Global Economic Shocks, Local Economic Institutions, and Legislative Responses

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Abstract

The dramatic rise in import competition from developing countries since the 1990s has given new urgency to explain political responses in the United States and other advanced economies. While recent scholarship has demonstrated multiple effects of trade shocks, far less attention has been paid to unbundling the mechanisms through which import competition affects political responses on compensatory and trade policies. In this paper, we assess the possibility that an important part of the political effect works through local economic institutions – labor unions – that shape the relative strength of workers in the policymaking process. We study the US House of Representatives to untangle this institutional channel from other channels. In our empirical analysis we leverage two distinct sources of exogenous variation: an instrument for import competition drawn from the literature and a novel instrument for district-level union membership based on history and geography. We use them in a new semi-parametric approach to disentangle causal mechanisms. We find that (i) import competition lowers district-level unionization, (ii) weaker unions lead to less legislative support for compensating economic losers as well as less opposition to trade deregulation, and (iii) the union channel represents a large fraction of the overall effect of import exposure on legislative votes. We show that results are robust under alternative specifications and when accounting for possible local violations of exclusion restrictions. These findings have important implications for the political economy of globalization. They suggest that the political impact of globalization is broad with lasting consequences for civil society and democratic politics.

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1. Introduction

A fundamental question in political science and political economy concerns the impact of economic globalization on domestic politics and public policy. The dramatic rise in import competition from China and other developing countries since the early 1990s has given new urgency to explain political responses—or lack thereof—in affected regions in the United States and other affluent democracies. Recent research by economists has found that the dramatic growth in imports from the developing world is linked to a shrinking manufacturing sector, lower labor force participation and lower wages in local labor markets, and even increased mortality (Acemoglu et al. 2016; Autor, Dorn, and Hanson 2013*a*; Pierce and Schott 2016, 2018). Against this backdrop of deep and broad distributive effects of international trade, a burgeoning political economy literature examines the effect of import competition on elections (Autor et al. 2016; Colantone and Stanig 2018*a*, *b*; Margalit 2011). With respect to the actions of governments and politicians, some political economy theories predict that democratically elected politicians in affected regions have incentives to support policies that compensate trade losers through income support and redistributive programs. However, the evidence so far is quite limited. For instance, research on the US Congress finds that legislators from more trade exposed districts are more likely to oppose trade liberalization (Feigenbaum and Hall 2015) and are more critical of China (Kuk, Seligsohn, and Zhang 2018), but their legislative behavior on a broader range of issues may not be affected.

While this ongoing debate has already produced a tremendous amount of new evidence and profound insights, it has paid relatively little attention to unbundling the mechanisms through which international trade affects domestic politics. This is problematic for several reasons. First, the lack of attention to untangling mechanisms hampers our understanding of what theoretical perspectives can contribute to explain the political consequences of trade exposure. It neglects that trade may change politics through an especially persistent channel—local economic institutions—that is likely to affect the balance of political power between winners and losers in the long-run. Furthermore, the prevailing focus can also obfuscate countervailing effects of international trade, potentially leading to the false conclusion that it leaves political conflict over economic policy largely unaffected.

In contrast, we assess the possibility that an important part of the political effect of import competition is indirect—working through local economic institutions that shape the relative political clout of workers in democratic politics more broadly. This perspective complements the emphasis in the voluminous globalization literature on political institution and government partisanship (Garrett 1998; Grossman and Helpman 2005; Milner and Kubota 2005). Focusing on the US House of Representatives as an important test case, we assess the argument that import exposure shapes politics and policy to a significant extent through its negative impact on labor unions. Testing this logic faces well-known but difficult empirical challenges of measurement and causal identification. To overcome them, we leverage two distinct sources of exogenous variation, an instrumental variable for import competition drawn from the literature and a novel instrumental variable for district-level union membership, as well as fine-grained administrative data on local unions, and we employ a new semi-parametric approach to analyzing causal mechanisms. Our preliminary results suggest that (i) import competition lowers unionization, (ii) weaker unions lead to less legislative support for compensating economic losers as well as less opposition to trade deregulation, and (iii) the union channel represents a large fraction of the overall effect of import exposure on legislative votes. These findings have important implications for the political economy of globalization and democratic politics.

In a first step, we analyze the causal effect of import competition from China on union membership in congressional districts. Following Autor, Dorn, and Hanson (2013*a*) and subsequent work, we use Chinese import growth in other OECD economies as an instrument for trade shocks in the US. This addresses the concern that import competition is endogenous to local conditions, including the prior strength of unions. We also control for state fixed effects and pre-shock district characteristics such as automation risk and socio-demographics. Our district-level analysis requires fine-grained data on labor union membership that is not generally available from surveys but has been compiled from mandatory government reports by Becher, Stegmueller, and Kaeppner (2018). Our estimates imply that a $1,000 increase in per-worker import exposure lowers union density by about 20%.

In a second step, we analyze the causal effect of district-level unionization on legislative voting in Congress. While the literature offers numerous studies of how unions shape roll-call voting and policymaking, it almost exclusively relies on selection on observables assumptions to deal with threats to causal inference (e.g., Becher, Stegmueller, and Kaeppner 2018; Box-Steffensmeier, Arnold, and Zorn 1997; Freeman and Medoff 1984). This makes it difficult to rule out that omitted variables, such as local tastes and policies or informal elite power, may drive both union mobilization and legislative voting (Ahlquist 2017).

To address this problem, we leverage variation in union strength that stems from geography and the history of union mobilization long before the onset of the trade shock under study. In the middle of the twentieth century, after the passage the new deal labor legislation and the achievement of full employment during World War II, virtually all coal and metal mines were unionized throughout the country.[1] The location of these industries is determined by nature and the unionization at the time reflected the relative pro-union legal and political environment as well as industry characteristics, such as a high investment in fixed assets, that made union mobilization comparatively easy. Holmes (2006) shows that mining-based unionization subsequently spilled over into

[1] Freeman and Medoff (1979) document unionization rates of 98 to 100 percent in several mining and steel industries.

non-manufacturing sectors. In line with this, we demonstrate that congressional districts with higher shares of mining and steel employment in the 1950s have significantly higher unionization rates in the 2000s. Using the historical employment shares as an instrumental variable for contemporary unionization, there is a consistent effect of local unions on legislative support for policies to compensate economic losers as well as opposition to trade liberalization.

In a third step, we formally unbundle different mechanisms through which import exposure may affect legislative votes. For this purpose, we specify a structural model that allows for endogenous imports from China and endogenous unionization. While standard regression-based models to unbundle causal effects require very strong assumptions in this setting (Imai, Keele, and Tingley 2010), a recent methodological innovation in semi-parametric causal chain analysis with instruments (Frölich and Huber 2017) enables us to incorporate our two instrumental variables and relax functional form assumptions to make more credible inferences about the relative importance of the union mechanism. The preliminary estimates suggest that the local unions account for roughly 40% of the impact of import exposure on legislative votes concerning compensation and about 50% for legislative votes concerning trade.

In sum, this paper aims to makes three main contributions. First, our findings complement previous empirical research on the economic effects of the "China shock" showing how import competition affects industry-level, commuting-zone level, and individual level labor market outcomes (Acemoglu et al. 2016; Autor, Dorn, and Hanson 2013*a*; Pierce and Schott 2016). The first step of our analysis shows that import competition also has pronounced effects on local labor unions in congressional districts. In turn, this has important economic and political consequences. A related study finds that import exposure is linked to a decline in unionization in manufacturing industries but not to overall unionization at the state level (Ahlquist and Downey 2019). Our results differ probably because much of the variation in import exposure stems from different regions in the same state (Autor, Dorn, and Hanson 2013*a*, 2016), and a more disaggregated analysis can better untangle import competition from exposure to technological shocks (Autor, Dorn, and Hanson 2013*b*). Pre-China shock cross-national and industry-level research has examined how economic globalization affects organized labor without reaching a firm conclusion (Schnabel 2013; Slaughter 2007) and without addressing the problem that import exposure itself may be endogenous to the factors that give rise to weaker or stronger unions. Our results are consistent with the view that effects should be pronounced in countries lacking broader corporatist or centralized wage-bargaining institutions (Scruggs and Lange 2002).

Second, we provide novel evidence that the strength of local unions has a causal effect on legislative behavior in Congress. Because this relationship represents a theoretically relevant part in the indirect causal chain through which trade shocks may affect the responses of legislators with respect to votes on trade and compensation, it is essential to re-assess its causal interpretation. The finding is also of interest to the larger literature on the political effects of unions (Freeman and Medoff 1984). Identifying plausibly exogenous sources of variation in unionization has proven largely elusive. However, our findings are broadly consistent with recent work on county-level election outcomes using the removal of agency shop protection through right-to-work laws across counties in neighboring states (Feigenbaum, Hertel-Fernandez, and Williamson 2018). Our complementary focus enables us actually measure union membership and trace its effect on legislative voting.[2]

Third, we are the first to quantify the relevance of the union mechanism relative to other (i.e., direct) political effects of import competition. While the estimates are preliminary, it is fair to say that the relative importance of the institutional channel, working through the strength of local unions, has received fairly little attention in the current debate (Owen 2015). It implies an enduring shift in the relative political strength of workers, especially those in lower part of income or skill distribution, versus business and affluent constituents.

2. Does import exposure reduce district-level unionization?

We explore the relevance of local labor unions as a neglected institutional mechanism explaining political responses in Congress to international trade. A first step in our analysis is to asses the impact of import exposure from China on district-level union strength. Despite its potential economic and political importance, union strength as an outcome has received surprisingly little attention in the recent wave of research exploiting within-industry and between-region variation in the dramatic growth in import exposure from China and other developing countries.

2.1. Theoretical motivation

Theoretically, the effect of import competition on unionization is ambiguous. On the one hand, the combination of trade-induced plant closures and federal laws governing unions exert a downward pressure on union density. Empirically, firm exit and especially plant closures account for most of the decline in manufacturing employment since the 1980s compared to employment adjustments within an existing plant (Fort, Pierce, and Schott 2018). This matters because local unions are formed at the establishment (i.e., plant) level. Federal legislation implies that any new plant is union-free by default, even if other employees in the same company are unionized. Within the institutional

[2]For recent studies of union effect on mass preferences for trade or redistribution, see Ahlquist, Clayton, and Levi (2014); Kim and Margalit (2017); Mosimann and Pontusson (2017).

framework supervised by the National Labor Relations Board[3], forming a union for collective bargaining with the employer is costly as it requires first, collecting a sufficient amount of signatures to request a certification election for the new bargaining unit, and, if this is achieved, winning a majority in the election. The number of certification elections has been declining since the 1980s and, conditional on taking the first hurdle of achieving a certification election, a union victory is by no means a foregone conclusion, even as union win rates have increased from below 50% during their lowest point in the early 1980s (Farber and Western 2002: 388, Farber 2015).[4] As a result, the capitalist dynamics of plant and firm exits interacted with the establishment-based union certification process suggests that import competition leads to a decline in the stock of union members.

Reinforcing this logic, companies that shut down an existing plant may also pursue a strategy of "domestic offshoring" and open a new plant in a different location in the country where it is more difficult to unionize. In line with this, a county-level analysis finds that manufacturing employment increases sharply if one crosses the border from a state without to a state with a right-to-work law (Holmes 1998).

