NBER WORKING PAPER SERIES

TRADE COMPETITION AND THE DECLINE IN UNION ORGANIZING: EVIDENCE FROM CERTIFICATION ELECTIONS

Kerwin Kofi Charles
Matthew S. Johnson
Nagisa Tadjfar

Working Paper 29464
http://www.nber.org/papers/w29464

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2021, Revised October 2023

Do Lee, Anna Mather, Mitchell Ochse, and Jeremy Tang provided excellent research assistance. We thank Andrew Greenland for helpful comments and Alex Mas for sharing data on union elections. We received constructive comments from conference participants at the SOLE Annual Conference and the LERA Annual conference, as well as seminar participants at the Federal Reserve Board of Governors. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Trade Competition and the Decline in Union Organizing: Evidence from Certification Elections
Kerwin Kofi Charles, Matthew S. Johnson, and Nagisa Tadjfar
NBER Working Paper No. 29464
November 2021, Revised October 2023
JEL No. F16,J41,J50,J51,J52

ABSTRACT

The long-term decline in U.S. workers' attempts to organize labor unions accelerated after 2000. We find that the swift rise of imports from China arising from a change in trade policy accounts for nearly all of this post-2000 acceleration: union certification elections decreased substantially among workers in manufacturing industries directly exposed to imports, but also among workers indirectly exposed through their local labor market. Consistent with a simple model of workers' decision to seek union representation, direct exposure lowered the expected wage gain from unionization, whereas indirect exposure increased the cost of job loss - both of which discourage organizing.

Kerwin Kofi Charles
School of Management
Yale University
165 Whitney Avenue
New Haven, CT. 06511
and NBER
kerwin.charles@yale.edu

Matthew S. Johnson
Sanford School of Public Policy
Duke University
Box #90312
Durham, NC 27708
and NBER
matthew.johnson@duke.edu

Nagisa Tadjfar
Massachusetts Institute of Technology
ntadjfar@mit.edu

1 Introduction

Workers in the U.S. seeking to form a labor union at their job must first conduct and win a union certification election, overseen by the National Labor Relations Board (NLRB).[1] Over the past several decades there has been a dramatic decline in workers' attempts to unionize. Figure 1a shows that from just over 6,800 union certification elections in 1964, by 2008 the number had fallen to just over 1,600. Whereas the unionization *rate* (the stock of unionized workers) is largely a function of broader structural shifts in the economy (Dickens and Leonard, 1985), the occurrence of a union election is arguably a direct measure of workers' "revealed preference" for unions (Kochan et al., 2019). Unions enjoy consistently favorable esteem, both in the public at large (Jones, 2019) and among workers (Kochan et al., 2019), and a massive literature suggests that unions increase their members' wages (Farber et al., 2018; Freeman and Medoff, 1984). Why, then, are ever-fewer workers trying to unionize? What forces in the economy have driven changes in workers' expected benefits and costs from union organizing? Virtually no work in the voluminous economics literature on labor unions directly empirically analyzes workers' organizing attempts and the success of those efforts.[2] This paper attempts to partially fill this gap.

We study how, and by what mechanisms, import competition from China has affected union organization efforts. Figure 1b shows that there was an acceleration in the secular decline in union elections around the year 2000 and that this coincided with the acceleration of Chinese imports into the U.S. Furthermore, Chinese imports were concentrated in manufacturing industries (Acemoglu et al., 2016), a traditional bastion of unionization. Together, these patterns hint at a causal connection. On the other hand, union elections have declined nearly as quickly in non-manufacturing as in manufacturing (Figure 1c), leading some to

[1] If 30 percent of workers in a proposed bargaining unit express support for union representation, the NLRB conducts a secret ballot election; if a majority of workers vote favorably, the NLRB certifies the union as the sole representative of the bargaining unit. In rare cases, some employers voluntarily choose to recognize a union without an NLRB certified election.

[2] One notable exception is Farber and Western (2002), who attribute the dramatic decline in organizing in the early 1980s evidenced in Figure 1a to a newly unfavorable political climate following the 1981 Professional Air Traffic Controllers Organization strike. We discuss other such exceptions later in the introduction.

doubt that Chinese imports appreciably affected unionization efforts (Mishel et al., 2020). Convincing evidence for a causal effect of the "China Shock" on union elections must thus account not only for falling organization attempts in manufacturing but also among workers in sectors not directly exposed to import competition — something our paper does.

Our analysis distinguishes between two channels whereby the China Shock might affect workers' organizing decision. The *direct* influence on a worker comes from the adverse effect of Chinese trade competition on the profitability or economic strength of the worker's firm. Chinese import competition's *indirect* influence operates through its adverse effect on the profitability of import-exposed firms in the worker's labor market, and thus on the strength of the worker's outside option.

We empirically assess the direct and indirect effects of Chinese import competition on union organizing using detailed information on union certification election We proxy for the strength of the direct influence on workers in the manufacturing sector by the extent of their industry's exposure to the China Shock. We measure the size of the indirect influence on workers (in all sectors) by the degree of trade exposure of all firms in their local labor market, which (following many prior studies) we define as Commuting Zones. We geocode and fuzzy link union elections to the local labor market and industry of the affected establishment.

Given that rates of union organizing had been declining since well before the 1990s (as shown in Figure 1a), it is particularly important that we disentangle the causal effect of the China Shock from ongoing trends that preceded it. To do so, our primary approach follows Pierce and Schott (2016) to measure industries' exposure to the China Shock based on differential exposure to a change in U.S. trade policy in 2000 in which the U.S. granted Permanent Normal Trade Relations (PNTR) to China. The granting of PNTR eliminated potential tariff increases on Chinese imports, and industries more exposed to this change experienced a surge in imports from China (Pierce and Schott, 2016). We assess the robustness of this approach using a separate identification strategy, pioneered by Autor et al. (2013), that instruments for industry-level Chinese import penetration to the U.S. with the import levels of the same industries in other advanced countries.

We find that both *direct* and *indirect* exposure to the China Shock lowered union organizing efforts by an economically meaningful amount. Industries more exposed to the granting of PNTR experienced significantly fewer union elections during 2001–2007. Moving an industry from the 25th to 75th percentile of the observed distribution of PNTR exposure led to 11–21 percent fewer elections being held. The estimate is robust to controlling for potential confounding factors, and event study estimates corroborate a causal interpretation. Using information on the voting outcomes in elections that occurred, we find a similarly sized negative effect on successful union elections in which the union won.[3]

Examining next the indirect channel, we find that CZs more exposed to PNTR, via their initial industry mix, also experienced substantially fewer union elections. Furthermore, this effect is present for workers in sectors not themselves directly exposed to import competition, confirming that our estimate captures the effect of indirect exposure and that our CZ-level measure is not merely repurposing the measures of direct exposure. Moving a CZ from the 25th to 75th percentile of PNTR exposure led to 15–25 percent fewer elections held.

These magnitudes are economically meaningful. In the manufacturing sector, our estimates imply that the granting of PNTR accounted for at least 40 percent of the "trend break" acceleration in the decline in union elections in 2000 in this sector. Across all sectors, the granting of PNTR accounts for essentially *all* of the 2000 trend-break in organizing for the aggregate economy. We estimate that direct industry exposure to PNTR caused 32,000–57,000 fewer workers to become unionized in the manufacturing sector over the period 2001–2007, and that indirect local labor market exposure caused 272,000–483,000 fewer newly-unionized workers across all sectors.

What explains these estimated effects? We present a simple framework of a worker's decision whether to organize for a union that formalizes insights first developed in Ashenfelter

[3]This finding implies that import exposure did not affect unions' likelihood of success conditional on an election occurring, which is consistent with the model in (Farber, 2001) of how changes to the organizing environment affect how unions target elections.

and Pencavel (1969). Our framework highlights two distinct forces that affect this decision. The first of these forces operates through the expected benefits that workers anticipate from organizing. A worker has a higher desire to attempt to unionize the larger the gap between the wage she expects to earn as a unionized worker and her current non-unionized wage. The China Shock likely reduced this "union wage premium in industries directly exposed to it. Direct exposure to import competition lowered firms' profitability (Autor et al., 2020), thereby reducing the size of rents that a union could bargain over; it also plausibly increased the elasticity of exposed firms' labor demand (by way of Marshall's law of derived labor demand), which would make unions reluctant to raise wages above the market wage. We hypothesize that this force accounts for the negative estimated *direct* effect of industry exposure on certification elections.

The second force our framework highlights stems from the increased risk of job loss that a worker might face because of union organizing.[4] Commuting Zones with many firms in import-exposed industries experienced lower employment rates (Pierce and Schott, 2020) and a reduction in market wages (Autor et al., 2013). Workers in such markets would thus become more hesitant to organize given their worsened prospects were they to lose their jobs. Importantly, this consideration affects *both* workers whose firms are directly affected by China *and* workers whose firms are unaffected. We hypothesize that this worsening of workers' outside option accounts for the large negative estimated *indirect* effect of local labor market exposure on certification elections.[5]

To assess the empirical support for our framework's two hypothesized mechanisms, we use newly quantified data on the negotiated wages in every collective bargaining agreement by private-sector unions since 1990. Consistent with our hypothesis, direct exposure to the

[4]There are two main reasons why attempting to organize a union might increase job loss risk. One, as predicted by standard theory, is that raising wages above competitive levels (presumably a primary aim of a union's activities), reduces the quantity of labor demanded. Secondly, there is substantial evidence that employers often retaliate against workers who engage in union organizing activity (Abowd and Farber, 1990; McNicholas et al., 2019; Stansbury, 2021).

[5]Indirect exposure via one's local labor market could also affect union-negotiated wages. As we discuss theoretically and show empirically, this force is more muted than the effects on the cost of job loss.

China Shock shrinks the wage gains that unions are able to secure in negotiations, thereby making new organizing less appealing to workers. Moving a manufacturing industry from the 25th to 75th percentile of exposure to PNTR led the wage increase negotiated in an average union contract to be 18 percent lower than it otherwise would be.

We next provide supporting evidence that a higher cost of job loss at least partially explains why CZs exposed to the China Shock experienced lower rates of union organizing. First, consistent with Pierce and Schott (2020) and Autor et al. (2013), we show that CZs exposed to imports experienced increases in the unemployment rate and reductions to the market wage, suggesting a widened gap between a worker's current wage and her outside option if she loses her job. Second, local labor market exposure had a much more muted effect on certification elections in states that had explicit protections for workers against employer retaliation for actions like union organizing. Taken together, these results imply that workers living in markets heavily affected by trade competition became reluctant to organize for unions because doing so became riskier—via the higher cost of job loss.

Our findings are not only of independent interest but also extend previous work. Although labor economists have had a long-standing interest in the determinants of union organizing and union membership, there have been few empirical papers on the subject. Ashenfelter and Pencavel (1969) and Sheflin et al. (1981) used time series data to link aggregate trends in union membership to variables like unemployment. Farber and Saks (1980) found that the likelihood that a worker voted positively on a union certification election was inversely related to her relative position in the wage distribution. We are aware of only a few other papers that empirically analyze the determinants of union organizing. Abowd and Farber (1990) find that the frequency of union elections at the industry level is positively associated with the availability of quasi-rents per worker, but they note the "puzzle" that union organizing exhibits a downward trend even conditioning on available quasi-rents. Our paper sheds light on this puzzle by considering the role of changes in a worker's *outside options*, as well as the expected wage gain from becoming unionized at the current job, in driving the decision to organize. Farber (2001) develops a model of how unions target elections and fits it to aggregate time series. A third such paper is Schaller (2023), discussed below.

Trade, including the "China shock," has long been proposed as a cause of the decline of unionization in the U.S. (Slaughter, 2007; Farber and Western, 2001; Hirsch, 2008). However, much of this previous work has only speculated about this relationship; furthermore, it has a) primarily focused on the effects of trade on unionization *within manufacturing* (Slaughter, 2007), whereas our paper examines its (direct and indirect) effect on all sectors, and b) almost exclusively sought to determine how trade affects the overall unionization *rate*, whereas our paper assesses how trade affects new union *organizing* via certification elections.

Two papers are particularly related to ours. Schaller (2023) finds that manufacturing industries facing more import competition since the 1960s experienced a relative decline in union elections over the same period. We complement this paper by a) considering the indirect effect of Chinese imports on elections *outside* of manufacturing (which we show is a substantial effect), b) using multiple identification strategies, and c) investigating the mechanisms underlying the effects of trade exposure on union organizing. Ahlquist and Downey (2022) investigate the effects of Chinese imports on the *stock* of unionized workers (the unionization rate). They find that manufacturing industries more exposed to the China Shock experienced small relative declines in the unionization rate. However, they find that states more exposed to Chinese imports experienced *higher* unionization *rates* outside manufacturing, whereas we find that Commuting Zones more exposed to Chinese imports experienced *lower* union *organizing*. Ahlquist and Downey (2022) interpret their finding as local labor market import exposure causing structural shifts of women shifting from the low-union retail sector to the high-union healthcare and education sectors, rather than from causing more workers seeking to unionize. We complement their analysis by a) using more detailed industry codes (357 4-digit SIC codes vs. 64 Census Industry codes) and a finer level of geography (722 Commuting Zones vs. 50 states) and b) considering union *organizing*, which is tightly linked to the theoretical concern of worker's demand for unionization.

2 Data and Measures

To examine the direct and indirect effects of Chinese import competition on labor union organizing, we need measures of industries' and local labor markets' exposure to Chinese import competition and data on union organizing election efforts. We discuss the data sources we use for each of these purposes, in turn.

2.1 Measuring Exposure to Chinese Import Competition

China's emergence as a great power in international trade began in the early 1990s following a series of reforms, and it accelerated further when China joined the World Trade Organization in 2000 (Autor et al., 2013). Prior studies have identified two distinct reasons that some U.S. industries were more exposed to the so-called "China Shock" than others. First, industries were differentially affected by a change to U.S. trade policy in 2000 that eliminated potential tariff increases on Chinese imports (Pierce and Schott, 2016). Second, because China's exports were concentrated in certain industries, there was substantial variation in industries' "supply-side" exposure to Chinese import competition (Autor et al., 2013). Furthermore, due to regional variation in historical industrial composition, local labor markets varied in the degree to which they were exposed to Chinese imports. Our primary empirical approach measures industries' and local labor markets' import exposure based on the change in trade policy pioneered by Pierce and Schott (2016) and Pierce and Schott (2020). In robustness checks, we instead measure exposure based on the approach from Autor et al. (2013). We only briefly describe these data and methods since detailed descriptions may be found in published papers. Reassuringly, our findings are essentially unaffected by which of these measures of exposure we use.

2.1.1 Industry Exposure

Historically, the U.S. applied relatively low "Normal Trade Relations (NTR)" tariff rates on imports from fellow member countries of the World Trade Organization (WTO) and relatively higher non-NTR tariff rates on imports from nonmarket economies (such as China). Starting in 1980, the U.S. started granting NTR rates to Chinese imports, but this approval needed to be renewed by Congress on an annual basis, and such renewals were often politically contentious and uncertain (Pierce and Schott, 2016). In October 2000, Congress granted Permanent Normal Trade Relations (PNTR) to China, which eliminated the risk that tariffs on Chinese goods would revert back to the higher levels. Thus, PNTR dramatically reduced both the risk to U.S. companies considering trade and investment in China and the expected U.S. import tariffs on Chinese goods.

Historical differences in both NTR and non-NTR tariff rates across industries led to substantial variation across industries in the "NTR gap," defined for industry j as:

$$\text{NTR}_{gap,j} = \text{non-NTR rate}_j - \text{NTR rate}_j. \tag{1}$$

Industries with larger NTR gaps would have faced greater disincentive for trade with China pre-PNTR and thus a relatively larger increase in Chinese imports following PNTR (which Pierce and Schott 2016 show indeed occurred). We follow Pierce and Schott (2016) and define industries as "families" that link four-digit Standard Industrial Classification (SIC) to six-digit North American Industry Classification System (NAICS) codes.

In Appendix C1, we describe how we create our alternative measure of industries' exposure to Chinese imports using the strategy pioneered by Autor et al. (2013).

2.1.2 Local Labor Market Exposure

We follow many prior studies and define local labor markets as Commuting Zones (CZs), or clusters of adjacent counties that have the commuting structure of a local labor market

(Tolbert and Sizer, 1996). To measure CZs' exposure to Chinese import competition, we follow Pierce and Schott (2020) and compute CZ-level exposure to PNTR as the weighted-average "NTR gap" across industry families in the active CZ, where weights correspond to each industry's initial employment share in 1990.[6] For each Commuting Zone c we define:

$$\text{NTR gap}_c = \Sigma_j \frac{L_{jc}^{1990}}{L_c^{1990}} * \text{NTR gap}_j \tag{2}$$

In Appendix C2, we describe how we create analogous measures of CZ-level exposure to Chinese imports using the approach from Autor et al. (2013) and Acemoglu et al. (2016).

2.2 NLRB-certified Union Elections

We use data from the National Labor Relations Board (NLRB) on every union election from 1960–2009. For each election, we observe information including the employer name and address, broad industry classification, and outcome of each election.We geocode the address of each election to obtain its county, and we use a county-Commuting Zone crosswalk to obtain its Commuting Zone.[7] The election data only includes the broad sector of the affected establishment. To measure industry-level exposure of the establishment tied to an election, we link the election data to the National Establishment Time Series (NETS) database using fuzzy matching methods; see Appendix B for details. We refer readers to DiNardo and Lee (2004) and Frandsen (2021) for more institutional background on the process through which workers seeking to form a union organize NLRB-certified elections.

We collapse thee data to measure the number of union elections in each industry family and in each CZ for each year. Table A1 shows summary statistics for the union election data and our measures of import penetration before and after the PNTR policy change. Figure A1 shows the distribution of the number of annual union elections by industry and by CZ.

[6] Pierce and Schott (2020) conduct analysis at the county level, whereas we aggregate up the CZ level.

[7] This crosswalk is from David Dorn's website, available at https://www.ddorn.net/data.htm.

2.3 Other Data

We obtain data on a variety of measures that also could have contributed to a decrease in union elections after 2000. At the industry level, these include: 1990 capital and skill intensity, exposure to the expiration of the global Multi-Fiber Arrangement (MFA), changes in Chinese import tariffs, changes in Chinese production subsidies, average import NTR tariff rates, contract intensity, and an indicator for advanced technology products. We measure these same variables at the CZ level by computing the weighted-average across industry families in the active CZ, where weights correspond to each industry's initial employment share in 1990. At the CZ level, we also obtain the 1990 manufacturing employment share. See Pierce and Schott (2016) and Pierce and Schott (2020) for more details of these measures.

We also obtain two datasets from unionstats.com (Hirsch and Macpherson, 2003) with the 1989 unionization rate in each state and with the 1989 unionization rate in each Census Industry Code (CIC) nationally.[8]

3 Direct and Indirect Effects of Import Competition on Union Organizing Efforts

The acceleration of Chinese imports in the early 2000s substantially reduced new union organizing through both direct industry exposure and indirect local labor market exposure.

3.1 Industry Trade Exposure and Union Certification Elections

Figure A2 provides a descriptive sense of the relationship between industries' exposure to import competition and union organizing. We separate manufacturing industries into terciles of their NTR_{gap}, as defined in Equation 1. The figure displays the average number of annual

[8] unionstats.com uses data from the Current Population Survey Monthly Outgoing Rotation Group (CPS-MORG) to estimate unionization rates for various units of analysis. We convert CIC codes to 4-digit SIC codes using a crosswalk from David Dorn's data page: https://www.ddorn.net/data.htm. Because CIC codes do not correspond one-to-one with 4-digit SIC codes, we take the mean of the various unionization rates that correspond to the same SIC code.

union elections in these three terciles, normalized to 1999 values. Prior to the year 2000 (when PNTR was granted), trends in elections are flat and similar for the three industry groups. Beginning in 2001, the trends diverge: the number of annual elections decrease for all three groups, but the decrease is most pronounced among industries with the highest exposure and least pronounced for those with the lowest exposure.

