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 modest decline in unionization within manufacturing industries. The magnitude is small because unionized manufacturers competed in higher-quality product segments. Manufacturers in Right-to-Work states experienced more direct competition with low-quality Chinese imports. Outside manufacturing, however, import competition causes an important increase in union membership as less-educated women shift away from retail and towards jobs in healthcare and education where unions are stronger. We calculate that Chinese imports prevented 26% of the union density decline that would have otherwise occurred.

Keywords: Unions, China, Imports

JEL Classification Numbers: F14, J50, J60

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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., 2021; Hirsch, 2008). Union density has fallen steadily ever since, reaching 10.3% in 2019 with private sector unionization falling even further (6.2%). Dramatic declines have occurred since 1980, coinciding with rising income inequality, stagnating median wages, and a declining labor share of income. It is now well-accepted that unions have important effects boosting wages and constraining inequality (Ahlquist, 2017; Farber et al., 2021). Recent work shows that American unions– even in their atrophied state–continue to shape important social outcomes, including racial attitudes (Frymer and Grumbach, 2021), voting behavior (Feigenbaum et al., 2019), and Congressional representation (Becher et al., 2018). Consequently, many observers have called for rebuilding the labor movement as a core part of any program for addressing economic disparities.

But how? The answer depends on why unionization has fallen in the first place. In one telling, employer resistance to unions began increasing in the 1970s and coincided with policy drift and weakened enforcement around labor regulation (Mishel, Rhinehart, and Windham, 2020; Stansbury and Summers, 2020; Galvin and Hacker, 2020). Reforming labor law and its implementation is a necessary condition for rebuilding worker voice. On the other hand, this same period also saw the modern era of globalization, with increased import competition affecting domestic manufacturing employment (Autor, Dorn, and Hanson, 2013; Pierce and Schott, 2016). With unions concentrated in manufacturing, observers immediately linked international competition with deunionization (Bluestone and Harrison, 1982). Some argue that more foreign competition in fact stimulated employers' increasingly anti-union stance (Scruggs and Lange, 2002; Kochan et al., 1984). If globalization is the key factor, then making changes to laws governing union recognition and strikes is unlikely to alter organized labor's prospects at this late stage. The policy stakes of disagreements about the cause of de-unionization are high.

We view the globalization hypothesis as widely held by a variety of academics, policymakers, and union activists. 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 particularly 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 continued blaming trade for ongoing deunionization (Gunn, 2018; Trumka, 2015). We exploit these recent trade shocks to explore whether 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 accelerate deunionization in the United States. We anticipated that this paper would use new evidence to better quantify the magnitude of the negative effect of import competition on US unionization rates. But our findings are more nuanced and highlight an implicit assumption in the standard story, namely that manufacturing is what matters when it comes to understanding unionization.

Manufacturing is no longer the heart of the labor movement in the United States.[1] By looking at unionization across the entire economy, we show that 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 geographic variation and changes in employment and unionization patterns across households over time.

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. Existing levels of imports (compared to a counterfactual of no Chinese import growth) can explain roughly a sixth of the average manufacturing industry's union decline over the period. We show that Chinese import competition has large effects on

[1] In 2019, just 9.6% of union members were employed in private sector manufacturing compared to 43% in 1973.

unionization in industries producing homogeneous, undifferentiated goods (e.g., unprocessed lead). However, the overwhelming majority of US manufacturing employment is in industries producing heterogeneous, differentiated goods, and in these industries the effects are small.

To interpret this result, we draw on evidence that unionized firms are more productive (Sojourner et al., 2015) and that more productive firms produce higher quality goods (Kugler and Verhoogen, 2011). This implies that unionized firms tend to compete in higher-quality market segments than non-unionized firms in industries producing heterogeneous goods—most US manufacturing. Because imports from low-wage countries like China tend to be low-quality (Hallak and Schott, 2011; Khandelwal, 2010; Schott, 2004, 2008), unionized firms engaged in less direct competition with imported Chinese products. In homogenous-goods industries, however, firms are in direct competition with imports and unionized firms face greater cost pressures, as we find empirically. In our view, this failure to consider market segmentation 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 import exposure to the state-level in order to estimate effects on state labor market outcomes.[2] 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 accounts for only around 15% of US employment, these outside-of-manufacturing spillover effects turn out to be larger than the within-manufacturing direct effects. To our surprise, our estimates imply that Chinese import competition actually *slowed* the decline in overall unionization in the United States.

We further document important policy-driven geographic heterogeneity in our state-level estimates. One might imagine that less-unionized "Right-to-Work" (RtW) states, by virtue of having lower average wages, might be relatively shielded from low-wage competition. To the contrary—and echoing our finding 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.[3,4] Moreover, in RtW states, a much larger share of the

[2] ADH also popularized commuting zones as a sub-state unit of analysis. We are interested in union membership, which requires using the Current Population Survey (instead of the Census, as ADH use), which requires using states since neither commuting zones nor MSA's can be reliably observed in the CPS.

[3] We rule out several potential mechanical explanations for this finding. In Appendix Table A16 we show that we only observe differential adverse RtW effects in heterogeneous-goods industries. In homogeneous-goods industries, non-RtW (pro-union) states are actually affected worse by exposure.

[4] 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 4.2 and Appendix Table A18, we provide evidence in favor of the RtW interpretation over the education interpretation.

manufacturing job loss was absorbed into non-employment.

What accounts for union-increasing changes outside manufacturing? Are individual workers who would have worked in unionized manufacturing jobs ending up in unionized jobs in other sectors? Or is adjustment happening at the household level? To gain purchase here, we describe changing household employment and unionization patterns between 1990 and 2014. We use rich demographic data from the 1990 CPS to train a machine learning algorithm to identify those most likely to have worked in manufacturing. We then apply this predictive model to the 2014 cohort, identifying respondents in 2014 who would likely work in manufacturing *if the economic conditions in 2014 were identical to those in 1990.* That is, our machine learning approach finds workers in 2014 who would work in manufacturing if selection-on-observables into manufacturing employment were the same in 2014 as in 1990. We find that, compared to predicted manufacturing workers in 1990, demographically similar "manufacturing-type" individuals in 2014 were much less likely to work in manufacturing. 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 2014 manufacturing-type individuals exhibited large changes in their employment patterns compared to the 1990 cohort. Specifically, we document reduced representation in retail jobs and increased employment in healthcare and education–sectors with relatively high and stable levels of unionization over this period.

To connect these descriptive changes to the effects of import exposure, we implement a modified triple-difference strategy. We find that 2014 "manufacturing-type" workers saw greater representation in service jobs and reduced average industry-level unionization rates when state-level import exposure was high, both relative to other workers in the state and relative to demographically-similar workers in less exposed states. Similarly, workers in the same household as a 2014 "manufacturing-type" worker 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 in 2014 compared to 1990.

Why didn't the manufacturing-type workers themselves make this transition? Why didn't their spouses do so even before the collapse of manufacturing? Why were these jobs available after the collapse? Were they intentionally pursuing unionized sectors, or simply high paying opportunities? In Section 6.4, we address these important questions of interpretation and offer what evidence we can, though we acknowledge that some of our conclusions are necessarily speculative, providing avenues for future research.

Our results contribute to two 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 and Clawson, 1999; Scruggs and Lange, 2002).[5] 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.[6] 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) and Fort, Pierce, and Schott (2018) also emphasize employment spillovers outside manufacturing. Specifically, both papers use establishment level data to show that trade exposure increases a firm's employment in its non-manufacturing establishments while decreasing employment in its manufacturing establishments. In other words, both papers focus on *within-firm* spillovers outside of manufacturing. We view our findings as complementary. The types of insights possible from the establishment-level data those authors use (such as firms' industry switching and or employment reallocation towards non-production work within the firm) are impossible to get at with individual-level data. Likewise, many of the important dynamics that we focus on (such as changing employment patterns within households) are impossible to observe with establishment-level data. Since most of our empirical results point to the importance of switching employment across very distinct sectors, they are unlikely to reflect the types of within-firm reallocation that Bloom et al. (2019) and Fort et al. (2018) describe.

[5]Stansbury and Summers (2020) argue that globalization is unlikely to explain US de-unionization partly because unionization also fell in non-tradeable sectors. Our paper provides a more rigorous analysis. It highlights that unionization in other sectors might itself be affected by imports, and we conclude something stronger than the Stansbury-Summers view: On the whole, Chinese imports actually *increased* unionization.

[6]Interestingly, the point estimates from Slaughter (2007) are similar to ours. Our estimates are more precise due to improved data availability and an improved research design that exploits an explicit source of exogenous industry-level variation in import exposure.

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 in Chinese import exposure 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 recording union membership—we are forced to coarsen both exposure measures into Census industries ($n = 64$).[7] Although we lose 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.[8] 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.[9,10]

2.2 Pooling ADH and PS

ADH and PS rely on different assumptions and sources of variation for identification, something we exploit here. 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.[11] We refer to this measure of import exposure as ΔChina-

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

[8] We find that import exposure leads to large increases in realized industry-level imports (the "first stage") and decreases in industry-level employment. Using the coarser industry codes, we find estimated employment effects larger than those from SIC-based analysis. We show this is because of spillover effects across closely related industries in the relatively narrow SIC categorization. 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]).

[9] 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).

[10] 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.

[11] 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.[12,13] 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.

[12]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.

