The Effects of Import Competition on Unionization

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Abstract

We study direct and indirect effects of Chinese import competition on union membership in the United States, 1990-2014. Import competition in manufacturing induced a slight decline in unionization within manufacturing industries. The magnitude is small because manufacturers in Right-to-Work states experienced more direct competition with low-quality Chinese imports. Outside manufacturing, however, import competition causes a large increase in union membership as less-educated women shift away from retail and towards jobs in healthcare and education where unions are stronger. Due to these responses, we calculate that Chinese imports prevented 26% of the union density decline that would have otherwise occurred.

Keywords: Unions, China, Imports, Household labor market adjustments

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> "As we create an environment in China where people are working under slave labor conditions, earning 3 cents an hour... what happens in America? Those same corporations go back to the American working men and women, and they tell American working men and women they are going to have to take a wage cut. We do not want them to have a union anymore to speak for them. They better not complain about their working conditions. Do not go on trying to negotiate with us. There is nothing to negotiate."
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> – Dennis Kucinich (D-OH) during 2000 House debates over Permanent Normalized Trade Relations with China

1 Introduction

When unionization in the United States peaked in the early 1950s, roughly one in three American workers claimed union membership (Farber et al., 2018; Hirsch, 2008). Union density has fallen steadily ever since, reaching 10.3% in 2019 with private sector unionization falling even further (6.2%).

In the 1980s, observers began blaming increased competition with foreign imports as abetting union decline (Bluestone and Harrison, 1982). In what we will call the "standard story", the impetus for trade was domestic demand for lower-priced goods. Foreign producers enjoyed a cost advantage due to their access to low-wage labor. Unionized American firms had higher labor costs than non-unionized firms, making unionized firms disproportionately vulnerable to low-wage foreign competition. Thus, unionized establishments were the first to close or demand concessions from their workers, undermining union bargaining power. In such an environment, organizing displaced workers into other unions became nearly impossible.

As China rose to global prominence in the late 1990's and joined the WTO in 2001, journalists and union leaders continuing to blame trade with China for ongoing deunionization (Gunn, 2018; Trumka, 2015). We exploit these recent trade shocks to explore how well the standard story holds up in the post-1990 period.

Like many others, we found the standard story plausible. When combined with the negative employment effects documented by Autor et al. (2013) and Pierce and Schott (2016)—ADH and PS, respectively—we expected greater competition from Chinese imports would undermine American unions. We anticipated that this paper would use new evidence to better quantify the magnitude of the (presumably large) negative effect of import competition on US unionization rates. But our findings highlighted an implicit assumption in the standard story, namely that manufacturing is all that matters when it comes to unionization. But manufacturing is no longer the heart of the labor movement in the United States. We look across sectors to understand the effects of import competition in manufacturing.

Our evidence clearly points to a more nuanced story, where the causal effect of Chinese import competition on overall US unionization is *positive* even while Chinese import competition produced small declines in union density within manufacturing. Moreover, we describe important within-household adjustment as well as geographic variation.

To tell this story, we begin by estimating the effects of Chinese import exposure on changes in union density at the manufacturing industry-level from 1990-2014. Chinese productivity gains (Autor, Dorn, and Hanson 2013; Acemoglu et al. 2016) and changing trade policy (Pierce and Schott, 2016) produce exogenous variation in import competition across industries. We find negative effects on union density that are robust and statistically significant, but surprisingly small, explaining roughly a sixth of the average manufacturing industry's union decline over the period. We show that in industries producing homogeneous, non-differentiated goods (like unprocessed lead), Chinese import competition has large effects on unionization. However, the overwhelming majority of US manufacturing employment is in industries producing heterogeneous, differentiated goods, and in these industries the effects are small. This is consistent with evidence that unionization increases worker productivity (Sojourner et al., 2015), that more productive firms produce higher quality goods (Kugler and Verhoogen, 2011), and that high-quality goods face minimal product market competition from low-wage imports (Khandelwal, 2010). In our view, this is the first way in which the standard story was an over-simplification.

We then use the shift-share approach popularized in Autor et al. (2013) to re-weight industry-level exposure to the state-level, and we estimate effects on state labor market outcomes. In doing so, we confirm the well-known result that import exposure reduces manufacturing employment and increases non-employment, but we also find modest, surprisingly robust increases in unionized employment *outside* of manufacturing. Combining the small effects on unionization within manufacturing with the fact that manufacturing is less than 20% of US employment, these outside-of-manufacturing spillover effects turn out to be larger than the within-manufacturing direct effects. As a result, and much to our surprise, our estimates imply that Chinese import competition actually *increased* total unionization in the United States.

What accounts for union-increasing changes outside manufacturing? Are individual workers shifting from manufacturing into unionized jobs in other sectors? Or is adjustment at the household level? We develop a machine learning approach to identify the demographic groups most likely to have worked in manufacturing in 1990, and look at demographically identical groups in 2014. We find that "manufacturing-type" individuals in 2014 saw a massive reduction in actual manufacturing employment. Instead, these workers largely ended up in low-wage service jobs in non-unionized sectors such as restaurants and landscaping. However the spouses and children of these manufacturing-type individuals showed large changes in their employment composition. Specifically, they exhibit reduced representation in retail jobs and increases in healthcare and education, sectors with relatively high and stable levels of unionization.

To connect these descriptive changes to the effects of exposure, we implement a triple-difference strategy. We find that the types of workers (again based on observable demographics) who would have been likely to work in manufacturing in 1990 saw greater shifts into service jobs and reduced average industry-level unionization rates, both relative to other workers in the state and relative to demographically-similar workers in less exposed states. Similarly, the types of workers likely to work in retail in 1990 saw greater shifts out of retail and into healthcare and education (and overall into more unionized industries) as a result of import exposure. Our results suggest that the increase in unionized employment outside of manufacturing was the result of a structural transformation of women's place in the labor market. The spouses of "manufacturing type" individuals ended up in higher paying, more unionized industries.[1]

We close the paper by considering an obvious potential source of heterogeneity in our state-level effects: states' Right-to-Work (RtW) laws. One might assume that less-unionized RtW states, by virtue of having lower average wages, might be relatively shielded from low-wage country import competition. To the contrary and echoing our earlier evidence that unionized firms face less direct competition with Chinese imports, we find that import exposure in a RtW state has *double* the impact on manufacturing employment.[2,3] Moreover, in RtW states, a much larger share of the manufacturing job loss is absorbed into non-employment. We present evidence that this latter finding is not a coincidence. OLS Mincer regressions show that RtW states have no wage premia in healthcare or education, eliminating any incentive for family members to flow into these sectors to offset the income declines associated with the disappearance of manufacturing jobs.

Our results contribute to three significant literatures. First, we speak to the explanations for declining unionization in the United States (Western, 1997; Wallerstein and Western, 2000; Farber and Western, 2001; Southworth and Stepan-Norris, 2009; Hirsch, 2008; Clawson

[1]See the conclusion for a discussion of the reasons why women were more prone to make the adjustment into these industries than "manufacturing-type" (largely male) workers.

[2]We rule out several potential mechanical explanations for this finding. In Appendix Table A15 we show that we only observe differential adverse RtW effects in heterogeneous-goods industries. In homogeneous-goods goods industries, non-RtW (pro-union) states are actually affected worse by exposure.

[3]Bloom et al. (2019) also document geographic heterogeneity in the effects of Chinese import exposure, which they attribute to human capital differences across US states, which is correlated (-.42) with RtW laws. In Section 5 and Appendix Table A17, we provide evidence in favor of the RtW interpretation over the education interpretation.

and Clawson, 1999). The early literature on globalization and deunionization took the standard story seriously and focused on trade-related "deindustrialization" and the relatively unionized manufacturing sector. 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 identification strategy. Neither finds evidence that industries facing more import competition saw greater declines in union density. Using a longer time series and a clearer identification strategy, we revise this conclusion.

Second, we contribute to the recent literature on the consequences of Chinese import competition. This research has shown that the "China Shock" has transformed the American 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), migration (Greenland, Lopresti, and McHenry, 2019), and crime levels (Che, Xu, and Zhang, 2018). Although they do not study unionization, Bloom et al. (2019) present a recent entry in this literature that, like us, emphasizes employment spillovers outside manufacturing. They use establishment-level data to show that the China shock led to *both* a decline in manufacturing employment *and* growth in employment in services, with the negative effects in manufacturing concentrated in areas with a lower proportion of college-educated workers. We view our findings as complementary. Bloom et al. (2019) document adjustments at the establishment-level—industry switching—that are impossible to recover from individual-level data whereas we describe household adjustments using individual-level data that are invisible at the establishment level.

Finally, our findings on the importance of household adjustment contribute to the study of the "added worker effect," in which spousal employment responds to negative shocks to the prime earner (Lundberg, 1985). Second earner adjustments are a key to understanding the incidence of many policies (Ahlquist, Hamman, and Jones, 2017; Blundell, Pistaferri, and Saporta-Eksten, 2016; Borella, De Nardi, and Yang, 2018; Mankart and Oikonomou, 2016), and models of household decision making (Donni and Chiappori, 2011). Most empirical studies of added worker effects focus on short-term decisions around whether and how much to work. Our results highlight an important margin with longer-run consequences: shifting across *types of work* towards higher paying jobs. Understanding large-scale, long-run changes in labor market engagement is particularly important given growing evidence that adverse labor market shocks are persistent (Amior and Manning, 2018; Dix-Carneiro and Kovak, 2017).

