Import Exposure and Unionization in the United States*

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Abstract

We study how import competition affects union membership in the United States, adapting identification strategies from recent work on imports from China. Within manufacturing, union workers are slightly more affected than non-union ones, inducing modest declines in unionization. At the same time, total manufacturing declines are greatest among Right-to-Work states. We provide evidence that firms in Right-to-Work states tend to specialize in lower-quality products, making them more susceptible to competition with Chinese goods. However, while reducing unionization within manufacturing, import competition causes a robust increase in unionization outside of manufacturing, more than offsetting within-manufacturing declines. This appears to be driven by family members of would-be manufacturing workers shifting to higher-wage jobs: for less-educated women, the highest paying opportunities are often in healthcare and education, which are disproportionately unionized. Altogether, we calculate that the decline in US union density would have been 36% *larger* without Chinese imports.

Version 1.0. COMMENTS WELCOME.

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1 Introduction

Over the last 40 years, we have witnessed dramatic growth in wage and income inequality in the United States. Rising inequality coincided with a persistent, secular decline in the size and strength of labor unions along with growth in imports from low-wage countries. Against this backdrop, two things are well-known. First, low-wage import competition (especially with China) has decimated the manufacturing sector (Autor, Dorn, and Hanson, 2013; Pierce and Schott, 2016), the traditional mainstay of middle-class employment. Second, unions are powerful forces for constraining inequality (Ahlquist, 2017; Farber et al., 2018). There is also some evidence–albeit more controversial–that unions raise average wages (e.g., Card (1996)).

If competition with Chinese imports induces downward wage pressure in manufacturing then import competition can undermine unions' bargaining positions. By eroding wage-enhancing labor market institutions, trade exposure can have *indirect* effects on inequality beyond reduced availability of middle-wage jobs in manufacturing. If unions are as central for inequality as some scholars have suggested, then our understanding of how Chinese imports have transformed the US labor market might be ignoring an important aspect of the story.

Did import competition accelerate the decline of American unions? To answer this question we draw upon two widely-cited strategies used to identify exogenous variation in US manufacturing imports from China: Autor et al. (2013)–hereafter, ADH–and Pierce and Schott (2016)–hereafter, PS. We adapt these approaches for our empirical context and then use them to estimate the effects of Chinese imports on unionization, both within manufacturing industries and across US states, between 1990 and 2014. A major focus of this paper is our attention to *indirect* effects of Chinese imports on the unionization rates of those not directly employed in manufacturing.

First, we find that highly unionized industries were relatively insulated from Chinese import competition (i.e., there is a negative correlation between 1990 unionization and subsequent exposure to competition). And because import competition decreases industry-level employment (Acemoglu et al., 2016), less unionized industries saw disproportionate reductions in employment. Chinese imports therefore had a between-industry reallocation effect that raises union density in manufacturing. But we also find that within a particular manufacturing industry, Chinese import competition reduces employment among unionized workers more than among the non-unionized, driving modest but significant decline in the union share. Quantitatively, this within-industry effect is bigger. We estimate that exposure to import competition induced a 2 percentage point decline in union density within manufacturing, roughly one-sixth of the observed decline between 1990 and 2014.

We then follow ADH to create a shift-share, Bartik (1991)-style measure of each state's exposure to Chinese manufacturing import competition. Echoing ADH, we find large effects on manufacturing employment (an 11% decrease for each standard deviation of import exposure). Workers exiting the labor market account for roughly half of this decline. Of the remainder, roughly half go into unionized non-manufacturing jobs. Unionized non-manufacturing employment increases by as much as non-union employment (in levels), but represents a much smaller employment share at baseline. Thus we find that a standard deviation of import exposure drives up union density outside of manufacturing by 0.3 percentage points (or 4%), evenly split between the public and private sector.

Growth in unionized non-manufacturing work occurs only in states that do not have so-called "Right to Work" (RtW) laws. However, patterns in RtW and non-RtW states differ in other ways: the effects of exposure on manufacturing employment are roughly twice as large in RtW states and non-employment absorbs a larger share (two-thirds). We provide evidence that differences in output quality explain differences in manufacturing employment effects. Within a given industry, the lowest paying states are the most negatively affected by Chinese imports, and these are more likely to be RtW states. Likewise, in industries where output is homogeneous and there are no quality differences, we find that non-RtW states are slightly more affected.

Quantitatively combining our estimates, we find that union density gains outside of manufacturing outweigh declines within manufacturing. Although import competition drove down union density in manufacturing, we calculate that it prevented 26% (1.6 percentage points) of the decline in density that *would have* happened absent the changes induced by the Chinese import shock. This net effect derives from *i*) relatively modest effects within manufacturing, *ii*) the fact that manufacturing is a small share of total employment in this period, and *iii*) increases in non-employment (i.e., more union jobs per employed person comes partly from fewer employed persons).

Were these new union members outside manufacturing simply displaced manufacturing workers or were they others? Using a machine learning approach, we identify individuals in 2014 who would have been likely to work in manufacturing had they been demographically identical and working in the same place in 1990. We also identify those living in households with would-be manufacturing workers, typically spouses and children. Would-be manufacturing workers seem to primarily shift into non-employment and the low-wage non-unionized service sector. Household members of would-be manufacturing workers, however, saw large (30%) increases in employment in education and health, where union density has been stable. Our results suggest that the family members of would-be manufacturing workers, not the workers themselves, drove the trade-induced increase in non-manufacturing unionization. We show that the employment shift was larger in states more exposed to import competition, that higher wages in these industries can explain the shift without workers specifically targeting unionized industries, and that this mechanism can explain why labor markets did not adjust as effectively in RtW states (where these industries are less unionized and do not have wage premia).

Our results contribute to the large literature on explanations for declining unionization in the United States (Western, 1997; Wallerstein and Western, 2000; Farber and Western, 2001; Southworth and Stepan-Norris, 2009). We are not the first to consider import competition from low-wage countries as a potential explanation. Most closely related are Baldwin (2003) and Slaughter (2007) who use data through the early 1990's and industry differences in imports without an explicit source of exogenous variation. They do not find evidence that industries facing more import competition saw greater declines in union density.[1] Using later data and a clearer identification strategy, we revise this conclusion.

We also contribute to the recent literature on the consequences of Chinese import competition. This research has shown that the "China Shock" has transformed the United States economy, including labor markets (Autor, Dorn, and Hanson, 2013; Caliendo, Dvorkin, and Parro, 2018), marriage markets (Autor, Dorn, and Hanson, 2019), political environments (Autor et al., 2016), household debt (Barrot et al., 2017), worker health (Pierce and Schott, 2018), and crime levels (Che, Xu, and Zhang, 2018). We contribute to this work by identifying a complex series of consequences for labor unions.

Finally, our findings on the importance of household adjustment contribute to work on the "added worker effect," in which spouses' employment responds to negative shocks to the prime earner (Lundberg, 1985). Second earner adjustments are a key mechanism in structural analyses of social insurance and inequality (Blundell, Pistaferri, and Saporta-Eksten, 2016), preferences over unemployment insurance (Ahlquist, Hamman, and Jones, 2017), retirement and tax policy (Borella, De Nardi, and Yang, 2018), and macroeconomic fluctuations (Mankart and Oikonomou, 2016). They also help distinguish between models of household decision making (Donni and Chiappori, 2011). Most empirical studies of added worker effects focus on decisions of whether and how much to work, and focus on short-term responses. In contrast, our results find that an important channel is shifting across *types of work* towards higher paying jobs, echoing the importance of occupation and industry switching for understanding labor market adjustment (Kambourov and Manovskii, 2008). Our findings also differ by focusing on large-scale, long-run changes in the labor market, rather than temporary unemployment spells. This is useful because there is growing evidence that adverse labor market shocks are very persistent, so it is important to understand how the long-term second earner adjustment process works.[2]

[1] The magnitude of the estimates from Slaughter (2007) are similar to ours but do not cross traditional significance thresholds. Slaughter had to rely on within-industry over-time variation to estimate short-run effects, while we are able to rely on the exogenous component of the (much greater) between-industry variation to estimate (likely larger) long-run effects.

[2] see (Amior and Manning, 2018) for evidence from the USA and Dix-Carneiro and Kovak (2017) on the

The rest of the paper is organized as follows. Section 2 summarizes the two identification strategies we draw upon for estimating the effects of Chinese imports. Section 3 discusses data and adjustments to industry codes, and shows that the central results from past work hold up with the more aggregated industry codes. Section 4 shows that cross-industry import exposure is correlated with historic unionization and discusses the implications for identification. Section 5 describes our methods for estimating the effects of imports. Section 6 presents our industry-level effects and Section 7 presents our state-level effects. We next turn to interpretation. Section 8 presents the decompositions we use to quantitatively interpret the magnitudes of our estimates and Section 9 presents evidence to distinguish responses of would-be manufacturing workers and their household members. Section 10 concludes.

2 Estimating the causal effects of Chinese imports

Two papers are particularly noteworthy for developing empirical strategies to isolate the causal effects of manufacturers' exposure to import competition from China: Autor, Dorn, and Hanson (2013) and Pierce and Schott (2016). We begin with a brief review of these identification strategies and then present evidence on some issues and limitations for studying unionization.

These identification strategies take different approaches, drawing upon two major changes during the 1990's and early 2000's. First, beginning in the late 1980's, China transitioned to a market-oriented economy, including a dramatic overhaul of virtually all features of production. Second, in 2001, China joined the World Trade Organization (WTO) and secured the Normal Trade Relations (NTR) tariffs (i.e., WTO-negotiated tariff rates apply).

Autor, Dorn, and Hanson (2013) emphasize that growth in Chinese imports since 1990 was driven by pro-market reforms and was concentrated in a limited set of industries. For instance, they note that 1% of industries account for 40% of growth in US imports. Methodologically, ADH make two major contributions to facilitate causal inference in this context. First, they use industry-level Chinese exports to other OECD countries as an instrument for industry-level exports from China to the US.[3] The goal is to isolate the large and heterogeneous industry-specific improvements in Chinese productivity (Khandelwal, Schott, and Wei, 2013) from shocks to Americans' demand for specific products. Acemoglu et al. (2016) use this industry-level instrument in their analysis. For their second, widely cited innovation, ADH recognize that, historically, industrial composition varies dramatically in space. Regional and local industrial specialization persists and has lasting effects on local labor markets. ADH construct a Bartik-style shift-share instrument that maps cross-industry variation in Chinese import competition

long-term effects of trade shocks in Brazil.

[3] Specifically Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

into local (commuting zone) labor markets.

Pierce and Schott (2016) develop the second identification strategy we use. China's accession to the WTO at the end of 2001 granted it permanent Normal Trade Relations (NTR) status, securing the associated low tariff rates. While the US had maintained these low tariff rates on Chinese imports since 1980, Congress revisited this decision each year in contentious votes. There was considerable risk that US tariffs could revert to the (much higher) rates applied to "non-market economies." PS show that making the NTR rates permanent in 2001 induced dramatic increases in imports from China. Import growth was strongest in the industries where the "NTR gap"– the difference between the NTR tariffs and the non-market tariff reversion point – was largest. Following their strategy, we use the NTR gap as an alternative method to generate exogenous variation in industry-specific import competition. We then follow ADH's general approach to map this industry variation onto geography (like Pierce and Schott (2018)).

Importantly, the OECD countries ADH use for their instrument had granted China permanent NTR status before 1990. Thus, the ADH variation is unrelated to WTO accession and implied tariff changes, which are the explicit focus of Pierce and Schott (2016). The two identification strategies rely on different sources of exogenous variation. Empirically, the correlation between the two instruments is relatively low (.27 across industries and .49 across states). In the appendix we show that all of our results are very similar between the two identification strategies, though our baseline results pool the two strategies to improve statistical power (explained in more detail below). Given the differences between the identification strategies, the similarity of results increases our confidence in the credibility of our causal estimates.

3 Data

We rely on four data sources. First, we take Chinese import data from the ADH public replication files, extended through 2014 thanks to updates provided by Gordon Hanson. Second, we take NTR and non-NTR tariff rates from the PS public replication files. Third, we use the Annual Survey of Manufacturing (ASM) for (SIC) industry-level employment and capital-labor ratios. Fourth, we use the Current Population Survey (CPS) for data on union membership.[4] Our core employment results for both states and industries are based on Census-defined industries.

[4] We use the Integrated Public Use Microdata Sample (IPUMS) versions of the CPS, which has cleaned the data and made variables as consistent as possible over time (Flood et al., 2017). Since the industry- and state-level sample sizes can be small, we follow the common practice and pool three consecutive years for all calculations based on CPS employment, i.e., "1990 employment" is based on the 1989-1991 CPS samples.

3.1 Adjusting industry codes

There are two industry classification systems in the United States. Data dealing directly with firms, such as the ASM, as well a firm-level administrative data use the Standard Industrial Classification (SIC) and the North American Industrial Classification (NAICS, which replaced SIC in 1997). The original ADH and PS papers use these industry codes. They are detailed and easy to connect to product-level import and tariff data. Surveys of individuals use a less granular classification system based on Census-defined categories.[5]

To link NAICS/SIC-based import and tariff data with CPS-based union membership, we construct a crosswalk from the 1997 NAICS to 1990 Census industry codes using the 2000 Census and the 2001-2002 American Community Survey (ACS, again from IPUMS), which has included both industry codes since 2000. We identify the Census industry accounting for the largest share of a NAICS industry's employment. We then use files available on David Dorn's website to map SIC industries into NAICS, again using the NAICS industry accounting for the largest share of a SIC industry's employment. Throughout, when we refer to "SIC industries," we use the "sic87dd" scheme used by ADH. These codes are slightly coarser than the original 1987 SIC codes (used by PS). We therefore aggregate the PS SIC-based tariff measures to the ADH scheme based on unweighted averages across HS codes.

3.2 Replicating existing results with Census industries

Aggregating imports to Census-based industry codes means we go from 357 SIC-based manufacturing industries comparable over time to 64 under the Census codes. As a first step we demonstrate that the core findings from ADH and PS still hold under coarser industrial classification.

Table 1 shows the relationship between both the PS and ADH import exposure measures and the changes in industry imports and employment over the full 1991-2014 period.[6] The upper panel (A) uses the change in China-Other trade as the measure of import penetration.[7] Panel B uses the NTR gap.

Column 1 regresses the change in China-US trade on these instruments at the SIC-industry level, and finds that both are strongly and significantly predictive of increased imports. Column 2 replicates this using 64 Census-defined industries. The table shows that the standard deviation of both instruments falls slightly going from SIC to Census industries (5% for China-Other

[5] The Census Bureau's industry codes are re-evaluated every 10 years following the decennial census. The IPUMS project provides a crosswalk of all Census-based industry classifications back to the 1990 scheme (Flood et al., 2017; Ruggles et al., 2018), which we use.

[6] This updates both the Acemoglu et al. (2016) and PS results, which end in 2011 and 2005, respectively.

[7] Specifically, the change in Chinese imports divided by lagged employment.

trade, 15% for the NTR gap); i.e., aggregation costs us only a small amount of variation. Both instruments continue to predict import growth ($p < .05$) and the coefficients actually grow.

[Table 1 about here.]