[13]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.[14] 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 produces interpretable estimates (i.e., the extent to which actual US imports concentrate in local production industries), but these do not represent 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

[14]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.

Second, once we condition on 1990 density, our results are virtually unchanged when adding in the additional industry-level covariates mentioned above (captial intensity, skills, and textiles). The fact that the inclusion of these variables does not alter our main estimates suggests that conditioning on 1990 unionization is sufficient to summarize the characteristics of the unionizing environment producing the correlation between 1990 unionization levels and subsequent import exposure. With state-level results, our core findings are unaffected by including controls for baseline state-level union density or a large number of characteristics that have become common in the literature (Table A7).

2.5 Interpreting difference-in-difference results

Difference-in-difference estimates provide estimates of differential changes between two types of units (in our case, states or industries with different exposure to Chinese import competition). Thus, estimates reflect the relative change. To take a stance on the total effect, one must make some assumptions about what changes would have occurred in the absence of exposure. If industries or states with low measured exposure are still affected by rising imports from China —as growing evidence suggests that they are (Greenland et al., 2020)—then they do not serve as a useful counterfactual for a "no exposure" state of the world. The growing consensus among scholars of "the China shock" is that less-exposed firms, industries, and localities benefited from increased export opportunities, cheaper intermediate inputs, and related consequences of trade liberalization. Liberalization drove up employment in these firms, industries, and localities. As a result, comparing more exposed units to less exposed ones overestimates the size of employment effects.

In the context of deunionization, we believe these concerns are less central. Although our estimated effect of import exposure on employment is large, our estimated effect on within-manufacturing union density is small. If this estimated effect is biased upward, as many worry they might be, then the de-unionization effects of Chinese imports are even smaller, consistent with the broad message of the paper. Similarly, the core message of our state-level analysis is that unionization *increased* because of employment in non-tradeable sectors with minimal exposure to Chinese intermediate inputs (healthcare and education, in particular). Any general equilibrium effects are likely second-order for these state-level results. For our purposes in this paper, we are comfortable with the assumption that non-exposed units roughly approximate a "zero liberalization" counterfactual, and therefore interpret our estimates as the total effect of Chinese trade exposure, rather than simply the relative effects between the more and less-exposed units.

3 Effect of import exposure on industry-level unionization

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 20.3 log points or 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.[15]

[Table 1 about here.]

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 (weighted by baseline employment) 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

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

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.[16]

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 effects of Chinese imports were not larger.

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 presented some evidence that unionization increases productivity (Allen, 1984, 1986, 1987; Clark, 1980b,a, 1984) in addition to their effects on 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 because Chinese imports tend to be lower quality (Hallak and Schott, 2011; Schott, 2004, 2008), it is the low-quality domestic producers who face the most import competition from low-wage countries (Khandelwal, 2010). 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.

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 and which produce heterogeneous, branded products (e.g., shoes).[17] If it is true that unionized firms are shielded from import competition by producing higher quality products, then this should only hold in industries producing more heterogeneous goods. In homogeneous-goods industries

[16] Table A4 shows none of the estimates are significantly different between the strategies. We find this encouraging given the low correlation between the two sources of identification (.27). Table A5 shows that more and less exposed industries had identical trends in union membership in the 1984-1990 period.

[17] Homogenous goods are products in which quality is standardized and goods from different producers are near-perfect substitutes, e.g., unprocessed lead. Rauch (1999) considers a good "homogenous" if the product has an internationally listed reference price quoted without reference to which company produced it. Note that this is an intrinsic feature of the industry and does not depend on the choices of firms over time. While there is evidence that Chinese import exposure drives firms to produce differentiated products (Holmes and Stevens, 2014), Rauch's measure will not be affected by such behavior because it is based on whether it is possible to produce differentiated products.

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.[18] 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.

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 minimum (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 in manufacturing. If, on the other hand, all industries produced only homogeneous goods, eliminating imports would have prevented 6.3pp of the decline (half of the observed average decline).

4 State-level effects of exposure

4.1 Average effects

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. 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, mutually exhaustive 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%). Because these population shares sum to 100 and we are looking at changes, the coefficients must

[18]Industries produce multiple goods so the industry-level classification is non-binary. Across industries, the 1990-employment-weighted average of "homogeneous goods" is 22%. In 1990, 77% of US manufacturing employment was in industries where homogeneous goods account for less than 1/3 of output. Roughly 15% of 1990 US manufacturing employment was in industries where homogeneous goods made up the majority of output. Our empirical results are similar if we use a binary variable for industries that mostly produce homogeneous goods.

mechanically sum to 0. In other words, we are estimated exposure-induced reallocations of population across these groups.

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).

Our core result is that exposure increases the share of the population working in unionized jobs outside manufacturing. We consider this surprising, so we subject the finding to extensive robustness checks that are detailed in the appendix. We show that this result is the same when using each identification strategy 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. We also show that there is no "placebo effect" of exposure on changes in non-manufacturing union employment prior to 1990.

4.2 Geographic heterogeneity: 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 Chinese import exposure might vary geographically. We focus on the presence of "Right-to-Work" legislation which is widely considered an important state-level law with negative consequences for unionization as well as a bellwether for the state's political stance towards organized labor.

In Table 3, we interact import exposure with states' RtW status. The results show important differences by RtW status. Column 1 shows that non-RtW states saw 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. Most noteworthy, 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 3 about here.]

This is not an artifact of linear regression; RtW and non-RtW states saw similar average levels of exposure, and Appendix Figure A6 shows non-parametrically that 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 A16). This difference in employment effects is 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 (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 return to our earlier hypothesis that – only in heterogeneous goods industries – unionized firms compete in a less exposed market segment (with higher quality output) than non-unionized firms. In this case, we should expect that the larger effects of import exposure on RtW states' *total* manufacturing employment should be confined to heterogeneous goods industries (where product differentiation is possible). In Appendix Table A16 we test this using a CBP-based panel of employment by state and industry.[19] Consistent with this hypothesis, import exposure decreases RtW states' employment (relative to non-RtW states) in heterogeneous goods industries only. In the small number of homogeneous goods industries, RtW states actually experienced significantly *smaller* effects of exposure ($p < .10$).

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 3 shows this did not happen; the additional manufacturing job loss in RtW states flowed almost entirely

[19]Union status is not available in the CBP.

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 that we document below. In Table A17 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 wage premia in healthcare and education among these women, but only in non-RtW states. In RtW states (where unions are rare), we find no wage premia for these sectors. Absent any wage signals, workers in RtW states 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 A18), we include both variables interacted with exposure. We find that the interaction with RtW is statistically significant while the interaction with education is not (though it is of a similar magnitude).

5 Interpreting within- and outside-manufacturing effects

In Section 3, we exploited variation in exposure to Chinese imports across manufacturing industries to show that more exposed manufacturing industries saw significant declines in unionization. But this did not speak to effects outside manufacturing. In Section 4, we show that state-level exposure to Chinese imports increased unionization outside manufacturing. However, it is not straightforward to compare the magnitudes of these positive and negative effects, and so in this section, we provide a decomposition that links these and quantifies the relative importance of within-manufacturing versus outside-manufacturing effects.

Our decomposition follows Berman, Bound, and Griliches (1994) (see the appendix for the derivation). Specifically, we can write the change in union density for the entire economy as:

$$\Delta u = \bar{m}\Big[\underbrace{\sum_i \bar{s}_i \Delta u_i}_{\text{Within-industry}} + \underbrace{\sum_i \Delta s_i \bar{u}_i}_{\text{Between-industry}} \Big] + \underbrace{\Delta m(\bar{u}_m - \bar{u}_{-m})}_{\text{Between-sector}} + \underbrace{(1-\bar{m})\Delta u_{-m}}_{\text{Non-manufacturing}} \tag{1}$$

where s_i denotes industry i's share of manufacturing employment, m denotes manufacturing's share of total employment, u_i denotes union density in industry i, u_m and u_{-m} denote total unionization within and outside of manufacturing (respectively), Δ denotes the change between 1990-2014, and $\bar{x}$ denotes the average level of a variable $x \in \{u, m, s\}$, averaged across the two periods.

The first term (labeled "within-industry") captures the change in aggregate unionization that is driven solely by declining membership within the various different manufacturing industries (Δu_i), holding fixed the relative size of the different industries at their average level ($\bar{s}_i$) and the overall size of the manufacturing sector at its average level ($\bar{m}$). If we took as given the within-industry declines observed in the data, but there were no changes in the sizes employment shares for different industries (or for manufacturing as a whole), then this component yields the aggregate decline in unionization that would have occurred. The second term (labeled "between-industry") captures the change driven by changes in the relative size of different manufacturing industries (Δs_i), while assuming that unionization did not change within any of those industries (i.e., held fixed at $\bar{u}_i$, its average level over time) and that the overall size of manufacturing did not change from $\bar{m}$. Together, these first two terms would sum to the change (from 1990-2014) in the probability that a random worker taken from manufacturing is unionized.

The third term (labeled "between-sector") captures the change in aggregate unionization that would occur solely as a result of changing manufacturing's share of total employment (holding fixed union density within manufacturing at $\bar{u}_m$ and outside of manufacturing at $\bar{u}_{-m}$). The decline in manufacturing's share of total employment has been large, and if manufacturing is much more unionized that non-manufacturing employment, then this decline will drive substantial de-unionization. However, in 1990-2014, unionization was only modestly higher within manufacturing than outside of it,[20] and so $\bar{u}_m - \bar{u}_{-m}$ is small and the mechanical compositional effect of "deindustrialization" (a large negative Δm) on unionization during our period is relatively small.