Social spillovers are a mitigating force (Ahlquist and Downey 2019; Holmes 2006). Many workers who lose a unionized job due to a plant closure have the taste and skills to try to unionize the next establishment they are employed in. Or their close family and friends will undertake this. This partially offsets the mechanical effect outlined above. However, theoretically it is not clear that spillovers can fully compensate for the decline in unionization through firm exits and plant closure caused by international trade. This economic context is quite different from a context where most workers voluntarily leave a unionized job, are replaced by another unionized worker, and may go on to help unionize a different establishment. In addition to the electorally uncertain outcome of the certification process, another countervailing factor is that in exposed local labor markets the decline in manufacturing employment has negative effects on the whole local economy (Autor, Dorn, and Hanson 2013*a*, 2016). The resulting downward pressure on wages and benefits also reduces the incentives to unionize.[5]

In sum, the effect of import exposure on union membership is an empirical question that we examine at the geographically disaggregated level of congressional districts. This unit of analysis is theoretically appropriate for the purpose of studying the causal chains from international trade to legislative votes. It is also attractive from an inferential perspective because much of the variation in import exposure from China stems from

[3]Established by the National Labor Relations (Wagner) Act of 1935 and modified by the Labor-Management Relations (Taft-Hartley) Act (1947) and the Labor-Management Reporting and Disclosure (Landrum-Griffin) Act (1959).

[4]A model of rational union targeting in a deteriorating environment can explain the simultaneous decline in certification elections, lower turnout and increasing union win-rate since the mid-1990s (Farber 2015).

[5]Garrett (1998) argues that the demand for collective representation in the workplace may also increase in tough economic times.

different local labor markets in the same state (Autor, Dorn, and Hanson 2013*a*), and a within-state analysis can untangle import competition from exposure to technological shocks (Autor, Dorn, and Hanson 2013*b*).

2.2. Data and identification strategy

We examine the impact of imports from China between 1990 and 2000 on subsequent union membership in electoral districts for the US House of Representatives during the 107-110th Congress (i.e., 2001-2008). During this decade, imports from China to the US rose by 362% or 95.3 billion (in 2007 US$) (Autor, Dorn, and Hanson 2013*a*: 2131). In the wake of its effects, Congress considered numerous policies concerning compensation and trade.

To measure union membership (as a share of the working population) we draw on fine-grained administrative data compiled by Becher, Stegmueller, and Kaeppner (2018). They are based on mandatory annual reports (so-called LM forms) filed by each local union to the Department of Labor. There are more than 350,000 individual reports covering almost 30,000 unions. Crucially, reports contain the membership size and address of each local union, which was geocoded and then mapped onto congressional districts for each of the two apportionment periods in the period of analysis (apportionment-based redistricting occurred between the 107th and the 108th Congress based on the 2000 census). This approach has the central advantage over using survey data that provides more reliable district-level estimates. The required local identifiers are not available in the Current Population Survey, the most widely used source for measuring union membership. Other relatively large surveys, such as the Cooperative Congressional Study, still have small sample sizes at the district level and the district samples are unlikely to be representative of the district population.[6] Substantively, most of the variation in union density in a given congressional term is between districts within the same state.[7]

To measure a congressional district's exposure to import competition from China, we follow the by now well-known approach pioneered by Autor et al. (2013*a*; 2016). The basic idea is to map industry-specific import shocks onto local labor markets in proportion to the localities' industrial employment structure.[8] More formally, denote by ΔM_{uj} the 10-year change in US imports from China from 1990 to 2000 in industry j. The employment in industry j in district d is denoted by L_{dj}, L_{uj} is total US industry employment. Finally,

[6] One possible drawback of the measure is that some unions are exempt from filling LM forms. However Becher, Stegmueller, and Kaeppner (2018) validate the administrative data with state-level survey data and find that "that LM forms provide a rather comprehensive accounting of unions."

[7] Between 28-31% of the variation is accounted for by state fixed effects.

[8] A different measure of import exposure from China in the literature uses the 2001 change in tariff policy based on granting China Permanent Normal Trade Relations (PNTR), which had a differential impact across industries given variation in non-PNTR tariffs (Pierce and Schott 2016). Conceptually, we prefer the Author-Dorn-Hanson measure since it allows us to use trade shocks in the 1990s that more clearly precede the congressional votes we are examining later on.

L_{dt_0} signifies a districts employment in industry j at the beginning the 1990s. Then, the measure of district-level import exposure is:

$$\Delta IPW_d^{US} = \sum_j \frac{L_{dj}}{L_{uj}} \frac{\Delta M_{uj}}{L_{dt_0}} \tag{1}$$

It captures the change in Chinese import exposure per worker in a district apportioned by its share in total national employment.[9] Table I lists the most trade-exposed and least trade-exposed districts in the data.

Table I
Districts with most and least exposure to Chinese import shock in 109thCongress

Five most impacted districts			*Five least impacted districts*		
Tennessee	1st	4.62	Virginia	8th	0.21
Missouri	8th	4.42	Virginia	11th	0.21
Missouri	7th	3.92	Florida	14th	0.19
Arkansas	1st	3.87	Louisiana	2nd	0.19
Mississippi	1st	3.42	Louisiana	3rd	0.17

In a standard regression analysis, one concern for causal identification is that district-level imports from China are endogenous to other district-level factors related to unionization (or, later, legislative votes). While there may be unobserved local demand shocks, it is less clear how they are related to unionization. Another possibility is that industries where unions are stronger may be better at lobbying for protectionism and thus face less international competition as a result. For instance, Owen (2013) finds that industry-level union strength is associated with more restrictions on inward foreign direct investment. To address these endogeneity issues, we follow Autor, Dorn, and Hanson (2013*a*) who construct an instrument from Chinese imports to other high-income markets. Their patterns of import growth across industries are highly correlated with those in US industries (Autor, Dorn, and Hanson 2016: 219) and form a strong instrument plausibly independent of local factors in congressional districts.

$$\Delta IPW_d^{oth} = \sum_j \frac{L_{dj}^{1980}}{L_{uj}^{1980}} \frac{\Delta M_{oj}}{L_{dt_0}^{1980}} \tag{2}$$

[9]The data from Autor, Dorn, and Hanson (2013*a*) are available for 741 commuting zones. We map them to congressional districts using commuting zone to district crosswalks, with Census tract based population weights. See Appendix A.

Here, ΔM_{oj} is the 10-year change in imports from China in industry j between 1990 and 2000 in eight other highly developed economies (Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland). Note that employment shares ($L_{\cdot}^{1980}$) refer to the previous decade. We follow Autor, Dorn, and Hanson (2013*a*) and Acemoglu et al. (2016) in using these lags in order to mitigate simultaneity bias, where contemporaneous district employment might be a function of anticipated Chinese imports. This instrument leverages the component in Chinese export growth that is common across developed economies, independent of local conditions, due to its rising comparative advantage or falling trade costs.

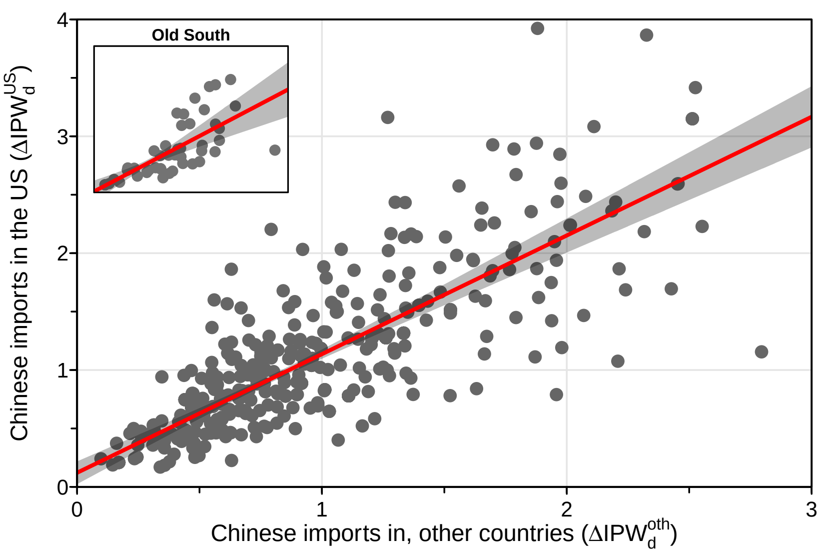


Figure I
Instrumenting Chinese import exposure

This figure plots 10-year-equivalent US imports from China per worker [in $1000] for districts of the 109th Congress (on the y-axis) against 10-year-equivalent per-worker imports in eight other high-income markets (on the x-axis). Linear fit with robust 95% confidence interval superimposed. The plot in the upper left plot shows the same relationship in states of the 'Old South'.

Previous work shows that the instrument has a strong first-stage impact on realized import exposure in American commuting zones (Autor, Dorn, and Hanson 2013*a*). We verify that the same holds true at the level of congressional districts. Figure I illustrates this graphically for districts from the 109th Congress. In substantive terms, a one percent increase in the instrument induces a 0.84 (±0.03) percent increase in import exposure.[10] Furthermore, in our analyses below we include tests that are robust under weak instruments.

[10]More tests are discussed below.

Given unionization as the dependent variable, one potential threat to identification is that the structure of unions is highly correlated across industries in different countries. However, in practice this not the case. We know from the comparative political economy literature that the strength of unions in particular, and labor market institutions more broadly, varies widely across the pool of countries from which we calculate "other"' import exposure. For instance, in 1990 union density was 45% in Australia, 75% in Denmark, and 31% in Germany compared to 16% in the United States (Visser 2016).

A different issue is that sector-biased technological change (or technology-induced demands shocks) may both be correlated with import exposure and the strength of unions. In the 1990s, the automation of routine task through computerization was a development that coincided with rising imports from the developing world. Recent research has linked automation to patterns of occupational polarization but not necessarily employment decline (Autor, Dorn, and Hanson 2015). The risk of automation also has the potential to undermine the bargaining power of unions and hence the benefits of forming a union in the first place. In smaller geographic units, such as commuting zones or congressional districts, the susceptibility to automation and import competition are only moderately correlated (Autor, Dorn, and Hanson 2013*b*, 2015, 2016). Ignoring automation may nonetheless lead to a biased estimate of the effect of import exposure on unions if it has a sufficiently strong effect on unions itself. To address this concern, we control for the pre-China shock susceptibility to automation in a congressional district's local economy.

To adjust for automation, we need a measure that broadly captures the extent to which employment in a given district is at risk of being replaced by technology. It should capture more than robotization, since many clerical white collar jobs are at risk as well. We use the routine task intensity (RTI) measure of Autor, Dorn, and Hanson (2013*b*, 2015), who use data from the Dictionary of Occupational Titles to classify job activities that were "intrinsically amenable to computerization" (Autor, Dorn, and Hanson 2013*b*: 221). For each occupation, we create an indicator variable for computerization risk, equal to one if that occupation's RTI falls in the top third of the RTI distribution in the prior decade. Then, a district's routine employment share (RSH) is the fraction of its workforce employed in occupations at risk of computerization.[11]

In extended specifications, we also include historical district-level controls and district-level socio-demographics measured at the beginning of the China shock. This further enhances the credibility of the conditional exogeneity assumption for the instrumental variable. From the 1910 census, we measure the share of black population and the share of immigrants, which requires mapping county observations to congressional districts (see Appendix A). The former captures that race has been a basis for both political

[11]Formally: $RSH_{dc} = (\sum_{k=1}^{K} L_{dkc} \times CR_k) \big/ (\sum_{k=1}^{K} L_{dkc})$ where CR_k is the automation risk indicator and L_{dkc} is employment in occupation k in district d in congress c. See Appendix A for how the commuting zone level data from Autor, Dorn, and Hanson (2015) are mapped to congressional districts.

disenfranchisement and union busting for a large part of the twentieth century. The latter captures that European immigrants, by far the largest share of immigrants in the pre-World War I era, have been argued to bring particular norms and informal institutions that may be more favorable to organized labor and left-wing politics more broadly. From the 1990 census, we calculate districts' socio-economic composition before the trade shock. This includes the share of foreign born, blacks, college-educated, employed in manufacturing, and living in urban areas. Naturally, we exclude post-shock outcomes as controls.

