Global Competition, Local Unions, and Political Representation: Disentangling Mechanisms *

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Abstract

While recent scholarship has demonstrated multiple political effects of international trade, less attention has been paid to unbundling the mechanisms through which import competition affects democratic politics. One mechanism, in theory, works through labor unions as domestic countervailing powers shaping legislative responses on compensation and trade votes. We assess the relevance of unions as a mediating variable in the US Congress. For identification, we leverage two distinct sources of exogenous variation, one instrument for import exposure and another for unionization, and combine them in a semiparametric estimator. We find that (i) import competition lowers district-level unionization, (ii) weaker unions lead to less legislative support for compensating economic losers and less opposition to trade deregulation, and (iii) the union mechanism represents a large fraction of the overall effect of import exposure on legislative votes. The results help explain weak compensation and further trade liberalization in the face of rising global competition.

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Introduction

The success of democratic capitalism rests on its ability to accommodate the sharp distributive conflicts resulting from exposure to global markets and technological change. When international trade lowers the wages or destroys the jobs of a significant number of workers, compensating trade losers through social insurance, retraining, and income redistribution can enhance efficiency, dampen economic inequality, and may even limit dissatisfaction with democracy. However, research in political science and economics demonstrates that compensation is not automatic. Winners from economic change, and the politicians representing them, may have few incentives to allocate resources to economic losers and possess the political power to block such policies.

To the degree that unequal economic resources entail unequal political influence biased toward economic elites and corporations, countervailing powers, which reduce this bias, help to make democracy work. Labor unions remain a major countervailing power for non-elite workers.[1] Not only do unions increase wages (Ahlquist 2017; Farber et al. 2021), they also reduce unequal political responsiveness in a new gilded age characterized by high economic inequality in developed economies (Becher and Stegmueller 2021; Flavin 2018). But what if economic shocks that increase the need for policies to support economically disadvantaged citizens simultaneously undermine the labor unions that give them voice? Such a development would further weaken the political representation of trade losers and undermine the system of "embedded liberalism" forged after World War II, which combines trade openness with social protection (Ruggie 1982; Mansfield and Rudra 2021). Disillusionment with the promise of democracy and growing receptiveness to identity politics and populism may follow (Bisbee et al. 2020; Rodrik 2018).

The dramatic rise of import competition from China and other developing countries since the 1990s has magnified the challenges faced by democratic capitalism. It has also provided new opportunities for research on how global markets shape domestic politics. Building on research that studies its economic effects (e.g., Autor, Dorn, and Hanson 2016), a burgeoning literature has studied the political effects of the China shock (Autor et al. 2020; Ballard-Rosa et al. 2021; Bisbee et al. 2020; Bisbee and Rosendorff 2021; Colantone and Stanig 2018*a*, *b*; Feigenbaum and Hall 2015; Kim and Pelc 2021; Kuk, Seligsohn, and Zhang 2018). A seminal study analyzing the responses of elected policymakers to import competition from China revealed that legislators from more trade-exposed districts were more likely to take the protectionist side on trade votes (Feigenbaum and Hall 2015). While not separately looking at policies that compensate trade losers, it finds that, on average, legislative votes on non-trade issues were not affected.

However, this literature has paid less attention to institutional mechanisms in general and labor unions in particular. A recent review concludes that the mediating factors linking import shocks to political outcomes "are still very much a mystery" (Frieden 2022: 588). We address this gap and analyze the role of unions as a mediating variable that shapes legislative responses to global economic shocks and is itself changed by rising import exposure. We test the theory that labor unions are a major missing link between international markets and domestic politics in the American political economy. Following Feigenbaum and Hall (2015) and others, we focus on the US House of Representatives and study the impact of the shock on legislative votes on economic issues.

[1] For example, Galbraith (1952) conceives of labor unions, which are voluntary organizations that represent workers in collective bargaining with employers, as the canonical countervailing power in the labor market. We focus on the political marketplace.

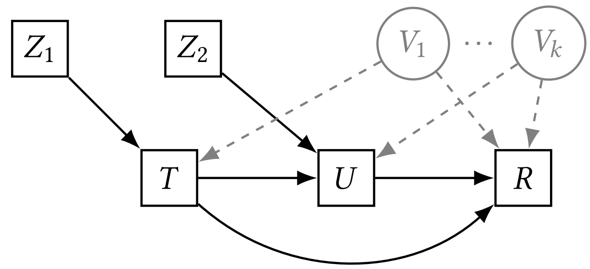

Figure I
Decomposing the effect of import shocks on congressional representation

How important is the union mechanism? This figure illustrates the causal mediation identification problem and how it can be addressed using two instrumental variables. The goal is to decompose the impact of import competition (T), the treatment, on legislative votes on compensation and trade (R) into a direct effect and an indirect effect via labor unions (U), the mediator. If there are unobserved confounders ($V_1, \ldots, V_k$), the decomposition needs two instruments: one to shift trade exposure (T), another to shift unionization (U).

In line with political economy theories, we distinguish between trade and compensation votes. Our main theoretical and empirical departure from recent work on the legislative effects of trade (e.g., Feigenbaum and Hall 2015; Owen 2017) and the separate literature on the legislative impact of unions (e.g., Becher and Stegmueller 2021; Becher, Stegmueller, and Kaeppner 2018) lies in our focus on disentangling and quantifying mechanisms through which import competition shapes political representation. From the extant literature, it is not obvious that the union mechanism will be substantively important. Trade may not have more than a minor effect on union strength (Ahlquist and Downey 2023; Scruggs and Lange 2002), and even if it does, unions might not be able to shape legislative votes on key economic issues. Even if they do, their mediating impact may be dwarfed by other mechanisms. For example, a canonical view of democratic politics, formalized in political agency models, suggests that reelection-seeking policymakers may seamlessly respond to constituencies hit by economic shocks (for evidence, see Mian, Sufi, and Trebbi 2010; Owen 2017). Moreover, other mediating institutions, such as the media (Snyder and Strömber 2010), may be more important than unions. We provide clear empirical evidence for the importance of unions as a mechanism.

Of course, analyzing causal mechanisms is difficult (Imai, Keele, and Yamamoto 2010; Imai et al. 2011). It requires analysts to deal with a double threat of confounding. Figure I illustrates the problem and our research design. First, estimates of the total effect of the treatment, import exposure (T), on political representation in terms of legislative (or roll-call) votes (R) will be biased if there is an unmeasured confounder (V_1) that shifts both import exposure and representation. For example, politically influential (e.g., swing) districts may be better protected from import competition because they have more political weight and, for the same reason, are better represented on numerous other issues. Absent experimental manipulation, these endogeneity problems can be addressed using an instrumental variable (Z_1). The literature has identified plausibly exogenous instruments for import exposure based on import exposure in the same industries in other developed economies (Autor, Dorn, and Hanson 2013*a*, 2016).

Importantly, having an instrument for import exposure alone is not sufficient to decompose the effect of import exposure into an (indirect) effect, working through unions, and a direct effect. The second confounding problem stems from the fact that one cannot independently randomize union strength, our mediating variable. It is not difficult to think of historical factors and institutions that may shape both unions and policymaking (Ahlquist 2017). For instance, historically, race has been used as a tool to divide workers, prevent the growth of unions and dampen support for compensation. State-level laws regulating unions and collective bargaining tend to be more restrictive in the US South (Feigenbaum, Hertel-Fernandez, and Williamson 2018; Flavin and Hartney 2015), and some scholars argue that legacies of slavery still explain differences in preferences and political behavior within states (Acharya, Blackwell, and Sen 2019). Moreover, correlated voter preferences may explain lower demand for unions in the workplace and less support for policies favored by unions. In general terms, Figure I captures that there may be confounding variables (V_k) that shape union strength and congressional representation. This makes it challenging to identify the effect going from U to R. Therefore, we also need an independent instrumental variable for district-level union membership (Z_2). Turning to a separate literature, we draw on history and leverage across-the-board unionization in coal and metal mines and steel plants just after World War II as a source of plausibly exogenous variation (Becher and Stegmueller 2021; Holmes 2006). To combine both instruments without making strong functional form assumptions, we employ a novel semiparametric instrumental variable (IV) model (Frölich and Huber 2017). In sum, our approach leverages two natural experiments and advances in statistical methods for causal mediation analysis. While we cannot conduct a parallel experiment randomizing both trade exposure and union membership at the scale of congressional districts, our approach enables us to relax strong selection on observables and functional form assumptions required in the standard regression approach for unpacking causal mechanisms.

Altogether, our analysis makes two primary contributions. First, we advance the large literature on how institutions in general and labor market institutions in particular condition the impact of international economic shocks on domestic politics (Garrett 1998; Guiso et al. 2019; Milner and Kubota 2005; Rudra 2008). Our empirical approach captures that conditioning institutions may themselves be altered by trade shocks and operate as a mediating force, enabling us to unbundle direct and indirect effects on political representation. Combining arguably exogenous variation for both international shocks and trade and unions, we are the first to quantify how much of the impact of rising import competition on legislative responses on compensation and trade policies works *via* the adverse effects of import competition on unionization. We estimate that about one half of the effect of import exposure on declining legislative support for compensation works though the channel of weaker unions. On trade policy, unions account for approximately one third of the total effect in the direction of less opposition to further trade liberalization. Our results help explain the otherwise surprising lack of support by legislators from highly trade-exposed districts on legislative votes about compensation. They also highlight that import competition can increase legislative support for hyperglobalization by weakening unions.

Second, our analysis speaks to long-standing research on the consequences of globalization for organized labor and the recent literature on the economic effects of the China shock. Recent scholarship has shown how import competition affects labor market outcomes like wages and employment, though it has paid less attention to unions (for a review, see Autor, Dorn, and Hanson 2016). The first step of our analysis reveals that import competition has pronounced effects on local labor unions. While the hypothesis that global markets undermine domestic commitments to compensation by weakening unions is not new, the earlier (i.e., pre-China shock) literature on the link between globalization and unions focused on a period when most trade was between developed economies and it largely relied on country-year-level analyses (for an exception, see Slaughter 2007). It suggested that globalization was no smoking gun for explaining union decline (Garrett 1998: 62; Scruggs and Lange 2002). However, patterns of world trade have since changed. One of the few studies on unions in the China shock literature finds that import exposure is linked to a modest decline in unionization in manufacturing industries but not to overall de-unionization at the state level (Ahlquist and Downey 2023).[2] We leverage *within-state* variation in import exposure using administrative data and find that import exposure since the 1990s has diminished union membership at the district level.[3]

Global competition and endogenous countervailing institutions

We consider labor unions as an endogenous countervailing power in the democratic politics of compensation and trade policy. They enhance the political representation of economic losers from global competition. But they are not immune to the economic effects of globalization. Therefore, it is key to explore the relevance of unions as a mediator between import competition and the political representation of economic losers.

Labor unions and political representation

Labor unions matter for the political representation of non-elite workers, whether they are unionized or not. First, unions shape policy preferences. They push back against the fatalistic view that the negative distributive effects of globalization are inevitable or even necessary collateral damage on the road to greater aggregate prosperity. Through leadership and socialization, unions tend to promote the view that governments can and should take actions to help economic losers and reduce economic inequality. Accordingly, economic losers deserve collective solidarity as people's economic fate is in part determined by circumstances and luck, not just hard work and competence (Ahlquist and Levy 2013; Mosimann and Pontusson 2017).

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 and maintain the required revenues through taxes, and are more critical of free trade. In what is probably the most comprehensive study of the nexus between public opinion and interest group positions covering hundreds of issues, Gilens (2012: 158) finds that unions in the US exhibit a "strong tendency to share the preferences of the less well-off", more so than any other group analyzed. These positions are broadly consistent with self-interest, other-regarding considerations, and ideology. Unions in trade-exposed local labor markets have an interest in providing a collective voice in support of policies that benefit their members' economic interests. For instance, this includes support for unemployment insurance, worker training, and income redistribution more generally. 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

[2] Charles, Johnson, and Tadjfar (2021) find a negative effect on certification elections.

[3] There are multiple reasons why our results may be different from Ahlquist and Downey (2023), including the focus on different geographic units (districts versus states) and the use of administrative data to measure union membership, which entail different strengths and weaknesses compared to survey data.

the upper half of the income distribution or working in competitive exporting firms—for income redistribution and protectionist measures (Ahlquist, Clayton, and Levi 2014; Kim and Margalit 2017; Mosimann and Pontusson 2017).

Second, through electoral mobilization unions shape the outcome of elections, potentially tilting the balance away from economic elites and big business. Over decades, a cross-disciplinary literature has documented union effects on elections and policymaking (Ahlquist 2017; Freeman and Medoff 1984; Leighley and Nagler 2007). Relatedly, in the marbled halls of power unions are one of the few lobbying groups speaking on behalf of non-elite workers, and stronger district-level unions are associated with lower bias in the perceptions of congressional staffers of their district's support for redistributive policies (Hertel-Fernandez, Mildenberger, and Stokes 2019). Overall, organized labor has anchored the Democratic Party and made it more attentive to economic losers (Schlozman 2015). While earlier studies often lack credible causal identification (Ahlquist 2017), recent research is increasingly based on natural experiments and other designs that make it more plausible to infer union effects on democratic politics.

Import competition and unionization

The crux is that labor unions are themselves directly subjected to the forces of globalization. On the one hand, increasing exposure to economic risks in the global economy may increase demand for unions. On the other hand, international trade and, in particular, import exposure can weaken unions by undermining their position at the bargaining table, eliminating entire unionized workplaces, and debilitating efforts to organize non-union workplaces in other sectors. While cross-national research has yielded mixed findings about the impact of economic globalization on unionization and soundly rejected the idea of an inevitable race to the bottom with respect to labor market institutions, the institutional environment makes American unions vulnerable to import competition.