2 Data and methods

2.1 Sources of variation

Our core sources of exogenous variation in exposure to Chinese imports are drawn from ADH and PS. Both papers rely on variation across manufacturing industries measured at the detailed SIC level ($n = 357$). Because we rely on the Current Population Survey (CPS)—one of the only data sets with union membership—we are forced to coarsen both exposure measures into to Census industries ($n = 64$).[4] Although we lose substantial variation through aggregation (summary statistics in Table A1), we are able to replicate the large and significant SIC industry-level employment effects from PS and Acemoglu et al. (2016) in Appendix A.1.[5] To calculate state-level import exposure, we follow the ADH approach and reweight industry-level variation using the County Business Patterns (CBP) dataset to calculate 1990 industry shares at the state level.[6,7]

2.2 Pooling ADH and PS

Critically for our purposes, ADH and PS rely on different assumptions and sources of variation for identification. ADH emphasize that pro-market reforms in a limited set of industries accounted for the majority of growth in Chinese imports since 1990. For instance, they note that 1% of industries account for 40% of growth in US imports. To isolate this supply-driven component of China-US exports, they propose using Chinese exports to other OECD countries as an instrument.[8] We refer to this measure of import exposure as ΔChina-

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[4]We take CPS data from IPUMS (Flood et al., 2017) using the IPUMS time-consistent industry categories.

[5]We find that import exposure leads to large increases in realized industry-level imports (the "first stage") and decreases in industry-level employment. Our estimated employment effects are actually larger using coarser industry codes. This is because imports at the narrow SIC-based industry level spill over onto other closely related industries. Coarser Census-based industry codes tend to aggregate these spillovers. For example, exposure to poultry imports [SIC code: 2015] can affect employment in meat packing [SIC code: 2011], but both are coarsened to meat products [Census code: 100]).

[6]We follow ADH and set import growth to zero outside of manufacturing. As they acknowledge, this creates a mechanical correlation between lagged manufacturing employment share and exposure to import competition. Our results are unaffected by how we handle non-manufacturing industries in this calculation (Table A9).

[7]State-level industry shares are based on SIC industries. ADH use commuting zones instead of states. State-level variation is sufficient for reasonably precise employment effects of a very similar magnitude. While the CPS does include MSA (more detailed than state), the basic CPS only collects this from 1994 on, and a large share of the sample lives outside identifiable MSA's.

[8]Specifically: Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. We thank Gordon Hanson for providing data to update China-other exports through 2014.

other trade.[9,10] PS exploit the fact that, at the end of 2001, the US granted permanent Normal Trade Relations (NTR) status to China, eliminating the risk that tariffs would revert to the the higher rate applied to non-market economies. PS show that making NTR rates permanent dramatically increased imports and reduced employment in industries with the highest "NTR gap" (the difference between the NTR tariffs and the non-market tariff reversion point). Importantly, all the "comparable" countries ADH use in constructing their instrument had already 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).

As one would expect, the correlation between the two instruments is not large (0.27 across industries and 0.49 across states). We show that each strategy used individually produces nearly identical results to the other (appendix Tables A4 and A6). We find this reassuring; if some omitted variable were driving our results, we find it unlikely that this would apply equally to two substantively and empirically distinct sources of exposure. For none of our eight regressions is there a statistically significant difference between the estimated effects of ADH and PS exposure.

One difference between these sources of exposure is the temporal dimension. The ADH approach uses the full variation in import growth from 1990-2014, while the PS variation is not relevant until China's accession into the WTO a decade later (2001). Thus, the ADH measure of exposure partly captures effects occurring in the 1990's, well-before the effects driven by PS exposure. In practice, this matters very little. The summary statistics in Table A1 show that there was scant growth in China-other trade during the 1990's. Comparing the 2000-2007 or 2007-2014 periods to the 1990-2000 period, growth in the average industry's import competition is 3-4 times as large, and the standard deviation across industries is 3-7 times as large. Nearly all of our identifying variation is coming from post-2000, regardless of which instrument we use.

To fully exploit both sources of variation we create a measure of import exposure that pools both the ADH and PS instruments. Specifically, we normalize each measure to have unit standard deviation, sum them, and normalize the sum to have unit standard deviation. All results in the main text use this pooled exposure variable.

[9] Following ADH, we measure growth in exposure by calculating the change in the inflation-adjusted volume of imports, divided by baseline (1991) industry-level employment.

[10] Although Autor, Dorn, and Hanson (2013) is perhaps better known for their strategy for reweighting industry-level exposure to the geography-level (an approach we follow), they do use this instrument throughout in their paper and argue extensively for its exogeneity.

2.3 Econometric specifications

All industry- and state-level analyses are based on long-difference changes from 1990 to 2014 with industry-level regressions weighted by 1990 industry employment and state-level regressions weighted by 1990 population.[11] We estimate the reduced form effect of exposure as opposed to an instrumental variables (IV) specification. We avoid the IV approach for two reasons. First, the exclusion restriction is violated. If the threat of foreign competition leads US-based producers to adopt cost-saving technology to fend off that competition, then *exposure itself* can affect domestic employment even without actual, realized imports increasing (Bloom, Draca, and Van Reenen, 2016). Second, many of our results are state-level, and there is no data on "state-level imports." The common practice of reweighting national industry-level imports to the state level using baseline employment shares is interpretable (i.e., the extent to which actual US imports concentrate in local production industries), but it is *not* the actual import-induced displacement of the state's production. Data for constructing such a measure do not exist.

2.4 Threats to identification

Before presenting our main results, it is important to evaluate potential threats to causal inference. We summarize those threats and our tests here. Most directly, if prior unionization predicts subsequent import exposure then we should control for baseline unionization levels. In appendix A.2 we show that this is indeed the case (consistent with findings in the PS appendix). We show that this correlation can be explained entirely using three industry-level covariates: capital intensity, skill share, and a dummy for the textiles sector. All three variables are known to be related to both unionization and Chinese imports. Once we condition on these covariates, the significant relationship between 1990 unionization and subsequent import exposure disappears, regardless of the exposure variable used (ADH, PS, or pooled). At the state-level, there is no relationship between 1990 union density and subsequent import exposure.

The correlation between industry-level exposure and 1990 union density is obviously concerning, so we make sure to control for baseline density in all our industry-level regressions below. Thus, our identification assumption is that, conditional on 1990 union density, the NTR Gap and ΔChina-other trade are exogenous determinants of Chinese import competition. It is possible that, despite this control, there remains omitted variables that simultaneously drive density declines and import exposure. Two further results suggest this is not

[11] We follow the convention of using adjacent years to improve the precision of the CPS (so 1990 is based on 1989-1991; 2014 is based on 2013-2015).

the case.

First, we show in Table A5 that there are no "placebo" effects. Neither measure of exposure is correlated with industry-level changes in union density from 1985-1990. Thus, pre-trends in density were similar regardless of subsequent exposure. Closely related, there is no relationship between NTR gap and changes in density from 1990-2000 (before NTR tariffs were made permanent). In Table A8 we also show that there was no relationship between changes in non-manufacturing union density from 1985-1990 at the state-level.[12]

Second, once we controlled for 1990 density, our results are nearly identical when adding in additional controls for the industry-level characteristics mentioned above. The fact that these variables do not affect our main estimates at all suggests that controlling for 1990 unionization is sufficient to summarize whichever characteristics of the unionizing environment produced the correlation between 1990 unionization levels and subsequent import exposure. With state-level results, our core findings are unaffected by controls for baseline state-level union density or a large number of characteristics that have become common in the literature (Table A7).

3 Main results

3.1 Industry-level effects of exposure

We first estimate the effect of increased Chinese imports on manufacturing industry-level employment outcomes. Table 1 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.[13]

[Table 1 about here.]

[12] There is a relationship between exposure and pre-1990 changes in manufacturing employment and in non-employment. These effects are the substantive interest of ADH, rather than us, and we note that those authors show such a correlation in Table 2 of their paper.

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

In column 4, we calculate the change in industry-level union density, defined as the share of workers who are union members. 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 include the three covariates that explain the relationship between 1990 density and subsequent exposure: skill share, capital intensity, and textiles. (Again, Section A.2 of the appendix discusses these extensively.) The coefficient on exposure 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 the appendix, we present additional robustness checks showing that the core results are the same between the two identification strategies and that there is no relationship between exposure and pre-1990 (placebo) changes in union membership.[14]

Here, we focus on a more substantive puzzle suggested by our results: Why are the effects of exposure on density so small? Below, we present a formal decomposition of our estimates which shows that, relative to a counterfactual that sets each industry's exposure equal to the sample minimum, Chinese import exposure can only explain 2.3 percentage points (or 17%) of the average decline in unionization. Given that unions raise wages, one would expect unionized firms to be much more adversely affected by low-wage competition. With this expectation in mind (what we call the "standard story"), it is surprising that the scale of imports had such negligible effects.