The final term (labeled "non-manufacturing") captures the aggregate change in unionization driven by the change in union density outside of manufacturing, holding fixed the overall size of manufacturing. It is worth noting that manufacturing is only about 15% of employment during our sample (falling from 18% in 1990 to 11% in 2014). Thus, with roughly five non-manufacturing workers in the economy for every manufacturing worker, any modest changes *outside* of manufacturing (Δu_{-m}) can easily outweigh changes within manufacturing.

Table 4 presents the results. The first two columns are the decomposition preformed on the raw data. Union density within manufacturing as a whole declined by 12.3 percentage points from 1990 to 2014, and this was driven by within-industry declines averaging 13.2

[20] In 1990: 21.4% vs. 13.2%. By 2014, it had flipped to be slightly *lower* within manufacturing than outside of it: 9.1% vs. 10.3%.

percentage points (holding fixed the size of different industries), which is only slightly offset by a small reallocation of manufacturing labor towards more unionized industries (the between-industry component that holds the unionization rate of each industry constant), which increased within-manufacturing unionization by 0.9pp. In other words, most industries saw declining unionization, and the aggregate effect was slightly offset by low unionization industries shrinking more than high unionization ones.

However, the non-manufacturing sector is so much larger than manufacturing, that, even the smaller decline (2.9pp from 13.2% to 10.3%, not presented in the table) means more for the aggregate economy. Declines outside of manufacturing make up 2.5 percentage points of the aggregate 4.5 percentage point decline that we observe in the data. This can be compared to the 1.9pp decline driven by the observed within-industry de-unionization (holding fixed both the employment shares of different industries and the employment share of manufacturing as a whole), despite the fact that this within-industry de-unionization was large.

[Table 4 about here.]

Columns 3 and 4 use our estimates from above to 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 regressions results to calculate what change in employment or within-industry unionization would have occurred.[21] Columns 3 and 4 report our results. Within-manufacturing union density would have declined by 10.9 percentage points. In other words, we estimate that 83% of the decline in union density within manufacturing would have occurred even without import competition. Again, this results from the relatively small effects on union density that we obtain from our industry-level exposure regressions.[22] 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).

[21]Between-industry and between-sector 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 state-level results in columns 2 and 3 of Table 2. Specifically, as long as *i*) outside-of-manufacturing spillovers are concentrated within the same state and *ii*) state-level population changes are uncorrelated with exposure, then our state-level results (which are based on regressions weighted by 1990 population) can be used to calculate the predicted change in population shares in unionized and non-unionized jobs outside of manufacturing in the case where all industries see minimal exposure (as opposed to the actual, observed, average level of exposure).

[22]Similarly, comparing the observed between-industry component (0.9pp in columnn 1) with the counterfactual between-industry component (0.7pp in column 3) implies that most of the differential decline in employment in low-unionization industries was not related to differential exposure to Chinese import competition.

6 Spillovers

Our results show that effects of import competition on unionization *outside of manufacturing* are the most important revisions to the standard story. How should we interpret this? Are people who would otherwise work in manufacturing finding themselves in relatively unionized jobs outside manufacturing, e.g., construction (Charles, Hurst, and Notowidigdo, 2019)? Or is it more likely that declining manufacturing induces *other* household members to take jobs in disproportionately unionized sectors?

6.1 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 apply the trained algorithm to the 2014 CPS sample to identify the respondents who most "look like" manufacturing workers from 1990.[23] The purpose of this exercise is not to identify 2014 respondents who actually worked in manufacturing, either in 1990 or 2014. 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 to characterize changes in the labor market experience of well-defined demographic groups for whom manufacturing employment was historically important.

Appendix Table A12 illustrates some of the characteristics that help identify manufacturing-type workers, comparing them to the full sample. The table also summarizes the traits of these manufacturing-types' household members (spouses, children, etc.), people indirectly affected by lost manufacturing employment opportunities. Importantly, we seek to describe how the labor market experiences of manufacturing-type respondents and members of their households have changed between 1990 and 2014, i.e., *across cohorts*. This exercise does *not* offer direct evidence on how the households of *actually displaced* workers adjusted to their new circumstances.

[23] For this initial descriptive exercise, we consider a worker to be "manufacturing type" if their predicted probability of working in manufacturing is in their cohort's top decile.

Table 5 presents a detailed breakdown of the industries seeing the largest change in population shares between 1990 and 2014 among manufacturing-type respondents. Specifically, we present the change in population share for manufacturing (the industry seeing the largest decline), as well as the six industries seeing the largest growth in population share. To characterize these industries, we also present their 1990 wages and union densities, both calculated using the full sample (i.e., not only the manufacturing-type respondents).

Among this demographic group, the share employed in manufacturing fell by nearly 16 percentage points (from 35.4% to 19.1%) between 1990 and 2014. The next rows show that just six "industries" (including non-employment) can account for 14 percentage points of the 16 lost in manufacturing. Most concerning, the "industry" seeing the most growth is non-employment. The share of this demographic group that is without work rose by 6.4 percentage points (from 12.5% to 18.9%). This large non-employment growth is consistent with results from ADH. Consistent with Charles et al. (2019), the next largest increase appears in construction, absorbing roughly one fifth of would-be manufacturing workers.

Construction has similar wages and unionization levels to manufacturing. On this basis we might interpret the flow into construction as leaving these workers' well-being largely unchanged. However, most of the remaining employment shifts are towards low-wage, non-unionized industries like restaurants, landscaping, and automotive repair (though high-wage computer processing services also saw meaningful growth). This implies reduced income prospects for a large proportion of manufacturing-type workers, even those who do end up finding employment. It also suggests that a cross-sector reallocation of manufacturing-type workers is unlikely to explain the growth in unionized employment outside of manufacturing: with the exception of construction, the industries seeing the most growth rarely have unions, and non-employed workers are obviously not unionized.

[Table 5 about here.]

Panel B considers a logical alternative explanation: spouses and children of these would-be manufacturing workers are the ones accounting for the import exposure-induced increase in non-manufacturing unionization. The table presents the 11 "industries" (including non-employment) seeing the largest growth in population share among this sample. As with the manufacturing-type workers themselves, growth in non-employment is substantial, rising by 0.8 percentage points from 39% to 39.9%. While this is the third highest population growth among these "industries," it is substantially smaller than the 6.4 percentage point change that we see among manufacturing-type workers. Looking at the remaining 10 industries (i.e., actual employment), it is remarkable that three are in education, three are in healthcare, and two are in social services.[24] Only two of the top 10 industries fall outside of these three sectors (miscellaneous entertainment/recreation and government offices).

At the bottom of the table, we show that these three sectors each saw substantial growth in population shares between 1990 and 2014 (2.5 percentage points for education, 2.2pp for healthcare, and 1.2pp for social services). This growth makes up a total of 5.9 percentage points. The growth in education and healthcare is noteworthy because both industries exhibit relatively high wages. In 1990, median wages within education were \$18.32 (in 2015 dollars), actually slightly higher than in manufacturing (\$18.14). Moreover, 34% of workers were unionized, far higher than either the general population or manufacturing workers. In healthcare, median wages were only slightly lower (\$16.55) and although the union density was lower than the national average, unionization within healthcare has remained stable compared to the economy as a whole.[25]

Of course, how to interpret increasing healthcare and education employment among this population depends on where these workers are coming from. In Figure A3, we present simple scatterplots showing the relationship between the change in the industry's share of "manufacturing-type respondents' household members" employed and the industry's 1990 wage and union density. The plots are clearly upward sloping: the industries that attracted these workers had higher 1990 wages and union density and the industries seeing declines had low wages and union density. The figure also isolates healthcare, education, and retail industries. Consistent with Table 5, healthcare and education largely account for the relative growth among high-paying highly-unionized industries. To our surprise, the figure also shows that nearly all of the decline in employment shares in low-paying, low-union density industries came from retail. At the bottom of Table 5, we present the change population share for retail as a whole (36 industries). The share of this population working in retail fell by 4.2 percentage points between 1990 and 2014 (from 16.4% to 12.2%), roughly equal to the 4.7pp growth that we saw in education and healthcare. This is important partly because baseline wages and union density were much lower in retail.

6.2 Household spillovers and state-level import exposure

We presented three descriptive facts to account for why non-manufacturing union density rose in responses to Chinese import exposure. First, people in 2014 who "look like" 1990 manufacturing workers have today found themselves in low-wage, low-unionization service

[24]We follow the BLS classification to define social services. We note, however, that nearly all industries in the "social services" sector are a form of education (like child care) or healthcare (like residential care facilities without nurses).

[25]In 1990, 11.6% of healthcare workers were unionized, compared to 16% of all workers. But by 2014, healthcare unionization fell by only 2.6 percentage points, compared to 4.5pp among all workers.

industries when compared to the 1990 cohort. Second, the spouses (and children) of these 2014 "manufacturing-type" workers are more likely to be employed in relatively unionized education and healthcare industries compared to 1990. Third, this shift came from declining employment in retail. Here, we ask whether these trends were stronger in states with higher import exposure.