2.3. The impact of import exposure on union membership

Table II instrumental-variable (IV) estimates of the effect of Chinese import exposure during the 1990s on union density in House districts in the 107-110th Congress.[12] The basic model includes two-way fixed effects for states and time (i.e., Congress). It thus holds constant state-level union policies (such as right-to-work legislation) and accounts for idiosyncratic characteristics of each Congress. Some specifications also include the pre-shock district-level control variables introduced above. In the main text, we present results from a stacked analysis that pools all four congressional terms (correcting the standard errors appropriately). This is for comparability with later sections and economy of presentation. All results go through in a congress-by-congress analysis (see Appendix Table B.1). The reduced-form model with controls can be written as follows:

$$U_{dc} = \beta \Delta IPW_{dc}^{US} + \gamma H_{dc} + \delta X_{dc} + \eta A_{dc} + \psi_s + \lambda_c + \epsilon_{dc} \qquad (3)$$

where U_{dc} is the logged number of union members, as a fraction of the working population, in district d and Congress c; ΔIPW_{dc} is the import exposure during the previous decade matched to district d in Congress c; ψ_s and λ_c are state and congress specific constants. In the first stage of the estimator, import exposure in the US is instrumented with import exposure from eight other developed economies, ΔIPW_{dc}^{oth}. In terms of controls, H_{dc} represents the historical stock of blacks and immigration in a district measured from the 1910 Census. A district's socio-demographic composition is captured in X_{dc}. It is computed from the 1990 Census (and spatially matched to current congressional districts) and thus captures district characteristics during the period of the trade shock. In Appendix B we show that using contemporary values (calculated from the 2000 Census) instead does not alter our core results. Technological change through the risk of automation is represented by A_{dc}. When included, controls in X_{dc}, H_{dc} and A_{dc} are normalized to have mean zero, so that the constant term in (3) represent the change in the outcome only conditional on changes in trade exposure.

In specification (1) of Table II we report estimates for the negative impact of trade shocks on district-level union membership. We instrument Chines imports by imports

[12] Alaska and Hawaii are excluded due to missing data.

Table II
Effect of trade shocks on (logged) union membership.

	(1) IV	(2) IV	(3) IV	(4) IV	(5) dML-IV[a]
Chinese import exposure	−0.330	−0.295	−0.303	−0.287	−0.198
	(0.057)	(0.059)	(0.059)	(0.060)	(0.040)
District characteristics					
Slavery & immigration history		✓	✓	✓	✓
Socio-economic composition			✓	✓	✓
Technological change				✓	✓
State & time FE	✓	✓	✓	✓	✓
IV violation robust tests					
H_0: $\beta = 0$ w. weak IV[b]	0.000	0.000	0.000	0.000	–
H_0: $\beta = 0$ w. $Cov(Z, e) \approx 0$[c]	0.000	0.000	0.000	0.000	–
First stage F^d	637.3	575.6	519.7	528.1	
N	1729	1729	1729	1705	1705

Note: Robust standard errors. In IV models, Chinese imports per worker (ΔIPW_d^{US}) instrumented by imports to eight other highly industrialized countries (ΔIPW_d^{oth}). Estimated using 2SLS. Historical patterns of slavery and immigration proxied by 1910 Census district-shares of Blacks and foreign-born. A district's socio-economic composition includes share of foreign born, Blacks, with college degrees, employed in manufacturing, living in urban areas. Technological change measured by risk of automation (RSH_d).

a Partial linear IV model estimated using debiased machine learning with sample splitting (100 sets) and 5-fold cross-fitting (Chernozhukov et al. 2018). DML2 estimate and s.e. calculated using the median method. See appendix D for detailed model equations and machine learning algorithms used.

b Conditional likelihood ratio test p-value for trade shock allowing for weak instruments (Moreira 2009).

c Test of trade shock allowing for local violation of IV exclusion restriction (Wang et al. 2018). See Appendix C.

d F statistic of first stage regression, robust to heteroscedasticity.

in other highly developed countries, as specified in equation (2) above. We find that a \$1,000 increase in per-worker imports lowers union density by 0.33 log points (or about 28 percent). This first specification adjust for two-way (state and time) fixed effects but does not include any additional variables.

The bottom half of Table II shows that the first stage relationship of the 2SLS estimate is rather strong with the (robust) F statistic far exceeding conventional critical values. It is thus not surprising that the null hypothesis of our first violation-robust test, which tests for no effect of trade on union density ($H_0 : \beta = 0$) while allowing for arbitrarily weak instruments (Moreira 2009), is rejected at $p = 0.000$. Our second test allows for local violations of the instrument exclusion restriction. The issue here is less that the exclusion restriction of an IV estimator is in principle not empirically testable (no methodological tool will change that fact). Rather it is the fact that the exclusion has to be *exactly* zero and even minor violations may result in unreliable hypothesis tests (Berkowitz, Caner, and Fang 2008). One strategy to mitigate the strictness of this assumption is to allow for some 'local to zero' (Conley, Hansen, and Rossi 2012) violations of the exclusion. Different strategies have recently been developed, some relying on prior distributions others on re-sampling (Berkowitz, Caner, and Fang 2012) or simulation (Wang et al. 2018) methods. We employ a test based on the latter (see Appendix C). The entries in Table II show p values from an Anderson-Rubin test of the null hypothesis of no effect of trade on union density allowing for some local violation (up to a tenth of a standard deviation) of the exclusion restriction. Our results indicate that small violations of the exogeneity of the trade instrument would not change our core findings.

In column (2) we add controls for the historical legacy of slavery and immigration in each district, while column (3) adds the (time-varying) socio-demographic composition of each district. We find that adding these covariates does not substantively change the adverse effect of import shocks on unionization. In column (4) we adjust for a district's exposure to technology shocks captured by a measure of exposure to automation risk (Autor, Dorn, and Hanson 2013*b*). Adjusting for district-specific technological change does not substantially alter our conclusion. Allowing for instrument weakness and (local) invalidity does not alter the significant relationship between trade shocks and union membership.

Finally, column (5) introduces a much more robust specification that dispenses with any functional form assumption made on covariates. In keeping with the approach employed in the vast majority of empirical analyses in political science and economics, our previous models included covariates as additively separable linear combinations. To the extent that this functional form restriction is not met (e.g., if an aspect of current demographic composition interacts with historical legacies of slavery in complex ways) the model is fundamentally misspecified. This is of particular relevance in an instrumental variable context, where the instrument might be valid only conditional on covariates. To tackle this concern, we estimate the following partially linear IV model

$$U_{dc} = \beta \Delta IPW_{dc}^{US} + \mu(X) + u_{dc} \tag{4}$$

$$\Delta IPW_{d}^{oth} = \xi(X) + v_{dc} \tag{5}$$

where X is a set of controls that enters both structural equations via (possibly highly nonlinear) functions μ and ξ. We approximate these using debiased machine learning inference following the strategy outlined in Chernozhukov et al. (2018). Appendix D.1 provides more details. Our results from this much more flexible specification indicate a somewhat reduced effect magnitude: a \$1,000 increase in per-worker imports lowers union density by about 18 (±4) percent (compared to 28 percent in our first model). However, this estimate (and its robust standard error) still points towards a clear relationship between import shocks and declining union strength.

3. Do unions affect congressional votes on compensation and trade?

The next step in our analysis is to re-assess whether unions have a causal effect on legislative votes on compensation and trade issues in the House of Representatives. While there are theoretical reason to think that unions matter, the strength of unions is endogenous, potentially to the same determinants that also explain legislative votes. To address this problem, we propose an identification strategy based on history and geography.

3.1. Entering the political arena

In the first instance, local unions are organized to represent workers in collective bargaining with employers over wages and work conditions. However, once they have overcome the collective action problem to deal with specific interests they may also enter the political arena and try influence national economic policy through electoral mobilization, contributions, and lobbying (Olson 1965: 136).

It is apparent that American unions regularly take policy positions in support of the less affluent and those more exposed to labor market risks, through policies that compensate for or insure against income loss, as well as more critical positions on trade policies (Box-Steffensmeier, Arnold, and Zorn 1997; Freeman and Medoff 1984).[13] These positions are broadly consistent with self-interest, other-regarding considerations and ideology, and for the purpose of this paper we remain agnostic about the underlying mix of motives. Unions in trade-exposed sectors or trade-exposed local labor markets have an interest in providing a collective voice in support of policies that benefit their members' economic interests. This includes support for unemployment insurance, family assistance for housing and food, worker training, and income redistribution more generally. It is also important to note that most local unions belong to a national union that includes members from different sectors. Members of *United Automobile Workers*, for instance, do not only work in car manufacturing, but also in health care, higher education or gambling. Organizationally, this creates incentives for unions to internalize the potential effects of a policy across different sectors and regions. Moreover, recent micro-level evidence is consistent with the argument that norms of solidarity, often invoked by leaders, shape the relatively high support among union members—even those in the upper half of the income distribution or working in exporting sectors—for income redistribution and protectionist measures (Ahlquist, Clayton, and Levi 2014; Kim and Margalit 2017; Mosimann and Pontusson 2017).

Empirically, a central challenge is to ascertain whether unions affect the legislative behavior of national policymakers in line with their positions. Many previous studies have found associations between union strength (usually at the state level) and congressional

[13] For recent examples, see the Legislative Scorecard of the AFL-CIO: `https://aflcio.org/scorecard`.

voting or state-level policy, working to an important part through contributions and the selection of Democratic politicians. But they remain open to the interpretation that the strength of unions and political outcomes are jointly determined by the same unobserved factors (Ahlquist 2017). First, local tastes or preferences may explain both support for unions and particular economic policies. For instance, people in conservative districts may be less likely to form unions and are less supportive of government efforts to help trade losers. The same outcome can also arise because of between-district variation in the informal power of economic elites, who have an interest in avoiding unionization as well as redistribution. Relatedly, state-level or local-level institutions shape both unionization and political behavior. For instance, the enactment of right-to-work laws, which reduces the material incentives to become a union member, has been found to influence elections (Feigenbaum, Hertel-Fernandez, and Williamson 2018). Historical factors may also be an omitted variable. As a result of identity politics and racial resentment, places with a history of slavery may be less likely to unionize and coalesce around policies of compensation.

It would be a daunting task to correctly identify and measure all potential confounders. Instead, we pursue an identification strategy based on finding an instrumental variable that shifts the strength of local unions in congressional districts.[14]

3.2. History and geography as a source for identification

The history of organized labor combined with geography suggest a natural source of variation in contemporary union membership that we can leverage to assess the effect of unions on congressional voting. Following Holmes (2006), the basic idea is to leverage spillovers from unionization in a few capital-intensive industries at the heyday of unionism just after World War II, which resulted from natural endowments and the pro-union national environment rather than local tastes or local institutions. Specifically, we argue that mid-century employment in mining and steel can serve as an instrumental variable for contemporary union density. There are two main parts to this argument.

First, in the early 1950s coal and metal mines as well as steel plants were unionized across the whole country, regardless of the differences in politics or preferences. In the wake of landmark New Deal legislation facilitating unionization, such as the National Labor Relations (Wagner) Act, the booming war economy and generally more pro-union climate, "participation in unions exploded" (Freeman 1988: 265). Success in union organization nonetheless varied across sectors and even within many industries across states. However, Holmes notes an important exception: "virtually all coal and steel mines were unionized regardless of location" (Holmes 2006: 2). In the environment of the time, these were easy targets for unionization due to their capital intensity and high sunk cost,

[14]Imports from China are not a plausible IV, given the theoretical priors and evidence that suggests its effects on politics through non-union channels.