Even in the absence of large-scale job losses, import competition can contribute to declining unionization by weakening unions' bargaining power and eroding the benefits of union membership. In standard models of wage bargaining between profit-maximizing firms and unions, an increase in international product market competition lowers firms' quasi-rents up for negotiation, leading to deteriorating outcomes for unions at the bargaining table (Rodrik 1997: ch. 2). Crucially, lower bargaining power and declining union wage premiums make it less attractive for workers to join a union and more difficult for unions to recruit new members.

Moreover, recent evidence suggests that import exposure has reduced manufacturing employment and, through general equilibrium effects, negatively affected local labor markets (Autor, Dorn, and Hanson 2013*a*). The resulting job losses, plant closures, and declining outside options exacerbate the organizational problems faced by unions. The comparatively combative nature of wage-setting at the establishment level (rather than at the industry level), gives firms incentives to take a more hostile stand against unions and leverage dismissals and the threat of plant relocation to weaken organized labor (Scruggs and Lange 2002).

Trade-induced plant closures exert additional downward pressure on unions. Plant closures account for a significant share of the decline in manufacturing employment (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 establishment is union-free by default, even if other employees in the same company are unionized. Within the institutional framework supervised by the National Labor Relations Board, 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 against the (often substantial) opposition of management. Reinforcing this logic, companies that shut down an existing plant may pursue a strategy of domestic offshoring and open a new plant in a different state where it is more difficult to unionize. Altogether, the dynamics of wage bargaining under increased international competition as well as trade-induced job losses and plant closures imply that import competition leads to a decline in the stock of union members.

Social spillovers may be a mitigating force. Holmes (2006: 1) argues that "unionism is contagious at the geographic level" (cf. Ahlquist and Downey 2023). Union members tend to have more positive views of organized labor than comparable non-members, due to socialization or experiencing the benefits of membership. When changing jobs, union members will take their taste for unions with them. Furthermore, union preferences can be transmitted to children working in other sectors. In line with this argument, Holmes (2006), using contract-level data, finds that proximity to mining and metal industries in middle of the twentieth century is strongly correlated with unionization in health care facilities five decades later. The study also finds evidence of positive spillovers from mining and steel to retail and construction. However, theoretically it is not clear that spillovers can fully compensate for the decline in unionization stemming from trade-induced declines in bargaining power, job losses and plant closures. But, as explained in more detail below, we argue that geographic spillovers before the dramatic change in world trade in the 1990s constitute a key source of identification for our effect decomposition.

Empirical implications

We empirically assess the following mechanisms linking import competition at the level of congressional districts to legislative votes on compensation and trade policy of the member representing each district. Districts are the theoretically appropriate unit of analysis for the purpose of studying the causal chains from international trade to legislative votes. They are also attractive from an inferential perspective because much of the variation in import exposure from China stems from different local labor markets in the same state (Autor, Dorn, and Hanson 2013*a*) and a within-state analysis can better untangle import competition from exposure to technological shocks (Autor, Dorn, and Hanson 2013*b*).

- First, higher import exposure reduces unionization.
- Second, stronger unions increase legislative support for compensation and reduce legislative support for further trade liberalization.
- Third, we ask how important is the union mechanism relative to the total effect of import exposure on legislative votes. If unions are an important countervailing power that is undermined by global competition, the channel operating through district-level unions should be substantively important.

Data and instrumental variables

We examine the impact of imports from China between 1990 and 2000 on subsequent political representation via union membership and other channels in electoral districts for the House of Representatives during the 107-112th Congress (i.e., 2001-2012). During the 1990s, imports from China rose by 95.3 billion (in 2007 US$) or 362% (Autor, Dorn, and Hanson 2013*a*: 2131). Subsequently, Congress considered numerous policies concerning compensation and trade.

Roll-call votes on trade and compensation

We examine high-profile votes on trade policy and compensation policies, which are conceived to include income protection and social insurance in the face of labor market risks (Garrett 1998; Rodrik 1997). On trade, we have 10 votes. Following the selection of Owen (2017: 302), this includes votes on free trade agreements and the vote on granting the president fast-track authority to negotiate trade agreements (which was opposed by unions). We added three more recent free-trade votes not covered by Owen's study. Based on interest group ratings from the Cato Institute, voting yes on these votes means supporting trade liberalization.

On compensation, we select 13 key votes. This includes votes to extend unemployment benefits for people who have exhausted regular benefits, expand job training, housing assistance, food stamps, or increase the minimum wage, and votes on broader increases in social expenditures or tax cuts financed by cutting future social spending, as identified by the legislative scorecard of Americans for Democratic Action.

These are policies that mitigate economic hardship in areas affected by international competition (Autor, Dorn, and Hanson 2013*a*: 2149). We also include three House votes on the extension of trade adjustment assistance that took place during the period of study. All compensation votes are coded so that 1 means supporting the pro-compensation direction and 0 means opposing it. For details on included votes, see Appendix A. While these votes do not exhaust the issues that are salient to unions, they match our theoretical focus on the politics of trade and compensation.[4]

Import exposure

To measure a district's exposure to import competition from China, we follow the well-known approach pioneered by Autor et al. (2013*a*; 2016). The idea is to map industry-specific import shocks onto local labor markets in proportion to the localities' pre-shock industrial employment structure. The shift-share measure captures the change in Chinese import exposure per worker in a local labor market apportioned by its share in total national employment. The measure from Autor, Dorn, and Hanson (2013*a*) covers 741 local labor markets (commuting zones, each consisting of a group of counties). We capture the exposure of a district's population to import shocks, by creating a mapping from commuting zones (in their 1990s definition) to congressional districts,

[4]Our analysis does not take a stand on whether protection and compensation are substitutes (cf. Kim and Pelc 2021). In theory, compensation for increased openness is more efficient than protectionism, but promises of sustained compensation in exchange for openness may not be credible. Our analysis takes place in a time of low trade barriers, where opposition to further integration (e.g., with Chile) need not benefit current losers from Chinese import competition.

based on the spatial overlap of the commuting zone and district weighted by the population in the spatial intersection for *each* Congress (see Appendix B for more details).[5]

Instrumenting import exposure Imports from China are possibly related to unobserved district-level characteristics that also drive unionization or legislative votes. 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.

Previous work shows that this 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: a one percent increase in the instrument induces a 0.84 (±0.03) percent increase in import exposure (Appendix B). Recent methodological work on shift-share designs clarifies the identification challenges for a region-level analysis of trade shocks, focusing either on the plausibility of assuming exogeneity of the shocks (Borusyak, Hull, and Jaravel 2021) or the shares (Goldsmith-Pinkham, Sorkin, and Swift 2020). The latter requires that pre-shock employment shares are conditionally exogenous (while the trade shift may be endogenous). Their analyses (albeit for a different outcome) suggest that this may be implausible, and they provide strategies to assess the sensitivity of IV results to overly influential individual industries. While we discuss this issue in more detail in Appendix C, we outline here four ways in which we tackle this issue. First, as suggested by both studies, we control for technological change. Second, as discussed later, we adopt a flexible approach to make conditional exogeneity more plausible. Third, following Goldsmith-Pinkham, Sorkin, and Swift (2020), we calculate Rotemberg weights and exclude industries with the largest weights from the analysis (Tables C.1 and G.1). Finally, we show that our trade effect estimates can also be replicated with other measures of exposure (Table C.3).

Union membership

To measure union membership (as a share of the working population) we rely on fine-grained administrative union data collected by Becher, Stegmueller, and Kaeppner (2018). They use LM forms—mandatory annual reports filed by each local union to the Department of Labor—providing more than 350,000 individual reports covering almost 30,000 unions. The data includes membership size and geocoding information for each union, which were used to map unions onto congressional districts.

Instrumenting Union membership As already discussed, a key challenge for our analysis is that unionization is not necessarily exogenous to factors (e.g., preferences, policies, institutions) that shape political representation. We thus employ an instrumental variable for district-level unionization. The history of organized labor combined with geography suggest a plausibly exogenous source of variation in contemporary union membership that we can use as an instrument in our analyses. Following Holmes (2006) and Becher and Stegmueller (2021), we leverage record-

[5]Because commuting zone geographies serve as a time-constant reference point, this approach captures decennial redistricting in each apportionment cycle and mid-period court-ordered redistricting. The unit of analysis in the decomposition is the district-vote, where districts are stacked over congressional terms.

high unionization levels in coal and metal mines as well as steel plants during the heyday of unionism just after World War II as a plausibly exogenous source of variation. The instrument rests on the argument that initial unionization in mining and steel spilled over into stronger unions in other sectors, including non-manufacturing sectors like health care and groceries, through socialization and union organizational efforts. Holmes (2006) provides detailed evidence on spillovers from mining and steel in the 1950s into non-manufacturing unionization decades later. Building on this finding, Becher and Stegmueller (2021) use the historical employment share in mining and steel as an instrument for contemporary union strength. Importantly, the location of mining and even steel production is mainly determined by nature, based on availability of coal and ore in the ground or the access to raw materials for steel mills. In the 1940s and 1950s coal, metal and steel were unionized across the country, regardless of states' political leaning and prior strength of the labor movement (Holmes 2006: 3-5). 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.[6]

We show this relationship graphically in panel (a) of Figure II. 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. The plot reveals a clearly positive relationship that is statistically and substantively significant. A simple linear model (with cluster-robust s.e.) reveals that a one percent increase in 1950s employment in mining and steel induces a 0.32 ($\pm$0.05) percent increase in the district-level share of union members. Thus, we use district-level variation in mining and steel employment in the 1950 Census as instrument for present-day unionization levels.

As our main analysis combines both instrumental variables to disentangle the relative importance of the union mechanism from other mechanism through which import competition shapes congressional representation, we have to verify that the two instruments are separable (conditional on covariates). If the two instruments were closely related, they would not serve our effect decomposition. One possibility is that historical mining and steel employment drives exposure to import competition, perhaps because stronger unions got more trade protection (Owen 2013; Ahlquist and Downey 2023). While this would not invalidate identification as long as the instrument for unionization remains uncorrelated with the instrument for import exposure, it raises an important concern about temporal sequencing that is assessed empirically in panels (b) and (c) of Figure II.[7] We see that historical mining and steel employment at the district level in 1950 is not associated with actual import exposure in the 1990s (panel b). This pattern is consistent with the argument and evidence in Holmes (2006) that unionization in mining and steel during the

[6]Given the theory behind the instrument, we prefer an instrument that captures the number of union members. Spillover effects are more pronounced when more individuals are members of a union in an area. This is why we use employment shares in mining and steel plants as opposed to, say, data on mineral deposits or a simple count of mines. A weakness of this approach might be that employment shares are partly determined by endogenous factors (e.g., larger mining operations might occur in regions that have a larger industrial base or more entrepreneurial environment). While we cannot fully rule out these issues, we provide a robustness test in which we account for existing historical (1950s) shares of industrial employment in a region. Furthermore, our empirical analyses also include hypotheses tests that assume a degree of exclusion restriction violation.

[7]Furthermore, we provide robustness tests that adjust for pre-shock industry-level unionization estimated from survey data.

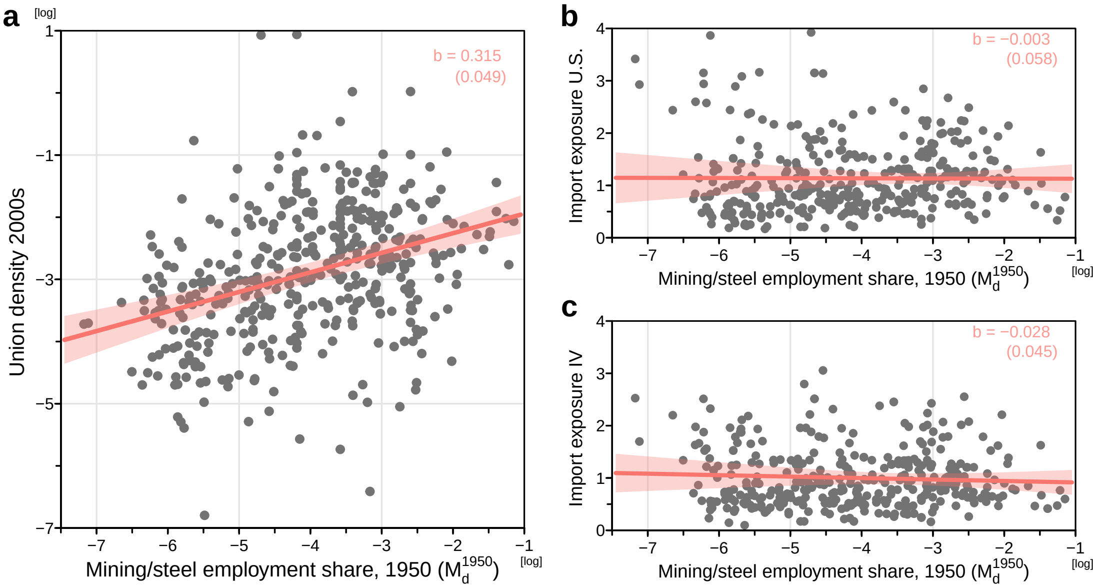

Figure II
Instrumenting union density with post-war mining/steel industry employment

Panel (a) 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) in the 109th Congress. Panels (b) and (c) plot district-level employment in mining and steel industries in 1950 against U.S. district-level import exposure (b) and its instrument (c). Linear fit with cluster-robust 95% confidence interval superimposed.

historical peak of unionization spilled over into other sectors of the economy, including services (e.g., health care and groceries), that gain from cheaper imports. Importantly, there is only a weak (non-significant) association between the two instruments (panel c): an increase in mining and steel employment in 1950 is associated with a −0.03 (±0.05) percentage point decrease in the import exposure instrument.

