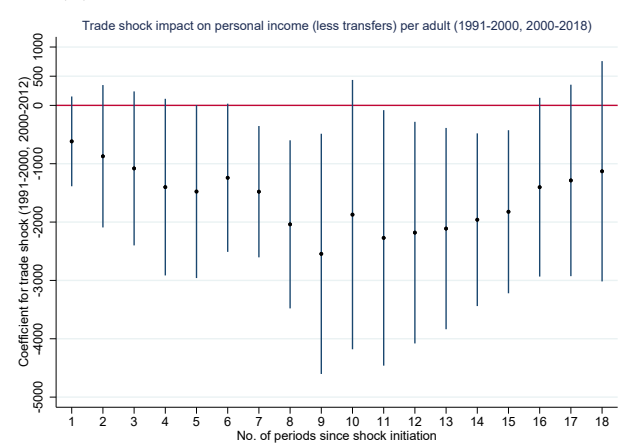
In Figure 5a, the impact of greater exposure to import competition on CZ personal income per capita is negative and precisely estimated out to 15 years beyond the initiation of the shock. Impacts remain negative, though less precisely estimated, out to 18 years after shock initiation. Negative effects reach their peak nine years after shock initiation, for which the impact coefficient is $-\$2,545$ (t-value $= -2.42$). The coefficient estimate of $-\$1,823$ (t-value $= -2.56$) at $h = 15$ indicates that when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a $-\$1,750$ ($-\$1,823 \times [1.69 - .73]$) smaller increase in personal income per capita over the 2000 to 2015 period. For comparison, the 25^{th} – 75^{th} percentile difference in the growth of personal income per capita for CZs over 2000 to 2015 was $\$5,559$ ($= \$8,179 - \$2,620$). Here, we measure personal income excluding government transfers, such that it includes wages and salaries; proprietor's income; dividend, interest, and rental income; and imputed income from employer contributions to health and pension plans.[38] As such, CZ personal income per capita approximates local GDP per capita. Impact coefficients for trade exposure on wage and salary compensation, seen in Figure 5c, and private non-farm compensation, seen in Figure 5d, are very similar to those for personal income, indicating that the depressive effects of import competition on local average incomes are primarily coming through changes in labor compensation (including employer benefits).

[38] See https://apps.bea.gov/regional/definitions/.

Figure 5: Trade Shock Impact on Income per Working-Age Adult, Stacked First Differences

(trade shock periods: 1991-2000 and 2000-2012)

(a) Personal income less government transfers



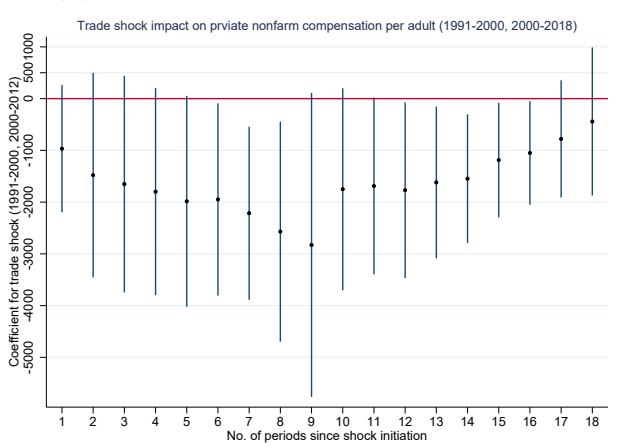
(b) Government transfers



(c) Wage and salary compensation



(d) Private non-farm compensation



Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

To what extent do increases in government transfers replace income lost to greater trade exposure? In Figure 5b, we see that CZs more exposed to import competition experienced larger increases in government transfers per capita at all time horizons, though impact coefficients are precisely estimated in just seven of the 18 regressions. The peak impact coefficient estimate of \$222 (t-value = 1.82) at $h = 15$ indicates that when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have received a \$213 (\$223 $\times$ [1.69 $-$ .73]) larger increase

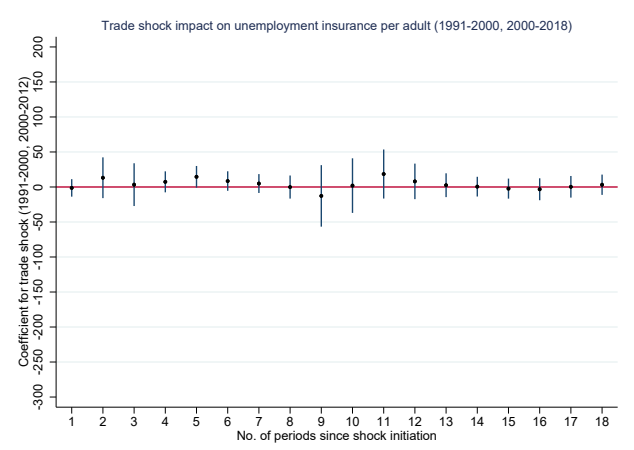
in government transfers per capita over the 2000 to 2015 time period. This income gain offsets just 12.2% (= 213/1750) of the trade-induced 25^{th} – 75^{th} differential loss in personal income per capita over the same 2000 to 2015 horizon. The results thus suggest that taken as a package government transfer programs are only modestly effective in helping commuting zones replace income lost to greater exposure to import competition from China.

In Figures 6 and 7, we explore the impact of trade shocks on transfers by government program. Three results stand out. First, in per capita terms, the response of Trade Adjustment Assistance (TAA) to trade shocks, seen in Figure 6b, is small. Although in elasticity terms, adjustment in TAA per capita is sizable (see Appendix Figure A13b), the associated monetary values are not. At its peak, three years after shock initiation, the TAA impact coefficient is just $29.96 (t-value = 2.05) per capita. This value falls to $11.06 (t-value = 1.13) nine years after shock initiation and is negative thereafter. Negative effects at longer horizons suggest that once the China trade shock has caused manufacturing job loss in a CZ, there are relatively few eligible workers left to receive TAA.

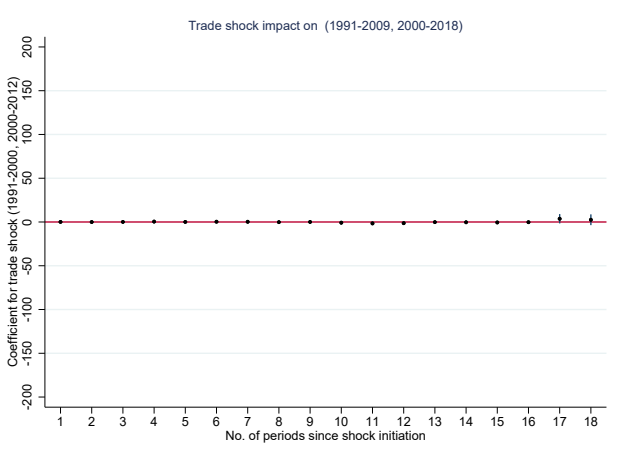
A second notable finding is that adjustment in Social Security and Medicare benefits, shown in Figure 6c-d, account for most of the increase in government transfers per capita induced by greater exposure to import competition. Moreover, the magnitude of these benefits increases as the time horizon expands. At 18 years after shock initiation, the impact coefficients for Social Security benefits, $70 (t-value = 1.44), and Medicare, $119 (t-value = 2.14), are 87.1% (= 189/217) of that for total transfers per capita, $217 (t-value = 1.46). Social Security payments include retirement benefits, from the Social Security Administration pension system, and disability benefits, from Social Security Disability Insurance. To be eligible for these benefits, an individual must leave the labor force, either by retiring or by being declared medically unable to work. It thus appears that the primary means through which government transfers are helping workers adjust to income losses from greater exposure to import competition is by inducing exit from paid work. These apparently negative impacts on work incentives may help account for the long-run depressive effects of greater trade exposure on employment-population ratios, seen in Figure 3.

Figure 6: Trade Shock Impact on Government Transfers per Working-Age Adult by Program
(trade shock periods: 1991-2000 and 2000-2012)

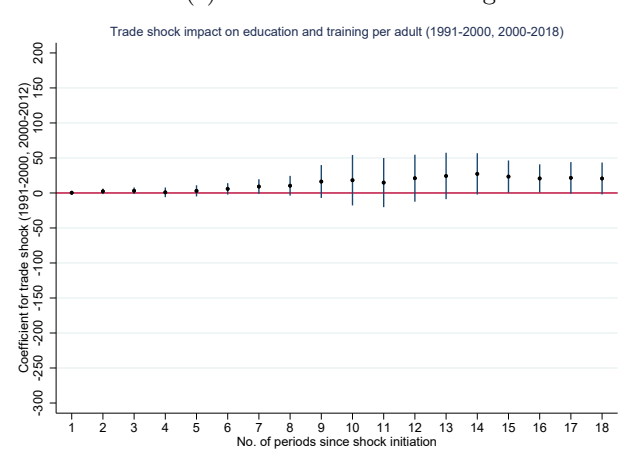
(a) Unemployment Insurance benefits



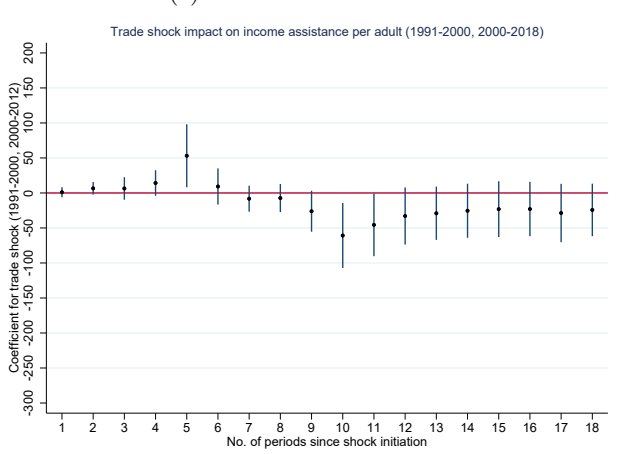
(b) Trade Adjustment Assistance



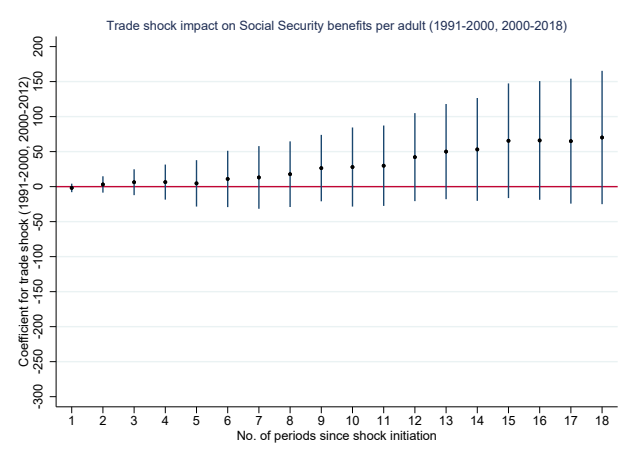
(c) Education and training assistance.



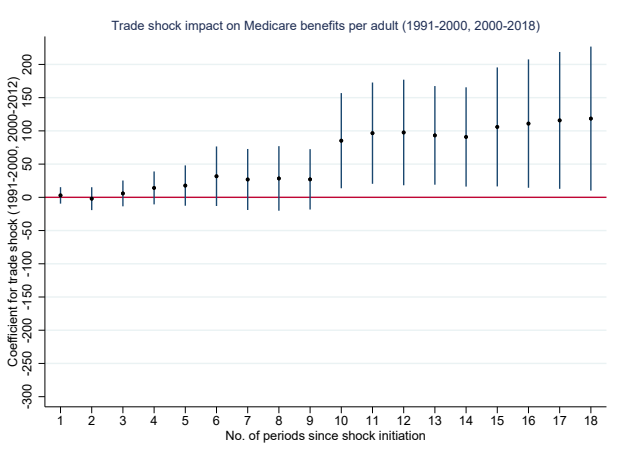
(c) Income assistance



(c) Social Security benefits



(d) Medicare benefits



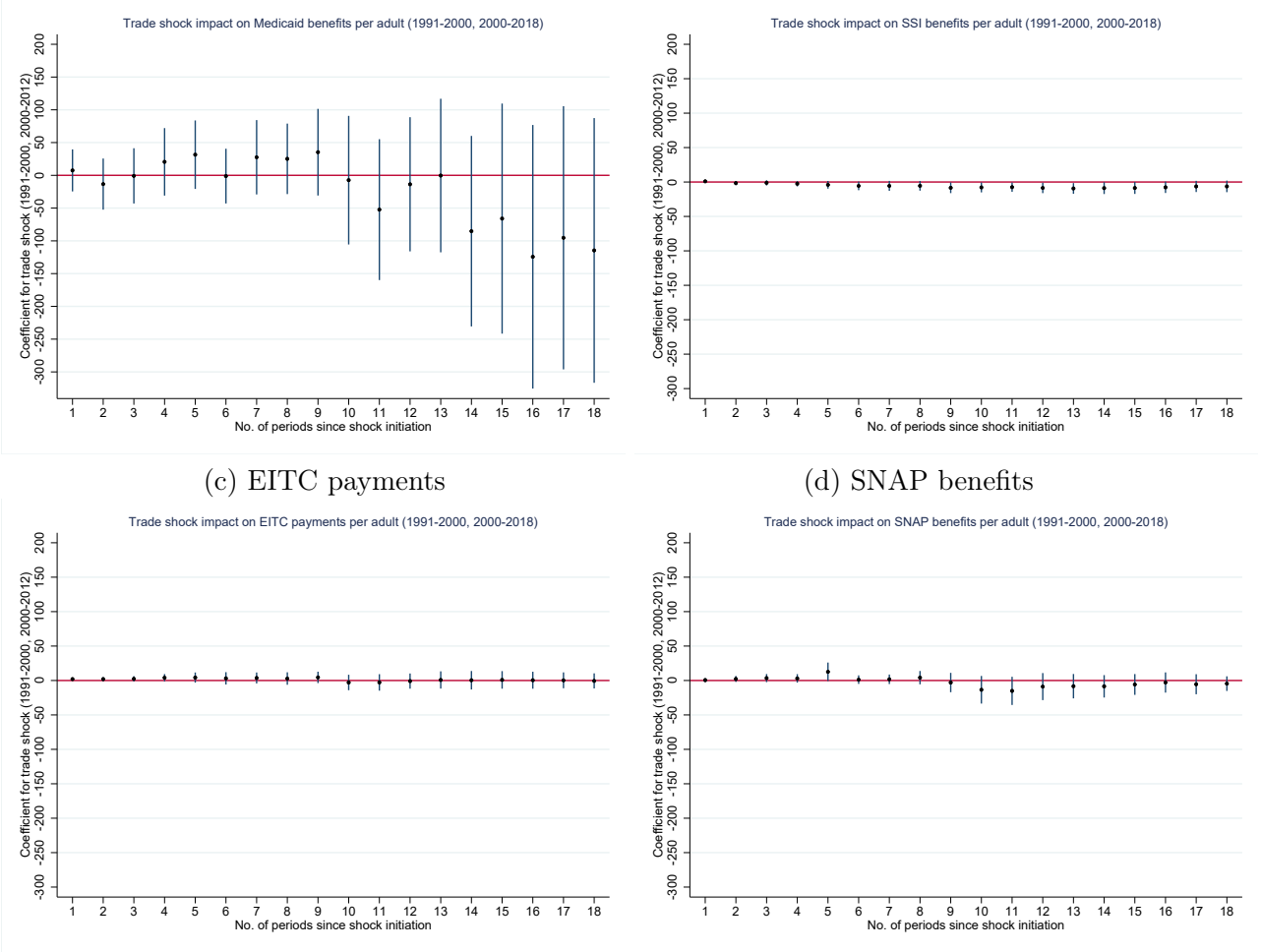
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure 7: Trade Shock Impact on Income Assistance per Working-Age Adult by Program

(trade shock periods: 1991-2000 and 2000-2012)

(a) Medicaid Benefits

(b) Supplemental Security Income



(c) EITC payments

(d) SNAP benefits

Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

A third notable finding regarding government transfers is that programs designed to encourage participation in work are largely unresponsive to trade shocks. The Earned Income Tax Credit (EITC) replaces earnings losses for low-income workers via a negative income tax. In Figure 7c, the responses of EITC payments per capita to trade shocks are close to zero. Impact coefficients vary from −$2.88 per capita ($h = 10$) to $3.98 per capita ($h = 4$), and are highly imprecisely estimated at all time horizons except $h = 1$. The unresponsive of EITC to trade shocks may reflect the fact

that the primary labor-market impact of greater trade exposure appears to be job loss, which would not directly induce greater payouts through EITC.

3.2.4 Extended Results (TBC)

In Appendix Figures A15 to A23, we re-estimate the regressions in Figure 3 for alternative beginning years (1991, 1992), mid-point years (1999, 2000, 2001, 2002), and end years (2007, 2010, 2012) for the China trade shock. The results are very similar across time-period definitions.

[We may also show results for alternative measures of the China trade shock (change in imports per worker, Pierce & Schott measure, gravity-based exposure measure). We will show results for alternative clustering of standard errors.]

4 Interpreting the Empirical Results (TBC)

Empirical analysis of the China trade shock reveals that local labor markets more exposed to import competition suffered larger declines in manufacturing jobs, employment-population ratios, and income per working-age adult, along with larger increases in unemployment and the uptake of government benefits. These effects persist for a decade and a half beyond shock initiation and for more than a half decade beyond peak impacts of the shock. The absence of an out-migration response to the shock—among over-40 and under-25 workers in particular—suggests that resident populations in exposed CZs bore the brunt of local manufacturing job loss and its repercussions. These results are consistent with more-trade-exposed commuting zones suffering larger losses in economic well-being than less-exposed CZs, and are thus indicative of the rank ordering of distributional impacts of the China trade shock across CZs. On their own, however, the results on relative trade impacts do not reveal which CZs, if any, perceived absolute declines in welfare. Such assessments require combining reduced-form empirical analysis with a quantitative general equilibrium model.

There are four complications in translating reduced-form shock impacts on employment and income into absolute impacts on welfare. First, there may be spillovers from impacted regions to other regions, through changes in regional imports and exports (or the migration of labor), which are not captured by own-CZ shocks. Spillovers could change both the sign of welfare impacts for individual CZs and the rank ordering of CZs in terms of welfare changes. The evidence that we

present in Figures 4 and A8-A11 suggests that such spillovers are modest in size. Second, trade shocks may change the national consumer price index, such that trade shock impacts on average incomes include national-level effects that would be absorbed into time dummies in (2). If the China trade shock lowered consumer prices or expanded product variety, estimated shock impacts on average incomes may overstate negative impacts on real earnings. Third, adverse trade shocks may reduce local housing prices. On the one hand, lower housing prices may reduce the local price of non-traded goods, and thereby offset negative impacts of trade shocks on nominal incomes; on the other hand, lower housing prices may lower household wealth. Fourth, to the extent that adverse trade shocks push workers into non-employment, one must know the reasons for non-employment to draw inferences on changes welfare. In the absence of labor-market distortions, increases in non-employment indicate decreases in real earnings and local economic well-being. In the presence of distortions, such as means-tested government transfers, assessing welfare impacts requires balancing increases in transfers, which may raise local incomes (and dampen inequality), against efficiency losses resulting from reduced employment and the distorting effects of taxation to fund transfers.

In this section, we discuss literature on the magnitude of welfare impacts from the China trade shock associated with changes in consumption possibilities and sectoral employment allocations, and what increases in non-employment say about changes in economic well-being. Progress on the first issue has been greater than on the second.

