Appendix

A.1 Trade Data

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

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

A.2 Employment and Earnings Data

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

In much of our analysis, we evaluate employment and earnings outcomes based on data from the Bureau of Economic Analysis (BEA) Regional Economic Information System (REIS). REIS data on employment at the county level are primarily based on the comprehensive quarterly tabulations of unemployment insurance contribution reports that the Bureau of Labor Statistics uses to construct the Quarterly Census of Employment and Wages (QCEW). The BEA also uses supplementary data sources to additionally account for employment in industries that are not fully covered by unemployment insurance, and thus achieves slightly more comprehensive coverage of employment than the QCEW. The REIS data is also more comprehensive than employment counts from the County Business Patterns (CBP), which is an annual extension of the Census Bureau's quinquennial economic censuses that covers the private non-farm sector (see https://www.bea.gov/help/faq/104).[55]

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

Autor et al. (2013a) instead measure changes in employment rates based on data from the decennial population Census and the American Community Survey (ACS). These household survey-based data enumerate the total number of individuals who have a job rather than the total number of jobs. Since Census data are available only at decennial frequency while annual ACS samples are relatively small, the Census/ACS data are not well suited for the data analysis at annual frequency that we conduct here, although we show Census/ACS-based results for selected time periods in Figure (A5). For that analysis, the employment rate is based on civilian working-age individuals

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

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

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

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

As in previous work (Autor et al., 2013a), we use SIC industries to define the aggregate trade shock facing CZs in equation (1). By construction, only SIC manufacturing industries are exposed to competition though imports of Chinese manufactures. Our outcome measure for aggregate manufacturing employment based on REIS data is instead based on NAICS industries. Because the transition from the SIC to the NAICS moved just five of 459 SIC industries from manufacturing to non-manufacturing, this difference does not matter materially in the estimation (which we verify in unreported robustness exercises). The affected industries are SIC 2411 (logging), SIC 2711 (newspapers), SIC 2721 (periodicals), SIC 2731 (book publishing), and SIC 2741 (miscellaneous publishing). None of these are ones in which import competition from China grew substantially.

A.3 Summary Statistics on Outcomes and Controls

Table A1: Summary Statistics for CZ Outcome Variables

Outcome variable	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
2001 to 2019 change in:					
Manufacturing employment/population 18-64	-2.68	1.76	-3.79	-2.66	-1.62
Non-manufacturing employment/population 18-64	2.87	4.17	0.37	2.86	5.49
Wage and salary employment/population 18-64	0.19	4.29	-2.81	0.27	1.79
2000 to 2019 change in:					
Wage and salary employment/population 18-64	-1.00	4.67	-4.21	-1.01	1.27
Log population 18-64	12.92	13.98	3.45	10.24	21.33
Log population 18-24	9.05	14.08	0.04	6.28	18.98
Log population 25-39	6.41	15.48	-4.84	4.91	16.07
Log population 40-64	18.73	15.25	9.79	16.10	26.85
Log personal income/total population	26.95	8.07	21.31	26.09	33.49
Log government transfers/total population	61.85	8.70	56.70	62.67	67.59
Log wages, salaries, benefits/total population	17.38	10.40	10.42	17.02	24.09
Government transfers/total population	4,071.74	760.65	3,530.81	4,039.76	4,451.07
Social Security benefits/total population	1,112.62	309.34	886.72	1,056.72	1,310.32
Medicare benefits/total population	1,259.07	276.95	1,086.54	1,204.63	1,425.66
Medicaid benefits/total population	903.43	450.41	534.54	883.83	1,222.22
Income assistance/total population	266.73	102.74	210.35	258.72	332.41

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

Table A2: Summary Statistics for CZ Control Variables

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

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

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

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

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

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

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

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

A.4 Causal Identification and Inference

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

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

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

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

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

In Appendix Figure A1, we report falsification tests in which we regress the changes in outcomes whose end year is 1991, just prior to the onset of the China trade shock, and whose initial year ranges from 1975 to 1990 on the trade shock in (1), defined over the period 1991 to 2000. (Results are very similar when we use the 1991 to 2012 trade shock, instead.) This allows us to examine whether future trade shocks are correlated with pre-China shock changes in labor-market conditions. Control variables include Census region dummies and the CZ share of employment in manufacturing in 1970. For the manufacturing employment-working-age population ratio in Figure A1a, there is near zero correlation with the trade shock for changes in outcomes over the 1985 to 1991 horizon. As we extend the outcome period further back in time, a slight positive correlation emerges between the future trade shock and past changes in manufacturing employment, similar to that reported in Autor et al. (2013a). This indicates that CZs subject to larger increases in import competition after 1991 had *faster* manufacturing employment growth in preceding decades. There is no evidence of negative pre-trends in manufacturing employment growth in more trade-exposed CZs. A similar pattern emerges when we examine the overall employment-to-population ratio in Figure A1c, and personal income per capita in Figure A1e. For non-manufacturing employment in Figure A1b, the log change in the working-age population in Figure A1d, and government transfers per capita in Figure A1f, there is near zero correlation between the future trade shock and past changes at all time horizons. (The regression for the log change in population head counts includes lagged CZ population growth as a control.) We interpret these results as evidence against the existence of negative pre-trends in labor-market conditions for commuting zones subject to the China trade shock.