To more formally assess how industry exposure to the China Shock affected new union organizing, we estimate the following regression:

$$elections_{jt} = \theta PostPNTR_t \times NTRGap_j + \gamma PostPNTR_t \times X_j + \lambda X_{jt} + \delta_j + \delta_t + \epsilon_{jt}, \quad (3)$$

where $PostPNTR_t$ is an indicator for years 2001–2007 and where the $NTRGap$ for industry j is the difference between the NTR and non-NTR tariff rates for industry j, as defined in Equation 1.[9] X_j represents a vector of time-invariant industry-level controls. Depending on the model, X_j may include: 1989 unionization rates, 1990 capital and skill intensity, contract intensity, changes in Chinese import tariffs, and an indicator for whether the industry includes advanced technology products. X_{jt} represents a vector of time-varying controls including NTR tariff rate and exposure to MFA reductions. δ_j and δ_t are industry and year fixed effects, respectively. Because $elections_{jt}$ is a count variable that includes a non-trivial number of zeroes, we estimate Equation 3 using a Poisson Pseudo-Maximum Likelihood estimator (Silva and Tenreyro, 2006). We cluster standard errors by industry and weight observations by each industry's 1990 employment.[10]

The regression results in Table 1 reveal that industries more exposed to Chinese import competition experienced fewer union certification elections. In Column 1, which only includes

[9] NTR gaps are only available for 424 industry family codes, of which 378 are in manufacturing. Further, following Pierce and Schott (2016), we restrict to the 293 manufacturing industry families with non-missing measures for NTR tariffs, changes in tariffs, contract intensity, and capital and skill intensity. Of these 293 industry families, 17 have zero elections across all years in our sample and are therefore excluded from estimations using the Poisson Pseudo-Maximum Likelihood estimator.

[10] Because our empirical design uses a policy change from a single year (2001) that had differential bite across industries, our estimate of θ in Equation 3 will not be affected by the potential bias arising in difference-in-difference designs based on staggered adoption of a treatment in different years (Goodman-Bacon, 2021).

industry and year fixed effects, the estimated coefficient is negative and highly significant.

Since industries more exposed to Chinese imports experienced relative declines in employment (Pierce and Schott, 2016), union elections may have fallen in these industries simply because there were fewer workers to organize. In Column 2, we include an exposure measure for each industry's one-year lagged employment that effectively treats the dependent variable as a rate of elections per worker. The coefficient does decrease slightly in magnitude but remains highly significant.

The estimate is stable—and if anything, increases in magnitude— across specifications that add a progressively richer set of controls, including industries' initial capital and skill intensity (Column 3), a range of other time-varying industry characteristics (Column 4), and industries' initial unionization rate (Column 5).

We can convey the economic significance of this effect two ways. First, the coefficient in Column 5 implies that moving an industry from the NTR-gap at the twenty-fifth (0.26) to the seventy-fifth (0.38) percentile of the observed distribution led to 21% fewer union elections from 2001–2007 ($\hat{\beta} = -1.98$, $e^{-1.98*0.12} - 1 = -.21$; $0.12 = .38 - .26$). Second, we can calculate the implied overall relative effect of PNTR on rates of union elections, accounting for differing employment shares across industries. This exercise implies that industries' exposure to Chinese imports (via PNTR) led to 45 percent fewer union elections.

Finally, in Column 6, we replace the dependent variable from the number of total elections with the number of *successful* elections (in which the union is victorious). The point estimate is essentially unchanged, suggesting that the China Shock had little effect on voting outcomes conditional on an election being held. This null effect on the probability that elections were successful, conditional on taking place, is consistent with models from Farber (2001) and Farber (2015), both of which show that a deteriorating organizing environment can lead unions to target workplaces for elections where they have a higher probability of winning.

If industry import exposure caused not just a decline in the number of elections, but also a change in the composition of which workplaces *did* get organized, our results might not reflect the change in the share of each industry's workforce that organized for union elections. To address this concern, we calculate the number of *workers* eligible to vote in organizing elections. We construct this measure using the variable in the NLRB dataset capturing the number of workers eligible to vote in an election; we sum this variable to the industry-year level (coding it as zero in years with no elections). Table A2 recreates Table 1 except that the dependent variable is the number of *workers* eligible to vote in organizing elections. The estimates are similar to those in Table 1 (and if anything are slightly larger), indicating that industry trade exposure reduced union organizing in smaller and larger establishments alike.

For the decline in union elections to be attributed to a causal effect of PNTR, the NTR gap should be correlated with the number of union elections in the years following the passage of PNTR, but not before. We use an event-study specification that replaces the $PostPNTR$ indicator with a series of year dummies interacted with each industry's NTR gap:

$$elections_{jt} = \sum_{\tau=1991}^{2007} \theta_\tau \mathbb{1}\{t = \tau\} \times NTRGap_j + \gamma PostPNTR_t \times X_j + \lambda X_{jt} + \delta_j + \delta_t + \epsilon_{jt} \tag{4}$$

The $\hat{\theta_\tau}$ coefficients are displayed graphically in Figure 2. Panel (a) shows estimates from a model with industry and year fixed effects only (akin to Column 1 of Table 1); panel (b) shows estimates that additionally include the full set of controls (akin to Column 4 of Table 1). For both specifications, the coefficients for the years prior to the passage of PNTR all hover near zero and are all statistically insignificant, indicating that differential pre-trends in elections do not contaminate our difference-in-difference estimates. Beginning in 2002, the coefficients become negative, with slighly larger magnitudes for the specification in panel (b). In both cases, the post-period coefficients are jointly statistically significant ($p = 0.024$ and $p < .01$ in panels (a) and (b), respectively).

Workers in manufacturing industries directly exposed to Chinese import competition were substantially less likely to organize for a union election. We next examine if organizing efforts changed for workers indirectly exposed to Chinese imports via their local labor market.

3.2 Local Labor Market Exposure and Union Certification Elections

Figure A3 provides a descriptive sense of the relationship between Commuting Zones' (CZs') exposure to import competition and union organizing. As with the industry exposure, the figure displays the average number of annual union elections (normalized to 1999 values) for three groups of CZs, where the groups correspond to three terciles of NTR_{gap}, as defined in Equation 2. Prior to the year 2000 (when PNTR was granted), the trends in elections are flat and similar for the three groups of CZs. Beginning in 2001, the trends diverge: the number of annual elections sharply decreases for the most exposed commuting zones, whereas the number stays essentiall flat among CZs with the lowest exposure.

To examine the *indirect* effect of local labor market exposure to import competition on workers' union organizing efforts, we estimate:

$$Elections_{ct} = \theta PostPNTR_t \times NTRGap_c + \gamma PostPNTR_t \times X_c + \lambda X_{ct} + \delta_c + \delta_t + \epsilon_{ct}, \tag{5}$$

where our time-invariant controls X_c are state unionization rate in 1989, percent of initial CZ workforce employed in manufacturing, changes in Chinese tariffs and exposure to changes in Chinese subsidies and where time-varying controls X_{ct} include the overall import tariff rate for industries within a CZ and the exposure to MFA reduction. δ_c are CZ fixed effects and δ_t are year fixed effects. We again use a PPML estimator to estimate Equation 5; we cluster standard errors by state and weight observations by each CZ's 1990 employment.

The regression results in Table 2 reveal that CZs with greater exposure to Chinese imports (via their initial industry mix) experienced fewer new union certification elections. The magnitudes are economically meaningful. In Column 1, which only includes CZ and year fixed effects, the coefficient implies that moving a CZ from the NTR gap at the twenty-fifth (0.141) to the seventy-fifth (0.185) percentile of the observed distribution led to 25% fewer union elections from 2001–2007 ($\hat{\beta} = -6.64$, $e^{-6.64*0.043} - 1 = -0.25$; $0.043 = .185 - 141$). The implied overall effect indicates that the granting of PNTR led to 60% fewer new union elections than there otherwise would have been over this period.

The estimated effect decreases slightly in magnitude but remains highly significant when we include an exposure measure for lagged employment (Column 2). Controlling for several CZ-level variables that could be correlated with the NTR gap (Column 3) or the state's 1989 unionization rate (Column 4) has little effect on the point estimate, though the additional CZ-level variables increase the standard error on our coefficient of interest.[11] When we include all of these controls (Column 5), the point estimate remains large in magnitude, but the standard error increases so much that the estimate loses statistical significance ($p = 0.232$).

To examine if CZ exposure changed the composition of which workplaces were organized, Table A3 recreates Table 2 except that the dependent variable is the number of *workers* eligible to vote in organizing elections. In most columns the estimates are quite similar to those in Table 2, though the point estimates are attenuated and are insignificant in Columns 3 and 5 that include the battery of CZ-level controls.

As with our industry-level analysis, we use an event-study specification to assess both potential contamination from pre-trends and dynamic effects of local labor market exposure:

$$Elections_{ct} = \sum_{\tau=1991}^{2007} \theta_\tau \mathbb{1}\{t = \tau\} \times NTRGap_c + \gamma PostPNTR_t \times X_c + \lambda X_{ct} + \delta_c + \delta_t + \epsilon_{ct} \tag{6}$$

Figure 3 displays the $\hat{\theta}_\tau$ coefficients graphically. As before, Panel (a) shows estimates from a model with CZ and year fixed effects only (akin to Column 1 of Table 2); panel (b) shows

[11]The inclusion in particular of *Mfg employment in 1990* is quite demanding; our identifying variation essentially compares CZs with an identical *share* of initial employment in manufacturing, but with different composition of manufacturing industries. It is thus not surprising that the standard error increases substantially when including this control variable.

estimates that additionally include the full set of controls (akin to Column 5 of Table 2). In both specifications, the coefficients for the years prior to the passage of PNTR all hover near zero and are all statistically insignificant. Beginning in 2002, the coefficients become negative and increase in magnitude over time. The post-period coefficients are jointly significant in both specifications ($p < .01$) and all individually significant in the baseline specification.

These local labor market estimates are intended to capture the effect of‘*indirect* exposure: how workers' organizing efforts are affected when other firms in their local labor market face import competition. However, within manufacturing, our measure of CZ exposure also reflects the degree to which firms faced differential *direct* exposure.

To isolate indirect exposure, we split industries into *Manufacturing* and *Non-Manufacturing* based on industry codes. We separately estimate our baseline version of Equation 5 (akin to Column 1 of Table 2) for each sector. Table A4 reports results. Greater CZ exposure has a particularly pronounced negative effect on new elections in the manufacturing sector, but it also leads to fewer elections in the non-manufacturing sector. Both estimates are large and highly statistically signiicant. These results reveal that workers in local labor markets that faced exposure to Chinese import competition were less likely to organize for union representation across all sectors and that much of this effect can be attributed to *indirect* exposure.

3.3 Robustness to Different Measures of Import Exposure

In Appendices C3 and C4, we describe our estimates of the effect of industry and CZ import exposure that use the approach from Autor et al. (2013) that leverages the "supply side shock" of Chinese exports. In both cases, the implied effects of import exposure on union elections are quite similar to our primary results that use variation induced by the NTR gap.

3.4 How Many Union Elections Did the China Shock Avert?

We motivated our analysis with Figure 1b, which showed an acceleration in the declining rates of union elections around the year 2000, which coincided with the granting of PNTR to China (and China's joining the WTO). A natural question arises: how much of this accelerated decline in union organizing does the China Shock actually explain?

We first calculate the number of elections "lost" due to the post-2000 trend break. Figure A4 illustrates this calculation, both for the manufacturing sector (panel A) and overall (panel B). In both panels, the solid line is the actual number of annual NLRB-certified union elections. The dashed line reflects the number of annual elections predicted to occur 2001–2008 based on a linear trend fitted on the years 1991–2000. The gap between the solid and dashed lines reflects the additional elections that would have occurred had the rate of union organizing continued following its pre-2000 trend. This is the quantity "to be explained."

In the manufacturing sector, there were 3,086 union elections between 2001–2008. Had the post-2000 trend break not occurred, there would have been an additional 2,083–or 67% more–elections over that same period. As comparison, the estimates in Table 1 imply that the granting of PNTR to China caused at least 27% fewer union elections in the manufacturing sector than would have otherwise occurred, based purely on industries' direct exposure to Chinese imports. That is, direct industry exposure explains at least 40% (0.27 / 0.67) of the post–2000 trend break in union organizing in the manufacturing sector.

Across all sectors, Panel (b) of Figure A4 reveals there would have been 7,340 (44%) more union elections 2001–2008 had the post-2000 trend break not occurred. The estimates in Table 2 imply that the granting of PNTR to China caused at least 46% fewer union elections across all sectors over this same period. That is, indirect local labor market exposure explains essentially *all* of the overall post-2000 trend break in union organizing.[12]

[12] While this implied effect of PNTR is strikingly large, Figure A3 suggests it is not implausible. Descriptively, CZs in the lowest tercile of PNTR exposure show no trend break in union organizing after 2000, which corroborates the calculation that CZs' indirect exposure to PNTR explains essentially all of the overall post-2000 trend break in organizing.

We can also use these results to estimate a related question: how many fewer workers became unionized via elections due to the China Shock?[13] The estimates in Table 1 indicate that 27–48 percent more union elections would have occurred over 2001–2007 in the manufacturing sector had PNTR not been granted. Using information reported in Table A1 on the share of elections in the manufacturing sector (0.18), the share of elections in which the union was successful (0.60), and average number of eligible employees in an election (74) over 2001–2007, we estimate that between 32,300 and 57,200 workers would have become unionized in the manufacturing sector over this period through the elections that did not occur due to the direct effect of PNTR.[14] As reference, in 2007 there were 1.1 million unionized workers in manufacturing (Bureau of Labor Statistics, 2007).

We similarly estimate how many workers did not become unionized across all sectors due to the *indirect* effect of PNTR, using the estimates from Table 2. This calculation implies between 272,186 and 438,153 workers would have become unionized in the private sector over this period through elections that did not occur due to PNTR's indirect effect on local labor markets. As reference, in 2007 there were 8 million unionized workers in the private sector (Bureau of Labor Statistics, 2007).

4 A Worker's Decision to Seek Union Representation

Why did Chinese import competition lower union organizing, both among workers in directly exposed industries and among workers indirectly exposed in their local labor market? We present a simple formalization of a worker's decision to seek unionization and organize a certification election. This framework highlights two factors—one a benefit, the other an expected cost—that undergird this decision. We assess how direct and indirect exposure to Chinese imports competition affects organizing through its effects on these two factors.

[13] This is not the same thing as asking what the China Shock's effect was on the unionization *rate*, which is a function not just of organizing but also broader structural shifts in the economy. See Ahlquist and Downey (2022) for an investigation into this important related question.

[14] The calculation is: (104 or 184) (estimated number of annual manufacturing elections averted due to direct effect of PNTR) $\times$ 7 (number of years in 2001–2007) $\times$ 0.60 (average election win rate) $\times$ 74 (average number of eligible employees), where 104 and 184 are calculated by 384*.27 and 384*.48, respectively.

We consider a worker, currently employed and not a member of a labor union, who is weighing whether to organize to hold an election to become unionized.[15] We abstract from the question of the likelihood that an election is successful and assume that if a worker's bargaining unit organizes to hold an election, the union will win with certainty. This simplifying assumption affects none of the main insights forthcoming from the formal framework.

A worker's decision to organize will depend, in part, on the presumed benefits of union membership. The most salient such benefit is the wage premium he can expect as a unionized worker, relative to the wage he currently earns as a non-union worker. A long literature consistently finds this "union wage premium" to be positive (Card, 1996; Farber et al., 2018); union membership can also increase other forms of compensation such as pension contributions (Knepper, 2020). We assume that the worker currently earns a wage of w_c at his non-unionized job and anticipates that if the job were unionized it would pay $w_u > w_c$.

The worker weighs the benefit of organizing against its various costs. Some of these costs are logistical like the opportunity cost of time spent attending organizing meetings or the monetary costs of union dues; we represent such costs, collectively, by k.

Another cost of organizing is that organizing for a union might increase the probability that a worker loses his job. This higher job loss risk could come from multiple sources. Employers may be more likely to lay off workers after a successful union election; this could arise if unions reduce firms' valuation (Lee and Mas, 2012) or if managers are opposed to working with unions (Wang and Young, 2022). Indeed, successful union elections are associated with decreased likelihood of establishment survival and lower employment at surviving establishments (Frandsen, 2021; Wang and Young, 2022). Furthermore, though we have abstracted from the possibility of an unsuccessful union campaign, employers frequently fire workers in retaliation for union organizing (Farber, 1987; Abowd and Farber, 1990; Bronfenbrenner, 2009; Stansbury, 2021).[16] We normalize the probability of job loss to zero if

[15] Our framework extends and somewhat formalizes the Ashenfelter and Pencavel (1969) model of the determinants of the growth in the unionization rate; we consider the decision to organize for a union.

[16] While such retaliatory firing is technically illegal under the National Labor Relations Act (NLRA), firms in practice face little incentive to comply with this law. There is no meaningful legal penalty for firms that

a worker remains non-unionized; if the worker seeks union certification, he keeps his (newly unionized) job with probability $p_i \in \{0, 1\}$ and loses his job with probability $1 - p_i$.

This higher risk of job loss is costly. A large literature, pioneered by Jacobson et al. (1993), illustrates that job loss is associated with a sustained and meaningful reduction in workers' earnings. This reduction reflects loss of a firm-specific premium, firm-specific human capital, or match-specific benefits (Lachowska et al., 2018), among other factors. To capture this "cost of job loss," we assume that a worker's "alternative wage" w_a—the wage he would earn if he were to lose his job—is lower than his current wage, so $w_a < w_c < w_u$.[17]

Given this set up, we can write the worker's expected wage if he organizes as

$$E(\text{wage} \mid \text{organize}) = p_i w_u + (1 - p_i) w_a - k$$

and his expected wage if he does not organize as simply $E(\text{wage} \mid \text{not organize}) = w_c$. The worker chooses to organize if and only if

$$p_i w_u + (1 - p_i) w_a - k > w_c.$$

or, rearranging terms, if:

$$p_i(w_u - w_c) + (1 - p_i)(w_a - w_c) - k > 0. \tag{7}$$

The first term on the left side in (7) represents the *union wage premium*—the increase in the wage a worker can expect if he successfully organizes his workplace to be unionized—multiplied by the probability that he keeps his (newly unionized) job. The second term

violate this law. The NLRA can only issue "make-whole" remedies to compensate a fired worker for losses incurred because of the employer's actions. There is no legal scope to subject firms to financial penalties or for criminal liability. Given these weak incentives, it is unsurprising that there is evidence of widespread noncompliance with the NLRA (Stansbury, 2021).

[17] A more appropriate term than "alternative wage" might be the hourly value of the worker's next best alternative. This value would be a weighted average of what a worker receives if unemployed and his anticipated wage at a new job, where the weights would depend on the anticipated period of unemployment. Our term w_a, which we use for the sake of exposition, is meant to correspond to this value.

on the left side (which is negative) represents the *cost of job loss*—the wage decrease a worker will experience in the event of job loss, relative to his current wage—multiplied by the probability that he loses his current job.

4.1 Chinese Import Competition and the Organizing Decision

Does this framework help explain the empirical results in Section 3? Viewed through the lens of our framework, we propose that greater *industry* exposure to Chinese import competition constituted a negative shock to the union wage premium. Greater *local labor market* exposure could also have modestly reduced the union wage premium, but its primary effect was to exacerbate the cost of job loss. We discuss our rationale for these arguments in turn.