Columns 3-6 display the estimated reduced form effects of both instruments on the change in industry-level employment. Column 3 estimates the effects of each instrument on changes in SIC-based employment (from the ASM).[8] A one standard deviation increase in China-Other trade implies a 20% (22 log point) decrease in industry employment. Similarly, Panel B estimates that a one standard deviation increase in the NTR gap leads to a 19% reduction in employment. These results, like most that we report in the paper, are strikingly similar between the two identification strategies.

Column 4 aggregates the ASM data into the 64 Census-based industries and estimates larger effects, with 23% and 28% employment declines for each standard deviation increase in China-Other trade and the NTR gap, respectively. Why might we find larger import effects when we aggregate data to the Census industry level? We investigate the possibility of spillovers across SIC-industries due to product substitutability.[9] SIC industry codes are quite granular. For instance, there is one Census-based code for the manufacturing of any meat product whereas there are 3 SIC industries for meat product manufacturing (meat packing, sausages and prepared meats, and poultry slaughtering and processing). From 1990-2000, US imports of Chinese meat packing products increased by 160%, while US imports of Chinese poultry products increased by 1,130%. If different types of prepared meats are substitutes, then increased availability of inexpensive poultry might affect demand for other packed meats.

To estimate import spillovers into SIC-based industry i, we calculate the total increase in China-Other trade in *other* SIC industries that map into the same Census industry as i (likewise for the NTR gap). We then regress changes in SIC industries' employment on import exposure within that SIC as well as in other, similar SIC industries. Results are in column 5. Imports from other industries have large employment effects (equally sized with ADH, over 3 times as large with PS). Thus, the coarser Census-based codes may perform better than the precise SIC codes for estimating employment effects.

All employment effects in columns 3-5 relied on ASM data, which is based on surveys of firms. Column 6 replicates column 4 and estimates the effects of the instruments on employment using the noisier CPS. These estimates are somewhat smaller than those using ASM employment

[8] Pierce and Schott (2016) use similar but restricted access employment data. Acemoglu et al. (2016) use SIC-based industries and the ASM.

[9] Pierce and Schott (2016) study spillovers along the supply chain using input-output tables. Our spillovers are fundamentally different. Ours reflect the substitutability between different products that are similar enough to be in the same broad industry.

but similar to the SIC-level effects reported in column 3. One standard deviation increase in exposure reduces employment by 14% (using the PS instrument) to 19% (using ADH).

In summary, the coarser Census industries–which we must rely on to study unionization–perform at least as well as the detailed industries from past work. While we lose some cross-industry variation through aggregation and the CPS estimates are noisier, results suggest significant trade-induced employment declines similar in magnitude to existing estimates.

4 Identification challenges

4.1 Autor, Dorn, and Hanson (2013)

The ADH identification strategy fundamentally relies on Chinese productivity growth concentrated in certain industries. These industries were not chosen randomly. For instance, import growth was concentrated in labor-intensive industries where China held a comparative advantage (Amiti and Freund, 2010). Figure 1 shows that these industries differ in their historical unionization rates. On average, industries with the most growth in China-Other trade had lower rates of unionization in 1990.[10]

[Figure 1 about here.]

We entertain three potential explanations for the negative relationship between Chinese export growth and lagged unionization. First, we consider industries' skill profile, measured as the non-production workers share of all workers (from the ASM). Production workers are more likely to unionize than non-production workers, so industries with relatively more non-production staff will have relatively low unionization rates. Second, we consider capital-labor ratios since China's comparative advantage is concentrated in labor-intensive industries. Finally, we consider 6 industries in the textile, apparel, and leather sector, which had the lowest rate of unionization and which had distinctive patterns of both trade policy (Brambilla, Khandelwal, and Schott, 2010) and Chinese export growth.[11]

In the top panel of Table 2 we regress 1990 industry-level union density on the change in China-Other trade from 1990 to 2014, sequentially including these industry-level covariates. Each covariate individually explains only a modest share (10-20%) of the unionization-trade relationship. In column (5), however, after controlling for all three variables, the coefficient on Chinese export growth is only 36% as large and is no longer statistically distinguishable

[10] The negative correlation remains even excluding outlier industries.

[11] We classify manufacturing industries into 9 sectors based on two-digit Census industry codes. This sector has the lowest union density.

from zero ($p = .24$). These three factors appear to explain the correlation between lagged unionization and import growth.[12]

[Table 2 about here.]

In light of this, our primary industry-level specifications control for 1990 union membership rates. We also present specifications that include the three additional covariates from Table 2. We show that, conditional on 1990 unionization, including these variables does nothing to our estimates of the effect of imports on unionization. We view this as a useful test of our identifying assumptions. These factors are strongly correlated with industry variation relevant to both unionization and Chinese imports. If these covariates do not alter our key estimates, it suggests either that historical union context is unrelated to the changes seen over the past 25 years (which we believe unlikely) or that controlling for lagged unionization adequately accounts for this historical context.

4.2 Pierce and Schott (2016)

PS show that after 2001, US imports from China rose in the industries where the NTR gap was largest. They also show that lagged unionization is negatively correlated with the NTR gap (their Table A.2), but that controlling for lagged unionization has no effect on their main results (their Table 2). Although PS devoted little attention to this relationship, it is obviously more important here.

The NTR gap depends on both NTR tariffs (applied to WTO members) and the non-NTR tariffs that would be applied to non-market economies absent a Congressional waiver. Either could produce a correlation between unionization and the NTR gap. Figure 2 shows that it is the non-NTR tariffs that drive this relationship: Historically unionized industries had *lower* nonmarket tariff rates in 1999 (the opposite of what a simple political economy explanation based on union power would suggest).

[Figure 2 about here.]

In the bottom panel of Table 2 we again ask whether capital-intensity, skill-intensity, or the textile/apparel sector explain this correlation. Capital-labor ratios and the textile and apparel

[12] ADH estimate models in first differences, so lagged *levels* of unionization are not directly a concern. However, if historic levels of unionization are correlated with subsequent changes in unionization, taking first differences will not solve the problem. We regress changes in industry-level unionization from 1990-2014 on 1990 unionization and find the two are strongly negatively correlated ($p < .001$): An industry with a 10 percentage point higher union density in 1990 saw density decline by an additional 6 percentage points by 2014, i.e., industries with "more room to fall" saw bigger reductions in union membership.

dummy each account for 20-40% of the relationship. Together, the three variables explain over half of the relationship. Conditioning on all three we see that unioniztion-NTR gap relationship is no longer statistically significant at conventional levels ($p = .11$).

In summary, across both the ADH and Pierce-Schott instruments, it appears that more unionized manufacturing industries were relatively insulated from the Chinese import penetration. This is largely due the fact that the pockets of unionization still remaining in US manufacturing by 1990 were in relatively capital-intensive industries that Chinese exporters avoided, and that unions in labor-intensive industries (like textiles) had been under pressure for decades by this time (Silver, 2003).

5 Methods

5.1 Industry-level estimates

All of our core specifications reflect "long-differences" (i.e., total change over 1990-2014) in outcomes at the industry- or state-level. When these outcomes are based on CPS data (our main results), we follow the convention of using adjacent years to improve the precision of the estimates (so 1990 is based on 1989-1991; 2014 is based on 2013-2015). Industry-level regressions are weighted by 1990 industry employment and state-level regressions are weighted by 1990 population, although using weights does not substantively affect the results.

For both industry-level and state-level outcomes, we use both the ADH and PS strategies. For ADH, the key explanatory variable is the change in industry-level exports from China to comparable OECD countries from 1991 to 2014, divided by 1990 US employment at the industry level. We use the same set of OECD countries and we calculate changes in real terms (rather than percent changes) as they do. Letting i index industries, our empirical specification is:

$$\Delta Y_i = \alpha + \beta \Delta \text{China-Other Trade}_i + \gamma \text{Union Density in 1990}_i + \varepsilon_i$$

For the PS strategy, we again follow the authors and use the industry-level 1999 NTR Gap, which determines the amount by which expected tariffs fell after China's WTO accession. For these regressions, our empirical specification is:

$$\Delta Y_i = \alpha + \beta \text{NTR Gap}_i + \gamma \text{Union Density in 1990}_i + \varepsilon_i$$

5.2 State-level estimates

To study local labor market effects (including outside of manufacturing) we rely on variation across US states (small samples in the CPS prevent disaggregation to the commuting zone).[13] Our state-level measure of import competition exposure is a shift-share Bartik-style instrument, taking a weighted average of industry-level variables (here, the change in China-Other trade and the NTR gap), where the weights are given by the industry's share of state employment in the initial period (here, 1990).[14] Denoting employment as "emp" and indexing states with s and industries with i, our state-level measure of exposure, based on the ADH instrument, is:

$$\begin{aligned}
\text{Exposure}_s^{\text{ADH}} &= \sum_i \left(\frac{\text{emp}_{i,s,1990}}{\sum_i \text{emp}_{i,s,1990}} \right) \times \frac{\Delta\text{China-Other Trade}_i}{\sum_s \text{emp}_{i,s,1990}} \\
&= \sum_i \left(\frac{\text{emp}_{i,s,1990}}{\text{emp}_{s,1990}} \right) \times \frac{\Delta\text{China-Other Trade}_i}{\text{emp}_{i,1990}}
\end{aligned}$$

In this specification, we follow ADH and calculate change in Chinese imports *per 1990 worker* and set import growth to zero outside of manufacturing.[15]

Our measure of state-level import exposure using the PS instrument is:

$$\text{Exposure}_s^{\text{PS}} = \sum_i \left(\frac{\text{emp}_{i,s,1990}}{\sum_i \text{emp}_{i,s,1990}} \right) \times \text{NTR Gap}_i$$

Using 51 states instead of 741 commuting zones, we lose a substantial amount of useful variation. For instance, ADH report that across CZ's, the standard deviation of exposure is about 80% of the mean. At the state-level, even with the benefit of the extended time series, the standard deviation is less than 30% of the mean. Thus, our estimates will be less precise than those of ADH. Nonetheless they are precise enough to tell a consistent, robust story, partly because the PS instrument gives us additional identifying variation.

The empirical specification for our state-level regressions is:

$$\Delta Y_s = \alpha + \beta \text{Exposure}_s + \varepsilon_s$$

[13] The CPS does include some respondents' MSA, and MSA's are a subset CZ's. Unfortunately, the basic CPS only began including MSA in 1994. For our initial period, we would have to use the ASEC. Since union status is only available in the ORG and the 1989 ASEC cannot be merged with the ORG, this would restrict us to only the 1990 and 1991 samples, and only the one fourth of respondents who are in waves 4 or 8 (the ORG waves) in March (the ASEC month). Additionally, only 50% of 1990/1991 respondents lived in an identifiable MSA. Thus, sample sizes for our baseline period would be only 1/12 of what we can use for the state-level analyses, and this much smaller sample would be divided across 250 MSA's instead of 50 states.

[14] Like ADH, we use the County Business Patterns for industry share data.

[15] As they acknowledge, this creates a mechanical correlation between lagged manufacturing employment share and exposure to import competition. Our state-level results are similar if we average ΔChina-Other Trade only for the manufacturing industries, or if we control for state-level lagged manufacturing share.

Our primary outcomes of interest at the state level is the 1990-2014 change in working age population shares for each of six mutually exclusive groups: 1) non-employed, 2) union non-members employed outside manufacturing, 3) private-sector union members working outside of manufacturing, 4) public-sector union members working outside of manufacturing, 5) union non-members working in manufacturing, and 6) union members working in manufacturing. Population shares necessarily sum to one within states and time periods; changes across time must sum to zero within states.[16] Our core long-difference strategy studies how these 25-year changes vary with import exposure.

5.3 Identification

Our identification assumption is that, conditional on 1990 union density, the NTR Gap and ΔChina-other trade are exogenous determinants of Chinese import competition. To support this identification assumption, we show that our main industry-level results are almost identical (but more precise) when we control for the industry characteristics that explain the import competition-lagged union density correlation. While we sometimes refer to the ADH and PS "instruments," we report only OLS estimates, not IV regressions. Because we have already shown that they increase imports, one may think of our models as reduced form instrumental variables strategy.

5.4 Combining identification strategies for improved power

Importantly, our two instruments for Chinese import competition are only imperfectly correlated: 0.27 at the industry-level and 0.49 at the state-level. Since they rely on different identifying variation, we can combine them to improve statistical power. To pool our instruments we normalize each to have unit standard deviation across industries or states, depending on the analysis, add the standardized instruments together, and re-standardize the sum to have unit standard deviation. All results described in the main text refer to this pooled instrument but the appendix contains mirror tables in which we consider each identification strategy separately. When we do, we include explicit tests for the equality of coefficients across the two and nearly always fail to reject the null because the two point estimates are nearly identical.[17]

[16] As a robustness check, Table A10 uses changes in log group size as the dependent variable, conditioning on the change in state log population. Results match what we present in the main text.

[17] As the only exception, we find a 1SD increase in ADH instrument implies a 0.7 percentage point decline in the share of the population who are non-union members in manufacturing, compared to a 1.6 percentage point decline using the PS instrument.

6 Chinese imports and industry-level unionization

We first estimate the effect of increased Chinese imports on manufacturing industry-level employment outcomes. Table 3 presents our core results. Column 1 indicates that a standard deviation increase in import exposure reduces total employment by 18% ($p < .01$). In columns 2 and 3, we separate union members from non-union members. We find significant effects on both ($p < .01$) but larger proportional effects on members (though not reported in the table, the coefficients are significantly different from one another). The estimates imply that a one standard deviation increase in exposure reduces employment of union members by 37% and of non-members by 18%. Union density in manufacturing is only around 15% during this period, so, although proportional effects are twice as large for union members, our results imply there would be three non-union jobs lost for every union job lost.[18]

[Table 3 about here.]

In column 4, we calculate the change in industry-level union density, defined as the share of workers who are union members. Of course, since union members are affected more than non-members (proportionally), this must reduce density. We estimate effects on the change in density both because it is a useful summary measure, and because the magnitudes will be useful for our decompositions later. We find that a one standard deviation increase in import competition reduces union density by 1.4 percentage points ($p < .01$). For context, during this period, the average industry saw a 13.2 percentage point decline. Thus, Chinese imports are a modest but statistically and economically significant cause of this decline.

In column 5, we present a model that includes the covariates connected with Chinese exports and 1990 union density (see Table 2). The coefficient on exposure to imports is virtually unchanged from Column 4 and remains statistically significant ($p < .05$). The decline in industry-level unionization is not explained by lingering industry differences unaccounted for by 1990 levels of unionization, increasing confidence in our identification strategy.

In Appendix Table A4, we show these results are nearly identical across the two identification strategies, both of which show effects on union members that are significantly different from zero ($p < .05$) and non-members ($p < .01$), and 2-3 times as large as non-member effects. Both show declines in union density – with magnitudes unchanged by the controls – although the ADH estimate is smaller (.6 percentage points) and only significant *with* the controls. None of the specifications produce estimates that are significantly different between the strategies. Given the low correlation between the two sources of identification (.27), this gives us confidence in the validity of our estimates.

[18]Union jobs lost: $.368 \times .15 = .055$; Non union jobs lost: $.175 \times (1 - .15) = .148$

6.1 Labor costs as a mechanism

If unions raise wages, competition from low-wage countries might disproportionately affect unionized workers by exacerbating labor cost pressures. Estimated union wage premia vary substantially across industries, and this variation might be useful for testing the labor cost mechanism. Using CPS ORG data from 1989-1991, we estimate that the average union member earns a 12% higher wage than an observationally equivalent non-union worker, but, across 64 industries, the 10^{th} percentile is only 4.3%, while the 90^{th} percentile is 17.7%.