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

In Appendix Figure A2, we replicate the specifications in Figure 5, using the Borusyak et al. (2020) procedure for constructing standard errors. To aid in comparing the estimates, we report the Figure 5 estimates side-by-side with Borusyak et al. (2020) based estimates. (Here, we use the CZ manufacturing employment share in 2000 as a control. Results are similar when using the CZ manufacturing employment share in 1990, as suggested by Borusyak et al. (2020).) By construction, the two methods yield identical coefficient estimates and only differ in how they calculate standard errors. For manufacturing employment (Figure A2a) and non-manufacturing employment (Figure A2b), standard errors are slightly larger when using the Borusyak et al. (2020) method; for wage and salary employment, standard errors are modestly small when applying Borusyak et al. (2020). In the results that follow, we continue to use standard errors clustered by state.

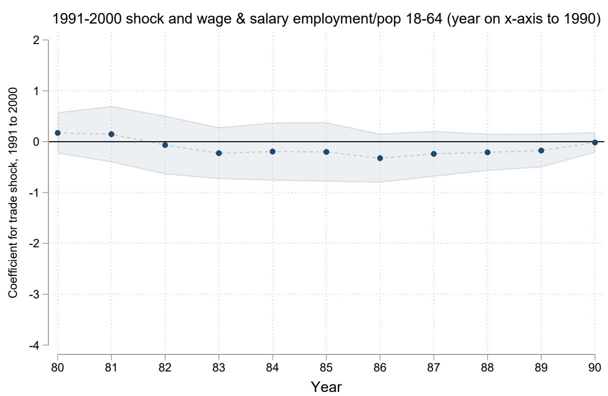
In finite samples, a question arises whether approaches based on asymptotic theory, such Borusyak et al. (2018) and Adao et al. (2019b), yield results that are more reliable than a simple cluster robust variance estimator. Ferman (2019) develops a simulation approach to assess this question, which he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

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

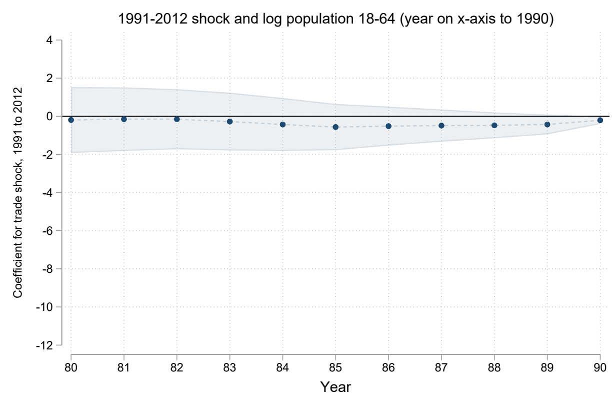
(a) Manuf. emp./Working-age pop.

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

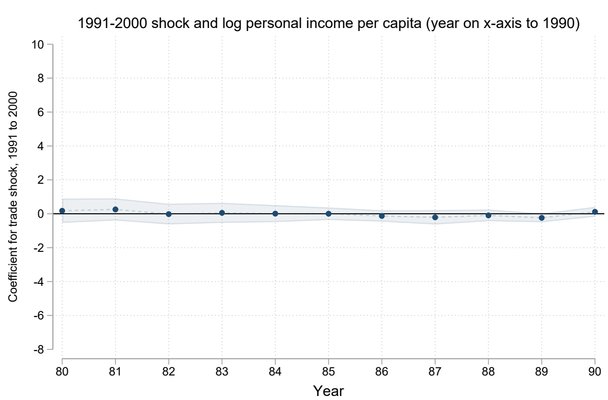
(c) Total wage and salary emp./Working-age pop.