4.1 Welfare Analysis of the China Trade Shock

For individuals living in a given U.S. commuting zone, China's trade expansion affects well-being by altering the prices and the available varieties of inputs and outputs. To explore the mechanisms through which these changes affect welfare, we begin with models in which labor is homogeneous and effectively mobile across sectors and regions, and then consider more realistic frameworks in which workers are imperfectly mobile across categories.

4.1.1 No Labor Mobility

One mechanism through which trade shocks affect welfare is through changes in an economy's terms of trade. China's trade expansion is equivalent to an increase in the global supply of factor services embodied in apparel, furniture, electronics, and related goods, and an increase the global demand

for factor services embodied in agricultural goods, raw materials, and other non-manufacturing items (Adao et al., 2017; Costinot and Rodríguez-Clare, 2018). Because of the China trade shock, economies exporting factor services embodied in the first set of goods likely saw relatively weak demand for their factor exports, whereas economies exporting factor services embodied in the second set of goods likely saw relatively strong demand for theirs. Weaker demand for an economy's factor services, in turn, restricts domestic consumption possibilities and therefore demand for foreign factor services, such that trade-shock-induced changes in welfare are reflected in changes in an economy's spending on imports, as shown by Arkolakis et al. (2012) (ACR hereafter).

To explore the welfare impacts of the China trade shock, consider the analysis in Galle et al. (2017). They combine an Eaton and Kortum (2002) model of heterogeneous firms with a Roy (1951) model of heterogeneous workers (Lagakos and Waugh, 2013; Hsieh et al., 2019). Firms in country c and sector j draw their productivity from a Fréchet distribution with location parameter T_{cj} and shape parameter θ (here, assumed constant across sectors). The location parameters represent the fundamental productivities of a country in different sectors and embody the determinants of comparative advantage at the national level. On the labor side, workers from region i draw their number of efficiency units in sector j from a Fréchet distribution that has location parameter A_{cij} and shape parameter κ (here, assumed constant across regions). Because workers are geographically immobile, the A_{cij} location parameters create regional differences in sector-specific labor supplies, which embody the regional determinants of comparative advantage. In this setting, product prices are set at the national level. Changes in these prices therefore affect welfare through changes in a price index that is common across regions within a country. We can then separate welfare impacts that are national versus region-specific in origin.

The welfare impacts of trade shocks are given by the following expression:

$$d \ln W_{ci} = -\sum_j \frac{\beta_j}{\theta} d \ln \lambda_{ccj} - \sum_j \frac{\beta_j}{\kappa} d \ln \pi_{cij} \tag{6}$$

where $d \ln W_{ci}$ is the log change in real income in region i of country c, β_j is the Cobb-Douglas expenditure share for products in sector j, $d \ln \lambda_{ccj}$ is the trade-shock-induced log change in the share of expenditure on sector j products that a country produces itself. If economic reform in China causes its fundamental productivities to improve more in some sectors than in others—say, by allowing rural labor, imported inputs, and foreign capital and technology to flow into urban factories pro-

ducing labor-intensive goods—economies with relatively strong fundamental productivities in these sectors would see their terms of trade deteriorate, while economies whose comparative advantage lies elsewhere would see their terms of trade improve. A strengthening of the terms of trade would translate into greater expenditure on foreign goods.

Caliendo et al. (2019) construct a dynamic model of labor-market adjustment to the China trade shock, to which we return below, which in the long-run collapses to an ACR structure. They allow for multiple factors of production (labor, structures, intermediates), input-output linkages across 12 manufacturing and 8 non-manufacturing sectors, and trade imbalances for 50 U.S. states and 37 countries. To calculate the China trade shock, they back out the implied growth in sectoral productivity needed to generate the observed change in U.S. manufacturing imports from China, as predicted by the first-stage regression of Autor et al. (2013).[39] The implied productivity changes for the 12 sectors over 2000 to 2007 have a median value of 2.6%, which range from a low of 1.5% (mineral fuels) to a high of 5.1% (transportation equipment). In the long run, they estimate that the 2000 to 2007 China trade shock caused aggregate U.S. welfare to rise by 0.2%. The welfare effects are small because the shock-induced changes in spending on foreign goods is small. Such small national-level welfare changes arising from shifts in the terms of trade suggest that there is scope for substantial variation in welfare impacts at the regional level, which we explore next.

4.1.2 Partial Labor Mobility

Now, suppose that labor is immobile across regions within a country and only partially immobile across sectors. In the Galle et al. (2017) model, this would be achieved by letting the parameter κ in (6) go to infinity. The resulting change in real income from the China trade shock is 0.2%.

To estimate the welfare impacts of China's rise for individual regions, Caliendo et al. (2019) construct a dynamic model which in the long-run generates welfare changes that converge to the expression in (6), expanded to allow for multiple factors of production (labor, structures, intermediates), input-output linkages across 12 manufacturing and 8 non-manufacturing sectors, and trade imbalances for 50 U.S. states and 37 countries. They calculate the sectoral shocks that caused China's trade to expand by backing out the implied growth in sectoral productivity needed to gen-

[39] In our context, this would be equivalent to using the value in (3) to predict the value in (1) and then using the structure of the Caliendo-Dvorkin-Parro model to determine the sectoral changes in Chinese productivity that would be consistent with these predictions.

erate the observed change in U.S. imports from China, as predicted by the first-stage regression of Autor et al. (2013). In our context, this would be equivalent to using the value in (3) to predict the value in (1) and then using the structure of the Caliendo-Dvorkin-Parro model to determine the sectoral changes in Chinese productivity that would be consistent with the predictions.

The Caliendo et al. (2019) results are instructive for they provide a benchmark for changes in U.S. welfare caused by China's rise. In the long-run equilibrium of their model, labor is fully mobile across sectors and regions, as is standard in many quantitative analyses of trade shocks.[40]

4.1.3 Housing Prices

4.2 Other Issues regarding Interpretation

- *Trade imbalances*:
  - In a trade model with balanced trade and manufacturing as the only traded activity, then by construction aggregate effects of trade on manufacturing employment have to be neutral (aside from any modest manufacturing job loss that may result from resources being reallocated from labor-intensive, import-competing manufacturing industries to capital-intensive export-oriented manufacturing industries). As we show in the appendix to Autor et al. (2013), this logic breaks down in the presence of unbalanced trade.
  - New empirical results suggest that we can elaborate this discussion by noting that our model doesn't require aggregate trade imbalances, just trade imbalances in manufacturing, which don't have to be macroeconomic in origin. If the U.S. has a comparative advantage in services and a comparative disadvantage in manufacturing, then expanded trade with China will enlarge the U.S. trade surplus in the former sector and the U.S. trade deficit in the latter sector. We now know that the U.S. runs a large trade surplus in

[40] In related work, Hsieh and Ossa (2016) apply a version (6), expanded to allow for variation in trade-cost elasticities across sectors, multiple production factors, input-output linkages, trade imbalances, and heterogeneous firms as in Melitz (2003). The trade shock itself is measured using observed manufacturing productivity growth in China over 1995 to 2007. In the long run, with free entry of firms, U.S. welfare gains are 0 to 0.03%. These small net gains from trade with China combine larger adverse changes in the U.S. terms of trade, which reduce welfare, and positives effects from firm delocation (i.e., greater firm entry in sectors with relatively low trade-cost elasticities). One potential limitation of their approach is that they quantify the China trade shock using observed Chinese manufacturing productivity growth, whose median annual value across industries is 3.5% (with a range 2.1% to 4.7% across 15 sectors).This measure of productivity growth adjusts for usage of intermediate inputs (and equals the growth rate of real output per composite factor of production, scaled by the share of valued added in gross output); not accounting for intermediates yields much larger raw productivity growth (median value of 11.2%). Measured productivity growth is little changed when accounting for the impacts of firm entry implied by the Melitz (2003) model.

services in the aggregate and specifically with China. Hence, it appears that the China Shock induced a reallocation of labor not from import-competing manufacturing to export manufacturing but from import-competing manufacturing to exportable services, such as higher education, tourism, and the licensing of intellectual property.

  - The widening U.S.-China trade surplus in education and tourism is easy to document. Net trade in IP services is tougher to measure but recent work provides helpful insights. Guvenen, Mataloni, Rassier & Ruhl (2018) measure the equivalent of net U.S. trade in IP services in the form of changes in net intangible assets that U.S. multinationals hold abroad (their finding of substantial net intangible asset accumulation is buttressed by the recent findings of Torslov, Wier & Zucman (2918) that MNEs park 40% of foreign profits in tax havens, a practice that is especially attractive to firms whose profits can be stashed alongside their foreign patent boxes). Hence, we no longer have to talk about trade balances as a pesky macro issue. We can instead discuss the trade balance in manufacturing as emblematic of U.S. comparative advantage.

- *It's not trade, it's technology* (eg, Lawrence, Edwards & Lawrence). To address: (a) walk readers through our controls and the Pierce & Schott controls for technology shocks, (b) appeal to the strong correlation in timing between China's post-2001 surge and the collapse of U.S. manufacturing jobs and the absence of obvious technology shocks that materialize in 2001 (the year, in fact, when the dot.com bust occurs), (c) discuss the qualitative similarity of China Shock impacts in many other manufacturing economies, as referenced in Autor et al. (2016), and (d) emphasize the findings in Autor et al. (2015) that trade and technology shocks are effectively uncorrelated at the CZ level.

- *The China Shock was due to temporary currency manipulation, and therefore temporary* (eg, Bergsten and Gagnon). In a simple model, there is no industry-specific impact of China's currency manipulation, as any currency effect would be loaded onto time dummies in the regression analysis. There are two ways in which currency manipulation could have mattered. The pass-through of exchange rates to prices varies across sectors according to industry variation in the markup of price over marginal cost, which could introduce an industry-specific angle to the impacts of currency changes. It could also be the case that there is variation across

industries in the mass of firms on the margin of exiting, such that variation in the aggregate intensity of the China Shock could arise from (i) variation in this mass of vulnerable firms, and (ii) variation in regional specialization. Even with one or the other of the above stories holding, the impacts of the China Shock should have attenuated after 2006, when China's currency manipulation appeared to subside. Our data show the China Shock didn't go away.

- *Overall U.S. export growth in manufacturing is sufficient to counteract the China Shock* (eg, Feenstra, Ma & Xu).

5 Final Discussion (TBC)

Policy issues: discuss how China trade shock results have been used by policy makers.

- The Obama Administration's reaction was constructive. Its members (e.g., U.S. Trade Representative Michael Froman) asked, how can the government help protect workers from adverse consequences of trade shocks? Their quite reasonable answer: expand EITC (and propose wage insurance, though it had no hope of passing given GOP control of Congress at the time).

- The Trump Administration's reaction, as characterized by Peter Navarro, has not been constructive. There is no reason to believe that U.S. import tariffs on steel and autos will restore jobs lost to the China Shock. First off, these are the wrong sectors to target; more importantly, import tariffs are an Nth-best current policy to help low-wage workers who were negatively affected by trade in the 1990s and early 2000s.

- The future China Shock is about industrial policy. The 1990 to 2010 China Shock was about the one-time transition to a market economy, which affected vestigial industries in the U.S. and therefore was inevitable. The next China shock will be about its industrial policy (China 2025). This shock is not inevitable and will be heavily affected by China's and U.S. technology policy (examples for industries). Thus, while the 1st China Shock would in principle have been forecastable in 1991, this 2nd China Shock does not appear to be forecastable.

References

Acemoglu, D., D. Autor, D. Dorn, G. H. Hanson, and B. Price (2016): "Import Competition and the Great US Employment Sag of the 2000s," *Journal of Labor Economics*, 34, S141–S198.

Adao, R., C. Arkolakis, and F. Esposito (2019a): "Spatial linkages, global shocks, and local labor markets: Theory and evidence," Tech. rep., National Bureau of Economic Research.

Adao, R., A. Costinot, and D. Donaldson (2017): "Nonparametric counterfactual predictions in neoclassical models of international trade," *American Economic Review*, 107, 633–89.

Adao, R., M. Kolesár, and E. Morales (2019b): "Shift-share designs: Theory and inference," *The Quarterly Journal of Economics*, 134, 1949–2010.

Amior, M. and A. Manning (2018): "The persistence of local joblessness," *American Economic Review*, 108, 1942–70.

Arkolakis, C., A. Costinot, and A. Rodríguez-Clare (2012): "New trade models, same old gains?" *American Economic Review*, 102, 94–130.

Asquith, B., S. Goswami, D. Neumark, and A. Rodriguez-Lopez (2019): "Us job flows and the china shock," *Journal of International Economics*, 118, 123–137.

Austin, B. A., E. L. Glaeser, and L. H. Summers (2016): "Jobs for the Heartland: Place-based policies in 21st century America," *Brookings Papers on Economic Activity*, 1, 151–232.

Autor, D. H. and D. Dorn (2013): "The Growth of Low Skill Service Jobs and the Polarization of the U.S. Labor Market," *American Economic Review*, 103, 1553–1597.

Autor, D. H., D. Dorn, and G. H. Hanson (2013): "The China Syndrome : Local Labor Market Effects of Import Competition in the United States," *American Economic Review*, 103, 2121–2168.

——— (2015): "Untangling Trade and Technology: Evidence from Local Labor Markets," *Economic Journal*, 125, 621–645.

——— (2016): "The China Shock: Learning from Labor Market Adjustment to Large Changes in Trade," *Annual Review of Economics*, 8, 205–240.

——— (2019): "When work disappears: Manufacturing decline and the falling marriage market value of young men," *American Economic Review: Insights*, 1, 161–78.

Autor, D. H., D. Dorn, G. H. Hanson, and K. Majlesi (2020): "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure," *American Economic Review*, 0, forthcoming.

Autor, D. H., D. Dorn, G. H. Hanson, and J. Song (2014): "Trade Adjustment: Worker-Level Evidence," *Quarterly Journal of Economics*, 129, 1799–1860.

Bai, L. and S. Stumpner (2019): "Estimating US consumer gains from Chinese imports," *American Economic Review: Insights*, 1, 209–24.

Bai, X., K. Krishna, and H. Ma (2017): "How you export matters: Export mode, learning and productivity in China," *Journal of International Economics*, 104, 122–137.

Baicker, K. and M. M. Rehavi (2004): "Policy watch: trade adjustment assistance," *Journal of Economic Perspectives*, 18, 239–255.

Balassa, B. (1965): "Trade Liberalisation and Revealed Comparative Advantage," *The Manchester School*, 33, 99–123.

Bartik, T. J. (1991): "Who benefits from state and local economic development policies?" .

——— (2020): "Should Place-Based Jobs Policies Be Used to Help Distressed Communities?" *Journal of Economic Perspectives*, 0, forthcoming.

Bergsten, C. F. and J. E. Gagnon (2017): *Currency Conflict and Trade Policy: A New Strategy for the United States*, Columbia University Press.

Bernard, A. B., J. B. Jensen, and P. K. Schott (2006): "Survival of the Best Fit: Exposure to Low-Wage Countries and the (Uneven) Growth of US Manufacturing Plants," *Journal of international Economics*, 68, 219–237.

Blanchard, O. J. and L. F. Katz (1992): "Regional evolutions," *Brookings Papers on Economic Activity*, 23, 1–75.

Bloom, N., K. Handley, A. Kurman, and P. Luck (2019): "The impact of chinese trade on us employment: The good, the bad, and the debatable," *Unpublished draft*.

Borusyak, K., P. Hull, and X. Jaravel (2018): "Quasi-experimental shift-share research designs," Tech. rep., National Bureau of Economic Research.

Bound, J. and H. J. Holzer (2000): "Demand shifts, population adjustments, and labor market outcomes during the 1980s," *Journal of labor Economics*, 18, 20–54.

Brandt, L. and K. Lim (2019): "Accounting for Chinese Exports," Tech. rep., Working paper.

Brandt, L. and P. M. Morrow (2017): "Tariffs and the Organization of Trade in China," *Journal of International Economics*, 104, 85–103.

Brandt, L., T. Tombe, and X. Zhu (2013): "Factor market distortions across time, space and sectors in China," *Review of Economic Dynamics*, 16, 39–58.

Brandt, L., J. Van Biesebroeck, L. Wang, and Y. Zhang (2017a): "WTO Accession and Performance of Chinese Manufacturing Firms," *American Economic Review*, 107, 2784–2820.

Brandt, L., J. Van Biesebroeck, and Y. Zhang (2012): "Creative accounting or creative destruction? Firm-level productivity growth in Chinese manufacturing," *Journal of development economics*, 97, 339–351.

Brandt, L., L. Wang, and Y. Zhang (2017b): "Productivity in Chinese Industry: 1998-2013 (Very) Preliminary Findings," *Working Paper*.

Cadena, B. C. and B. K. Kovak (2016): "Immigrants equilibrate local labor markets: Evidence from the Great Recession," *American Economic Journal: Applied Economics*, 8, 257–90.

Caliendo, L., M. Dvorkin, and F. Parro (2019): "Trade and Labor Market Dynamics: General Equilibrium Analysis of the China Trade Shock," *Econometrica*, 87, 741–835.

Chan, J. (2019): "Labour market characteristics and surviving import shocks," *The World Economy*, 42, 1288–1315.

Chetverikov, D., B. Larsen, and C. Palmer (2016): "IV Quantile Regression for Group-Level Treatments, With an Application to the Distributional Effects of Trade," *Econometrica*, 84, 809–833.

Cheung, Y.-W., M. D. Chinn, and E. Fujii (2007): "The overvaluation of renminbi undervaluation," *Journal of International Money and Finance*, 26, 762–785.

Chodorow-Reich, G. (2019): "Geographic cross-sectional fiscal spending multipliers: What have we learned?" *American Economic Journal: Economic Policy*, 11, 1–34.

——— (2020): "Regional data in macroeconomics: Some advice for practitioners," *Journal of Economic Dynamics and Control*, 103875.

Colantone, I. and P. Stanig (2018a): "Global Competition and Brexit," *American Political Science Review*, 112, 201–218.

——— (2018b): "The Trade Origins of Economic Nationalism: Import Competition and Voting Behavior in Western Europe," *American Journal of Political Science*, 62, 936–953.

Costinot, A. and A. Rodríguez-Clare (2018): "The US gains from trade: Valuation using the demand for foreign factor services," *Journal of Economic Perspectives*, 32, 3–24.

Davis, S. J. and T. M. von Wachter (2011): "Recessions and the Costs of Job Loss," *Brookings Papers on Economic Activity*, 1, 1–72.

Diamond, R. (2016): "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980 – 2000," *American Economic Review*, 106, 479–524.

Dix-Carneiro, R. and B. K. Kovak (2017): "Trade liberalization and regional dynamics," *American Economic Review*, 107, 2908–46.

Dorn, D. (2009): "Essays on Inequality, Spatial Interaction, and the Demand for Skills," Dissertation University of St. Gallen No. 3613.

Eaton, J. and S. Kortum (2002): "Technology, geography, and trade," *Econometrica*, 70, 1741–1779.

Eriksson, K., K. Russ, J. C. Shambaugh, and M. Xu (2019): "Trade shocks and the shifting landscape of us manufacturing," Tech. rep., National Bureau of Economic Research.

Fajgelbaum, P. D. and C. Gaubert (2020): "Optimal spatial policies, geography, and sorting," *The Quarterly Journal of Economics*, 135, 959–1036.

Feenstra, R. (2015): *Advanced international trade: theory and evidence*, Princeton University Press.