One possibility that we consider is that unionized firms don't actually compete with Chinese producers as directly as one might expect. Specifically, an old literature in labor economics argued (and presented some evidence) that unionization increases productivity (Allen, 1984, 1986, 1987; Clark, 1980b,a, 1984) and wages (Card, 1996). This is important because a recent literature in trade has shown that higher paying, more productive firms produce higher quality output (Kugler and Verhoogen, 2011) and low-quality producers are the ones facing the most competition from low-wage country imports (Khandelwal, 2010; Amiti and Khandelwal, 2013). Thus, one potential explanation for our small effects is that unionized producers simply compete in a different market segment than Chinese producers because they primarily produce high quality products within the industry.

[14] Table A4 shows none of the estimates 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. Table A5 we show that more and less exposed industries had identical pre-1990 trends in union membership (though admittedly the CPS only allows us to go back a few years before 1990, since union membership wasn't collected until 1984).

Is there any evidence for this hypothesis? We use data from Rauch (1999), who developed a widely used measure describing which industries produce homogeneous goods—quality is standardized and different products are near-perfect substitutes (e.g., unprocessed lead)—and which industries produce heterogeneous, branded products (e.g., shoes). If it is true that unionized firms are shielded from import competition by producing higher quality products, then this should only hold in heterogeneous-goods industries. In homogeneous-goods industries where, by definition, product quality cannot vary, we should see more evidence for the standard story that Chinese imports drive down union density.

In Column 6 we include an interaction between the industry-level measure of product homogeneity and the industry-level measure of exposure.[15] For industries with only *heterogeneous* goods, one standard deviation increase in exposure reduces density by only 0.8 percentage points ($p < .10$), a third less than our primary specification in column 5. For industries with only *homogeneous* goods, however, the implied decline is 3.2 percentage points ($p < .10$), four times as large. This suggests a plausible mechanism for why the industry-level effects expected under the standard story turned out to be quite small: Only when producing homogeneous products are unionized firms more susceptible to low-wage country import competition, and this is a relatively small share of US manufacturing.[16]

To underscore how much larger effects are in homogenous industries, we can calculate the implied effects of shifting industries from the average level of exposure to the sample mean (a shift of 1.9 standard deviations). If all industries produced only heterogeneous goods, then eliminating Chinese imports would have only prevented a 1.6pp decline in deunionization. If, on the other hand, all industries produced only homogeneous goods, eliminating the imports would have prevented 6.3pp of the decline (half of the observed average decline).

[15] Rauch (1999) produces a *product-level* classification in which a good is homogeneous if there exists an internationally listed "reference price" (as with crude oil, unprocessed lead, etc.). Because industries produce multiple goods (e.g., the oil and gas industry produces homogeneous crude oil and non-homogeneous gasoline), the industry-level classification is non-binary. Across industries, the 1990-employment-weighted average of "homogeneous goods" is 22%. In 1990, 51% of US manufacturing employment was in industries where homogeneous goods account for less than 1% of output (77%: less than 1/3 of output). Thus, most US manufacturing is of heterogeneous goods. Roughly 15% of 1990 US manufacturing employment was in industries where homogeneous goods made up the majority of output (12%: 80% or more of output). Our empirical results are similar if we use a binary variable for industries that mostly produce homogeneous goods.

[16] Another test one might imagine is whether exposure has greater effects on the industries with higher estimated union wage premia (perhaps only for homogeneous-goods industries). In results available upon request, we find some evidence for this, but with only 62 industries, a triple-interaction, and noisy measures of the premium, none of those interactions are precise or statistically significant.

3.2 State-level effects of exposure

Although Chinese import penetration caused de-unionization within manufacturing, effects on overall unionization are unclear. Displaced manufacturing workers may become union members in other parts of the economy, such as construction (Charles, Hurst, and Notowidigdo, 2019).[17] To examine the broader effects of exposure, we look to state-level variation. In Table 2 we consider changes in state-level population shares for four mutually exclusive groups: non-employment (28% of working age people at baseline); non-manufacturing, non-union workers (51%); union workers outside manufacturing (8%); and manufacturing workers (13%). Consistent with ADH, column 1 shows that import exposure significantly increased non-employment and column 4 shows it significantly reduced manufacturing employment. The results show that non-employment absorbed roughly half of the 1.5 percentage point decline in manufacturing employment among the population.

[Table 2 about here.]

Interestingly, column 3 shows that a one standard deviation increase in exposure increases unionized employment *outside manufacturing* by 0.3 percentage points. This effect is statistically significant ($p < .01$), roughly a fifth of the decline in manufacturing employment, and is nearly as large as the non-significant increase in non-union jobs outside manufacturing (despite those non-union jobs being so much more prevalent in the labor market as a whole). We view this as a large effect. For interpretation, however, it is important to note that the average state 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 of the decline (roughly a quarter of the average decline).

As with the industry-level effects, we relegate our extensive robustness checks to the appendix. Our core result, which we consider surprising, is that exposure increases the share of the population working in unionized jobs outside manufacturing. We show that this result is the same when using the two identification strategies separately; when adding a rich set of controls (for baseline characteristics, industry characteristics, and important geographic characteristics); regardless of how we handle "exposure" outside manufacturing; and when we use the Borusyak, Hull, and Jaravel (2018a) approach to calculate standard errors.[18] We

[17] Unionization used to be substantially higher in manufacturing than outside of it (1990: 20% within manufacturing vs. 13% outside of it), but this is longer true (2014: 9% within vs. 10% outside). It is plausible that reallocating workers from manufacturing to other sectors would leave unionization rates unchanged.

[18] Borusyak et al. (2018a) rightly acknowledge that our core identifying variation is across industries, not states. Ignoring this, the standard errors from a state-level regression are wrong. They propose a method

also show that there is no "placebo effect" of exposure on changes in non-manufacturing union employment prior to 1990.

We do not view any of these results as changing our substantive interpretation, and refer the interested reader to the appendix. Here, we again focus on interpreting what we consider to be the surprising results. In particular, we consider the magnitude of the outside-of-manufacturing effects in light of the small within-manufacturing effects.

3.3 Interpreting magnitudes

We decompose the effect of exposure on unionization into a within-manufacturing effect and a between sector effect. The derivation of decomposition is available in Appendix A.4; the results are presented in Table 3.

The first two columns are based on calculations from the raw data. They show that total union density within manufacturing declined by 12.3 percentage points from 1990 to 2014. This was mainly driven by within-industry declines (holding the relative size of different manufacturing industries constant) averaging 13.2 percentage points, which is only slightly offset by a small increase in the between-industry component (i.e., holding the unionization rate of each industry constant, highly unionized industries shrank by somewhat less than less unionized ones, increasing average unionization in manufacturing through an industry composition change). However, given that manufacturing makes up only around 15% of total employment, even a dramatic decline *within* manufacturing has only effects on economy-wide unionization. While the non-manufacturing sector saw a smaller decline (2.9pp), column 2 shows those declines (holding sectoral shares constant) drive the majority of the economy-wide pattern (explaining 2.5pp of the observed 4.5pp decline).

[Table 3 about here.]

Thus, it might appear that Chinese imports are not important for aggregate US unionization. But in Section 3.2, we found that import competition does affect unionization outside manufacturing. Columns 3 and 4 assess the aggregate effects of exposure. Specifically, we construct a counterfactual scenario in which we set each manufacturing industry's exposure equal to the sample minimum, and use our estimates to calculate what the components of the decomposition would be in this counterfactual.[19] Columns 3 and 4 report our results. Within-manufacturing union density would have declined by 10.9 percentage points. In other

to restructure the regression to the industry-level, which provides accurate standard errors. In our case, industry-based standard errors are roughly one-third as large, and all coefficients are highly significant.

[19]Between-industry effects are from Table 1's estimates for total employment; within-industry effects are from Table 1's estimates for union density. Effects on union share outside manufacturing are from Table 2.

words, we estimate that 83% of the decline in union density within manufacturing would have occurred even without import competition. Combining the within-manufacturing-industry effects and the between-manufacturing-industry effects, total effects within manufacturing account for only 0.3pp (or 7%) of the 4.5pp decline we see in the data.[20]

A much larger effect emerges 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 (6.1 percentage points instead of 4.5).

4 Spillovers

Our decomposition suggests the effects of import competition on unionization *outside of manufacturing* are the most important part of the revised story. 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 induces *other* household members to take disproportionately unionized jobs?

4.1 Descriptive evidence

We start by identifying "manufacturing-type" workers in the 2014 CPS sample. Specifically, we use the 1990 CPS sample to train a machine learning algorithm to predict manufacturing employment using a rich set of demographics (details in Appendix A.5). We then use these same observed demographic variables in the 2014 sample to identify the respondents who most "look like" manufacturing workers from 1990. The purpose of this exercise is not to identify 2014 respondents who actually worked in manufacturing in 1990. Rather, we seek to identify the 2014 respondents who likely *would have* worked in manufacturing had they been in the economy of 1990. This approach is conceptually similar to the well-known DiNardo, Fortin, and Lemieux (1996) decomposition. We view it as a valuable approach for understanding how the labor market experiences of these demographic groups have changed. However, it is fundamentally a descriptive approach. Many of the demographic characteristics most predictive of manufacturing employment (i.e., most valuable for defining manufacturing-type workers) are endogenous variables like education and state-of-residence that could be affected by import exposure. Nonetheless, they allow us

[20] In columns 2 and 4, the between-industry component is modified to include the relative size of manufacturing as a whole (rather than just the size of individual manufacturing industries, see the appendix for details and equations), though this matters little in practice.

to characterize changes in the labor market experience of well-defined demographic groups (groups for whom manufacturing jobs used to be critical). Appendix Table A12 illustrates some of the characteristics that help identify manufacturing-type workers by comparing them to the full sample. It also summarizes the traits of these manufacturing-types' household members (spouses, children, etc.), whom we might think of as individuals indirectly affected by lost manufacturing opportunities. We seek to describe how the labor market experiences of manufacturing-type respondents and members of their households have changed.