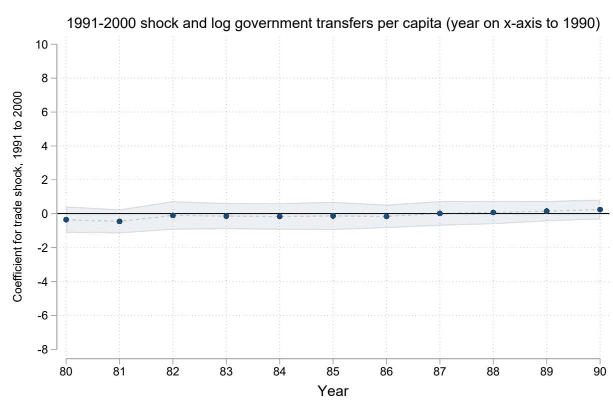
(d) Log working-age population



(e) Log personal income per capita



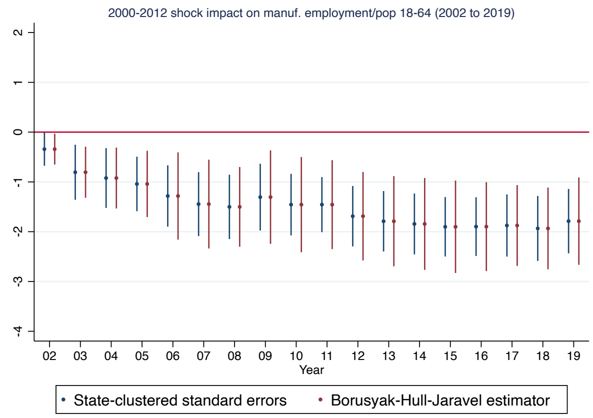
(f) Log government transfers per capita



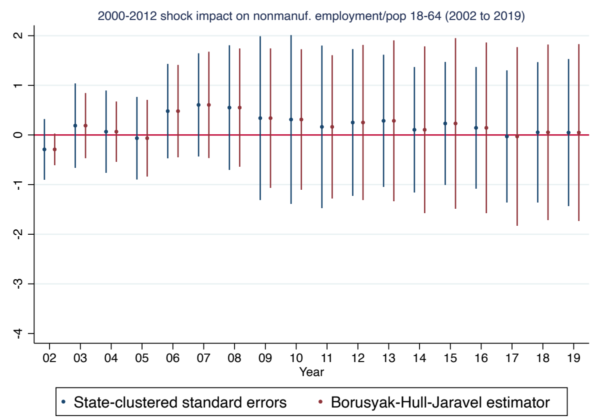
Note: Panels (a)-(f) report OLS regressions of the change in the indicated outcome between the year indicated on the horizontal axis and 1991 on the trade shock in (1) for the 1991-2000 period. Control variables include CZ Census region dummies, and the share of manufacturing in CZ employment in 1970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

Figure A2: Estimation Results Based on Borusyak, Hull, and Jaravel (2020)

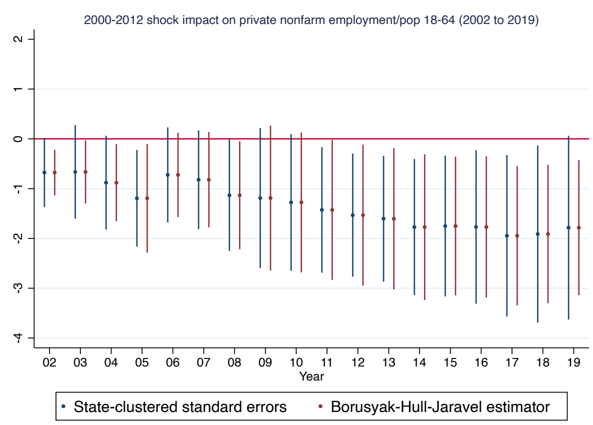
(a) Manufacturing employment/Working-age population



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



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



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

A.5 Supplementary Figures

A.5.1 Dynamic Adjustment to the China Trade Shock

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

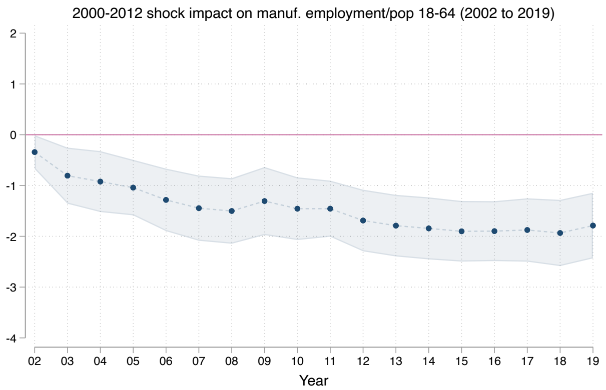
Appendix Figure A3 compares results for the change in manufacturing employment-population ratio over 2001 to 2019 when using our baseline 2000-2012 shock (Figure A3a), replacing this shock with that for 1991-2000 (Figure A3b), and including both shocks together (Figure A3c).[56] Because the trade shocks are highly correlated across decades, coefficient estimates in Figures A3a and A3b are very similar. Impact magnitudes are naturally larger for the 2000-2012 trade shock, which contains more information about shock impacts in the 2000s. The high correlation of the trade shocks means that when including both shocks together in the same regression (Figure A3c), coefficient estimates for the 2000-2012 trade shock become smaller and less precisely estimated.[57] We conclude that we cannot separately identify impacts of the 2000-2012 trade shock and continued adjustment to the 1991-2000 trade shock on outcomes in the 2000s. The estimated impact of the 2000-2012 trade shock is therefore a composite of these two effects.

[56] In Figure A3d, we include an orthogonalized version of the 1991-2000 trade shock (i.e., the residuals from a regression of the 1991-2000 shock on the 2000-2012 shock). The resulting coefficient estimates for the 2000-2012 shock are very close to those in Figure A3a (but not identical, due to covariance between the residual and the controls). Adding the residualized pre-2000 trade shock would thus have little impact on results for the post-2000 trade shock.