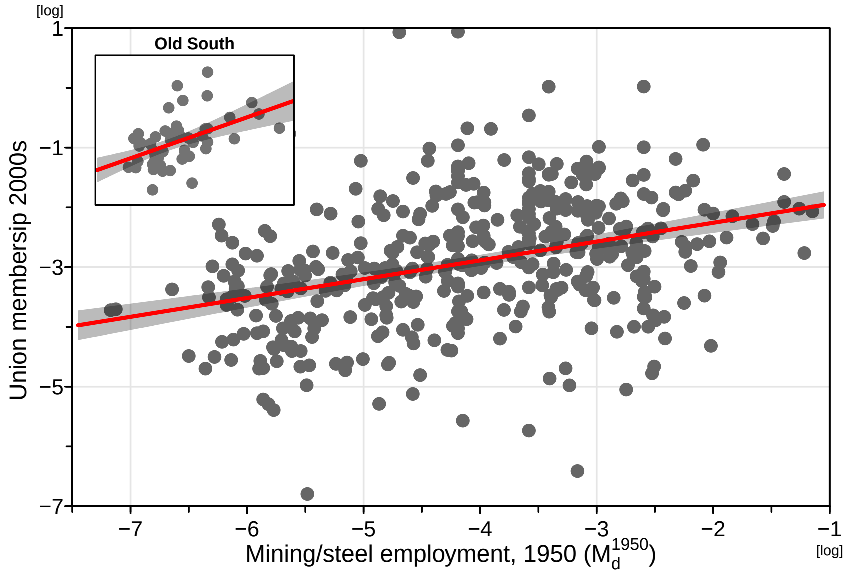


Figure II

Instrumenting union density with post-war mining/steel industry employment

This Figure plots logged district-level employment in mining and steel industries (as share of working population) in 1950 against logged contemporary district-level union density (share of union members among working population). 109th Congress. Linear fit with robust 95% confidence interval superimposed. Upper left plot shows same relationship in Old South states.

which reduce firms' credible exit options, and work conditions. These industries were unionized even in the otherwise much more anti-union South.

The location of mining and even steel production is mainly determined by nature, whether the availability of coal in the ground or the access to raw materials for steel mills. This means that the post-war level unionization in these industries is largely orthogonal to local factors.

Second, over time there are positive spillovers from unionization in mining and steel to other industries in the same location. Using data on union contracts and union elections, Holmes (2006) shows a positive within-state correlation between the unionization of establishments in non-trade sectors (i.e., health care and wholesale) in the 1990s and the proximity to mining and metal industries in the 1950s. Theoretically, this is consistent with unions using their existing resources and staff to organize other establishments in the future as well as social spillovers, which we already discussed before. According to the latter, workers who experience the benefits of union membership or have acquired normative commitments will take this positive attitude to unions to a new job and may transmit it to their social network.

Due to the difficulty of comprehensively measuring union membership in small geographic units, the existing evidence in Holmes (2006) on the relationship between mining/steel employment and subsequent unionization is limited to selected sectors and it ends before our period of analysis. Given the new district-level measure of unionization calculated from mandatory union reports introduced above (Becher, Stegmueller, and Kaeppner 2018), we can test the relationship between a district's historical employment in mining and steel production and overall unionization during the 107-110th Congress. Figure II illustrates that the relationship is positive and statistically and substantively significant. It plots logged district-level employment in mining and steel industries (as share of working population) from the 1950 census against logged contemporary district-level union density (share of union members among working population) in the 109th Congress. An OLS model with state fixed effects suggests that a one percent increase in 1950s employment in mining and steel induces a 0.34 ($\pm$0.04) percent increase in the district-level share of union members.

Taken together, the combination of natural origins and spillovers implies that part of the variation in union membership in congressional districts at the beginning of the twenty-first century is exogenous to local factors conditional on the intensity of mining and steel employment in the middle of the twentieth century. There is also no clear reason to expect that historical unionization in these capital-intensive sectors affects contemporary legislative votes other than through contemporary unionization. Today, for instance, neither mining nor steel are uniformly unionized.

3.3. Measuring votes on trade and compensation

We examine high-profile votes on trade liberalization and compensation during the 107-110th Congress.[15] We follow existing work in selection key votes. On trade, following Owen (2017: 302) we include six votes on free trade agreements (also included in Feigenbaum and Hall 2015) and the vote on granting the president fast-track authority to negotiate trade agreements. Based on interest group ratings from the Cato Institute, voting yes on these votes means supporting trade liberalization.

On compensation, we include ten key votes on economic assistance, social insurance and redistribution identified by the legislative scorecard of Americans for Democratic Action.[16] This includes votes on whether to extend unemployment benefits for people who have exhausted regular benefits, expand job training, housing assistance, food stamps, or increase the minimum wage. These are policies that mitigate economic hardship in areas affected hardest by international competition (Autor, Dorn, and Hanson 2013*a*: 2149). In line with compensation theories (Garrett 1998: 46), it also includes votes on broad increases in social expenditures or tax cuts financed by cutting future social spending. All compensation votes are recoded so that 1 means supporting the pro-compensation direction and 0 means opposing it. See Appendix Table A.1 for a list of included votes.

[15] The raw roll-call votes are available from `http://www.voteview.com`.

[16] See `https://adaction.org/ada-voting-records/` (last retrieved February 19, 2019).

3.4. Instrumental variable approach and results

The basic empirical specification to assess the effect of unionization on congressional voting, separately for trade and compensation, can be written as follows:

$$R_{dc} = \beta U_{dc} + \gamma H_{dc} + \delta X_{dc0} + \eta A_{dc} + \psi_s + \lambda_c + \epsilon_{dc} \tag{6}$$

The dependent variable R_{dc} is a roll-call of representative in district d in congress c. On trade issues, it is coded so that 1 is a vote for trade liberalization and 0 is a vote against. On compensation issues, votes are coded such that 1 is voting in favor of expanding/protecting compensation and 0 is against. On the right-hand side, U_{dc} represent the (logged) share of union members among a district's working population. This is our main variable of interest and it is instrumented using the share of employment in mining and steel in 1950, M_d^{1950}. As before, ψ_s and λ_c are a state- and congress-specific constants.

District-level controls follow the previous analysis, with a small difference in the lag structure, and are added sequentially: X_{dc0} are socio-demographics at the beginning of the 2000s; H_{dc} are historical characteristics matched to current districts; A_{dc} captures automation shocks in the previous decade matched to current districts. When included, controls in X_{dc0}, H_{dc} and A_{dc} are normalized to have mean zero.

In Table III, we present the main results for this section. They are clearly consistent with the view that stronger unions lead to less legislative support for trade deregulation and more support for economic compensation. The first four specifications consider trade votes. Specification (1) includes state and Congress fixed effects. The negative and statistically significant coefficient estimate suggests that a one-standard deviation *decrease* in union membership (i.e., −0.186 in log points) increases legislative support for trade liberalization by about 7 (±1.2) percentage points on average. Adding further district-level covariates in specifications (2) and (3) confirms this finding. Specifications (5)-(7) turn to compensation votes. Again, the finding is qualitatively the same whether district-level controls are included or not. Specification (7), based on all covariates, implies that a one-standard deviation decrease in union membership decreases support for compensation by approximately 5 (±1.1) percentage points. These are politically meaningful magnitudes.

The (robust) F statistic reported in Table III shows that the first stage is strong across specifications. The effect of unions remains significant if we allow for a local violation of the exclusion restrictions (Wang et al. 2018). Moreover, very similar results obtain if each vote is analyzed separately (Appendix Figure B.1).

Columns (4) and (8) of Table III estimate the following partially linear IV specification (see the discussion above and Appendix D):

$$R_{dc} = \beta U_{dc} + \mu(X) + u_{dc}, \quad M_d^{1950} = \xi(X) + \nu_{dc}. \tag{7}$$

In contrast to our previous analysis, conditioning on nonparametric controls strengthens our estimated coefficients for both dependent variables. Note, however, that uncertainty around those estimates also increases considerably. In the case of trade votes, our estimates indicate that a standard deviation decrease in union membership increases the probability of pro-trade legislative votes by about 14 ($\pm$4) percentage points, while for compensation votes they indicate a shift of about 10 ($\pm$3) points.

Table III
Effect of union density on legislative behavior.

	Trade				Compensation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV	IV	IV	dML-IV[a]	IV	IV	IV	dML-IV[a]
Union density	−0.379	−0.372	−0.411	−0.810	0.289	0.219	0.274	0.519
	(0.067)	(0.079)	(0.080)	(0.204)	(0.050)	(0.058)	(0.057)	(0.157)
District characteristics								
Slavery & immigration		✓	✓	✓		✓	✓	✓
Socio-economic comp.		✓	✓	✓		✓	✓	✓
Technological change			✓	✓			✓	✓
State & time FE	✓	✓	✓	✓	✓	✓	✓	✓
IV violation robust tests								
H_0: $\beta = 0$ w. weak IV[b]	0.000	0.000	0.000	–	0.000	0.000	0.000	–
H_0: $\beta = 0$ w. $Cov(Z, e) \approx 0$[c]	0.000	0.001	0.000	–	0.000	0.054	0.003	–
First stage F^d	55.8	44.5	44.5		90.6	73.0	73.8	
N	2969	2969	2927	2927	4189	4189	4132	4132

Note: Robust standard errors. 2SLS estimates. Union density instrumented by share of mining employment in 1950s (see text for detailed discussion; appendix Table B.3 shows that our results also obtain without this instrument). Historical patterns of slavery and immigration proxied by 1910 Census district-shares of Blacks and foreign-born. A district's socio-economic composition includes share of foreign born, Blacks, with college degrees, employed in manufacturing, living in urban areas. Technological change measured by risk of automation (RSH_{jt}).

a Partial linear IV model estimated using debiased machine learning with sample splitting (100 sets) and 5-fold cross-fitting (Chernozhukov et al. 2018). DML2 estimate and s.e. calculated using the median method. See appendix D for detailed model equations and machine learning algorithms used.

b Conditional likelihood ratio test *p*-value for trade shock allowing for weak instruments (Moreira 2009).

c Test of union coefficient allowing for local violation of IV exclusion restriction (Wang et al. 2018).

d F statistic of first stage regression. Robust to heteroscedasticity.

4. Decomposing the effect of trade shocks

So far, we have presented two sets of new results. First, exposure to import competition from China lowers unionization in affected districts. Second, lower unionization entails less legislative support for compensation and less opposition to trade deregulation. In this section, we turn to estimating the impact of the full hypothesized causal chain linking trade shocks to declining union density which in turn affects congressional votes. We ask (i) if there is a significant effect of trade shocks on legislator's votes *via* unionization and (ii) how much of the total impact of trade shocks is due to unions.

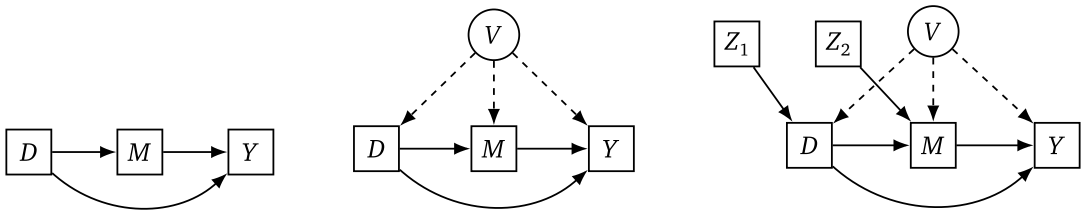
While of great theoretical interest, the identification of such effect decomposition is much more demanding than estimating the causal effect of import competition on unions or legislative behavior. Recent work has stressed that even under the ideal of a randomized treatment (i.e., being able to randomize a trade shock) the mediated effect is not identified without making further assumptions (e.g., Imai, Keele, and Tingley 2010; Imai et al. 2011; Keele 2015). Our analysis of the impact of the trade shock during the 1990s is of course observational compounding issues of identification.