Feenstra, R. C. and G. H. Hanson (2005): "Ownership and control in outsourcing to China: Estimating the property-rights theory of the firm," *The Quarterly Journal of Economics*, 120, 729–761.

Feenstra, R. C., H. Ma, and Y. Xu (2019): "US exports and employment," *Journal of International Economics*, 120, 46–58.

Feler, L. and M. Z. Senses (2017): "Trade shocks and the provision of local public goods," *American Economic Journal: Economic Policy*, 9, 101–43.

Galle, S., A. Rodríguez-Clare, and M. Yi (2017): "Slicing the pie: Quantifying the aggregate and distributional effects of trade," Tech. rep., National Bureau of Economic Research.

Gaubert, C., P. Kline, and D. Yagan (2020): "Place-Based Redistribution," .

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2020): "Bartik Instruments: What, When, Why, and How," *American Economic Review*, 110.

Goos, M., A. Manning, and A. Salomons (2014): "Explaining Job Polarization: Routine-Biased Technological Change and Offshoring," *American Economic Review*, 104, 2509–2526.

Greenland, A. and J. Lopresti (2016): "Import exposure and human capital adjustment: Evidence from the US," *Journal of International economics*, 100, 50–60.

Greenland, A., J. Lopresti, and P. McHenry (2019): "Import competition and internal migration," *Review of Economics and Statistics*, 101, 44–59.

Grossman, G. M. (1982): "The employment and wage effects of import competition in the United States," Tech. rep., National Bureau of Economic Research.

Guren, A., A. McKay, E. Nakamura, and J. Steinsson (2020): "What Do We Learn From Cross-Regional Empirical Estimates in Macroeconomics?" in *NBER Macroeconomics Annual 2020, volume 35*, University of Chicago Press.

Hanson, G. H., N. Lind, and M.-A. Muendler (2015): "The dynamics of comparative advantage," Tech. rep., National bureau of economic research.

Heckman, J. J., L. Lochner, and C. Taber (1998): "Explaining rising wage inequality: Explorations with a dynamic general equilibrium model of labor earnings with heterogeneous agents," *Review of Economic Dynamics*, 1, 1–58.

Helpman, E. (2018): *Globalization and Inequality*, Harvard University Press.

Hsieh, C.-T., E. Hurst, C. I. Jones, and P. J. Klenow (2019): "The allocation of talent and us economic growth," *Econometrica*, 87, 1439–1474.

Hsieh, C.-T. and P. J. Klenow (2009): "Misallocation and manufacturing TFP in China and India," *The Quarterly journal of economics*, 124, 1403–1448.

Hsieh, C.-T. and R. Ossa (2016): "A global view of productivity growth in China," *Journal of International Economics*, 102, 209–224.

Hsieh, C.-T. and Z. Song (2015): "Grasp the Large, Let Go of the Small: The Transformation of the State Sector in China," *Brookings Papers on Economic Activity*, 1, 295–366.

Hu, A. G. and G. H. Jefferson (2009): "A great wall of patents: What is behind China's recent patent explosion?" *Journal of Development Economics*, 90, 57–68.

Hyman, B. (2018): "Can displaced labor be retrained? Evidence from quasi-random assignment to trade adjustment assistance," Tech. rep., Working Paper.

Jacobson, L., R. LaLonde, and D. Sullivan (1993): "Earnings losses of displaced workers," *The American economic review*, 685–709.

Jaravel, X. and E. Sager (2019): "What are the price effects of trade? Evidence from the US and implications for quantitative trade models," .

Jordà, Ò. (2005): "Estimation and inference of impulse responses by local projections," *American economic review*, 95, 161–182.

Kee, H. L. and H. Tang (2016): "Domestic Value Added in Exports: Theory and Firm Evidence from China," *American Economic Review*, 106, 1402–36.

Khandelwal, A. K., P. K. Schott, and S.-J. Wei (2013): "Trade liberalization and embedded institutional reform: Evidence from Chinese exporters," *American Economic Review*, 103, 2169–95.

Kim, R. and J. Vogel (2020): "Trade and welfare (across local labor markets)," Tech. rep., National Bureau of Economic Research.

Kletzer, L. G. (2001): *Job loss from imports: Measuring the costs*, Peterson Institute for International Economics.

Koopman, R., Z. Wang, and S.-J. Wei (2012): "Estimating domestic content in exports when processing trade is pervasive," *Journal of Development Economics*, 99, 178–189.

Lagakos, D. and M. E. Waugh (2013): "Selection, agriculture, and cross-country productivity differences," *American Economic Review*, 103, 948–80.

Lardy, N. (2019): *The State Strikes Back: The End of Economic Reform in China?*, Peterson Institute for International Economics.

Maneschi, A. (2008): "How would David Ricardo have taught the principle of comparative advantage?" *Southern economic journal*, 1167–1176.

Melitz, M. J. (2003): "The impact of trade on intra-industry reallocations and aggregate industry productivity," *econometrica*, 71, 1695–1725.

Monte, F., S. J. Redding, and E. Rossi-Hansberg (2018): "Commuting, migration, and local employment elasticities," *American Economic Review*, 108, 3855–90.

Naughton, B. (2007): *The Chinese Economy: Transitions and Growth*, MIT Press.

——— (2016): "Inside and outside: The modernized hierarchy that runs China," *Journal of Comparative Economics*, 44, 404–415.

——— (2017): "The current wave of state enterprise reform in China: A preliminary appraisal," *Asian Economic Policy Review*, 12, 282–298.

Notowidigdo, M. J. (2020): "The incidence of local labor demand shocks," *Journal of Labor Economics*, 38, 000–000.

Pierce, J. R. and P. K. Schott (2016): "The Surprisingly Swift Decline of US Manufacturing Employment," *American Economic Review*, 106, 1632–1662.

——— (2020): "Trade liberalization and mortality: evidence from US counties," *American Economic Review: Insights*, 2, 47–64.

Redding, S. J. (2020): "Trade and Geography," *National Bureau of Economic Research Working Paper Series*.

Ricardo, D. (1815): *An essay on the influence of a low price of corn on the profits of stock, with remarks on mr. Malthus' two last publications*.

Rodríguez-Clare, A., M. Ulate, and J. P. Vásquez (2019): "New-Keynesian Trade: Understanding the Employment and Welfare Effects of Sector-Level Shocks," .

Rodrik, D. (2020): "Why Does Globalization Fuel Populism? Economics, Culture, and the Rise of Right-wing Populism," Tech. rep., National Bureau of Economic Research.

Roy, A. D. (1951): "Some thoughts on the distribution of earnings," *Oxford economic papers*, 3, 135–146.

Song, Z., K. Storesletten, and F. Zilibotti (2011): "Growing like china," *American Economic Review*, 101, 196–233.

Tolbert, C. M. and M. Sizer (1996): "US Commuting Zones and Labor Market Areas: A 1990 Update," *U.S. Department of Agriculture*, Economic Research Service Staff Paper.

Vogel, E. F. (2011): *Deng Xiaoping and the transformation of China*, vol. 10.

Wei, S.-J., Z. Xie, and X. Zhang (2017): "From" Made in China" to" Innovated in China": Necessity, prospect, and challenges," *Journal of Economic Perspectives*, 31, 49–70.

Wood, A. (1995): "How trade hurt unskilled workers," *Journal of Economic perspectives*, 9, 57–80.

——— (2018): "The 1990s trade and wages debate in retrospect," *The World Economy*, 41, 975–999.

Yu, M. (2010): "Trade Liberalization and Productivity: Evidence from Chinese Firms," *Economic Research Journal*, 12, 97–110.

Appendix A: Figures and Tables

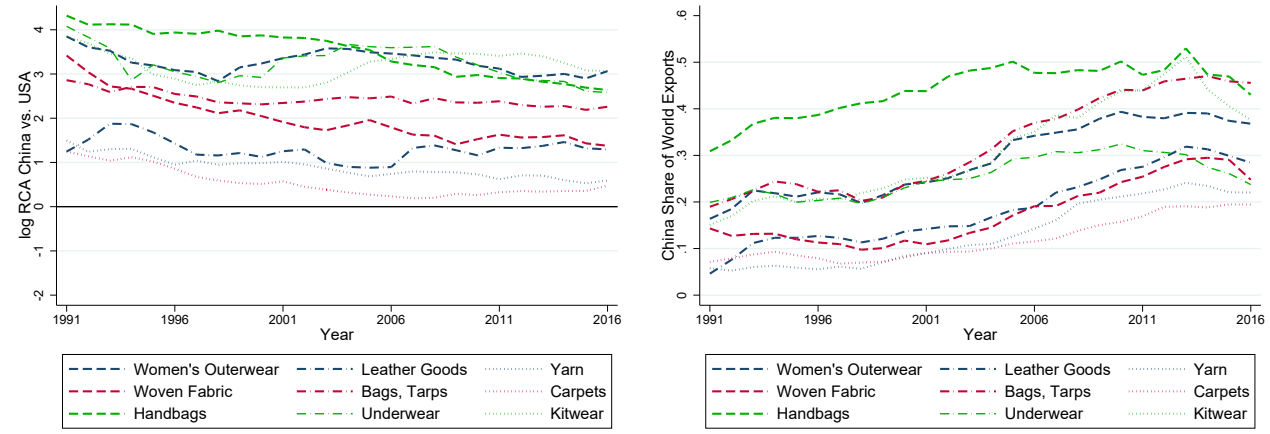
The trade data we use to examine revealed comparative advantage are from UN Comtrade and based on SITC Rev. 2 product codes (https://comtrade.un.org). We thank Robert Feenstra and Mingzhi Xu for access to a cleaned version of these data. We use SITC and not HS product categories in order to calculate world exports by product in all years. HS product codes, which we use to construct the China trade shock so as to be able to concord trade values to SIC industries, were adopted well after 1991 by many countries. We use 2016 as the final year for the data, given changes in SITC classifications in 2017 and 2018, which appear to have moved trade in many of China's manufactured goods into SITC 931 (Special transactions and commodities not classified according to kind, which functions effectively as a category for anomalies and errors in trade flows).

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 reported here.

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

(a) China-U.S. Log RCA

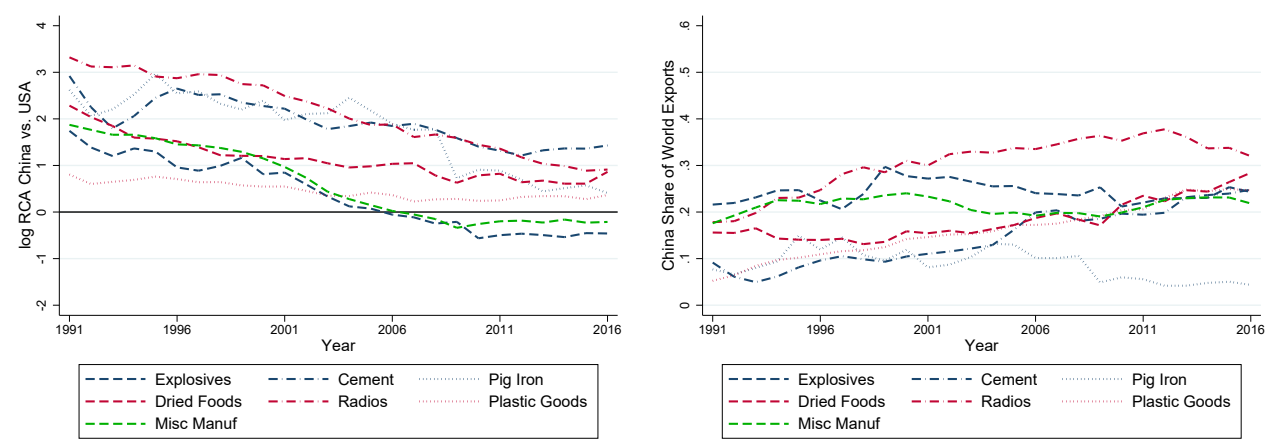
(b) China share of World Exports



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

(c) China-U.S. Log RCA

(d) China share of World Exports

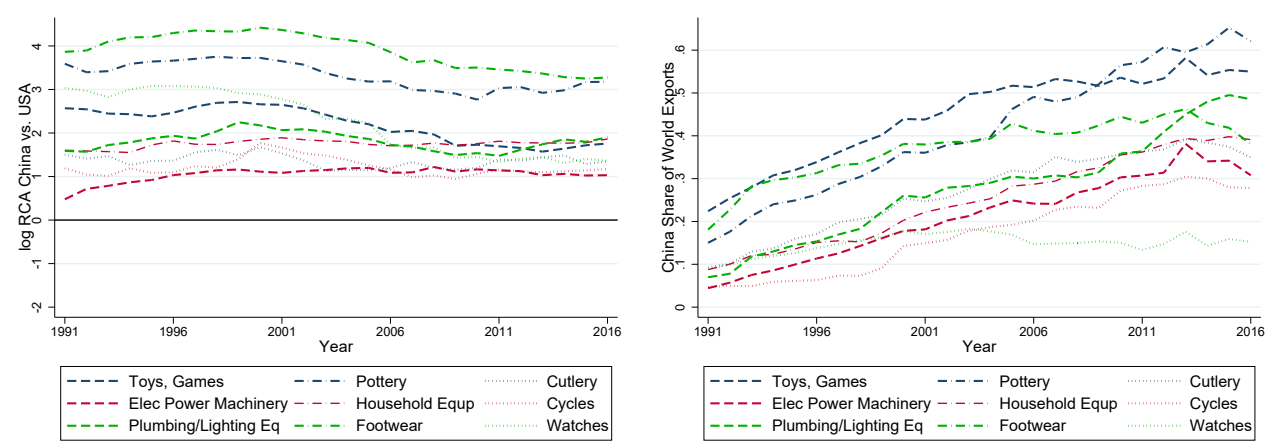


Note: SITC Rev. 2 three-digit categories 056 (dried foods), 572 (explosives and fireworks), 661 (cement, lime), 671 (pig iron), 762 (radios), 893 (assorted plastic goods), 899 (miscellaneous manufactured goods).

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

(a) China-U.S. Log RCA

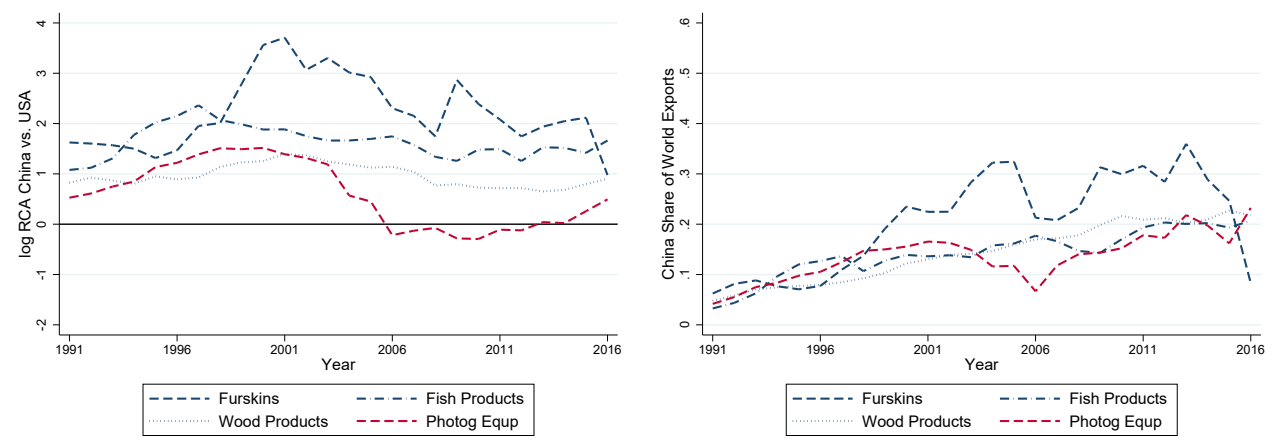
(b) China share of World Exports



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

(c) China-U.S. Log RCA

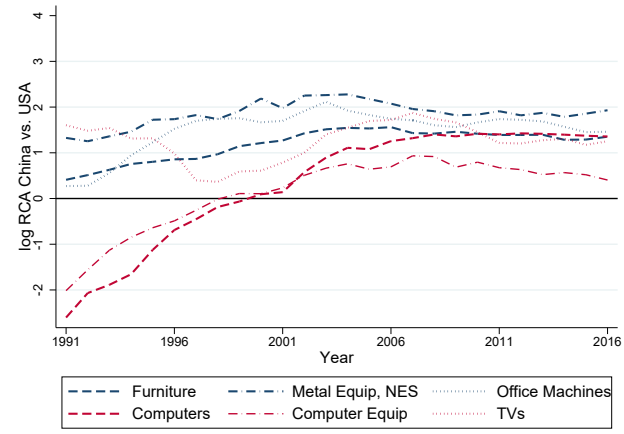
(d) China share of World Exports



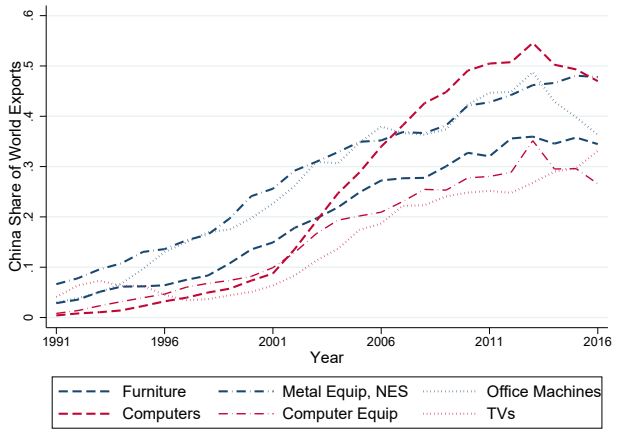
Note: SITC Rev. 2 three-digit categories 037 (prepared or preserved fish, crustaceans, and mollusks), 613 (tanned or dressed furskins), 635 (wood products, NES), and 881 (photographic equipment).

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

(a) China-U.S. Log RCA

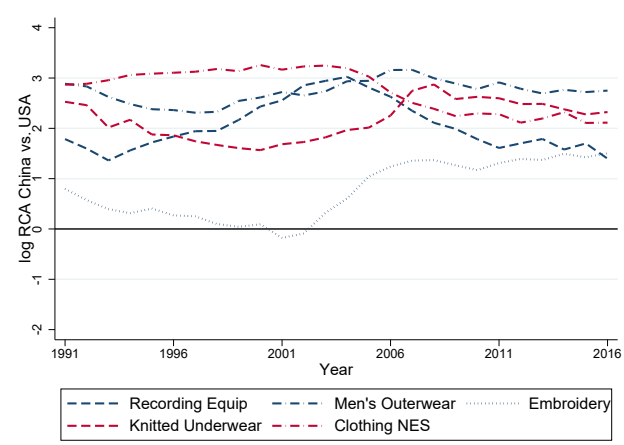


(b) China share of World Exports

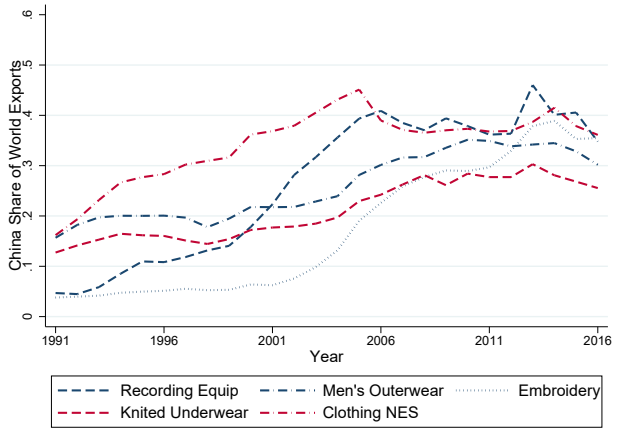


Note: SITC Rev. 2 three-digit categories 697 (metal equipment, NES), 751 (office machines), 752 (computers, laptops), 759 (computer parts and peripheral devices), 761 (televisions), and 821 (furniture).