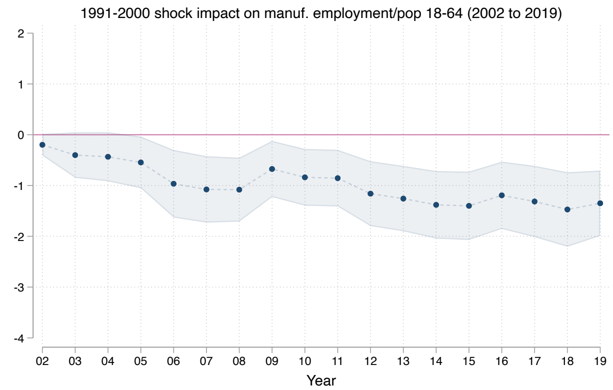
[57] In Figure A3c, unreported coefficients on the 1991-2000 trade shock are small and imprecisely estimated.

Figure A3: Combined Trade Shock Impacts on Manufacturing Employment

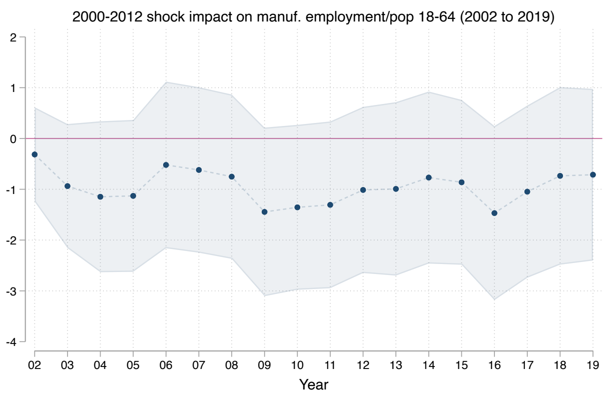
(a) 2000-2012 trade shock



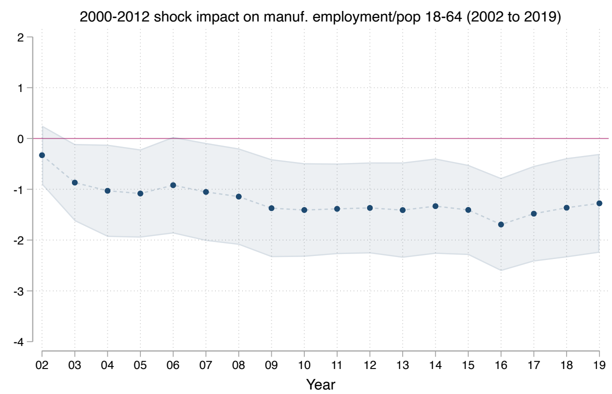
(b) 1991-2000 trade shock



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



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

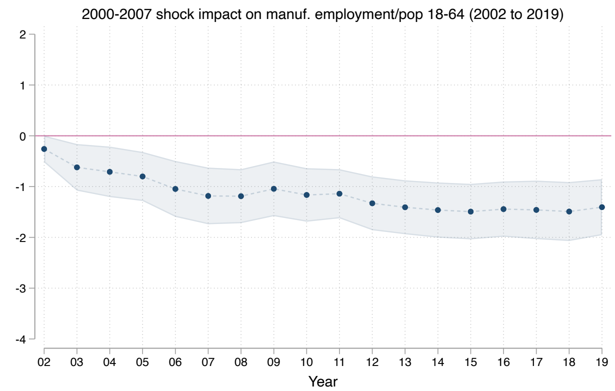


Note: Panels (a)-(d) report 2SLS estimates of (2). The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis. Panels (a) and (b) include the 2000-2012 and 1991-2012 trade shocks alone, respectively;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

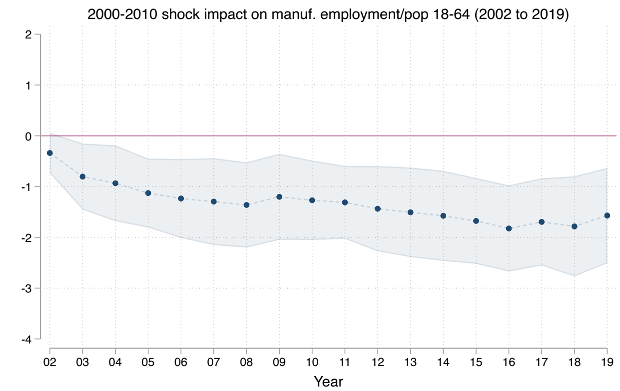
A.5.2 Employment and Population Headcounts