6.2.1 Method

We continue with the machine learning approach described above. Using the algorithm trained on 1990 data, we generate a predicted probability of working in manufacturing for each CPS respondent j (who could be in either cohort, $t \in \{1990, 2014\}$).[26] We denote this quantity as $\hat{p}_j$. For the ease of interpretation below, we define $\text{ManufProb}_j = (\hat{p}_j - \min(\hat{p}))/(\max(\hat{p}) - \min(\hat{p}))$, i.e., we normalized the predicted probabilities to have minimum zero and maximum one within the full sample.

We then identify respondents who live in the household of a manufacturing-type worker (hereafter, "household members" by defining $\text{Max-HH-ManufProb}_j$ as the maximum value of ManufProb_k among all $k \neq j$ observed in the CPS as members of j's household.[27]

With these quantities we estimate the following modified triple-difference regression to simultaneously study manufacturing-type workers and their household members:

$$\begin{aligned} Y_{jst} = \alpha_s + \delta_t &+ \beta_1 \big(\text{Exposure}_s \times \mathbb{1}\{t = 2014\}\big) \\ &+ \beta_2 \big(\text{Exposure}_s \times \mathbb{1}\{t = 2014\} \times \text{ManufProb}_j\big) \\ &+ \beta_3 \big(\text{Exposure}_s \times \mathbb{1}\{t = 2014\} \times \text{Max-HH-ManufProb}_j\big) \\ &+ \beta_4 \text{ManufProb}_j + \beta_5 \text{Max-HH-ManufProb}_j + \varepsilon_{jst} \end{aligned} \tag{2}$$

We investigate the following outcomes (Y_{jst}): employment; employment in service sector jobs;[28] employment in health or education industries; employment in retail; and the 1990 union density and median wage for the industry in which j is employed, conditional on employment. We focus on these industry-level measures to systematically assess whether workers are being pushed towards industries with higher or lower baseline wages and unionization, overall, while our earlier outcomes (service sector, healthcare or education, and retail) only isolate some specific industries.

We include state (s) and period (t) fixed effects to isolate the effect of exposure on

[26] State of residence was included in the feature set for the algorithm.

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

[28] Specifically, we focus on the three low-wage service sector jobs identified as being important in our descriptive analysis in Table 5: eating and drinking places, landscaping, and automotive repair.

later-cohort outcomes, after adjusting for time-invariant cross-state differences and aggregate nationwide changes over time. We condition on ManufProb_j and $\text{Max-HH-ManufProb}_j$ to adjust for time-invariant ways in which manufacturing-type workers and their household members might differ from the larger population. Recalling that ManufProb is normalized, β_1 can be interpreted as the effect of state-level import exposure for the sample individual predicted to be *least* likely to work in manufacturing. The coefficient β_2 represents the differential effect of exposure for the individual *most* likely to work in manufacturing while β_3 represents the effect of exposure on someone who lives with the most-likely manufacturing worker.

6.2.2 Results

We begin by estimating the differential effects of exposure on the employment outcomes of manufacturing-type workers and their household members. Table 6 presents the results.

In column 1, we estimate that import exposure has no statistically significant effect on the employment of individuals who are neither likely to work in manufacturing themselves, nor have a household member who is ($\hat{\beta}_1 = 0.6$, $p = .271$). However, for "manufacturing-type" respondents (those with demographics similar to 1990 manufacturing workers) compared to those non-manufacturing-type workers, a one standard deviation increase in exposure differentially decreases the probability of employment by 2 percentage points ($p < .01$). For "household members" (i.e., individuals who live with one of these manufacturing-type workers), we also find that exposure leads to a differential 0.3pp decrease in the probability of employment ($p < .05$). These results are similar to those from our descriptive analysis presented in Table 5, which found increased non-employment among both populations, but dramatically larger for manufacturing-type workers than their household members. Table 6 shows that this pattern was accentuated in highly exposed states.

In column 2, we focus on the three low-wage, low-unionization service industries identified in Table 5: eating and drinking establishments, landscaping, and automotive repair. We estimate that import exposure modestly but significantly reduces the probability that non-manufacturing workers are employed in these industries ($\hat{\beta}_1 = -0.6$; $p < .01$), but that it increases the probability that manufacturing-type workers are by roughly 0.8pp ($\hat{\beta}_1 + \hat{\beta}_3 = 0.8$, $p < .01$). In column 3 we focus on health and education industries. The main effect shows that exposure increases health/education employment among workers who are neither likely to work in manufacturing themselves nor live with someone who does, but this effect is significantly stronger among those who live with a manufacturing-type individual ($p < .01$). Likewise, column 4 estimates a decrease in retail employment that is significantly stronger for household members. Overall, these are the exact industry shifts that we found descriptively above: In response to increased import competition, would-be manufacturing workers in 2014 are were more likely to end up in specific low-wage service jobs while their household members shifted out of retail and into healthcare and education compared to the 1990 cohort.

[Table 6 about here.]

Columns 5 and 6 of Table 6 focus on our main interest: unionization. For both columns, we consider only employed individuals and identify their industry of employment. We calculate the 1990 unionization and 1990 median wage for that industry and regress these values on our triple-difference specification. The results summarize how manufacturing-type workers and their household members differentially sorted across industries in response to state-level import exposure. Rather than focusing on a few specific industries and sectors, these two columns summarize changes in the characteristics of the industries where the *average* import-exposed worker ends up.

Column 5 shows that exposure shifts non-manufacturing non-household members towards industries with higher 1990 unionization. The main effect shows that a 1 standard deviation increase in exposure raises the 1990 union density rate for the average worker by nearly 1 percentage point. For the average manufacturing-type worker, however, exposure pushes *down* the 1990 union density rate of their industry of employment by 0.4 percentage points ($p < .01$). For household members, however, the differential increase in 1990 union density of their industries of employment is *even larger* than for the full population ($p < .01$). In column 6, we observe nearly identical patterns when characterizing industries by their 1990 wage: exposure shifts the non-manufacturing-type worker towards higher wage industries, but the effects are significantly stronger for household members and reversed for the 2014 would-be manufacturing workers themselves.

6.3 Limitations

The results in Table 6 show that across-cohort shifts in household members' employment and unionization patterns in high exposure states mirrored the aggregate trends we showed in Section 6.1. However, all of these results implicitly condition on household formation because we focused on respondents who were actually observed to live in a household with a manufacturing-type worker. A concern is that marriage and household formation is itself endogenous to import exposure (Autor et al., 2019).

To partially address this concern, we repeat the exercise in section 6.2.2, using our same machine learning algorithm to identify *retail-type* workers.[29] Examining predicted retail-type

[29] We use 1990 observables to train the algorithm on the 1990 CPS and then use this fitted model to predict retail employment in the 2014 cohort.

respondents lets us focus on the core sector that household members shift out of (Table 5) without explicitly conditioning on household formation. Results for the retail-type analysis appear in Appendix Table A14. As with table 6, the results largely mirror our descriptive analysis, with retail-type workers are significantly less likely to be employed or employed in retail in 2014 in high exposure states; exposure differentially increased their likelihood of working in healthcare or education; and 2014 retail-type workers in more exposed states are more likely to work in more unionized and higher-wage industries.

The fact that we find such similar results using two complementary strategies increases our confidence. Both strategies have meaningful limitations. Our main results (focusing on household members in Table 6) do not condition on the endogenous demographics of household members (only those of the manufacturing-type workers that they live with), but do condition on endogenous household formation. The alternative results looking at retail-type workers (Table A14) do not condition on household formation, but do condition on demographics like education which have been shown to be affected by exposure import competition (Atkin, 2016; Feler and Senses, 2017; Greenland and Lopresti, 2016). Fundamentally, the challenge is that the broad nature of the effects driven by Chinese import competition leaves few observable characteristics truly unaffected. This is a meaningful limitation in moving from descriptive analysis above towards causal estimates.

6.4 Questions about household spillovers

We were surprised to estimate that import competition had large effects increasing unionized employment outside of manufacturing, and more surprised to discover how robust these effects were. A natural first question was whether this seemed to be driven by the workers who themselves *would have* worked in manufacturing had it not been so drastically shrunken by globalization. Our descriptive analysis showed that this was unlikely and that there was much more evidence for employment differences of their spouses. Our triple-difference analysis confirmed that these responses were sharper in highly-exposed states, and that they particularly represented the acceleration of a long-run shift out of retail and into healthcare and education. We found these results surprising, and acknowledge that they beg *more* questions of interpretation. Here, we address some of those questions somewhat speculatively, pointing the way for more research.

An obvious question: why did manufacturing-type workers themselves not end up in the higher-paying, more unionized sectors? Why was it their spouses? We suspect that gender norms are a large part of the explanation. Industries and occupations in the United States remain gender segregated (Pan, 2015). Over 97% of the "manufacturing-type" workers that we identify are males. Unsurprisingly, then, we see that would-be manufacturing workers tended to end up in male-dominated industries. Of the five industries seeing the biggest growth (see Table 5), three were more than 90% male in 1991 (construction, landscaping, and automotive repair), and only one was less than half male (eating and drinking places; 45%). Similarly, all five of the healthcare and education industries seeing the most growth in household members' employment were more than 70% female in 1991, and only one gender-balanced industry saw substantial growth in household members' employment over the period (entertainment and recreation; 43% female at baseline).