4.1.1 Industry Exposure as a Shock to the Union Wage Premium

Firms operating in industries more exposed to the surge in Chinese import competition experienced lower sales, profitability, and patenting (Autor et al., 2020). A worker whose employer was directly exposed to the China Shock would thus plausibly expect a lower *union wage premium*. One way that unions bargain for wage increases above and beyond the "market" wage is by extracting quasi-rents from employers; indeed, prior studies reveal that changes in firm-level quasi-rents lead to changes in union-negotiated wages (Abowd and Lemieux, 1993) and the union wage premium (Rose, 1987). The negative impacts on firms directly exposed to Chinese imports plausibly reduced quasi-rents and—as a result— the union wage premium in these firms. Marshall's law of derived demand further illustrates why exposed unions would negotiate for lower wages: Chinese imports increases the availability of product substitutes to consumers, thereby increasing U.S. firms' elasticity of product (and thus labor) demand. With a more elastic demand for labor, unions would need to absorb higher employment reductions for a given wage premium above the market wage.

Below, we assess whether industries more exposed to import competition saw smaller negotiated union wage gains. If present in the data, this pattern would suggest that the benefits of union representation declined in these industries, potentially explaining why fewer workers in them sought union representation via a certification election.

4.1.2 Local Labor Market Exposure as a Shock to the Alternative Wage

Commuting Zones (CZs) with an initial industry mix that rendered them more exposed to Chinese imports experienced substantial deterioration in employment prospects, including higher unemployment rates (Pierce and Schott, 2020) and losses in employment in manufacturing industries (Acemoglu et al., 2016). Furthermore, more exposed CZs experienced a decline in average wages, particularly in *non-manufacturing* sectors (Autor et al., 2013; Bloom et al., 2019), likely due to outward shifts in labor supply. That is, workers in import-exposed local labor markets experienced a negative shock to their "alternative wage" (w_a).

Such changes in import-exposed local labor markets clearly affects the second force in our model: the cost of job loss. A worker losing her job in a local labor market in which other firms were heavily exposed to the China Shock would expect a longer duration of joblessness (due to the deterioration in employment prospects) and a lower expected wage when she finds a new job (because of the reduction in the market wage and that downward wage stickiness is higher for already-employed workers than for new workers (Kahn, 1997; Pissarides, 2009)). Combined, these results imply that the cost of job loss increased for workers in local labor markets highly exposed to Chinese import competition. If union organizing raises the risk of job loss (which the discussion above indicates it does), then this higher cost of job loss would make workers more reluctant to seek union representation via a certification election.

A reduced alternative wage in import-exposed labor markets could also affect the first force in our model—unions' ability to extract wage gains (w_u)—but potentially to a more muted extent. If union wage-setting reflects a bargaining over joint surplus, then a reduction in w_a would reduce w_u by worsening workers' threat point (Abowd and Lemieux, 1993). However, Card (1990)'s model of bargaining implies that the "predicted elasticity of the [union] wage with respect to the alternative wage...is relatively small" (pg 634).[18] A more muted effect would be consistent with empirical evidence that a worsening of workers' outside options (as measured by increasing employer concentration) lowers wages much more for non-union than for union workers (Benmelech et al., 2022).[19]

5 Why Did Import Competition Lower Union Organizing Efforts?

Above we proposed that direct industry exposure to Chinese import competition led to lower union organizing by reducing the size of union-negotiated wage increases and that indirect local labor market exposure led to lower organizing primarily by increasing the cost of job loss. We use data on the outcomes of collective bargaining agreements between private-sector unions and their employers—as well as data from other sources—to test for these two proposed mechanisms empirically.

5.1 Data on Union Contract Settlements

We obtain summaries of every collective bargaining agreement between private-sector employers and labor unions over the years 1992–2007 from the Bloomberg Bureau of National Affairs (BNA) Labor Plus database. Of these 8,296 contracts, we restrict our attention to 7,925 that were negotiated in the mainland U.S. For each contract, we observe the settlement year, employer name and broad industry classification, city and state(s) where the contract was negotiated, the number of workers covered by the contract, the contract expiration date, and a textual summary of the negotiated wage and benefit changes.

[18] Other models of union-wage setting imply similar predictions. if unions set wages as a monopolist, and thus trade off higher wage premiums against higher cuts to employment, then a reduction in w_a would lead the union to negotiate for a lower wage to minimize employment losses. However, this "general equilibrium" effect on w_u is likely smaller than the more direct effect of a shock to profitability.

[19] Evidence that laid-off union workers are substantially more likely to receive unemployment insurance (UI) benefits than comparable nonunion workers (Budd and McCall, 1997) reinforces this notion that shocks to the alternative wage would have a more muted effect on union-negotiated wages. That is, because union workers are more likely to receive UI benefits conditional on job loss, their outside option falls by less for a given increased period of expected joblessness, relative to non-union workers.

Each observation represents an employer–union bargaining unit contract settlement. For some analyses, it is useful to identify repeat observations of the same employer-union dyad; however, there is no standardized employer (or employer-union) ID. In Appendix B, we describe in detail how we identify repeat observations of the same employer-union dyad. Among all employer-union dyads, the mean number of contracts we observe is 1.4 contracts; among those dyads that we observe more than once, the mean number of contracts is 3.0 contracts. Some contract durations were specified in months rather than years. We converted these month-based contract durations to years and rounded them to the nearest integer. Across all contracts, the mean contract duration, defined this way, is 3 years.

Importantly for our purpose, the data contain a text summary of the amount of negotiated wage increases and any other non-wage provisions included in the contract such as pensions and healthcare packages. Because these measures were text summaries, we had to take several processing steps to convert them to numeric values and construct a measure of the negotiated wage increase for each contract. We describe this process in Appendix B.

Table A5 displays summary statistics on contract settlements for the negotiated wage increases, contract duration, and number of covered workers,[20] and Figure A5 provides a histogram of wage increases in our sample period. Since this variable exhibits right skew, we use its log as our dependent variable (first adding the lowest non-zero wage increase to accommodate the few cases in which a contract negotiated for a wage increase of zero).[21]

In Appendix B, we discuss additional processing steps we took to assign contracts to SIC industry codes (and thus industry families as defined by Pierce and Schott (2016)), and to assign contracts to CZs. Some contracts cover multiple CZs. When estimating the effect of local labor market import exposure, we transformed our data set to be at the level of an employer-union-contract-CZ, and our primary analysis restrict attention to the subset of contracts that cover only one CZ (roughly 88% of contracts). (We expand the sample to

[20]We topcode the number of covered workers at the 99th percentile to avoid undue influence from a few large outliers.

[21]Results are essentially identical (in percent terms) if we instead use the un-logged percent wage change.

include contracts that corresponded to multiple CZs in robustness checks.)

5.2 Industry Trade Exposure and Union-Negotiated Wages

We first assess if unions representing workers in highly exposed industries negotiated for relatively smaller wage increases. Our estimating equation is:

$$ln(w_{it}) = \theta PostPNTR_t \times NTRGap_{j(i)} + \gamma PostPNTR_t \times X_{j(i)} + \lambda X_{j(i)t} + \delta_{j(i)} + \delta_t + \epsilon_{it} \tag{8}$$

where $ln(w_{it})$ is the log of the percent wage increase negotiated in a contract negotiated in year t by a union-employer dyad i (which has associated industry $j(i)$). $PostPNTR_t$ is an indicator for years 2001–2007, and the $NTRGap$ for industry $j(i)$ is the difference between the NTR and non-NTR tariff rates for industry $j(i)$, defined in Equation 1.[22] $X_{j(i)}$, $X_{j(i)t}$, $\delta_{j(i)}$, and δ_t are as described in Section 3.1. We cluster standard errors by industry family and weight observations by the (topcoded) number of employees covered by the contract.

The estimates reported in Table 3 reveal that unions in industries more exposed to Chinese imports negotiated for lower wage increases. The coefficient in Column 1, from a model that conditions on industry family and year fixed effects only, implies that moving an industry's NTR gap from the 25th to 75th percentile leads unions to negotiate for 25% lower wage increases ($p < .01$). Adding progressively more industry-level controls in Columns 2 and 3 leaves the implied effect largely unchanged.

We examine the sensitivity of these estimates to various considerations in Table A6. Column 1 reproduces our baseline estimate. In Column 2, we give all contracts equal weight (that is, we do not weight observations by the number of workers covered by the negotiated contract); the estimate is smaller in magnitude but remains negative and is significant at the 10-percent level ($p = 0.065$). We next include various sets of additional fixed effects might

[22]As in the elections analysis, we follow Pierce and Schott (2016) and restrict to the 293 manufacturing industry families with non-missing NTR tariffs, changes in tariffs, contract intensity, and capital and skill intensity.

be important. Including fixed effects for the contract duration number of years (Column 3) or for the Census division in which the employer-union dyad is located (Column 4) leaves the estimates essentially unchanged.[23] Finally, in Column 5 we include employer-union *dyad* fixed effects instead of industry fixed effects. In theory, this specification is appealing since import exposure could plausibly change the composition of unionized establishments, for example if establishments with strong unions are more likely to shut down. In practice, this specification is problematic because we lose a substantial amount of data and identifying variation: we observe multiple contracts for only 20 percent of the dyads in our sample, and among these not all have contracts both before and after PNTR. With this caveat in mind, the coefficient in Column 5 is quite similar to our baseline estimate, though more imprecise ($p = 0.20$).

These results imply that direct industry exposure to the China Shock reduced the wage gains that unions could bargain for, reducing a (nonunion) worker's expected benefit from organizing to hold a union election.

5.3 Local Labor Market Import Exposure, the "Alternative Wage," and Union Organizing

After corroborating prior work that local labor market import exposure decreased overall employment and wages, we conduct separate tests of whether this negative shock to the "alternative wage" affected the two channels that our framework predicts deter union organizing: union-negotiated wages and the expected costs of job loss from organizing.

5.3.1 Local Labor Market Exposure and the "Alternative Wage"

Prior work has found that Commuting Zones more exposed to Chinese imports experienced substantial deterioration in overall labor market conditions. We replicate and extend some

[23] In theory, contract duration could be endogenous to direct import exposure, in which case controlling for it could bias our coefficient of interest. With this consideration in mind, it is still reassuring that including it does not materially affect our main coefficient.

of these results in Table 5. Panel A shows that CZs more exposed to PNTR experienced higher unemployment rates; this finding mirrors results from Pierce and Schott (2020), who performed this analysis at the county level.[24][25] We then estimate the effect of exposure to PNTR on the *market* wage. We use data from the Quarterly Workforce Indicators (QWI), which contains average earnings and employment by county–quarter–sex–age-group. We aggregate these data to the CZ level. We run a similar regression to Equation 3 with our dependent variable as log average earnings; we also include fixed effects for quarter, sex, and age group. We report results in Panel B. The baseline estimates indicate that the average (market) wage was significantly lower in CZs more exposed to PNTR, which corroborates findings from Autor et al. (2013) who use a different identification strategy. However, the estimate is sensitive to specification: it is attenuated to zero when we control for the 1990 manufacturing employment share in Column 4.

In import-exposed Commuting Zones, workers who found themselves without a job would have expected a longer duration of unemployment and (according to some specifications) a lower wage conditional on finding a job; that is, the cost of job loss increased in these markets.

5.3.2 Local Labor Market Exposure and Union-Negotiated Wages

Though we argued above that this negative shock to the "alternative wage" would primarily reduce union organizing by raising the cost of job loss, it could also do so by reducing union-negotiated wages. To examine this possibility, we estimate the following regression:

[24] Our specification differs from that reported in Panel A of Figure 5 in Pierce and Schott (2020) in other ways: their estimate is based on a sample through 2012 (whereas ours ends in 2007), they do not control for the baseline manufacturing employment share (which we do in Column 4), and they include a few other county-level controls less relevant for our analysis such as the percent of the population that are veterans.

[25] Pierce and Schott (2020) show the effect of PNTR on the unemployment rate increased in magnitude over time (Figure 5 in their paper). This could partially explain why our event study estimates of the effects of CZ import exposure on elections in Figure 3a increase in magnitude over time.

$$ln(w_{it}) = \theta PostPNTR_t \times NTRGap_{c(i)} + \gamma PostPNTR_t \times X_{c(i)} + \lambda X_{c(i)t} + \delta_{c(i)} + \delta_t + \epsilon_{it}, \tag{9}$$

where $ln(w_{it})$ is the log of the percent wage increase negotiated in a contract negotiated in year t by union-employer dyad i located in CZ $c(i)$. $PostPNTR_t$ is an indicator for years 2001–2007, and the $NTRGap$ for CZ $c(i)$ as defined in Equation 2. $X_{c(i)}$, $X_{c(i)t}$, $\delta_{c(i)}$, and δ_t as defined in Section 3.2. We cluster standard errors by state and weight observations by the (topcoded) number of employees covered by the contract.

We report estimates of θ from Equation 9 in Table 4 with varying levels of controls. Across each specification, higher CZ exposure has a negative effect on the average wage increases negotiated by unions; however, the coefficients are always statistically insignificant at conventional levels and are less than half the magnitude of the direct industry effects in Table 3. These estimates are largely unchanged when we assess their sensitivity to similar considerations as we did for the industry-level analysis, as reported in Table A7. They are also essentially unchanged if we expand the sample to include contracts that cover more than one CZ, as reported in Table A8. These estimates provide suggestive evidence that a negative shock to the alternative wage could lead to lower union-negotiated wages, but to a more muted extent than the direct negative shock to firm profitability.

5.3.3 Protections Against Employer Retaliation Mitigated Local Labor Market Exposure's Effect on Organizing

Our framework in Section 4 highlights a second (and, we argue, central) way that a negative shock to a worker's "alternative wage" w_a deters union organizing: by increasing the cost of job loss $(w_c - w_a)$. However, this shock deters organizing only if organizing actually *raises* the risk of job loss (that is, if $(1 - p_i) > 0$). One reason organizing raises the risk of job loss is if workers perceive their employer might retaliate against them for doing so. As described in Section 4, reports of such retaliation are widespread, plausibly in part because federal anti-retaliation laws are either nonexistent or rarely enforced. However, several states have passed their own anti-retaliation laws. Our framework implies that the the presence of such laws should attenuate the effect of local labor market exposure on union elections.

To examine this hypothesis empirically, we consider two distinct state-level laws that limit employer retaliation for actions like union organizing. The first is a measure of statutory protection against retaliation for workers who speak up about workplace violations. This measure, created by Henderson (2022), groups states into four tiers based on the level of protections to workers who report violations and the size of potential fines imposed on retaliating employers.[26] Sixteen states had some form of such anti-retaliation protections in place in 2019. The second is an indicator for whether (as of 1999) a state had passed the *good faith exception* to at-will employment. The good-faith exception is one of three "wrongful discharge laws" adopted through court decisions that put limits on employers' ability to fire at-will workers. The good-faith exception applies when a court deems that an employer discharged a worker in an unjust manner; workers who believe they have been terminated in violation of the exception can sue their employer for compensatory and punitive damages. As of 1999, 9 of the 48 contiguous US states had adopted the Good Faith exception.[27]

Both of these measures come with advantages and disadvantages for our purpose. The anti-retaliation measure explicitly applies to workers who report violations of workplace laws (like the NLRA)—precisely the kind of situation our framework considers.[28] However, this measure is from 2019; we do not know whether such protections were in place as of 1999

[26]States receive a score of 1, 0.75, 0.5, or 0, based on a methodology from National Employment Law Center (2019). Higher scores indicate that workers are afforded more protection (such as have an explicit right to bring a complaint to a government agency or to court) and that violating employers face higher penalties (such as requiring back pay, monetary damages, attorneys' fees, and potential government-imposed fines. See Appendix 1 of (Henderson, 2022) for details.

[27]There are two other wrongful discharge laws that are less relevant for this paper. The *public policy exception* applies to workers who are fired for taking an action in line with a stated public policy. The *implied contract* exception applies when a worker can verify that her employer promised a permanent employment relationship. See, e.g., Autor et al. (2006) or Johnson et al. (2022) for more details.

[28]Henderson (2022) measure protections for reporting wage and hour violations. However, these protections presumably make employers reluctant to retaliate in ways that violate other labor laws like the NRLA.

(the last year of our "pre-period"), introducing error in this measure for our purposes. An advantage of the good-faith exception is that we observe it in the year 1999, so it is measured without error. However, it is not explicitly about violations of workplace laws. That said, retaliation against union organizing is a clear example of termination in "bad faith" and would thus be in clear violation of the exception.

To examine if protections against retaliation attenuate the effect of local import exposure on union organizing, we amend Equation 5 as follows:

$$
\begin{aligned}
Elections_{ct} = {} & \theta_1 PostPNTR_t \times NTRGap_c + \theta_2 PostPNTR_t \times NTRGap_c \times Retal_{s(c)} \\
& + \gamma PostPNTR_t \times Retal_{s(c)} + \gamma PostPNTR_t \times X_c + \lambda X_{ct} + \delta_c + \delta_t + \epsilon_{ct}
\end{aligned}
$$

Our coefficients of interest are θ_1, which captures the effect of Chinese import exposure in CZs located in states without any anti-retaliation protection, and θ_2, which captures the differential effect of import exposure for CZs in states with such protection. We operationalize *Retal* as either a continuous variable between 0 and 1 measuring statutory protections for workplace violations, or as a binary variable indicating passage of the Good-Faith exception. All other variables are as described under Equation 5. Because states with retaliation protection may differ in other ways than those without, we report results that include the full set of controls, corresponding to Column 5 of Table 2.

Table 6 displays results. Column 1 shows that in states with no statutory protection against retaliation for reporting workplace violations, CZ import exposure had a large and negative effect on union elections, though the estimate is not quite statistically significant ($\hat{\beta} = -4.10$, $p = 0.142$). The interaction term indicates a significantly *less* negative effect in states with a full set of protections ($p = 0.088$). Column 2 reports similar results when we measure protections against retaliation using the good-faith exception: here the interaction term is even more significant ($p < .01$). In Column 3, we measure retaliation protection with an indicator variable equal to one if a state had either of the protections considered in

Columns 1 and 2: the interaction term is again positive and significant ($p = 0.022$).

Workers in import-exposed CZs became more reluctant to organize to hold union elections, but only in environments in which organizing presumably raised the risk of job loss (via employer retaliation). Combined with the results in Section 5.3.1, these results further support the interpretation from our conceptual framework for why local labor market import exposure reduced rates of union certification elections.

5.4 Import Exposure and Employers' Interference in Organizing

The results in Sections and 5.2 and 5.3 support our hypotheses that direct industry exposure to Chinese imports reduced the expected wage gains from unionization, and indirect local local labor market exposure increased the cost of job loss–both of which discourage a worker from organizing. In Appendix E, we present analysis of how import exposure affected *employers'* interference in union elections. We find some evidence that direct industry exposure led to an increase in employer resistance to union drives, and we find no detectable effect of indirect local labor market exposure. This analysis complements our framework in Section 4 that considers the worker's perspective.

6 Conclusion

Worker attempts to organize labor unions, reflected in the number of union certification elections, have fallen sharply in the last few decades. We show that the surge in Chinese imports into the U.S. beginning in the 1990s accounted for a meaningful share of this decline through two channels. The "China Shock" led to lower union organizing both among workers in industries that faced direct import exposure and among those in local labor markets where a large share of firms (even if not a worker's own) faced such exposure. We propose and present evidence that direct industry exposure eroded the expected wage gains from union representation, whereas indirect local labor market exposure raised the cost of job loss.

Our results inform a puzzle in labor economics that has been called the "representation gap:" if workers' stated desire for unionization has been steadily high over the last few decades (Kochan et al., 2019), why are fewer workers organizing to seek union representation? Undoubtedly, forces outside the scope of this paper, such as legal and political hostility to organized labor (Stansbury, 2021), have played a role. Our paper offers evidence for another partial explanation. As international trade has weakened many workers' labor market prospects, it has increased the costs associated with job loss—which union organizing makes more likely. Thus, a growing number of workers may have become reluctant to seek union representation, even if their benefits of doing so could be large. At the same time, our paper reveals that some portion of the decline in organizing may also reflect declining benefits of unionization for some workers.