(c) China-U.S. Log RCA



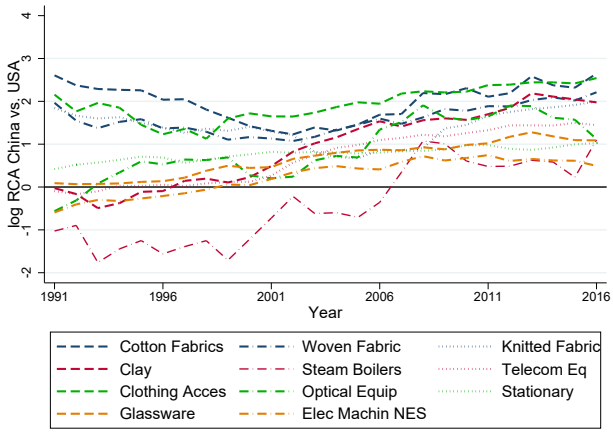
(d) China share of World Exports



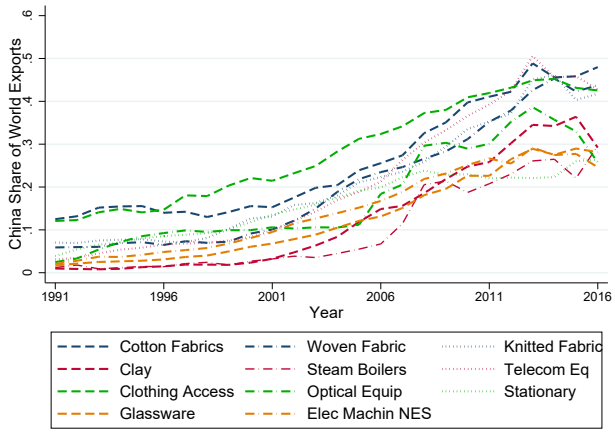
Note: SITC Rev. 2 three-digit categories 656 (embroidery, lace, ribbons), 763 (sound recording devices), 842 (men's and boy's outerwear), 846 (knitted undergarments), 848 (articles of clothing, NES).

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

(a) China-U.S. Log RCA



(b) China share of World Exports

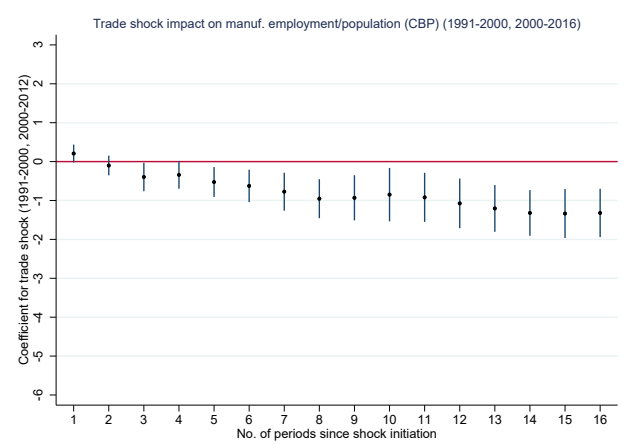


Note: SITC Rev. 2 three-digit categories 652 (cotton fabrics), 653 (woven fabrics, man-made fibers), 655 (knitted fabrics), 662 (clay and refractory construction materials), 665 (glassware), 711 (steam boilers), 764 (telecommunications equipment), 778 (electrical machinery, NES), 847 (clothing accessories), 871 (optical instruments), and 895 (office and stationary supplies).

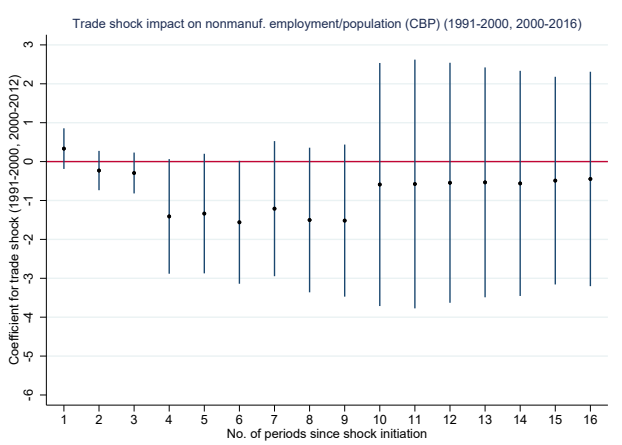
Figure A5: Trade Shock Impact on Employment, Lagged Manufacturing Controls

(trade shock periods: 1991-2000 and 2000-2012)

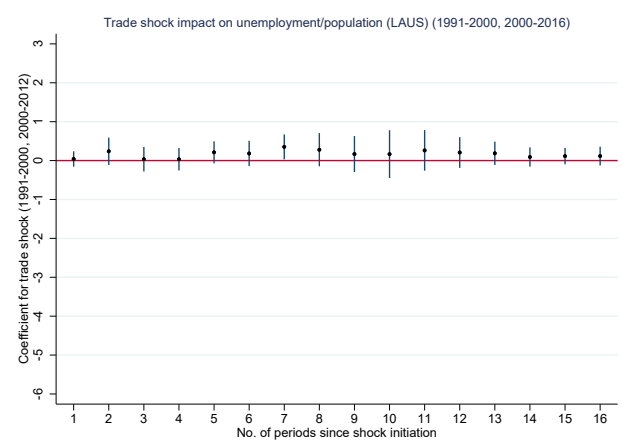
(a) Manuf. emp./Working-age pop.



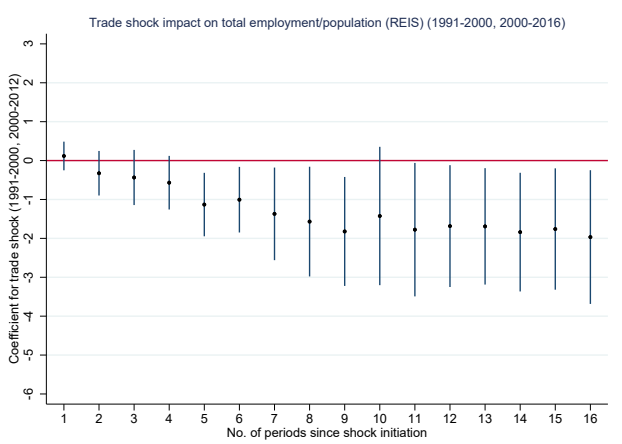
(b) Non-manuf. emp./Working-age pop.



(c) Unemployment/Working-age pop.



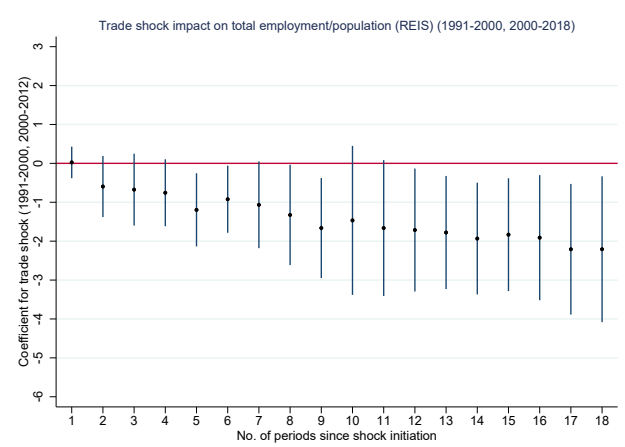
(d) Total emp./Working-age pop.



Note: Panels (a)-(d) report 2SLS stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state. These regressions differ from those in Figure 3 by replacing the initial-period CZ manufacturing employment share (and its interaction with a period dummy) in the control set with the 10-year lag on the CZ manufacturing employment share (and its interaction with a period dummy).

Figure A6: Trade Shock Impact on Employment/Population, Alternative Employment Measures
(trade shock periods: 1991-2000 and 2000-2012)

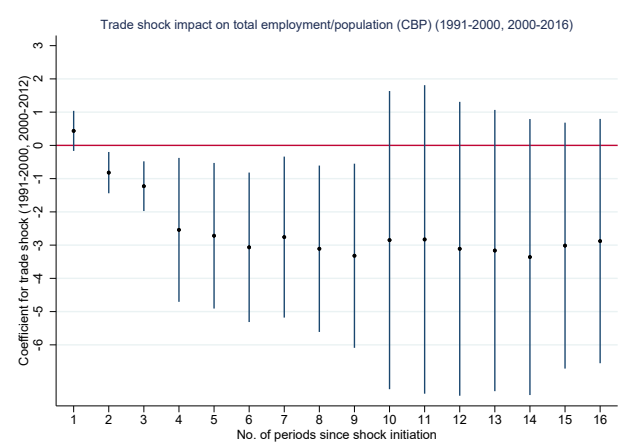
(a) REIS total emp./Working-age pop.



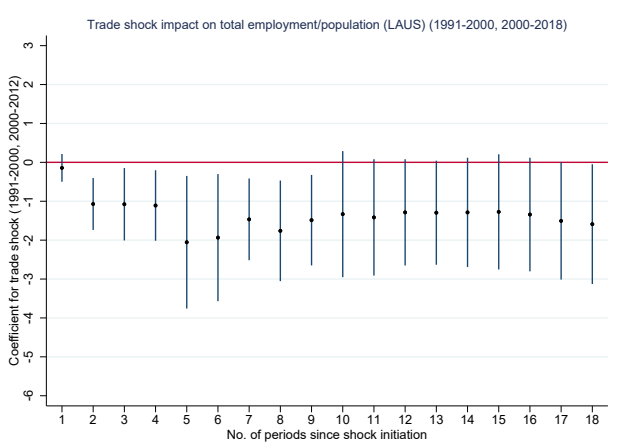
(b) REIS wage & salary emp./Working-age pop.



(c) CBP total emp./Working-age pop.



(d) LAUS total emp./Working-age pop.

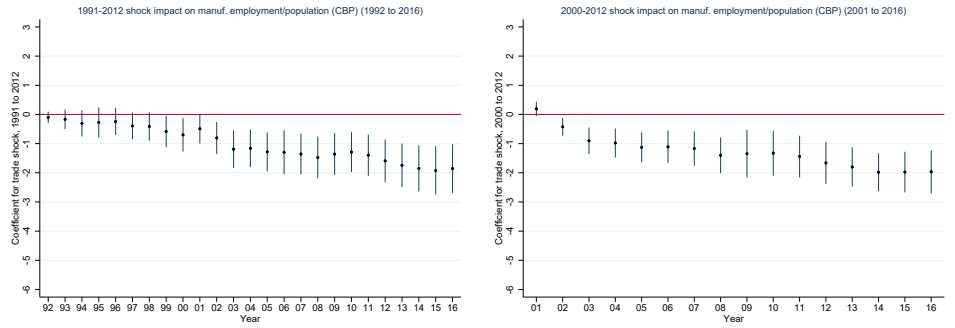


Note: Panels (a)-(d) report 2SLS stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

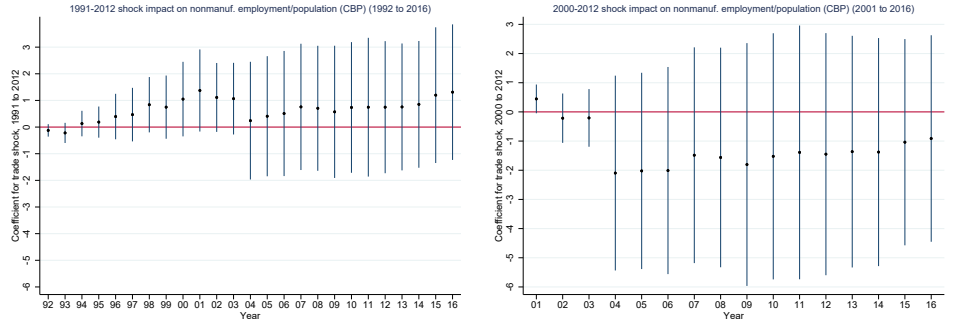
Figure A7: Trade Shock Impact on Employment, Long Differences

(A)1992-2016 (trade shock 1991-2012) (B) 2000-2016 (trade shock 2000-2012)

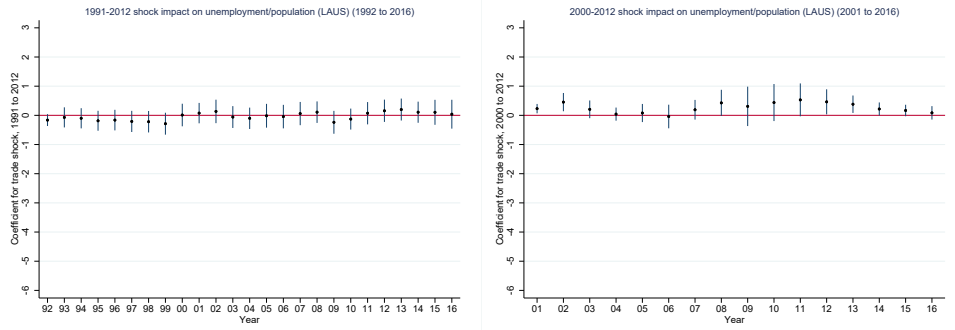
I. Manufacturing employment/Working-age population



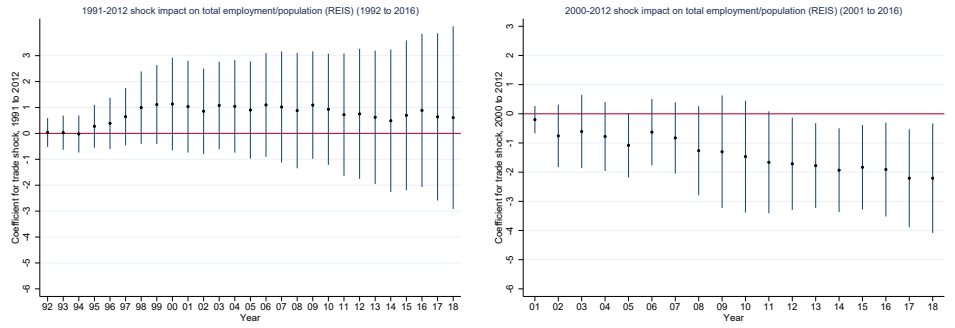
II. Non-manufacturing employment/Working-age population



III. Unemployment/Working-age population



IV. Total employment/Working-age population

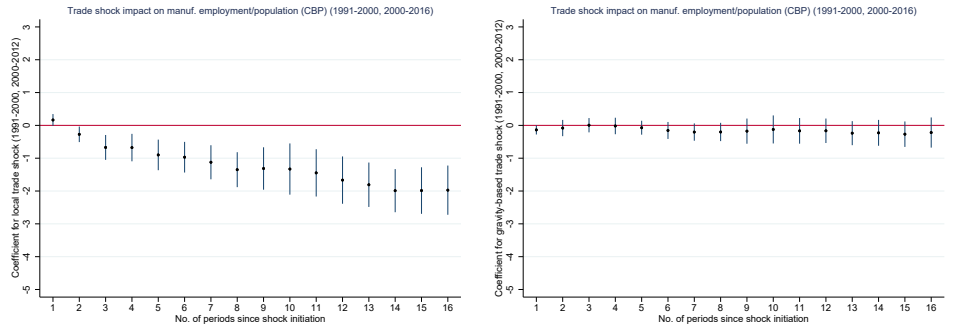


Note: Columns (A) and (B) report 2SLS long-difference estimates of coefficient β_{1h} in equation (2) for outcomes in panels I-IV. The dependent variable is the change in the indicated outcome from the initial period (1991 or 2000) to the end period given on the horizontal axis; the trade shock is the decadalized change in CZ import exposure (for 1991-2012 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, and Census region dummies. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

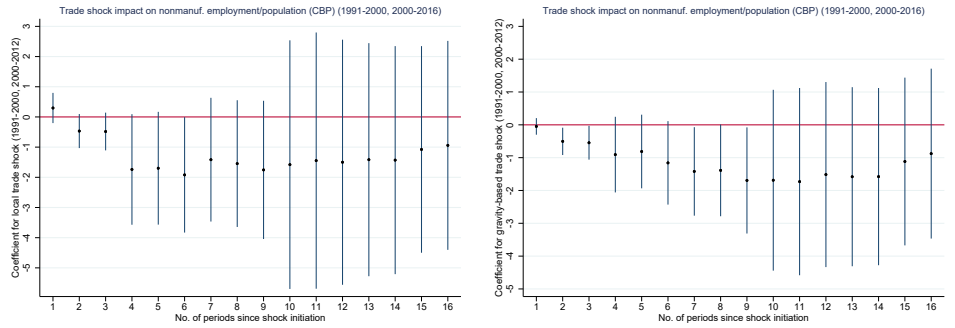
Figure A8: Impacts of Local vs. Gravity-Based Trade Shocks on Employment

(A) Impact of Local Trade Shock (B) Impact of Gravity-Based Trade Shock

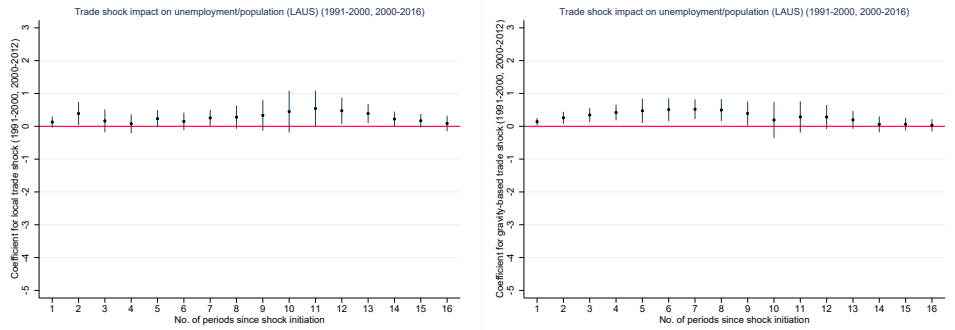
I. Manufacturing employment/Working-age population



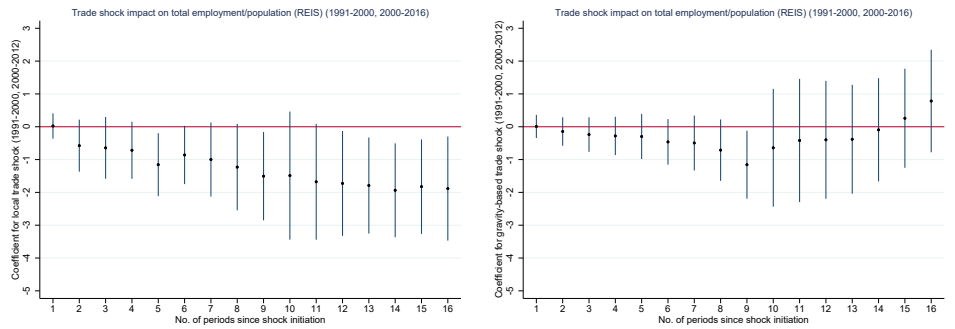
II. Non-manufacturing employment/Working-age population