The problem and our empirical approach is illustrated graphically in Figure III. Panel (a) shows a graph representing the theoretical argument that trade (D) impacts union membership (M) which in turn affects roll call votes (Y). The full causal path $D \to M \to Y$ is usually termed the *indirect effect*. Trade may also have a *direct effect* on roll call votes ($D \to Y$), now representing channels other than union density linking the shock to votes. For example, holding union strength constant, the trade shock changes voters' demands, which might be anticipated by rational politicians. In line with such a direct effect Bisbee (2019) finds that local trade shocks are linked to public opinion on trade, immigration, and the role of the US in international affairs (without accounting for local unions).

The logic of this effect decomposition is best illustrated via its constituent counterfactuals (a more complete description is available in Appendix E). Denote the potential vote outcome for an observation (omitting subscripts) under some hypothetical treatment $D = d$ by $Y(d, M(d'))$. This signifies that a roll call vote is driven by (non)exposure to a trade shock as well as union density, $M(d')$, which is itself affected by the shock. The *indirect effect* (Robins and Greenland 1992) of the trade shock is obtained by holding the exposure to the shock constant in both counterfactual scenarios, but shifting union density $M(d)$ from its level without trade exposure $M(0)$ to its level when exposed to the shock $M(1)$.[17] The difference between the two terms is the impact of the trade shock on roll call votes *due to* the change in union density:

$$E\left[Y(1, M(1)) - Y(1, M(0))\right].$$

[17]For simplicity of exposition, we discuss NIE(1) and NDE(1) and omit notation indicating the subpopulation of instrument compliers. More detailed definitions are given in Appendix E.



(a) Classical decomposition **(b) Decomposition with confounding** **(c) Decomposition with two instruments**

Figure III
The problem of causal effect decomposition.

Illustration of effect decomposition. Panel (A) shows typical causal mediation setup hampered by existence of unobserved confounder V which induces exposure-mediator confounding and/or mediator-outcome confounding. Panel (B) shows that a causal effect needs *two* instruments: one two shift trade exposure, one to undo variation in M induced by changes in D.

The *direct effect* of the trade shock is obtained by holding union density constant while shifting the potential outcome from the value it takes absent trade exposure to the value it takes when exposed to the trade shock:

$$E\left[Y(1, M(1)) - Y(0, M(1))\right].$$

The direct effect thus indicates the total effect of the trade shock on roll call votes *net* of the effect the trade shock has on votes via changes in union density.

The identification problem of such a causal structure arises from the likely existence of unobserved confounders. As Panel (b) illustrates, there may be district-level characteristics, V, that affect the strength of unions, roll-call voting behavior and trade exposure. One can think of local preferences, informal institutions or policies playing such a role. Even in the ideal scenario of controlled random assignment of D (or a credible instrument for it), a classical decomposition does not provide a valid estimate of the indirect effect—one can still only identify the causal effects of D on M and Y, but not the causal effect of D on Y *via* M.[18]

Panel (c) illustrates the strategy we employ to tackle this problem. Following the approach proposed by Frölich and Huber (2017) we employ *two* instruments to identify the indirect effect of trade exposure. The role of the first instrument, Z_1, is to generate an exogenous shift in the treatment. In our case, this is the instrument shifting US

[18] Recent work by Imai and coauthors (Imai, Keele, and Yamamoto 2010; Imai et al. 2011) has explicated what additional assumptions, which they call sequential ignorability, need to be maintained to estimate indirect effects. However, these kind of 'double randomization' assumptions (Heckman and Pinto 2015) are best implemented when the researcher has full experimental control and are rather difficult to satisfy in an observational study such as ours.

imports from China based on Chinese imports in other high-income markets. The second instrument, Z_2, enables us to vary union density independently from trade shocks. In our application, Z_2 is a district's mining and steel employment share in 1950 discussed earlier. Intuitively, the role of the second instrument is to undo the changes in M induced by D in order to identify the indirect effect $D \rightarrow M \rightarrow Y$. In Appendix E we show how the relevant potential outcomes needed to identify the direct and indirect effects can be obtained from a semi-parametrically estimated system of equations with two instrumental variables.[19] We set $D = 1(IPW_d > IPW_d^{p70})$, that is, we define an 'intensive trade shock treatment' as occurring whenever a district's import exposure exceeds the 70th percentile of the national distribution.[20]

Table IV shows the resulting estimates. Following our previous analyses, we sequentially add district-level controls. The table entries are indirect and direct effect estimates and their standard errors, as well as the share of the total effect of trade shocks that is mediated by union membership. For compensation roll-call votes, the channel working through local unions is negative and statistically significant. Because it undermines unions, import competition from China also weakens the political forces in favor of supporting those hurt from the distributive effects of trade. While less precisely estimated, the direct effect is also negative. The indirect institutional channel corresponds to roughly 39% of the sum indirect and direct effects.

For trade votes, the effect working through unions is positive and statistically significant. By undermining unions, import exposure weakens opposition to further trade liberalization with other countries. Somewhat puzzling, the direct effect not working through unions has a positive sign (not significant at any conventional level of significance). The relative magnitude of the indirect institutional channel is somewhat larger, around 54%, except if one accounts for the risk of automation.

5. Conclusion

Our analysis indicates that the steep rise in import competition from China after 1989 has shaped congressional support for policies to compensate trade losers as well as trade policy to a significant extent through its negative impact on labor unions. By weakening an economic institution that shapes the political power of workers at the

[19] Independently varying the mediator M is achieved via a control function approach (e.g., Imbens and Newey 2009). We estimate the control function nonparametrically using a kernel conditional density estimator (Li and Racine 2007).

[20] The choice of percentile is arbitrary. We used different plausible thresholds with no substantive difference in results. We also investigated wether the corresponding discretized instrument violates empirical patterns implied by the LATE assumptions (using the strategy suggested by Huber and Mellace 2015), which would strongly suggest its invalidity as instrument in this system. See section F for details. Our results do not show any obvious violations.

Table IV
Decomposing the effect of trade shocks. Semiparametric instrumental variable direct and indirect effect estimates derived from nonparametric system of equations

	Compensation votes			Trade votes		
	Indirect	Direct	%	Indirect	Direct	%
(A) Historic controls						
Chinese import exposure	−0.096	−0.152	38.1	0.063	0.049	57.0
	(0.020)	(0.092)		(0.021)	(0.043)	
(B) A + district controls						
Chinese import exposure	−0.095	−0.149	38.9	0.063	0.048	56.2
	(0.020)	(0.091)		(0.021)	(0.046)	
(C) B + technological change						
Chinese import exposure	−0.094	−0.153	37.0	0.052	0.139	25.6
	(0.019)	(0.098)		(0.022)	(0.094)	

Note: Entries are $NIE(1)$ and $NDE(1)$ estimates for the population of compliers. The % column shows percent of total trade shock on votes mediated via union density. Semiparametric instrumental variable estimates; cf. Appendix E. All specifications include state fixed effects (removed via orthogonal projection). Robust statistics (median and MAD) based on 1,000 bootstrap draws.

district level, import competition reduces House members' support for policies helping those most affected by the distributive effects of trade. It also reduces opposition to trade liberalization. Furthermore, the institutional channel highlighted in our analysis suggests that the political impact of international trade on representation and economic policymaking is likely to last beyond the electoral cycle when its economic impact is felt most acutely. Given the costs and uncertain prospects of trying to unionize the workplace, a large negative shock to union membership, through plant closures or relocation, is difficult to offset. The weakening of organized labor can also be expected to affect the partisan composition in Congress, thus altering representation across a range of issues.

Appendix

A. Data sources

Chinese import exposure. Import exposure data from Autor, Dorn, and Hanson (2013*a*) are defined for groups of counties that create 741 local labor market areas or commuting zones (CZ). We create a crosswalk from commuting zones (in their 1990 definition) to the congressional districts of four Congresses (107th to 110th). For each district, the crosswalk provides its spatial overlap with one or several commuting zones and the fraction of a district's population in the overlap. For example, both Arizona's 5th and 6th districts (in the 109th Congress) are fully contained in one commuting zone (Phoenix-Mesa, AZ) and thus subject to the same labor market shocks. A large fraction (75%) of the population of Arizona's 2nd district is exposed to the same commuting zone, while some fraction (24%) is exposed to a neighboring commuting zone (Las Vegas, NV-AZ). The data for the population fractions in each spatial intersection are based on fine-grained Census tract population counts available from from the MABLE/Geocorr2K Geographic Correspondence Engine. With this crosswalk in hand, we calculate district values for IPW_d^{US} and IPW_d^{oth} as the population-weighted sum of the underlying spatial CZ-district intersections.

1990 and 2000 Census Covariates from the 1990 to 2000 period are obtained from the IPUMS 5 percent sample of the 1990 Census (providing about 1.2 million cases). Census public use data does not contain congressional district identifiers, but instead provides information on counties, or groups of countries, so called 'Public Use Microdata Areas' (PUMA). We use geographic shapefiles for 1990 PUMAs and calculate their spatial (polygon) intersection with Congressional districts using standard GIS tools (specifically, the GEOS C++ library). Information on district geographies for each Congress were compiled by Jeffrey B. Lewis. All 1990 Census covariates are calculated as the weighted sum of PUMAs making up a specific district in a given Congress. For covariates based on the 2000 Census, we rely on data from the NHGIS project, which provides Census SF1 and SF3 tables matched to congressional districts. Again, while the Census values themselves are constant (they refer to 2000) they are matched to each Congress separately in order to account for changing district boundaries.

1910 Census Our historical district characteristics are calculated from the 1910 Census. We use the IPUMS 100% sample (providing about 92.4 million cases). The 1910 Census does not provide information about which district respondents reside in. Instead, it includes so called State Economic Areas (SEA), which were introduced in the 1940 Census and have been added to the IPUMS 1910 Census release. We use geographic shapefiles for 1910/1940 SEAs and calculate their polygon intersection with Congressional districts

(107th to 110th) using standard GIS tools. Based on this crosswalk we spatially weight the Black and foreign-born population in 1910 to current congressional districts.

Roll-call votes Table A.1 lists the selected votes. Roll calls data were retrieved from `http://www.voteview.com`.

Table A.1
Roll-call votes on compensation and trade issues included in the analysis

Roll-call vote	Date	Result	Voteview no.
A. Trade			
Trade promotion authority (presidential "fast-track" authority in trade negotiations), House version	12/06/2001	216-114	476
Free trade agreement Chile implementation act	7/24/2003	271-156	434
Free trade agreement Singapore implementation act	7/24/2003	273-155	430
Free trade agreement Australia implementation act	7/14/2004	315-109	1049
Free trade agreement Morocco implementation act	7/22/2004	324-99	1087
Free trade agreement Dominican Republic-Central America (DR-CAFTA) implementation act	7/28/2005	218-215	442
Free trade agreement Oman implementation act	7/20/2006	222-205	1060
B. Compensation			
Income tax reduction bill (nay-vote is pro-compensation).	3/08/2001	231-198	44
Food stamps (Baca motion).	04/23/2002	244-171	612
Fiscal 2004 budget resolution (nay-vote is pro-compensation).	3/21/2003	215-212	81
Job training reauthorization (Miller motion).	5/08/2003	202-223	173
Extending unemployment benefits (Miller amendment).	2/4/2004	227-179	692
Sanders amendment increasing low-income assistance for home energy.	09/08/2004	305-114	1101
Watt amendment to increase spending for education, job training, homeland security and veterans' programs.	03/17/2005	134-292	84
Nadler amendment to increase spending on housing vouchers.	6/29/2005	225-194	338
Minimum wage increase.	1/10/2007	315-117	17
Increasing spending for the departments of Labor, Health and Human Services, and Education (veto override).	11/15/2007	277-142	1114

Descriptive Statistics Table A.2 shows descriptive statistics for the key variables used in our models.