It is well documented that wages have stagnated for the majority of U.S. workers in recent decades. A key question is the degree to which this trend is driven by fundamentals (such as the rise in international trade) versus institutions (such as the decline in labor unions). Our paper implies that these explanations are not mutually exclusive. We find that the rise in Chinese import competition (one of the "fundamentals") led to a decline in workers' efforts to organize for unions (one of the "institutions"), which may have interacted with and compounded each other to cause changes in the wage structure.

Finally, our paper's framework provides a lens to understand how other economic shifts can affect union organizing. In 2022, the number of union certification elections increased by over 50 percent relative to the prior year, the largest single-year increase in at least two decades.[29] One high-profile election during this time—at warehouses owned by Amazon—is at a firm that had experienced a tremendous increase in profitability in the years prior. The potential benefits of unionization undoubtedly rose for these workers. Other notable elections, such as at Starbucks, are among a group of workers experiencing the tightest labor market in recent memory, and for whom the cost of job loss has fallen dramatically.

[29] National Labor Relations Board, "Number of Elections Held per FY," https://www.nlrb.gov/reports/nlrb-case-activity-reports/representation-cases/election/election-statistics (accessed November 2022).

References

Abowd, John A and Thomas Lemieux, "The effects of product market competition on collective bargaining agreements: The case of foreign competition in Canada," *The Quarterly Journal of Economics*, 1993, *108* (4), 983–1014.

Abowd, John M and Henry S Farber, "Product Market Competition, Union Organizing Activity, and Employer Resistance," Technical Report, National Bureau of Economic Research 1990.

Acemoglu, Daron, David Autor, David Dorn, Gordon H Hanson, and Brendan Price, "Import competition and the great US employment sag of the 2000s," *Journal of Labor Economics*, 2016, *34* (S1), S141–S198.

Ahlquist, John S and Mitch Downey, "The effects of import competition on unionization," *American Economic Journal: Economic Policy (forthcoming)*, 2022.

Ashenfelter, Orley and John H. Pencavel, "Trade Union Growth: 1900-1960," *The Quarterly Journal of Economics*, 1969, *83* (3), 434–448.

Autor, David, David Dorn, Gordon H Hanson, Gary Pisano, and Pian Shu, "Foreign competition and domestic innovation: Evidence from US patents," *American Economic Review: Insights*, 2020, *2* (3), 357–74.

Autor, David H, John J Donohue III, and Stewart J Schwab, "The costs of wrongful-discharge laws," *The Review of Economics and Statistics*, 2006, *88* (2), 211–231.

Benmelech, Efraim, Nittai K Bergman, and Hyunseob Kim, "Strong Employers and Weak Employees How Does Employer Concentration Affect Wages?," *Journal of Human Resources*, 2022, *57* (S), S200–S250.

Bidar, MUSADIQ, Caroline LINTON, and Irina IVANOVA, "Challenged ballots leave outcome in doubt of Amazon union vote in Alabama," *CBS News*, 2022.

Bloom, Nicholas, Kyle Handley, Andre Kurman, and Phillip Luck, "The impact of chinese trade on us employment: The good, the bad, and the debatable," *Unpublished draft*, 2019.

Bronfenbrenner, Kate, "No holds barred: The intensification of employer opposition to organizing," *Economic Policy Institute*, 2009.

Budd, John W and Brian P McCall, "The effect of unions on the receipt of unemployment insurance benefits," *ILR Review*, 1997, *50* (3), 478–492.

Bureau of Labor Statistics, "News Release: Union Members in 2007," Technical Report 2007.

Card, David, "Strikes and wages: a test of an asymmetric information model," *The Quarterly Journal of Economics*, 1990, *105* (3), 625–659.

_ , "The effect of unions on the structure of wages: A longitudinal analysis," *Econometrica: Journal of the Econometric Society*, 1996, pp. 957–979.

David, H Autor, David Dorn, and Gordon H Hanson, "The China syndrome: Local labor market effects of import competition in the United States," *American Economic Review*, 2013, *103* (6), 2121–68.

Dickens, William T, "The effect of company campaigns on certification elections: Law and reality once again," *ILR Review*, 1983, *36* (4), 560–575.

_ **and Jonathan S Leonard**, "Accounting for the decline in union membership, 1950–1980," *ILR Review*, 1985, *38* (3), 323–334.

DiNardo, John and David S Lee, "Economic impacts of new unionization on private sector employers: 1984–2001," *The quarterly journal of economics*, 2004, *119* (4), 1383–1441.

Farber, Henry, “Union organizing decisions in a deteriorating environment: the composition of representation elections and the decline in turnout,” *ILR Review*, 2015, *68* (5), 1126–1156.

Farber, Henry S, “The recent decline of unionization in the United States,” *Science*, 1987, *238* (4829), 915–920.

— **and Bruce Western**, “Accounting for the decline of unions in the private sector, 1973–1998,” *Journal of Labor Research*, 2001, *22* (3), 459.

— **and —** , “Ronald Reagan and the politics of declining union organization,” *British Journal of Industrial Relations*, 2002, *40* (3), 385–401.

— **and Daniel H Saks**, “Why workers want unions: The role of relative wages and job characteristics,” *Journal of Political Economy*, 1980, *88* (2), 349–369.

— **, Daniel Herbst, Ilyana Kuziemko, and Suresh Naidu**, “Unions and inequality over the twentieth century: New evidence from survey data,” Technical Report, National Bureau of Economic Research 2018.

Farber, Henrys, “Union success in representation elections: Why does unit size matter?,” *ILR Review*, 2001, *54* (2), 329–348.

Ferguson, John-Paul, “The eyes of the needles: A sequential model of union organizing drives, 1999–2004,” *ILR Review*, 2008, *62* (1), 3–21.

Frandsen, Brigham R, “The surprising impacts of unionization: Evidence from matched employer-employee data,” *Journal of Labor Economics*, 2021, *39* (4), 000–000.

Freeman, Richard B and James L Medoff, “What do unions do,” *Indus. & Lab. Rel. Rev.*, 1984, *38*, 244.

Goodman-Bacon, Andrew, “Difference-in-differences with variation in treatment timing,” *Journal of Econometrics*, 2021, *225* (2), 254–277.

Henderson, Kaitlyn, "Best and Worst States to Work in America 2022," *OXFAM Report*, 2022.

Hirsch, Barry T, "Sluggish institutions in a dynamic world: Can unions and industrial competition coexist?," *Journal of Economic Perspectives*, 2008, *22* (1), 153–176.

_ **and David A Macpherson**, "Union membership and coverage database from the current population survey: Note," *ILR Review*, 2003, *56* (2), 349–354.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan, "Earnings losses of displaced workers," *The American economic review*, 1993, pp. 685–709.

Johnson, Matthew S, Daniel Schwab, and Patrick Koval, "Legal protection against retaliatory firing improves workplace safety," *The Review of Economics and Statistics (forthcoming)*, 2022.

Jones, Jeffrey, "As Labor Day Turns 125, Union Approval Near 50-Year High," *Gallup*, 2019.

Kahn, Shulamit, "Evidence of nominal wage stickiness from microdata," *The American Economic Review*, 1997, *87* (5), 993–1008.

Kleiner, Morris M, "Intensity of management resistance: Understanding the decline of unionization in the private sector," *Journal of Labor Research*, 2001, *22* (3), 519–540.

Knepper, Matthew, "From the Fringe to the Fore: Labor Unions and Employee Compensation," *Review of Economics and Statistics*, 2020, *102* (1), 98–112.

Kochan, Thomas A, Duanyi Yang, William T Kimball, and Erin L Kelly, "Worker voice in America: Is there a gap between what workers expect and what they experience?," *ILR Review*, 2019, *72* (1), 3–38.

Lachowska, Marta, Alexandre Mas, and Stephen A Woodbury, "Sources of displaced workers' long-term earnings losses," Technical Report, National Bureau of Economic Research 2018.

Lee, David S and Alexandre Mas, "Long-run impacts of unions on firms: New evidence from financial markets, 1961–1999," *The Quarterly Journal of Economics*, 2012, *127* (1), 333–378.

McNicholas, Celine, Margaret Poydock, Julia Wolfe, Ben Zipperer, Gordon Lafer, and Lola Loustaunau, "Unlawful: US employers are charged with violating federal law in 41.5% of all union election campaigns," *Economic Policy Institute: Washington, DC, USA*, 2019.

Mishel, Lawrence, Lynn Rhinehart, and Lane Windham, "Explaining the erosion of private-sector unions," *Economic Policy Institute*, 2020.

National Employment Law Center, "Exposing Wage Theft Without Fear: States Must Protect Workers From Retaliation," *Report*, 2019.

Palmer, Annie, "Amazon illegally interfered in Alabama warehouse vote, union alleges," *CNBC*, 2022.

Pierce, Justin R and Peter K Schott, "The surprisingly swift decline of US manufacturing employment," *American Economic Review*, 2016, *106* (7), 1632–62.

_ **and** _ , "Trade liberalization and mortality: evidence from US counties," *American Economic Review: Insights*, 2020, *2* (1), 47–64.

Pissarides, Christopher A, "The unemployment volatility puzzle: Is wage stickiness the answer?," *Econometrica*, 2009, *77* (5), 1339–1369.

Rose, Nancy L, "Labor rent sharing and regulation: Evidence from the trucking industry," *Journal of Political Economy*, 1987, *95* (6), 1146–1178.

Schaller, Zachary, "The Decline of US Labor Unions: Import Competition and NLRB Elections," *Labor Studies Journal*, 2023, *48* (1), 5–34.

Sheflin, Neil, Leo Troy, and C. Timothy Koeller, "Structural Stability in Models of Union Growth," *The Quarterly Journal of Economics*, 1981, *96* (1), 77–88.

Silva, JMC Santos and Silvana Tenreyro, "The log of gravity," *The Review of Economics and statistics*, 2006, *88* (4), 641–658.

Slaughter, Matthew J, "Globalization and declining unionization in the United States," *Industrial Relations: A Journal of Economy and Society*, 2007, *46* (2), 329–346.

Stansbury, Anna, "Do US Firms Have an Incentive To Comply With the FLSA and the NLRA?," *Peterson Institute for International Economics Working Paper*, 2021, (21-9).

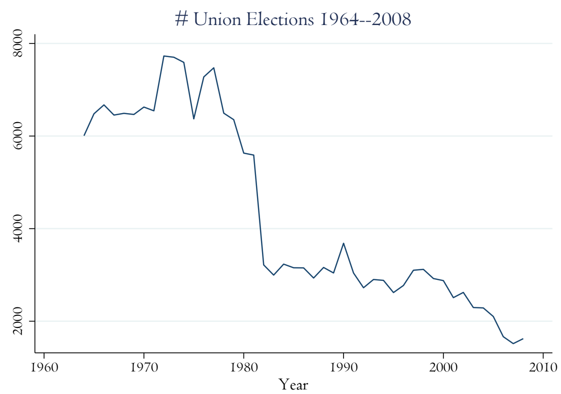
Tolbert, Charles M and Molly Sizer, "US commuting zones and labor market areas: A 1990 update," Technical Report 1996.

Wang, Sean and Samuel Young, "Unionization, Employer Opposition, and Establishment Closure," 2022.

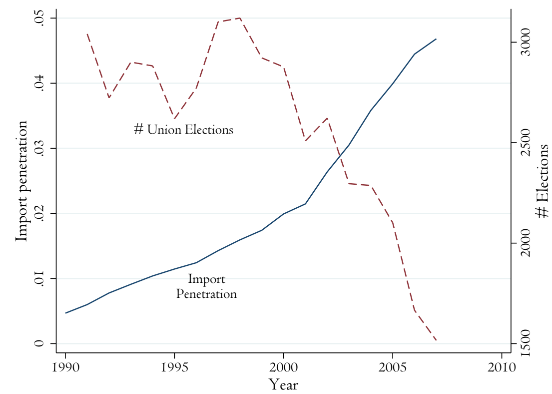
Wasi, Nada and Aaron Flaaen, "Record linkage using Stata: Preprocessing, linking, and reviewing utilities," *The Stata Journal*, 2015, *15* (3), 672–697.

7 Tables and Figures

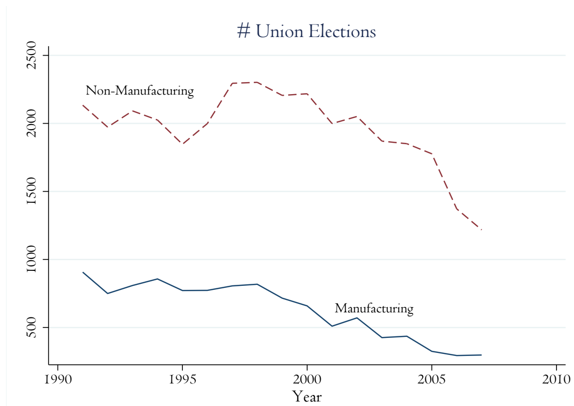
Figure 1: Annual Rates of Union Organizing Elections



(a) Number of certification elections per year, 1964–2008



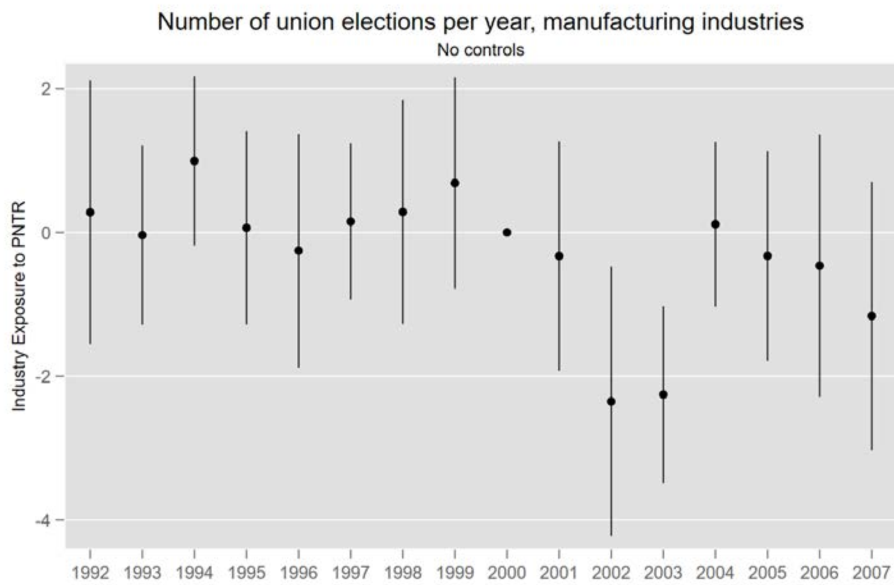
(b) Number of elections per year and annual Chinese import penetration, 1990–2008



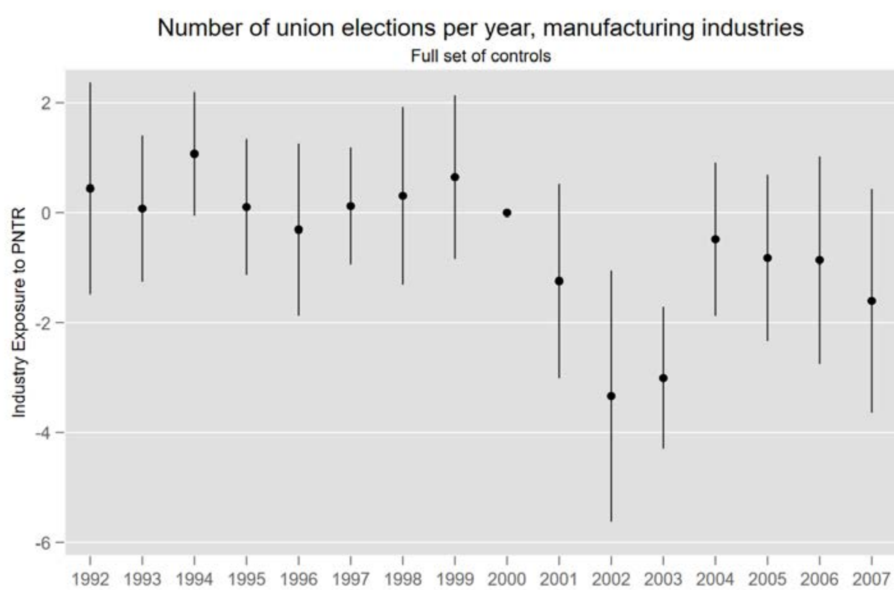
(c) Number of elections per year by sector, 1990–2008

Notes: Elections taking place in Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Figure 2: The Effect of Industry Exposure to Import Competition on the Number of Union Elections in Manufacturing Industries: Event Study Results



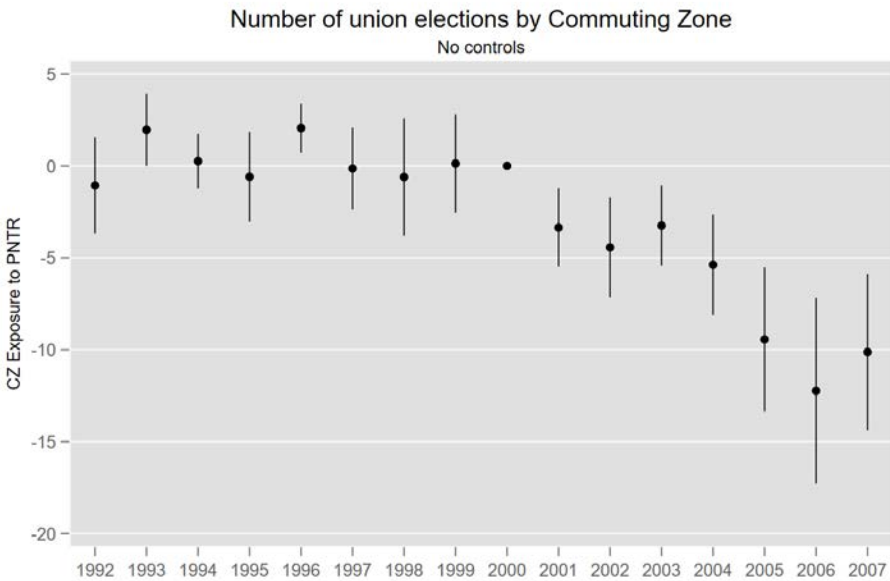
(a) Baseline model (no controls)



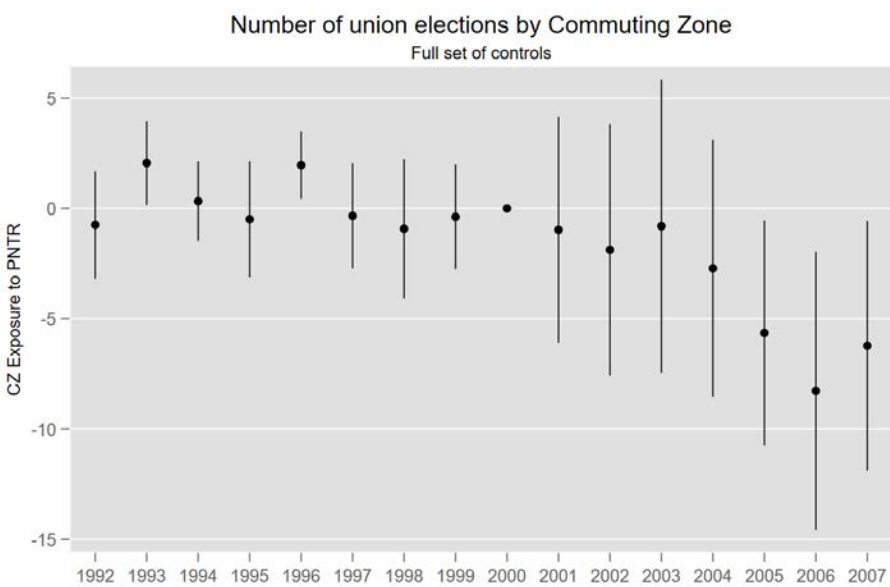
(b) Saturated model (all controls)

Notes: Figure displays θ_τ coefficients and corresponding 95 percent confidence interval from the Poisson regression specified in Equation 4. A unit of observation is an industry-year. Time-varying controls include NTR tariffs and MFA exposure; time-invariant controls (1989 unionization rates, capital and skill intensity, advance technology indicator, and contract intensity) are interacted with a post-PNTR indicator for years including and after 2001. Year fixed effects and industry family fixed effects are included. Standard errors are clustered at the industry family level. Regressions include an exposure unit for one-year lagged industry employment.