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

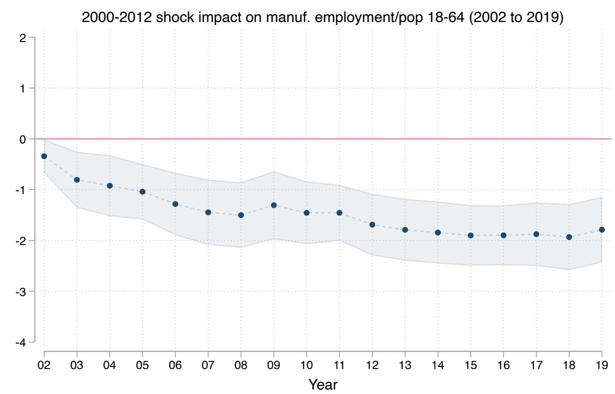
(a) Trade shock, 2000 to 2007



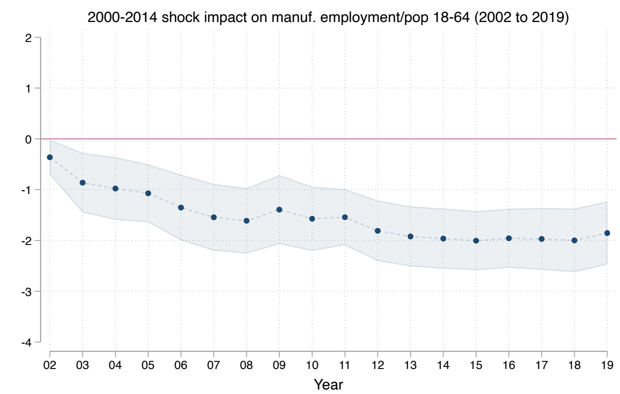
(b) Trade shock, 2000 to 2010



(c) Trade shock, 2000 to 2012



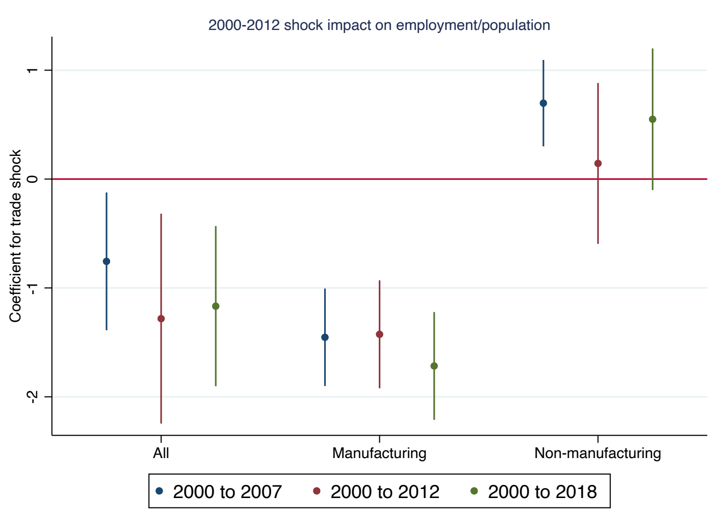
(d) Trade shock, 2000 to 2014



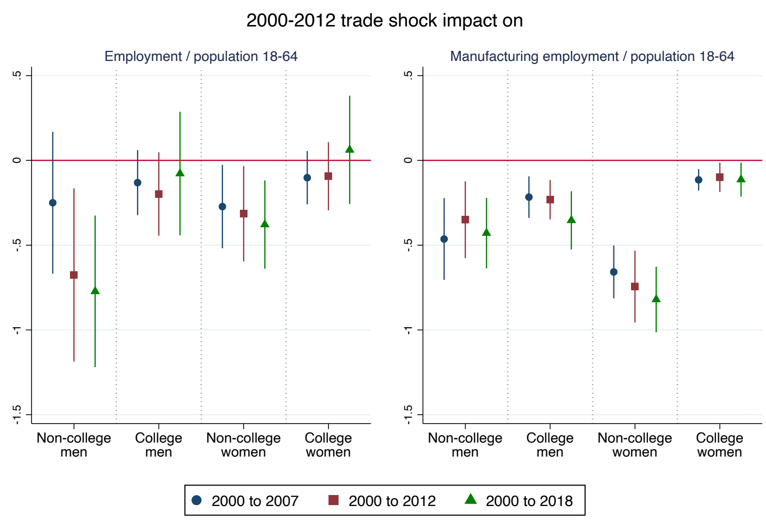
Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis The trade shock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

Figure A5: Trade Shock Impact on Employment-Population Ratios, Census-ACS data for 2000-2010, 2000-2014, and 2000-2019