Table A.2
Descriptive statistics

Statistic	Mean	St. Dev.	Pctl(25)	Pctl(75)
Union membership [/empl. pop]	0.101	0.187	0.029	0.114
Mining employment, 1950s	0.034	0.045	0.007	0.046
Chinese imports to US [/worker, 10-year eq.]	1.137	0.685	0.683	1.316
Chinese imports to other countries	1.001	0.531	0.591	1.261
Routine task intensity	0.371	0.036	0.352	0.395
Historical share of Blacks	0.112	0.161	0.009	0.183
Historical share of Foreign Born	0.151	0.117	0.031	0.223
Manufacturing employment	0.104	0.017	0.098	0.115
Share Black	0.110	0.072	0.073	0.115
Share Urban	0.268	0.053	0.231	0.300
Share with at least college degree	0.332	0.045	0.306	0.342
Median HH income [/1E4]	2.994	0.520	2.680	3.095
Out-migration share [10-year ave.]	0.046	0.013	0.035	0.054
Debt-to-income ratio	1.229	0.280	1.002	1.458
Roll-calls: Trade	0.615	0.487	0.000	1.000
Roll-calls: Compensation	0.554	0.497	0.000	1.000

B. Further model specifications

In this section we discuss a number of alternative model specifications, starting with models relating trade shocks to union density.

Congress-specific estimates Table B.1 shows regressions of union density on trade separately for each Congress. We find the relationship to be very similar across Congresses (and bills). All estimates are statistically distinguishable from zero. Hypothesis tests allowing for weak instruments and local-to-zero violations of the exclusion restriction (not shown here) also confirm the overall results in the main text.

Table B.1
Effect of trade shocks on union membership by Congress.

	107th	108th	109th	110th
Chinese import penetration	−0.241	−0.297	−0.330	−0.312
	(0.110)	(0.117)	(0.121)	(0.143)

Note: N=432. IV estimates for $U_d = \beta \Delta IPW_d + H_d' \gamma + \psi_i + \epsilon_i$ with robust standard errors.

Table B.2
Robustness tests for the effect of trade shocks on union membership.

	Coef.
(1) OLS (no instrument)	−0.205
	(0.034)
(2) Contemporary district controls (X_{dc}^{2000})	−0.278
	(0.069)
(3) Automation instrument	−0.287
	(0.061)
(4) Offshorability index	−0.302
	(0.059)
(5) Gravity-based trade measure	−0.378
	(0.087)

Note: (2)-(4) are 2SLS estimates, (1) and (5) are OLS. Robust standard errors.

Robustness tests for trade → union density estimates In specification (1) of Table B.2 we show that the effect of trade on district-level union membership also obtains when we account for contemporary values (derived from the 2000 Census) of district-level controls. These district-level characteristics, such as the share of college graduates, might be partially endogenous to realized trade shocks in the period prior. Nonetheless, these results substantiate the point that district differences in union membership are not simply a mirror image of the current economic or demographic structure of districts. In our main models we account for district levels of exposure to automation measured as fraction of workers with high-routine task jobs. However, the prevalence of (or shift away from) routine-task employment might itself be the results of trade shocks and related changes in unionization levels. In specification (2) we thus instrument district-level routine task intensity with its 1950s levels following Autor, Dorn, and Hanson (2015). We find that this does not change our overall results. In specification (3) we add a measure of offshorability based on Autor and Dorn (2013). This captures the impact that the falling cost of automating routine, codifiable tasks has on employment shares in exposed job categories in congressional districts. Note that the measure is one of *potential offshorability*, i.e., a measure to what extent jobs in a given district are likely at risk of outsourcing, not one of jobs actually lost (Autor and Dorn 2013: 1584). We find that adjusting for changing offshorability exposure does not alter our core findings. Finally, in specification (4) we replace the Chinese imports measure (and its instrument) with the residual from a gravity equation, which uses inferred changes in China's comparative advantage and market access (see Autor, Dorn, and Hanson (2013*a*) for more details on its construction) and reestimate our model using OLS with two-way fixed effects. We find our core results confirmed.

Robustness tests for union density → votes estimates Turning to the effect of union membership on legislative behavior, Table B.3 lists a series of robustness tests. In specification (1), we show that our core results also obtain without relying on the mining instrument (note that under this specification the table entry now refers to a different parameter). Specification (2) shows that our results do not depend on the set of district-level controls used in the IV regression. Specification (3) shows that omitting time effects and including fixed effects for each roll call does not change our results. Finally, specification (4) includes the measure of offshorability as discussed for specification (3) in Table B.2. We find our core findings unchanged by this adjustment.

Table B.3
Robustness tests for the effect of union membership on roll call votes.

	Trade	Compensation
(1) OLS (no instrument)	−0.118	0.146
	(0.009)	(0.007)
(2) IV, no controls	−0.379	0.289
	(0.067)	(0.050)
(3) Roll call FE	−0.410	0.276
	(0.080)	(0.056)
(4) Offshorability index	−0.372	0.219
	(0.079)	(0.058)

Note: 2SLS estimates (except (1)). Robust standard errors.

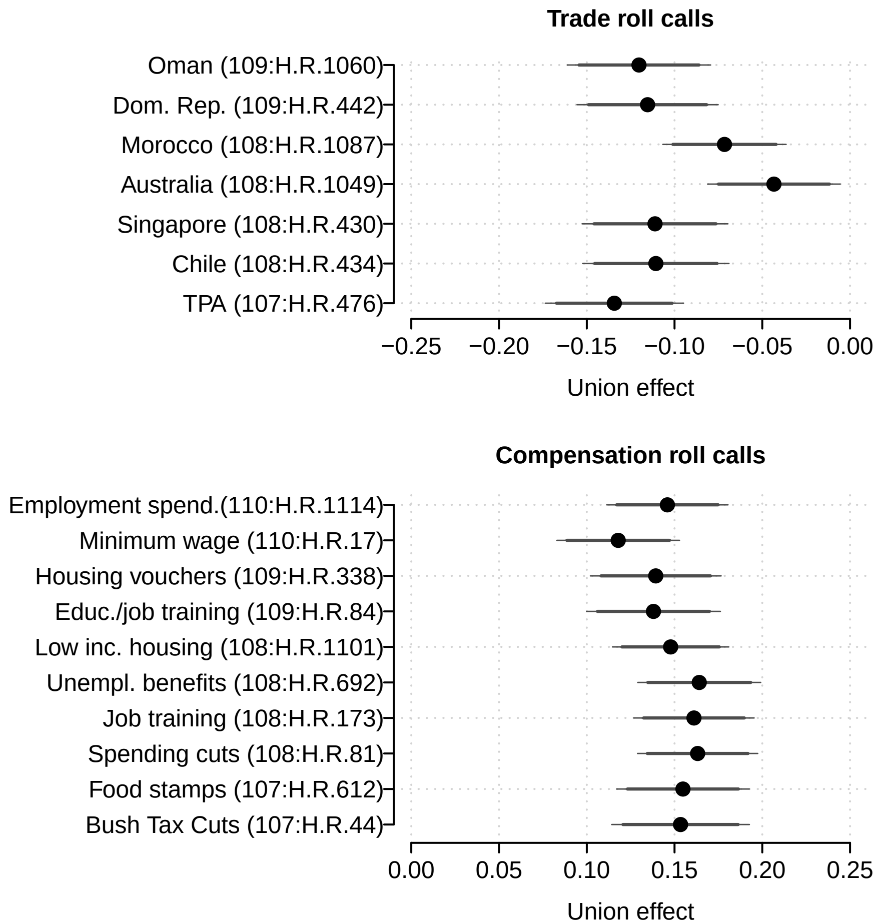


Figure B.1
Union density and legislative votes. Estimates by roll call with 90% and 95% confidence intervals (based on robust variance estimates)

Roll call-specific estimates Figure B.1 shows estimates separate for each roll call vote (with correspondingly smaller sample sizes). It shows that our core results do not depend on the pooling of all observations and the assumption of common effects for trade and compensation roll call votes.

C. Invalid IV robust inference

In generic notation, for outcome Y, treatment D and instrument Z the structural model is:

$$Y = X\beta_X + \beta D + \delta Z + u \tag{C.1}$$

$$D = X\gamma_X + \gamma Z + v, \tag{C.2}$$

with errors u and v bivariate normally distributed and independent of Z. After projecting out covariates using the FWL theorem, and rescaling δ we get

$$Y = \beta T^* + \delta\sigma_1 Z^* + u^* \tag{C.3}$$

$$D^* = \gamma Z^* + v^*, \tag{C.4}$$

where quantities with star superscripts obtained by multiplying them with a projection matrix, and the rescaled sensitivity parameter $\delta\sigma_1$ now expresses the impact of a unit change in the instrument on the structural error in δ standard deviations.[21] In our analyses, we assume that $\delta \in (-\bar{\delta}, +\bar{\delta})$ and present p-values from the corresponding Andersen-Rubin test $H_0 : \beta = \beta_0$ with given δ (cf. Wang et al. 2018):

$$AR(\beta_0) = \frac{(Y^* - \beta_0 D)' P_{Z^*}(Y^* - \beta_0 D)}{(Y^* - \beta_0 D)' M_{Z^*}(Y^* - \beta_0 D)/(n-k-1)} \tag{C.5}$$

which follows a non-central F distribution with degrees of freedom 1 and $n-k-1$ and non-centrality parameter $\delta^2 Z^{*'} Z^*$.

We set a rather large value of $\bar{\delta} = 0.1$, which implies that a unit change in the instrument has an up to 0.1 standard deviation effect on the structural error. Note that the test might have low power for large values of the sensitivity parameter due to the large sample size required (Wang et al. 2018: 1157).

[21] The relevant projection matrices are $P_{Z^*} = Z^*(Z^{*'} Z^*)^{-1} Z^{*'}$ and $M_{Z^*} = I_n - P_{Z^*}$, where I_n is a $n \times n$ identity matrix.

D. Debiased Machine Learning Estimation of Partially Linear IV Model

In general form, the model we estimate is given by the following, partially linear, form:

$$Y = D\theta_0 + g_0(X) + \zeta \tag{D.1}$$

$$Z = m_0(X) + V \tag{D.2}$$

with $E(\zeta|X,Z) = 0$ and $E(V|X) = 0$. Here Y is an outcome, say union density, D is a treatment, say Chinese import exposure, and Z is the corresponding instrument. Z induces variation in D and is exogenous conditional on X. Note that we make no functional form assumptions on the structure of the set of controls X; g_0 and m_0 are nonlinear functions. This is essentially a semiparametric estimation problem in an instrumental variable context (see, e.g., Robinson (1988) for a classic treatment, and Van der Laan and Rose (2011) for more recent work in a targeted learning framework). Note further that confounding of the instrument occurs when elements of X influence not only the outcome but also the instrument via the function $m_0 \neq 0$.

One can re-express this model in residualized form

$$W = U\theta_0 + \zeta, \quad E[\zeta \mid V,X] = 0, \tag{D.3}$$

$$W = (Y - \ell_0(X)), \quad \ell_0(X) = E[Y \mid X] \tag{D.4}$$

$$U = (D - r_0(X)), \quad r_0(X) = E[D \mid X] \tag{D.5}$$

$$V = (Z - m_0(X)), \quad m_0(X) = E[Z \mid X], \tag{D.6}$$

where the variables above 'partial out' the effect of X.[22]

In this setup, all three nuisance parameter are conditional mean functions, which we learn using standard machine learning tools, such as neural networks. With estimates of

[22] This setup uses the score function

$$\psi(Y,D,Z,X;\theta,\eta) := (Y - \ell(X) - \theta(D - r(X)))(Z - m(X)), \;\; \eta = (\ell, m, r), \tag{D.7}$$

where ℓ, m and r and P-square-integrable functions mapping X to $\mathbb{R}$. For $\eta_0 = (\ell_0, m_0, r_0)$ with $\ell_0(X) = E_P(Y|X)$ and $r_0(X) = E_P(D|X)$ it satisfies both the moment condition (Chernozhukov et al. 2018: eq.2.1)

$$E_P[\psi(Y,D,Z,X;\theta_0,\eta_0)] = 0 \tag{D.8}$$

and the Neyman orthogonality condition (Chernozhukov et al. 2018: eq.2.3)

$$\partial_\eta E_P[\psi(Y,D,Z,X;\theta_0,\eta_0)][\eta - \eta_0] = 0. \tag{D.9}$$

The (Gateaux) derivative being equal to zero ensures that our score function is robust to small pertubations of the nuisance functions estimates.