Figure 3: The Effect of Local Labor Market Exposure to Import Competition on the Number of Private Sector Union Elections: Event Study Results



(a) Baseline model (no controls)



(b) Saturated model (all controls)

Notes: Figure displays θ_τ coefficients and corresponding 95 percent confidence interval from the Poisson regression specified in Equation 6. A unit of observation is a CZ-year. Time-varying controls include the overall import tariff rate for industries within a CZ and the local exposure to MFA reduction; time-invariant controls (state-level unionization rate in 1989, percent of initial CZ workforce employed in manufacturing, changes in Chinese tariffs, and exposure to changes in Chinese subsidies) are interacted with a post-PNTR indicator for years including and after 2001. Year and CZ fixed effects are included. Standard errors are clustered at the state level. Regressions include an exposure unit for one-year lagged CZ employment.

Table 1: The Effect of Industry Exposure to Import Competition on Rate of Union Organizing Elections in the Manufacturing Sector

	(1)	(2)	(3)	(4)	(5)	(6)
	Elections	Elections	Elections	Elections	Elections	Wins
Dep Var:	Mfg	Mfg	Mfg	Mfg	Mfg	Mfg
Post x Industry's NTR Gap	-1.264***	-0.985**	-1.671***	-2.037***	-2.024***	-2.226***
	[0.385]	[0.438]	[0.472]	[0.515]	[0.511]	[0.852]
Post x ln(K/Emp)			-0.086	0.017	0.011	0.031
			[0.077]	[0.078]	[0.079]	[0.127]
Post x ln(NP/Emp)			0.463***	0.235*	0.226*	0.271
			[0.171]	[0.120]	[0.120]	[0.169]
Post x Contract Intensity				0.548*	0.550*	1.107***
				[0.285]	[0.289]	[0.419]
Post x Δ China Import Tariffs				0.107	0.142	0.976
				[0.702]	[0.685]	[0.853]
Post x Advanced Technology				0.338**	0.346**	0.002
				[0.169]	[0.170]	[0.240]
MFA Exposure				3,352.071*	3,362.740*	-429.247
				[2,020.000]	[1,991.759]	[4,390.696]
NTR				-1.156	-1.165	-3.282
				[3.692]	[3.682]	[5.126]
Post x 1989 Unionization Rate					0.002	-0.001
					[0.004]	[0.008]
Observations	4,692	4,692	4,692	4,692	4,692	4,199
Year FE	Y	Y	Y	Y	Y	Y
Fam50 FE	Y	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y	Y
# Fam50 Codes	276	276	276	276	276	247
Mean Dep Var	0.867	0.867	0.867	0.867	0.867	0.377
Implied effect 25th to 75th percentile	-0.138	-0.109	-0.178	-0.213	-0.211	-0.230
Implied Overall Effect of PNTR	-0.325	-0.266	-0.402	-0.462	-0.460	-0.490

Notes: The table reports results from the Poisson regression specified in Equation 3. The model is estimated on 293 manufacturing industries over the sample period 1991–2007, which reduces to 276 manufacturing industries after excluding the 17 industries for which the number of elections is zero across all years. The dependent variable is number of elections in an industry-year. The independent variable representing the effect of PNTR is the interaction of the NTR gap and a post-PNTR indicator. All regressions include industry and year fixed effects. Robust standard errors adjusted for clustering at the industry level are displayed below each coefficient. Models are weighted by 1990 industry employment. Regressions use one-year lagged industry employment as the exposure unit. Elections pertaining to employers located in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table 2: The Effect of Local Labor Market Exposure to Import Competition on Rate of Union Organizing Elections in all Sectors

	(1)	(2)	(3)	(4)	(5)	(6)
	Elections	Elections	Elections	Elections	Elections	Wins
Dep Var:	Total	Total	Total	Total	Total	Total
Post x CZ's NTR Gap	-6.636***	-5.908***	-4.623*	-5.057***	-3.697	-3.175
	[1.217]	[1.417]	[2.498]	[1.269]	[3.095]	[3.412]
Post x Mfg Employment in 1990			0.006		0.013	0.018
			[0.019]		[0.018]	[0.023]
MFA Exposure			0.406*		0.319	0.405
			[0.233]		[0.198]	[0.248]
Avg US Import NTR Tariff			-0.021		-0.086	0.022
			[0.278]		[0.268]	[0.362]
Post x Δ Chinese Tariffs			0.080		0.157**	0.212**
			[0.065]		[0.067]	[0.090]
Post x Δ Chinese Subsidies			0.030		-0.007	-0.031
			[0.044]		[0.033]	[0.052]
Post x 1989 Unionization Rate				0.019***	0.020***	0.016**
				[0.006]	[0.006]	[0.007]
Observations	10,455	10,455	10,455	10,455	10,455	9,673
Year FE	Y	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y	Y
Mean of Dep Var	4.159	4.159	4.159	4.159	4.159	2.403
Implied effect 25th to 75th percentile	-0.249	-0.225	-0.181	-0.196	-0.147	-0.128
Implied Overall Effect of PNTR	-0.659	-0.617	-0.531	-0.562	-0.455	-0.407

Notes: The table reports results from the Poisson regression specified in Equation 5. The model is estimated on 722 Commuting Zones (CZs) over the sample period 1991–2007. Of these 722 CZs, 107 have zero elections across all 17 years in our sample and are therefore excluded in the parameter estimation under the Poisson Pseudo-Maximum Likelihood estimator. The dependent variable is number of elections (or successful elections–that is, wins) in a CZ-year. The independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator with the weighted average NTR gap of a CZ's industries, with weights corresponding to initial industry employment shares. All regressions include CZ and year fixed effects. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. Models are weighted by 1990 CZ employment. Regressions use one-year lagged CZ employment as the exposure unit.

Table 3: The Effect of Industry Exposure to Import Competition on Union-Negotiated Wage Increases in the Manufacturing Sector

Dep Var:	(1) Log Wage Increase	(2) Log Wage Increase	(3) Log Wage Increase
Post x Industry's NTR Gap	-1.689***	-1.743***	-1.164**
	[0.546]	[0.605]	[0.460]
Post x ln(K/Emp)		0.078	0.041
		[0.099]	[0.100]
Post x ln(NP/Emp)		0.160	-0.000
		[0.137]	[0.135]
Post x Contract Intensity			0.131
			[0.284]
Post x Δ China Import Tariffs			0.227
			[0.502]
Post x Advanced Technology			0.074
			[0.163]
MFA Exposure			-64,354.623***
			[11,426.040]
NTR			1.404
			[1.744]
Observations	1,223	1,223	1,223
R^2	0.596	0.598	0.610
Year FE	Y	Y	Y
Fam50 FE	Y	Y	Y
# Fam50 Codes	165	165	165
# Unique Contracts	1223	1223	1223
Mean Wage Increase	3.565	3.565	3.565
Implied effect 25th to 75th percentile	-0.253	-0.260	-0.182

Notes: The table reports results from the OLS regression specified in Equation 8. The dependent variable is the log of the negotiated wage increase (in percent terms) in a union contract settlement. The independent variable representing the effect of PNTR is the interaction of the industry's NTR gap and a post-PNTR indicator. The sample is drawn from contracts in establishments in 293 manufacturing industries for which we observe NTR gaps and other controls. Data span 1992-2007. Robust standard errors adjusted for clustering at the industry family level are displayed below each coefficient. Models are weighted by the number of employees each contract covers. Contracts in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table 4: The Effect of Local Labor Market Exposure to Import Competition on Union-Negotiated Wage Increases

	(1)	(2)	(3)	(4)
	Log Wage	Log Wage	Log Wage	Log Wage
Dep Var:	Increase	Increase	Increase	Increase
Post x CZ's NTR Gap	-0.811	-1.023	-1.630	-1.564
	[0.801]	[2.009]	[1.843]	[1.746]
Post x Mfg Employment in 1990		0.001	0.008	0.007
		[0.011]	[0.014]	[0.013]
MFA Exposure			0.057	0.050
			[0.146]	[0.125]
Avg US Import NTR Tariff			-0.538	-0.503
			[0.411]	[0.407]
Post x Δ Chinese Tariffs			0.058	0.043
			[0.111]	[0.098]
Post x Δ Chinese Subsidies			-0.006	-0.007
			[0.042]	[0.040]
Post x 1989 Unionization Rate				-0.012**
				[0.006]
Observations	6,760	6,760	6,760	6,760
R^2	0.175	0.175	0.177	0.178
Year FE	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y
# Commuting Zones	308	308	308	308
Mean Wage Increase	1.155	1.155	1.155	1.155
Implied effect 25th to 75th percentile	-0.060	-0.075	-0.116	-0.112

Notes: The table reports results from the OLS regression specified in Equation 9. The dependent variable is the log of the negotiated wage increase (in percent terms) in a union contract settlement. The independent variable representing the effect of local labor market exposure to PNTR is the interaction of a post-PNTR indicator with the NTR gap of the CZ in which the contract was negotiated, where a CZ's NTR gap is as defined in Equation 2. The sample is drawn from contracts that covered at most one CZ. Data span 1992-2007. Robust standard errors adjusted for clustering at the state family level are displayed below each coefficient. Models are weighted by the number of employees each contract covers. Contracts in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table 5: The Effect of Local Labor Market Exposure to Import Competition on the Unemployment Rate and the Market Wage

Panel A: **Dep Var = Unemployment Rate**

	(1)	(2)	(3)	(4)
Post x CZ's NTR Gap	0.072***	0.072***	0.085**	0.048
	[0.027]	[0.025]	[0.037]	[0.038]
Observations	8,464	8,464	8,464	8,464
R^2	0.825	0.825	0.826	0.827
1989 Unionization x Post	N	Y	Y	Y
PS Controls	N	N	Y	Y
1990 Share Mfg x Post	N	N	N	Y

Panel B: **Dep Var = Log Market Earnings**

	(1)	(2)	(3)	(4)
Post x CZ's NTR Gap	-0.417***	-0.414***	-0.183	0.041
	[0.098]	[0.096]	[0.126]	[0.130]
Observations	526,899	526,899	526,899	526,899
R^2	0.950	0.950	0.950	0.950
1989 Unionization x Post	N	Y	Y	Y
PS Controls	N	N	Y	Y
1990 Share Mfg x Post	N	N	N	Y

Notes: In Panel A, a unit of observation is a CZ–year and the dependent variable (obtained from the Local Area Unemployment Statistics from the Bureau of Labor Statistics) is the CZ-level unemployment. In Panel B, a unit of observation is a CZ–year–quarter–sex–age-group and the dependent variable is log average earnings of that observation (obtained from the Quarterly Workforce Indicators [QWI]). In both panels, the sample spans 1991–2007 and the independent variable representing the effect of local labor market exposure to PNTR is the interaction of a post-PNTR indicator with a CZ's NTR gap, where a CZ's NTR gap is as defined in Equation 2. All regressions include year and CZ fixed effects, and regressions in Panel B additionally include age group, sex, and quarter fixed effects. Robust standard errors in parentheses are clustered at the state level.

Table 6: State-level Protections Against Retaliation Attenuate the Effect of Local Labor Market Import Exposure on Union Organizing

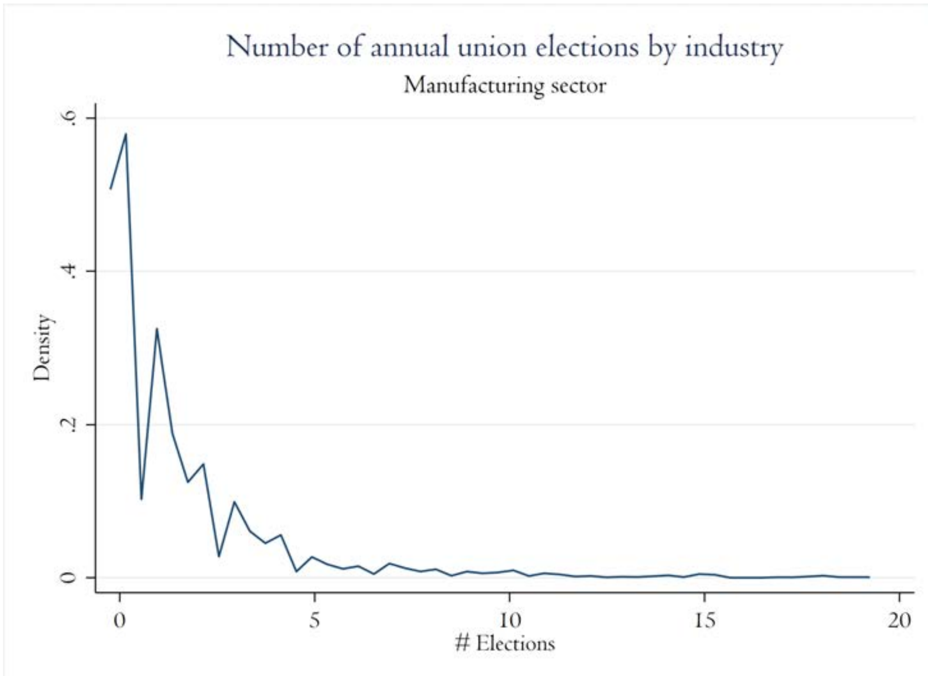
	(1)	(2)	(3)
Dep Var:	# Elections	# Elections	# Elections
Post x CZ's NTR Gap	-4.097	-3.690	-4.433
	[2.789]	[2.532]	[2.932]
Post x CZ's NTR Gap x Anti-Retaliation Protection	4.068*		
	[2.383]		
Post x CZ's NTR Gap x Good-Faith Exception		7.506***	
		[1.813]	
Post x CZ's NTR Gap x Anti-Retaliation or Good-Faith			5.312**
			[2.325]
Observations	10,455	10,455	10,455
Year FE	Y	Y	Y
CZ FE	Y	Y	Y
Exposure Included	Y	Y	Y
Extra Controls	Y	Y	Y
# Commuting Zones	615	615	615
Mean # Elections	4.159	4.159	4.159
Mean of Anti-Retaliation Protection	0.413		
Mean of Good-Faith Exception		0.197	
Mean of Either Anti-Retaliation or Good Faith			0.519

Notes: The table reports results from the Poisson regression specified in Equation 5. The model is estimated on 722 Commuting Zones (CZs) over the sample period 1991–2007. The dependent variable is number of elections in a CZ-year. The independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator with the weighted average NTR gap of a CZ's industries, with weights corresponding to initial industry employment shares. All regressions include CZ and year fixed effects. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. Models are weighted by 1990 CZ employment. Regressions use one-year lagged CZ employment as the exposure unit.

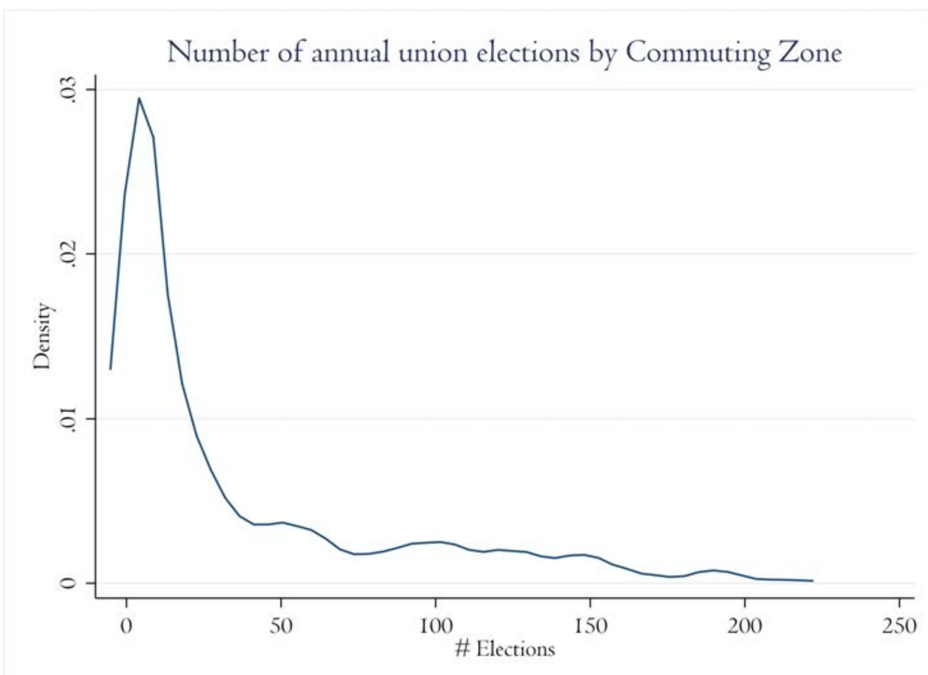
Appendix

A Appendix Tables and Figures

Figure A1: Distribution of the Number of Annual Union Elections at the Industry– and Commuting-Zone–level



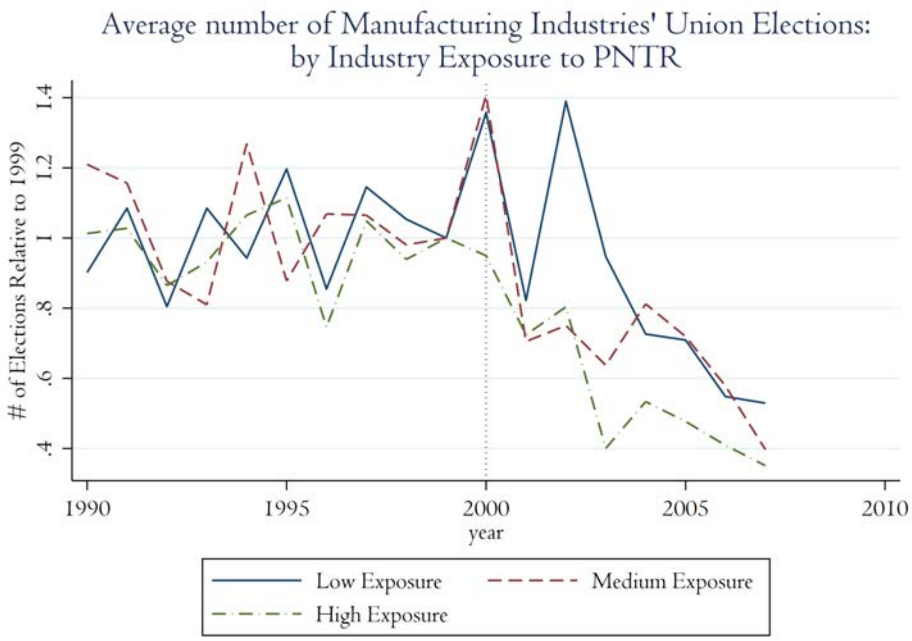
(a) Industry-level



(b) CZ-level

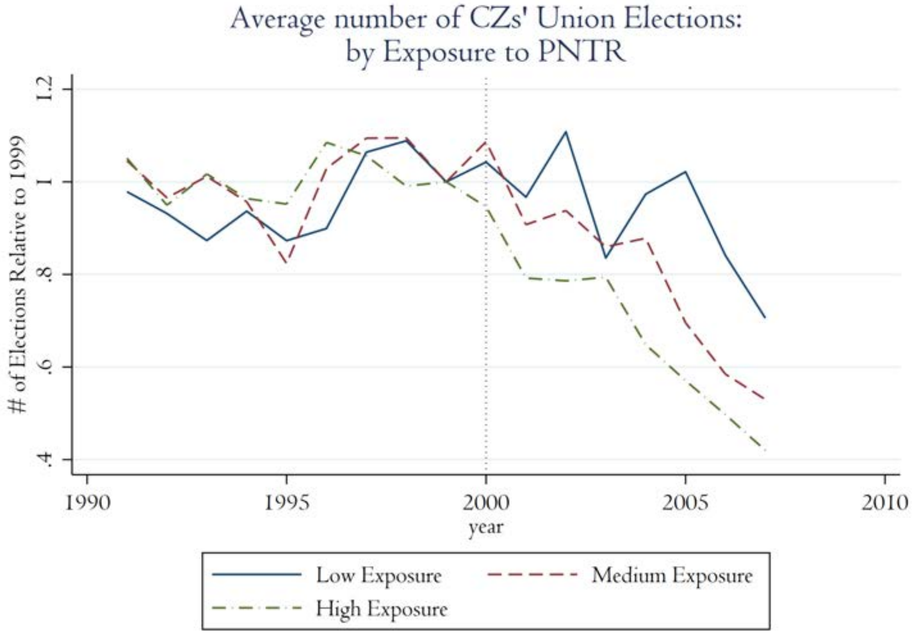
Notes: Figure displays kernel density plots that are weighted by industry-level and CZ-level 1990 employment, in panels (a) and (b) respectively.