In Table A5, we test whether the effects of import competition differed by industry wage premia. Estimates are imprecise, with confidence intervals unable to rule out either large negative or large positive effects.[19] We also test whether imports reduced premia by undermining unions' bargaining position. Again, our results are imprecise and uninformative. Thus, despite the appeal of using industry heterogeneity to test whether union members are more affected by Chinese imports because of higher wages, the data provide little information.

7 Chinese imports and state-level unionization

Although Chinese import penetration caused de-unionization in manufacturing, effects on overall unionization are unclear. Displaced manufacturing workers may become union members in other parts of manufacturing or elsewhere in the economy.[20] To examine the broader effects of exposure, we look to state-level variation.

7.1 State-level results

In Table 4 we regress changes in state-level population shares for our six mutually exclusive employment categories on state-level import exposure. 1990 group population shares and average 1990-2014 changes appear at the top of the table while tests for various hypotheses appear in Panel C. Recall that coefficient on the combined ADH-PS measure of import exposure represent the effect of a 1 standard deviation change in exposure.

Consistent with ADH, column 1 shows that exposure to import competition significantly raised the non-employed share of the population ($p < .05$) while Columns 5-6 show that states seeing greater exposure saw larger declines in manufacturing employment. In total, and considering the joint hypotheses in Panel C, we find that the manufacturing population share fell by 1.5 percentage points (from a baseline of 13.1%). Comparing this magnitude with column

[19] Results are unchanged if we use more years to estimate premia or use a less rich specification.

[20] According the CPS, in 1990, 20% of manufacturing workers were union members, compared to 13% of workers employed outside of manufacturing. In 2014, the 9% of manufacturing workers belonging to a union is actually *less* than the 10% of non-manufacturing workers.

1, we find that non-employment accounts for 49% of the decline in manufacturing, suggesting many workers were not absorbed back into the labor market.

[Table 4 about here.]

Columns 2-4 consider the 51% of the manufacturing decline that was absorbed back into employment. Surprisingly, we find that non-manufacturing union members' population share rises by .3 percentage points, roughly a fifth of the manufacturing decline. Jointly, the coefficients in columns 3 and 4 are highly significant (Panel C; $p < .01$), and the public and private sectors account for equal shares of this increase. Interpreting this, it is important to note that, on average, states saw a 1.3 percentage points decline in non-manufacturing unionized share during this period. It is therefore more accurate to say that our estimates imply a one standard deviation increase in exposure would *offset* 0.3 percentage points (or 23%) of the decline.

Because unionized employment re-absorbs as much of the manufacturing decline as non-unionized, but starts at a much lower level, our results imply that exposure increases unionization outside manufacturing. Including the full range of effects on employment, manufacturing, and non-manufacturing, Panel C implies a significant ($p < .10$) increase of .5 percentage points in the unionized share of employment. The large relative gains outside manufacturing more than offset declines within it. Later, we ask whether this stems from would-be manufacturing workers themselves or their household members.

7.2 The role of Right-to-Work laws

On average, we find that Chinese import competition slowed the decline of state-level unionization. But this average may conceal local variation. We focus on one particularly important feature of the state-level environment: Right-to-Work (RtW) laws. These laws, long associated with lower unionization (Ellwood and Fine, 1987; Eren and Ozbeklik, 2016), were in place in 25 of 51 states.[21]

In Panel B of Table 4, we interact import exposure with RtW laws. As one would expect, import competition only increases non-manufacturing unionization in non-RtW states. There, it rises by .5pp ($p < .05$), compared to virtually no estimated increase in RtW states. This, however, is not the only difference in labor market adjustment.

Column 1 shows that the estimated effect on non-employment is nearly five times as large in RtW states (1.25 percentage points vs. 0.25 percentage points), a difference that is statistically significant at the 10% level. Columns 5 and 6 (and the joint hypothesis in Panel C) show

[21] We include laws passed through 2012. Only Oklahoma passed a RtW law during our sample (2001). As this early in our sample period, we include Oklahoma alongside the other 24 RtW states.

that manufacturing employment share declined more than twice as much in RtW states (2pp vs. 1pp), a difference that is significant at the 1% level. Combining these, nearly all of the additional manufacturing loss in RtW states (1 percentage point of the population) seems to have been absorbed into non-employment.

7.2.1 Explaining the RtW difference in manufacturing declines

Why did Chinese import penetration induce bigger manufacturing employment losses in RtW states? We consider four potential explanations: *i*) different levels of exposure; *ii*) different levels of initial manufacturing employment; *iii*) different industrial composition; and *iv*) product quality differences. We consider the first three to be forms of bias that would imply our heterogeneous RtW effects are spurious artifacts of the data. We find some support for these, but they are unable to explain the entirety. The fourth explanation, for which we also find support, carries a substantive economic interpretation.

First, do different estimated effects imply stem from different levels of exposure to import competition? No. The correlation between RtW status and import exposure is 0.06.[22] Figure 3 shows the relationship between import exposure and the change in manufacturing employment (Panel a) and non-employment (Panel b), separating RtW states (red crosses) from non-RtW states (blue circles). Not only is there a clear difference in the slopes, but there is also near-perfect overlap in the import competition between RtW and non-RtW states.

[Figure 3 about here.]

To gain further insight into cross-state differences, we construct a state-by-industry panel Using CBP. Our core sample is based on 308 SIC-defined industries across 51 states, and we again focus on employment changes from 1990 to 2014. Details on data construction are in the appendix.

The second potential explanation for the RtW heterogeneity in Table 4 is that RtW states may have had higher manufacturing employment to begin with (Holmes, 1998), perhaps disproportionately in high-exposure industries. In this case, import exposure could induce similar *percentage* changes in manufacturing employment in all states, but they would mechanically be larger *population share* changes in RtW states. In column 1 of Table 5, we regress the log of 1990 state-industry employment on import exposure, RtW status, and their interaction. The interaction is positive (indicating that RtW states had more employment in high exposure industries), though neither statistically significant nor especially large. Nonetheless,

[22] This is true for both measures of exposure separately. The correlation with RtW is .12 for the ADH instrument and -.01 for PS.

the remaining columns focus on changes in log employment to capture proportional changes, irrespective of initial levels.

[Table 5 about here.]

In column 2, we substantively replicate our state-level result from Table 4. Specifically, regressing the change in log employment on exposure, RtW, and their interaction, we find that the effects of exposure are significantly more negative in RtW states ($p < .01$). A one standard deviation increase in exposure implies a 36 log point (30%) decline in employment in non-RtW states, but a 62 log point (46%) decline. In other words, the effects of exposure on proportional changes in manufacturing employment are 53% larger in RtW states.

The third explanation we consider is that RtW and non-RtW states simply differ in industrial composition. Even if they have similar exposure to Chinese imports on average, it may still be the case that especially sensitive industries might be disproportionately found in RtW states. To address this concern, the column 3 includes industry fixed effects, so that coefficients represent employment declines *within the same industry* across RtW and non-RtW states.[23] Using only within-industry variation, the coefficient on the RtW/exposure interaction shrinks by a third, but remains statistically significant ($p < .05$). Compared to the main effect of exposure from column 2, the effects of exposure are now only 34% larger in RtW states (of course, with industry fixed effects we cannot estimate a main effect).[24] We calculate that the effects of exposure on employment are 34% larger in RtW states than in firms in non-RtW state in the same industry. Thus, some of the differential RtW/non-RtW effects are driven by mechanical biases, but a substantial portion reflects fundamentally different effects of import exposure.

The above three explanations test whether our differential estimated effects essential reflect statistical biases. Our final explanation, on the other hand, is grounded in both the literature on unions and the literature on import competition: differences in product quality. A long labor literature has suggested that unions improve worker productivity (Allen, 1984, 1986, 1987; Clark, 1980a,b, 1984) along with raising wages Card (1996). The trade literature has shown that higher paying, more productive firms tend to produce higher quality output (Kugler and Verhoogen, 2011) and that low-quality products face greater competition from low-wage country imports (Khandelwal, 2010; Amiti and Khandelwal, 2013). Combining these arguments, it is possible that lower-paying, non-unionized producers in RtW states face greater competition from Chinese goods.

[23] We see this as a conservative specification. A difference in sensitivity to imports across highly-exposed industries in RtW states compared to highly-exposed industries in non-RtW states might be an effect of RtW laws on industry-level outcomes, rather than a threat to identification.

[24] $[1 - \exp(-.357 - .159) = .403]/[1 - \exp(-.357) = .300] = 1.34$

We provide two types of evidence to support this explanation. First, we rely on within-industry, cross-state differences in average compensation as a proxy for product market quality. From the 1990 CBP, we calculate average quarterly compensation per worker within the industry-state. Necessary data are missing for roughly half the industry-state pairs in our sample, so column 4 replicates our column 3 specification on the subset of state-industries with compensation available. In column 5, we then interact compensation with exposure.[25] This interaction is significantly positive ($p < .05$), implying that the effects of import exposure are smaller in states with higher compensation, consistent with differential competition by product quality. The magnitudes are somewhat small. We estimate that within-industry, average compensation is about .4 standard deviations lower in RtW states. This implies that differential average compensation can explain about 3 log points of the 17 log point difference in the effects of import competition by RtW status. Given the noisiness of the CBP-based compensation measure, however, and the fact that compensation is a rough proxy for output quality, we think of this as a lower bound.

As a second approach, we use the well-known Rauch (1999) classification to identify industries producing homogeneous goods without meaningful quality differences (e.g., unprocessed lead) and those producing differentiated or branded goods (e.g., shoes). In column 6 we include a triple interaction between RtW status, industry-level import exposure, and whether the industry produces homogeneous goods. The results imply that, for an industry producing heterogeneous goods, the effects of exposure are 19 log points larger in RtW states ($p < .05$). For industries producing homogeneous goods, however, the effects of exposure are 15 log points *smaller* in RtW states ($p < .10$). Both findings are consistent with our product quality explanation. When there are meaningful product quality differences in the industry, low-wage producers in RtW states appear to specialize in low-quality products that face the most competition from China. When there cannot be meaningful quality differences, high-paying producers in non-RtW states are more affected because they are more susceptible to cost pressure from low-wage countries.

7.3 Robustness

In the appendix we present a variety of alternative specifications. We use each identification strategy to separately estimate the average state-level effects (Table A6), the heterogeneity by RtW status (Table A7), and the explanations for RtW heterogeneity (Tables A8 and A9). The

[25] We normalize our compensation variable to have minimum zero and unit standard deviation. Because our compensation measure has minimum zero, the RtW/exposure interaction reflects the differential effect of exposure by RtW status between two industry-states with very low compensation. The compensation/exposure interaction reflects how the effects of exposure change with a one standard deviation increase in compensation.

results are quite similar.

For all three outcomes where the aggregate import exposure measure in Table 4 yields significant effects, one of the two identification strategies produces significant estimates, while the other typically has a p-value of less than .2 and is of a similar magnitude. For none of the six estimated coefficients can we reject the null that the ADH and PS instruments produce the same estimate, and the joint hypothesis testing results are nearly identical. Patterns of heterogeneity by RtW status are also extremely similar. There are fewer statistically significant estimates, particularly for the joint hypotheses, but the magnitudes match our pooled estimates well. Results using the industry-state panel to understand RtW heterogeneity (Table 5) are also similar between the two strategies, though the interactions are rarely significant with just the PS variable.

Finally, we replicated our empirical specifications omitting non-manufacturing industries from the calculation of ΔChina-Other trade rather than setting it equal to zero (see fn. 15). We simply controlled for 1990 manufacturing share, to ensure that identifying variation does not simply reflect the importance of manufacturing as a whole. These results are similar.

8 Interpreting magnitudes

8.1 Decomposition methods

In this section, we interpret how changes in different parts of the labor market contribute to the nationwide decline in union density. To do so, we propose two decomposition strategies. These decompositions are mathematical identities that, in themselves, rely on no assumptions. Full derivations appear in the appendix.

First, we follow Berman, Bound, and Griliches (1994) to decompose the decline in union density within manufacturing into a within-industry component (driven by the fact that within any industry, Chinese import competition affects union members more than non-members) and a between-industry component (driven by the fact more unionized industries were relatively shielded from competition, and therefore experienced smaller declines). Specifically, we can write the change in union density within manufacturing as:

$$\Delta u_m = \underbrace{\sum_i \bar{s}_i \Delta u_i}_{\text{Within-industry}} + \underbrace{\sum_i \Delta s_i \bar{u}_i}_{\text{Between-industry}}$$

where u_i denotes union density in industry i, s_i denotes industry i's share of manufacturing employment, Δ denotes the change from 1990-2014, and $\bar{x}$ denotes the average level of a variable

$x \in \{u, s\}$, averaged between the two periods.

The first term captures the within-industry component; it is a weighted average of within-industry density declines, where the weights (based on industry size) are fixed over time. The second term captures the between-industry component; it is driven entirely by changes in the size of different industries, holding fixed each industry's density at its average level.

The second decomposition explains the change in union density for total employment (including non-manufacturing). Again, we can express this using the standard decomposition:

$$\Delta u = \bar{m}\Delta u_m + (1-\bar{m})\Delta u_{-m} + \Delta m \bar{u}_m + \Delta(1-m)\bar{u}_{-m}$$

where the subscript m denotes manufacturing, and the variable m denotes manufacturing's share of total employment. Since we (above) provide an expression for Δu_m, this decomposition can be rewritten into the following interpretable expression:

$$\Delta u = \underbrace{\bar{m}\sum_i \bar{s}_i \Delta u_i}_{\text{Within-industry}} + \underbrace{\bar{m}\sum_i \Delta s_i \bar{u}_i + \Delta m(\bar{u}_m - \bar{u}_{-m})}_{\text{Between-industry}} + \underbrace{(1-\bar{m})\Delta u_{-m}}_{\text{Out-of-manufacturing}}$$

The first term is the same within-industry component from above, but now weighted by manufacturing's share of total employment. This component reflects only changes in union density within manufacturing industries. The second term is a new, modified between-industry component. It reflects changes in each industry's share of manufacturing employment (the first part) as well as manufacturing's share of total employment (the second part), but is not affected by changes in union density within any industry (including within non-manufacturing). The third expression is the out-of-manufacturing component. It reflects only the change in union density within the non-manufacturing sector.

8.2 Decomposition results

These decomposition require only statistics from the raw data. Table 6 presents the results. In the first column we see there was a 12 percentage point decline in union density among manufacturing workers, overwhelmingly accounted for by within-industry deunionization. Holding each industry share constant, the average industry saw a 13 percentage point decline in union density, offset by a small increase from the between-industry component (i.e., a small increase in unionized industries' employment share). This compositional change was only enough to offset 7% of the union decline that would have happened had employment shares not changed.

[Table 6 about here.]

Across the broader economy, holding unionization fixed inside and outside of manufacturing, the decline in total manufacturing accounts for only 0.3 percentage points of the 4.5pp decline in overall union density (Column 2). This is because union density is only, on average, 3.5pp higher in manufacturing than outside of it. Thus, the raw collapse of the manufacturing sector (ignoring within-manufacturing unionization) explains little of US de-unionization *since 1990*. The large 13pp decline within the average industry was more important, but still explains less than half of the aggregate decline in density (largely because manufacturing was only one-sixth of total employment by 1990).