$\hat{\ell}_0$, $\hat{m}_0$, and $\hat{r}_0$ in hand, we can estimate residuals (similar to Robinson 1988)

$$\hat{W} = Y - \hat{\ell}_0(X) \tag{D.10}$$

$$\hat{U} = D - \hat{r}_0(X) \tag{D.11}$$

$$\hat{V} = Z - \hat{m}_0(X) \tag{D.12}$$

and use these to estimate our parameter of interest. We estimate θ_0 by 2SLS regression of $\hat{W}$ on $\hat{U}$ with $\hat{V}$ as instrument. The resulting estimator for θ_0 is approximately standard normally distributed (Chernozhukov et al. 2018: Theorem 4.2) and we can conduct inference in the usual way.

We estimate $\hat{\ell}_0$, $\hat{m}_0$, and $\hat{r}_0$ using four different ML tools in 100 split samples (for earlier discussions of sample splitting see Belloni et al. 2012 and Robins et al. 2013: appendix C).[23] In each split, we use 5-fold cross-fitting. One issue with using increasingly flexible ML models (in contrast to, say, the square-root LASSO we use in Becher, Stegmueller, and Kaeppner (2018)) is that there are a larger number of hyperparameters that influence the quality of the function approximation. We thus tune the most important parameters using 10-fold cross-validation (in each split). See Table D.1 for parameter details and estimates. We find that different ML tools yield rather similar estimates for θ_0. The last column of Table D.1 shows results where we combine the best performing (in terms of predictive accuracy) ML algorithm for each of the three nuisance functions and reestimate the model.

The estimates in our main tables are based on this combined model. We use the robust median method (Chernozhukov et al. 2018: C30) to summarize estimates from 100 sample splits (using the median estimate and median deviations).

For reasons of space, our discussion here is rather informal. See Chernozhukov et al. (2018) for more details, especially on the relevance of the Neyman orthogonality condition to tackle the regularization bias arising from a naive application of machine learning tools and the use of sample splitting to limit bias from overfitting (as well as the use of cross-fitting to regain efficiency lost from sample splitting).

[23]The results in Chernozhukov et al. (2018) imply that the ML estimators employed should converge at a rate of at least $o(n^{-1/4})$. Such results are only beginning to be available for a subset of ML tools. For example, Chen and White (1999) provide rate results for the single hidden layer feed-forward neural network we use here (but limited to i.i.d. data). Thus, we stick to a rather 'conservative' set of machine learning tools.

Table D.1
Debiased Machine Learning Instrumental Variable Estimates.

	Reg.trees[a]	Boosting[b]	R. Forest[c]	Neural N.[d]	Best[e]
(A) Union density					
Estimate	−0.197	−0.195	−0.192	−0.188	−0.198
SE	(0.038)	(0.035)	(0.033)	(0.033)	(0.040)
$MSE(Y\|X)$	0.985	0.975	0.891	0.999	
$MSE(D\|X)$	0.959	0.923	0.803	0.989	
$MSE(Z\|X)$	0.959	0.933	0.817	1.049	
(B) Trade roll calls					
Estimate	−0.843	−0.839	−0.677	−0.710	−0.810
SE	(0.198)	(0.179)	(0.129)	(0.128)	(0.204)
$MSE(Y\|X)$	0.992	0.956	0.931	0.996	
$MSE(D\|X)$	0.971	0.899	0.813	0.999	
$MSE(Z\|X)$	0.961	0.834	0.704	0.988	
(C) Compensation roll calls					
Estimate	0.552	0.560	0.515	0.557	0.519
SE	(0.132)	(0.132)	(0.095)	(0.094)	(0.157)
$MSE(Y\|X)$	0.990	0.952	0.907	0.995	
$MSE(D\|X)$	0.972	0.885	0.782	0.999	
$MSE(Z\|X)$	0.944	0.809	0.651	0.989	

Note: Based on debiased Machine Learning Instrumental Variable estimates using 5-fold cross-fitting on 200 sample splits. DML2 (Chernozhukov et al. 2018: Def.3.2) estimates and standard errors computed using the median method (Chernozhukov et al. 2018: Def.3.3). Mean squared error of prediction averaged over 100 sample splits.

a Classification and Regression Tree using ANOVA splitting with complexity parameter tuned via 10-fold cross-validation.

b Boosted Regression Trees using Gaussian distribution with 1000 trees with regularization parameters chosen by 10-fold cross-validation.

c Random forest averaging over 1000 trees.

d Neural Network with two neurons and linear activation function. Decay parameter tuned via 10-fold cross-validation.

e Uses different ML methods for different nuisance functions. Choice of 'best' algorithm for each nuisance function is based on averaged out-of-sample prediction performance.

E. Causal effect decomposition with two instruments

We have the following nonparametric system of equations that describes the relationship between an intense trade shock (D, treatment), degrees of union strength (M, mediator), and the outcome Y:

$$Y = \lambda(D, M, X, U) \tag{E.1}$$

$$M = \mu(D, Z_2, X, V) \tag{E.2}$$

$$D = \mathbf{1}(\xi(Z_1, X, W) \geq 0) \tag{E.3}$$

Here, λ, μ, ξ are unknown functions, X is a set of covariates, and Z_1 and Z_2 are instruments. $\mathbf{1}(\cdot)$ is an indicator function equal to 1 if its argument is true and 0 otherwise. Unobservables U, V, W can be related in arbitrary fashion leading to endogeneity of D and M. Frölich and Huber (2017) propose to deal with this endogeneity in two ways: endogeneity of D is addressed using standard semiparametric LATE-type assumptions (Abadie 2003), endogeneity in M is addressed using a nonparametric control function approach.

Key causal parameters First, however, we define the relevant potential outcomes that are needed to define the total, direct, and indirect causal paths of our decomposition. Under treatment $d, d' \in \{0, 1\}$, the potential outcome is given by $Y(d, M(d'))$, while the potential mediator is given by $M(d)$. In terms of the model equations defined above, these parameters are defined as:

$$M(d) = \mu(d, z_2, X, V) \tag{E.4}$$

and

$$Y(d, M(d') = \lambda(d, M(d'), X, U) = \lambda(d, \mu(d', Z_2, X, V)), X, U). \tag{E.5}$$

Finally, the potential treatment state $D(z_1)$ for $z \in \{0, 1\}$ is given by

$$D(z_1) = \mathbf{1}(\xi(z_1, X, W) \geq 0). \tag{E.6}$$

In our decomposition the key causal parameters of interest are (Robins and Greenland 1992; Pearl 2001): the total effect (LATE), the effect of the treatment via the mediator (NIE, the natural indirect effect), and the remaining natural direct effect (NDE). They are given by:

$$\begin{aligned} \text{LATE} &= E\{Y(1) - Y(0) | \Pi = co\} && \text{(E.7)} \\ &= E\{Y(1, M(1)) - Y(0, M(0)) | \Pi = co\} \\ NDE(d) &= E\{Y(1, M(d)) - Y(0, M(d)) | \Pi = co\} && \text{(E.8)} \\ NIE(d) &= E\{Y(d, M(1)) - Y(d, M(0)) | \Pi = co\} && \text{(E.9)} \end{aligned}$$

for $d \in \{0, 1\}$ and where $\Pi = co$ denotes the set of compliers (where $D(1) - D(0) = 1$; cf. the discussion in the next section). Note that NDE and NIE are heterogeneous over treatment, thus allowing for treatment-mediator interactions.

Control function approach The endogeneity of the mediator could be addressed trivially by conditioning on V if it were observable. Because it is unobserved, replace it with a control function (Imbens and Newey 2009). The control function $C = C(M, D, Z_2, X)$ is given by

$$\begin{aligned} C(m, d, z_2, x) = & \frac{E\{[d + D - 1][Z_1 - \pi(x)] | M \leq m, Z_2 = z_2, X = x\}}{E\{D[Z - \pi(x)]| Z_2 = z_2, X = x\}} \\ & \times F_{M|Z_2,X}(m, z_2, x). && \text{(E.10)} \end{aligned}$$

Here, $\pi(x)$ denotes the propensity score of the trade shock instrument, $P(Z_1 = 1|X)$, and $F_{M|Z_2,X}$ denotes the conditional cumulative distribution of union strength conditional on the union instrument and controls. Under the assumptions listed above, it can be shown that C is a one-to-one mapping of V (Frölich and Huber 2017, Lemma 1). Thus conditioning on C controls for the endogeneity of the mediator. We estimate the control function nonparametrically. The conditional expectations of (E.10) are estimated via least squares while the cumulative distribution function (the second term) is estimated using nonparametric kernel density estimation with a second order Gaussian kernel (Li and Racine 2007).[24]

Assumptions Next to the standard instrumental variable conditions (both instruments are relevant and satisfy their exclusions; monotonicity), identification of the causal decomposition parameters rests on these assumptions:[25]

- $(Z_1, Z_2) \perp (U, V, W)|X$, instruments are independent of unobservables given X
- $Z_1 \perp Z_2|X$, the instrument for the treatment is independent of the instrument for the mediator after conditioning on controls

[24]We select the kernel bandwidths using the normal-reference approximation of Silverman (1986).

[25]We also require a common support restriction that $0 \geq P(Z_1 = 1|M, V, X, D(1) - D(0) = 1) \leq 1$ a.s., which amounts to the restriction that the weights (see below) do not approach infinity.

- strict monotonicity of mediator in V which is either a scalar unobservable or a latent index (cf. Frölich and Huber 2017: 1651)

Estimable causal parameters Now the potential outcomes can be identified:

$$E\{Y(1,M(1))|\Pi = co\} = \frac{E\{Y\,D\,\Psi\}}{E\{D\,\Psi\}} \tag{E.11}$$

$$E\{Y(1,M(0))|\Pi = co\} = \frac{E\{Y\,D\,\Omega\,\Psi\}}{E\{D\,\Psi\}} \tag{E.12}$$

$$E\{Y(0,M(1))|\Pi = co\} = \frac{E\{Y\,(D-1)\,\Omega^{-1}\,\Psi\}}{E\{D\,\Psi\}} \tag{E.13}$$

$$E\{Y(0,M(0))|\Pi = co\} = \frac{E\{Y\,(D-1)\,\Psi\}}{E\{D\,\Psi\}} \tag{E.14}$$

where $\Psi = Z_1/\pi(X) - (1-Z_1)/(1-\pi(X))$ and weights are given by (Frölich and Huber 2017: 1653)

$$\Omega = \frac{E\{(D-1)(Z_1 - P(Z_1 = 1))|M,C\}}{E\{D(Z_1 - P(Z_1 = 1))|M,C\}} \tag{E.15}$$

With an estimate for the control function obtained in a previous step, we estimate the conditional expectations and propensity scores in the potential outcome equations using probit models.

With these potential outcomes in hand, our key causal decomposition parameters can be obtained straightforwardly. For example, the natural indirect effect in (E.9) for $d = 1$ is defined as $E\{Y(1,M(1)) - Y(1,M(0))|D(1) - D(0) = 1\}$ and can thus be obtained by subtracting (E.12) from (E.11). The corresponding natural direct effect, defined as $E\{Y(1,M(1)) - Y(0,M(1))|D(1) - D(0) = 1\}$, is obtained by subtracting (E.13) from (E.11).