Figure A2: The Relationship Between Manufacturing Industries' Exposure to PNTR and Annual Union Elections: Descriptive Evidence



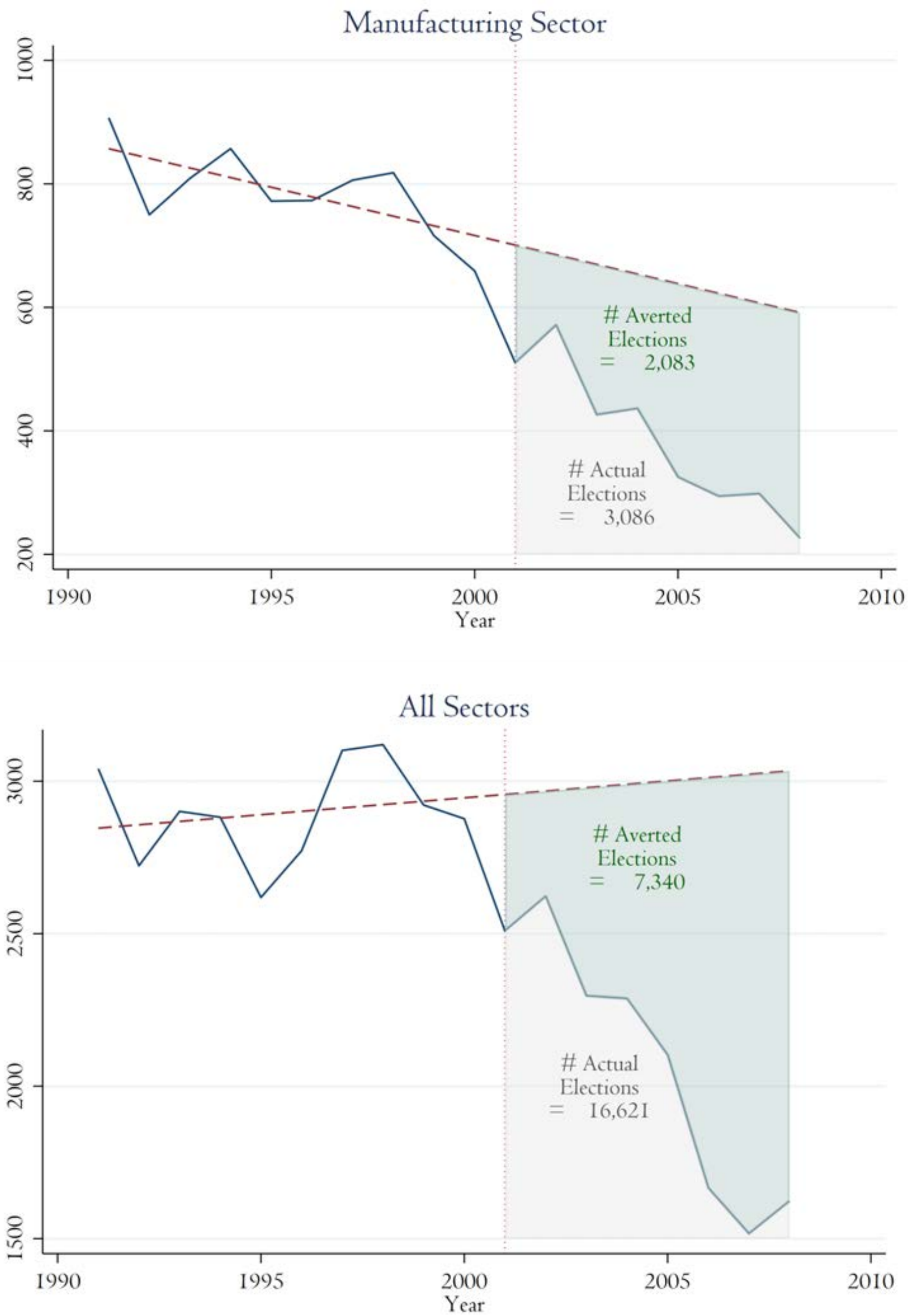
Notes: The figure shows the average annual number of union elections for three groups of manufacturing industries, where the groups correspond to terciles of direct exposure to PNTR, as defined by their NTR_{gap} in Equation 1.

Figure A3: The Relationship Between Commuting Zones' Exposure to PNTR and Annual Union Elections: Descriptive Evidence



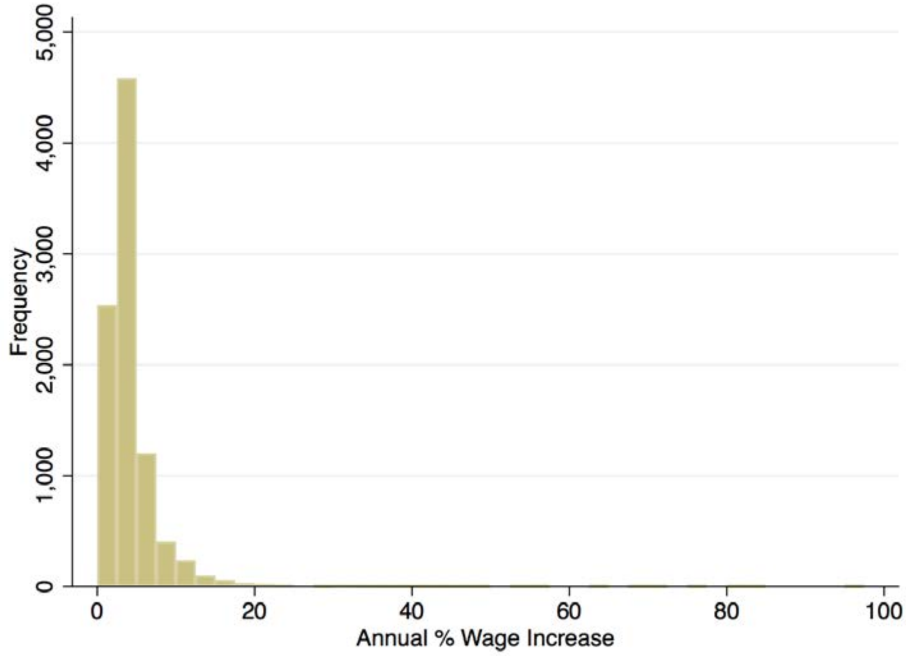
Notes: The figure shows the average annual number of union elections for three groups of Commuting Zones, where the groups correspond to terciles of indirect exposure to PNTR, as defined by their NTR_{gap} in Equation 2.

Figure A4: The Number of Averted Union Elections Due to the Post-2000 Trend Break in Organizing



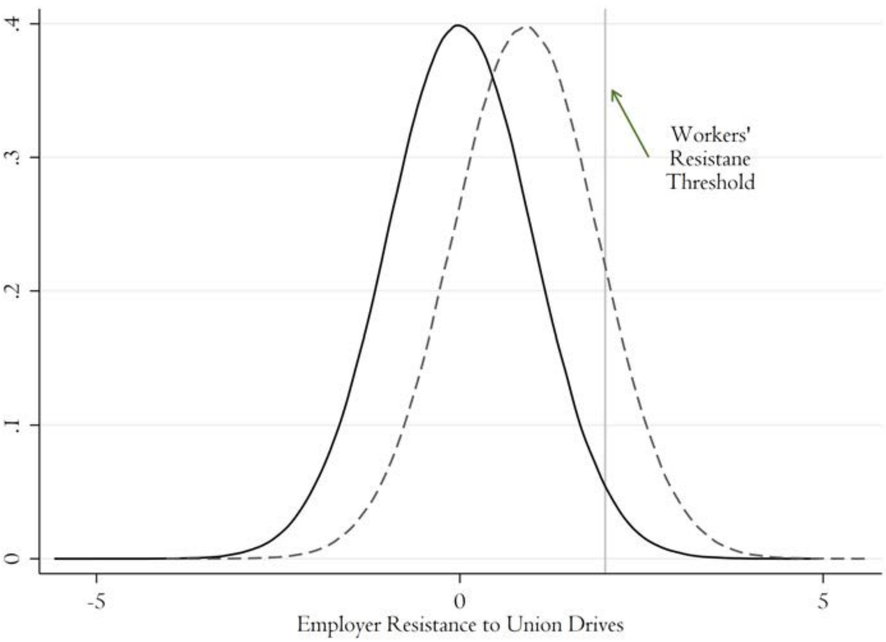
Notes: In both figures, the solid blue line is the number of NLRB-certified union elections that occurred each year. The dashed red line is the predicted number of annual elections that would have occurred each year 2001–2008, based on a linear time trend fitted on observed elections over the period 1991–2000.

Figure A5: Distribution of Negotiated Wage Increases, 1991-2007

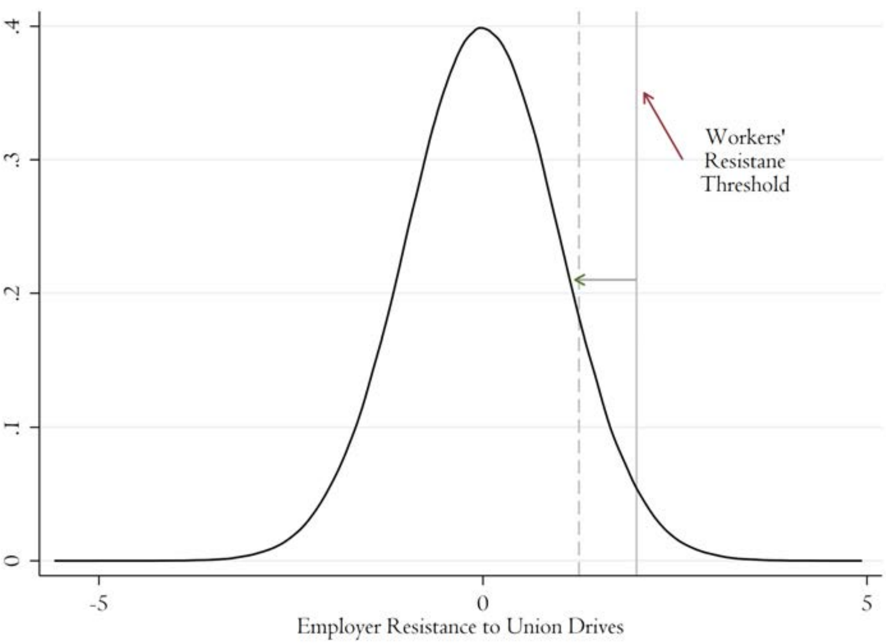


Notes: Distribution of all % hourly wage increases observed in private contract settlements between 1991 and 2007 in the BNA database. Wage increases include lump-sum payments such as bonuses; see Appendix for further details on methodology.

Figure A6: Using the Frequency of Challenged Ballots Conditional on an Election Occurring to Infer the Effect of Import Exposure on Underlying Employer Resistance to Union Drives



(a) A rightward shift in the distribution of employer resistance



(b) A leftward shift in workers' "resistance threshold"

Notes: The figures plot hypothetical distributions of employer resistance to union drives, and workers "resistance threshold:" a level of resistance above which they will definitely not attempt to hold a union election.

Table A1: Union Election Summary Statistics

	(1)	(2)	(3)
	1991-2000	2001-2007	All Years
Entire U.S.			
# of Elections Overall per Year	2,853	2,136	2,558
Share of Mfg Elections per Year (%)	0.26	0.18	0.23
Share of Elections Won per Year (%)	0.50	0.60	0.54
Mean # of Eligible Employees in Elections	69.30	73.57	71.06
Industry-level variables (Observation = industry-year for 293 Manufacturing Families)			
# of Elections per Industry Family per Year	0.98	0.58	0.82
	(1.79)	(1.32)	(1.63)
Industry Level NTR Gap	0.32	0.32	0.32
	(0.15)	(0.15)	(0.15)
Local labor market variables (Observation = Commuting Zone [CZ]-year for 722 CZs)			
# of Elections per CZ per Year	3.95	2.96	3.54
	(13.21)	(11.62)	(12.59)
CZ-level NTR Gap	0.18	0.18	0.18
	(0.06)	(0.06)	(0.06)

Notes: The table reports variable means, as well as standard deviations in parentheses. Elections taking place in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not reported.

Table A2: The Effect of Industry Exposure to Import Competition on Number of Employees Organized for Union Elections in the Manufacturing Sector

Dep Var:	(1) Employees Mfg	(2) Employees Mfg	(3) Employees Mfg	(4) Employees Mfg	(5) Employees Mfg
Post x Industry's NTR Gap	-2.491***	-1.965***	-3.291***	-3.664***	-3.649***
	[0.567]	[0.650]	[0.948]	[1.095]	[1.060]
Post x ln(K/Emp)			-0.280*	-0.163	-0.170
			[0.145]	[0.166]	[0.166]
Post x ln(NP/Emp)			0.730**	0.438*	0.432*
			[0.329]	[0.264]	[0.258]
Post x Contract Intensity				0.633	0.629
				[0.536]	[0.531]
Post x Δ China Import Tariffs				-0.632	-0.609
				[1.114]	[1.121]
Post x Advanced Technology				0.288	0.293
				[0.269]	[0.271]
MFA Exposure				1,384.694	1,426.885
				[6,832.427]	[6,808.647]
NTR				-3.900	-3.891
				[7.153]	[7.124]
Post x 1989 Unionization Rate					0.001
					[0.008]
Observations	4,692	4,692	4,692	4,692	4,692
Year FE	Y	Y	Y	Y	Y
Fam50 FE	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y
# Fam50 Codes	276	276	276	276	276
Mean Dep Var	89.25	89.25	89.25	89.25	89.25
Implied effect 25th to 75th percentile	-0.253	-0.206	-0.320	-0.349	-0.348
Implied Overall Effect of PNTR	-0.527	-0.451	-0.620	-0.655	-0.654

Notes: The table reports results from the Poisson regression specified in Equation 3. The model is estimated on the 276 manufacturing industries for which we elections over the sample period 1991–2007. The dependent variable is the number of employees eligible to vote in certification elections held in an industry-year. The independent variable representing the effect of PNTR is the interaction of the NTR gap and a post-PNTR indicator. All regressions include industry and year fixed effects. Robust standard errors adjusted for clustering at the industry level are displayed below each coefficient. Models are weighted by 1990 industry employment. Regressions use one-year lagged industry employment as the exposure unit.

Table A3: The Effect of Local Labor Market Exposure to Import Competition on Number of Employees Organized for Union Elections

	(1)	(2)	(3)	(4)	(5)
	Employees	Employees	Employees	Employees	Employees
Dep Var:	Total	Total	Total	Total	Total
Post x CZ's NTR Gap	-4.213***	-3.127***	-1.546	-3.143***	-0.835
	[1.058]	[0.943]	[2.128]	[1.024]	[2.482]
Post x Mfg Employment in 1990			-0.004		-0.004
			[0.017]		[0.020]
MFA Exposure			0.143*		0.134
			[0.084]		[0.083]
Avg US Import NTR Tariff			-0.132		-0.151
			[0.277]		[0.281]
Post x Δ Chinese Tariffs			0.076		0.114*
			[0.063]		[0.060]
Post x Δ Chinese Subsidies			-0.051		-0.069
			[0.044]		[0.045]
Post x 1989 Unionization Rate				0.009*	0.012**
				[0.005]	[0.005]
Observations	10,455	10,455	10,455	10,455	10,455
Year FE	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y
Mean of Dep Var	255.9	255.9	255.9	255.9	255.9
Implied effect 25th to 75th percentile	-0.166	-0.126	-0.065	-0.127	-0.035
Implied Overall Effect of PNTR	-0.499	-0.403	-0.227	-0.404	-0.130

Notes: The table reports results from the Poisson regression specified in Equation 5. The model is estimated on 615 Commuting Zones (CZs) with non-missing values for employees who are eligible vote in an election (missing for CZs where zero elections are observed) over the sample period 1991–2007. The dependent variable is the number of employees eligible to vote in certification elections held in a CZ-year. The independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator with the weighted average NTR gap of a CZ's industries, with weights corresponding to initial industry employment shares. All regressions include CZ and year fixed effects. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. Models are weighted by 1990 CZ employment. Regressions use one-year lagged CZ employment as the exposure unit.

Table A4: The Effect of Local Labor Market Exposure to Import Competition on Rate of Union Organizing Elections, by Sector

	(1)	(2)
	Elections	Elections
Dep Var:	Manufacturing	Non-Manufacturing
Post x CZ's NTR Gap	-7.628***	-5.854***
	[1.749]	[1.184]
Observations	9,112	9,826
Year FE	Y	Y
CZ FE	Y	Y
Mean # elections	1.165	3.345
Implied effect 25th to 75th percentile	-0.440	-0.359
Implied Overall Effect of PNTR	-0.708	-0.614

Notes: The table reports results from the Poisson regression specified in Equation 5 separately for two sectors: manufacturing and non-manufacturing based on SIC and NAICS industry codes. The model is estimated on 722 Commuting Zones (CZs) over the sample period 1991–2007. The dependent variable is number of elections in a CZ-sector-year. Of these 722 CZs, 186 have zero manufacturing elections and 144 CZs have zero non-manufacturing elections across all 17 years in our sample, thereby reducing the number of CZs included in columns 1 and 2 by 186 and 144, respectively. The independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator with the weighted average NTR gap of a CZ's industries, with weights corresponding to initial industry employment shares. All regressions include CZ and year fixed effects. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. Models are weighted by 1990 CZ employment.