Thus, individuals' employment allocation in response to the decline of manufacturing largely coincided with predetermined gender norms. Beyond construction, male-dominated industries offer few high-paying substitutes to manufacturing employment for less-educated men. On the other hand, traditionally female sectors like healthcare and education do provide higher paying opportunities. In particular, healthcare—which was 81% female in 1991—has long been seen as an engine for economic mobility (Pindus, Flynn, and Nightingale, 1995), with numerous high return Associates' Degrees available through community colleges, for instance (Grosz, 2020).

If these higher-paying opportunities existed in 1990, why didn't women take advantage of them at the time? Why was it the decline of manufacturing that (in part) caused this shift? Again, we suspect the answer has to do with gender norms. Bertrand, Kamenica, and Pan (2015), for example, show evidence of a widespread preference in the United States for the man to be the primary earner, particularly among less-educated respondents of both genders. Thus, when it is *possible* for the man to earn relatively high manufacturing wages, it is plausible that many families are willing to sacrifice marginally higher paying opportunities for the spouse in accordance with traditional gender roles. However, the decline of manufacturing changed incentives. Assuming that changes to economic behavior outpace changes to norms, identity, and expectations, this sort of transformation can also affect household formation in the way that Autor et al. (2019) document.

The previous discussion presupposes that these relatively attractive jobs were available in 1990. Were these jobs in healthcare and education even available at the time? Although our discussion so far has focused on labor *supply* responses, exposure to import competition can also affect labor *demand*. Autor, Dorn, and Hanson (2016) find that for every $1,000 in lost per capita annual income caused by import competition, there is $100 in increased government transfers per capita and one-third of this (the largest share) is government-provided medical benefits.[30] In education, Greenland and Lopresti (2016) show that import

[30]Related, Pierce and Schott (2018) and Lang et al. (2019) find import exposure increases mortality and reduces self-reported health quality, respectively. When inferring expected employment responses among

competition increases high school graduation rates and Feler and Senses (2017) show that it increases college attendance. While Feler and Senses (2017) find that local government spending on education decreased, other types of spending decreased by substantially more so that the *share* of local government spending on education actually rose in response to import competition.

In an effort to understand supply and demand effects, we use the CPS to regress changes in state-level log employment and log wages for each of 21 sectors on state-level exposure.[31] We present the results in Appendix Figure A4, where sector-specific coefficients have been ordered by 1991 unionization rate. In Panel (a), we find that employment in most sectors (with the exception of manufacturing) does not appear to be affected by import exposure. Only two sectors show a significant employment *increase*: education and social services, where we find 4% and 3% increases, respectively. We find no effect on healthcare employment. In Panel (b), we find that nearly all low-unionization sectors saw decreased wages, and that this includes a statistically significant 1.5% decline in healthcare and 3.5% decline for social services. In highly-unionized education, on the other hand, we estimate no change in wages (the confidence intervals rule out a decline of more than 1.5%).

Looking at education, healthcare, and social services as a whole, then, we conclude that import competition drove an increase in demand for education which was able to offset any labor supply response (increasing total employment without affecting the wage), but that in healthcare and social services, any demand increase was too small to offset the supply response, which has led to falling wages in those sectors.

Were workers pursuing unionized jobs? Manufacturing was a traditional a stronghold of organized labor. The American labor movement vocally opposed the types of trade liberalization that led to the Chinese import surge that we study. It is possible that, in areas where the China shock was most acutely felt, workers responding to the decline of manufacturing saw particular value in the labor movement. Thus, it is possible that they were intentionally targeting unionized jobs because of a preference for labor unions. We provide two simple analyses that suggest otherwise.

First, we calculate the change in employment at the industry level for household members of manufacturing-type workers. We ask whether 1990 wages or unionization better explains the growth we see in certain industries by regressing the changes on 1990 median wage and union density. Table A13 shows that an industry with a median wage one standard

publicly traded firms, Greenland et al. (2020) find that healthcare companies of all sizes expected to see employment *gains*. This suggests that PNTR status may have had large labor demand effects in the industry.

[31] That is, we estimate our standard first-difference specification for state-level exposure separately for each of the 21 sectors. This approach places strong demands on the data, with the potential for few respondents in some sector-state-year cells (21% have 100 or fewer). We therefore view these findings as suggestive only.

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, however, 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 higher wages, rather than unionization, is a better predictor of which industry attracted these workers.

Second, we return to our industry-state-year panel and estimate whether exposure affected unionization rates *within sector*. Figure A5 presents the sector-specific estimates, again ordered by 1990 unionization rates. Only in manufacturing and mining do we find significant effects on unionization. For each of healthcare, education, and social services, point estimates are near zero and not statistically significant. Thus, we see no evidence that the marginal workers pushed into these sectors carried some strong demand for labor unions that one would have expected to push up industry unionization rates.

Finally, what about actually-displaced manufacturing workers? Our discussion of spillovers focused on across-cohort changes, comparing *would-be* manufacturing workers and their household members in 2014 against similar groups from an earlier cohort. But within-household adjustment among actually displaced manufacturing workers represents a complementary adjustment channel. The within-household channel represents adjustments happening within the working life of displaced manufacturing workers as opposed to across cohorts separated by 25 years.

To investigate whether there is any evidence for this within-household channel, we use the Displaced Worker Supplement (DWS) to the CPS (asked during January or February of even-numbered years). The DWS asks respondents whether they have been displaced from a job in the last three years and, if so, various characteristics about the job.[32] We focus on prime age married women with a spouse present and examine their employment, depending on their husband's employment history. Specifically, we define "Manufacturing husbands" as those who currently work or have been displaced from manufacturing. We define "Displaced husbands" as those who have been displaced from a job in the last three years according to the BLS definition. In regressions of employment on household characteristics, our coefficient of interest is the *interaction* of these two, which characterizes the employment status of wives whose husbands were displaced from manufacturing jobs, both relative to those whose husbands work in manufacturing but haven't been displaced, and relative to those whose husband have been displaced but never worked in manufacturing.

[32]We focus on the survey from 1994-2014 which *i*) overlaps with our sample years and *ii*) uses a consistent reference period of three years.

Table A15 presents the results. We find that women with husbands recently displaced from manufacturing are not more likely to be employed, but *are* significantly more likely to work in healthcare or education.[33] Interestingly, when we further interact this quantity with whether the displaced job was unionized, we find that this effect is larger among those whose husbands lost a *union* job. Thus, we do find some direct evidence that the loss of a manufacturing job (particularly unionized ones) fuels spouses' transitions into healthcare and education. In other words, we find evidence for an "added worker effect" (Lundberg, 1985) in the type of employment, and rather than the extensive margin of employment.

We note that this evidence on the within-household channel is suggestive and not yet directly linked to Chinese import exposure. It represents a different conceptual exercise from our investigation of the cross-cohort adjustment over a 25 year period as well as an intriguing research opportunity.

7 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. Although modest, this decline may still represent a significant blow to union bargaining power in manufacturing, given the fragmented, establishment-centered nature of unionization in the United States.

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 provide evidence that import exposure drove less-educated men (and specifically the demographic profiles most reliant on manufacturing employment in 1990) towards low-wage non-unionized industries (in addition to construction) and accelerated their spouses' transition out of retail and towards jobs in healthcare and education.

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

[33] Inconsistent with our story, they are also more likely to work in retail.

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). Our results suggest that declining manufacturing may unite these patterns: the collapse of manufacturing 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 a sense, then, our results point towards a cross-cohort analog to the "added worker effect" originally identified by Lundberg (1985). This long-run transformation is particularly important because of growing evidence that labor demand shocks tend to be very long-lived (Amior and Manning, 2018; Dix-Carneiro and Kovak, 2017).

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, the fact that unionized firms were relatively shielded from competition from low-quality Chinese imports, and the changing structure of the US labor movement all combined to imply that "the China shock" actually slowed the 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 × 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. "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. Columns 5 and 6 condition on the covariates considered in Table A3 (capital intensity, skill share, textiles). 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: 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).

Table 4: 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 manufacturing industry	0.9	0.1	0.7	0.1
Within manufacturing industry	-13.2	-1.9	-10.9	-1.6
Between sector (manuf. vs. non-manuf.)		-0.3		-0.1
Non-manufacturing		-2.5		-4.5
Total	-12.3	-4.5	-10.2	-6.1

For the decomposition and definition of terms, see equation 1. Columns 1 and 3 are based on changing union density only within manufacturing: $\Delta u_m = \sum_i \bar{s}_i \Delta u_i + \sum_i \Delta s_i \bar{u}_i$. Columns 2 and 4 are based on changing union density in the economy as a whole: $\Delta u = \bar{m}\big(\sum_i \bar{s}_i \Delta u_i + \sum_i \Delta s_i \bar{u}_i\big) + \Delta m(\bar{u}_m - \bar{u}_{-m}) + (1-\bar{m})\Delta u_{-m}$. For the counterfactuals, we base our between-industry and between-sector effects on Table 1 (industry-level effects on overall employment), our within-industry effects on Table 1 (industry-level effects on union density), and our outside of manufacturing effects on Table 2 (state-level effects on non-manufacturing union and non-manufacturing non-union employment). Counterfactual change based on setting each industry's exposure is equal to the sample minimum across industries.