III. Unemployment/Working-age population



IV. Total employment/Working-age population

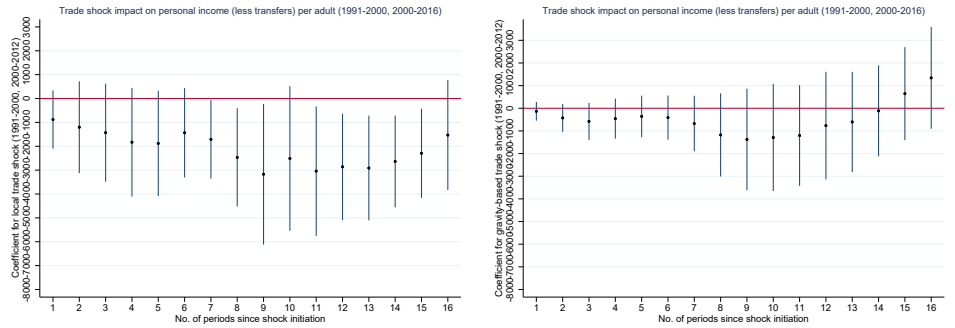


Note: Columns (A)and (B) report 2SLS stacked time-difference estimates of coefficients β_{1h} (on the own-CZ trade shock) and β_{2h} (on the gravity-weighted trade shock) in equation (4) for outcomes in panels I-IV. The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

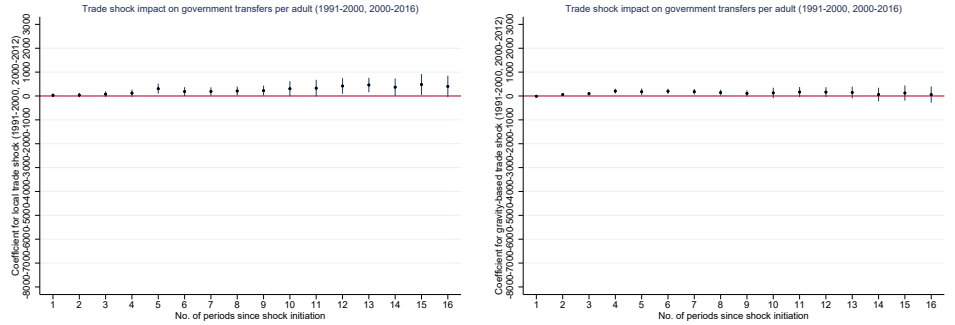
Figure A9: Impacts of Local vs. Gravity-Based Trade Shocks on Income, Earnings, and Transfers

(A) Impact of Local Trade Shock (B) Impact of Gravity-Based Trade Shock

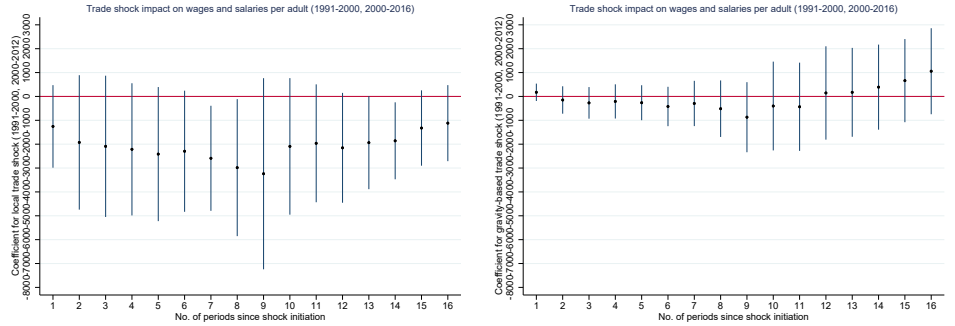
I. Personal income (less government transfers)/Working-age population



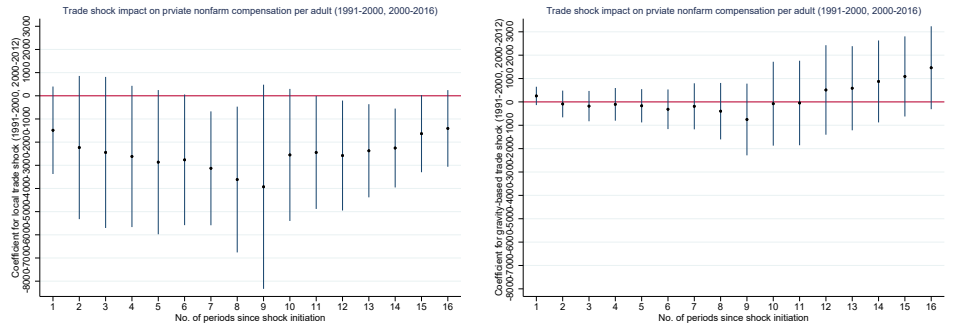
II. Government transfers/Working-age population



III. Wage and salary compensation/Working-age population



IV. Private non-farm compensation/Working-age population

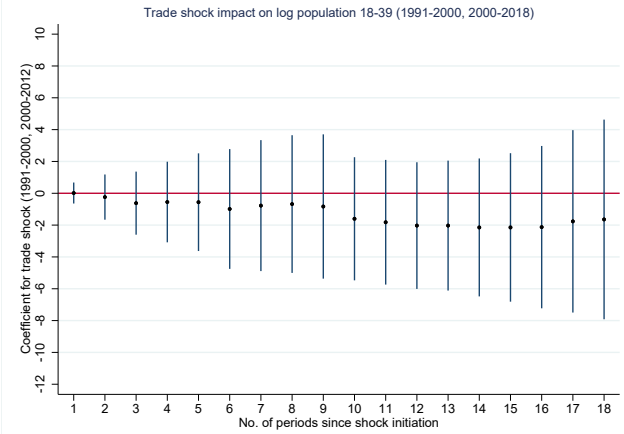


Note: Columns (A)and (B) report 2SLS stacked time-difference estimates of coefficients β_{1h} (on the own-CZ trade shock) and β_{2h} (on the gravity-weighted trade shock) in equation (4) for outcomes in panels I-IV. The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A10: Trade Shock Impact on Log Population Head Counts

(trade shock periods: 1991-2000 and 2000-2012)

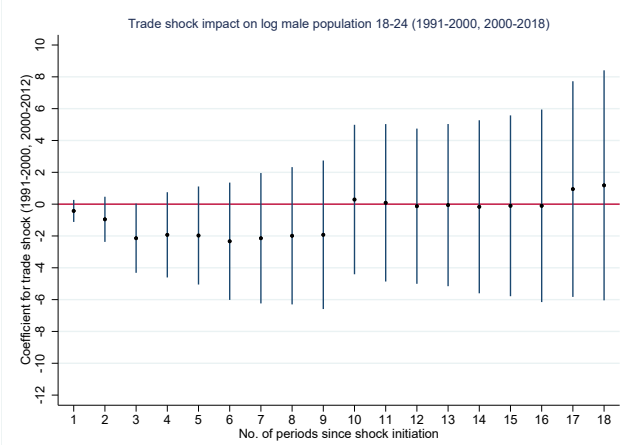
(a) Population, ages 18-39



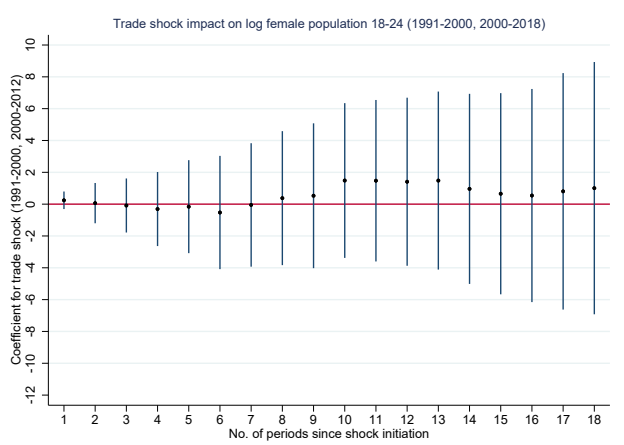
(b) Population, ages 40-64



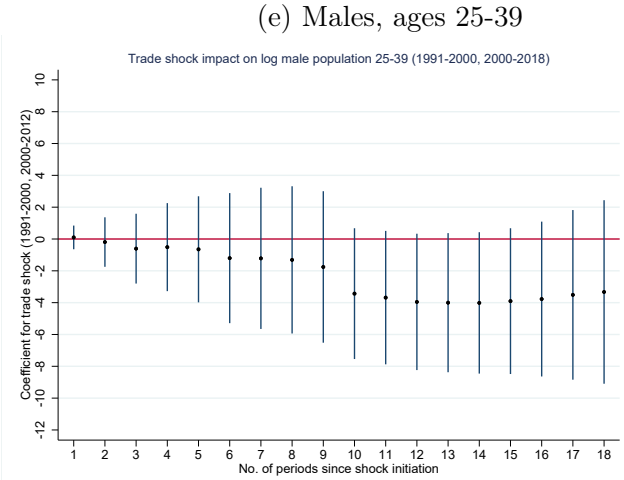
(c) Males, ages 18-24



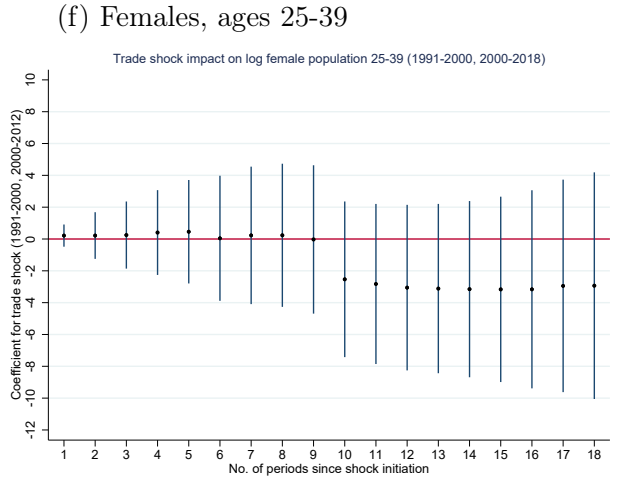
(d) Females, ages 18-24



(e) Males, ages 25-39



(f) Females, ages 25-39

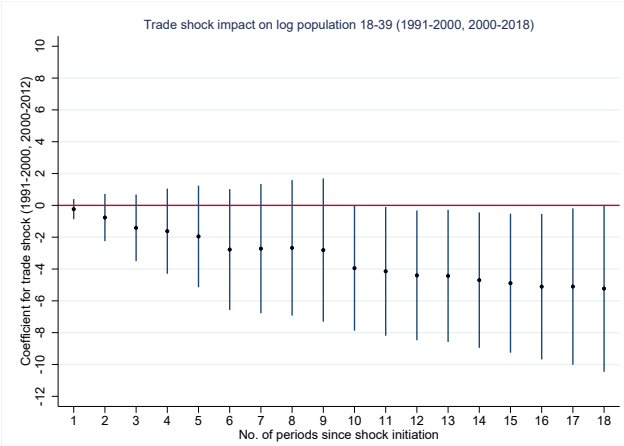


Note: Panels (a)-(f) report 2SLS stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

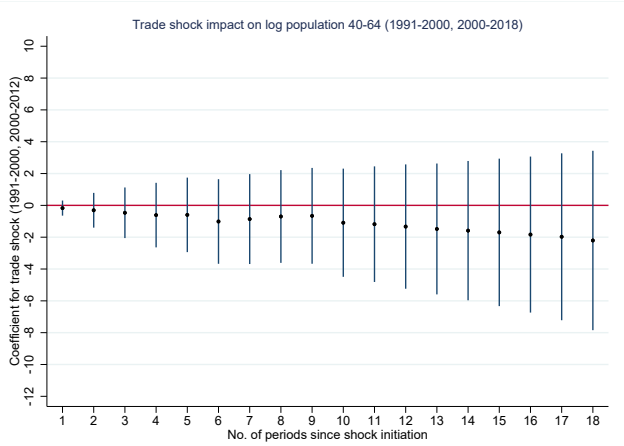
Figure A11: Trade Shock Impact on Log Population, Controlling for Lagged Population Growth

(trade shock periods: 1991-2000 and 2000-2012)

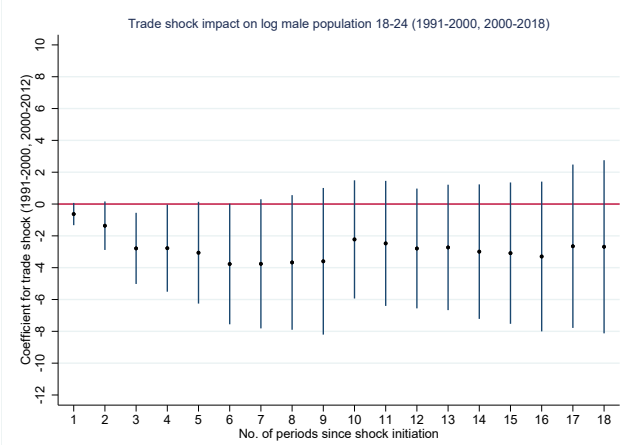
(a) Population, ages 18-39



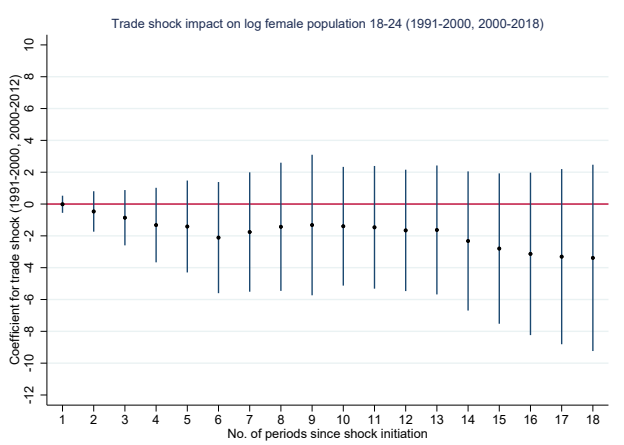
(b) Population, ages 40-64



(c) Males, ages 18-24



(d) Females, ages 18-24



(e) Males, ages 25-39



(f) Females, ages 25-39

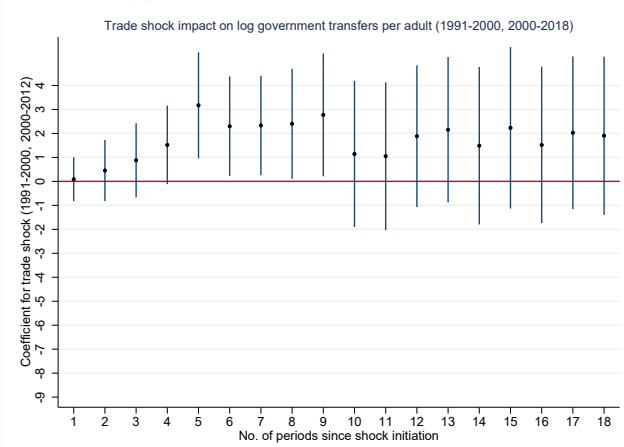


Note: Panels (a)-(f) report 2SLS stacked time-difference estimates of coefficient β_{1h} in (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, a dummy for the second time period, and the 1970-1990 change in CZ log total population and its interaction with a period dummy. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

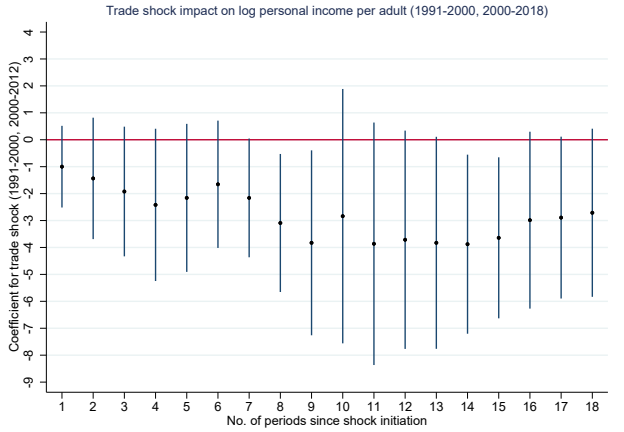
Figure A12: Trade Shock Impact on Log Transfers and Income per Working-Age Adult

(trade shock periods: 1991-2000 and 2000-2012)

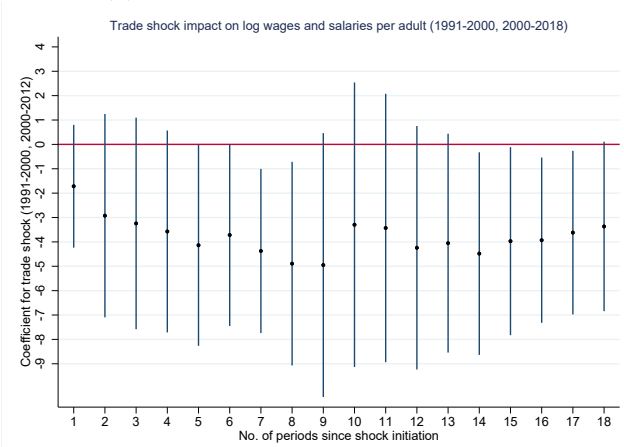
(a) Government transfers



(b) Person income less government transfers



(c) Wage and salary compensation



(d) Private non-farm compensation

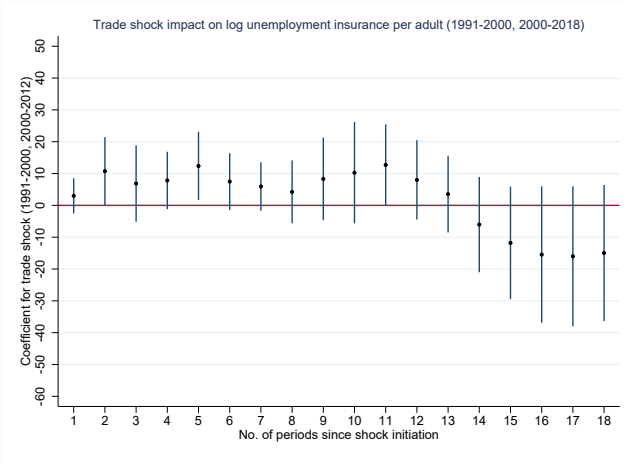


Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A13: Trade Shock Impact on Log Government Transfers by Program per Working-Age Adult

(trade shock periods: 1991-2000 and 2000-2012)

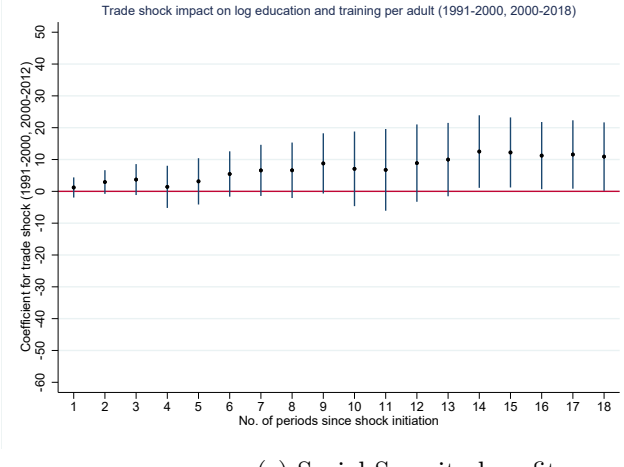
(a) Unemployment Insurance benefits



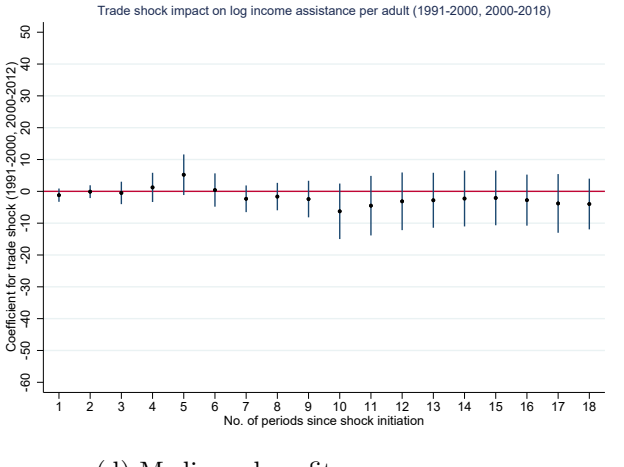
(b) Trade Adjustment Assistance



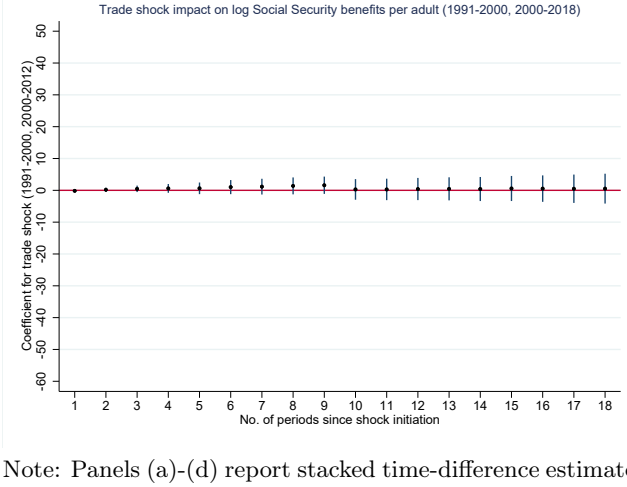
(c) Education and training assistance.