Empirical strategy

Instrumental variable models

Before turning to the effect decomposition, we separately assess (i) the effect of import exposure on unions and (ii) the effect of unions on legislative votes using instrumental variable analyses. Using the instruments introduced above, our preferred specification employs a semiparametric IV model that relaxes functional form assumptions for control variables.

Depending on the analysis, the outcome will be either district-level union membership or votes on compensation and trade by the district's representative in each Congress. In each analysis, the causal or treatment variable of interest, import exposure or unionization, is instrumented with the instruments discussed above. While control variables are not needed when the instruments are valid, exogeneity of the instruments may be more plausible conditioning on pre-exposure controls.

The appeal of using debiased machine-learning for the IV models is that it is more robust against misspecification of the functional form of the controls, as it can flexibly account for higher-order interactions between control variables and for non-linear terms.

The set of possible district-level control variables includes district characteristics measured from the 1990 Census (share of college degrees, share of black and foreign-born population, urbanization, manufacturing employment), before the trade shock. To capture possible legacies of slavery and immigration (Acharya, Blackwell, and Sen 2019), we also include variables from the 1910 Census (share black and foreign born), deliberately measured before the great migration. We also account for pre-China shock susceptibility to automation because sector-biased technological change may be correlated with import exposure and the strength of unions. Instrument validity would be threatened if changes in automation drive import exposure the same way across countries. While in smaller geographic units, such as commuting zones or districts, the susceptibility to automation and import competition are only moderately correlated (Autor, Dorn, and Hanson 2013*b*, 2016), we control for occupational routine task intensity (Autor, Dorn, and Hanson 2013*b*) matched to districts, resulting in a measure of the fraction of the workforce in the district at the risk of automation. To proxy for political factors not accounted for by state-fixed effects and other district characteristics, our specifications also include districts' political ideology in the 1980s based on Kernell (2009). Appendix Table B.1 provides descriptive statistics. When included, controls are normalized to have mean zero. Additional controls are included in robustness checks (reported in Appendix C and D).

Following the strategy outlined in Chernozhukov et al. (2018), the set of controls can flexibly enter both the first stage, where the treatment variable of interest is regressed on the instrument and controls, and the second stage, where the outcome variable is regressed on the instrumented treatment variable and controls, via possibly highly non-linear functions. We estimate these using debiased ensemble machine learning for IV (dML-IV). See Appendix F for technical details.

Effect decomposition using two instruments

Our central quantities of interest are the indirect effect of trade shocks on legislative votes, via unions, and its magnitude relative to the direct effect. Figure I summarized the theoretical argument that trade (T) impacts union membership (U) which, in turn, affects legislative (i.e., roll-call) votes (R) and the identification problem. Using standard terminology, the full causal path working through unionization, $T \rightarrow U \rightarrow R$, is called the *natural indirect effect* (NIE). Trade may also have a *natural direct effect* (NDE) on roll-call votes, representing channels other than union density linking the shock to votes. For example, politicians may respond to voter demand regardless of union strength.

Recent work has explicated the challenge for such an effect decomposition. Intuitively, it entails a double confounding problem (called sequential ignorability by Imai, Keele, and Yamamoto (2010); Imai et al. (2011)). Following the identification strategy of Frölich and Huber (2017), we combine our two instrumental variables, one for trade exposure and another for unionization, in a single statistical model that enables us to unbundle the total effect of import exposure under weaker assumptions compared to the classical approach requiring selection on observables and linear functional form assumptions. In the spirit of instrumental variable designs for non-experimental data, we think of our approach as replacing implausible assumptions about selection on observables and known functional forms with more plausible (but not infallible) assumptions leveraging natural experiments and advances in the statistical analysis of effect decomposition.

To clarify the counterfactual quantities we want to estimate, let us denote two counterfactual levels of trade exposure T as t and t' (omitting subscripts). Then the potential vote outcome for an observation under some hypothetical trade exposure treatment is represented by $R(t, U(t'))$. This signifies that a roll-call vote is driven by (non)exposure to a trade shock as well as union density, $U(t')$, which is itself a function of the shock. The natural indirect effect of the trade shock is obtained by holding the exposure to the shock constant in both counterfactual scenarios, but shifting union density $U(t)$ from its level with without trade exposure $U(0)$ to its level when exposed to the shock $U(1)$. The difference between the two terms is the impact of the trade shock on roll-call votes due to the change in union density:

$$E\left[R(1, U(1)) - R(1, U(0))\right]. \tag{1}$$

The natural 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:

$$E\left[R(1, U(1)) - R(0, U(1))\right]. \tag{2}$$

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. For the analysis, we define an 'intensive trade shock treatment' as occurring whenever a district's import exposure exceeds the median of the district distribution, which corresponds to about \$1000 per worker.[8]

The identification problem of such a causal structure arises from the likely existence of unobserved confounders $(V_1, \ldots, V_k)$. Even in the ideal scenario of controlled random assignment of T (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 T on U and T on R, but not the causal effect of T on R *via* U. Our empirical strategy to address the problem employs *two* instruments to identify the indirect effect of trade exposure, as illustrated in Figure I. It follows the approach proposed by Frölich and Huber (2017). 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 imports from China based on Chinese imports in other high-income markets. The second instrument, Z_2, enables us to vary union density independently of trade shocks. In our application, Z_2 is a district's mining and steel employment share in 1950. Going beyond the simplification in Figure I, we do not need to assume constant effects (or the absence of treatment-mediator interactions). Our approach captures that there may be heterogeneity in the different causal chains, not only when estimating the total effect, as in standard instrumental variables models estimating local

[8] That is, we set $T = \mathbf{1}(IPW_d > Q_{IPW}(0.505))$. As in IV approaches generally, effects are defined for the subpopulation of compliers, that is, districts whose import exposure is shifted upwards when import exposure in the same industries in other advanced economies moved above the threshold defined above (see Appendix G.1 for a more detailed discussion). Note that a more stringent threshold yields similar results (see Table G.1).

average treatment effects, but also in the chain from import exposure to unions and unions to legislative representation.

As outlined in the section "Data and instrumental variables", Z_2 uses geographical spillovers from unionized mining and steel establishments in the middle of the twentieth century to other sectors of the economy in the same locality based on ongoing socialization and union mobilization efforts. The onset of the exogenous trade shock captured by Z_1 is more recent. While not instantaneous, trade effects driven by lower wages and job displacement will be visible within a few years. Using the first instrument, we can estimate the overall effect of import competition on congressional representation. The second instrument enables us to model the counterfactual comparisons represented by equations (1) and (2). Under assumptions summarized below, we can thus decompose the total effect into an indirect effect, working through unions, and a remaining direct channel. Intuitively, the role of the second instrument is to undo the changes in U induced by T in order to identify the indirect effect $T \to U \to R$. For a detailed exposition of the model, assumptions, and estimation, see Appendix G.1.[9]

A simplified example may convey how the two instruments help us to approximate the counterfactual natural indirect effect. Imagine that there are two districts, A and B, that are exogenously exposed to import competition, which weakens unions in both places. But the second instrument, based on plausibly exogenous spillovers from historical mining and steel realized before the trade shock in district B, shifts post-shock union density in B to the same level as the pre-shock union density in district A. In this comparison, the second instrument undoes the effect of import exposure on unionization, and we can compare legislative votes under high import exposure and varying union strength. Importantly, the post-treatment difference in unionization between A and B due to the second instrument is not a result of (observed or unobserved) confounders.

For the decomposition to have a causal interpretation, standard instrumental variable assumptions need to hold. The instruments must be relevant, plausibly exogenous, and satisfy an exclusion restriction. Drawing on the literature, evidence of a strong first stage, and additional robustness checks we argue that they are plausible. In addition, but closely related, the instrument for the trade exposure treatment (T) must be (conditionally) independent of the instrument for the mediator (U). As was already shown in Figure II, the correlation between the two instruments is indeed small. Districts with higher mining and steel employment in the 1950s do not face substantively more (or less) potential import competition throughout the 1990s based on exposure in the same industries in other advanced economies. Finally, unobserved confounders (V_k), which can be thought of a latent variable, that shift the moderator should do so monotonically. The model is identified nonparametrically. It allows for arbitrary treatment-mediator interactions and thus heterogeneity in the direct and indirect effects of the trade shock on representation. While not certain, these assumptions are weaker than a standard linear structural equation approach requiring selection on observables and linearity.

[9] A placebo Monte Carlo study (G.5) indicates that the model adequately recovers null effects for the direct and indirect effect under substantial confounding.

Results

Does import exposure reduce district-level unionization?

Table I shows instrumental-variable (IV) estimates of the effect of Chinese import exposure during the 1990s on union density in House districts in the 107-112th Congress.[10] The most conservative estimate comes from the most flexible specification using the debiased machine learning approach (dML-IV) on all district-level controls (column 5). It implies that a $1,000 increase in per-worker import exposure reduces unionization by 0.17 log points or about 15 percent. The other columns of Table I show the estimates from standard IV models, which adjust for state and time fixed effects to focus on within-state variation. State fixed effects account for time-invariant differences in institutions, including right-to-work legislation, and preferences across states. District controls enter sequentially. The resulting estimates are somewhat larger than those in column (5). One advantage of these models is that they allow us to conduct inference under some (local-to-zero) violations of the instrument exclusion restriction. As even small violations can lead to unreliable hypothesis tests, we employ a test based on the approach of Wang et al. (2018). The entries in Table I show p-values from an Anderson-Rubin test of the null hypothesis of no effect of trade on union density allowing for a local violation of the exclusion restriction (cf. Appendix E). Results indicate that small violations of the exogeneity of the trade instrument would not change our conclusions.

In all models, import exposure from China has a precise negative effect on district-level union membership. Additional robustness tests are reported in the Online Appendix. They include alternative specifications of district socio-demographics, adjusting for offshorability, for industry-specific union shares in 1990, instrumenting automation exposure, using post-2000 trade exposure, and a sensitivity analysis based on Rotemberg weights (C.2). To ensure that our results are not dependent on the trade instrument, Appendix C.4 reports results using (i) variation in tariffs across industries induced by China's entry into the WTO (Pierce and Schott 2016), and (ii) residuals from a gravity model (Autor, Dorn, and Hanson 2013*a*). Invariably, these models reveal a negative impact of trade exposure on union density.

Do unions affect congressional votes on compensation and trade?

The second step in our analysis is to reassess whether unions have a causal effect on legislative votes on compensation and trade issues. The dependent variable in this analysis are roll-call votes of representatives. For trade issues, votes are coded so that 1 is a vote for trade liberalization and 0 is a vote against. For compensation issues, votes are coded such that 1 is voting in favor of expanding/protecting compensation and 0 is against. The key variable on the right-hand side of the regression is the (logged) share of union members among a district's working population instrumented using the share of employment in mining and steel in 1950.

Table II presents the resulting estimates. Panel *A* displays trade votes; panel *B* compensation votes. Across specifications, the estimates are consistent with the view that stronger unions lead

[10]Alaska and Hawaii are excluded due to missing data. For comparability with later sections, Table I presents results from a stacked analysis that pools all congressional terms. All results go through in a congress-by-congress analysis (Appendix C.3).

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

	(1) IV	(2) IV	(3) IV	(4) IV	(5) dML-IV[a]
Chinese import exposure	−0.309	−0.265	−0.349	−0.363	−0.170
	(0.045)	(0.049)	(0.036)	(0.035)	(0.030)
IV violation robust inference $[p]^b$	0.000	0.000	0.000	0.000	–
District characteristics					
Slavery & immigration history		✓	✓	✓	✓
Demographics & ideology			✓	✓	✓
Technological change				✓	✓
N	2592	2592	2590	2554	2554

Note: 2SLS estimates with state-congress fixed effects and cluster-robust standard errors. Chinese imports per worker is instrumented by imports to eight other highly industrialized countries. First stage robust F statistics range from 307.7 to 465.6. Historical patterns of slavery and immigration proxied by 1910 Census district-shares of Blacks and foreign-born. A district's demographics include 1990s values of the share of foreign born, Blacks, college degrees, employed in manufacturing, living in urban areas. District ideology is based on Kernell (2009) for the 1980s. Technological change measured by risk of automation (RSH_d).

a Partial linear IV model estimated using debiased ensemble of machine learners 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 F for detailed model equations and machine learning algorithms used.

b Test of trade shock allowing for violation of IV exclusion restriction, H_0: $\beta = 0$ w. $Cov(Z, e) \approx 0$, (Wang et al. 2018). See Appendix E.

to less legislative support for trade deregulation and more support for economic compensation. Column (5) report results from the IV model estimated using debiased ensemble machine learning (dML-IV). The estimates imply that an increase in union density leads to a significant decrease in the likelihood of agreeing to trade roll-calls (-0.90 ± 0.16) and to a significant increase (0.47 ± 0.12) in voting for compensatory policies. In substantive terms, a standard deviation *reduction* in union membership (i.e., a −0.186 log point change) raises the probability of pro-trade legislative votes by about 15 percentage points. The same reduction in union strength increases the probability of compensation votes by about 9 percentage points.

We again show estimates from standard IV models in columns (1)–(4), which start with an empty model and sequentially enter district controls. The estimates are smaller compared to the dML-IV model. Focusing on the specification (4) with full controls, the magnitude of the estimates relative to their (cluster-robust) standard errors indicates that they remains substantively and statistically significant.[11] As in the previous analysis, the effect of unions remains significant if we allow for a local violation of the exclusion restriction. In additional robustness tests, we account for offshorability, foreign direct investment, district social capital, and the historical size of the industrial base (Appendix D.1). We also study results using an alternative estimator (D.3). Our results are also consistent when analyzing votes separately (D.2) and adjusting for the share of public unions (Appendix H).