Reduced union membership outside of manufacturing accounts for the bulk (56%) of the overall decline in union density. At first glance, this suggests that Chinese imports are irrelevant for aggregate US unionization trends. But in Section 7, we found that import competition drove workers into unionized jobs outside of manufacturing. We turn to assessing these factors' relative importance.

8.3 Counterfactual simulations

We construct a counterfactual scenario in which we set each manufacturing industry's exposure to the sample minimum (this avoids out-of-sample extrapolation of our estimates), and use our estimates from above to calculate what the components of the decomposition would be in this counterfactual.[26] Table 6 reports our results. Within-manufacturing union density would have declined by 10 percentage points. In other words, we estimate that 83% of the decline in manufacturing density would have occurred even without import competition. Combining the within-manufacturing-industry effects and the between-manufacturing-industry effects, total declines in manufacturing density account for only 0.3pp (or 7%) of the 4.5pp decline we see in the data.

A much larger effect comes from effects outside of manufacturing. There, we estimate the counterfactual decline would have been 2 percentage points larger *without* import competition. Combining within-manufacturing and outside-of-manufacturing effects, we estimate the nationwide decline in union density would have been 1.6 percentage points greater with minimal Chinese import exposure.

[26] Between-industry effects are from Table 3's estimates for total employment; within-industry effects are from Table 3's estimates for union density. We estimate effects on union share outside manufacturing using specifications analogous to those from Table 4.

9 Employment spillovers outside of manufacturing

Our decompositions suggest the most important part of the story is the effects of import competition on unionization *outside of manufacturing*. How should we interpret this? Is it driven by a reallocation of workers who would otherwise be in manufacturing? Or is it more likely that declining manufacturing drives *other* household members towards unionized work?

In this section, we provide evidence that family members of would-be manufacturing workers, rather than the workers themselves, explain the shift. We also show that the mechanism is less-educated women leaving retail for the healthcare and education–relatively unionized fields paying the highest wages available to these workers. This can also help explain why non-employment absorbs more of the lost jobs in RtW states: these fields do *not* pay higher wages in RtW states, so they are less attractive for household members.

9.1 Identifying manufacturing-type workers

We use a machine-learning approach to identify workers most directly affected by the manufacturing decline. Specifically, we use the 1990 CPS to estimate the probability that an individual works in manufacturing (in the baseline period) based on state of residence and a rich set of demographics. We then use the estimated model from the 1990 data to produce predicted probabilities of working in manufacturing among the 2014 respondents. To be clear, our goal is not to identify individuals who actually worked in manufacturing 25 years earlier. Rather, we seek to identify *types* of individuals who likely *would have* worked in manufacturing had they lived in the same state with the same demographic characteristics in 1990. These individuals are affected by the decline of manufacturing, which reduces job opportunities available to them, regardless of whether they themselves have ever had a manufacturing job. We also identify individuals who themselves are unlikely to work in manufacturing (based on our estimated probability model), but who have a household member who is likely to do so.

The appendix describes our procedure in detail. Briefly, we use the 1989-1991 CPS, a rich set of covariates, and a LASSO model to estimate the probability of employment in manufacturing.[27] We apply our estimated probability model (based on the 1990 data) to the 2013-2015 CPS sample, calculating the predicted probabilities of manufacturing for each respondent. We refer to respondents in the top 10% of predicted probabilities as "manufacturing-type workers." We think of these as the individuals who likely *would have* worked in manufacturing had they looked the same in the past and had the labor market not changed; thus, they were particularly acutely affected by import competition.[28] Our approach follows in the tradition of the well-

[27] Covariates include state, age, education, race, sex, marital status, and some two- and three-way interactions.

[28] We interpret our results here suggestively. We recognize that many of the observable characteristics used

known DiNardo, Fortin, and Lemieux (1996) decomposition.[29] Our second use of the estimated probability model is to identify household members of manufacturing-type workers. Specifically we refer to anyone with below median predicted manufacturing probability but who lives with a manufacturing-type worker as a "household member."

Panel A of Table 7 characterizes manufacturing-type workers and household members, comparing them to the general population in 1990 and 2014. Our estimated probability model performs well; in both time periods, manufacturing-type workers are two and a half times more likely than the full population to work in manufacturing. These workers differ from the full population in many ways. They are almost almost entirely male, somewhat older, more likely to be married, more likely to be White, and less educated, on average. Household members, on the other hand, are overwhelmingly female (85%), and are younger than and similarly educated to the full population. Our sample of household members is younger, more gender-balanced, and less likely to be married than the manufacturing-type workers, suggesting household members includes children in addition to spouses.

[Table 7 about here.]

9.2 Changes in manufacturing-type workers' employment patterns

Panel B of Table 7 characterizes labor market outcomes. We see that the employment share among manufacturing-type workers fell by 6pp from 1990 to 2014 (compared to 4pp among the full population). For household members, on the other hand, the employment-population ratio decreased only slightly (less than 1pp). While household members are not working enough to offset manufacturing declines, they also do not explain the declining employment-population ratios from Table 4.

The remainder of Panel B shows the union membership status of individuals. Among manufacturing-type workers, there is a large decline both for those within manufacturing (19pp, 58%) and outside of it (12pp, 49%). The declining membership rates we see among these workers both in non-manufacturing and in total (15pp, 54%) exceed the declines observed in the full population (non-manufacturing: 4.6pp, 30%; total: 5.9pp, 36%). This implies that manufacturing-type workers themselves are unlikely candidates for the relative increase in non-manufacturing union density that we linked to Chinese import competition.

in our probability model are likely to be themselves affected by the manufacturing decline (see Autor, Dorn, and Hanson (2019) for evidence on marriage, Amior and Manning (2018) for evidence on place of residence, and Atkin (2016) for evidence on education).

[29] Böhm (2018) uses a similar approach to estimate how earnings have changed for individuals who *would have* been well-suited for routine jobs in decades past.

On the other hand, household members saw virtually no decline in union membership among total employment (0.1pp, 1%) or outside of manufacturing, where it actually increased slightly (0.3pp, 3%). Thus, among non-manufacturing-type workers who live with a manufacturing-type worker, union density held constant at a time of massive membership declines in the overall population. Union membership declined outside manufacturing, simply not by as much as it would have otherwise. Our results in Table 7 are consistent with this finding: while unionization was collapsing around them, spouses and children of would-be manufacturing workers still end up in unionized jobs in 2014 at about the same rate as in 1990.

To explore the source of these shifts across workers, we calculate the change in industries' employment shares from the 1990 to the 2014 among these populations. The 3-digit Census-defined industries seeing the biggest changes are displayed in Table 8. Manufacturing-type workers saw a 15.5pp decline in manufacturing's share. Roughly 40% of this seems to have been absorbed into non-employment, rising from 12.5% to 18.9% among this population. Combined with non-employment, five industries account for 90% of the shift away from manufacturing. Construction is the largest share, seeing a 2.8pp increase. Construction has median wage and unionization rate similar to manufacturing (columns 4 and 5), so a shift into construction has very little effect on household income or on aggregate union density.

[Table 8 about here.]

This is not true for the other industries. Employment at eating and drinking places, in landscaping, and in auto repair saw a collective 3.8pp increase in their population shares for manufacturing-type workers, a third larger than the shift into construction. These industries have low unionization rates (around 2% in 1990) and much lower wages than manufacturing. Only one of the top industries has a wage higher than manufacturing (computer processing services, which absorbs .8pp) and it, too, has a low union density (1.3%). Much of the decline in unionization among manufacturing-type workers is due to shifts into low-paying industries where unions are rare.

For household members, on the other hand, we find large increases in employment in relatively unionized sectors. Of the 10 narrow industries seeing the biggest increase, six are in the education or health sectors, which saw 2.5pp and 2.2pp increases, respectively. Both then and now, education has unionization rates more than double the national average. Union density in healthcare is not especially high, but, unlike the rest of the economy, it remained relatively stable over the past 25 years (falling by 2.6pp, compared to 5.9pp in the overall labor market).

Our state-level results (Table 4) and decomposition (Table 6) showed that exposure to Chinese import competition slowed the decline in unionization. In light of Tables 7 and 8, we conclude that would-be manufacturing workers (who overwhelmingly reallocate to non-union sectors) do not drive this relationship. Rather, these workers' household members relocated into industries with high and/or stable union density.

Although we cannot definitively say whether household members chose jobs based on wages or union opportunities, we provide suggestive evidence that they tended toward relatively high-wage industries, which happened to be relatively unionized. In Table 9, we regress each industry's change in population shares among household members (1990-2014) on its 1990 median wage and union density (both normalized to have unit standard deviation).[30] Column 1 shows that an industry with a median wage one standard deviation higher saw 0.45pp more growth ($p < .01$). Column 2 shows that an industry with one standard deviation higher union density saw 0.38pp more growth ($p < .10$), a similar magnitude. Conditioning on both in column 3, the coefficient on median wages falls by 20% and remains significant ($p < .05$), while the coefficient on union density falls by half and is no longer significantly different from zero ($p = .383$). We see this as suggestive evidence that it is higher wages, rather than unionization itself, which attracted these individuals.

[Table 9 about here.]

9.3 Differential effects of state-level exposure

Are these shifts more pronounced in states experiencing greater import exposure? To answer this questions, we estimate individual-level regressions in which we interact state-level exposure with individual-level probability of being a manufacturing worker. Specifically, for an individual j living in state s at time t, we estimate:

$$\begin{aligned} Y_{jst} = \alpha_s + \delta_t &+ \beta_1\big(\text{Exposure}_s \times 1\{t = 2014\}\big) \\ &+ \beta_2\big(\text{Exposure}_s \times 1\{t = 2014\} \times \text{ManufProb}_{js}\big) \\ &+ \beta_3\big(\text{Exposure}_s \times 1\{t = 2014\} \times \text{Max-HH-ManufProb}_{js}\big) \\ &+ \gamma_1 \text{ManufProb}_{js} + \gamma_2 \text{Max-HH-ManufProb}_{js} + \varepsilon_{jst} \end{aligned}$$

where ManufProb_{js} is the estimated probability that individual j works in manufacturing–based on observed demographics, state-of-residence, and the probability model estimated in 1990– and $\text{Max-HH-ManufProb}_{js}$ is the maximum manufacturing-type probability of *other* members within j's household (that is, it is the maximum across individuals other than j).[31]

We include state and time fixed effects to isolate the effect of exposure on later-cohort outcomes, after adjusting for time-invariant cross-state differences and aggregate changes over

[30] Figure A1 non-parametrically shows these changes among household members. It shows the largest declines were concentrated in retail and even within broad sectors, higher paying industries saw more growth.

[31] For individuals with no other household members, we set this probability equal to zero.

time. We control for ManufProb_{js} and Max-HH-ManufProb_{js} to account for the possibility that employment outcomes of manufacturing-type individuals and household members always differ from the full population.

For interpretation, we normalize our estimated probability of manufacturing employment so that the sample maximum is one and the sample minimum is zero. With this normalization, β_1 captures the effect of exposure on the 2014 outcomes of an individual with the minimum probability of being in manufacturing themselves, and where no other household members are likely to work in manufacturing. $\beta_1 + \beta_2$ captures the effects of exposure on the outcomes of an individual with the maximum estimated probability of manufacturing employment, but with no manufacturing-type household members. Conversely, $\beta_1 + \beta_3$ captures the effects of exposure on individuals who themselves have the minimum probability of employment in manufacturing, but for whom one household member has the maximum probability of manufacturing employment.

Table 10 presents our results, focusing on the patterns identified in Section 9.2. In column 1, we find that a one standard deviation increase in exposure has no significant effect on employment of individuals who are unlikely to work in manufacturing or have a household member who would. However, the effects on both manufacturing-type workers and their household members are significantly more negative: by 2pp ($p < .01$) and .3pp ($p < .05$), respectively. In other words, dramatic employment declines are concentrated among manufacturing-type workers, with small declines among household members, similar to the results in Table 7.

[Table 10 about here.]

The remaining four columns of Table 7 include only employed individuals. In column 2, we focus on the three low-wage, low-unionization service industries identified in Table 8: eating and drinking establishments, landscaping, and automotive repair. We estimate that import exposure significantly reduces the probability that non-manufacturing-type workers are in these low-skilled service industries, but that it increases by 1.1pp the probability that manufacturing-type workers are. In column 3 we focus on health and education industries. The main effects show a 1.4pp increase in employment (per standard deviation of exposure), but this effect is 40% larger for household members (along with a small decrease among manufacturing-type workers).

Columns 4 and 5 summarize the overall characteristics of the industries in which employed individuals work. Specifically, column 4 is based on the 1990 union density in each industry, and column 5 is based on the 1990 median wage (in 2015 dollars) in each industry. Column 4 shows that import exposure pushes workers into industries that were more unionized in 1990. For non-manufacturing-type, non-household-members, one standard deviation increase in exposure pushes the average worker into an industry with a 0.9pp higher 1990 union density.

When we focus on manufacturing-type workers, however, this story reverses completely. For an employed manufacturing-type worker, one standard deviation of exposure reduces the average 1990 unionization in the industry of employment by 0.4pp. Consistent with our descriptive work, we find that, compared to the general population, exposed household members find themselves employed in industries with significantly higher average union density (increasing the effects of exposure by 13%). Finally, column 5 looks at median wages in the industry of employment. For non-manufacturing workers, exposure pushes workers into slightly higher-wage industries: Roughly \$0.20 (or 4% of a standard deviations of the cross-industry wage distribution) per standard deviation of exposure. Manufacturing-type workers, on the other hand, are pushed into lower-wage industries (by \$0.25 per standard deviation of exposure). As with industry union density we find effects on household members' industry wages are 18% larger than the rest of the population ($p < .01$).

9.4 Explaining Right-to-Work heterogeneity

Our results suggest that household members flowing into relatively high wage industries–where unionization happened to be high and stable–was a key channel of labor market adjustment in response to Chinese imports and the associated decline in manufacturing. Earlier, we found that employment less effectively absorbed lost manufacturing jobs in RtW states. Are these patterns related?

In Table 11 we revisit the RtW difference by estimating wage premia in health and education industries–differentially for RtW and non-RtW states–in 1990. To focus on the relevant population, we restrict the sample to women (85% of household members we identify in Table 7) with a high school education or less (55% of household members).

In column 1 we find that, on average, jobs in health and education pay 5.2% higher hourly wages ($p < .01$). In column 2, we show that this is entirely driven by non-RtW states, where the premium is 7.2% ($p < .01$) and 5.6pp ($p < .01$) higher than in RtW states. When we add state fixed effects and a rich set of controls (column 3), we see that most of the *average* premium is explained away. Column 4, however, shows that there is still a modest premium in non-RtW stats (3%, $p < .01$), counterbalanced by a 5pp *lower* ($p < .01$) premium in RtW states.

[Table 11 about here.]