Table A5: Union Contract Settlements: Summary Statistics

	(1) 1992-2000	(2) 2001-2007	(3) All Years
All Industries			
% Wage Increase	5.00	4.00	4.42
	(24.10)	(7.64)	(16.65)
Contract Duration	3.41	3.27	3.33
	(1.22)	(1.14)	(1.18)
# of Employees	1740.82	1497.74	1599.49
	(3191.01)	(3084.21)	(3131.46)
Observations	3291	4571	7862
Manufacturing			
% Wage Increase	3.65	3.49	3.58
	(4.23)	(4.41)	(4.31)
Contract Duration	3.61	3.58	3.59
	(1.27)	(1.13)	(1.21)
# of Employees	1056.06	961.22	1015.08
	(1739.25)	(1875.70)	(1799.69)
Observations	1292	983	2275

Notes: The table reports variable means and standard deviations in parentheses. Contract data from the Bloomberg Bureau of National Affairs (BNA) Labor Plus database. Contracts negotiated in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table A6: Effect of Industry Exposure to Chinese Imports on Union-Negotiated Wage Increases: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
			Contract		
Dep Var:	Baseline	Unweighted	Length FE	Region FE	Dyad FE
Post x Industry's NTR Gap	-1.689***	-0.585*	-1.823***	-1.632***	-1.308
	[0.546]	[0.315]	[0.574]	[0.553]	[1.022]
Observations	1,223	1,223	1,222	1,223	501
R^2	0.596	0.329	0.607	0.614	0.757
Year FE	Y	Y	Y	Y	Y
Fam50 FE	Y	Y	Y	Y	N
Dyad FE	N	N	N	N	Y
Spec	OLS	OLS	OLS	OLS	OLS
# Fam50 Codes	165	165	165	165	106
# Unique Contracts	1223	1223	1222	1223	501
Mean Wage Increase	3.565	3.565	3.567	3.565	3.858
Implied effect 25th to 75th percentile	-0.253	-0.0960	-0.270	-0.245	-0.202

Notes: The table reports results from the OLS regression specified in Equation 8. See Section 5.2 for details on the modification applied in each column. The dependent variable is the log of the negotiated wage increase (in percent terms) in a union contract settlement. The independent variable representing the effect of PNTR is the interaction of the industry's NTR gap and a post-PNTR indicator. The sample is drawn from contracts in establishments in 165 manufacturing industries for which we observe contracts in at least two years and NTR gaps and other controls. Data span 1992-2007. Robust standard errors adjusted for clustering at the industry family level are displayed below each coefficient. Models are weighted by the number of employees each contract covers. Contracts in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table A7: Effect of Local Labor Market Exposure to Chinese Imports on Union-Negotiated Wage Increases: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
			Contract		
Dep Var:	Baseline	Unweighted	Length FE	Industry FE	Dyad FE
Post x CZ's NTR Gap	-0.811	-0.848	-0.870	-0.440	0.705
	[0.801]	[0.690]	[0.835]	[0.830]	[1.806]
Observations	6,760	6,760	6,756	6,760	2,852
R^2	0.175	0.095	0.194	0.246	0.588
Year FE	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	N
Dyad FE	N	N	N	N	Y
Spec	OLS	OLS	OLS	OLS	OLS
# Commuting Zones	308	308	308	308	292
Mean Wage Increase	1.155	1.155	1.155	1.155	1.165
Implied effect 25th to 75th percentile	-0.060	-0.062	-0.064	-0.033	0.055

Notes: The table reports results from the OLS regression specified in Equation 9. See Section 5.2 for details on the modification applied in each column. The dependent variable is the log of the negotiated wage increase (in percent terms) in a union contract settlement. The independent variable representing the effect of local labor market exposure to PNTR is the interaction of a post-PNTR indicator with the NTR gap of the CZ in which the contract was negotiated, where a CZ's NTR gap is as defined in Equation 2. The sample is drawn from contracts that covered at most one CZ. Data span 1992-2007. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. All models except Column 2 are weighted by the number of employees each contract covers. Contracts in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table A8: Effect of Local Labor Market Exposure to Chinese Imports on Union-Negotiated Wage Increases: Expanding Sample to Include Contracts Covering Multiple Commuting Zones

	(1)	(2)	(3)	(4)	(5)	(6)
	Num CZs	Num CZs	Num CZs	Num CZs	Num CZs	Num CZs
Dep Var:	1	2	4	6	8	10
Post x CZ's NTR Gap	-0.811	-0.663	-0.564	-0.452	-0.646	-0.629
	[0.801]	[0.792]	[0.841]	[0.813]	[0.822]	[0.785]
Observations	6,760	7,066	7,704	8,492	8,809	9,115
R^2	0.175	0.170	0.178	0.171	0.167	0.166
Year FE	Y	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	Y	Y
# Commuting Zones	308	313	330	341	348	375
Mean Wage Increase	1.155	1.160	1.171	1.163	1.169	1.172
Implied effect 25th to 75th percentile	-0.060	-0.049	-0.042	-0.034	-0.048	-0.047

Notes: The table reports results from the OLS regression specified in Equation 9. In Column 1, the sample is drawn from contracts that covered at most one CZ. In each column thereafter, the sample is expanded to include contracts covering at most the number of CZs specified in the column title. The dependent variable is the log of the negotiated wage increase (in percent terms) in a union contract settlement. The independent variable representing the effect of local labor market exposure to PNTR is the interaction of a post-PNTR indicator with the NTR gap of the CZ in which the contract was negotiated, where a CZ's NTR gap is as defined in Equation 2. Data span 1992-2007. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. All models are weighted by the number of employees each contract covers. Contracts in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not included.

Table A9: The Effect of Industry Exposure to Import Competition on the Percent of Elections with Challenged Ballots

Dep Var:	(1) Challenges Mfg	(2) Challenges Mfg	(3) Challenges Mfg	(4) Challenges Mfg	(5) Challenges Mfg
Post x Industry's NTR Gap	0.182	0.182	0.328*	0.344*	0.324*
	[0.183]	[0.183]	[0.193]	[0.191]	[0.180]
Post x ln(K/Emp)			0.015	-0.009	-0.003
			[0.037]	[0.042]	[0.045]
Post x ln(NP/Emp)			-0.154***	-0.089	-0.090
			[0.058]	[0.067]	[0.065]
Post x Contract Intensity				-0.119	-0.110
				[0.125]	[0.121]
Post x Δ China Import Tariffs				0.193	0.174
				[0.160]	[0.163]
Post x Advanced Technology				-0.057	-0.061
				[0.080]	[0.077]
MFA Exposure				1,111.855***	1,103.240***
				[423.156]	[406.233]
NTR				0.388	0.398
				[0.976]	[0.975]
Post x 1989 Unionization Rate					-0.001
					[0.003]
Observations	1,933	1,933	1,933	1,933	1,933
R^2	0.198	0.198	0.204	0.207	0.207
Year FE	Y	Y	Y	Y	Y
Fam50 FE	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y
# Fam50 Codes	250	250	250	250	250
Mean Dep Var	0.489	0.489	0.489	0.489	0.489
Implied effect 25th to 75th percentile	0.044	0.044	0.079	0.083	0.078
Implied Overall Effect of PNTR	0.120	0.120	0.216	0.227	0.213

Notes: The table reports results from OLS regressions corresponding to Equation 3. The model is estimated on 250 manufacturing industries over the sample period 1991–2007 with non-missing values for challenged ballots. The dependent variable is the fraction of elections held in an industry-year in which at least one ballot was challenged. The independent variable representing the effect of PNTR is the interaction of the NTR gap and a post-PNTR indicator. All regressions include industry and year fixed effects. Robust standard errors adjusted for clustering at the industry level are displayed below each coefficient. Models are weighted by 1990 industry employment. Regressions use one-year lagged industry employment as the exposure unit.

Table A10: The Effect of Local Labor Market Exposure to Import Competition on the Percent of Elections with Challenged Ballots

Dep Var:	(1) Challenges Total	(2) Challenges Total	(3) Challenges Total	(4) Challenges Total	(5) Challenges Total
Post x CZ's NTR Gap	-0.228	-0.228	-0.564	-0.232	-0.542
	[0.162]	[0.162]	[0.349]	[0.154]	[0.388]
Post x Mfg Employment in 1990			0.001		0.001
			[0.002]		[0.002]
MFA Exposure			0.029		0.030
			[0.020]		[0.020]
Avg US Import NTR Tariff			0.017		0.016
			[0.042]		[0.041]
Post x Δ Chinese Tariffs			-0.002		-0.002
			[0.008]		[0.008]
Post x Δ Chinese Subsidies			0.001		0.000
			[0.007]		[0.007]
Post x 1989 Unionization Rate				0.000	0.000
				[0.001]	[0.001]
Observations	5,757	5,757	5,757	5,757	5,757
R^2	0.130	0.130	0.131	0.130	0.131
Year FE	Y	Y	Y	Y	Y
CZ FE	Y	Y	Y	Y	Y
Exposure included	N	Y	Y	Y	Y
Mean of Dep Var	0.425	0.425	0.425	0.425	0.425
Implied effect 25th to 75th percentile	-0.023	-0.023	-0.057	-0.024	-0.055
Implied Overall Effect of PNTR	-0.090	-0.090	-0.222	-0.092	-0.214

Notes: The table reports results from the OLS regression corresponding to Equation 5. The model is estimated on 563 Commuting Zones (CZs) over the sample period 1991–2007. The dependent variable is the fraction of elections held in an industry-year in which at least one ballot was challenged. The independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator with the weighted average NTR gap of a CZ's industries, with weights corresponding to initial industry employment shares. All regressions include CZ and year fixed effects. Robust standard errors adjusted for clustering at the state level are displayed below each coefficient. Models are weighted by 1990 CZ employment. Regressions use one-year lagged CZ employment as the exposure unit.

B More Details on Constructing our Data on Union Elections and on Union Contract Settlements

B1 Union Certification Elections

Identifying Industry Codes in the Union Certification Elections Data

The data on NLRB-certified union certification elections only includes the broad sector of the affected establishment. To measure industry-level exposure of the establishment tied to an election, which is roughly at the level of industry 4-digit SIC codes, we link the election data to the National Establishment Time Series (NETS) database using fuzzy matching methods.[30] NETS is an annual panel data set extracted from Dun & Bradstreet data that seeks to include all establishments ever in operation since 1990. From NETS, we obtain each establishment's Data Universal Numbering System (DUNS) number, a unique establishment identifier. For elections for which we are unable to fuzzy match to NETS, research assistants manually obtain the DUNS number of the election's associated establishment.[31] We use this DUNS number to obtain the associated 4-digit SIC code from NETS. Finally, for the small remainder of elections for which research assistants cannot find a DUNS number, they obtain the establishment's 4-digit SIC code from a combination of the LexisNexis database, www.manta.com, www.mailinglists.com, and www.siccode.com. We successfully link 90% of elections in the elections dataset to either a DUNS number, 6-digit NAICS code, or 4-digit SIC code obtained through the other means. We use crosswalks from Pierce and Schott (2016) to map these elections coded with NAICS and SIC codes to the 772 unique "family" codes used in our analysis.[32]

B2 Contract Settlements Between Unions and Employers

Identifying Repeat Observations of the Same Employer-Union Dyad

We identify repeat contracts of the same employer-union dyad as follows. First, we manually clean the employer names to standardize different spellings and naming conventions across contracts and ensure the same establishments are not erroneously treated as separate entities. Second, we treat establishments that re-branded, changed names after going public,

[30]Specifically, we used the Stata package reclink2 (Wasi and Flaaen, 2015), and we used multiple rounds of matching on employer name, address, city, state, and 2-digit SIC or NAICS industry code.

[31]This information was from www.dnb.com

[32]One concern we had was that the several research assistants who performed the manual linking might not be consistent in their approach. To assess this concern, we drew a random sample of 100 elections and had multiple research assistants link that same subset. Research assistants arrived at an identical industry classification for 94% of elections in this subset, indicating that lack of consistency was not in fact a practical concern.

or were otherwise acquired by another company as the same employer in an employer-union dyad.[33] Finally, we eliminate duplicate contracts.[34]

Standardization of Negotiated Wage Increases

Here, we provide further details on how we construct our variable measuring the wage increase negotiated in a union contract settlement, using the textual summaries in the Bloomberg BNA database.

To process the text in the contracts, we first trimmed the clauses to preserve only the sections with numeric information. Next, we split the contract clause into two components. The first component is the "main" clause that corresponds to annual increases in the baseline wage expressed either as a percentage or dollar increase in hourly, weekly, monthly, or yearly pay. We parsed this first component from the text by searching for indexed years such as "1st," "2nd," and "3rd"; keywords such as "initially"; specific years such as "2004"; or comma-separated sequential values. The second component corresponds to one-time, lump-sum payments. We parsed this component using keywords such as "supplemental," "incentive," "lump-sum," "bonus," "cash payment," "merit," and "premium."[35]

After parsing the textual information from the contracts, we constructed a variable measuring the wage increase negotiated in a contract that is the sum of two components: a main wage increase and any remaining wage increases in the form of bonuses or other lump-sum payments.

When a wage increase is provided as a range, we take the midpoint of the range. For contracts that span multiple years, if the value assigned to one of the years is blank we fill it in by extending the value of the previous year. When increases are assigned to a specific date in MM-DD-YY format, we convert the date to a year indexed relative to the start year of the contract and do not consider the month and date of payment.

For the first component, wage increases in the settlements are reported sometimes as dollar increases and other times as percentage increases; increases are also reported over various spans including hourly, weekly, monthly, or annual wages. We standardize these

[33] We manually look up companies for any IPOs, re-brandings, or acquisitions and mergers and adjust the names to reflect the most recent naming convention.

[34] In the data, we observe 122 contracts that are each exact duplicates of another contract. Furthermore, we eliminate 68 contracts that are identical to another contract except for minor punctuation and wording differences. Finally, we observe 17 contracts that are identical in employer, union, negotiation year, location, number and type of workers, and "main" wage increase described below but do not include bonus payments or lump-sum payments. We treat these contracts as erroneously missing the lump-sum payments and omit them from our analysis.

[35] We also sought to extract only payments that are guaranteed to current employees. As such, we omit numbers associated with keywords in the contract such as "future new hire rates," "profit sharing gains," "payments conditional on accidents or safety incidents," "holiday or weekend premium," "graduate school reimbursements," or "seniority-based payments." We also restrict our analysis to extract only full-time rates and discard part-time rates.

reports to an average annual percent increase relative to the baseline hourly wage. For the second component, we first convert the annual dollar amounts to an hourly wage increase assuming a 40-hour workweek and 52 working weeks per year, and we then convert these hourly increases to percentage increases relative to the baseline hourly wages. Since most contracts do not provide information on the original hourly wages negotiated in prior contracts, we create contract-specific baseline wages using the average hourly wages for union members in manufacturing or non-manufacturing by year as reported in the CPS Merged Outgoing Rotation Groups (MORG) data set.

To arrive at the final wage increase value, we take the mean percentage wage increase of each of our two components over the duration of the contract, and we then sum up the two means.

One example is illustrative. One clause in our data, a 5-year contract that begins in 1995 associated with a non-manufacturing establishment, reads: "None initially, 2nd yr, $0.15-0.25 per hr 5-1-97, 5-1-98, 5-1-99, $300-700 lump-sum 5-1-95, 5-1-96, 5-1-97". We consider this clause in two parts, the first part containing the clause prior to the phrase "$300-700 lump-sum" and the second part containing the remaining section of the clause. We converted the string in the first part to a succession of annual wage increases in dollars by extending the "None" in year 1 onto the 2nd year that is blank and taking the midpoint of the range of increases for years 3, 4, and 5 (corresponding to 1997, 1998, and 1999 for this contract). This gives us the sequential annual increases in hourly wage in dollars: $0.00 in year 1, $0.00 in year 2, $0.20 in year 3, $0.20 in year 4, and $0.20 in year 5. Next, we link this contract with the average hourly wages for unionized workers in non-manufacturing sectors in years 1995 through 1999 from the MORG data set ($16.15, $16.55, $17.07, $17.62, and $18.34 per hour, respectively). We use these wages from the MORG data set as "baseline wages" for each of the years 1995 through 1999 and convert the dollar increases into percentage increases. We thus obtain the sequential annual increases in percentages: 0.00% in year 1, 0.00% in year 2, 1.17% in year 3, 1.14% in year 4, and 1.09%in year 5. Taking the average percentage increase over the 5-year period gives an increase of approximately 0.7%.

For the second section of the clause, we convert the string to a succession of annual wage increases in dollars by taking the midpoint of $300 and $700. This gives us the sequential annual increases in dollars: $500 in year 1, $500 in year 2, $500 in year 3, $0 in year 4, and $0 in year 5. Converting these to hourly wage increases assuming a 40-hour work week and 52 weeks worked per year gives: $0.24 in year 1, $0.24 in year 2, $0.24 in year 3, $0.00 in year 4, and $0.00 in year 5. Lastly, we convert these to percentage increases relative to the baseline wages: 1.48% in year 1, 1.45% in year 2, 1.40% in year, 0.00% in year 4, 0.00% in year 5. Taking the average percentage increase over the 5-year period gives an increase of approximately 0.9%.

Next, we arrive at the Δ Wage Increase % variable by taking the sum of the two average percentage increases to get 1.5% = 0.7% + 0.9%.

Assignment of Contracts to SIC Codes

To examine the effects of industry-level exposure on union bargaining outcomes, we must overcome the hurdle that most contracts only listed the employer's industry or sector at a level coarser than 4-digit SIC. Some contracts did include 4-digit SIC codes, enabling us to directly link them to the industry-level exposure defined in Equation 10. Others had 6-digit NAICS codes; for these we use the weighted crosswalk from Acemoglu et al. (2016) to map NAICS 1997 6-digit to SIC 1987 4-digit industry codes. The majority of contracts only included SIC and NAICS codes at the 1- or 2-digit level. To map these data to 4-digit SIC codes, research assistants used a similar approach as with union elections to either manually obtain the 4-digit SIC code or to identify the associated DUNS number from www.dnb.com.[36] Some establishments corresponded to more than one DUNS number and hence multiple industry classifications. For these establishments, we use the industry classification that has the highest import exposure as defined in Acemoglu et al. (2016).[37] We aggregate these SIC codes to industry family codes using the crosswalk from Pierce and Schott (2016).

Assignment of Contracts to Commuting Zones

To examine the effects of local labor market exposure to import competition on union bargaining outcomes, we map the contract settlements to 722 Commuting Zones (CZs) that cover the entire U.S. mainland. The contract settlements provide data on the state(s) and city (or cities) in which the establishments are located. Some contracts span geographic units larger than cities, such as metro areas, states, multiple states, Census divisions, or even nationwide.

For those contracts that correspond to a unique city and state, we geocode the latitude and longitude of the city centroid and mapped it to the county in which the centroid is located. For contracts with more expansive geographic coverage, a research assistant manually identified all of the FIPS county codes that belonged to the geographic level in the contract. We exclude contracts that are "nationwide" or simply "multi-state" from this approach.

[36] Forty-five contracts were manually linked by research assistants to SIC codes that do not exist. We omit these contracts for the industry-level analysis but include them in the geography-level analysis.

[37] Of our sample, only 38 establishment contracts were linked to multiple industry codes. For these 38 cases, We use the Acemoglu et al. (2016) measure of import exposure, rather than our primary measure of exposure reflecting the NTR gap from Pierce and Schott (2016), since a non-trivial number of industries have missing NTR gaps.

Among the contracts we matched to a county or counties, the mean contract mapped to 8 counties and the median contract mapped to 1 county.

We used the same county-CZ crosswalk to map contracts to Commuting Zones as we did for the union elections.

C Using Methods from Autor et al. (2013) and Acemoglu et al. (2016) to Estimate the Effect of the China Shock

Our primary analysis uses the identification strategy developed by Pierce and Schott (2016) and Pierce and Schott (2020) to estimate the causal effect of Chinese import exposure on union elections. As a robustness test, we also use the approach pioneered by Autor et al. (2013) and subsequently Acemoglu et al. (2016) to measure industries' and local labor markets' exposure to Chinese imports. We obtain similar results using this method.