[11] The effect of a SD decrease in union strength on trade votes is about −9 percentage points, while the effect on compensation votes is about 5 points.

Table II
Effect of union density on legislative behavior.

	(1) IV	(2) IV	(3) IV	(4) IV	(5) dML-IV[a]
A: Trade roll-call votes					
Union density [log]	−0.414	−0.382	−0.427	−0.463	−0.899
	(0.092)	(0.092)	(0.125)	(0.124)	(0.157)
IV violation robust [p][b]	0.000	0.000	0.006	0.003	–
N	4243	4243	4243	4183	4183
B: Compensation roll-call votes					
Union density [log]	0.266	0.205	0.205	0.246	0.466
	(0.060)	(0.057)	(0.080)	(0.074)	(0.116)
IV violation robust [p][b]	0.000	0.003	0.045	0.021	–
N	5458	5458	5454	5380	5380
District-level controls					
Slavery & immigration		✓	✓	✓	✓
Demographics & ideology			✓	✓	✓
Technological change				✓	✓

Note: 2SLS estimates with state-congress fixed effects and cluster-robust standard errors. Controls are as in Table I. Union density instrumented by share of mining employment in the 1950s. First stage robust F statistics range from 14.0 to 33.7. Appendix Table I.1 presents full table including inference robust to potentially weak instruments.

a Partial linear IV model estimated using debiased ensemble of machine learners 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 F for detailed model equations and machine learning algorithms used.

b Test of union coefficient allowing for local-to-zero violation of IV exclusion restriction, H_0: $\beta = 0$ w. $Cov(Z, e) \approx 0$, (Wang et al. 2018).

Decomposing the effect of trade shocks

Now we turn to the results of the effect decomposition that estimates the full chain from trade exposure to legislative votes working through district-level union membership as well as the direct effect not working through unions. Table III shows the resulting estimates from the semiparametric instrumental variable model for causal mediation that, for identification, leverages separate instrumental variables for trade exposure and unionization. The table entries are estimates of the local average treatment effect (LATE) of import exposure on legislative votes, the (natural) indirect effect (NIE), operating through union density, and direct effect (NDE) and their corresponding standard errors. Finally, the percent column indicates the share of the total effect of trade shocks (among the treated) that is mediated by union membership. All specifications include historical controls measuring the district-level shares of blacks and foreign born in 1910. District-level socio-demographics and political ideology, as well as automation risk are added sequentially.

For roll-call votes related to compensation, all specifications indicate that the estimate of the indirect effect is negative, in line with our two prior analyses. Given the size of the effects relative

Table III
Import exposure, unions, and legislative votes: Indirect and direct effect based on semiparametric IV model with two instruments

	A: Compensation votes				*B: Trade*			
	LATE	NIE	NDE	%	LATE	NIE	NDE	%
(1) Historic controls	−0.108	−0.079	−0.075	51	0.127	0.061	0.124	33
	(0.022)	(0.015)	(0.033)		(0.025)	(0.017)	(0.040)	
(2) + Demogr. & ideo.	−0.107	−0.074	−0.060	55	0.127	0.059	0.111	35
	(0.022)	(0.021)	(0.042)		(0.025)	(0.019)	(0.041)	
(3) + Technological chg.	−0.102	−0.068	−0.062	52	0.122	0.051	0.104	34
	(0.021)	(0.020)	(0.045)		(0.024)	(0.018)	(0.040)	

Note: Entries are estimates (and standard errors) of the treatment effect (i.e., LATE) and natural indirect and direct effects among the treated ($NIE(1)$ and $NDE(1)$). The %-column shows how much of the total effect among the treated on votes is mediated via union density (in percent). All specifications include state fixed effects (removed via orthogonal projection). Entries are robust statistics (median and MAD) based on 1,999 bootstrap draws. Based on semiparametric instrumental variable estimates (Frölich and Huber 2017); see Appendix G.1 for model and estimation details.

to their standard errors, we reject the null hypothesis of no indirect union effect. Substantively, the most conservative estimate including all controls (specification 3) suggests that a per-worker increase in Chinese import exposure experienced at the median (around $1,000) compared to no increase reduces the probability of a pro-compensation vote by about 7 percentage points, on average, *through* its depressing effect on unions. The estimate for the direct effect, which fixes unionization to the level without an increase in import exposure, is slightly smaller (and much less precisely estimated). About half—between 51 and 55%—of the total effect of district-level trade exposure on compensation votes is mediated by union membership. The estimates imply that trade-induced weakening of unions as a countervailing power is a large factor explaining the lack of political support for increased compensation in the face of a historic shift in the pattern of global trade generating many losers in the American economy.

For votes on trade policy, the indirect effect working through unions in positive in all specifications, in line with our prior analysis. But we are now able to quantify the relative magnitude of the indirect and direct effects. The most conservative estimate of the NIE is from the specification using all controls. It implies that a $1,000 per-worker increase in Chinese import exposure increases the probability of a free-trade vote by 5 percentage points by weakening local unions. The estimates of the NDE not working through unions is also positive (in contrast to the NDE of compensation votes, it is more precisely estimated). The union channel accounts for about a third (33-35%) of the effect of import exposure on how legislators vote on trade policy. This means that import exposure can increase support for further trade liberalization by undermining local unions, one of the few domestic countervailing powers increasing the political clout of non-elite workers.[12]

Additional analyses in Appendix G.2 further probe the robustness of the results. We adjust for offshorability, social capital, for historical industrial employment, for the contemporary industry mix, and for a proxy of the pre-trade shock distribution of union membership. We use Rotemberg weights to identify and exclude influential industries. We show that estimates are not driven by influential roll-calls (in Appendix G.3).

Conclusion

While many scholars in the recent literature have studied the effects of import exposure on democratic politics, they have paid less attention to labor unions as a mechanism. We take up the quest for mechanisms using a double instrumental variable approach for causal mediation that enables us to disentangle and quantify the mediating effect of unions. Our analysis revealed that labor unions are a central mechanism linking the steep rise in international economic competition from China after 1989 and the domestic politics of trade and compensation in the American political economy.

While earlier scholarship on globalization and labor unions correctly pointed out that there is nothing inevitable about the impact of globalization on unions, we demonstrate that regional trade shocks did substantively reduce unionization at the local level in the US, with important consequences for the political representation of economic losers. By weakening unions as a countervailing power shaping the political representation of non-elite workers in Congress, import competition reduced politicians support for economic policies helping those most affected by the distributive effects of trade. Through weaker unions, import competition also increased legislative support for additional trade liberalization, contributing to the hyperglobalization apparent before the onset of the trade wars during Donald Trump's presidency. Overall, global competition has undermined political support for embedded liberalism in the twenty-first century to a significant degree by weakening unions.

The institutional channel highlighted in our findings suggests that the political impact of international trade is likely to last beyond the electoral cycles when its economic impact was felt most acutely. Given the costs and uncertain prospects of trying to unionize the workplace, a large negative shock to union membership is difficult to offset, at least in the short run. The weakening of unions may also tilt partisan balance in domestic politics, thus altering representation across a much wider range of issues.

[12] Further disentangling the political mechanisms driving legislative votes is beyond the paper's scope. Both replacement and direct channels are plausible (Autor et al. 2020; Feigenbaum and Hall 2015). But it is difficult to distinguish whether unions affect votes mainly through partisan replacement or post-electoral influence (Becher and Stegmueller 2023) and our setup would even be more complicated, as political mechanisms may vary between the indirect (working through unions) and the direct effect.

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Online Appendix

Contents

A. Roll call votes

Table A.1 lists the selected roll-call votes. Roll call matrices for each vote were retrieved from `www.voteview.com`. We include 10 trade votes. As previewed in the main text, the selection of votes on trade policy follows research that studies the effect of international trade, in the form of exposure to offshoring, on legislative support for extending free trade in the House of Representatives during the first decade of the twentieth century (Owen 2017).

We include votes on free trade agreements identified by Owen (2017: 302) during the 107-109th Congress as well as the vote on granting the president Trade Promotion Authority (TPA) or fast-track authority to negotiate trade agreements (also included in her study). As is explained by Owen (2017: 302), the fast-track policy was "often viewed as essential" to the conclusion of free-trade agreements and it was opposed by the American Federation of Labor and Congress of Industrial Organizations (AFL-CIO).

> Prior to 2001, fast-track authority was in effect from 1974 to 1994, but efforts to renew fast-track during the Clinton administration were unsuccessful. During his campaign, President George W. Bush stated that the renewal of TPA would be one of the top priorities of his administration. [...] Although TPA itself does not directly reduce barriers to trade, it facilitates the negotiation of trade agreements considered in this study was implemented under TPA. (Owen 2017: 302)

In addition, we include three subsequent votes on free trade agreements (with Columbia, Panama, and South Korea) that took place in the 112th Congress. Voting yes on these votes means supporting trade liberalization.

On compensation policies, we select 13 key votes. Following the literature (e.g., Autor, Dorn, and Hanson 2016; Rodrik 1997), compensation is construed broadly to include income protection and social insurance in the face of labor market risks. It includes, but is not restricted, to bills that aim to maintain or enhance the trade adjustment assistance program managed by the Department of Labor. Thus, our analysis includes the 2009 Trade and Globalization Adjustment Assistance Act (which was bundled with the American Recovery and Reinvestment Act), Trade Adjustment Assistance Extension Act of 2011, and the Trade and Globalization Assistance Act of 2007. The latter bill was passed in the House but not the Senate, so it did not become law. Additional votes beyond the trade adjustment assistance program include proposals to extend unemployment benefits for people who have exhausted regular benefits, expand job training, housing assistance, food stamps, or increase the minimum wage, and votes on broader increases in social expenditures or tax cuts financed by cutting future social spending, as identified by the legislative scorecard of Americans for Democratic Action (`www.adaction.org/ada-voting-records/`).

Note that we could not include the Omnibus Trade Act of 2010 because the bill was passed by voice vote and there is no roll-call record in the House. We also do not consider the vote on the passage of the Adjustment Assistance Reform Act (2002) as a compensation vote because it was part of the Trade Act of 2002 (HR 3009) that bundled trade adjustment reform with trade promotion authority (fast-track), Andean trade preferences and other free-trade provisions. The AFL-CIO opposed the omnibus Trade Act of 2002 because of fast-track and Andean trade preferences, judging the compensation part to be insufficient.

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

Roll-call vote	Date	Result	Voteview ID
A. Trade			
Trade promotion authority (presidential “fast-track” authority in trade negotiations), House version	12/06/2001	215-214	476
Free trade agreement Chile implementation act	7/24/2003	270-156	434
Free trade agreement Singapore implementation act	7/24/2003	272-155	430
Free trade agreement Australia implementation act	7/14/2004	314-109	1049
Free trade agreement Morocco implementation act	7/22/2004	323-99	1087
Free trade agreement DR-CAFTA implementation act	7/28/2005	217-215	442
Free trade agreement Oman implementation act	7/20/2006	221-205	1060
Free trade agreement Colombia implementation act	10/12/2011	261-167	777
Free trade agreement Panama implementation act	10/12/2011	300-129	778
Free trade agreement South Korea implementation act	10/12/2011	278-151	779
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, 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
Extension of trade adjustment assistance (Trade and Globalization Assistance Act of 2007).	10/31/2007	264-157	1018
Increasing spending for the departments of Labor, Health and Human Services, and Education (veto override).	11/15/2007	277-142	1114
Extension of trade adjustment assistance (Trade and Globalization Adjustment Assistance Act, part of American Recovery and Reinvestment Act).	01/28/2009	244-188	45
Extension of trade adjustment assistance (Trade Adjustment Assistance Extension Act of 2011).	10/12/2011	307-122	780

Several of the included votes were deemed to be key votes by the Congressional Quarterly and/or were included as salient votes in the legislative scorecard of the AFL-CIO. That said, the selection does not aim to identify all votes that are salient to unions nor to focus only on votes deemed key by the AFL-CIO. Rather, the goal is to capture important votes on trade and compensation.

B. Other data sources and measures

Chinese import exposure. Following Autor, Dorn, and Hanson (2013*a*), our measure of district-level import exposure is: $\Delta IPW_d^{US} = \sum_j (L_{dj}/L_{uj}) \times (\Delta M_{uj}/L_{dt_0})$. Here, ΔM_{uj} denotes 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}, and L_{uj} is total US industry employment. Finally, L_{dt_0} signifies a district's employment in industry j at the beginning the 1990s. The data in Autor, Dorn, and Hanson (2013*a*) are defined for groups of counties that create 741 local labor market areas or commuting zones (CZ). Each district is covered by at least one commuting zone and trade exposure is apportioned as a function of the population distribution in the district-commuting zone intersection. To do so, we create a crosswalk from (time-constant) commuting zones (in their 1990 definition) to the (potentially time-varying) congressional districts of the 107th to 112th Congresses. For each congress and district, the crosswalk provides a district's 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 district (in the 108th 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 (25%) is exposed to a neighboring commuting zone (Las Vegas, NV-AZ). This procedure straightforwardly deals with district boundary change (i.e., from redistricting) by assigning new spatial overlaps and populations weights when needed.[1] The data for the population fractions in each spatial intersection are based on fine-grained Census tract population counts available 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.

Instrument We instrument Chinese import exposure in the US using the well-known instrument $\Delta IPW_d^{oth} = \sum_j (L_{dj}^{1980}/L_{uj}^{1980}) \times (\Delta M_{oj}/L_{dt_0}^{1980})$, where 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. Figure B.1 illustrates the relevance of the instrument graphically for districts from the 109th Congress. 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 (but note the discussion on validity concerns in C.4 below).