Overall, exposure to Chinese imports pushes manufacturing-type workers into low-wage, non-unionized industries while household members of these workers end up working disproportionately in higher-wage, more unionized industries, especially healthcare and education. These relatively-unionized industries do pay higher wages, but only in non-RtW states. This represents a partial explanation for the weaker labor market adjustment to the import shock that we observed in RtW states.[32]

10 Conclusion

We provided the first causal estimates of the effect of Chinese import competition on unionization within and outside of manufacturing. We found that less unionized industries bore the brunt of the import competition; this differential exposure is largely accounted for by industry variation in capital-intensity, skill-intensity, and the unusual experiences of the textiles sector. Within an industry, however, import penetration affected employment of union members more than non-members. Overall, our results imply that Chinese import competition can explain around 17% of the decline in unionization within manufacturing between 1990 and 2014.

While important, this represents only a small part of the story. A quantitatively bigger effect is that Chinese import competition reduced de-unionization outside of manufacturing. Since manufacturing is less than a fifth of the economy, the net effect is that overall declines in unionization would actually have been *larger* without Chinese import competition.

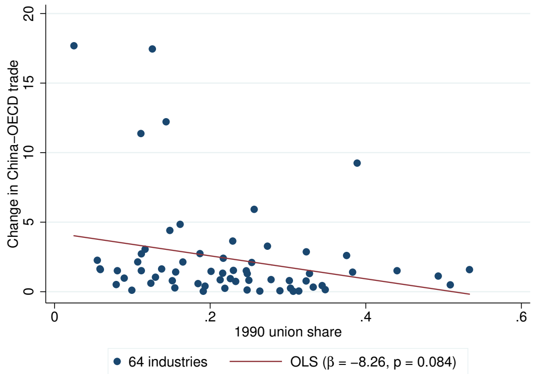
We provided a series of analyses to characterize how this occurred. We found that that those who were likely manufacturing workers disproportionately ended up in non-employment, construction, and low-wage, low-unionization services. These workers' household members accounted for the rising non-manufacturing unionization as they ended up disproportionately in higher paying industries, especially the relatively unionized healthcare and education sectors. We have no evidence that these shifts are related to union density, in itself, rather than the higher wages they offer. Our interpretation, then, is that spouses and children attempted to offset declining income from collapsing manufacturing by taking the best-paying positions they could find.

Finally, our results highlight the importance of state laws for understanding the labor market consequences of adverse shocks. We showed that states with right-to-work laws saw greater increases in non-employment per manufacturing job lost. Part of the explanation is that is that the effects of import exposure on manufacturing were larger in these states (because of differential competition with low-quality Chinese goods), making it more difficult for the labor market to absorb workers. But it also appears that, in these states, healthcare and education are less unionized and enjoy smaller wage premia, and so it is possible that the members of manufacturing households simply had no access to high paying sectors towards which they

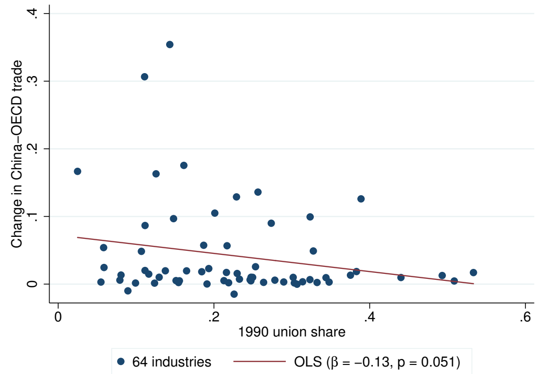
[32] When we estimate the effects of state-level exposure on individuals' employment in healthcare and education (like column 3 of Table 10) including interactions with RtW states, we find that the interaction with Exp. $\times$ '14 $\times$ Max HH Man. Prob. is negative (suggesting these workers were less likely to flow into health/education in RtW states), but not statistically significant ($p = .272$).

could reallocate.

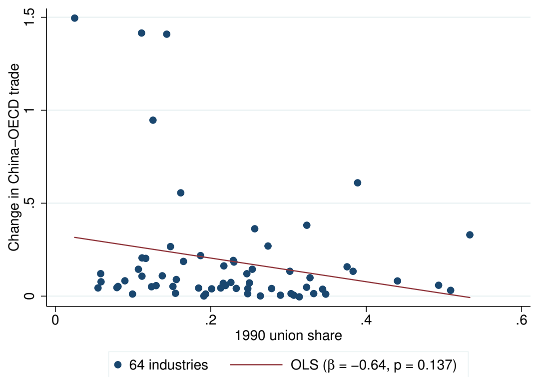
Figure 1: Autor-Dorn-Hanson instrument and lagged unionization



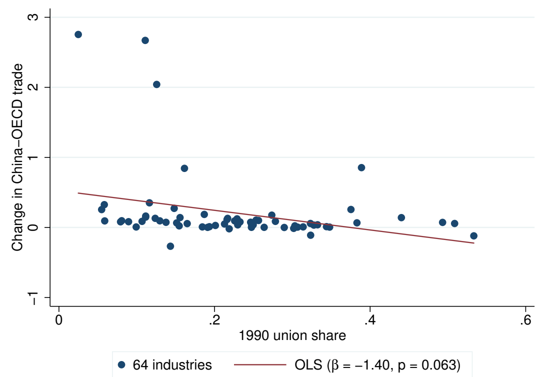
(a) Change 1990-2014



(b) Change 1990-2000



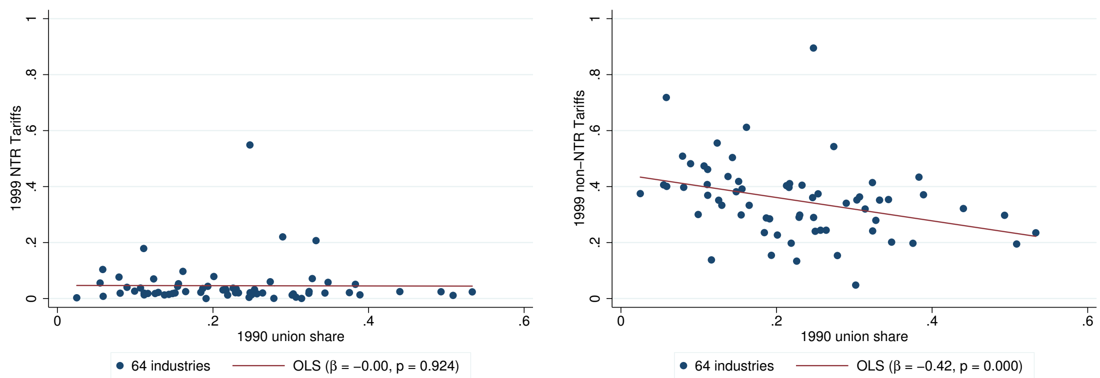
(c) Change 2000-2007



(d) Change 2007-2014

Line is OLS.

Figure 2: Pierce-Schott instrument and lagged unionization

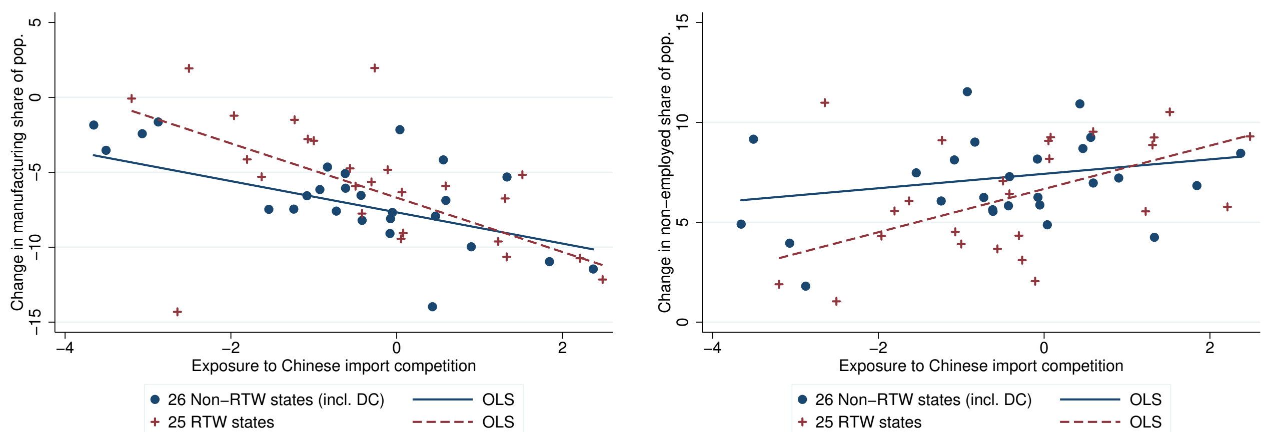


(a) NTR tariffs

(b) non-NTR tariffs

Line is OLS.

Figure 3: Chinese import competition and labor market declines by RtW status



(a) Manufacturing employment

(b) Non-employment

Right-to-Work (RtW) status includes only those laws implemented 2001 or earlier. Exposure to Chinese import competition is defined as the composite measure (combining the ADH and PS measures) described in Section 5, and has (weighted) mean zero and unit standard deviation by construction. OLS regressions weighted by 1990 state population. Slopes are significantly different (corresponding regressions are in Table 4); Panel (a): $p < .05$, Panel (b): $p < .10$.

Table 1: Replicating existing results

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Δ China-US Trade		Δ log(Employment)			
	Panel A: Autor-Dorn-Hanson identification strategy					
Δ China-Other Trade	1.340***	1.561***	-0.052***	-0.064***	-0.035**	-0.051***
	(0.110)	(0.061)	(0.012)	(0.017)	(0.014)	(0.010)
Δ Ch.-Oth. (other ind.)					-0.034**	
					(0.015)	
R^2	0.869	0.963	0.115	0.203	0.137	0.136
N	357	64	357	64	357	64
F-stat	148.7	655.9				
St. dev. of X_{own}	4.36	4.17	4.36	4.17	4.36	4.17
St. dev. of X_{other}					3.53	
	Panel B: Pierce-Shott identification strategy					
NTR Gap	8.901***	14.276**	-1.794***	-3.254***	-0.582	-1.471*
	(2.549)	(6.188)	(0.376)	(1.138)	(0.362)	(0.816)
NTR Gap (other ind.)					-2.140***	
					(0.482)	
R^2	0.029	0.049	0.113	0.323	0.194	0.068
N	350	64	350	64	350	64
F-stat	12.2	5.3				
St. dev. of X_{own}	0.12	0.10	0.12	0.10	0.12	0.10
St. dev. of X_{other}					0.11	
Industries	SIC	Census	SIC	Census	SIC	Census
Emp. data			ASM	ASM	ASM	CPS

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014. All regressions weighted by industry employment in 1990. "Other industries" refers to other SIC industry codes within the same census industry code. "F-stat" refers to the F-statistic testing the null that ΔChina-Other Trade or the NTR Gap has no effect on ΔChina-US Trade.

Table 2: Explaining the correlation between 1990 unionization and trade instruments

	(1)	(2)	(3)	(4)	(5)
DV:	1990 Union Density (members as share of employment)				
	Panel A: Autor-Dorn-Hanson instrument				
Δ China-Other Trade	-0.082***	-0.068**	-0.071**	-0.073**	-0.030
	(0.028)	(0.028)	(0.029)	(0.029)	(0.025)
Skill share (1990)		-0.205*			-0.439***
		(0.116)			(0.109)
Capital-labor ratio (1990)			0.012*		0.010*
			(0.006)		(0.005)
Textiles, apparel, leather				-0.126***	-0.192***
				(0.026)	(0.031)
R^2	0.068	0.109	0.116	0.171	0.357
N	64	64	64	64	64
Coefficient magnitude (relative to baseline)		0.831	0.869	0.894	0.363
	Panel B: Pierce-Schott instrument				
Non-NTR Tariff Rate (1999)	-0.286**	-0.335***	-0.239**	-0.197*	-0.139
	(0.110)	(0.124)	(0.108)	(0.110)	(0.087)
Skill share (1990)		-0.320***			-0.465***
		(0.103)			(0.099)
Capital-labor ratio (1990)			0.008		0.008
			(0.007)		(0.005)
Textiles, apparel, leather				-0.091***	-0.172***
				(0.031)	(0.031)
R^2	0.119	0.222	0.137	0.162	0.368
N	64	64	64	64	64
Coefficient magnitude (relative to baseline)		1.170	0.837	0.687	0.486

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. Skill share is non-production workers as a share of all workers. Capital-labor ratios and skill shares are drawn from the Annual Survey of Manufacturing (ASM). "Coefficient magnitude (relative to baseline)" compares the coefficient on Δ China-Other Trade (Panel A) and Non-NTR Tariff Rate (Panel B) with that from column 1. To improve the display of estimated coefficients, this table measures growth in China-Other Trade in *hundreds* of thousands of dollars per worker. The rest of the paper measures growth in *tens* of thousands of dollars per worker, following ADH.

Table 3: Import effects on manufacturing industry-level unionization

	(1)	(2)	(3)	(4)	(5)
DV:	Δ ln(Employment)			Change in Union member share	
	Total	Union mem.	Non-mem.		
Import exposure	-0.203***	-0.459***	-0.192**	-0.014***	-0.012**
	(0.075)	(0.118)	(0.076)	(0.005)	(0.005)
R^2	0.164	0.337	0.265	0.861	0.871
N	64	64	64	64	64
Controls:					
Union mem. (1990)	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, weighted by 1990 industry employment; and condition on 1990 union share. Column (5) conditions on the covariates considered in Table 2. Import exposure combines the NTR Gap and the ADH ΔChina-Other Trade, and has unit standard deviation across industries.

Table 4: State-level effects of exposure to import competition

	(1)	(2)	(3)	(4)	(5)	(6)
DV: Change in share of working age population	Non-emp.	Non-manuf., non-union members	Non-manuf., non-public, union mem.	Non-manuf., public sect., union mem.	Manufact., non-union members	Manufact. union members
DV mean in 1990	28.0	51.1	3.8	4.0	10.5	2.6
Avg change '90-'14	3.9	3.0	-0.8	-0.5	-3.7	-1.9
	Panel A: Average effects					
Import exposure	0.721**	0.434	0.172*	0.152	-1.524***	0.045
	(0.300)	(0.270)	(0.088)	(0.098)	(0.303)	(0.153)
R^2	0.134	0.044	0.075	0.062	0.463	0.001
N	51	51	51	51	51	51
	Panel B: Heterogeneity by RtW status					
Import exposure	0.243	0.257	0.257	0.211	-1.164***	0.196
	(0.362)	(0.457)	(0.167)	(0.171)	(0.232)	(0.222)
Right-to-work	-0.017	-0.018	0.008**	0.003	0.006	0.018**
	(0.011)	(0.011)	(0.004)	(0.005)	(0.011)	(0.008)
RTW $\times$ Exp.	0.959*	0.391	-0.186	-0.121	-0.700	-0.342
	(0.490)	(0.552)	(0.177)	(0.190)	(0.459)	(0.271)
R^2	0.211	0.093	0.187	0.072	0.539	0.186
N	51	51	51	51	51	51

Panel C: Joint hypothesis testing

	Mean	Average	Heterogeneity	
	(1990)	effect	Non-RtW	RtW
Manufacturing (as share of pop.):	13.1	-1.479***	-0.968***	-2.010***
$\beta_{(5)} + \beta_{(6)}$		(0.252)	(0.147)	(0.342)+++
Non-manuf. union mem. (as share of pop.):	7.8	0.324***	0.468**	0.161
$\beta_{(3)} + \beta_{(4)}$		(0.119)	(0.195)	(0.109)
Union membership (as share of emp.):	14.5	0.538*	1.003*	0.002
$\left(\beta_{(3)} + \beta_{(4)} + \beta_{(6)}\right)/\left(1 - \beta_{(1)}\right)$		(0.312)	(0.543)	(0.203)+

*,+ $p < .10$, **,++ $p < .05$, ***,+++ $p < .01$. Stars (*) test whether coefficient is significantly different from zero. Crosses (+) test whether RtW effect is significantly different from non-RtW effect. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, are weighted by state employment in 1990, and are based on prime age persons (age 16-64). "States" includes the District of Columbia. Coefficients in columns 1-6 sum to zero because the population shares sum to one (i.e., those six groups are mutually exclusive and exhaustive). To calculate exposure, we first standardized the state-level "NTR Gap" exposure measure to have standard deviation 1 across states. Next, we standardize the state-level ADH "ΔChina-Other Trade" exposure measure to have standard deviation 1 across states. We then sum the two measures, and standardize the sum to have standard deviation 1 across states. Results based on these two measures disaggregated can be found in Tables A6 and A7.