Table 4 presents a detailed breakdown of the main industries seeing changing employment shares among manufacturing-type workers. We see that 14 percentage points of the 16pp decline in manufacturing employment among manufacturing-type workers has been absorbed into a relatively small number of activities. Obviously concerning is the observation that nearly half of the decline went into non-employment, consistent with results from ADH. Roughly a fifth has gone into construction, consistent with Charles et al. (2019) who find that the housing boom helped mask the manufacturing decline of the early 2000s. Because construction tends to have similar wages and unionization levels as manufacturing, one might interpret this as leaving workers' well-being mostly unchanged. However, most of the remaining employment growth appeared in low-wage, non-unionized industries like restaurants, landscaping, and automotive repair. This implies a significant decline in real incomes for these workers (even among those who manage to become re-employed). It also suggests that a reallocation of manufacturing-type workers themselves is unlikely to explain growth in unionized employment outside of manufacturing.

[Table 4 about here.]

Panel B considers the most logical alternative explanation: That the spouses and children of these would-be manufacturing workers are the ones accounting for the trade exposure-induced increase in non-manufacturing unionization. Consistent with this hypothesis, we see dramatic growth in employment in education and health. For this population, these sectors represent relatively high wages and particular high (education) or stable (healthcare) levels of unionization during a period of stagnant wages and declining union density at the aggregate level. Figure A3 in the appendix shows the full set of compositional adjustments among these household members. It shows that, unlike manufacturing-type workers themselves, these individuals are persistently flowing towards higher wage and more unionized sectors,[21] particularly by abandoning retail for jobs in health and education.

[21]One might wonder whether these individuals are specifically targeting unionized industries, or whether this is simply a byproduct of flowing into higher-paying industries. A simple "horse race" suggests that growth is primarily explained by relative wages. Conditional on industry median wages at baseline, baseline unionization itself does not predict changes in employment among this population (Table A13).

4.2 Differential effects of state-level exposure

Thus far, we have shown that state-level import exposure causally increases employment in unionized jobs outside manufacturing. Our descriptive evidence above shows that manufacturing-type workers themselves are not increasingly found in highly unionized industries (quite the opposite) but their spouses and children are. This suggests a testable hypothesis for understanding the results from our state-level analyses: If Chinese imports—by decimating manufacturing employment opportunities—accelerated the shift from retail to work in healthcare and education, then these shifts should be strongest in the most exposed states.

To test this, we use our basic machine learning approach to identify manufacturing-type workers (as above) and retail-type workers (again using observable demographics, a probability model estimated among the 1990 sample, and applied in the 2014 sample). We then use a triple-difference approach to ask how the employment experiences of manufacturing-type and retail-type workers have changed in highly exposed states. Specifically, for an individual j living in state s at time t, we estimate:

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

where ManufProb_{js} and RetailProb_{js} are the estimated probabilities that individual j works in manufacturing and retail, respectively.

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 time. We control for ManufProb_{js} (RetailProb_{js}) to control for baseline differences between the full population and manufacturing-type (retail-type) individuals.

For ease of interpretation, we have normalized the estimated probabilities to have minimum zero and maximum one within the sample; thus, the main effect of exposure can be interpreted as the effect for the sample individual predicted to be *least* likely to work in manufacturing, and the interaction can be interpreted as difference for the individual *most* likely to work in manufacturing. The results are presented in Table 5.

[Table 5 about here.]

Column 1 shows that high-exposure states saw differential employment declines for the types of workers likely to work in retail or manufacturing. This underscores a consistent theme throughout the literature on trade exposure: less-educated workers disproportionately feel the effects of trade exposure. Columns 2-4 confirm the hypothesis suggested by the descriptive evidence above. In high-exposure states, manufacturing-type workers are more likely to shift into service jobs (1pp), and retail-like workers are more likely to leave retail (.9pp) and shift into jobs in health and education (.3pp). Overall, columns 5 and 6 show that, conditional on employment, manufacturing-type workers were pushed into industries with .9pp lower baseline union density and $.30/hour lower wages, while retail-type workers sorted into industries with .6pp higher baseline union density and roughly similar wages.

Overall, then, the triple-difference estimates in Table 5 support the descriptive evidence from above: import exposure increased union density outside of manufacturing by accelerating the shift of workers from retail towards more unionized jobs in healthcare and education. It did not increase unionized employment among the workers pushed out of manufacturing; those workers tended to be relocated to low-wage non-unionized jobs in the service sector.

5 Right-to-Work

Our results thus far describe the average effect of import exposure on state-level labor market outcomes. Given the wide variation in labor law across American states, we ask whether the effect of exposure might differ based on the presence of "Right-to-Work" legislation, widely considered the most central anti-union laws.

In Table 6, we interact import exposure with states' RtW status. The results show important differences by RtW status. Column 1 shows that non-RtW states saw very little increase in non-employment as a function of exposure, while RtW states saw significantly ($p < .10$) more: 1.2pp vs .2pp. Columns 2 and 3 show that both RtW and non-RtW states increased the share of the population employed outside of manufacturing by a similar amount (0.81pp for RtW states, 0.73pp for non-RtW states), although RtW states saw this growth concentrated in non-union jobs while non-RtW states saw more of it flow towards unionized jobs. However, column 4 shows that the manufacturing declines were more pronounced in RtW states. Estimates imply that, for a one standard deviation increase in exposure, RtW states saw double the manufacturing decline of non-RtW states (2pp vs. 1pp).

[Table 6 about here.]

This is not an artifact of linear regression; RtW and non-RtW states saw similar average levels of exposure, and Appendix Figure A4 non-parametrically shows the declines in manufacturing employment were much steeper in RtW states. Using the CBP, we can further show that even within very narrowly defined industries (SIC), RtW states see significantly larger employment declines from exposure (appendix Table A15). This is also not a function of RtW states having "further to fall": baseline manufacturing employment per capita did not significantly differ between RtW and non-RtW states, and if anything was slightly *lower* in RtW states (13.2pp vs. 12.9pp, $p = .77$). In other words, there is no mechanical reason why the estimated effects of exposure should be larger in RtW states.

With no mechanical explanation, we ask whether differential product heterogeneity can account for these RtW effects. We argued above that import exposure only has meaningful effects on union density in industries producing homogeneous goods, whereas in industries producing heterogeneous goods, unionized firms are relatively shielded from low-wage country competition. In the RtW case, we expect that the larger exposure effects on RtW states' *total* manufacturing employment should be confined to heterogeneous goods industries (where unionized firms in non-RtW states avoided direct competition). In Appendix Table A15 we test this using a CBP-based panel of employment by state and industry.[22] As predicted by our hypothesis, exposure differentially decreases RtW states' employment in heterogeneous goods industries, not homogeneous goods industries. In the small number of homogeneous goods industries, RtW states actually experienced significantly *smaller* effects of exposure ($p < .10$), consistent with the "standard story."

It is, of course, possible for manufacturing declines to be steeper in RtW states, but for the labor market to effectively absorb the additional declines. Table 6 shows this did not happen; the additional manufacturing job loss in RtW states flowed almost entirely into non-employment. One hypothesis for understanding this difference is that the absence of unions in RtW states prevented the emergence of wage premia in health and education and undermined the transformation in the industrial composition of women's employment. In Table A16 of the appendix, we report the results of basic Mincer regressions for less-educated women in 1990—the demographic group for whom the indirect effects of declining manufacturing were most important. We find large OLS wage premia in healthcare and education among these women, but only in non-RtW states. In RtW states (where unions are rare), these sectors pay no wage premia. This could explain why such workers did not transition into health and education.

We note that Bloom et al. (2019) also document geographic heterogeneity in the effects of Chinese import exposure, which they attribute to human capital differences across US states. Their measure of human capital (college degree proportion) is negatively correlated with Right-to-Work status (-.42). In a simple horse race regression on manufacturing job loss (Appendix Table A17), we include both variables interacted with exposure. We find that the interaction with RtW is statistically significant while the interaction with education

[22] Of course, union status is not directly available in the CBP.

is not (though we acknowledge it is of a similar magnitude).

6 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. To our surprise, a quantitatively bigger effect is that Chinese import competition slowed 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 would have been likely to work in manufacturing are disproportionately shifted towards non-employment, construction, and low-wage, low-unionization services. At the same time, however, import competition accelerated the shift of less-educated women out of retail and into higher paying industries, especially the relatively unionized healthcare and education sectors.[23] These results are consistent with broader patterns in the US labor market over the last 50 years. For one, labor force participation among less-educated men has declined alongside family formation. Among less-educated Americans today, more women than in the past are employed bread-winners whereas men are increasingly relying on parental income and do not support a family (Binder and Bound, 2019). Second, in the bottom half of the wage distribution, women have achieved dramatic progress in closing the residual gender wage gap (Blau and Kahn, 2017), even when compared with the (positively selected) *employed* men. Finally, attitudes have become much more favorable towards women (and especially mothers) working (Donnelly et al., 2016), which is partly *caused by* more women in the workplace (Bastian, 2020; Fernández, Fogli, and Olivetti, 2004). Our results suggest that the collapse in manufacturing may unite these patterns: the collapse of manufacturing

[23] As we note in the appendix, this shift is also apparent among those who live with "manufacturing-type" workers, although that requires conditioning on household formation, which may be problematic (Autor et al., 2019).

pushed women towards becoming bread-winners and taking more competitive salaries in the labor market, both of which, in turn, shift gender norms and create a reinforcing cycle.