(a) Employment/Working-age population by sector



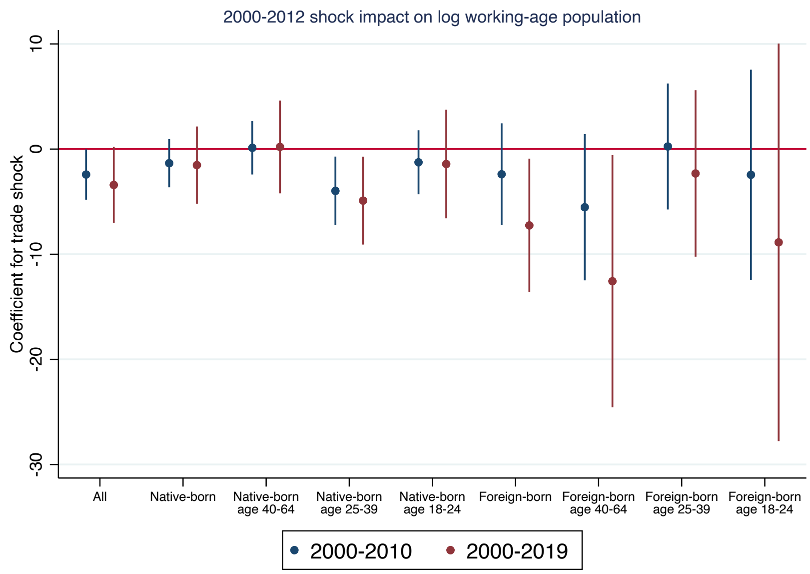
(b) Employment/Working-age population by sector and worker education



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

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

Log population by age cohort and nativity



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

A.5.3 Gravity-Based Spillovers

Adao et al. (2019a) build a general equilibrium trade model that captures spillovers between regions and generates reduced-form equilibrium conditions that have a shift-share structure. If national industries are subject to exogenous shocks, then employment and wages in regional economies will be affected through two channels. One is through changes in local industry revenue, which in the case of greater import competition will place downward pressures on local wages and employment. This is captured by changes in import penetration in (2). Second, in the presence of cross-region spillovers, wages and employment in one region will also be affected by localized changes in import penetration in other regions. For a given CZ, shocks to other CZs will matter more the larger and the closer are these other markets, as dictated by the gravity structure of trade. Adao et al. (2019a) quantify this spillover by adding to the specification in (2) the gravity-weighted changes in import competition all other regions (i.e., the sum of the trade shock in each region weighted by the size of and the distance to that region).[58]

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

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

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

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

and L_{kt} is the initial-period population of CZ k, D_{ik} is the distance between CZs i and k, and δ is the trade-cost elasticity, which following Adao et al. (2019a) we set equal to 5.[59] Appendix Figure A7 reports the results.

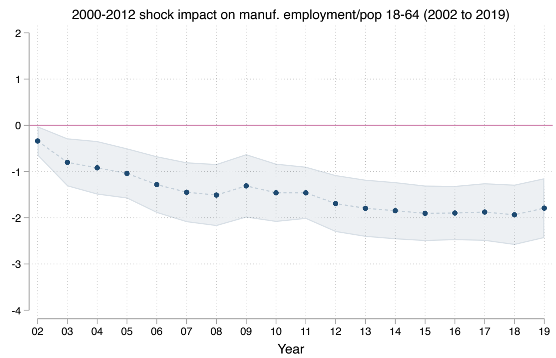
[58] A third channel through which national industry shocks affect local wages and employment is through changes in consumption costs. If greater import competition expands local consumption possibilities, real incomes and the demand for goods will increase. This will give rise to an own-region effect,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

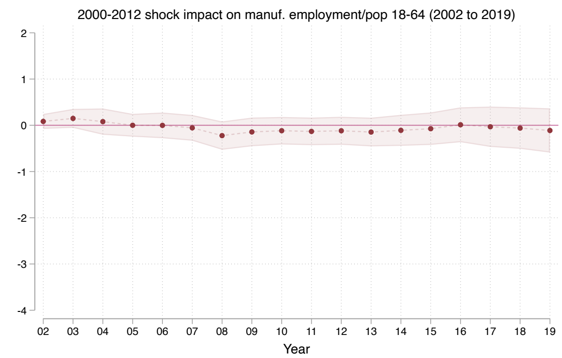
[59] We instrument for $\sum_k z_{ikt}\Delta IP^{cu}_{k\tau}$ by applying the gravity weights in (10) to the trade shock instrument in (3).

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

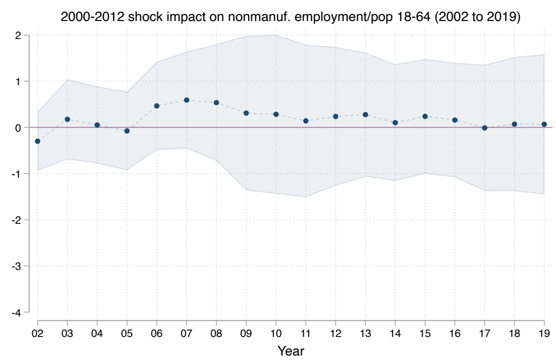
I. Impact of Local Trade Shock II. Impact of Gravity-Based Trade Shock