Table 5: Explaining Right-to-Work heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	$\ln(Emp)_{is,90}$	$\Delta \ln(Emp)_{is}$				
Exposure_i	-0.166	-0.357***				
	(0.138)	(0.094)				
RTW_s	-0.454*	-0.111	0.049	0.110	0.036	0.027
	(0.237)	(0.126)	(0.089)	(0.090)	(0.089)	(0.091)
$\text{Exp}_i \times \text{RTW}_s$	0.100	-0.266***	-0.159**	-0.174**	-0.142*	-0.188**
	(0.140)	(0.059)	(0.070)	(0.083)	(0.084)	(0.082)
Avg. compensation_{is}					-0.176***	
					(0.057)	
$\text{Exp}_i \times \text{Comp}_{is}$					0.068**	
					(0.029)	
$\text{RTW}_s \times$ Homogeneous goods_i						0.290**
						(0.120)
$\text{Exp}_i \times \text{RTW}_s \times \text{Homogen}_i$						0.343**
						(0.130)
R^2	0.030	0.115	0.669	0.772	0.776	0.674
N	11062	11062	11062	5027	5027	10516
Industry fixed effects			Yes	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Results based on panel of industry-state pairs, where industries are based on SIC definitions and states include DC. Columns 2-6 based on changes in log employment from 1990 to 2014. All regressions weighted by 1990 employment in the industry-state. Standard errors (based on two-way clustering on state and industry) are shown in parentheses. Core results are based on 308 industries, although half of industry-state pairs are missing compensation data. Employment is from the CBP. "Exposure" is based on ADH and PS instruments, and has unit standard deviation across industries by construction. "Average compensation" refers to total first quarter payroll divided by March 1 employment (both from CBP), and is normalized to have minimum zero and unit standard deviation across the sample. "Homogeneous goods" is based on the definition from Rauch (1999). Only one Right-to-Work state passed legislation during our period (Oklahoma, 2001); the rest passed it prior to 1990.

Table 6: Effects of import competition on changes in union density

	Actual change (observed in data)		Counterfactual change (exposure set to minimum)	
Channel	manufacturing	total emp.	manufacturing	total emp.
Between-industry	0.9	-0.1	0.7	0
within-manuf.		(0.1)		(0.1)
manuf. vs. non-man.		(-0.3)		(-0.1)
Within-industry	-13.2	-1.9	-10.9	-1.6
Outside-of-manuf.		-2.5		-4.5
Total	-12.3	-4.5	-10.2	-6.1

Estimates of the between-industry effects are based on column 1 of Table 3. Estimates of within-industry effects are based on column 5 of Table 3. Estimates of outside-manufacturing effects are based on columns 2-4 of Table 4. The numbers in parentheses are sub-components of the between-industry effect (one for changing the relative size of manufacturing industries but holding the whole manufacturing sector fixed, and one for changing the size of the manufacturing sector relative to non-manufacturing). They sum to the full between-industry effect, and do not separately enter the calculation of the total. For the counterfactual change, we set each industry's exposure is equal to the sample minimum across industries. Figures may not sum exactly due to rounding.

Table 7: Characteristics of manufacturing-type workers and household members

	(1)	(2)	(3)	(4)	(5)	(6)
Group:	Full sample	Manuf.-type person	Non-man. in manuf. household	Full sample	Manuf.-type person	Non-man. in manuf. household
	Panel A: Demographic characteristics					
Year:	1990			2014		
Manufacturing	.138	.345	.068	.073	.191	.044
Male	.472	.984	.157	.488	.970	.118
Age	36.4	40.0	29.2	39.7	43.3	34.3
Married	.560	.892	.552	.500	.811	.613
Black	.126	.083	.067	.141	.088	.071
Hispanic	.105	.104	.062	.173	.205	.109
Education						
HS or less	.605	.757	.548	.439	.693	.356
Some college	.204	.148	.278	.286	.202	.338
College degree	.191	.095	.173	.292	.138	.314
	Panel B: Labor market outcomes					
Year:	1990			2014		
Employed	.695	.875	.610	.655	.811	.601
Union membership						
Among all individuals	.113	.241	.067	.069	.102	.066
Among the employed	.163	.275	.110	.104	.126	.109
Among manufacturing workers	.209	.326	.112	.093	.136	.056
Among non-manufacturing workers	.152	.242	.110	.106	.123	.113

Calculations based on 1989-1991 and 2013-2015 CPS samples. “Manufacturing-type persons” are those with estimated probabilities of working in manufacturing (based on demographics and the 1990 probability model) above the cohort-specific 90^{th} percentile. “Non-manufacturing in manufacturing household persons” are those with estimated probabilities below the cohort-specific median, but for whom at least one household member has an estimated probability above the cohort-specific 90^{th} percentile.

Table 8: Industrial composition among manufacturing-type workers and household members

	(1)	(2)	(3)	(4)	(5)
Industry (includes non-employment)	Share of pop. 1990	2014	Change in pop. share	Median wage (1990)	Union share (1990)
	Panel A: Manufacturing-type workers				
Manufacturing	35.4%	19.1%	-15.5 pp	$18.14	20.1%
Non-employed	12.5	18.9	6.4		
Construction	8.8	11.5	2.8	18.47	22.4
Eating and drinking places	1.7	3.5	1.9	8.13	1.8
Landscaping	0.3	1.7	1.3	11.47	2.5
Computer processing services	0.4	1.2	0.8	26.30	1.3
Automotive repair	1.0	1.6	0.6	14.34	2.5
Cumulative			13.7		
	Panel B: Non-manuf. indiv. in manuf.-type households				
Health services	1.1%	2.9%	1.8 pp	$17.40	11.1%
Elementary & secondary schools	5.3	7.0	1.7	19.12	45.1
Non-employed	39.0	39.9	0.8		
Child day care services	0.7	1.2	0.5	9.56	2.9
Social services	0.5	1.0	0.5	16.03	15.1
Entertainment/recreation	0.7	1.1	0.5	10.96	9.4
Hospitals	5.1	5.6	0.4	19.12	14.6
Offices of physicians	0.9	1.2	0.3	15.54	1.3
Government offices	0.1	0.4	0.3	19.59	12.2
Educational services	0.1	0.3	0.3	18.17	6.4
Colleges & universities	1.6	1.8	0.2	17.40	12.3
Education (total)	9.4	11.9	2.5	18.32	34.3
Health (total)	7.1	9.3	2.2	16.55	11.6

Calculations based on 1989-1991 and 2013-2015 CPS samples with estimated probabilities of working in manufacturing (based on demographics and the 1990 probability model) above the cohort-specific 90^{th} percentile. Table displays the top industries in terms of change in population share from 1990-2014. Industries are based on 3-digit 1990 CPS industry codes (n=235). Wages are in 2015 dollars. "Government offices" is more conventionally called "Executive and Legislative Offices," which is defined as "government establishments serving as councils and boards of commissioners or supervisors and such bodies where the chief executive is a member of the legislative body." Median wages and union shares (1990) both refer to the full population (not the subset of the population isolated for the calculations in columns 1-3).

Table 9: Explaining household members' choice of industries

	(1)	(2)	(3)
DV:	100 × Δ Pop. share ('90-'14)		
Median wage (1990)	0.449***		0.347**
	(0.136)		(0.141)
Union density (1990)		0.378*	0.203
		(0.200)	(0.232)
R^2	0.321	0.227	0.370
N	201	201	201

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. Calculations based on 201 3-digit Census industries. Regressions weighted by industries' 1990 population share. We focus on "household members" (those for whom the estimated probability of working in manufacturing is below median, but for whom at least one household member has an estimated probability above the 90^{th} percentile), and calculate the change in each industry's employment share of this population, and relate that to industry median wages and union density, both measured in 1990. Both wages and union density have been normalized to have unit standard deviation across industries.

Table 10: Effects of state-level exposure for manufacturing-type and household-members

	(1)	(2)	(3)	(4)	(5)
		Conditional on employment			
		100×			
DV:	1{Emp.}	Service jobs	Health or Educ.	Industry union den.	Industry wages
Exposure × 1{Year=2014}	0.567	-0.728***	1.438***	0.943***	0.234***
	(0.509)	(0.174)	(0.261)	(0.092)	(0.070)
Exp. × '14 × Man. Prob.	-2.041***	1.855***	-1.869***	-1.380***	-0.488***
	(0.547)	(0.115)	(0.171)	(0.097)	(0.027)
Exp. × '14 × Max HH Man. Prob.	-0.328**	0.102**	0.593***	0.126***	0.042***
	(0.128)	(0.049)	(0.083)	(0.032)	(0.010)
R^2	0.059	0.006	0.070	0.020	0.032
N	1481638	1016580	1016580	1010775	1010775
DV mean (1990)	69.4	6.2	17.1	16.3	16.7
p for H_0: $\beta_1 + \beta_2 = 0$	0.000	0.000	0.070	0.003	0.001
p for H_0: $\beta_1 + \beta_3 = 0$	0.652	0.000	0.000	0.000	0.000

* $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors clustered at the state level are in parentheses. All regressions based on ORG respondents in 1989-1991 and 2013-2015 and use sample weights. "Manufacturing Probability" is an individual's estimated probability of working in manufacturing based on demographics, state-of-residence, and the probability model estimated on the 1990 sample. "Max HH Manufacturing Probability" is the maximum manufacturing probability across *other* individuals in the household (excluding oneself), or zero for single-person households. "Service jobs" refers to eating and drinking places, landscaping, and automotive repair (see Table 8). Health and education based on 2-digit Census industry codes. Industry union density is based on 1990 average unionization within the 3-digit industry. Industry wages refers to median wages within the 3-digit industry in 1990 (in 2015 dollars). All regressions control for individual-level "Manufacturing Probability" and "Max HH Manufacturing Probability".

Table 11: Wage differentials in Healthcare/Education

DV: ln(*wage*)	(1)	(2)	(3)	(4)
Health/Education	0.052***	0.072***	0.009	0.029***
	(0.010)	(0.008)	(0.009)	(0.008)
Health/Ed. × RTW		-0.056***		-0.050***
		(0.017)		(0.016)
R^2	0.002	0.020	0.211	0.212
N	138006	138006	138006	138006
Controls			Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors clustered at the state level are in parentheses. Sample is based on employed women with a high school education or less in years 1989-1991. All regressions weighted by sample weights. Column 2 includes a dummy for state RtW status. Columns 3 and 4 control for state fixed effects (which absorb the RtW dummy), a dummy for being married, a dummy for high school education, a quadratic in age, and dummies for black and hispanic. Unlike earlier results (based on the 1990-2014 change), right-to-work states excludes Oklahoma which didn't pass RtW legislation until 2001.

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A Appendix (for online publication only)

A.1 Decomposition

Our goal is to decompose the changes in the union density among employment and manufacturing. For the manufacturing decomposition, note that we can write the change in union density within manufacturing as

$$\begin{aligned}
\Delta u = u_1 - u_0 &\equiv \sum_i w_{i,1} u_{i,1} - \sum_i w_{i,0} u_{i,0} \\
&= \sum_i w_{i,1} u_{i,1} - \sum_i w_{i,0} u_{i,0} + \sum_i w_{i,1} u_{i,0} - \sum_i w_{i,1} u_{i,0} \\
&= \sum_i w_{i,1} (u_{i,1} - u_{i,0}) + \sum_i (w_{i,1} - w_{i,0}) u_{i,0} \\
&= \sum_i w_{i,1} \Delta u_i + \sum_i \Delta w_i u_{i,0}
\end{aligned}$$

or equivalently as:

$$\begin{aligned}
\Delta u = u_1 - u_0 &\equiv \sum_i w_{i,1} u_{i,1} - \sum_i w_{i,0} u_{i,0} \\
&= \sum_i w_{i,1} u_{i,1} - \sum_i w_{i,0} u_{i,0} + \sum_i w_{i,0} u_{i,1} - \sum_i w_{i,0} u_{i,1} \\
&= \sum_i u_{i,1} (w_{i,1} - w_{i,0}) + \sum_i (u_{i,1} - u_{i,0}) w_{i,0} \\
&= \sum_i u_{i,1} \Delta w_i + \sum_i \Delta u_i w_{i,0}
\end{aligned}$$

where $u_{i,t}$ is the union density in industry i at time t and $w_{i,t}$ is industry i's share of employment at time t.

Then we can use these two expressions for Δu and the fact that:

$$\begin{aligned}
\Delta u &= \frac{1}{2}\Delta u + \frac{1}{2}\Delta u \\
&= \frac{1}{2}\sum_i w_{i,1}\Delta u_i + \frac{1}{2}\sum_i \Delta w_i u_{i,0} + \frac{1}{2}\sum_i u_{i,1}\Delta w_i + \frac{1}{2}\sum_i \Delta u_i w_{i,0} \\
&= \frac{1}{2}\sum_i (w_{i,1} + w_{i,0})\Delta u_i + \frac{1}{2}\sum_i \Delta w_i (u_{i,0} + u_{i,1}) \\
&= \sum_i \bar{w}_i \Delta u_i + \sum_i \Delta w_i \bar{u}_i
\end{aligned}$$

where $\bar{x}_i$ is the average level of $x \in \{w, u\}$ in industry i between the two time periods. This is a standard decomposition of the sort popularized by Berman, Bound, and Griliches (1994).

Similarly, letting m_t denote the manufacturing share of employment in time t and letting subscript m denote manufacturing, we can write union density in the full labor market as:

$$\begin{aligned}
\Delta u &= \bar{m}\Delta u_m + (1-\bar{m})\Delta u_{-m} + \Delta m \bar{u}_m + \Delta(1-m)\bar{u}_{-m} \\
&= \bar{m}\sum_i \bar{w}_i \Delta u_i + \bar{m}\sum_i \Delta w_i \bar{u}_i + (1-\bar{m})\Delta u_{-m} + \Delta m \bar{u}_m + \Delta(1-m)\bar{u}_{-m} \\
&= \bar{m}\sum_i \bar{w}_i \Delta u_i + \bar{m}\sum_i \Delta w_i \bar{u}_i + (1-\bar{m})\Delta u_{-m} + \Delta m \bar{u}_m - \Delta m \bar{u}_{-m} \\
&= \bar{m}\sum_i \bar{w}_i \Delta u_i + \bar{m}\sum_i \Delta w_i \bar{u}_i + (1-\bar{m})\Delta u_{-m} + \Delta m(\bar{u}_m - \bar{u}_{-m}) \\
&= \bar{m}\sum_i \bar{w}_i \Delta u_i + \bar{m}\sum_i \Delta w_i \bar{u}_i + \Delta m(\bar{u}_m - \bar{u}_{-m}) + (1-\bar{m})\Delta u_{-m}
\end{aligned}$$

which is the decomposition appear in the paper.