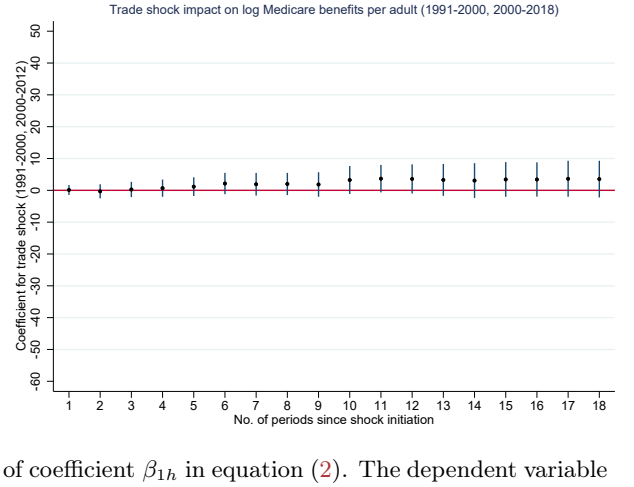
(c) Income assistance



(c) Social Security benefits



(d) Medicare benefits

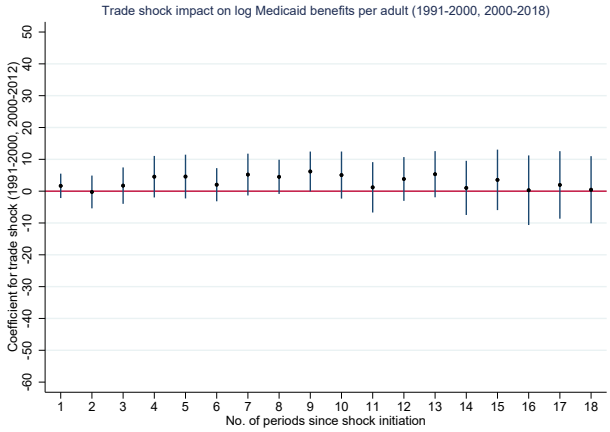


Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

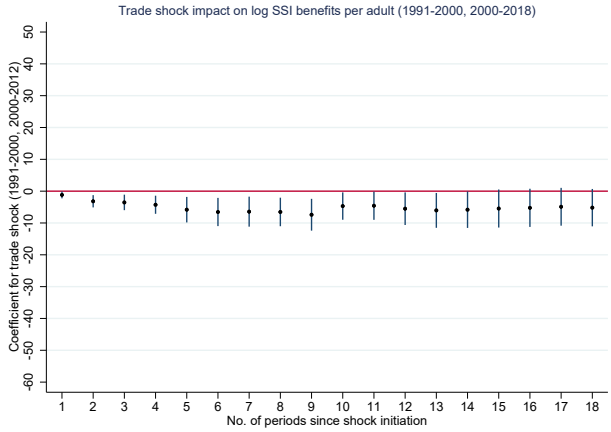
Figure A14: Trade Shock Impact on Log Income Assistance by Program per Working-Age Adult

(trade shock periods: 1991-2000 and 2000-2012)

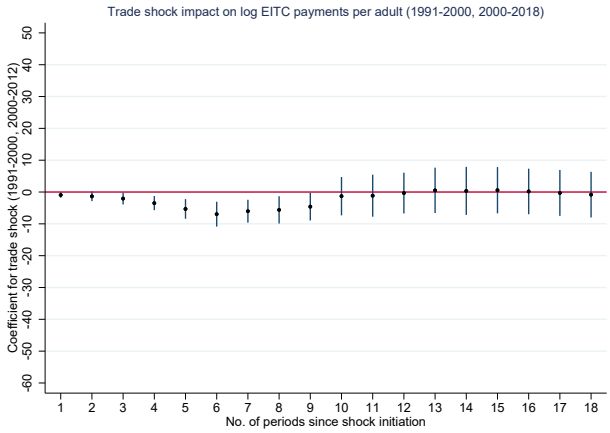
(a) Medicaid Benefits



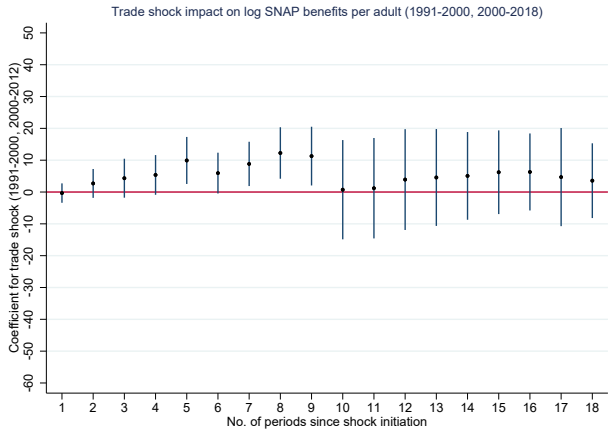
(b) Supplemental Security Income



(c) EITC payments



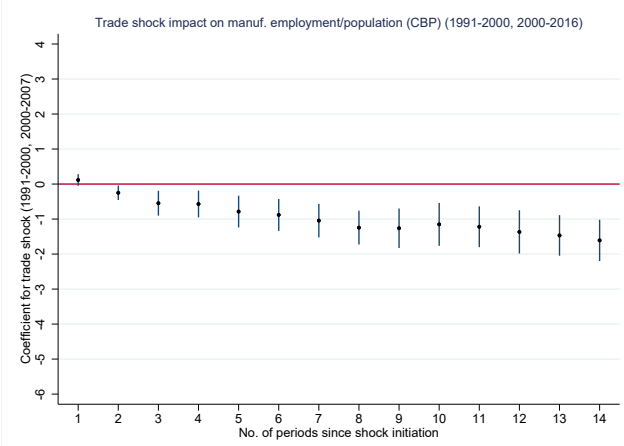
(d) SNAP benefits



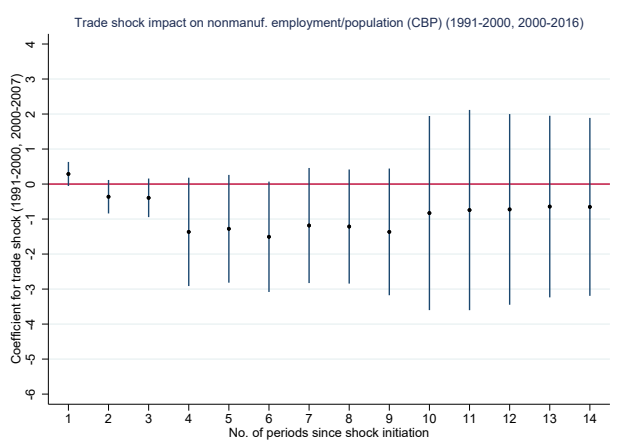
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2012); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A15: Trade Shock Impacts, 1991-2000-2007
(trade shock periods: 1991-2000 and 2000-2007)

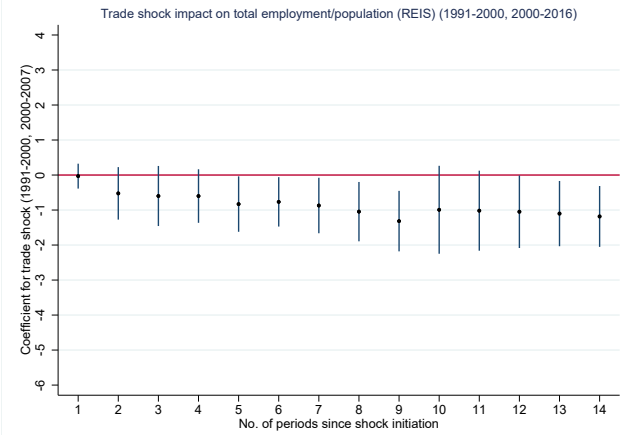
(a) Manuf. emp./Working-age pop.



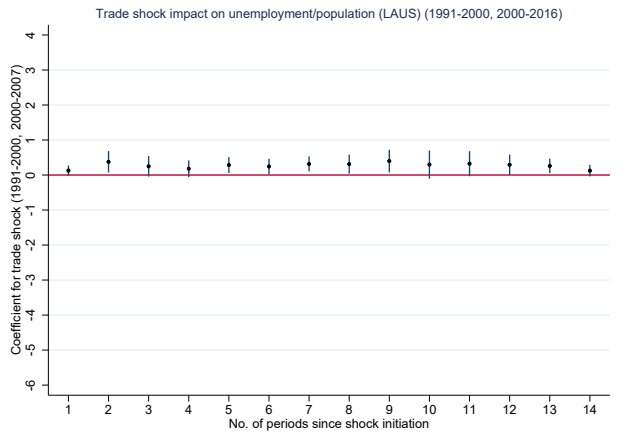
(b) Non-manuf. emp./Working-age pop.



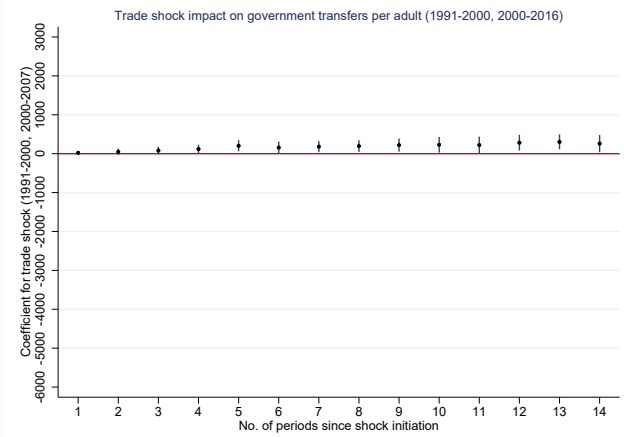
(c) Total emp./Working-age pop.



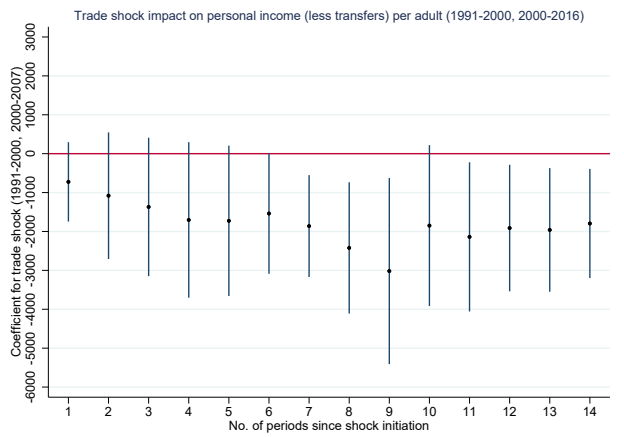
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



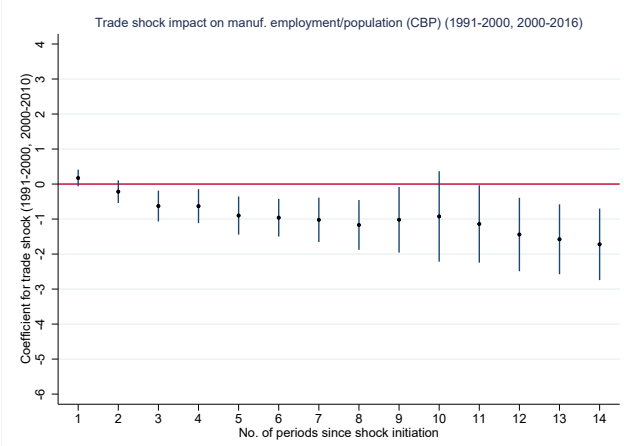
(d) Personal income – transfers/working-age pop.



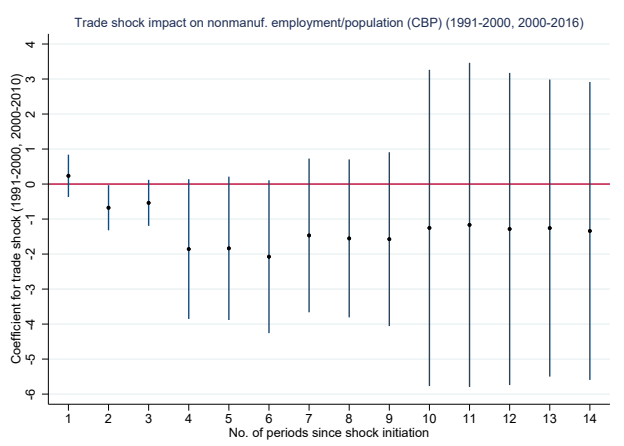
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2007); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A16: Trade Shock Impacts, 1991-2000-2010
(trade shock periods: 1991-2000 and 2000-2010)

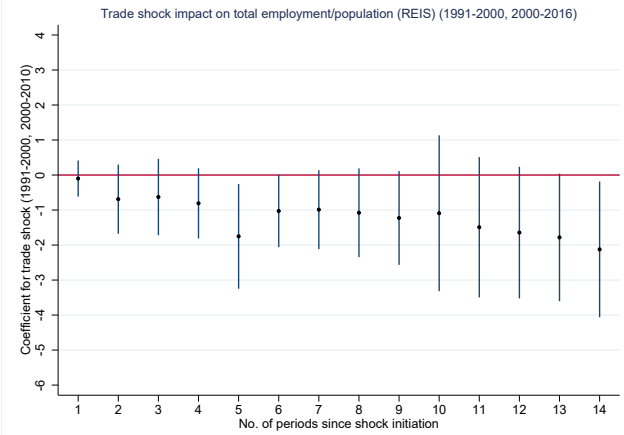
(a) Manuf. emp./Working-age pop.



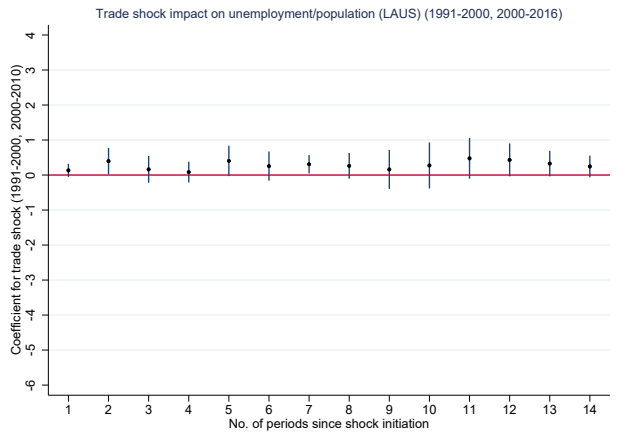
(b) Non-manuf. emp./Working-age pop.



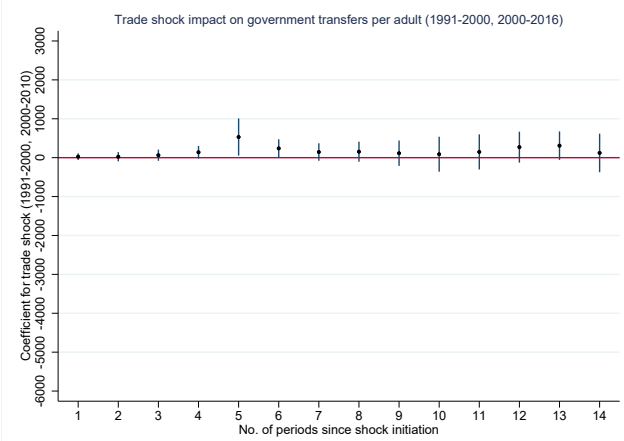
(c) Total emp./Working-age pop.



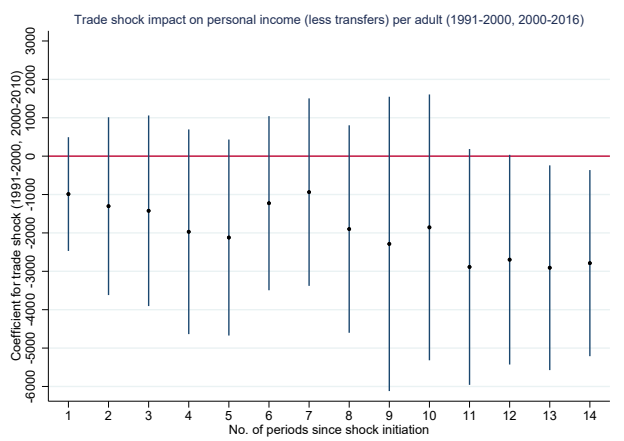
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



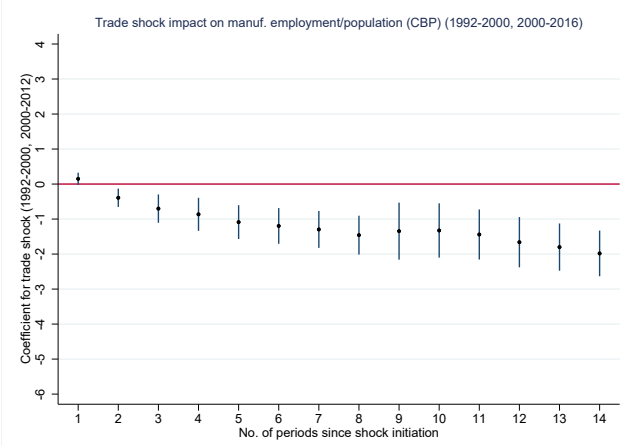
(d) Personal income – transfers/working-age pop.