A common issue with inverse probability weighting type estimators is that some observations might carry unduly large weights (Huber, Lechner, and Wunsch 2013). To limit large relative weights, which are determined by $D, D-1, \Psi, \Omega$ and Ω^{-1} in in the mean potential outcome equations (E.11) to (E.14), we use trimming such that the maximum relative weight for an observation is 5%. Standard errors are obtained via 1,000 bootstrap draws (see Frölich and Huber 2017: 1653 for a discussion of the validity of the bootstrap in this setting).

F. Instrument validity tests

In this subsection we describe the tests we carried out to inspect if our discretized instruments used in section 4 violate LATE assumptions. Of course, one should stress that the validity of an instrument will never be *confirmed*—it can only be *refuted* (Breusch (1986) provides an excellent discussion of testing nonverifiable assumptions). However, subjecting observable implications of a model to more empirically refutable tests should increase our confidence in the conclusions drawn from that model.

In section 4, we have a treatment $D \in \{0,1\}$ and a corresponding instrumental variable $Z \in \{0,1\}$. $D(z)$ denotes the potential treatment state for $Z = z$ and by $Y(d)$ the potential outcome for $D = d$. Following Angrist, Imbens, and Rubin (1996), the population of districts can be divided into four groups, $\Pi \in \{al, co, de, ne\}$, as a function of potential treatments:

Π	$D(1)$	$D(0)$
(co)mpliers	$D(1) = 1$	$D(0) = 0$
(al)ways takers	$D(1) = 1$	$D(0) = 1$
(ne)ver takers	$D(1) = 0$	$D(0) = 0$
(de)fiers	$D(0) = 0$	$D(0) = 1$

In our context, using union density and our mining instrument as an example, this language translates to the following: (co) are districts with a higher level of union density when historically exposed to mining; (al) are districts with already high levels of union density (and having previously been exposed to mining); (ne) are districts that do not have high levels of union density (and were not historically exposed to mining). Finally, and importantly, (de) are districts where historical exposure to mining leads to a *decrease* in union density.

The local average treatment effect, $E(Y(1) - Y(0)|\Pi = co)$, is identified from the following assumptions:

1. $E(Y(d)|\Pi = \pi, Z = z) = E(Y(d)|\Pi = \pi)$ for $d, z \in \{0,1\}$ and for $\Pi \in \{al, co, ne\}$
   (Exclusion restriction)

2. $P(\Pi = \pi|Z = 1) = P(\Pi = \pi|Z = 0)$ for$\Pi \in \{al, co, ne\}$
   (Unconfounded type)

3. $P(\Pi = de) = 0$
   (No defiers)

4. $P(\Pi = co) > 0$
   (Some compliers)

These assumptions imply a set of testable restrictions (see, e.g., Heckman and Vytlacil 2005: Prop. A.5). Huber and Mellace (2015) show that assumptions 1 to 3 imply the following inequalities

$$E(Y|D=1,Z=1,Y\leq y_q)\leq E(Y|D=1,Z=0)\leq E(Y|D=1,Z=1,Y\geq y_{1-q}) \quad \text{(F.1)}$$
$$E(Y|D=0,Z=0,Y\leq y_r)\leq E(Y|D=0,Z=1)\leq E(Y|D=0,Z=0,Y\geq y_{1-r}) \quad \text{(F.2)}$$

where q is the share of (al)ways takers conditional on $D=1,Z=1$:

$$q=\frac{P(D=1|Z=0)}{P(D=1|Z=1)} \quad \text{(F.3)}$$

and r is the rth quantile of Y given $D=0,Z=0$.

Intuitively equation D.1 shows that, in the absence of (de) types, a district with $D=1$ and $Z=0$ has to be of type (al) and thus $E(Y|D=1,Z=0)$ identifies the mean potential outcome of (al)ways takers in the treated state. It lies within the bounds of the means of the upper and lower outcome proportions where $D=1,Z=1$, which corresponds to share of (al) types in a mixed population with (co)mpliers: $E(Y|D=1,Z=1,Y\leq y_q)$ and $E(Y|D=1,Z=1,Y\geq y_{1-q})$, respectively. This produces an empirically verifiable statement. If $E(Y|D=1,Z=0)$ does not lie within these bounds assumptions 1 to 3 are necessarily violated. The same logic applies to the mean potential outcome of (ne) types under absence of treatment in equation D.2. The relevant test has to (simultaneously) evaluate these two moment inequalities. It can be based on the smoothed indicator method of Chen and Szroeter (2014), labeled *CS* in Table F.1, or the partially-recentered double bootstrap approach proposed by Bennett (2009), labeled *B.p*. More details can be found in Huber and Mellace (2015).

An alternative test is proposed by Kitagawa (2015) who derives inequality constraints using full statistical independence (as opposed to mean independence used in assumptions 1 and 2 above) between the instrument and potential treatment/outcomes. The relevant constraints are

$$Pr(Y\in S,D=1|Z=1)\geq Pr(Y\in S,D=1|Z=0) \quad \text{(F.4)}$$
$$Pr(Y\in S,D=0|Z=0)\geq Pr(Y\in S,D=0|Z=1). \quad \text{(F.5)}$$

Here S denotes a subset of the support of the outcome variable. If the above inequalities do not hold, the share of (co)mpliers in subset S given Z would be negative in violations of the assumptions we started with. Kitagawa (2015) proposes to use a (resampled) variance-weighted two-sample Kolmogorov-Smirnov statistic using the maximum of $\sup\{Pr(Y\in S,D=1|Z=0)-Pr(Y\in S,D=1|Z=1)\}$ and $\sup\{Pr(Y\in S,D=0|Z=1)-Pr(Y\in S,D=0|Z=0)\}$ across multiple subsets S (Kitagawa 2015: 2050). This test statistic thus quantifies the maximal violation of the inequalities defined above. In our application, we employ ten subsets and 999 bootstrap samples.

Table F.1
Instrument validity tests based on observed violations of LATE-implied parameter restrictions.

Outcome	Treatment	IV	Huber-Mellace CS p	Huber-Mellace B.p p	Kitagawa KS p
U_{dc}	ΔIPW_{dc}	ΔIPW_{dc}^{IV}	0.998	1.000	0.566
R_{dc}^{trade}	U_{dc}	M_d^{1950}	0.988	1.000	1.000
R_{dc}^{comp}	U_{dc}	M_d^{1950}	0.986	1.000	1.000

Note: LATE implied instrument validity tests based on treatment and instrumental variables discretized at 6th decile. Columns CS and $B.p$ list p-values from tests of inequalities based on Chen and Szroeter (2014) and Bennett (2009) following Huber and Mellace (2015). The $B.p$ test uses a double bootstrap with 999 draws for each stage. Column KS lists p-values from variance-weighted Kolmogorov-Smirnov test (resampled using 999 draws) following Kitagawa (2015).

Table F.1 shows p values for the tests discussed above. In all three cases the p values are rather large and we do not reject the null hypothesis of instrument validity in this specific application.

G. Accounting for public unions

A potential drawback of using LM forms is that some public sector unions are not required to file them. However, this exclusion only applies to public unions that *exclusively* represent state, county, or municipal government employees. Any union that covers at least one private sector employee is required to file. In practice, this leads to almost complete coverage, because unions are now increasingly organizing workers across different sectors and occupations (Lichtenstein 2013: 249).

Thus, calculating national aggregates based on LM forms is in close agreement with measures from the Current Population Survey (Hirsch, Macpherson, and Vroman 2001): the former estimates 13.21 million union members (excluding Washington, D.C.) while the latter yields 15.22 million. This difference is consistent with some degree of over-reporting in the (survey-based) CPS (Southworth and Stepan-Norris 2009: 311). It can also be interpreted as an upper bound for the non-coverage of some public sector unions. A more disaggregated (state-level) analysis in Becher, Stegmueller, and Kaeppner (2018) finds that LM form-based union membership numbers correlate highly with the CPS at $r = 0.86$.

While our measure likely provides good coverage of total union membership numbers (private and public) it does not capture the fact that public unions may act differently in the political arena. A recent strand of research stresses the peculiar characteristics of public unions and their political influence (e.g., Anzia and Moe 2016; Flavin and Hartney 2015). It is thus germane to ask if our results hold when partialling out the influence of public unions in a given district.

As discussed above, the administrative forms we use to measure union membership do not contain information on the public vs. private status of a union. Local unions may contain workers from both the private and the public sector. We calculate an approximate measure of district public union membership by identifying unions with public sector members based on their name using partial string matching. We then generate separate membership counts for “public”unions and calculate two quantities: (i) the share of total union members in a district that are members of a public union,[26] (ii) the number of employees in public union members in a district. While clearly being an approximation, our public union measures capture all the large public unions that play an important role in the political process. They include the National Education Association, American Federation of Teachers, American Federation of Government Employees, National Association of Government Employees, American Federation of State, County & Municipal Employees, United Public Service Employees Union, National Treasury Employees Union, American Postal Workers Union, National Association of Letter Carriers, Rural Letter Carriers Association, National Postal Mail Handlers Union, National Alliance of Postal

[26]Note that this is distinct from union members *employed* in the public sector, as self-reported in the standard CPS data

and Federal Employees, Patent Office Professional Association, National Labor Relations Board Union, International Association of Fire Fighters, Fraternal Order of Police, National Association of Police Organizations, various local police associations, and various local public school unions.

Table G.1
Accounting for public unions. IV estimates of local union membership effects adjusting for public union strength.

	Compensation			Trade		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Adjusted for share of members in public unions						
Union membership	0.266	0.251	0.303	−0.392	−0.410	−0.447
	(0.053)	(0.061)	(0.061)	(0.075)	(0.089)	(0.090)
Invalid IV robust p^a	0.002	0.027	0.001	0.000	0.000	0.000
First stage F^b	76.1	64.2	65.9	44.9	37.4	38.1
B: Adjusted for number of public unions members						
Union membership	0.225	0.214	0.269	−0.341	−0.353	−0.395
	(0.049)	(0.055)	(0.055)	(0.063)	(0.071)	(0.073)
Invalid IV robust p^a	0.008	0.040	0.002	0.000	0.000	0.000
First stage F^b	128.8	106.5	102.8	81.0	67.8	64.8
State & time FE	✓	✓	✓	✓	✓	✓
District characteristics						
Slavery Immigration	✓	✓	✓	✓	✓	✓
Socio-economic comp.		✓	✓		✓	✓
Technological change			✓			✓
N	4189	4189	4132	2969	2969	2927

Note: 2SLS estimates of effect of logged union membership on roll call votes with robust standard errors in parentheses. Union density instrumented by share of mining employment in 1950s (see text for detailed discussion). Panel A includes as additional control the estimated *share* of union members that are in a public union. Panel B includes the *number* of members in public unions. To reduce skewness, both variables are cuberoot transformed. For details on other district-level controls see Table III.

a Test p-value of union coefficient allowing for local violation of IV exclusion restriction (Wang et al. 2018).

b F statistic of first stage regression. Robust to heteroscedasticity.

In Table G.1 we report IV estimates of the effect of union membership on roll call votes (cf. Table III in the main text) after adjusting for public union membership.[27] Panel (A) accounts for the ratio of union members employed in a union classified as 'public'

[27] We include public unions (as share and in raw numbers) among the set of district controls. Thus, they are not instrumented by mining employment shares. Of course, the endogeneity concerns we cited as impetus for instrumenting union membership also apply *mutatis mutandis* to public union membership. Including a potentially endogenous covariate as 'control' is problematic. Our aim here is simply to

to all union members in a given district. Panel (B) adjusts for the number of members employed in public unions. Using both strategies, we find our core results confirmed. Adjusting for district-level public union strength changes our main union membership parameter estimates only minimally (and slightly increases standard errors). The *p* values for tests of the effect of union membership, which simulate a degree of violation of the strict exclusion restriction, still indicate a significant relationship between union strength and roll call votes on free trade and economic compensation.

show that our main results change very little—without making further statements about public union estimates.

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