In short, the "standard story" linking trade with China to US deunionization needs revising. Trade did contribute modestly to the decline in unionization in private sector manufacturing and this likely did weaken organized labor's bargaining position in manufacturing industries. But the spillover effects within households combined with the changing structure of the US labor movement implied that the China shock actually slowed the overall decline in union density.

Finally, our results highlight the importance of state laws for understanding the labor market consequences of adverse shocks. We showed that RtW states 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 less-educated women simply had no access to high paying sectors towards which they could reallocate.

Table 1: Import effects on manufacturing industry-level unionization

	(1)	(2)	(3)	(4)	(5)	(6)
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**	-0.008*
	(0.075)	(0.118)	(0.076)	(0.005)	(0.005)	(0.004)
Exposure $\times$ Homogen. goods						-0.024
						(0.018)
R^2	0.164	0.337	0.265	0.861	0.871	0.882
N	64	64	64	64	64	62
Controls:						
Union mem. (1990)	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	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. Columns 5 and 6 condition on the covariates considered in Table A3 (capital intensity, skill share, textiles). Import exposure combines the NTR Gap and the ADH ΔChina-Other Trade, and has unit standard deviation across industries. Results separating the identification strategies are available in the appendix. The sum of coefficients in column 6 is statistically significant ($p < .10$; i.e., there *is* a statistically significant effect of import exposure on union density in fully homogenous-goods industries).

Table 2: State-level effects of exposure to import competition

	(1)	(2)	(3)	(4)
DV: Δ share working age pop.	Non-emp.	Non-manuf., non-union	Non-manuf., union	Manufact.
Import exposure	0.721**	0.434	0.324***	-1.479***
	(0.300)	(0.270)	(0.119)	(0.252)
R^2	0.134	0.044	0.140	0.492
N	51	51	51	51
DV mean in 1990	28.0	51.1	7.8	13.1
Avg change '90-'14	3.9	3.0	-1.3	-5.7

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, are weighted by state employment in 1990, and are based on working age persons (age 16-64). "States" includes the District of Columbia. Coefficients in columns 1-4 sum to zero because the population shares sum to one (i.e., groups are mutually exclusive and exhaustive). To calculate exposure, we standardized state-level measures of "NTR Gap" and "ΔChina-Other Trade" to have standard deviation 1 across states, sum them, and re-standardize the sum to have standard deviation 1 across states. Results based on these two measures disaggregated can be found in the appendix.

Table 3: 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 manuf-industry	0.9	-0.1	0.7	0
Within manuf-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

Between-industry effects: Table 1, within-industry effects: Table 1, outside of manufacturing effects: Table 2. Counterfactual change based on setting each industry's exposure is equal to the sample minimum across industries.

Table 4: Industrial composition: Manufacturing-types and household members

Industry (includes non-employment)	(1) Share of pop. 1990	(2) Share of pop. 2014	(3) Change in pop. share	(4) Median wage (1990)	(5) 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 5: Exposure effects for manufacturing-type and retail-type workers

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Emp. (×100)	Service jobs (×100)	Health or Educ. (×100)	Retail (×100)	Industry union density	Industry median wages
$\text{Exposure}_s \times 1\{\text{Year} = 2014\}$	2.72***	-0.34***	0.33**	0.05	0.42***	0.15**
	(0.417)	(0.104)	(0.140)	(0.135)	(0.091)	(0.072)
$\text{Exp}_s \times \text{'14} \times \hat{P}_j(\text{Manuf.})$	-2.36***	1.05***	-0.49***	1.15***	-0.93***	-0.27***
	(0.414)	(0.085)	(0.101)	(0.105)	(0.095)	(0.028)
$\text{Exp}_s \times \text{'14} \times \hat{P}_j(\text{Retail})$	-3.99***	-0.07	0.34***	-0.86***	0.60***	-0.05***
	(0.131)	(0.069)	(0.086)	(0.066)	(0.052)	(0.018)
Conditional on emp.					Yes	Yes
R^2	0.070	0.022	0.054	0.029	0.044	0.097
N	1481638	1481638	1481638	1481638	1010775	1010775
DV mean (1990)	69.4	4.3	11.9	11.6	16.3	16.7
p for H_0: $\beta_1 + \beta_2 = 0$	0.229	0.000	0.204	0.000	0.000	0.088
p for H_0: $\beta_1 + \beta_3 = 0$	0.011	0.001	0.000	0.000	0.000	0.165

* $p < .10$, ** $p < .05$, *** $p < .01$. 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. "Retail Probability" is analogous. "Service jobs" refers to eating and drinking places, landscaping, and automotive repair (see Table 4). 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", "Retail Probability", and state and year fixed effects.

Table 6: Heterogeneous effects of exposure to import competition

	(1)	(2)	(3)	(4)
DV: Δ share working age pop.	Non-emp.	Non-manuf., non-union	Non-manuf., union	Manufact.
Import exposure	0.243	0.257	0.468**	-0.968***
	(0.362)	(0.457)	(0.195)	(0.147)
RtW $\times$ exposure	0.959*	0.391	-0.308	-1.042***
	(0.490)	(0.552)	(0.223)	(0.372)
R^2	0.211	0.093	0.222	0.553
N	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, and are based on working age persons (age 16-64). "States" includes the District of Columbia. Coefficients in columns 1-4 sum to zero because the population shares sum to one (i.e., groups are mutually exclusive and exhaustive). To calculate exposure, we standardized state-level measures of "NTR Gap" and "ΔChina-Other Trade" to have standard deviation 1 across states, sum them, and re-standardize the sum to have standard deviation 1 across states. Results based on these two measures disaggregated can be found in the appendix. RtW includes right-to-work laws implemented 2001 or earlier (only Oklahoma implemented an RtW law during our sample, in 2001). All regressions include RtW as a main effect (not reported).

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Ahlquist and Downey

Appendix

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A Appendix

A.1 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.[24] Our core employment results for both states and industries are based on Census-defined industries.

A.1.1 Adjusting industry codes

There are two industry classification systems in the United States. Data based on firms (the ASM, CBP, LBD, and more) use the Standard Industrial Classification (SIC) and the North American Industrial Classification (NAICS, which replaced SIC in 1997). The original ADH paper (using the CBP) and PS paper (using the LBD) 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.[25]

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 (as PS themselves do).

[24] 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.

[25] 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.

A.1.2 Summary statistics

[Table A1 about here.]

A.1.3 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. Thus, we lose a great deal of variation. As a first step we demonstrate that the core findings from ADH and PS still hold under coarser industrial classification.

Table A2 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.[26] The upper panel (A) uses the change in China-Other trade as the measure of import penetration.[27] 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 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 A2 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).[28] 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

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

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

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

of spillovers across SIC-industries due to product substitutability.[29] 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 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.

A.2 Correlation with baseline union density

A.2.1 Autor, Dorn, 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 A1 shows that these industries differ in their

[29]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.

historical unionization rates. On average, industries with the most growth in China-Other trade had lower rates of unionization in 1990.[30]

[Figure A1 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.[31]

As shown in columns 1 and 2 of Table A3, these three controls eliminate virtually all of the relationship between baseline unionization and subsequent growth in China-OECD trade. The coefficient in column 2 is no longer statistically significant, and the magnitude is less than 20% that of column 1.

[Table A3 about here.]

A.2.2 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 A2 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 A2 about here.]

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

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

In the bottom panel of Table A3 we show that, like China-OECD trade, capital-intensity, skill-intensity, and the textile/apparel sector explain this correlation. 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).

A.3 Robustness

A.3.1 Industry-level

[Table A4 about here.]

[Table A5 about here.]

A.3.2 State-level

[Table A6 about here.]

[Table A7 about here.]

[Table A8 about here.]

[Table A9 about here.]

[Table A10 about here.]

A.4 Decomposition

These decompositions are mathematical identities that, in themselves, rely on no assumptions. 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.

We can expand this decomposition to include the change in union density for total employment (including non-manufacturing):

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

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.

A.5 Manufacturing-type workers

A.5.1 Methodological approach

We use a machine-learning approach to identify workers most directly affected by the manufacturing decline. 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 preferred 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, including: 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 working-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.

We 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 A11 compares the performance of different approaches for defining "manufacturing-type workers," as a function of the same estimated probabilities.

[Table A11 about here.]

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.[32] Our approach follows in the tradition of the well-known DiNardo, Fortin, and Lemieux (1996) decomposition.

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

To define retail-type workers, we use this exact same approach, except predicting retail employment in 1990 instead of manufacturing employment.

We also 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."

A.5.2 Who are manufacturing-type workers?

Panel A of Table A12 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 A12 about here.]