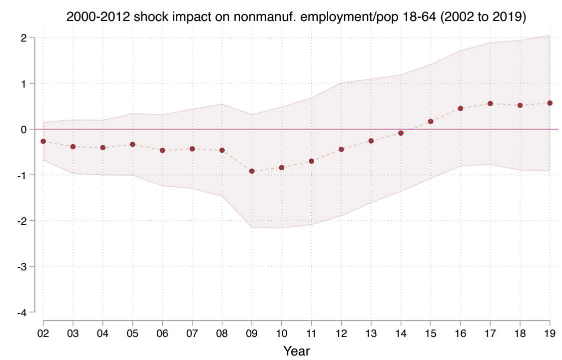
(a) Manufacturing employment/Working-age population



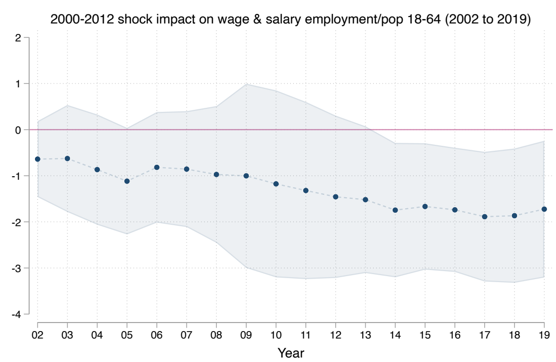


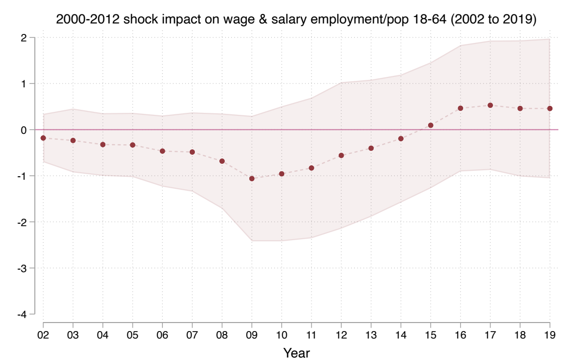
(b) Non-manufacturing employment/Working-age population





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





Note: Panels (a) and (b) report 2SLS coefficient estimates for β_{1h} and β_{2h} in (9) and 95% confidence intervals for these estimates. The dependent variable is the change in employment for the indicated measure between 2001 and the year on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, for the CZ itself, coefficients on which appear in the first column, and for a gravity-based version of import exposure in other CZs, coefficients on which appear in the second column; and control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

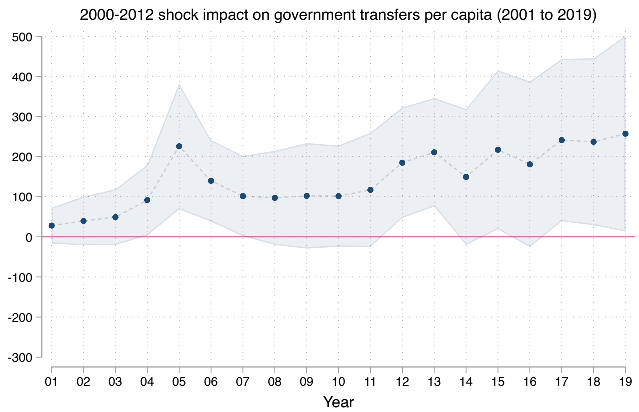
A.5.4 Personal Income and Government Transfers

In Figure A8, we explore the impact of trade shocks on government transfers by program type. To easily compare impact magnitudes across programs, we express them in terms of dollars per capita (rather than in logs). Two results stand out. First, consistent with results in Autor et al. (2013a) for earlier time periods, adjustments in Social Security and Medicare benefits, shown in Figure A8b, account for most of the responsiveness in government transfers induced by greater import competition, where the magnitude of these benefit gains expands as the time horizon lengthens. For the 2000 to 2019 time difference, the impact coefficient for Social Security and Medicare benefits per capita of \$201 (t-value = 2.46) is 78.2% (= 191/257) of that for total transfers per capita of \$257 (t-value = 2.07), shown in Figure A8a. Social Security payments include retirement benefits, from the Social Security Administration pension system, and disability benefits, from Social Security Disability Insurance. To receive these benefits, an individual must have left the labor force, either through retirement or by being declared medically unable to hold a job. The primary means through which government transfers replace labor income lost due to import competition is thus by accommodating an exit from paid work. The fact that the preponderance of transfer benefit payments accrue to non-workers may help account for the long-run negative effects of trade exposure on employment-population ratios see in Figure 5.