Table 5: 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				
11 industries seeing most growth					
Health services, NEC	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, NEC	0.5	1.0	0.5	16.03	15.1
Entertainment/recreation services, NEC	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, NEC	0.1	0.3	0.3	18.17	6.4
Colleges & universities	1.6	1.8	0.2	17.40	12.3
Selected sector aggregates					
Education (total)	9.4%	11.9%	2.5pp	$18.32	34.3%
Healthcare (total)	7.1	9.3	2.2	16.55	11.6
Social services (total)	1.6	2.8	1.2	12.14	8.9
Retail (total)	16.4	12.2	-4.2	9.73	6.4

Panels A and B are both based on 1989-1991 and 2013-2015 CPS samples. Both panels present the industries seeing the largest growth in population share among the sample. That is, neither panel intentionally selects which industries to present. Sample in Panel A is "manufacturing-type respondents": Those with estimated probabilities of working in manufacturing above the cohort-specific 90^{th} percentile. Sample in Panel B is manufacturing-type respondents' "household members": Those with estimated probabilities of working in manufacturing below the cohort-specific 50^{th} percentile, but for whom someone in the household has an estimated probability above the cohort-specific 90^{th} percentile. Estimated probabilities of working in manufacturing are based on demographics and the 1990 probability model. Industries are based on 3-digit 1990 CPS industry codes (n=235). Wages are in 2015 dollars. Median wages and union shares (1990) both refer to the full population. "NEC" denotes "not elsewhere classified." Sector aggregates include all industries in the sector, not only the specific industries appearing separately in the table.

Table 6: 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***	-0.63***	0.94***	0.23***
	(0.509)	(0.134)	(0.120)	(0.189)	(0.092)	(0.070)
Exp. × '14 × Man. Prob.	-2.04***	1.35***	-1.06***	1.79***	-1.38***	-0.49***
	(0.547)	(0.097)	(0.089)	(0.133)	(0.097)	(0.027)
Exp. × '14 × Max HH Man. Prob.	-0.33**	0.01	0.23***	-0.31***	0.13***	0.04***
	(0.128)	(0.030)	(0.056)	(0.047)	(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
DV mean (1990)	69.4	4.3	11.9	11.6	16.3	16.7
p for H_0: $\beta_1 + \beta_2 = 0$	0.000	0.000	0.020	0.000	0.003	0.001
p for H_0: $\beta_1 + \beta_3 = 0$	0.652	0.000	0.000	0.000	0.000	0.000

* $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors clustered at the state level are in parentheses. All regressions based on ORG respondents in 1989-1991 and 2013-2015 and use sample weights. "Manufacturing Probability" is an individual's estimated probability of working in manufacturing based on demographics, state-of-residence, and the probability model estimated on the 1990 sample. "Max HH Man Prob." is the maximum "Manufacturing Probability" of other individuals in the respondent's household. "Service jobs" refers to eating and drinking places, landscaping, and automotive repair (see Table 5). 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). Note that these can be calculated for all employed individuals based on the industry in which they are employed. Doing so provides a summary statistic for the characteristics of the industry in which the average worker (of a particular type) is employed. All regressions control for individual-level "Manufacturing Probability", "Max HH", and state and year fixed effects.

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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.[34] 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.[35]

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).

[34] 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.

[35] 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.[36] The upper panel (A) uses the change in China-Other trade as the measure of import penetration.[37] 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).[38] 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

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

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

[38] 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.[39] 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

[39] 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.[40]

[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.[41]

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.]

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

[41] 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

A.4.1 Derivation

For the manufacturing decomposition, note that we can write the change in union density within manufacturing as

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

or equivalently as:

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

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

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

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

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

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

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

which is the decomposition appearing in the paper.

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

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.6 Interpreting household adjustment

[Figure A3 about here.]

[Figure A4 about here.]

[Table A13 about here.]

[Table A14 about here.]

[Figure A5 about here.]

[Table A15 about here.]

A.7 Right-to-Work results

[Figure A6 about here.]

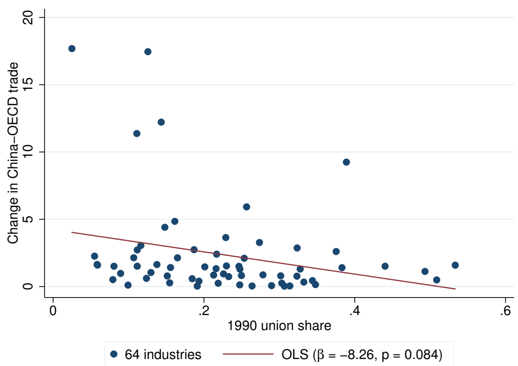
[Table A16 about here.]

[Table A17 about here.]

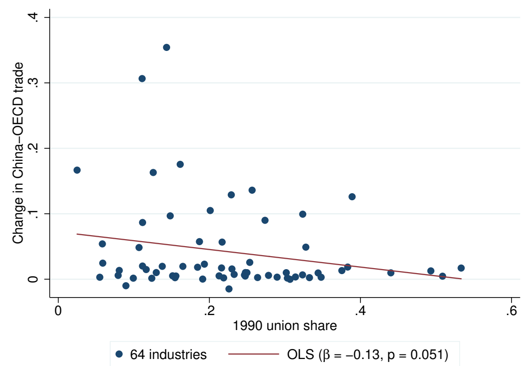
[Table A18 about here.]

[Figure A7 about here.]

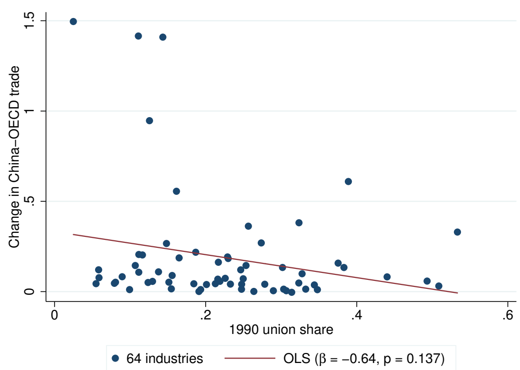
Figure A1: Autor-Dorn-Hanson instrument and lagged unionization



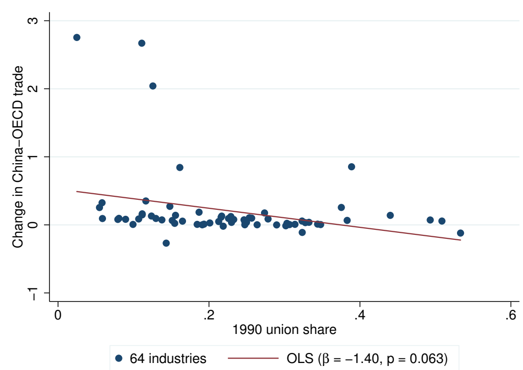
(a) Change 1990-2014



(b) Change 1990-2000



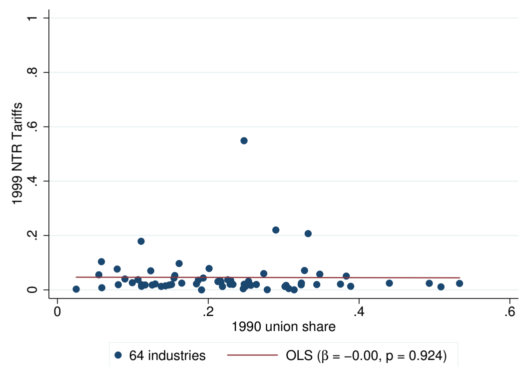
(c) Change 2000-2007



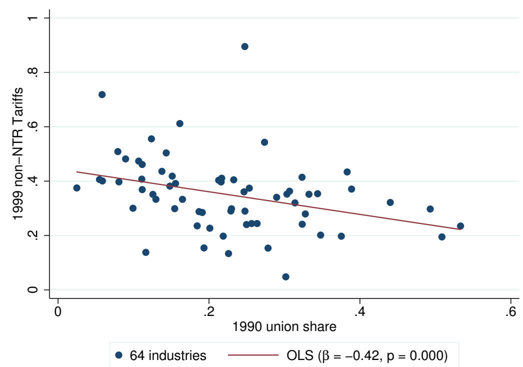
(d) Change 2007-2014

Each dot is an industry (for Census defined industries). Figure shows the relationship between baseline union density (1990) and subsequent changes in exports from China to eight "comparable" OECD countries (Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland) used by ADH in the construction of their instrument. All eight had established permanent NTR with China prior to 1990.

Figure A2: Pierce-Schott instrument and lagged unionization



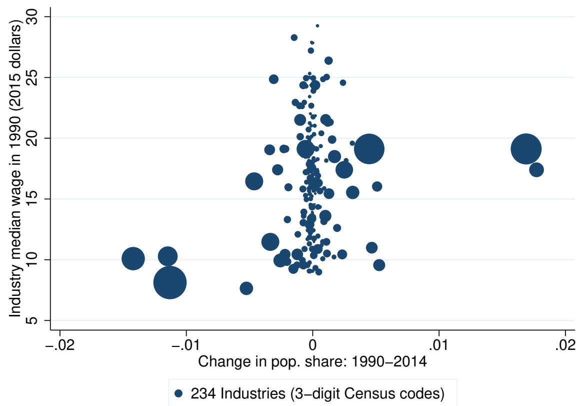
(a) NTR tariffs



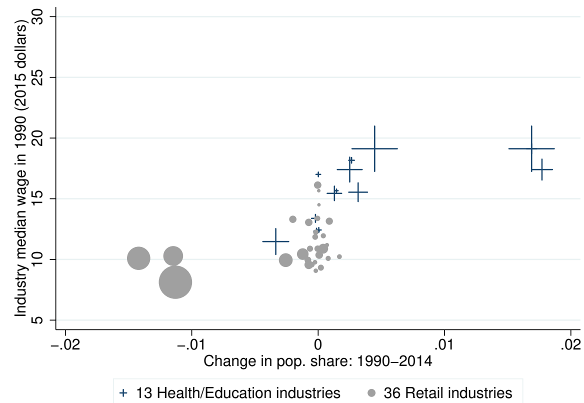
(b) non-NTR tariffs

Each dot is an industry (for Census defined industries). Figure shows the relationship between baseline union density (1990) and tariffs measured by Pierce-Schott. We note that Pierce-Schott showed a correlation between their NTR gap measure and 1999 unionization in their paper (see Table A.2).