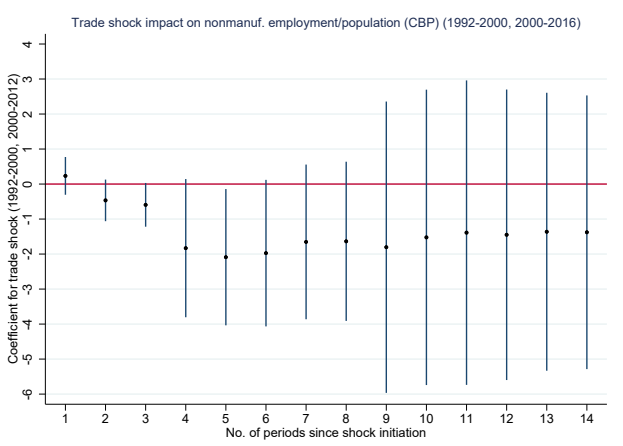
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for initial periods 1991 or 2000; the trade shock is the decadalized change in CZ import exposure (for 1991-2000 or 2000-2010); and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A17: Trade Shock Impacts, 1992-2000-2012
(trade shock periods: 1992-2000 and 2000-2012)

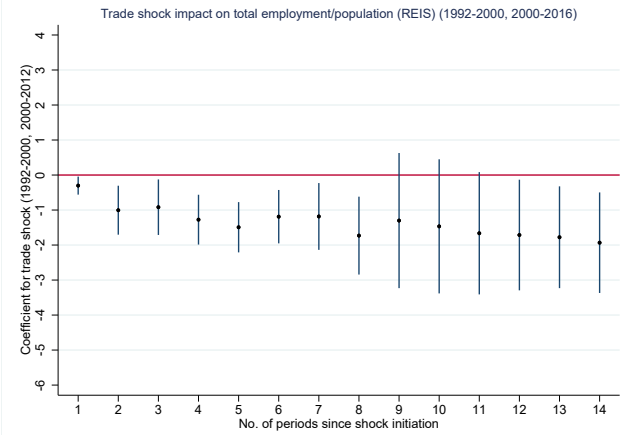
(a) Manuf. emp./Working-age pop.



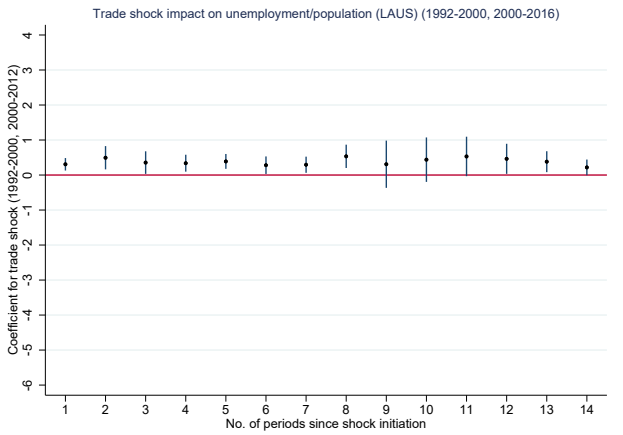
(b) Non-manuf. emp./Working-age pop.



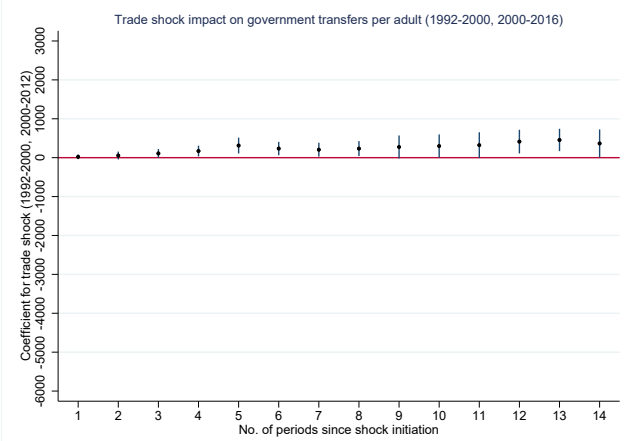
(c) Total emp./Working-age pop.



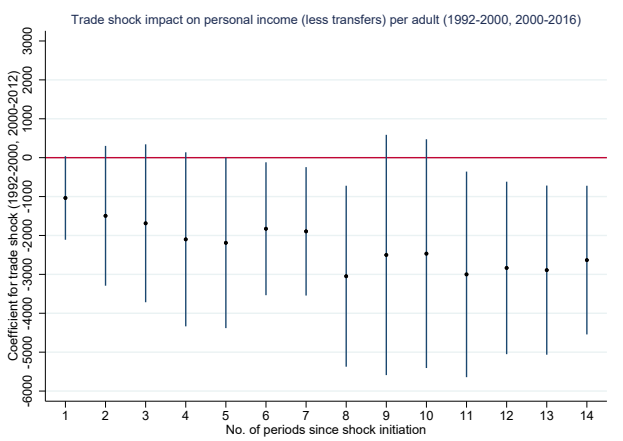
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



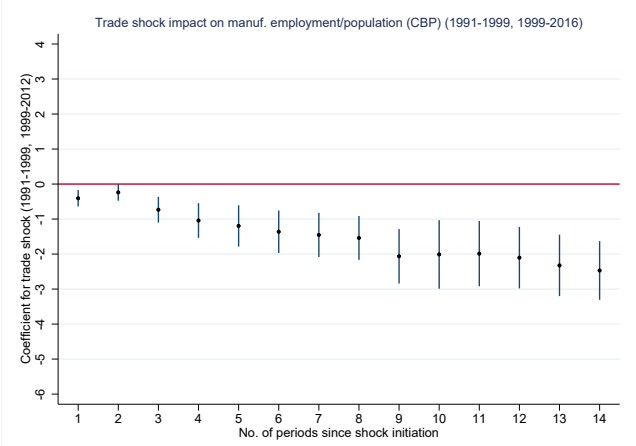
(d) Personal income – transfers/working-age pop.



Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A18: Trade Shock Impacts, 1991-1999-2012
(trade shock periods: 1991-1999 and 1999-2012)

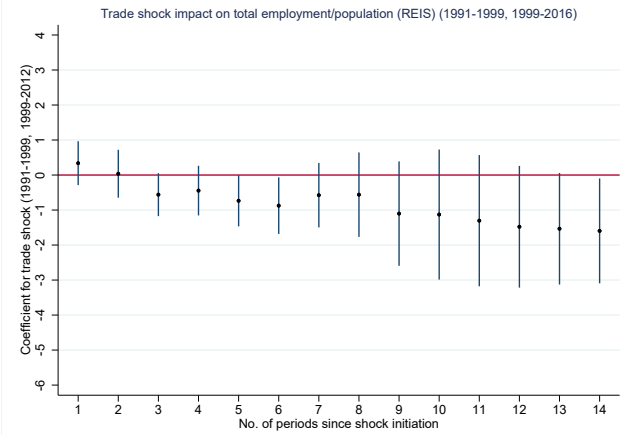
(a) Manuf. emp./Working-age pop.



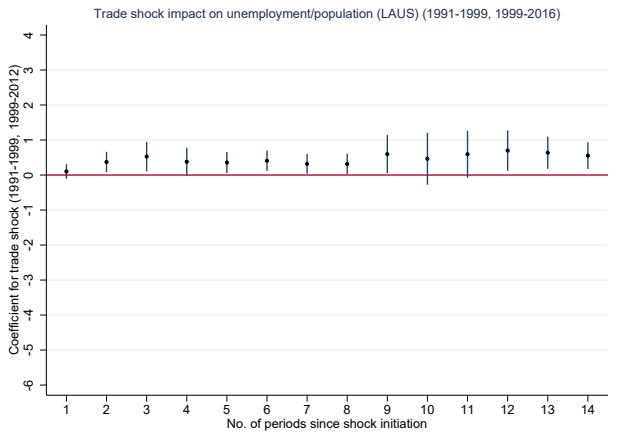
(b) Non-manuf. emp./Working-age pop.



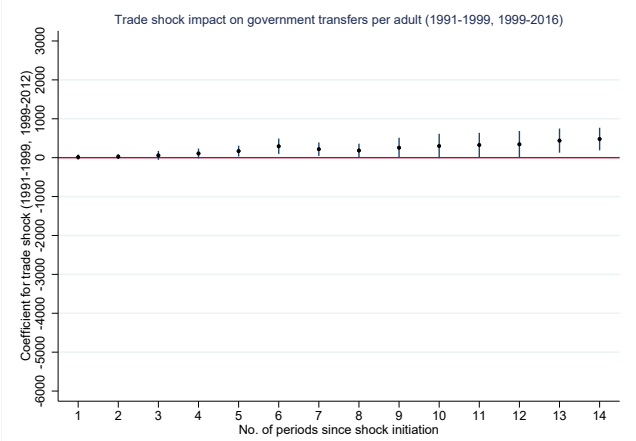
(c) Total emp./Working-age pop.



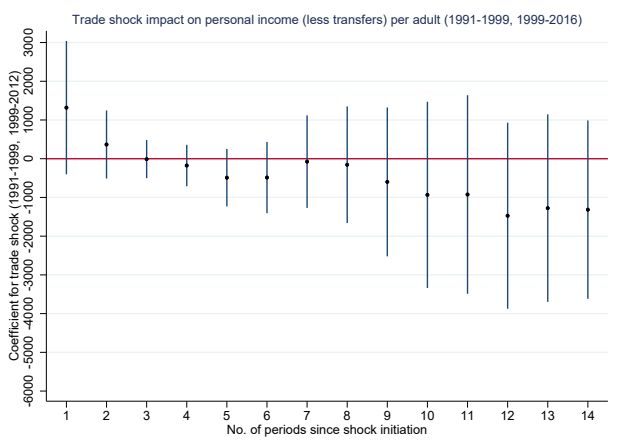
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



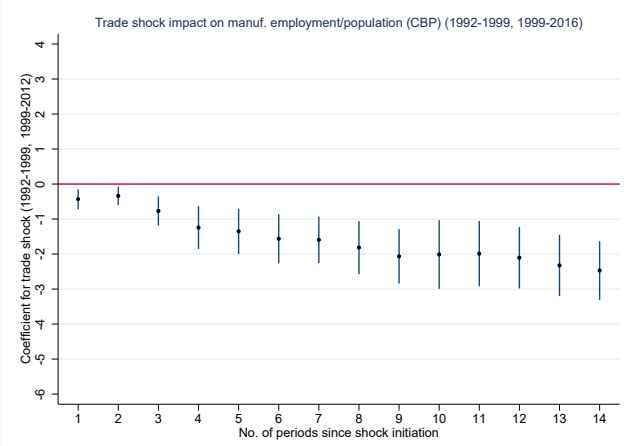
(d) Personal income – transfers/working-age pop.



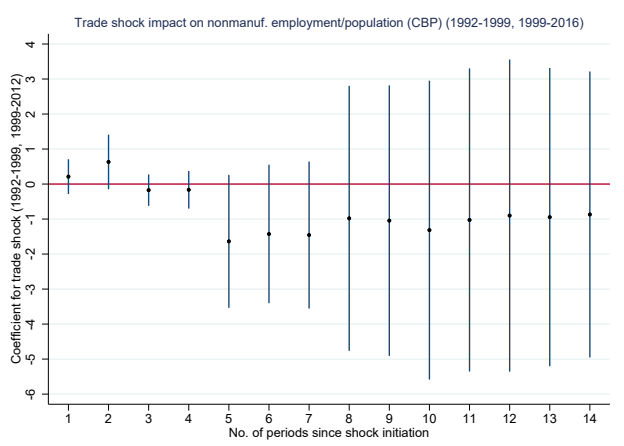
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A19: Trade Shock Impacts, 1992-1999-2012
(trade shock periods: 1992-1999 and 1999-2012)

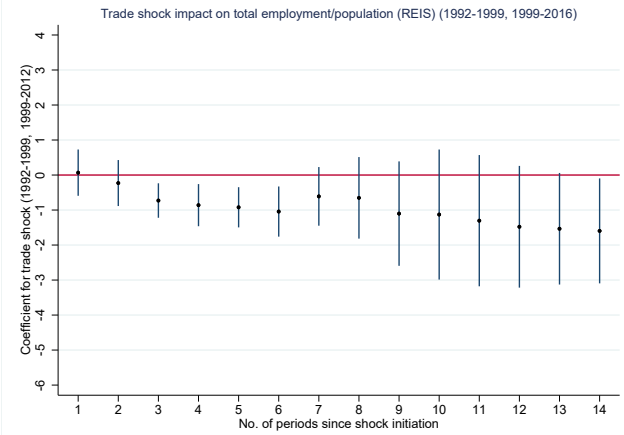
(a) Manuf. emp./Working-age pop.



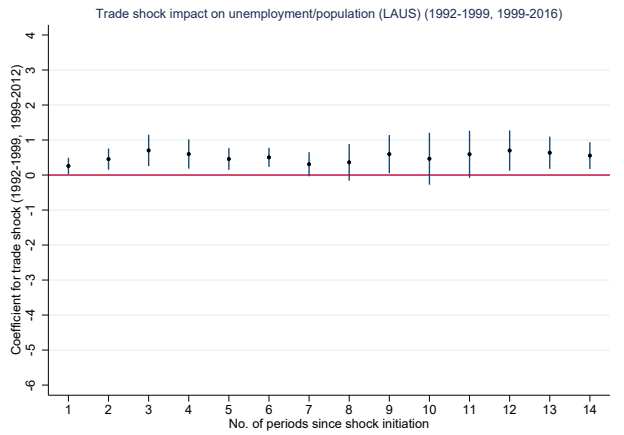
(b) Non-manuf. emp./Working-age pop.



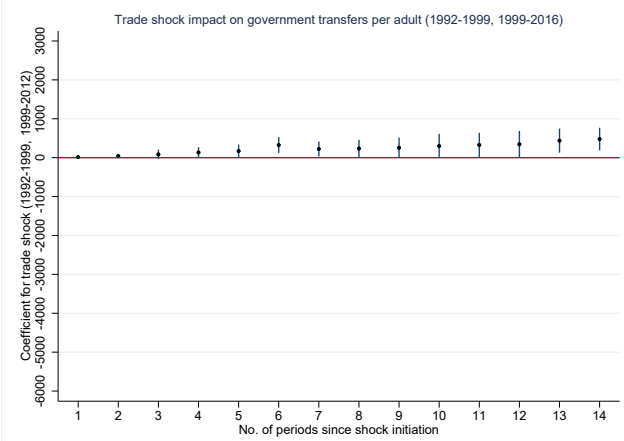
(c) Total emp./Working-age pop.



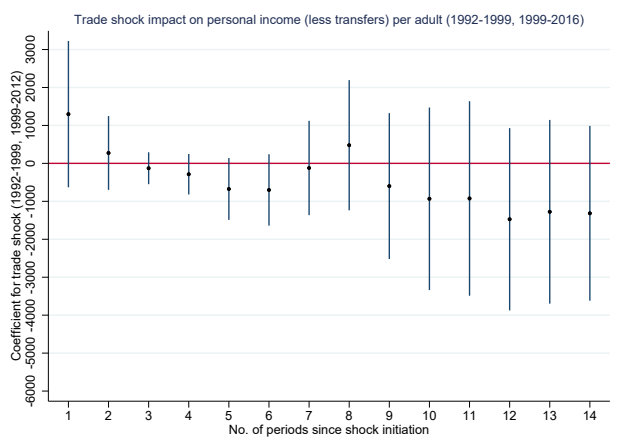
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



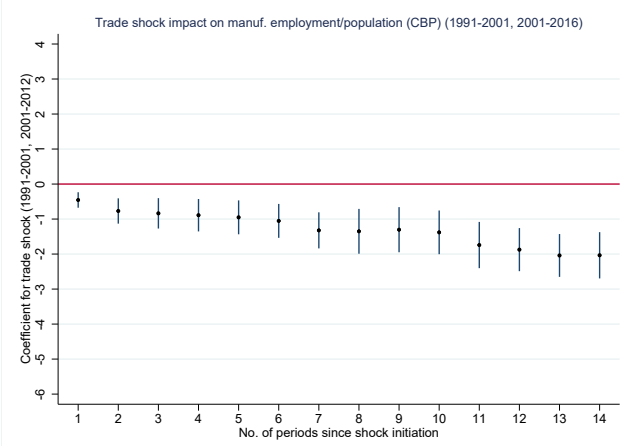
(d) Personal income – transfers/working-age pop.



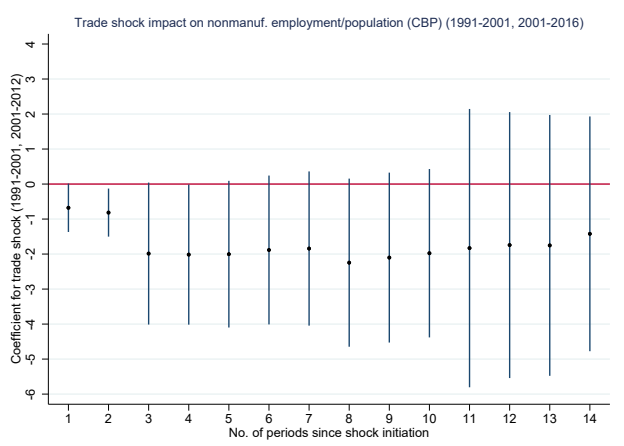
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A20: Trade Shock Impacts, 1991-2001-2012
(trade shock periods: 1991-2001 and 2001-2012)

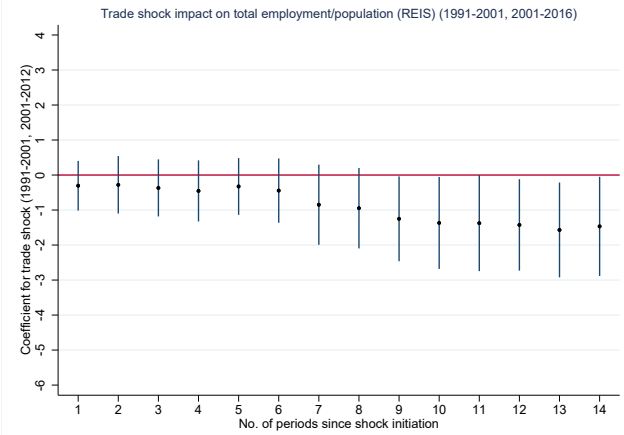
(a) Manuf. emp./Working-age pop.



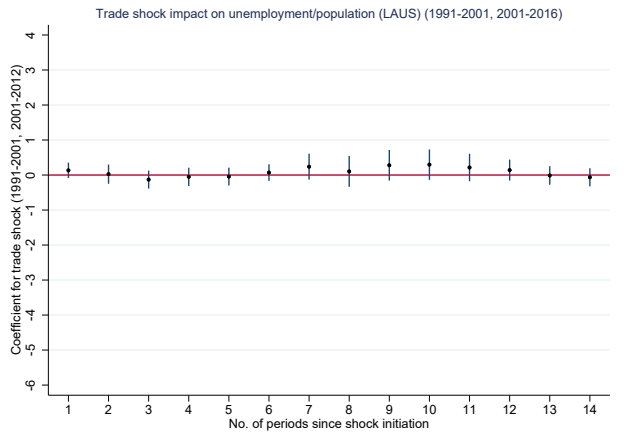
(b) Non-manuf. emp./Working-age pop.