A.2 Identifying manufacturing-type workers

We use a Lasso approach, with λ selected using the eBIC (selecting λ using cross-validation produces estimates of the probability of manufacturing employment which have a correlation, across individuals, with our prefered measure above .995). We use a rich set of demographic and geographic variables to predict the likelihood that 1989-1991 ORG respondents work in manufacturing. These include state fixed effects; a cubic in age; 5 education dummies; dummies for Hispanic, Black, other non-White race, and being married; and a series of interactions.

Specifically, we interact each state dummy with {age, male, 5 education dummies, Hispanic, Black, other non-White race, married}. We each education dummy with {age, male, Hispanic, Black, other non-White race, married}. We interact male with {age, Hispanic, Black, other non-White race, married}. We interact age with {Hispanic, Black, other non-White race, married}.

To illustrate why we use such a flexible model (including all of the interactions), consider that manufacturing employment accounted for 20% of North Carolina's prime-age population in 1990, compared to only 3% of Wyoming's. Thus, there are dramatic cross-state differences in the likelihood that observationally similar individuals work in manufacturing.

Our use a linear probability model in the Lasso estimation for simplicity. We define manufacturing-type workers as those with estimated probability above the 90^{th} percentile of the cohort-specific distribution because this is most effective. Table A1 compares the performance of different approaches for identifying manufacturing workers in 1990.

[Table A1 about here.]

A.3 Creating the industry-state panel

For Table 5, we use a panel of industry-state pairs based on the County Business Patterns (CBP) data. We use the 1990 CBP, which is already based on 1987 SIC-defined industries. We convert these to the slightly modified versions of SIC industries used by ADH using code from David Dorn's website.

The 2014 CBP is based on 2012 NAICS. One could use a variety of correspondences to convert 2012 NAICS industries to 2007 NAICS, then 2002 NAICS, then 1997 NAICS, and finally 1987 SIC. Instead, we use trade data from Peter Schott's website. This data includes HS product-level imports and exports where HS codes are mapped to 1987 SIC industries and contemporaneous NAICS industries. We use the import and export files from 2013-2016 (8 files total) which are based on 2012 NAICS codes. We calculate the real (inflation-adjusted) volume of trade (including both imports and exports) in each SIC-NAICS pair over this period. For each NAICS industry, we calculate the share of trade volume matched with each SIC. We then allocate that fraction of each NAICS-state's employment (from the 2014 CBP) to the corresponding SIC industry.

We measure compensation using the 1990 CBP. Specifically, we divide the variable for first quarter compensation by the variable for March 1 employment. Our main interest is in cross-state differences in within-industry compensation. In the table, we use the raw compensation measure (normalized to unit standard deviation and minimum zero). We have also experimented with normalizing the compensation distribution within industry; this makes no difference.

For roughly half of industry-state pairs in the CBP, employment levels are suppressed and only a range of employment is presented. In these cases, we use the midpoint of the reported range. In principle, this introduces measurement error into our employment levels. Measurement error in the dependent variable does not bias our estimates; it only increases the residual variance and makes it less likely that we estimate statistically significant effects. In practice, this is not likely to be important. For instance, in 1990 (when the suppression is somewhat worse than 2014), 44% of industry-state pairs have exact employment reported, and an additional 40% have employment ranges that are less than 150 employees wide (e.g., 100-249 employees), and less than 5 have ranges that are 1000 or more employees wide.

A.4 Summary statistics

[Table A2 about here.]

[Table A3 about here.]

A.5 Additional results

[Table A4 about here.]

[Table A5 about here.]

[Table A6 about here.]

[Table A7 about here.]

[Table A8 about here.]

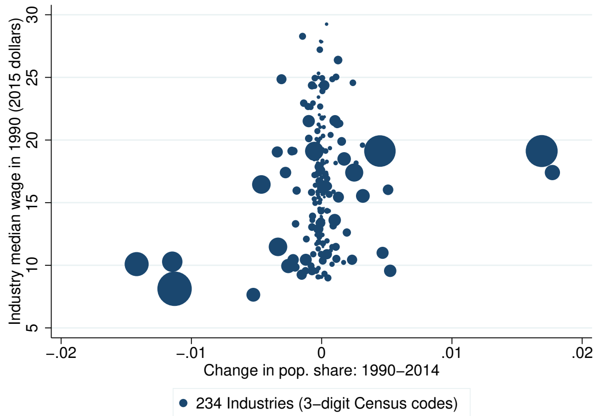
[Table A9 about here.]

[Table A10 about here.]

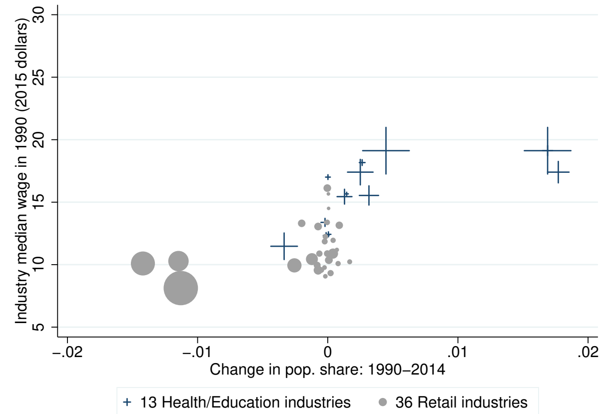
[Table A11 about here.]

[Figure A1 about here.]

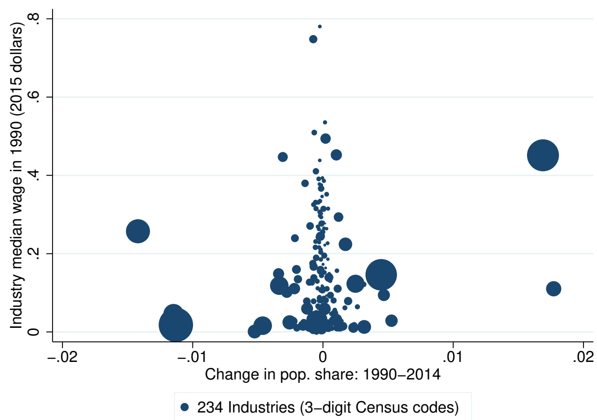
Figure A1: Characteristics of industries seeing largest changes in household members' employment



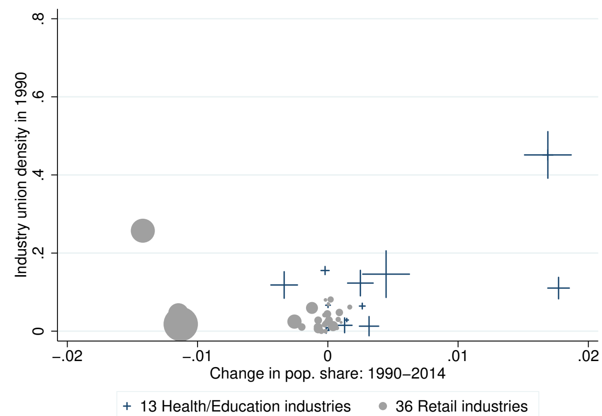
(a) Median wages (all industries)



(b) Median wages (retail, ed., health)



(c) Union density (all industries)



(d) Union density (retail, ed., health)

Sample is based on individuals for whom the estimated probability of working in manufacturing (based on demographics, state-of-residence, and a probability model estimated on the 1990 sample) is below the cohort-specific median, but for whom at least one household member has an estimated probability above the cohort-specific 90^{th} percentile. For these individuals, we calculate changes in the share of the population working in each 3-digit Census industry, from 1990 to 2014 (shown on the x-axis). We relate this to the median wage in the industry in 1990 (in 2015 dollars) and the union density in the industry in 1990.

Table A1: Probabilities of manufacturing employment

	(1)	(2)	(3)	(4)	(5)
Share working in manuf. (1990)	.138	.161	.208	.274	.345
Weights	Sample	Pr(Manuf.)	Sample	Sample	Sample
Estimated Prob. above:			50^{th} pctl.	75^{th} pctl.	90^{th} pctl.

Calculations based on 1989-1991 ORG respondents and the lasso-based probability model estimated using demographic and geographic predictors. Column 1 gives the manufacturing employment share among all respondents based on the sample weights. Column 2 uses the estimated probabilities as weights, in a more conventional DiNardo, Fortin, and Lemieux (1996) approach. Columns 3-5 restrict to the sample with estimated probabilities of working in manufacturing that are above the 50^{th}, 75^{th}, and 90^{th} percentiles.

Table A2: Summary statistics

Variable	Mean	SD	N	Percentiles 10	25	50	75	90
Δ China-US Trade (SIC)	0.16	0.67	1121	0.00	0.00	0.03	0.12	0.36
1990-2000	0.10	0.36	364	0.00	0.00	0.01	0.05	0.17
2000-2007	0.23	0.65	376	0.00	0.01	0.06	0.19	0.43
2007-2014	0.15	0.87	381	-0.03	0.00	0.03	0.14	0.36
Δ China-US Trade (Cen.)	0.17	0.50	199	0.00	0.01	0.04	0.12	0.34
1990-2000	0.08	0.20	68	0.00	0.00	0.01	0.06	0.26
2000-2007	0.22	0.45	65	0.00	0.03	0.07	0.17	0.50
2007-2014	0.22	0.72	66	-0.00	0.01	0.05	0.12	0.41
Δ China-Eur. Trade (SIC)	0.16	0.83	1157	0.00	0.00	0.03	0.12	0.34
1990-2000	0.06	0.18	385	0.00	0.00	0.01	0.05	0.14
2000-2007	0.20	0.50	384	0.00	0.01	0.06	0.17	0.40
2007-2014	0.23	1.33	388	-0.00	0.00	0.04	0.14	0.41
Δ China-Eur. Trade (Cen.)	0.14	0.37	199	0.00	0.01	0.05	0.13	0.27
1990-2000	0.05	0.08	68	0.00	0.00	0.01	0.06	0.14
2000-2007	0.19	0.32	65	0.01	0.04	0.07	0.18	0.38
2007-2014	0.20	0.53	66	0.00	0.01	0.07	0.13	0.33
NTR Gap (SIC)	0.33	0.14	382	0.13	0.24	0.34	0.41	0.48
NTR Gap (Cen.)	0.31	0.12	69	0.14	0.22	0.33	0.38	0.44
Δ ln(Emp) (ASM, SIC)	-1.00	3.33	1170	-3.09	-1.20	-0.33	-0.01	0.56
1990-2000	-0.05	3.43	386	-1.43	-0.29	-0.03	0.44	1.49
2000-2007	-1.22	2.60	390	-3.26	-1.39	-0.50	-0.10	0.21
2007-2011	-1.72	3.65	394	-3.67	-1.75	-0.65	-0.20	-0.03
Δ ln(Emp) (ASM, Cen.)	-0.30	0.43	197	-0.95	-0.52	-0.23	-0.01	0.15
1990-2000	-0.00	0.28	66	-0.28	-0.16	0.01	0.14	0.25
2000-2007	-0.33	0.42	65	-0.99	-0.42	-0.28	-0.10	0.07
2007-2011	-0.56	0.39	66	-1.11	-0.80	-0.51	-0.23	-0.14
Δ ln(Emp) (CPS, Cen.)	-0.16	0.65	203	-0.70	-0.35	-0.10	0.05	0.21
1990-2000	-0.09	0.43	68	-0.57	-0.20	-0.04	0.05	0.18
2000-2007	-0.25	0.98	67	-1.06	-0.69	-0.21	0.01	0.21
2007-2016	-0.13	0.33	68	-0.48	-0.31	-0.10	0.06	0.23
Δ Union share (Cen.)	-0.05	0.06	203	-0.13	-0.08	-0.05	-0.02	0.00
1990-2000	-0.05	0.06	68	-0.13	-0.08	-0.06	-0.03	0.00
2000-2007	-0.07	0.07	67	-0.18	-0.11	-0.05	-0.02	0.01
2007-2016	-0.04	0.04	68	-0.09	-0.05	-0.03	-0.01	0.00

Δ China-US Trade is change in real import volume (in $10,000) per worker (same as Autor et al. (2013)). NTR Gap is gap between China tariff the Normalized Trade Relations tariff rate applied to WTO members (same as Pierce and Schott (2016)). ASM = Annual Survey of Manufacturing, CPS = Current Population Survey, SIC = Standard Industrial Classification. Imports are annual changes, everything else is a decadal change.

Table A3: Industries with particularly high Chinese import penetration by decade.

Industry	Δ China-OECD Trade
Panel A: 1990-2000	
121. Misc. food preparations and kindred products	.105
151. Apparel and accessories, except knit	.176
221. Footwear, except rubber and plastic	.307
262. Misc. nonmetallic mineral and stone products	.129
322. Computers and related equipment	.167
340. Household appliances	.126
341. Radio, TV, and communication equipment	.163
370. Cycles and miscellaneous transportation equipment	.136
390. Toys, amusement, and sporting goods	.354
Panel B: 2000-2007	
151. Apparel and accessories, except knit	.556
221. Footwear, except rubber and plastic	1.415
322. Computers and related equipment	1.496
340. Household appliances	.610
341. Radio, TV, and communication equipment	.947
390. Toys, amusement, and sporting goods	1.41
Panel C: 2007-2014	
151. Apparel and accessories, except knit	.844
221. Footwear, except rubber and plastic	2.670
322. Computers and related equipment	2.755
340. Household appliances	.854
341. Radio, TV, and communication equipment	2.041

Outliers (see Figure 1). Numbers correspond to census industry codes (ind1990: The IPUMS-CPS scheme).

Table A4: Industry-level effects on unionization

	(1)	(2)	(3)	(4)
DV:	Δ ln(Employment) Union mem.	Non-mem.	Change in Union member share	
	Panel A: Autor-Dorn-Hanson identification strategy			
Δ China-Other Trade	-0.370***	-0.174***	-0.007	-0.006**
	(0.093)	(0.049)	(0.004)	(0.003)
R^2	0.272	0.261	0.843	0.864
N	64	64	64	64
p for H_0: $\beta_{mem} = \beta_{non}$	.008			
Controls:				
Union membership (1990)	Yes	Yes	Yes	Yes
Cap./Lab., Skill int., Textiles				Yes
	Panel B: Pierce-Shott identification strategy			
NTR Gap	-0.291**	-0.100	-0.015***	-0.012*
	(0.129)	(0.093)	(0.005)	(0.006)
R^2	0.189	0.214	0.863	0.870
N	64	64	64	64
p for H_0: $\beta_{mem} = \beta_{non}$	.001			
Controls:				
Union membership (1990)	Yes	Yes	Yes	Yes
Cap./Lab., Skill int., Textiles				Yes
p for H_0: $\beta_{ADH} = \beta_{PS}$	.622	.483	.191	.438

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014; are weighted by 1990 industry employment; and control for 1990 Union membership share. Column (4) controls for industry-level capital-labor ratios (from ASM), "skill intensity" (non-production workers as share of employment; from ASM), and a dummy for textiles, apparel, and leather. As shown in Table 2, these explain most of the relationship between 1990 unionization and the instruments. Standard deviation of change in China-Other trade is 4.17; standard deviation of NTR gap is .103.