Union data We use 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

[1] This implies the stacked analysis we carry out, where the units are state-districts stacked over congressional terms where identification stems from exogenous shifts to exposure. We also show that our results obtain without stacking, i.e., for each Congress.

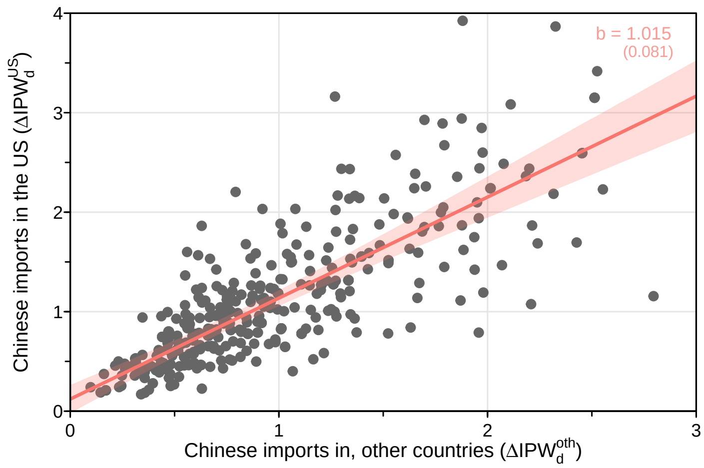

Figure B.1
Instrumenting Chinese import exposure

Note: 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 cluster-robust 95% confidence interval superimposed.

union, which was geocoded and then mapped onto congressional districts. Becher, Stegmueller, and Kaeppner (2018) conducted the mapping by point-polygon intersection, using the geolocation of each union and the polygons (shapefiles) for each Congressional district at each point in time from the National Historical Geographic Information System.

An advantage of this approach over using survey data is that it 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.[2] Substantively, most of the variation in union density in a given congressional term is between districts within the same state.[3]

Automation exposure To adjust for automation exposure, 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

[2]One 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."

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

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.[4]

District-level controls Covariates for districts based on the 1990 Census 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 (for each congress) using 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. 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. Finally, our measure of district ideology is based on Kernell (2009), which uses multiple election returns to estimate voters' ideological position in a district during the 1980s.[5]

Table B.1
Descriptive statistics

Statistic	Mean	St. Dev.	Pctl(25)	Pctl(75)
Union membership [/empl. pop]	0.099	0.191	0.027	0.110
Mining employment, 1950s	0.034	0.044	0.007	0.046
Chinese imports to US [/worker, 10-year eq.]	1.137	0.684	0.683	1.316
Chinese imports to other countries	1.001	0.531	0.591	1.261
Routine task intensity	0.323	0.024	0.307	0.342
Historical share of Blacks	0.113	0.161	0.009	0.183
Historical share of Foreign Born	0.151	0.116	0.031	0.222
Manufacturing employment	0.105	0.041	0.072	0.131
Share Black	0.101	0.121	0.020	0.135
Share Urban	0.237	0.090	0.164	0.316
Share with at least college degree	0.319	0.079	0.258	0.363

Historical covariates 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

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

[5]Kernell (2009) argues that this measure is superior to simply using presidential vote shares. However, we made sure that our results also hold when accounting for the district-level two-party vote share in the 1984 presidential election.

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 area intersection with Congressional districts (for each congress from the 107th to 112th) using standard GIS tools (the GEOS library). Based on this crosswalk we spatially weight the Black and foreign-born population in 1910 to congressional districts.

Descriptive Statistics Table B.1 shows descriptive statistics.

C. Additional results for trade shocks → union density

C.1. Model details

The instrumental variable models we estimate are as follows:

$$Y_{dc} = \beta_0 + \beta_1 IPW_{dc}^{US} + \gamma H_{dc} + \delta X_{dc} + \eta A_{dc} + \psi_{sc} + \epsilon_{dc} \tag{C.1}$$

where Y_{dc} represents union membership in district d during congressional term c, while IPW_{dc}^{US} represents district-level Chinese import exposure (instrumented with IPW_{dc}^{oth}), and ψ_{sc} are state-congress fixed effects. The model thus accounts for state-level characteristics that are constant over districts and for congress-specific shocks that can vary by state. District-level controls are: H_{dc}, the historical stocks of blacks and foreign born, X_{dc}, socio-demographic composition, and A_{dc}, potential automation exposure. All reported standard error are robust allowing for heterogeneity and within-state correlations, i.e., we use cluster-robust standard errors clustered at the state-congress level (in section D.4 below we discuss alternative strategies, including clustering at the district level and using a wild bootstrap for IV). The debiased/robust machine learning IV model follows the partially linear IV specification

$$Y_{dc} = \beta IPW_{dc}^{US} + \mu(H, X, A) + u_{dc} \tag{C.2}$$

$$IPW_{dc}^{oth} = \xi(H, X, A) + v_{dc} \tag{C.3}$$

where (H, X, A) is the set of controls that enters both structural equations via (possibly highly nonlinear) functions μ and ξ. See section F.1 on page 18 for more information.

C.2. Further robustness tests

Table C.1 reports additional estimates of the effect of import exposure on district-level unionization from six alternative specifications. In *specification (1)* of Table C.1 we show the OLS estimate of the effect of trade on district-level union membership. Note that under this specification the table entry now refers to a different parameter. The OLS estimate is slightly larger than the dML-IV estimate and somewhat smaller than the standard IV estimates in our main specifications. *Specification (2)* of Table C.1 shows 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.

Table C.1
Trade shocks and union membership. Robustness tests.

	Est.
(1) OLS (no instrument)	−0.194
	(0.029)
(2) Contemporary district controls (X_{dc}^{2000})	−0.230
	(0.050)
(3) Automation instrument	−0.314
	(0.044)
(4) Offshorability index	−0.348
	(0.034)
(5) Trade exposure 2000s	−0.140
	(0.031)
(6) Rotemberg weight IV sensitivity [a]	−0.519
	(0.059)
(7) Industry-union shares, 1990	−0.231
	(0.043)

Note: 2SLS estimates (exc. (1)). Cluster-robust standard errors in parentheses.
a Sensitivity to influential industry shares following Goldsmith-Pinkham, Sorkin, and Swift (2020). Analysis proceeds in two steps: (1) Estimate Rotemberg weights α_k for each industry-share-specific instrument; (2) re-calculate instrumental variable excluding overly influential industries (with $\alpha_k > 0.05$).

In specification (3), we instrument automation exposure. 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 (3) 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. *Specification (4)* adds a measure of offshorability based on Autor and Dorn (2013). 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. *Specification (5)* uses import exposure from 2000-2007 rather than the prior decade as the treatment variable. We prefer to use lagged import exposure as it is more plausibly exogenous. Nonetheless, using contemporaneous exposure yields the same basic result. In *specification (6)* we probe the sensitivity of the trade instrument. Following Goldsmith-Pinkham, Sorkin, and Swift (2020), we estimate Rotemberg weights for each industry-share-specific instrument. Table C.2 presents an overview of these weights. Notably it shows that the very largest weights occur for miscellaneous electronic components and computer peripherals. The next largest weights refer to photographic and household audio and video equipment. In our robustness tests, we exclude industries with the largest weights ($\alpha_k \geq 0.05$). Our substantive results are confirmed. We find a larger effect of the trade shock on union membership (with somewhat larger standard errors).

Table C.2
Industries with largest Rotemberg weights

	α_k
Electronic Components, nec	0.138
Computer Peripheral Equipment, nec	0.107
Photographic Equipment and Supplies	0.075
Household Audio and Video Equipment	0.063
Canned and Cured Fish and Seafoods	0.051
Electronic Computers	0.035
Games, Toys, Children's Vehicles (exc. dolls, bicyles)	0.031
Semiconductors and Related Devices	0.026

Note: Calculated following the methodology outlined in Goldsmith-Pinkham, Sorkin, and Swift (2020). Rotemberg weights, α_k for a given industry k. The analysis contains 395 SIC industries. Mean of negative weights: -0.049, mean of positive weights 1.049.

Finally, in *specification (7)* we include a proxy for industry-specific unionization rates in each district in the 1990s in order to account for the potentially differential impact of the trade shock in more or less unionized industries. The proxy is constructed as follows.[6] (i) We calculate state-level unionization rates (union members out of total employed persons) for 14 industries from the Current Population Survey Annual Social and Economic Supplement in 1990. The selected population consists of all individuals who are in the labor force. All calculations adjust for sample inclusion probabilities and are population weighted. This step yields $48 \times 14 = 672$ state-industry observations. (ii) Using the 1990 Census we estimate employment by industry for each district (in each Congress). The selected population consists of individuals in the labor force. All calculations adjust for sample inclusion probabilities and are population weighted. This step yields $432 \times 14 = 6048$ district-industry observations for the 107th Congress (and mutatis mutandis for the other Congresses) which we match to the corresponding state-industry observations from step (i). (iii) The product of the quantities calculated in (i) and (ii) yields an estimate of the number of unionized workers in each industry in each district. We express it as a share of the total employed population in a district. The final step thus yields 14 industry-variables, which capture variation in unionization rates in different districts in 1990. Including these industry-specific unionization rates in the model does indeed produce a smaller estimate compared to the main model and likely captures some of the effect heterogeneity of the trade shock. Nonetheless, its effect remains sizeable and statistically different from zero. We also account for these variables in the mediation analyses of the shock on roll call votes; see section G.2.

[6]Note that we do not have LM-form based information on union density in the 1990s, nor a breakdown of union membership by industry. Therefore, this measure is a proxy calculated by projecting state-level estimates of industry-union density to the district level.

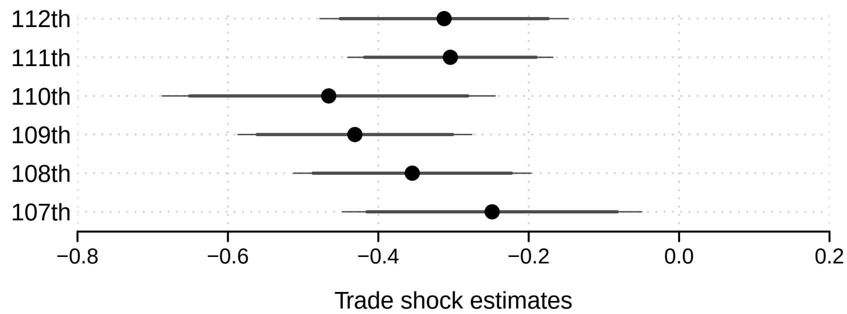

Figure C.1
Effect of trade shocks on union membership by Congress.

Note: Instrumental variable estimates for $U_d = \beta \Delta IPW_d + H'_d\gamma + X'_d\delta + \psi_i + \epsilon_i$ separately for each Congress. Cluster-robust standard errors. N=432.

C.3. Congress-specific estimates

Figure C.1 shows instrumental variable regressions of union density on trade shocks separately for each Congress. We find the relationship to be rather similar across Congresses (with smaller estimates in the 111th and 112th). All estimates are negative and statistically distinguishable from zero.

C.4. Alternative measures of trade exposure

In this section we explore if we recover our core result using alternative measures of trade exposure. Exploring alternative sources of variation is of relevance due to the fact the identifying variation in a shift-share type instrument is best understood as stemming from the initial employment shares (Goldsmith-Pinkham, Sorkin, and Swift 2020) or the industry-level import shocks (Borusyak, Hull, and Jaravel 2021). Remember that the instrument uses decade lags and our initial shares of industry employment refer to the 1980s. The existence of correlates to 1980s industry shares or of parallel pre-trends in some industries threatens the validity of the exclusion restriction. Goldsmith-Pinkham, Sorkin, and Swift (2020: Online appendix A) do indeed find evidence of pre-trends when using manufacturing employment as outcome variable. Unfortunately, we cannot carry out a similar inspection in our analysis, because our outcome of interest—district-level union membership—is not available before 2001. To increase the likelihood that our findings are not dependent on arbitrary characteristics of this particular instrument, we do the following: (i) we control for automation exposure (Table I) and increased offshorability of jobs (Table C.1), which arguably captures important parallel developments affecting similar industries; (ii) we use double machine learning IV specifications that allow for much more flexible (high-dimensional) controls and are more likely (compared to standard linear IV models) to capture confounders (or complex interactions of confounders) that cause omitted variable bias; (iii) we ensured that our results also obtain in a simple linear model without the instrument; (iv) we conduct an analysis of Rotemberg weights for our model and exclude industries with large weights in a robustness test (Table C.1); (v) we explore two alternative *measures* of Chinese import exposure in the remainder of this section.

Table C.3
Effect of trade shocks on union membership using alternative measures for trade exposure

	(1)	(2)
(1) Gravity model residuals	−0.174	−0.217
	(0.034)	(0.044)
(2) NTR gap	−0.144	−0.314
	(0.038)	(0.053)
District characteristics		
Slavery & immigration history	✓	✓
Demographics & ideology		✓
State & time FE	✓	✓

Note: OLS estimates with cluster-robust standard errors. Gravity residual based on Autor, Dorn, and Hanson (2013*a*). NTR gap calculated from data in Greenland, Lopresti, and McHenry (2019) spatially reweighted to Congressional districts. Both measures are scaled to have the same standard deviation as the Chinese import exposure measure.