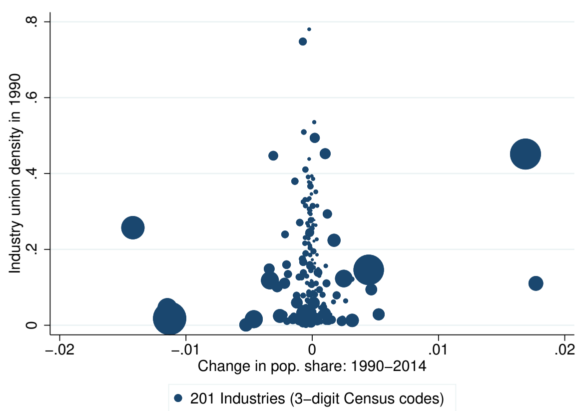
Figure A3: Characteristics of industries seeing largest changes in household members' employment



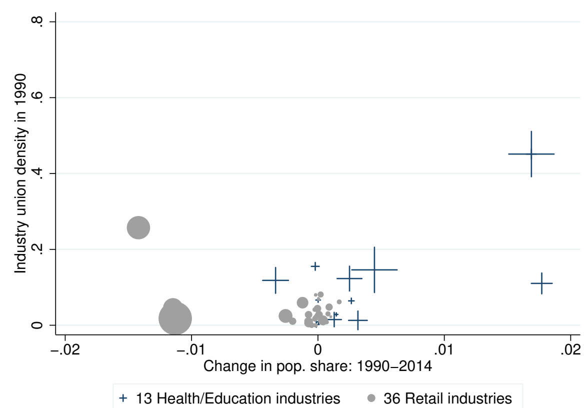
(a) Median wages (all industries)



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



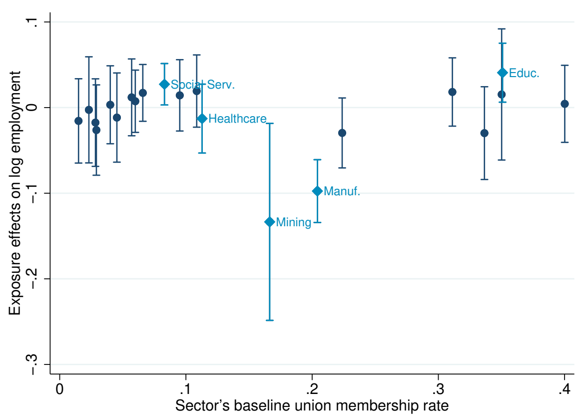
(c) Union density (all industries)



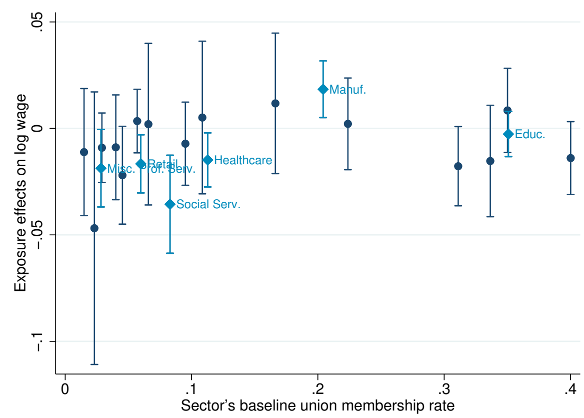
(d) Union density (retail, ed., health)

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. The three points furthest to the left (i.e., showing the largest decline) are department stores, grocery stores, and eating and drinking places.

Figure A4: Sector specific effects of import competition



(a) Log employment



(b) Log wage

Figure presents coefficient estimates (and 90% confidence intervals) for effects of import exposure (at the state level) on log employment and log wage for 21 sectors ordered by baseline (1991) union membership rate.

Figure A5: Effects of import competition on within-sector unionization rates

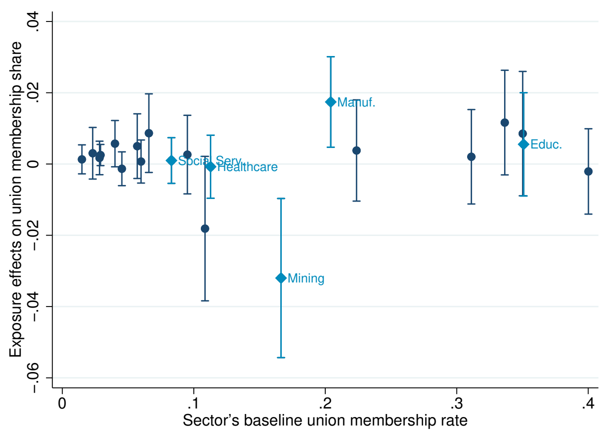
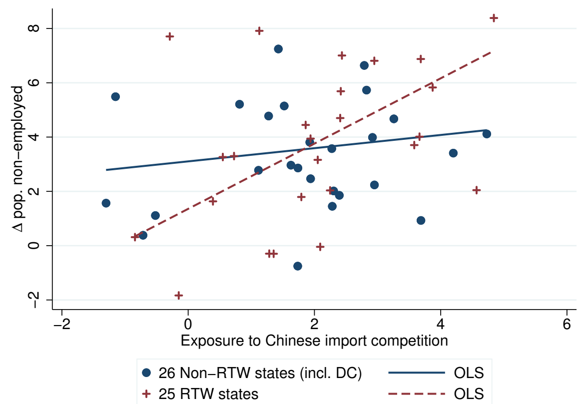
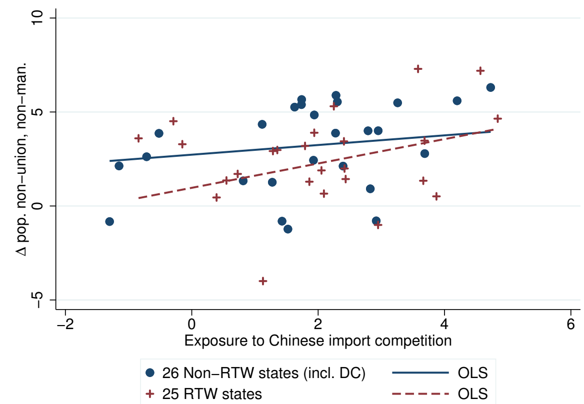


Figure presents coefficient estimates (and 90% confidence intervals) for effects of import exposure (at the state level) on union density for 21 sectors ordered by baseline (1991) union density.

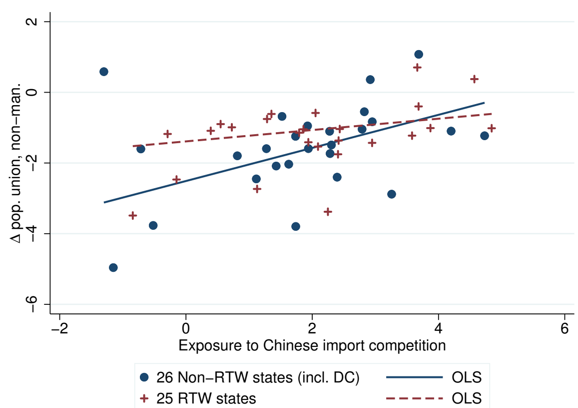
Figure A6: Non-parametric heterogeneity by RtW status



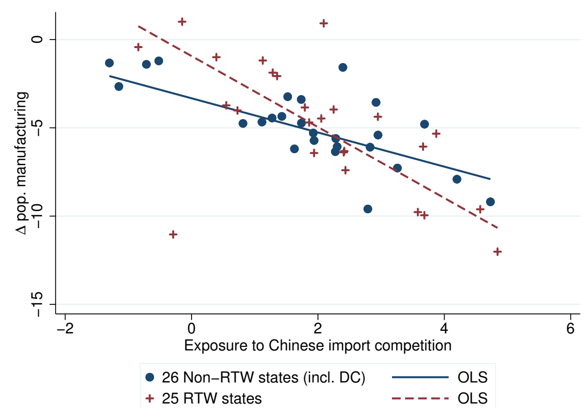
(a) Non-employed



(b) Non-union, non-man.