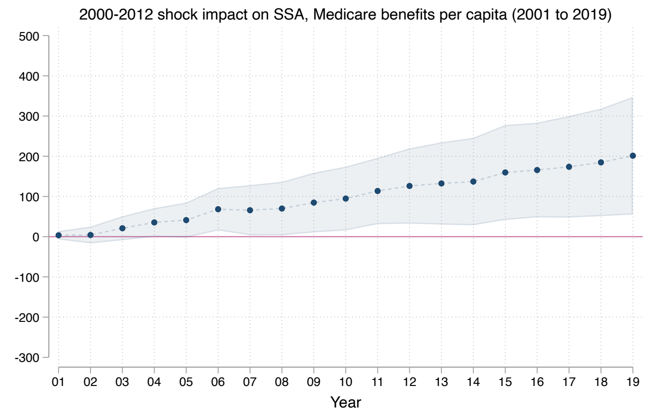
A second notable finding is that, despite trade-induced lower incomes, means-tested government programs meant to provide income assistance to poor households are largely unresponsive to greater import competition. We see this in the null or negative and insignificant responses of Medicaid and government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

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

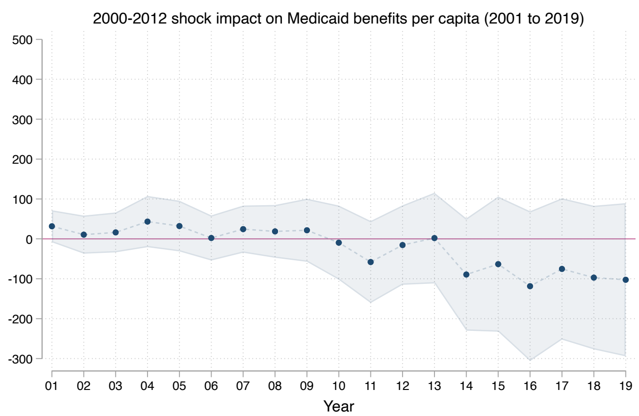
(a) Government transfers per capita



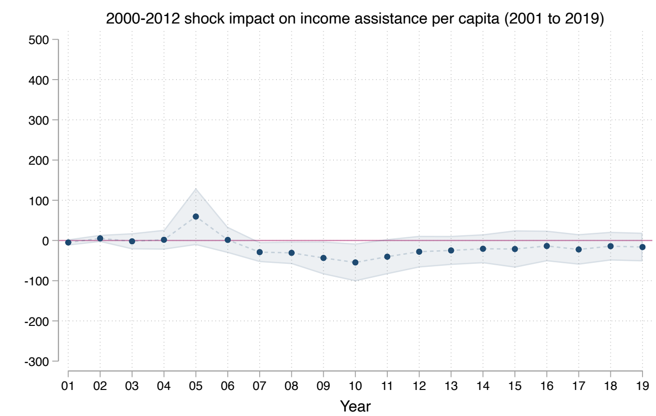
(b) SSA, SSDI, Medicare payments per capita



(c) Medicaid benefits per capita



(d) Income assistance per capita

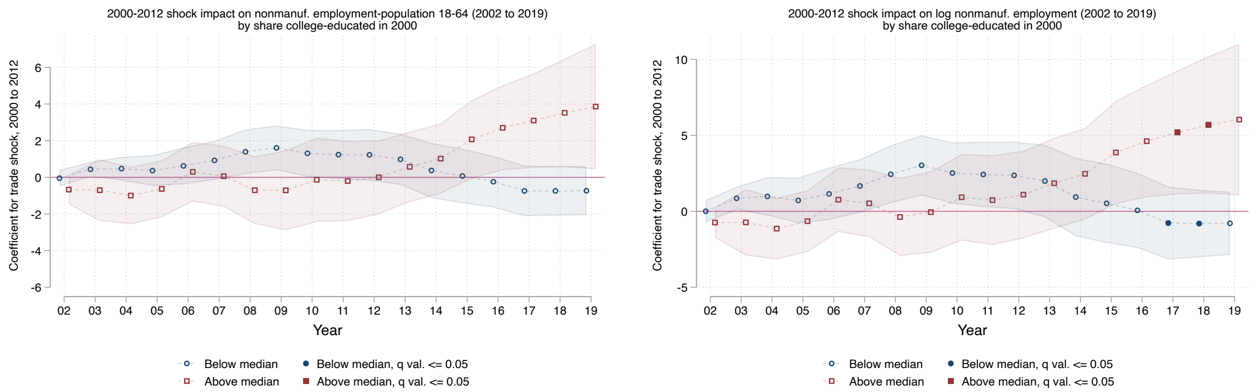


Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated value per capita between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change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

A.5.5 Heterogeneity in Impacts

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

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



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

A.6 Welfare Analysis of the China Trade Shock

A.6.1 Impacts of Import Competition on Housing Values

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

A.6.2 Other Quantitative Analyses of the China Trade Shock

Kim and Vogel (2020), in a related general equilibrium assessment of trade with China, allow for search and matching frictions in the labor market, which generate unemployment; distinct from the other approaches, they allow for amenities in employment, such that non-employed workers suffer an additional non-pecuniary loss. Applying their framework empirically and quantitatively, they find that trade exposure causes substantial variation in changes in welfare across commuting zones: the CZ at the 90^{th} percentile of exposure has a change in welfare that is 3.1 percentage points lower than in a CZ at the 10^{th} percentile of exposure. Because they measure trade exposure using U.S. tariffs (the threat of which was greatly diminished by China's entry into the WTO), as in Pierce and Schott (2016), and not using import penetration, as in Autor et al. (2014), it is difficult to translate the dispersion in income effects that they find with our analysis.