A.5.3 Results

Figure A3 shows the largest employment declines for household were concentrated in retail and even within broad sectors, higher paying industries saw more growth.

[Figure A3 about here.]

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 A13, 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). 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 A13 about here.]

In the main text of the paper (Table 5), we show that retail-type workers shifted out of retail and towards health and education more in more exposed states (compared to other workers in those states). Our focus on retail was motivated by our finding that household members tended to abandon retail. We focus on retail-type workers instead of household members because it avoids conditioning on the endogenous process of manufacturing-types' household formation. It does, however, condition on endogenous choices like education (which is predictive of retail status). Table A14 presents the alternative approach, focusing on household members of manufacturing-types. This conditions on household formation, but does not condition on education to define retail-type. The substantive results are the same: For the spouses and children of manufacturing-type individuals, exposure increases the shift into healthcare and education jobs and the average unionization and wages of the industries employing these workers.

[Table A14 about here.]

A.6 Right-to-Work results

[Figure A4 about here.]

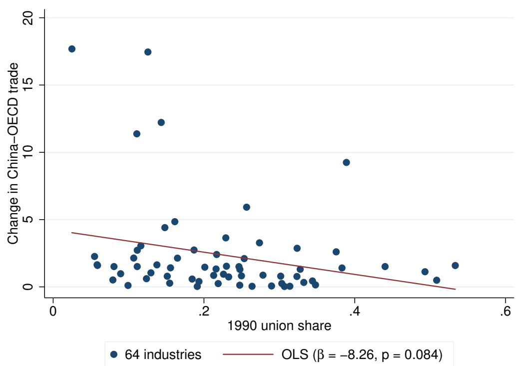
[Table A15 about here.]

[Table A16 about here.]

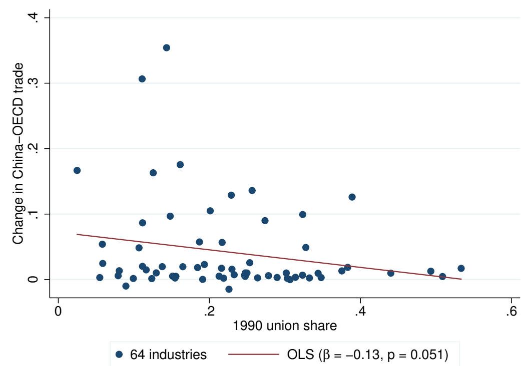
[Table A17 about here.]

[Figure A5 about here.]

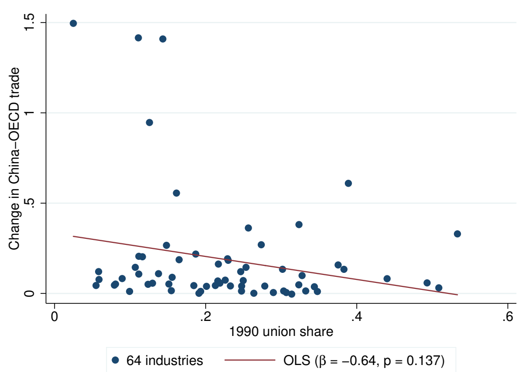
Figure A1: Autor-Dorn-Hanson instrument and lagged unionization



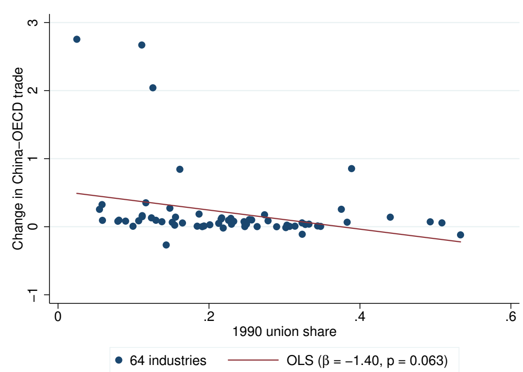
(a) Change 1990-2014



(b) Change 1990-2000



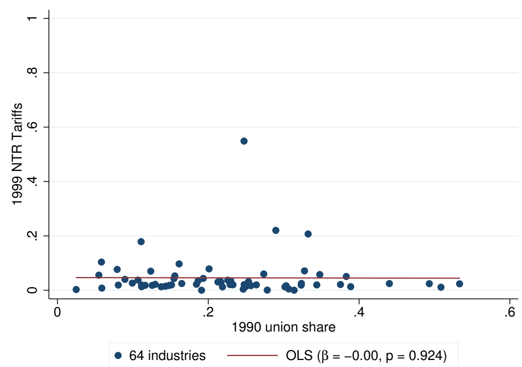
(c) Change 2000-2007



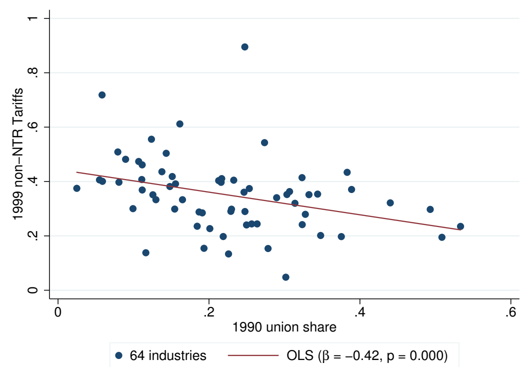
(d) Change 2007-2014

Line is OLS.

Figure A2: Pierce-Schott instrument and lagged unionization



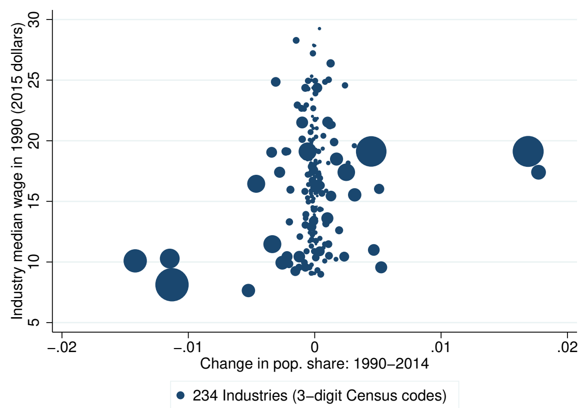
(a) NTR tariffs



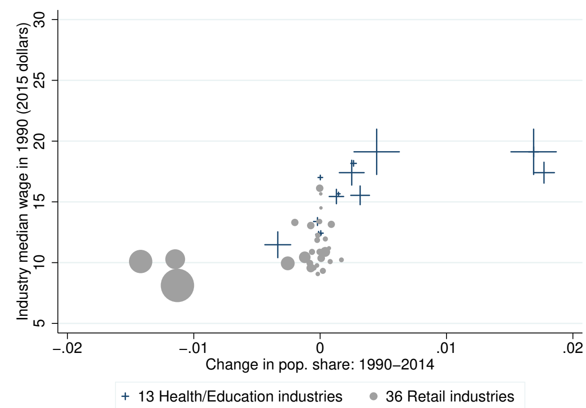
(b) non-NTR tariffs

Line is OLS.

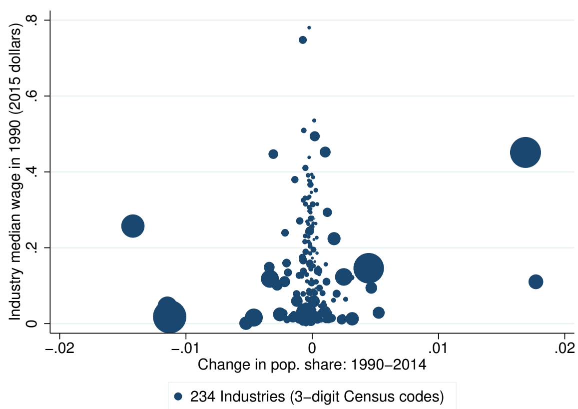
Figure A3: Characteristics of industries seeing largest changes in household members' employment



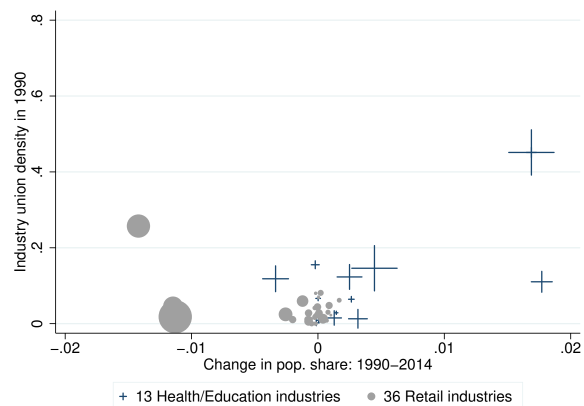
(a) Median wages (all industries)



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



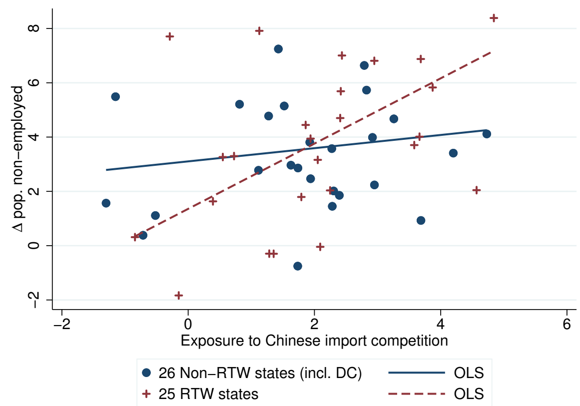
(c) Union density (all industries)



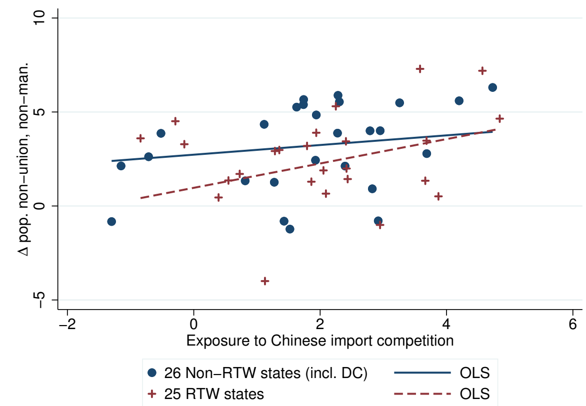
(d) Union density (retail, ed., health)

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.