In specification (1) 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). The (standardized) within-state relationship between district values of the instrument and the gravity-based measure is 0.63 ± 0.02. In specification (2) we use an alternative measure based on the work of Pierce and Schott (2016). It uses the difference in tariffs imposed under regular rules (applying to non-market economies) versus reduced tariffs imposed under Normal Trade Relations (NTR) with China. *Permanent* NTR came into force following China's accession to the WTO; in years prior (since the 1980s) the reduced tariffs had to be re-confirmed annually by the US. The switch to permanent low-tariff status reduced uncertainty of Chinese producers to invest or expand in the US market. Importantly, this tariff gap varies over industries and localities (Pierce and Schott 2016: 1633). We follow Greenland, Lopresti, and McHenry (2019), who construct a measure of Chinese import competition as an industry-weighted average of exposure to changing tariffs on the commuting zone level. More precisely, denote by λ_{cj} the employment share of industry j in commuting zone c. The measure of commuting zone-level exposure is given by (Greenland, Lopresti, and McHenry 2019: appendix 9): $\text{NTR gap}_c = \sum_j \lambda_{cj} \text{NTR gap}_j$. We spatially interpolate the commuting zone data to congressional districts (using the same methodology employed in the main text). The within-state relationship between district values of the ADH instrument and the NTR exposure gap measure is 0.64 ± 0.01.

Table C.3 shows OLS estimates resulting from models with historical controls (in the first column) and an extensive vector of district characteristics (in the second column). Our results show that using alternative measures of import exposure—one constructed using trade data, the other constructed using tariffs—leads to the same substantive conclusion.

D. Additional results for union density → roll call votes

D.1. Robustness tests

Table D.1
Union membership and roll call votes. Robustness tests.

	Trade	Compensation
(1) OLS (no instrument)	−0.124	0.139
	(0.012)	(0.011)
(2) IV without controls X_{dc}, H_{dc}	−0.414	0.266
	(0.092)	(0.060)
(3) Offshorability index	−0.416	0.199
	(0.120)	(0.076)
(4) Foreign direct investment	−0.457	0.234
	(0.131)	(0.078)
(5) Social capital index	−0.389	0.178
	(0.110)	(0.072)
(6) Industry empl. shares 1950s	−0.435	0.327
	(0.156)	(0.114)
(7) Contemporary industry mix	−0.346	0.273
	(0.109)	(0.088)

Note: 2SLS estimates (exc. (1)) with cluster-robust standard errors. See text for details on variable definitions.

In *specification (1)* of Table D.1, we show that our core results also obtain without relying on the mining instrument (again, 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)* includes the measure of offshorability as discussed for specification (4) in Table C.1. *Specification (4)* in Table D.1 shows estimates of the impact of union membership on roll call votes for economic compensation and trade bills when adjusting for district-level employment in firms with at least 10 percent of foreign ownership.[7] *Specification (5)* includes district levels of social capital (Rupasingha and Goetz 2008) as a proxy for a district's organizational capacity, which might shape both (past) union strength and legislators' voting behavior.

Specifications (6) and (7) provide a stricter specification of the instrumental variable analysis. First, because the mining instrument is based on employment *shares* in highly unionized industries, a potential threat to inference stems from endogenous factors shaping the denominator, i.e., the

[7]We use data from a joint database of the U.S. Bureau of Labor Statistics and the U.S. Bureau of Economic Analysis. It provides county-level information on employment numbers in domestically and foreign-owned establishments. Establishments with foreign ownership are defined as establishments with at least one foreign owner with at least 10 percent ownership during 2012 (counties with all firms below that threshold are excluded and we treat them as zero). We spatially aggregate county-level employment in foreign-owned firms to congressional districts using the polygon intersection between counties and districts weighting by the fraction of the population in each intersection.

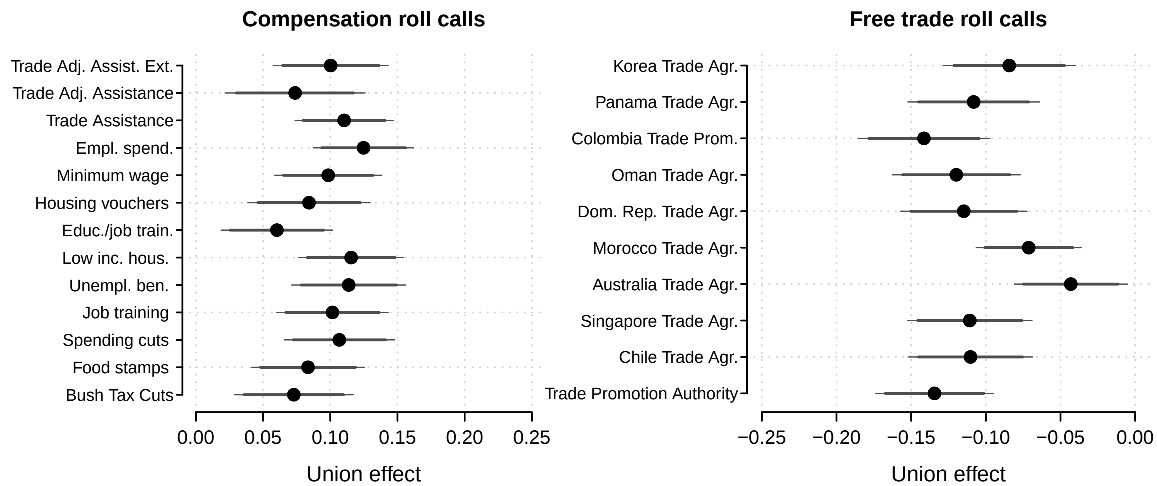

Figure D.1
Union density and individual legislative votes

Note: Estimates separately by roll call vote (with 90% and 95% confidence intervals based on cluster-robust variance estimates).

historical size of the industrial base. For example, larger mining operations might have occurred in regions that already had a larger industrial base due to a more entrepreneurial environment (which still affects outcomes today). To account for this possibility, we calculate the share of industrial employment (out of total employment) for each district in the 1950s. Adding this measure as a control in specification (6) leads to estimates with larger standard errors. However, for both trade and competition votes, estimates are still clearly statistically different from zero. The point estimate for trade votes is close to the one reported in the main text, while the point estimate for compensation votes increases by about 10 percentage points. Second, the size of the historical industrial base might be correlated with the current industrial composition of a district. We account for the latter by calculating beginning-of-period employment shares in mining, construction, durable- and non-durable goods manufacturing. These four variables are added as controls in specification (7). The resulting estimate for trade votes is somewhat smaller than the one reported in the main text, but still clearly statistically and substantively different from zero. The corresponding estimate for compensation votes is close to the one reported in the main text. All in all, these extended specifications confirm our core results.

D.2. Roll call-specific estimates

Figure D.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.

D.3. Examining the LPM

In this subsection, we examine an alternative specification to the linear probability instrumental variable estimation strategy employed in the main text. Roll call votes are dichotomous and applying a linear probability model can produce predicted probabilities that are outside the unit interval.[8] Proponents of using the LPM usually point to its simplicity, lack of functional form assumptions for the response distribution, and to the fact that average causal response (or average derivative) estimates tend to be very close to alternative limited dependent variable (LDV) ones (Angrist and Pischke 2009: 96-99,105-107,197f). This applies especially in models with endogenous covariates (Angrist and Pischke 2009: 197f; Wooldridge 2010: 585).

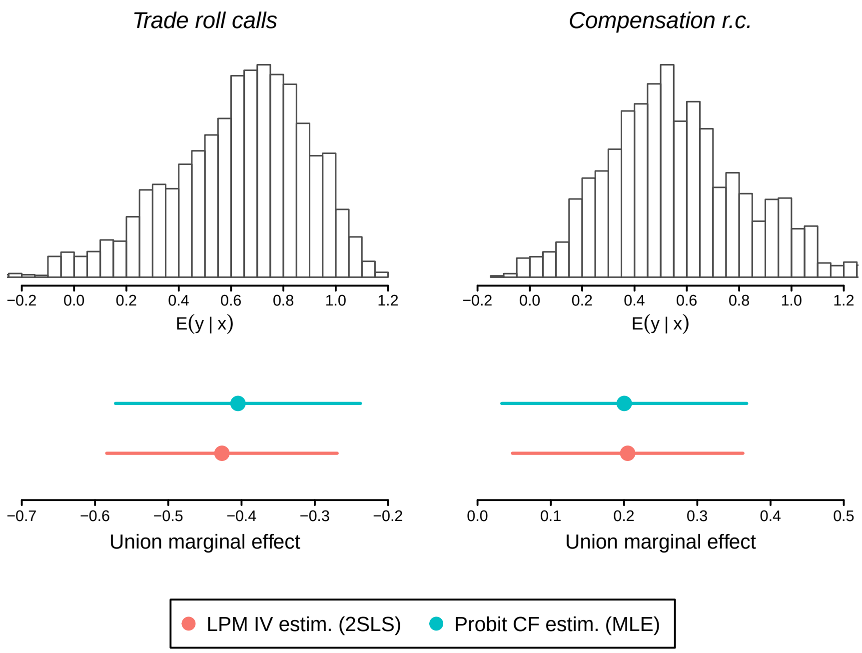

Figure D.2
Comparison of IV estimators: LPM (2SLS) vs. probit (control function, MLE)

Note: The top half shows histograms of expected values from the linear probability instrumental variable model for all legislator-roll call pairs (in all Congresses). The bottom half compares union strength marginal effects from the LPM IV estimator to ASF marginal effects from the probit control function estimator. Models follow specification (3) of Table II.

The top half of Figure D.2 shows histograms of expected values from the linear probability instrumental variable model for all legislator-roll call pairs (in all Congresses). We find that for both trade and compensation roll calls, the vast majority of predictions (more than 92%) are within the [0,1] interval. But some values do indeed occur outside the unit interval (although the magnitude of the excess predictions is rather limited). Further investigations showed that predictions outside the unit interval are not concentrated in specific roll calls or congress sessions. Nonetheless, it is prudent to examine the robustness of our results to using an alternative LDV estimator that forces probabilities to lie in the unit interval. We estimate instrumental variable probit models for roll-calls, where the endogeneity of union strength is addressed using a control function approach. The model is estimated via conditional maximum likelihood (Wooldridge 2010: 591). We then calculate the average structural function marginal effects (Blundell and Powell 2003;

[8]The LPM also produces heteroskedastic residuals, but we use heteroskedasticity-consistent standard errors throughout.

Wooldridge 2010: 588) of union strength. The lower half of Figure D.2 compares effect estimates from the LPM and the probit control function estimator. It reveals that union effect estimates are quite similar in both in the substantive sense and in quantitative magnitude (with a slight difference in robust 95% confidence bands). All in all, this supports our decision to rely on the LPM strategy for our main models.

D.4. Alternative cluster-robust variance estimates

In this subsection we explore the impact of different variance estimation strategies and different levels of clustering. First, we compare cluster robust variance estimation (CRVE) to the wild restricted efficient residual (WRE) bootstrap for the IV estimator (Davidson and MacKinnon 2010: 131), which allows for flexible clustering, heteroscedasticity of unknown form, and is robust under weak instruments. Second, we compare different clustering strategies, namely clustering at the state-congress level, which we use in the main text in order to allow for within-state correlation between districts (in each Congress), and clustering at the district level. For each case and for each outcome, we calculate interval estimates (in the form of 95% confidence intervals) shown in Figure D.3. We find, first, that the WRE bootstrap intervals are reasonably close to the CRVE-based ones (which are computationally much less costly), which increases confidence in our decision of using the latter in the main text. Second, we find that district-clustering does not have a large systematic impact on the interval estimates: they are somewhat larger for both union membership and trade votes, but slightly smaller for compensation votes. On balance, the differences are small, and our substantive results are obtained regardless of variance estimation strategy employed.

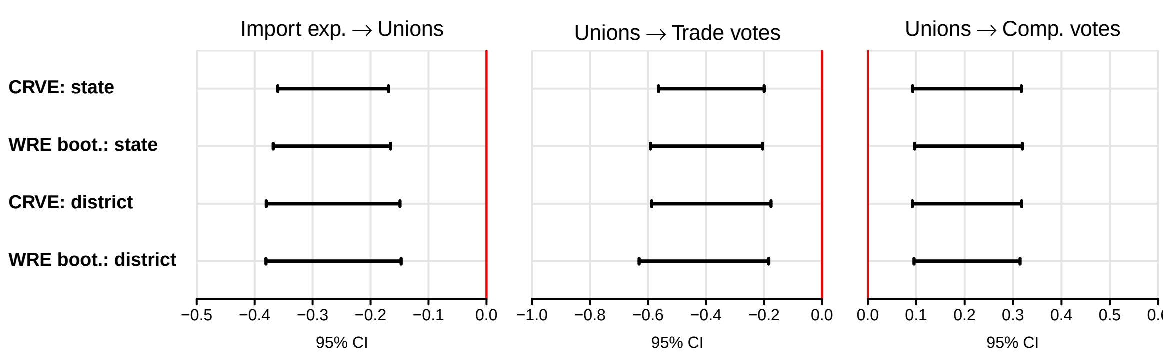

Figure D.3
Comparison of interval estimates for different levels of clustering and variance estimation

This plot shows interval estimates (95% CI) resulting from two-way cluster robust variance estimation (CRVE) and the wild restricted efficient residual (WRE) bootstrap (Davidson and MacKinnon 2010: 131). The top two entries follow the approach taken in the main text with clustering at the state-congress level in order to allow for arbitrary district-correlations within states. The bottom two entries cluster instead at the district-congress level. WRE bootstrap calculated imposing the null hypothesis over 1,999 replicates (using Rademacher weights).