Table A5: Effects on and heterogeneity by the union wage premium

	(1)	(2)	(3)	(4)	(5)
DV:	Change (from 1990-2014) in union share of emp.			wage premium	
Import exposure	-0.014*	-0.012	-0.018**	0.002	0.008
	(0.008)	(0.008)	(0.009)	(0.013)	(0.016)
Union wage premium (1990)	-0.107	-0.133	-0.335*		
	(0.120)	(0.119)	(0.190)		
Wage prem. × Exp.	-0.007	0.005	0.109		
	(0.057)	(0.062)	(0.081)		
Homogeneous goods (Rauch)			0.043		
			(0.065)		
Exp. × Homog.			-0.003		
			(0.034)		
Prem. × Homog.			0.266		
			(0.317)		
Prem. × Exp. × Homog.			-0.262		
			(0.191)		
R^2	0.868	0.877	0.893	0.001	0.041
N	64	64	62	64	64
Controls:					
Union membership (1990)	Yes	Yes	Yes	Yes	Yes
Cap./Lab., Skill int., Textiles		Yes	Yes		Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014; are weighted by 1990 industry employment; and control for 1990 Union membership share. Columns 2 and 4 control for industry-level capital-labor ratios (from ASM), "skill intensity" (non-production workers as share of employment; from ASM), and a dummy for textiles, apparel, and leather. As shown in Table 2, these explain most of the relationship between 1990 unionization and the instruments. Dependent variable is estimated union wage premium from a Mincer regression estimated using CPS ORG data on prime-age manufacturing workers that controls for a cubic in age, 5 education dummies, 6 occupation dummies, education-specific returns to age, marital status, sex, hispanic status, African-American, other-non-White race, full-time/part-time status, year, state, and industry, in addition to union membership status. Of these, only the union membership status is allowed to vary across industries. Simpler Mincer regressions, adjustments for small sample size, or longer-time periods make no difference in the result. Among our 64 industries, 1990 estimated union wage premia vary from 2.1% (5^{th} percentile) to 23.9% (95^{th} percentile) with a mean and median of 11%.

Table A6: State-level effects on unionization

	(1)	(2)	(3)	(4)	(5)	(6)
DV: Change in share of working age population	Non-emp.	Non-manuf., non-union members	Non-manuf., non-public, union mem.	Non-manuf., public sect., union mem.	Manufact., non-union members	Manufact. union members
DV mean in 1990	28.0	51.1	3.8	4.0	10.5	2.6
Avg change '90-'14	3.9	3.0	-0.8	-0.5	-3.7	-1.9
	Panel A: Autor-Dorn-Hanson identification					
Δ China-Other Trade	0.534*	0.457*	0.157**	0.155**	-1.191***	-0.114
	(0.298)	(0.270)	(0.069)	(0.074)	(0.356)	(0.158)
R^2	0.074	0.049	0.063	0.065	0.283	0.009
N	51	51	51	51	51	51
DV mean in 1990	28.0	51.1	3.8	4.0	10.5	2.6
	Panel B: Pierce-Schott identification					
NTR Gap	0.762**	0.324	0.153	0.118	-1.552***	0.194
	(0.334)	(0.271)	(0.123)	(0.114)	(0.332)	(0.148)
R^2	0.150	0.025	0.059	0.037	0.480	0.028
N	51	51	51	51	51	51
p for H_0: $\beta_{ADH} = \beta_{PS}$	.426	.593	.968	.639	.185	.040**
	Panel C: Joint hypothesis testing					
Identification strategy:				Mean ('90)	ADH	PS
Manufacturing (as share of pop.):				13.1	-1.30***	-1.36***
$\beta_{(5)} + \beta_{(6)}$					(0.279)	(0.294)
Non-manufacturing union membership (as share of pop.):				7.8	0.31***	0.27*
$\beta_{(3)} + \beta_{(4)}$					(0.101)	(0.144)
Union membership (as share of employment):				14.5	0.26	0.70*
$\left(\beta_{(3)} + \beta_{(4)} + \beta_{(6)}\right)/\left(1 - \beta_{(1)}\right)$					(0.284)	(0.357)

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014. All regressions weighted by state employment in 1990. "States" includes the District of Columbia. All regressions based on prime age persons (age 16-64). Coefficients in columns 1-6 sum to zero because the population shares sum to one (i.e., those six groups are mutually exclusive and exhaustive), so changes in shares sum to zero. NTR Gap and ΔChina-Other Trade are both normalized to have unit standard deviation across states.

Table A7: Differential effects of import competition in Right-to-Work states

DV: Change in share of working age population	(1) Non-emp.	(2) Non-manuf., non-union members	(3) Non-manuf., non-public, union mem.	(4) Non-manuf., public sect., union mem.	(5) Manufact., non-union members	(6) Manufact. union members
DV mean in 1990	28.0	51.1	3.8	4.0	10.5	2.6
Avg change '90-'14	3.9	3.0	-0.8	-0.5	-3.7	-1.9
	Panel A: Autor-Dorn-Hanson identification					
Δ China-Other Trade	0.219	0.321	0.373**	0.339	-1.419***	0.167
	(0.491)	(0.714)	(0.166)	(0.236)	(0.454)	(0.314)
Right-to-work	-1.870	-2.705	1.064**	0.449	0.346	2.716**
	(1.902)	(1.956)	(0.512)	(0.645)	(2.005)	(1.036)
RTW $\times$ Δ Ch.-Oth.	0.937	0.722	-0.278	-0.183	-0.508	-0.690*
	(0.769)	(0.877)	(0.198)	(0.256)	(0.866)	(0.399)
R^2	0.112	0.108	0.166	0.074	0.329	0.219
N	51	51	51	51	51	51
	Panel B: Pierce-Schott identification					
NTR Gap	0.408	0.370	0.334	0.251	-1.698***	0.336
	(0.564)	(0.627)	(0.305)	(0.274)	(0.368)	(0.308)
Right-to-work	0.045	-0.855	0.491***	0.095	-0.781*	1.005**
	(0.586)	(0.668)	(0.155)	(0.220)	(0.450)	(0.456)
RTW $\times$ NTR Gap	1.627**	0.183	-0.249	-0.190	-1.286*	-0.086
	(0.720)	(0.746)	(0.313)	(0.290)	(0.673)	(0.358)
R^2	0.256	0.061	0.177	0.049	0.592	0.196
N	51	51	51	51	51	51

Panel C: Joint hypothesis testing

Identification strategy:	Mean (1990)	ADH Non-RtW	ADH RtW	PS Non-RtW	PS RtW
Manuf. (pop. share):	13.1	-1.25***	-2.45***	-1.36***	-2.73***
$\beta_{(5)} + \beta_{(6)}$		(0.335)	(0.595)+	(0.316)	(0.609)+
Non-manuf. union (pop. share):	7.8	0.71**	0.25	0.59*	0.15
$\beta_{(3)} + \beta_{(4)}$		(0.279)	(0.160)	(0.326)	(0.123)
Union mem. (emp. share):	14.5	1.31*	-0.42	1.41*	0.56*
$\left(\beta_{(3)} + \beta_{(4)} + \beta_{(6)}\right)/\left(1 - \beta_{(1)}\right)$		(0.921)	(0.386)++	(0.813)	(0.283)

*,+ $p < .10$, **,++ $p < .05$, ***,+++ $p < .01$. Stars (*) test whether coefficient is significantly different from zero. Crosses (+) test whether RtW effect is significantly different from non-RtW effect. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014. All regressions weighted by state population in 1990. "States" includes the District of Columbia. All regressions based on prime age persons (age 16-64). Coefficients in columns 1-6 sum to zero because the population shares sum to one (i.e., those six groups are mutually exclusive and exhaustive), so changes in shares sum to zero. NTR Gap and ΔChina-Other Trade are both normalized to have unit standard deviation across states.

Table A8: Explaining Right-to-Work heterogeneity (Autor-Dorn-Hanson)

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	$\ln(Emp)_{is,90}$	$\Delta \ln(Emp)_{is}$				
ΔCh-Oth$_i$	-0.087	-0.313***				
	(0.114)	(0.097)				
RTW$_s$	-0.455*	-0.115	0.057	0.112	0.032	0.024
	(0.236)	(0.146)	(0.087)	(0.091)	(0.090)	(0.092)
ΔCh-Oth$_i$ $\times$ RTW$_s$	0.020	-0.224***	-0.178**	-0.153*	-0.106	-0.183**
	(0.083)	(0.037)	(0.076)	(0.081)	(0.079)	(0.081)
Avg. compensation$_{is}$					-0.189***	
					(0.057)	
ΔCh-Oth$_i$ $\times$ Comp$_{is}$					0.092***	
					(0.026)	
RTW$_s$ $\times$ Homogeneous goods$_i$						0.224*
						(0.120)
ΔCh-Oth$_i$ $\times$ RTW$_s$ $\times$ Homogen$_i$						0.335**
						(0.149)
R^2	0.025	0.087	0.670	0.771	0.776	0.675
N	11062	11062	11062	5027	5027	10516
Industry fixed effects			Yes	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Results based on panel of industry-state pairs, where industries are based on SIC definitions and states include DC. Columns 2-6 based on changes in log employment from 1990 to 2014. All regressions weighted by 1990 employment in the industry-state. Standard errors (based on two-way clustering on state and industry) are shown in parentheses. Core results are based on 308 industries, although half of industry-state pairs are missing compensation data. Employment is from the CBP. "Exposure" is based on ADH and PS instruments, and has unit standard deviation across industries by construction. "Average compensation" refers to total first quarter payroll divided by March 1 employment (both from CBP), and is normalized to have minimum zero and unit standard deviation across the sample. "Homogeneous goods" is based on the definition from Rauch (1999). Only one Right-to-Work state passed legislation during our period (Oklahoma, 2001); the rest passed it prior to 1990.

Table A9: Explaining Right-to-Work heterogeneity (Pierce-Schott)

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	$\ln(Emp)_{is,90}$	$\Delta \ln(Emp)_{is}$				
NTR Gap_i	-0.183	-0.232***				
	(0.120)	(0.068)				
RTW_s	-0.452*	-0.119	0.044	0.110	0.038	0.008
	(0.237)	(0.127)	(0.091)	(0.089)	(0.088)	(0.096)
$\text{Gap}_i \times \text{RTW}_s$	0.146	-0.197**	-0.060	-0.118*	-0.122*	-0.087
	(0.151)	(0.080)	(0.051)	(0.066)	(0.064)	(0.065)
Avg. compensation_{is}					-0.174***	
					(0.062)	
$\text{Gap}_i \times \text{Comp}_{is}$					-0.014	
					(0.042)	
$\text{RTW}_s \times$ Homogeneous goods_i						0.302**
						(0.135)
$\text{Gap}_i \times \text{RTW}_s \times \text{Homogen}_i$						0.195*
						(0.115)
R^2	0.030	0.054	0.667	0.770	0.774	0.672
N	11062	11062	11062	5027	5027	10516
Industry fixed effects			Yes	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Results based on panel of industry-state pairs, where industries are based on SIC definitions and states include DC. Columns 2-6 based on changes in log employment from 1990 to 2014. All regressions weighted by 1990 employment in the industry-state. Standard errors (based on two-way clustering on state and industry) are shown in parentheses. Core results are based on 308 industries, although half of industry-state pairs are missing compensation data. Employment is from the CBP. "Exposure" is based on ADH and PS instruments, and has unit standard deviation across industries by construction. "Average compensation" refers to total first quarter payroll divided by March 1 employment (both from CBP), and is normalized to have minimum zero and unit standard deviation across the sample. "Homogeneous goods" is based on the definition from Rauch (1999). Only one Right-to-Work state passed legislation during our period (Oklahoma, 2001); the rest passed it prior to 1990.

Table A10: State-level effects of exposure to import competition (log population)

	(1)	(2)	(3)	(4)	(5)	(6)
DV: Change in natural log of population	Non-emp.	Non-manuf., non-union members	Non-manuf., non-public, union mem.	Non-manuf., public sect., union mem.	Manufact., non-union members	Manufact. union members
Import exposure	0.022***	0.008*	0.024	0.008	-0.106***	-0.176***
	(0.008)	(0.005)	(0.028)	(0.039)	(0.024)	(0.039)
R^2	0.925	0.940	0.271	0.258	0.259	0.400
N	51	51	51	51	51	51

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, are weighted by state employment in 1990, are based on prime age persons (age 16-64), and control for state-level change in total prime age population. "States" includes the District of Columbia. Import exposure has unit standard deviation by construction.

Table A11: State-level effects on unionization on men vs. women

DV: Change in share of working age population	(1) Non-emp.	(2) Non-manuf., non-union members	(3) Non-manuf., non-public, union mem.	(4) Non-manuf., public sect., union mem.	(5) Manufact., non-union members	(6) Manufact. union members
	Panel A: Men					
Import exposure	0.669***	0.359	0.217*	0.064	-1.316***	0.008
	(0.212)	(0.328)	(0.119)	(0.110)	(0.293)	(0.185)
R^2	0.128	0.020	0.069	0.008	0.271	0.000
N	51	51	51	51	51	51
DV mean in 1990	19.6	53.1	5.2	4.0	13.8	4.2
	Panel B: Women					
Import exposure	0.743	0.209	0.182*	0.130	-1.215***	-0.049
	(0.452)	(0.376)	(0.105)	(0.128)	(0.388)	(0.067)
R^2	0.081	0.008	0.076	0.028	0.319	0.008
N	51	51	51	51	51	51
DV mean in 1990	36.0	49.3	2.3	4.0	7.2	1.1
p for H_0: $\beta_A = \beta_B$	.834	.662	.819	.664	.672	.670

Panel C: Joint hypothesis testing

Sample:	Men		Women	
	Mean ('90)	Effect	Mean ('90)	Effect
Manufacturing (as share of pop.):	18.1	-1.31***	8.3	-1.26***
$\beta_{(5)} + \beta_{(6)}$		(0.256)		(0.358)
Non-manuf. union mem. (as share of pop.):	9.3	0.28*	6.3	0.32*
$\beta_{(3)} + \beta_{(4)}$		(0.141)		(0.181)
Union membership (as share of employment):	16.8	0.40	11.7	0.416
$\big(\beta_{(3)} + \beta_{(4)} + \beta_{(6)}\big)/\big(1 - \beta_{(1)}\big)$		(0.304)		(0.287)

* $p < .10$, ** $p < .05$, *** $p < .01$. For none of the joint hypotheses (Panel C) can we reject that effects on men and women are the same. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014. All regressions weighted by state employment in 1990. "States" includes the District of Columbia. All regressions based on prime age persons (age 16-64). Coefficients in columns 1-6 sum to zero because the population shares sum to one (i.e., those six groups are mutually exclusive and exhaustive), so changes in shares sum to zero. Both panels include only prime age (16-64) persons.