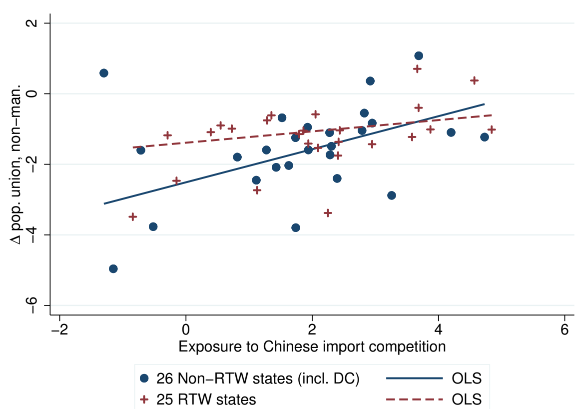
Figure A4: Non-parametric heterogeneity by RtW status



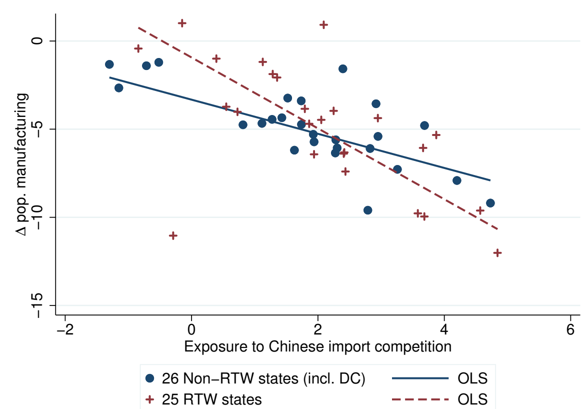
(a) Non-employed



(b) Non-union, non-man.



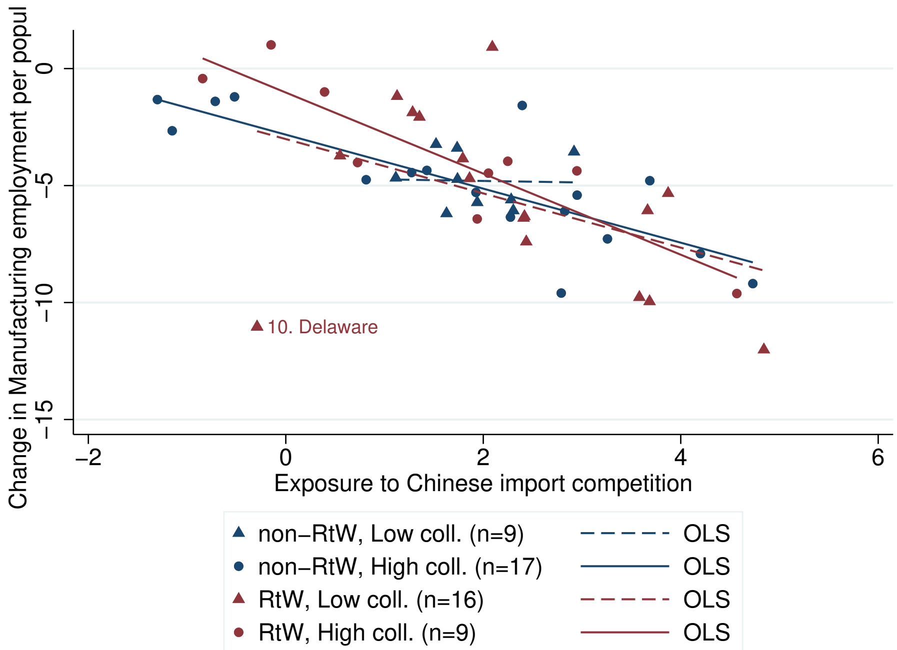
(c) Union, non-man.



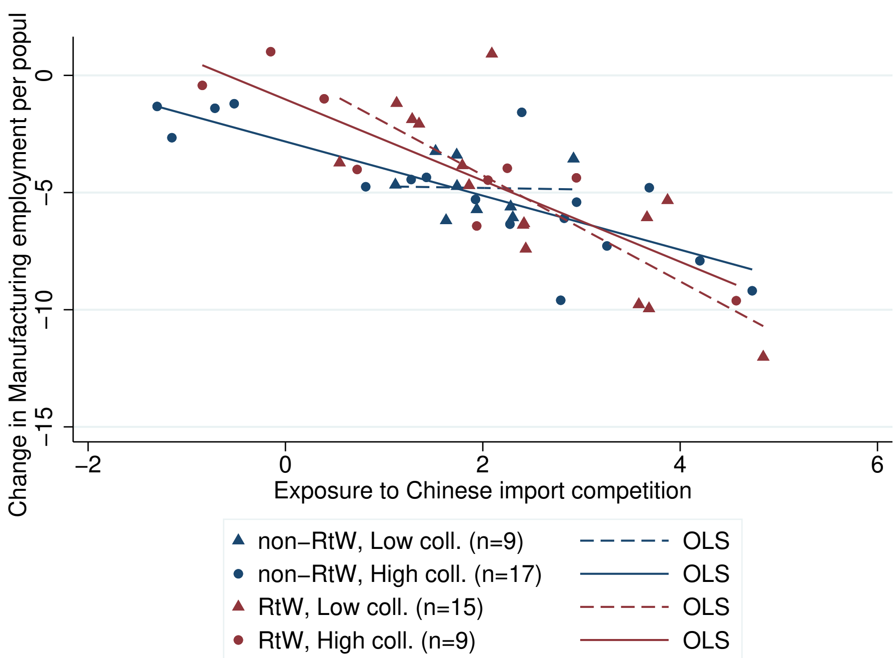
(d) Manufacturing

Figure reflects changes in share of the working age population (1990-2014), as a function of state-level import exposure. Formal regressions included in Table 6. Outlier in Panel (d) is Delaware.

Figure A5: Right-to-Work vs. Baseline (1990) education (non-parametric)



(a) All states



(b) Excluding Delaware

Figure reflects changes in manufacturing share of the working age population (1990-2014), as a function of state-level import exposure. Formal regressions included in Table A17.

Table A1: Summary statistics

				Percentiles				
Variable	Mean	SD	N	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-Other. 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-Other. 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 A2: 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 A3: Explaining the correlation between 1990 density and exposure

	(1)	(2)	(3)	(4)
DV:	1990 Union Density (members as share of employment)			
Δ China-Other Trade	-4.112***	-0.743		
	(1.291)	(1.500)		
Non-NTR Tariff Rate (1999)			-4.963***	-2.504
			(1.593)	(2.027)
R^2	0.104	0.388	0.152	0.404
N	64	64	64	64
Controls		Yes		Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. Controls: Skill share, capital-labor ratios, and dummy for textile sector. 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). Both measures of exposure are normalized to have unit standard deviation.

Table A4: Industry-level effects separately by identification strategy

	(1)	(2)	(3)	(4)
DV:	Δ ln(Employment)		Change in	
	Union mem.	Non-mem.	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 A3, these explain most of the relationship between 1990 unionization and the instruments.

Table A5: Placebo (pre-1990) industry-level effects

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Change in Union member share (1985-1990)					
Δ China-Other Trade	0.003	-0.001				
	(0.004)	(0.004)				
NTR Gap			0.005	0.005		
			(0.004)	(0.004)		
Import exposure					0.005	0.002
					(0.004)	(0.005)
R^2	0.015	0.097	0.029	0.116	0.033	0.099
N	64	64	64	64	64	64
Controls:		Yes		Yes		Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1985 to 1990 and are weighted by 1990 industry employment. Controls include 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. "Import exposure" refers to the composite measure combining the ADH and PS instruments. All three instruments have unit standard deviation (by construction).

Table A6: State-level effects separately by identification strategy

DV: Δ share working age pop.	(1) Non-emp.	(2) Non-manuf., non-union	(3) Non-manuf., union	(4) Manufact.
	Panel A: Autor-Dorn-Hanson identification strategy			
Δ China-Other Trade	0.534*	0.457*	0.313***	-1.304***
	(0.298)	(0.270)	(0.101)	(0.279)
R^2	0.074	0.049	0.131	0.383
N	51	51	51	51
	Panel A: Pierce-Schott identification strategy			
NTR Gap	0.762**	0.324	0.271*	-1.357***
	(0.334)	(0.271)	(0.144)	(0.294)
R^2	0.150	0.025	0.098	0.414
N	51	51	51	51
p for H_0: $\beta_{ADH} = \beta_{PS}$	.612	.729	.810	.896

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, are weighted by state employment in 1990, and are based on working age persons (age 16-64). "States" includes the District of Columbia. Coefficients in columns 1-4 sum to zero because the population shares sum to one (i.e., groups are mutually exclusive and exhaustive). "NTR Gap" and "ΔChina-Other Trade" have standard deviation 1 across states.