E. Invalid IV robust inference

Using simplified notation, for outcome Y, treatment T and instrument Z, the structural model is given the outcome equation $U = X\beta_X + \beta T + \delta Z + v$ and the treatment equation $T = X\gamma_X + \gamma Z + w$ with errors v and w bivariate normally distributed and independent of Z. After projecting out covariates using the Frisch-Waugh-Lovell theorem, and rescaling δ we get $U^* = \beta T^* + \delta\sigma_1 Z^* + v^*$ and $T^* = \gamma Z^* + w^*$, where quantities with star superscripts are 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.[9] In our analyses, we assume that $\delta \in (-\bar{\delta}, +\bar{\delta})$. We set $\bar{\delta}$ to be up to one fifth of the (standardized) treatment effect estimate. The reported quantities are p-values from the corresponding Andersen-Rubin test $H_0 : \beta = \beta_0$ (Wang et al. 2018):

$$AR(\beta_0) = \frac{(U^* - \beta_0 T)' P_{Z^*} (U^* - \beta_0 T)}{(U^* - \beta_0 T)' M_{Z^*} (U^* - \beta_0 T)/(n-k-1)} \tag{E.1}$$

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

F. Debiased Machine Learning Estimation of Partially Linear IV Model

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

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

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

with $E(\zeta|X, Z) = 0$ and $E(V|X) = 0$. Here Y is an outcome, say union density, T is a treatment, say Chinese import exposure, and Z is the corresponding instrument. The instrument Z induces variation in T 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, 2018) for more recent work in a TLME framework). Note further that confounding of the instrument occurs when elements of X influence not only the outcome but also the instrument, that is, if the function $m_0 \neq 0$.

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

One can re-express this model in residualized form

$$V_Y = V_T\theta_0 + \zeta, \quad E[\zeta \mid V_Z, X] = 0, \tag{F.3}$$
$$V_Y = (Y - \ell_0(X)), \quad \ell_0(X) = E[Y \mid X] \tag{F.4}$$
$$V_T = (T - r_0(X)), \quad r_0(X) = E[T \mid X] \tag{F.5}$$
$$V_Z = (Z - m_0(X)), \quad m_0(X) = E[Z \mid X], \tag{F.6}$$

where the variables above 'partial out' the effect of X.[10] In this setup, all three nuisance parameter are conditional mean functions, which can be learned using standard machine learning tools, such as neural networks or regression trees. With the resulting estimates of $\hat{\ell}_0$, $\hat{m}_0$, and $\hat{r}_0$ in hand, we can estimate residuals (similar to Robinson 1988)

$$\hat{V}_Y = Y - \hat{\ell}_0(X) \tag{F.7}$$
$$\hat{V}_T = T - \hat{r}_0(X) \tag{F.8}$$
$$\hat{V}_Z = Z - \hat{m}_0(X) \tag{F.9}$$

and use these to estimate our parameter of interest. We estimate θ_0 by 2SLS regression of $\hat{V}_Y$ on $\hat{V}_T$ with $\hat{V}_Z$ 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).[11] In each split, we use 5-fold cross-fitting (Chernozhukov et al. 2018: C23). One issue with using increasingly flexible ML models 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 F.1 for parameter details and estimates. We find that different ML tools yield rather similar estimates for θ_0. The last column of Table F.1 shows results where we combine the best performing (in terms of predictive accuracy, measured by MSE) machine learning 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

[10] This setup uses the score function $\psi(Y, T, Z, X; \theta, \eta) := (Y - \ell(X) - \theta(T - r(X)))(Z - m(X)), \quad \eta = (\ell, m, r)$, 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 $r_0(X) = E_P(T|X)$ it satisfies both the moment condition (Chernozhukov et al. 2018: eq.2.1) $E_P[\psi(Y, T, Z, X; \theta_0, \eta_0)] = 0$ and the Neyman orthogonality condition (Chernozhukov et al. 2018: eq.2.3) $\partial_\eta E_P[\psi(Y, T, Z, X; \theta_0, \eta_0)][\eta - \eta_0] = 0$. The (Gateaux) derivative being equal to zero ensures that our score function is robust to small perturbations of the nuisance functions estimates.

[11] The results in Chernozhukov et al. (2018) imply that the machine learning 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 F.1
Debiased Machine Learning Instrumental Variable Estimates.

	Reg.trees[a]	Boosting[b]	R. Forest[c]	Neural N.[d]	Best[e]
(A) Union density					
Estimate	−0.175	−0.170	−0.169	−0.166	−0.170
SE	(0.031)	(0.030)	(0.027)	(0.027)	(0.030)
$MSE(Y\|X)$	0.974	0.948	0.994	1.020	
$MSE(T\|X)$	0.952	0.860	0.974	0.990	
$MSE(Z\|X)$	0.935	0.875	0.987	1.064	
(B) Trade roll calls					
Estimate	−0.830	−0.899	−0.770	−0.799	−0.899
SE	(0.171)	(0.157)	(0.110)	(0.111)	(0.157)
$MSE(Y\|X)$	0.977	0.938	0.994	0.997	
$MSE(T\|X)$	0.967	0.872	0.993	0.999	
$MSE(Z\|X)$	0.945	0.808	0.981	0.985	
(C) Compensation roll calls					
Estimate	0.491	0.466	0.481	0.508	0.466
SE	(0.122)	(0.116)	(0.084)	(0.084)	(0.116)
$MSE(Y\|X)$	0.988	0.928	0.994	0.994	
$MSE(T\|X)$	0.967	0.866	0.992	1.026	
$MSE(Z\|X)$	0.934	0.795	0.982	0.987	

Note: Based on debiased Machine Learning Instrumental Variable estimates using 5-fold cross-fitting on 100 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.

regularization bias arising from a naive application of machine learning tools and the use of cross-fitting.

G. Causal effect decomposition with two instruments

G.1. Model details and estimation

We have the following nonparametric system of equations that describes the relationship between an intense trade shock, T, union strength, U, and roll call votes, R:

$$R = \lambda(T, U, X, V_R) \tag{G.1}$$
$$U = \mu(T, Z_2, X, V_U) \tag{G.2}$$
$$T = \mathbf{1}(\xi(Z_1, X, V_T) \geq 0) \tag{G.3}$$

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.[12] Unobservable V_R, V_U, V_T can be related in arbitrary fashion leading to endogeneity of T and U. Frölich and Huber (2017) propose to deal with this endogeneity in two ways: endogeneity of T is addressed using standard semiparametric LATE-type assumptions (Abadie 2003), endogeneity in U is addressed using a nonparametric control function approach.

Key causal parameters First, let's use the above notation to define the relevant potential outcomes that are needed to define the total, direct, and indirect causal paths of our decomposition. Under treatment $t, t' \in \{0, 1\}$, the potential outcome is given by $R(t, U(t'))$, while the potential mediator is given by $U(t)$. In terms of the model equations defined above, these parameters are defined as:

$$U(t) = \mu(t, z_2, X, V_U) \tag{G.4}$$

$$R(t, U(t') = \lambda(t, U(t'), X, V_R)) = \lambda(t, \mu(t', Z_2, X, V_U), X, V_R). \tag{G.5}$$

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

$$T(z_1) = \mathbf{1}(\xi(z_1, X, V_T) \geq 0). \tag{G.6}$$

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

[12] For this analysis, we need to create a binary treatment. We do so by splitting the continuous trade exposure measures at the median (more precisely, $Q_{IPW}(0.505)$), so that a value of 1 indicates a \$1,000 increase per worker. We also repeat our analysis using a split at a higher quantile ($Q_{IPW}(0.6)$) and find substantially similar results. We also inspect if our so discretized instrument violates observable implications of the LATE assumptions. Of course, one should stress that the validity of an instrument will never be *confirmed*; it can only be *refuted*. However, subjecting observable implications of a model to more empirically refutable tests should increase our confidence in the conclusions drawn from that model. We use the tests suggested by Huber and Mellace (2015) and Kitagawa (2015). In both cases we do not reject the null hypothesis of instrument validity in this specific application.

indirect effect), and the remaining natural direct effect (NDE). They are given by:

$$\begin{aligned}\text{LATE} &= E\{R(1) - R(0)|\Pi = co\} &\text{(G.7)}\\ &= E\{R(1, U(1)) - R(0, U(0))|\Pi = co\}\\ NDE(t) &= E\{R(1, U(t)) - R(0, U(t))|\Pi = co\} &\text{(G.8)}\\ NIE(t) &= E\{R(t, U(1)) - R(t, U(0))|\Pi = co\} &\text{(G.9)}\end{aligned}$$

for $t \in \{0, 1\}$ and where $\Pi = co$ denotes the set of compliers (where $T(1) - T(0) = 1$; cf. the discussion in the next section). Note that NDE and NIE are heterogeneous over treatment, thus allowing for treatment-mediator interactions. When reporting our results, we focus on $NDE(1)$ and $NIE(1)$.

Adjusting for mediator endogeneity The endogeneity of the mediator could be addressed trivially by conditioning on V_U—if it were observable. Because it is unobserved, replace it with an estimated control function that leverages instrument for the mediator (Z_2) (Heckman and Robb 1985; Imbens and Newey 2009), which is given by

$$C(m, t, z_2, x) = \frac{E\{[t + T - 1][Z_1 - \pi(x)]|M \leq m, Z_2 = z_2, X = x\}}{E\{T[Z - \pi(x)]|Z_2 = z_2, X = x\}} \times F_{M|Z_2,X}(m, z_2, x) \qquad \text{(G.10)}$$

Here, $F_{M|Z_2,X}$ denotes the conditional cumulative distribution of union strength conditional on the union instrument and controls, while $\pi(x)$ denotes the propensity score of the trade shock instrument, $P(Z_1 = 1|X)$. Under the assumptions listed below, it can be shown that C is a one-to-one mapping of V_U (Frölich and Huber 2017, Lemma 1). Thus conditioning on C controls for the endogeneity of the mediator. The conditional expectations of (G.10) are estimated via least squares while the cumulative distribution function, $F_{M|Z_2,X}(m, z_2, x)$ is estimated using nonparametric (multivariate) kernel density estimation (Li and Racine 2003, 2007). One issue with nonparametric density estimation is that convergence slows down as the dimension of the covariate space increases, because the number of observations required to estimate the density at any given point increases exponentially (Bellman 1957; Silverman 1986; Wasserman 2006). This makes bandwidth selection difficult and can lead to undersmoothing (and results in extreme local density estimates). To combat this problem we employ a dimension reduction step before the density estimation (Scott 2015) and project covariates into a lower-dimensional space using principal component analysis (Jolliffe 2002; Cunningham and Ghahramani 2015). In our analyses, we use the four largest eigenvectors (we study the robustness to other choices in section G.4). We employ a second order Gaussian kernel. Bandwidths are allowed to differ across dimensions, and we select them using the normal-reference approximation of Silverman (1986).

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:[13]

- $(Z_1, Z_2) \perp (V_R, V_U, V_T)|X$, instruments are independent of unobservables given X

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

- $Z_1 \perp Z_2|X$, the instrument for the treatment is independent of the instrument for the mediator after conditioning on controls (cf. panels (b) and (c) of Figure II)
- strict monotonicity of mediator in V_U 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:

$$E\{R(1,U(1))|\Pi = co\} = \frac{E\{Y\,T\,\Psi\}}{E\{T\,\Psi\}} \tag{G.11}$$

$$E\{R(1,U(0))|\Pi = co\} = \frac{E\{Y\,T\,\Omega\,\Psi\}}{E\{T\,\Psi\}} \tag{G.12}$$

$$E\{R(0,U(1))|\Pi = co\} = \frac{E\{Y\,(T-1)\,\Omega^{-1}\,\Psi\}}{E\{T\,\Psi\}} \tag{G.13}$$

$$E\{R(0,U(0))|\Pi = co\} = \frac{E\{Y\,(T-1)\,\Psi\}}{E\{T\,\Psi\}} \tag{G.14}$$

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

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

With an estimate for the control function obtained in the 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 (G.9) for $t = 1$ is defined as $E\{R(1,U(1)) - R(1,U(0))|T(1) - T(0) = 1\}$ and can thus be obtained by subtracting (G.12) from (G.11). Similarly, the natural direct effect, $E\{R(1,U(1)) - R(0,U(1))|T(1) - T(0) = 1\}$, is obtained by subtracting (G.13) from (G.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 $T, T-1, \Psi, \Omega$ and Ω^{-1} in the mean potential outcome equations (G.11) to (G.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).

G.2. Robustness tests

We conduct a number of robustness and specification tests. Table G.1 shows resulting estimates (indirect and direct effects as well as the proportion mediated). *Specifications (1) and (2)* include additional district-level controls (for details on the added covariates, see section D.1 on page 12). Results indicate that accounting for offshorability of jobs in a district and for the amount of social capital does not substantively alter the indirect effect of the trade shock on legislative behavior *via* union strength. *Specification (3)* employs import exposure during the later (2000-2007) period as the trade treatment, rather than 1990-2000 as in our main analysis. While we believe that lagged import exposure is more plausibly exogenous, the alternative measure broadly confirms our substantive results.

Table G.1
Indirect and direct effect estimates under alternative specifications.

	Compensation votes			Trade votes		
	Indirect	Direct	%	Indirect	Direct	%
(1) Offshorability	−0.068	−0.062	52	0.052	0.105	33
	(0.020)	(0.043)		(0.018)	(0.039)	
(2) Social capital index	−0.079	−0.068	53	0.064	0.114	35
	(0.021)	(0.042)		(0.020)	(0.041)	
(3) Trade exposure 2000s	−0.081	−0.084	49	0.077	0.097	44
	(0.021)	(0.042)		(0.021)	(0.033)	
(4) Weights trimmed at 1%	−0.074	−0.046	58	0.059	0.105	36
	(0.021)	(0.054)		(0.019)	(0.043)	
(5) Treatment quantile, $q = 0.6$	−0.111	−0.055	67	0.086	0.067	56
	(0.036)	(0.033)		(0.034)	(0.035)	
(6) Rotemberg weight IV sens.[a]	−0.092	−0.080	57	0.069	0.087	45
	(0.044)	(0.050)		(0.034)	(0.034)	
(7) Industry-union shares, 1990	−0.076	−0.059	55	0.059	0.111	35
	(0.021)	(0.043)		(0.021)	(0.042)	
(8) Industrial employment, 1950	−0.074	−0.059	55	0.059	0.111	35
	(0.020)	(0.042)		(0.020)	(0.042)	
(9) Contemporary industry mix	−0.075	−0.059	55	0.059	0.110	35
	(0.020)	(0.042)		(0.019)	(0.042)	

Note: Based on specification (2) of mediation model.

a Analysis of sensitivity of results to influential industry shares following Goldsmith-Pinkham, Sorkin, and Swift (2020). Analysis proceeds in two steps: (1) Estimate Rotemberg weights α_k for each industry-share-specific instrument; (2) re-calculate instrumental variable excluding overly influential industries (with $\alpha_k > 0.05$).