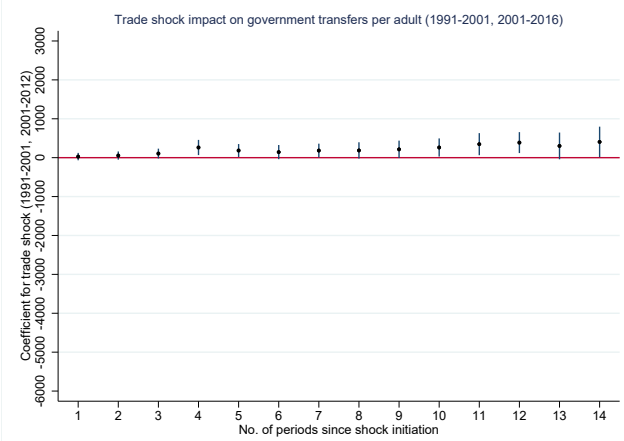
(c) Total emp./Working-age pop.



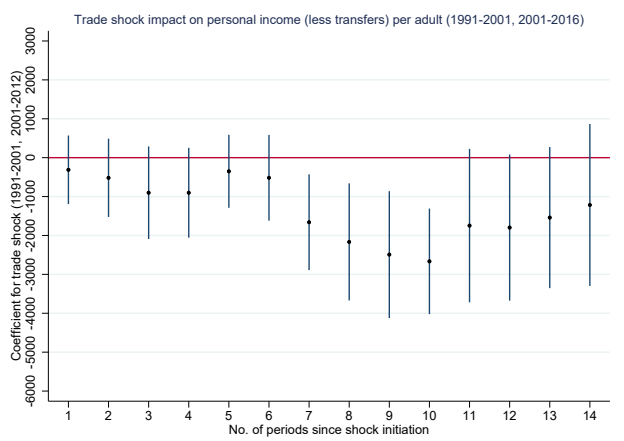
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



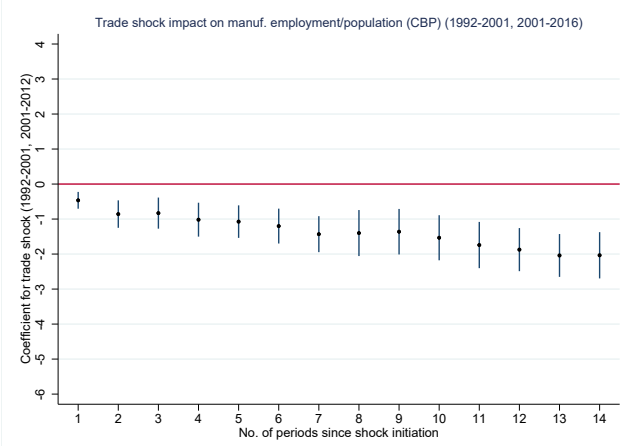
(d) Personal income – transfers/working-age pop.



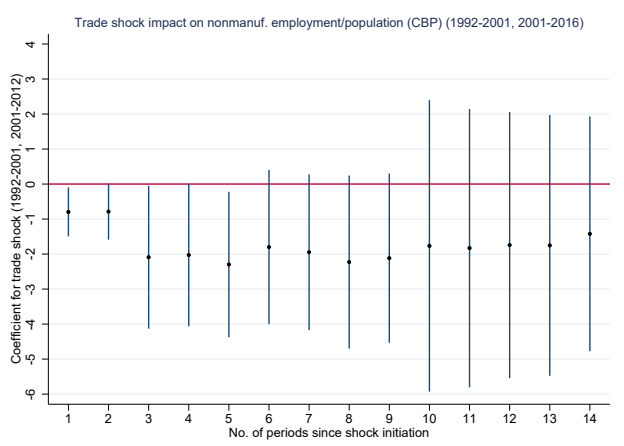
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A21: Trade Shock Impacts, 1992-2001-2012
(trade shock periods: 1992-2002 and 2002-2012)

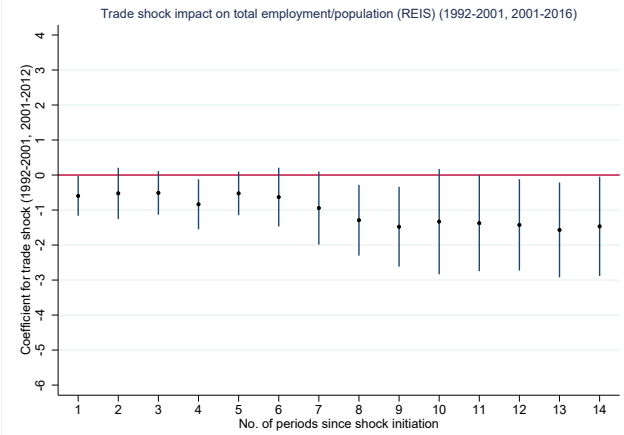
(a) Manuf. emp./Working-age pop.



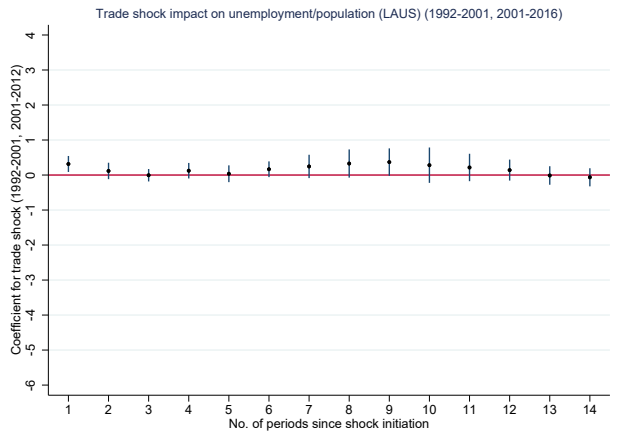
(b) Non-manuf. emp./Working-age pop.



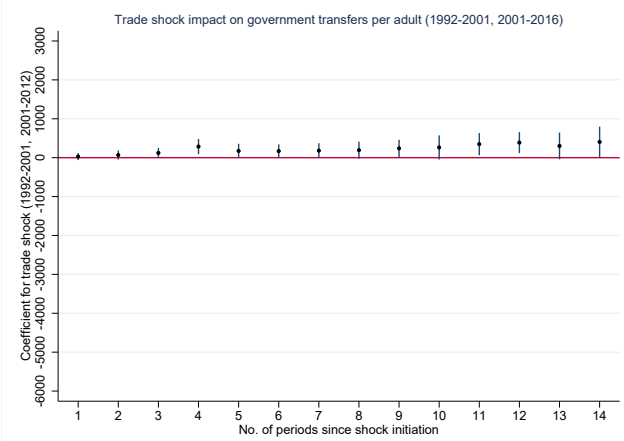
(c) Total emp./Working-age pop.



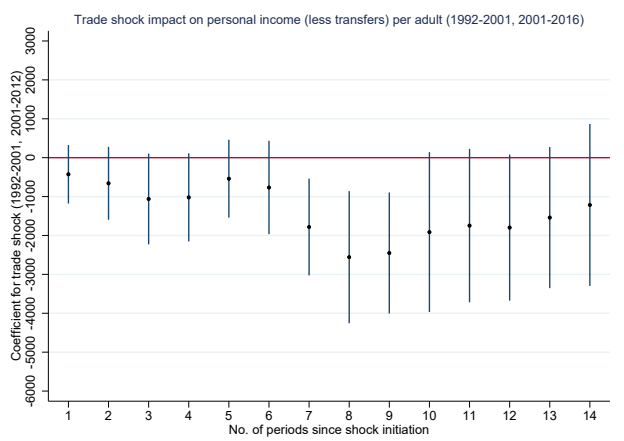
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



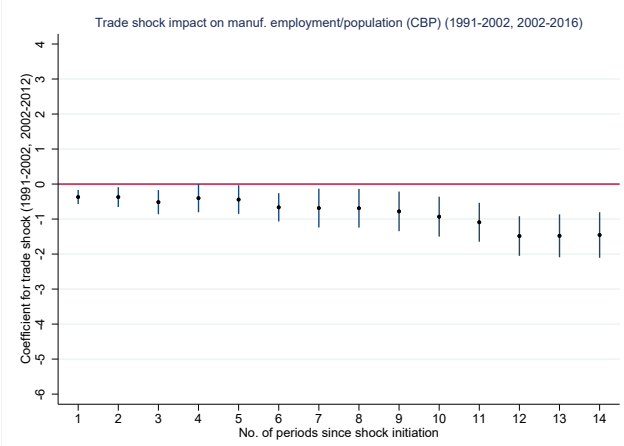
(d) Personal income – transfers/working-age pop.



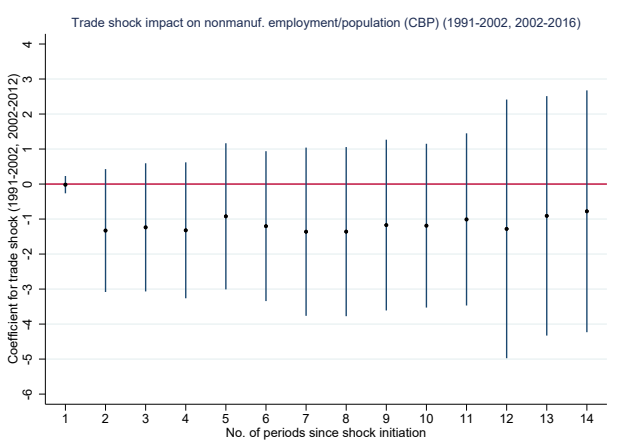
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A22: Trade Shock Impacts, 1991-2002-2012
(trade shock periods: 1991-2002 and 2002-2012)

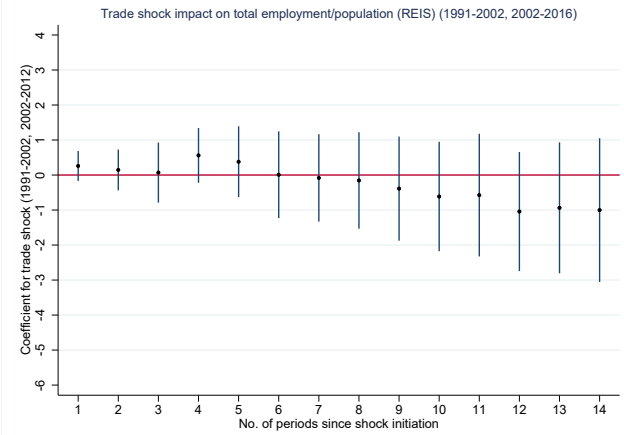
(a) Manuf. emp./Working-age pop.



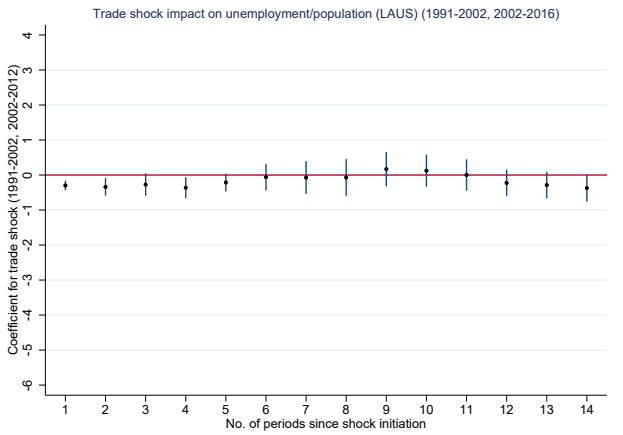
(b) Non-manuf. emp./Working-age pop.



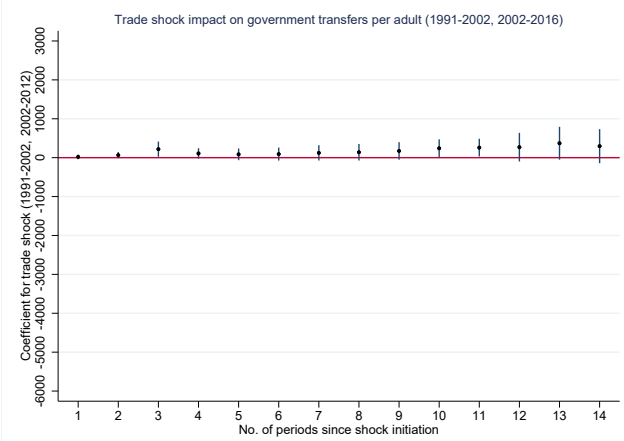
(c) Total emp./Working-age pop.



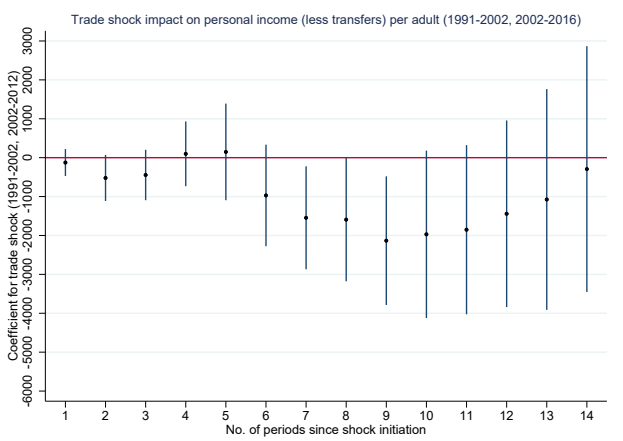
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



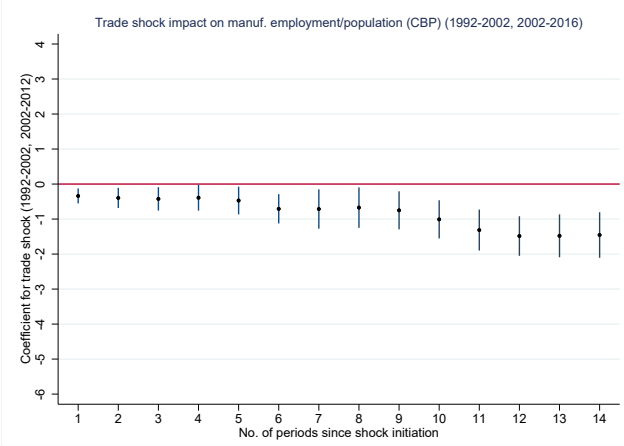
(d) Personal income – transfers/working-age pop.



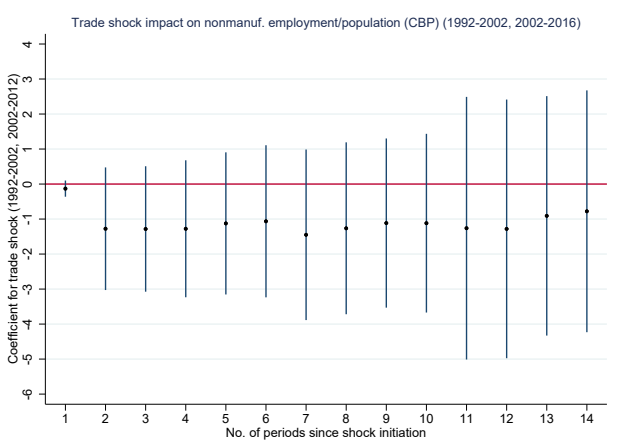
Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.

Figure A23: Trade Shock Impacts, 1992-2002-2012
(trade shock periods: 1992-2002 and 2002-2010)

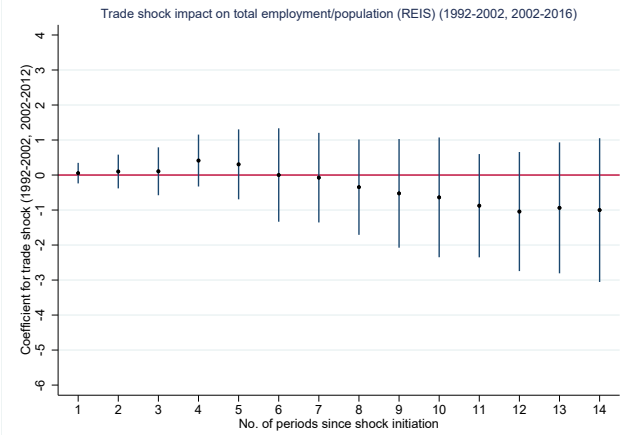
(a) Manuf. emp./Working-age pop.



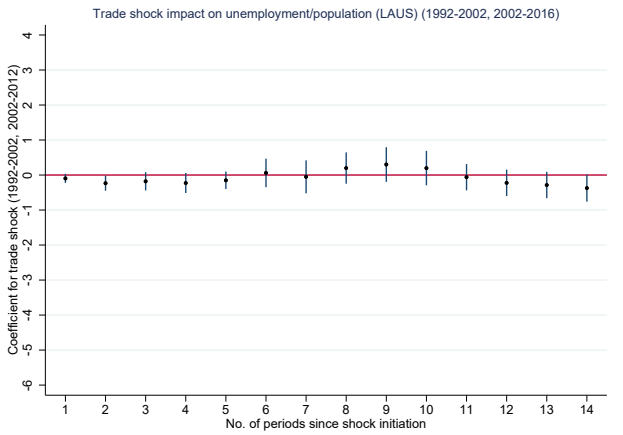
(b) Non-manuf. emp./Working-age pop.



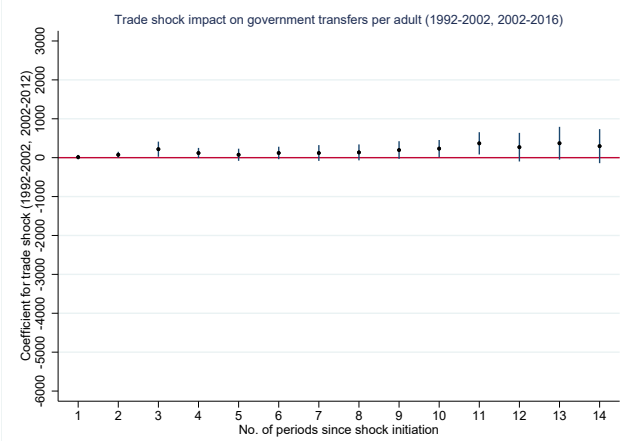
(c) Total emp./Working-age pop.



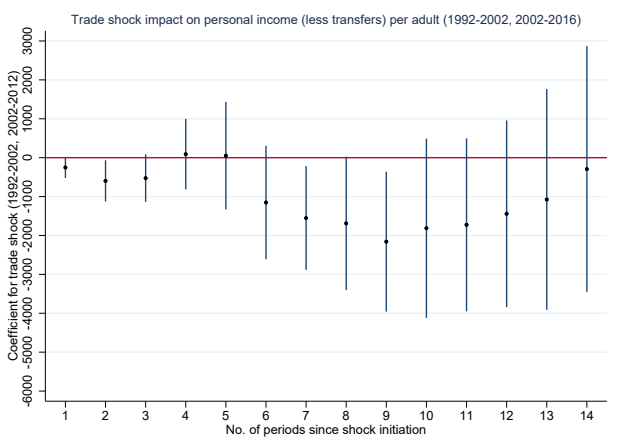
(d) Unemployment/Working-age pop.



(c) Gov't transfers/working-age pop.



(d) Personal income – transfers/working-age pop.



Note: Panels (a)-(d) report stacked time-difference estimates of coefficient β_{1h} in equation (2). The dependent variable is the change in the indicated outcome over the time span on the horizontal axis for the given initial periods; the trade shock is the given decadalized changes in CZ import exposure; and control variables include CZ employment and demographic conditions in the initial period, Census region dummies, and a dummy for the second time period. Regressions are weighted by the CZ share of the U.S. mainland population in the initial year; standard errors are clustered by state.