Table A7: Robustness to state-level controls

	(1)	(2)	(3)	(4)	(5)
Numerator:	Non-emp.	Non-man., non-union	Non-man., union	Man.	Union
Denominator:	Pop.	Pop.	Pop.	Pop.	Emp.
			Panel A: Baseline		
Import exposure	0.721**	0.434	0.324***	-1.479***	0.538*
	(0.300)	(0.270)	(0.119)	(0.252)	(0.312)
R^2	0.134	0.044	0.140	0.492	0.053
N	51	51	51	51	51
			Panel B: 9 controls (see notes for details)		
Import exposure	0.160	-0.304	0.340**	-0.196	0.539**
	(0.315)	(0.285)	(0.165)	(0.250)	(0.264)
R^2	0.615	0.711	0.364	0.802	0.809
N	51	51	51	51	51

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014 in either population or employment shares. All regressions weighted by state employment in 1990. "States" includes the District of Columbia. All regressions based on working age persons (age 16-64). Panel B controls for fixed effects for four Census regions, 1990 share of population (26-64) with a college degree, 1990 manufacturing share of employment, and 1990 union share of employment, as well as variables from Table A3 converted to the state-level in the same way as import exposure (skill share, capital-labor ratio, and a dummy for textiles).

Table A8: Placebo (pre-1990) state-level effects

	(1)	(2)	(3)	(4)
DV: Δ share working age pop.	Non-emp.	Non-manuf., non-union	Non-manuf., union	Manufact.
Import exposure	0.580**	0.062	-0.058	-0.584***
	(0.223)	(0.125)	(0.074)	(0.205)
R^2	0.143	0.003	0.011	0.222
N	51	51	51	51
DV mean in 1985	31.3	47.3	7.8	13.6
Avg change '85-'90	-3.3	3.8	-0.0	-0.5

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1985 to 1990, are weighted by state employment in 1990, and are based on working age persons (age 16-64). "States" includes the District of Columbia. Coefficients in columns 1-4 sum to zero because the population shares sum to one (i.e., groups are mutually exclusive and exhaustive). To calculate exposure, we standardized state-level measures of "NTR Gap" and "ΔChina-Other Trade" to have standard deviation 1 across states, sum them, and re-standardize the sum to have standard deviation 1 across states.

Table A9: Robustness to non-manufacturing exposure

	(1)	(2)	(3)	(4)	(5)
Numerator:	Non-emp.	Non-man., non-union	Non-man., union	Man.	Union
Denominator:	Pop.	Pop.	Pop.	Pop.	Emp.
	Panel A: ADH: zero, PS: excluded (Baseline)				
Import exposure	0.721**	0.434	0.324***	-1.479***	0.538*
	(0.300)	(0.270)	(0.119)	(0.252)	(0.312)
R^2	0.134	0.044	0.140	0.492	0.053
	Panel B: ADH: zero, PS: zero				
Import exposure	0.486	0.760***	0.171*	-1.417***	-0.354
	(0.305)	(0.232)	(0.098)	(0.266)	(0.302)
R^2	0.061	0.135	0.039	0.452	0.023
	Panel C: ADH: excluded, PS: zero				
Import exposure	0.683**	0.302	0.369***	-1.354***	0.526*
	(0.304)	(0.283)	(0.100)	(0.273)	(0.266)
R^2	0.121	0.021	0.182	0.412	0.051
	Panel D: ADH: excluded, PS: excluded				
Import exposure	0.688**	-0.148	0.401***	-0.940***	1.273***
	(0.321)	(0.357)	(0.122)	(0.267)	(0.357)
R^2	0.122	0.005	0.215	0.199	0.300

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014 in either population or employment shares. All regressions weighted by state employment in 1990. "States" includes the District of Columbia. All regressions based on prime age persons (age 16-64). Panels differ in whether non-manufacturing industries are assigned zero exposure when creating state-level aggregate exposure, or are excluded from the calculation (i.e., whether exposure is based only on exposure among manufacturing industries).

Table A10: Borusyak, Hull, and Jaravel (2018a) industry-level implementation

	(1)	(2)	(3)	(4)
DV: Δ share working age pop.	Non-emp.	Non-manuf., non-union	Non-manuf., union	Manufact.
Import exposure	0.753***	0.610***	0.286***	-1.650***
	(0.100)	(0.070)	(0.042)	(0.098)
R^2	0.254	0.234	0.266	0.624
N	330	330	330	330

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an industry (SIC 1987 with Dorn adjustment), where all non-manufacturing industries are combined into one single industry. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014. Coefficients in columns 1-4 sum to zero because the population shares sum to one (i.e., groups are mutually exclusive and exhaustive). See Borusyak, Hull, and Jaravel (2018a) for methodological details, and Borusyak, Hull, and Jaravel (2018b) for implementation. Results are nearly identical when omitted the non-manufacturing industry. Scatterplots (available upon request) show no outliers.

Table A11: 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 A12: 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 A13: 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 A14: Exposure effects for manufacturing-type workers and household members

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Emp. (×100)	Service jobs (×100)	Health or Educ. (×100)	Retail (×100)	Industry union density	Industry median wages
Exposure × 1{Year=2014}	0.57	-0.56***	0.74***	-0.01***	0.94***	0.23***
	(0.509)	(0.134)	(0.120)	(0.002)	(0.092)	(0.070)
Exp. × '14 × Man. Prob.	-2.04***	1.35***	-1.06***	0.02***	-1.38***	-0.49***
	(0.547)	(0.097)	(0.089)	(0.001)	(0.097)	(0.027)
Exp. × '14 × Max HH Man. Prob.	-0.33**	0.01	0.23***	-0.00***	0.13***	0.04***
	(0.128)	(0.030)	(0.056)	(0.000)	(0.032)	(0.010)
Conditional on emp.					Yes	Yes
R^2	0.059	0.003	0.024	0.003	0.020	0.032
N	1481638	1481638	1481638	1481638	1010775	1010775

* $p < .10$, ** $p < .05$, *** $p < .01$. 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. "Retail Probability" is analogous. "Service jobs" refers to eating and drinking places, landscaping, and automotive repair (see Table 4). 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", "Retail Probability", and state and year fixed effects.

Table A15: RTW-state heterogeneity in industry-level effects

DV: $\Delta \ln(Emp)_{i,s}$	(1)	(2)	(3)
Exposure_i	-0.357***		
	(0.094)		
RTW_s	-0.111	0.049	0.027
	(0.126)	(0.089)	(0.091)
$\text{Exp}_i \times \text{RTW}_s$	-0.266***	-0.159**	-0.188**
	(0.059)	(0.070)	(0.082)
$\text{RTW}_s \times \text{Homogeneous goods}_i$			0.290**
			(0.120)
$\text{Exp}_i \times \text{RTW}_s \times \text{Homogen}_i$			0.343**
			(0.130)
R^2	0.115	0.669	0.674
N	11062	11062	10516
Industry FE ($n = 293$)		Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is an industry-state (industries based on SIC 1987). Data drawn from CBP. Sample restricted to manufacturing industries. Panel is imbalanced; not all industries exist in all states. Two-way clustered standard errors (at the state and industry level) in parentheses. All regressions are changes from 1990 to 2014 and weighted by state-level total employment in 1990. Import exposure combines the NTR Gap and the ADH ΔChina-Other Trade, and has unit standard deviation across industries. Homogeneous goods classified by Rauch (1999). Adding the coefficient on $\text{Exp}_i \times \text{RTW}_s$ and the coefficient on $\text{Exp}_i \times \text{RTW}_s \times \text{Homogen}_i$ yields a sum that is positive (.154) and statistically significant ($p < .10$).

Table A16: 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. $\times$ 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.

Table A17: Right-to-Work vs. Baseline (1990) education

DV: Δ Manuf. emp./pop.	(1)	(2)	(3)
Import exposure	-0.968***	-0.113	-1.081**
	(0.147)	(0.615)	(0.520)
Right-to-work	2.391***	3.189***	2.212**
	(0.879)	(0.972)	(0.931)
RtW × exposure	-1.042***	-1.564***	-0.926*
	(0.372)	(0.445)	(0.466)
College share (normalized)		1.006	
		(2.096)	
College × exposure		-1.287	
		(0.967)	
High college (> median)			0.007
			(0.992)
High college × exposure			0.090
			(0.511)
R^2	0.553	0.577	0.555
N	51	51	51

* $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors clustered at the state level are in parentheses. All regressions weighted by 1990 state population. Baseline education based on 4-year college degree among population age 26-64 in 1989-1991. Column 2 includes college share in levels, but for interpretability it has been normalized to have minimum zero (actual minimum: 13% in West Virginia) and maximum one (actual maximum: 39% Washington DC). Column 3 follows Bloom et al. (2019) and divides states into above and below median college share. Figure A5 shows non-parametric results graphically.