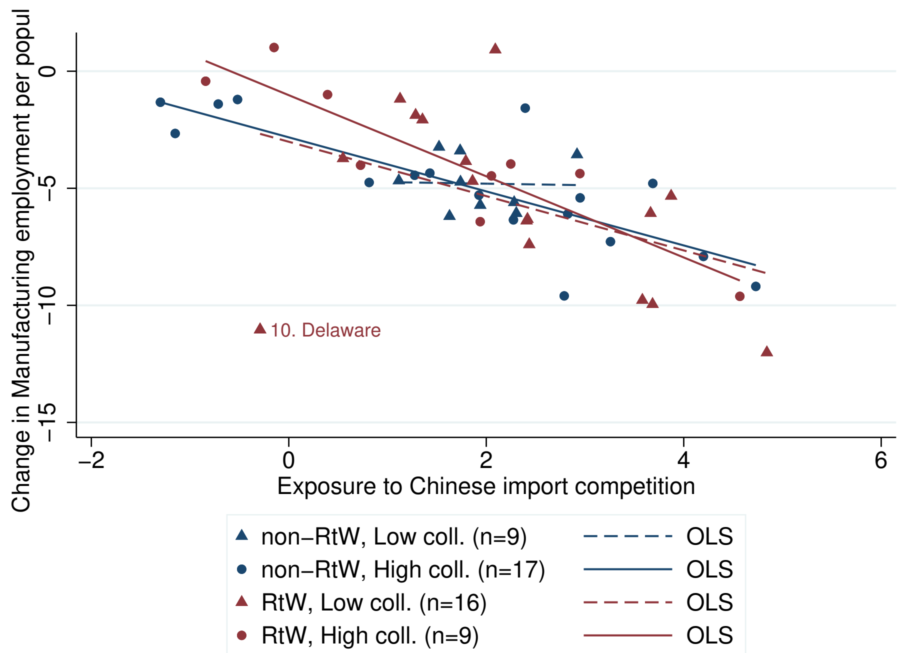
(c) Union, non-man.



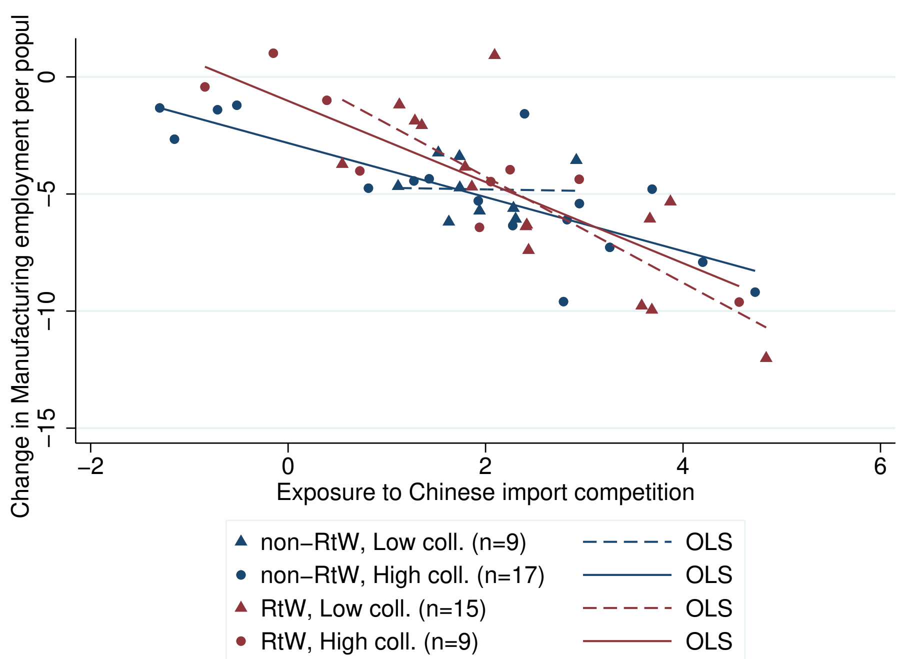
(d) Manufacturing

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 3. Outlier in Panel (d) is Delaware.

Figure A7: 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, separately depending on RtW status (our focus) and average education levels (the focus of Bloom et al. (2019)). Formal regressions included in Table A18.

Table A1: Summary statistics

Variable	Mean	SD	N	Percentiles 10	25	50	75	90
Δ China-US Trade (SIC)	0.16	0.67	1121	0.00	0.00	0.03	0.12	0.36
1990-2000	0.10	0.36	364	0.00	0.00	0.01	0.05	0.17
2000-2007	0.23	0.65	376	0.00	0.01	0.06	0.19	0.43
2007-2014	0.15	0.87	381	-0.03	0.00	0.03	0.14	0.36
Δ China-US Trade (Cen.)	0.17	0.50	199	0.00	0.01	0.04	0.12	0.34
1990-2000	0.08	0.20	68	0.00	0.00	0.01	0.06	0.26
2000-2007	0.22	0.45	65	0.00	0.03	0.07	0.17	0.50
2007-2014	0.22	0.72	66	-0.00	0.01	0.05	0.12	0.41
Δ China-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)	(5)	(6)
DV:	Δ ln(Employment)			Change in		
	Total	Union mem.	Non-mem.	Union member share		
	Panel A: Autor-Dorn-Hanson identification strategy					
Δ China-Other Trade	-0.184***	-0.370***	-0.174***	-0.007	-0.006**	-0.006*
	(0.050)	(0.093)	(0.049)	(0.004)	(0.003)	(0.003)
Exposure $\times$ Homogen. goods						-0.031
						(0.050)
R^2	0.158	0.272	0.261	0.843	0.864	0.874
N	64	64	64	64	64	62
Controls:						
Union mem. (1990)	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	Yes	Yes
	Panel B: Pierce-Schott identification strategy					
NTR Gap	-0.106	-0.291**	-0.100	-0.015***	-0.012*	-0.005
	(0.090)	(0.129)	(0.093)	(0.005)	(0.006)	(0.005)
Exposure $\times$ Homogen. goods						-0.017
						(0.013)
R^2	0.096	0.189	0.214	0.863	0.870	0.878
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 for $H_0 : \beta_{ADH} = \beta_{PS}$	.403	.558	.440	.107	.438	.859

* $p < .10$, ** $p < .05$, *** $p < .01$. Robust standard errors in parentheses. All regressions are changes from 1990 to 2014, weighted by 1990 industry employment; and condition on 1990 union share. Both the Pierce-Schott NTR Gap and the ADH ΔChina-Other Trade have unit standard deviation across industries. Results can be compared to Table 1 which pools both identification strategies. Columns 5 and 6 condition on the covariates considered in Table A3 (capital intensity, skill share, textiles).

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

	(1)	(2)	(3)	(4)
DV: Δ share working age pop.	Non-emp.	Non-manuf., non-union	Non-manuf., union	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 and retail-type workers

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Emp. ($\times$100)	Service jobs ($\times$100)	Health or Educ. ($\times$100)	Retail ($\times$100)	Industry union density	Industry median wages
Exposure$_s$ $\times$ 1{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)
Exp$_s$ $\times$ '14 $\times$ $\hat{P}_j$(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)
Exp$_s$ $\times$ '14 $\times$ $\hat{P}_j$(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 5). 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). Note that these can be calculated for all employed individuals based on the industry in which they are employed. Doing so provides a summary statistic for the characteristics of the industry in which the average worker (of a particular type) is employed. All regressions control for individual-level "Manufacturing Probability", "Retail Probability", and state and year fixed effects.

Table A15: Employment of prime age married women depending on spouse's employment status and history

	(1)	(2)	(3)	(4)	(5)	(6)
DV:	Any emp.	Emp. in retail	Emp. in health or educ.	Any emp.	Emp. in retail	Emp. in health or educ.
Husband: Manufacturing	0.011***	-0.016***	-0.017***	0.011***	-0.016***	-0.017***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Husband: Displaced	0.012**	0.008*	-0.017***	0.012**	0.008*	-0.017***
	(0.006)	(0.004)	(0.005)	(0.006)	(0.004)	(0.005)
H: Manuf. × H: Disp.	0.005	0.013*	0.018**	0.001	0.012*	0.012
	(0.010)	(0.007)	(0.009)	(0.011)	(0.007)	(0.010)
H: Manuf. × H: Disp. × Displaced job was unionized				0.034	0.007	0.046**
				(0.024)	(0.018)	(0.022)
R^2	0.053	0.013	0.046	0.053	0.013	0.046
N	201218	201218	201218	201218	201218	201218

* $p < .10$, ** $p < .05$, *** $p < .01$. Sample is married, female, prime age (26-55) CPS Displaced Worker Supplement Respondents (even numbered years from 1994-2014). Dependent variables refer to respondent's own employment. "Husband: Manuf." is a dummy indicating that respondent's spouse currently works in manufacturing or was displaced from a manufacturing job (following the post-1998 BLS definition of a Displaced Worker). "Husband: Displaced" is a dummy indicating that respondent's spouse was displaced from any job. The Displaced Worker Survey collects information about involuntary job loss over the past three years only. "Displaced job was unionized" is a dummy indicating that respondent's spouse was a member of a union at the displaced job. Columns 4-6 also control for the main effect of the displaced job having been unionized (i.e., the level effect without the interaction with manufacturing).

Table A16: 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)
Exp$_i$ $\times$ RTW$_s$	-0.266***	-0.159**	-0.188**
	(0.059)	(0.070)	(0.082)
RTW$_s$ $\times$ Homogeneous goods$_i$			0.290**
			(0.120)
Exp$_i$ $\times$ RTW$_s$ $\times$ 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 Exp$_i \times$ RTW$_s$ and the coefficient on Exp$_i \times$ RTW$_s \times$ Homogen$_i$ yields a sum that is positive (.154) and statistically significant ($p < .10$).

Table A17: Wage differentials in Healthcare/Education

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

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

Table A18: 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 A7 shows non-parametric results graphically.