C1 Measuring Industry Exposure

We measure industry k's exposure to Chinese import penetration over time period τ in the U.S. as:

$$\Delta IP^{cu}_{k\tau} = \frac{\Delta M^{cu}_{k\tau}}{Y_{k1991} + M_{k1991} - X_{k1991}} \tag{10}$$

Here, $\Delta IP^{cu}_{k\tau}$ is the change in import penetration from China (c) to the U.S. (u) for industry k over period τ. To calculate this variable, we divide $\Delta M^{cu}_{k\tau}$, the change in Chinese imports to the U.S. in industry k over period τ, by the U.S. market volume in 1991 for industry k, which is the sum of Y_{k1991} (US industry shipments in 1991) and $M_{k1991} - X_{k1991}$ (the net imports in k in 1991). When we use this measure in (10) as an explanatory variable in our regressions, we annualize it by multiplying $\Delta IP^{cu}_{k\tau}$ by a factor of $\frac{1}{\tau}$.

An issue with using $\Delta IP^{cu}_{k\tau}$ for estimation is that realized U.S. imports from China might reflect industry demand shocks. In this case, OLS estimates with $\Delta IP^{cu}_{k\tau}$ as an explanatory variable would be biased if, for example, unobserved industry-level demand raises both imports and employment prospects that directly affect unionization efforts. Consistent with the convention that has emerged in studies analyzing the China Shock using this approach, we instrument for U.S. exposure to Chinese import penetration using the increase in imports from China over period τ to a set of comparison countries:

$$\Delta IP^{co}_{k\tau} = \frac{\Delta M^{co}_{k\tau}}{Y_{k1988} + M_{k1988} - X_{k1988}}$$

Here, $\Delta IP^{co}_{k\tau}$ is the change in import penetration from China (c) to eight other advanced countries (o) for industry k over period τ. On the right-hand side, $\Delta M^{co}_{k\tau}$ is the change in imports from China in industry k over period τ to the comparison countries Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. We divide $\Delta M_{k\tau}^{co}$ by the U.S. market volume, defined above, in 1988.

Using files assembled by Autor et al. (2013), we create a dataset that includes $\Delta IP_{k\tau}^{co}$ and $\Delta IP_{k\tau}^{cu}$ for each 4-digit manufacturing industry across every subperiod between the years 1991 through 2007. We link data on international trade for 1991–2007 from the UN Comtrade Database[38] to a slightly aggregated set of 4-digit SIC codes (as explained in more detail in Acemoglu et al. 2016), so that our final data set contains 392 manufacturing industries.

C2 Measuring Local Labor Market Exposure

Following Acemoglu et al. (2016), we measure exposure of each CZ j as the average change in Chinese import penetration over period τ across industries located within a CZ, weighted by the share of each industry k in the CZ's initial employment:

$$\Delta IP_{j\tau}^{CZ} = \sum_k \frac{L_{jkt}}{L_{jt}} \Delta IP_{k\tau}^{cu} \tag{11}$$

In this expression, t is the start year of period τ and L_{jkt} is the employment in industry k, in CZ j, in year t. $\Delta IP_{k\tau}^{cu}$ is as defined in Equation 10. As before, we instrument $\Delta IP_{j\tau}^{CZ}$ with the corresponding industry-level import penetration across the eight comparison countries:

$$\Delta IP_{j\tau}^{co} = \sum_k \frac{L_{jk1988}}{L_{j1988}} \Delta IP_{k\tau}^{co}, \tag{12}$$

where L_{jk1988} is the employment in industry k in CZ j in 1988 as computed from the CBP.

We obtain data on local industry employment structure from the County Business Patterns (CBP). These data were also used by Autor et al. (2013), who only considered CZ employment for the years 1980, 1990, and 2000. Because in some analyses we consider multiple periods of exposure τ, we expand the CBP dataset to include data on employment for the years 1988 and all years 1991–1999 and 2001–2007. We impute employment by county by 4-digit SIC code for each year using the same approach as Autor et al. (2013); see their paper for more details on the exact methodology.

Table D1 describes summary statistics for the union election data and our measures of import penetration.

[38] See http://comtrade.un.org/db/default.aspx

C3 The Effects of Industry Exposure on Union Elections (Autor et al. 2013 Approach)

To estimate the effect of industry exposure to Chinese imports on new union elections, we estimate the following regression:

$$elect_{k\tau} = \beta_1 \Delta IP^{cu}_{k\tau} + \beta_2 X_{k0} + \delta_\tau + \delta_{s(k)} + \epsilon_{k\tau}, \tag{13}$$

The dependent variable, $elect_{k\tau}$, is the annualized number of union certification elections in industry k over period τ; we annualize this measure by dividing by the number of years in period τ. $\Delta IP^{cu}_{k\tau}$ is described above. X_{k0} is start-of-period industry-level controls which includes industry k's initial unionization rate in 1989. In some specifications, X_{k0} also includes various controls considered by Acemoglu et al. (2016), including: production workers as a share of total employment in 1991, the log average wage in 1991, the ratio of capital to value added in 1991, computer investment as a share of total investment in 1990, high-tech equipment as a share of total investment in 1990, and the 1976-1991 changes in both industry log average wages and in industry share of total US employment. δ_τ and $\delta_{s(k)}$ are fixed effects for decade and for 10 broad manufacturing subsectors, respectively. As has become conventional, we define τ as either 1990–1999 or 2000–2007, and we stack the data so that a unit of observation represents an industry-decade ($k\tau$).[39] As we did in our primary analysis, because our dependent variable is a count variable, we estimate Equation 13 using Poisson regression. In all models we include an exposure variable for each industry's initial employment. We weight observations by each industry's initial employment and cluster standard errors by industry.

Table D2 presents the regression results. Column 1 controls only for decade fixed effects; the coefficient on $\Delta IP^{cu}_{k\tau}$ is negative and significant at the 10-percent level ($p = 0.058$). The coefficient is essentially unchanged and becomes slightly more precise when we add dummies for 10 manufacturing subsectors (Column 2), each industry's 1989 unionization rate (Column 3), and a set of additional industry-level controls considered by Acemoglu et al. (2016) (Column 4). Using the estimate in Column 4, a 1 percentage point increase in industry-level Chinese import penetration leads to 24 percent ($p = 0.015$) fewer union elections per worker over the same period $\hat{\beta} = -0.268, exp(-0.268) - 1 = -0.24$). In Column 5, we replace the

[39] That is, during the period 1990–1999, we calculate the number of elections that took place in any year including and between 1990 and 1999, and we measure trade exposure using annualized $\Delta IP^{cu}_{k\tau}$ and $\Delta IP^{co}_{k\tau}$ with $\tau = 1999 - 1991$. Similarly, for elections that take place in any year including and between 2000 and 2007, we use $\tau = 2007 - 2000$.

number of total elections with the number of *successful* union elections per worker (elections in which the union is victorious). The point estimate is essentially unchanged, suggesting that the China Shock had little effect on voting outcomes conditional on an election being held.

Overall, these results corroborate our main analysis using the Pierce and Schott (2016) identification strategy: workers in industries directly exposed to Chinese import competition were less likely to seek union certification by quantitatively important amounts.

C4 The Effects of Local Labor Market Exposure on Union Elections (Autor et al. 2013 Approach)

To estimate the *indirect* effect of local labor market exposure to import competition on workers' union organizing efforts, we estimate:

$$elect_{j\tau} = \beta_1 \Delta IP^{CZ}_{j\tau} + \beta_2 X_{j0} + \delta_\tau + \delta_{d(j)} + \epsilon_{j\tau}. \tag{14}$$

$elect_{j\tau}$ is the number of union certification elections in CZ j over period τ, $\Delta IP^{CZ}_{j\tau}$ is as described above and X_{j0} is start-of-period CZ controls including initial unionization rate in 1989, whether the state was a right to work state in 1990, and controls considered by Autor et al. (2013) which include CZ manufacturing employment in 1990, the share of population that has a college education in 1990, the share of population that is foreign-born in 1990, and the share of working age women that are employed in 1990. δ_τ and $\delta_{d(j)}$ are fixed effects for decade and for the Census division in which CZ j is located, respectively.

Table D3 presents the results. Column 1 shows the estimate from a model that controls for decade fixed effects: the estimate is negative and statistically significant ($p = 0.033$). We progressively add richer controls for: Census division fixed effects (Column 2); the state unionization rate, the percent of the initial CZ workforce employed in manufacturing, and whether a state had adopted Right-to-Work laws as of 1990 (Column 3); and additional control variables considered by Autor et al. (2013) (Column 4). The coefficient is stable to these controls and, if anything, slightly increases in magnitude and precision. The estimate in Column 4 implies that a one percentage point increase in CZ-level import penetration over a decade leads to 97 percent fewer union certification elections over that decade in the CZ ($\hat{\beta} = -3.78, exp(-3.78) - 1 = -.97, p = 0.02$).[40]

[40] The coefficients in this table are an order of magnitude larger than those in Table D2 estimating the effects of industry exposure. However, these tables are not directly comparable. As shown in Table D1, the mean and standard deviation of industry-level import exposure are much larger than the mean and standard deviation of CZ-level exposure. Put another way, a one-percentage point increase in industry-level exposure represents a roughly 0.6 SD increase, whereas a one-percentage point increase in CZ-level exposure represents

a roughly 8 SD increase.

D Tables and Figures Using the Methods from Autor et al. (2013) and Acemoglu et al. (2016) to Measure Import Exposure

Table D1: Union Election Summary Statistics for Autor et al. (2013) Specification

	(1) 1990-1999	(2) 2000-2007	(3) All Years
Entire U.S.			
# of Elections Overall per Decade	29,131	17,817	23,474
# of Elections per 10,000 Workers per Decade	2.75	1.41	2.08
Share of Elections among Manufacturing Establishments	0.25	0.20	0.23
Share of Elections Won per Decade (%)	0.50	0.58	0.54
Mean # of Eligible Employees in Elections	67.62	74.36	70.99
Industry-level variables (N=392 4-digit SIC Manufacturing Industries)			
# of Elections per 4-digit SIC per Decade	13.82	7.37	10.59
	(25.51)	(13.68)	(20.71)
# of Elections per 10,000 Workers per 4-digit SIC per Decade	3.55	2.01	2.78
	(3.18)	(2.96)	(3.16)
Δ Industry Import Exposure	0.44	1.01	0.72
	(1.14)	(2.09)	(1.71)
Local labor market variables (N=722 Commuting Zones (CZs))			
# of Elections per CZ per Decade	40.35	26.91	33.63
	(130.88)	(101.62)	(117.32)
# of Elections per 10,000 Workers per CZ per Decade	2.40	1.11	1.75
	(2.21)	(1.25)	(1.91)
Δ CZ Import Exposure	0.07	0.14	0.10
	(0.09)	(0.13)	(0.12)

Notes: The table reports variable means, as well as standard deviations in parentheses. Elections taking place in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not reported. Data on number of workers per CZ in 1990 and 2000 are from the Quarterly Census of Employment and Wages (QCEW). Data on number of workers per 4-digit SIC code in 1990 and 2000 is from the County Business Patterns (CBP) data files.

Table D2: The Effect of Industry Exposure to Import Competition on Rate of Union Organizing Elections Using Acemoglu et al. (2016) Specification

	(1)	(2)	(3)	(4)	(5)
	Elections	Elections	Elections	Elections	Wins
Dep Var:	Mfg	Mfg	Mfg	Mfg	Mfg
Δ Industry Import Exposure	-0.264*	-0.241**	-0.218**	-0.268**	-0.278*
	[0.139]	[0.112]	[0.109]	[0.111]	[0.145]
Share of Production Workers, 1991				-0.018**	-0.018***
				[0.007]	[0.006]
Log Avg Wage, 1991				-1.190***	-1.436***
				[0.424]	[0.411]
Capital/value added, 1991				-0.147	-0.191
				[0.165]	[0.162]
Computer Investment Share, 1990				-0.082***	-0.069***
				[0.025]	[0.023]
High-tech Investment Share, 1990				-0.051	-0.042
				[0.038]	[0.042]
Δ Ind Employment Share, 1976-1991				-1.438	-2.046**
				[0.957]	[0.998]
Δ Log Real Wage, 1976-1991				-0.010	-0.007
				[0.007]	[0.007]
Observations	784	784	784	784	784
Decade FE	Y	Y	Y	Y	Y
1-digit Mfg Sub-sector FE	N	Y	Y	Y	Y
1989 Unionization	N	N	Y	Y	Y
# SIC Codes	392	392	392	392	392
Mean Dep Var	10.59	10.59	10.59	10.59	4.232

Notes: The table reports results from the Poisson regression specified in Equation 13. The model is estimated on 392 manufacturing industries over the sample period 1991–2007. The dependent variable is the number of manufacturing elections taking place in each decade. All regressions include a constant. Robust standard errors in parentheses are clustered at the 4-digit SIC code level. Models are weighted by start of period (1990 or 2000) 4-digit SIC employment levels. Import shocks are annualized. Regressions use lagged employment at the industry level as the exposure unit. Elections taking place in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not reported. Data on number of workers per 4-digit SIC code in 1990 and 2000 is from the County Business Patterns (CBP) data files. 1989 unionization rates by 4-digit SIC code are from CPS-MORG.

Table D3: The Effect of Local Labor Market Exposure to Import Competition on Rate of Union Organizing Elections Using Acemoglu et al. (2016) Specification

	(1)	(2)	(3)	(4)	(5)
	Elections	Elections	Elections	Num Elections	Wins
Dep Var:	Total	Total	Total	Total	Total
Δ CZ Import Exposure	-3.604**	-5.108***	-5.034***	-3.609**	-3.883**
	[1.688]	[0.950]	[1.600]	[1.468]	[1.527]
Employment in Mfg, Start of Period			0.009	0.002	-0.003
			[0.009]	[0.011]	[0.011]
1989 Unionization Rate			0.008	0.003	0.014
			[0.019]	[0.016]	[0.016]
Right to work state			-0.397***	-0.446***	-0.439***
			[0.115]	[0.139]	[0.151]
College-educated Share, Start of Period				0.013	0.028**
				[0.011]	[0.013]
Foreign-born Share, Start of Period				-0.011***	-0.010**
				[0.004]	[0.004]
Female Employment, Start of Period				-0.029**	-0.041***
				[0.013]	[0.015]
Observations	1,444	1,444	1,444	1,444	1,444
Decade FE	Y	Y	Y	Y	Y
Census Division FE	N	Y	Y	Y	Y
# Commuting Zones	722	722	722	722	722
Mean Dep Var	33.63	33.63	33.63	33.63	18

Notes: The table reports results from the Poisson regression specified in Equation 14. The model is estimated on 722 commuting zones (CZs) over the sample period 1991–2007. The dependent variable is the number of elections taking place in each decade. All regressions include a constant. Robust standard errors in parentheses are clustered at the state level. Models are weighted by start of period (1990 or 2000) CZ employment levels. Import shocks are annualized. Regressions use lagged employment at the CZ level as the exposure unit. Elections taking place in Hawaii, Alaska, Puerto Rico, U.S. Virgin Islands, and Guam are not reported. 1989 unionization rates by are from CPS-MORG.

E Did Direct and Indirect Exposure to the China Shock Affect Employers' Resistance to Union Drives?

The results in Sections and 5.2 and 5.3 support the hypotheses derived from our conceptual framework in Section 4: direct industry exposure to Chinese imports reduced the expected wage gains from unionization, and indirect local local labor market exposure increased the cost of job loss–both of which discourage a worker from organizing. It is also useful to interpret these results through the lens of the *employer*. As was briefly discussed in Section 4, there is much evidence that employers actively resist union drives, that this resistance has been increasing over time (Kleiner, 2001), and that resistance has a meaningful effect on a drive's success (Dickens, 1983; Ferguson, 2008).

Our framework in Section 4 can be extended consider the China Shock's effect on employers' resistance to union drives. The decline in quasi-rents arising from direct industry exposure to the China Shock may make employers in exposed industries more resistant to union drives over time. While Abowd and Farber (1990) note that the relationship between quasi-rents and employer resistance is theoretically ambiguous, they find empirical evidence that a decrease in available quasi-rents is associated with an increase in employer interference in elections. This relationship could arise, for example, because in more competitive environments, conceding quasi-rents to unions from a dwindling pot becomes too risky.[41] On the other hand, if local labor market exposure to the China Shock only affects workers' *outside option*, then this form of exposure does not affect the cost of a union drive from an *employer's* perspective. Thus, CZ-level exposure to the China Shock would have had no effect on employers' resistance to union drives.

To test these relationships, we measure employers' resistance to union drives by whether there were any challenged ballots in the course of an NLRB-certified election. Either the employer or the union can challenge the validity of individual ballots during the election or subsequent vote count. Such challenges can alter the election's outcome, particular those that are very close (Frandsen, 2021), and recent examples illustrate that employers use challenges to delay or deflate an organizing drive.[42] For each industry–year, or CZ–year, we measure the fraction of elections in which at least one ballot was challenged.

One complication with using the frequency of challenged ballots in elections for this

[41] For example, if unions have imperfect information about the amount of quasi-rents to be split, it is more costly (potentially catastrophically so) if the union overshoots what it believes is available and demands "too high" of a wage premium when the "pie" is smaller. Thus, the risk of unionization would be higher when quasi-rents decline.

[42] A 2022 union organizing drive at an Amazon warehouse in Alabama illustrates this relationship. Out of the roughly 2,400 votes cast in this election, over 400 of them were challenged (Bidar et al., 2022). While these challenges could have come from either Amazon or the union, it was well-publicized that Amazon went to great lengths to interfere in the election (Palmer, 2022).

analysis is that exposure to the China Shock could cause changes in the level of employer resistance to organizing drives, as well as the changes in workers' willingness to organize conditional on the level of employer resistance. Consider a simple model in which there is an initial distribution of employer resistance to unionization. All else equal, a higher level of employer resistance makes a worker more reluctant to hold an election; for example, more resistant employers might be likely to retaliate against organizing workers. This would lead to a resistance threshold, above which a worker would definitely not organize an election. The location of this threshold is a function of the worker's outside option and other factors determining the costs of retaliation. Because we only observe challenged ballots among elections that occur, we can only empirically measure employer resistance at workplaces below this threshold.

Figure A6 illustrates how this simple model complicates our ability to identify changes in underlying employer resistance. As shown in Panel (a), an increase in employers' resistance would manifest as a rightward shift in the distribution. If the worker's resistance threshold remains unchanged, then this would lead to an increase in observed resistance among elections that take place. Panel (b) illustrates a case where the distribution of employer resistance is unchanged, but the worker's resistance threshold shifts to the left, for example due to a worsening outside option. In this case, there would be a *decrease* in observed resistance among elections that take place, even though the underlying distribution has not changed.

With these caveats in mind, Tables A9 and A10 report our estimates of direct and indirect import exposure on the frequency of challenged ballots, respectively. Table A9 mimics Table 1, except that the dependent variable is the share of elections in which at least one ballot is challenged. The point estimates are all positive, and are statistically significant beginning in Column 3, indicating that direct industry import exposure led to increased rates of employer resistance in elections that occurred. Since it is unlikely that direct industry exposure increased workers' resistance threshold, one can plausibly interpret these estimates as evidence of increased underlying employer resistance to union drives.

Table A10, mimicking Table 2, estimates the effect of indirect local labor market exposure on the frequency of challenged ballots. Across all specifications, the point estimates are *negative* and fairly large in magnitude, though none are statistically significant. Since it seems unlikely that indirect local labor market exposure made employers *less* resistant to union drives, this negative estimate likely reflects a shift in workers' willingness to organize for any given level of employer resistance. That is, through the lens of the simple model outlined in panel (b) of Figure A6, indirect exposure likely caused a leftward shift in workers' resistance threshold (by worsening workers' outside option), leading to a reduction in the average level of employer resistance among elections that occurred.

These results, with appropriate caveat, provide further empirical support that our conceptual framework is a useful lens to interpret the effects of import exposure on union organizing. Direct industry exposure to the China Shock reduced firms' profitability, which simultaneously made union drives less attractive to workers and more costly for employers. Indirect local labor market exposure deteriorated workers' outside option, which made union drives more costly for workers even if it left employers' resistance to drives unchanged.