Specification (4) uses a different trim level for the distribution of the inverse probability weights (trimming only the most extreme 1% instead of 5%). In *specification (5)* we split the trade shock distribution at the 0.6th quantile, that is, we use a higher threshold to define what it means for a district to be treated. *Specification (6)* uses the alternative trade shock instrument that excludes industries with large Rotemberg weights as discussed in section D.1 on page 12. While we find slightly smaller percent mediated estimates for both compensation and roll call votes, the results are consistent with our main findings. *Specification (7)* includes the proxy measure for beginning-of-period industry unionization rates (for each district) discussed in section C.2. This accounts for the heterogeneous impact of the trade shock on districts with differing levels of union density in specific industries. This leaves our estimates relatively unchanged; the indirect effect is slightly larger than in the main text, but the overall share mediated remains at 55%. Finally, *specifications (8) and (9)* account for the size and composition of the industrial base in a district, both historically and contemporary (as discussed in more detail for specifications 7 and 8 in section D.1). All in all, we find our core findings on the mediation role of unions confirmed.

G.3. Influential roll calls and roll-call-specific estimates

Our main mediation model is estimated using a pooled sample of trade or compensation roll calls. The relative large sample size in these analyses is needed to adequately estimate the cumulative distribution function $F_{M|Z_2,X}(m, z_2, x)$ via nonparametric kernel density estimation. However, it is reasonable to ask whether the overall results are driven by heterogeneity in roll calls. In the worst case, a few roll calls could drive the results. We tackle this issue in two ways. First, by identifying potentially influential roll calls and reestimating our model with these roll calls removed. Second, we estimate the mediation model for each roll call separately.

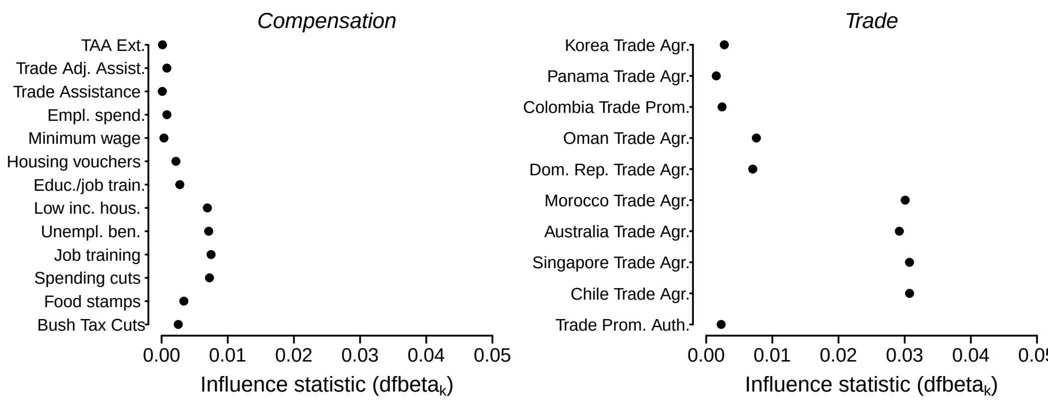

Figure G.1
Identifying potentially influential roll calls

Note: This figure plots influence statistics, dfbeta$_k$, for indirect effect estimates. Calculated from k-replicate group jackknife (Kott 1998) of trade and compensation roll calls using the two instrument mediation model (following specification (2) in Table III).

Influential roll calls We use a group jackknife procedure removing roll calls from the estimation of the mediation model in order to identify potentially influential roll calls. This yields K replicate data sets. Denote by $\hat{\beta}_k^{IE}$ the estimate of the indirect effect from the kth jackknife replicate and by $\hat{\beta}^{IE} = K^{-1} \sum_1^K \hat{\beta}_k^{IE}$ the overall jacknifed estimate. We calculate a common influence statistic, *dfbeta*, for each roll call as $dfbeta_k = \hat{\beta}_k^{IE} - \hat{\beta}^{IE}$. Figure G.1 shows the influence statistics for indirect effect estimates for each roll call. We find that for compensation roll calls no single roll call emerges as influential. For trade roll calls, four roll calls have influence statistics that stand out even though they are not overly large in magnitude (note the scale of the x-axis). We remove these four roll calls and reestimate the model for trade roll calls. The results are as follows: the estimated indirect effect is 0.078 (s.e. 0.030) while the direct effect has increased to 0.136 (s.e. 0.050). Thus, we find a slightly larger indirect effect estimate (with a share mediated of 37% vs. 35%), but the overall results remain consistent with our main findings.

Roll-call heterogeneity We also estimate the mediation model for each roll call separately. Figure G.2 on the following page shows indirect effect estimates for each roll call. The NIE estimate (and confidence interval) from the main model are shown in red. The reader should keep in mind, as discussed above, that at sample sizes of less than 500, estimates of the semiparametric estimator are less reliable. Nonetheless, we can interpret the pattern of heterogeneity across roll call votes. Starting with roll calls dealing with policies compensating workers, we find that the indirect effect estimates are remarkably stable across roll calls. For trade roll calls, the estimates are somewhat more variable. Several estimates are small, close to zero, and outside the confidence band of the main indirect effect estimate. Notably, these are the roll calls that we identified as having higher *dfbetas* in the jacknifing analyses above. However, none of the estimates indicates a clearly negative indirect effect. As reported above, we conduct an analysis where we remove the four most influential trade roll calls and find that the overall results remain consistent with our main findings.

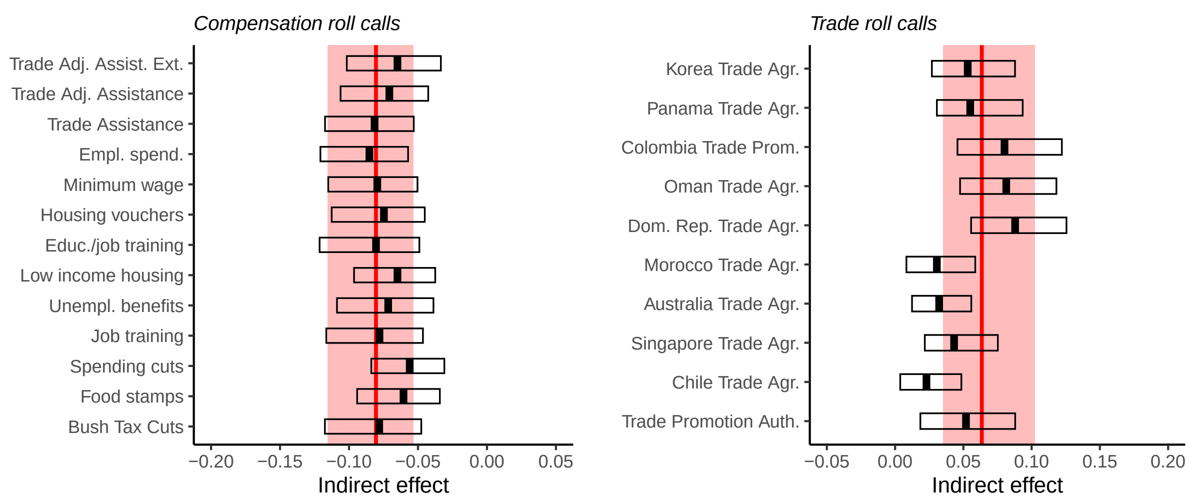

Figure G.2
Indirect effect estimates by roll call

G.4. Number of eigenvectors

We explore the robustness of the estimates to different choices for the number of eigenvectors used in the PCA step of the conditional density estimation (cf. equation (G.10)). Figure G.3 on the next page shows that different choices of dimensionality do not lead to substantial differences in the final estimates for both indirect and direct effects.

G.5. Placebo Monte Carlo study for NDE and NIE recovery

In this final subsection, we conduct a small placebo (or refutation) study of the 2-instrument estimator of the mediation model. In a data set with confounding between treatment, mediator, and outcome, it is germane to ask whether the estimator is able to correctly recover the *absence* of a direct or indirect effect (i.e., it answers the suspicion that the shifting instruments produce spurious results for the mediation paths). We do so by simulating data from a "placebo" model where either the NIE or the NDE is zero. Figure G.4 show directed acyclic graphs (DAGs) that

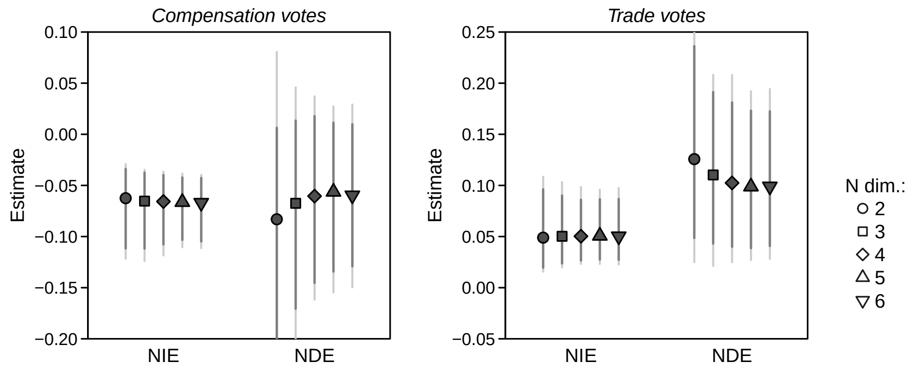

Figure G.3
NIE(1) and NDE(1) estimates for different numbers of eigenvectors

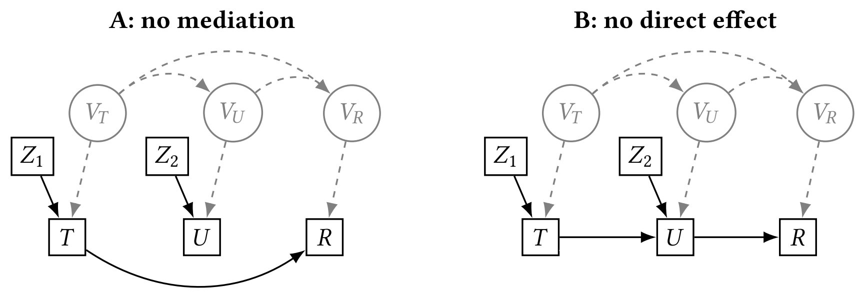

Figure G.4
DAG of simulated data for mediation analysis.

Note: These two figures show the data generating process underlying the "placebo" Monte Carlo simulations. Shown is a mediation setup with unobserved correlated confounders. Conditioning on observed confounders X is omitted for clarity.

we use for the placebo analyses. Note that conditioning on covariates is not shown for reasons of clarity. The DAGs are based on the data generating process of our mediation model, with substantial endogenity induced by the correlation between the unobservables V_U, V_T, and V_R. Graph A shows a placebo setting where no mediation takes place (i.e., the NIE equals 0), while graph B shows a setting without any direct effect (i.e, NDE equals 0). We are interested whether the mediation model, which relies on two instruments to disentangle the confounding shown in the DAGs, is able to recover the NULL effects for NIE and NDE. We conduct Monte Carlo studies for both scenarios. We generate 1,000 replicate data sets with a sample size of 4500 (close to the sample size used in our main text) and estimate our instrumental variable mediation model as well as a naive OLS mediation model.

Table G.2 shows the results. Its first column shows the true value of the direct and indirect effects (which are zero in the respective placebo scenarios). The second two columns show the instrumental variable mediation estimates followed by the naive OLS estimates. Panel (A), simulated from DAG A, starts with an analysis where there is no indirect effect of the treatment on the outcome. We find that, on average, the model recovers the absence of an indirect effect very well. Similarly, the OLS model (using the product of coefficients approach) also recovers the absence of an indirect effect reasonably well. However, while the instrumental variable mediation model also recovers the direct effect very well, the OLS model yield a direct effect estimate that is almost 1.5 times larger than the true value.

Table G.2
Placebo Monte Carlo study for NDE and NIE recovery

		Semipar 2-IV		Naive OLS	
	DGP	MC est	RMSE	MC est	RMSE
A: no mediation effect T→U→R					
Direct effect (NDE)	1	1.001	0.066	1.459	0.461
Indirect effect (NIE)	0	−0.000	0.021	0.046	0.046
B: no direct effect T→R					
Direct effect (NDE)	0	−0.060	0.425	0.405	0.406
Indirect effect (NIE)	0.6	0.531	0.179	1.204	0.610

Note: Based on 1,000 Monte Carlo simulations. Data generating process $R = \theta_1 T + \theta_2 U + 0.5X_1 + 0.25X_2 + V_R$, $U = \alpha_m Z_2 + \theta_3 T + 0.5X_1 + 0.25X_2 + V_U$, $T = 1(\alpha_d Z_1 + 0.5X_1 + 0.25X_2 + V_T > 0)$. Covariates $X \sim N(0,1)$. Instruments are generated as $Z_1 = 1(0.5X_1 + 0.25X_2 + P > 0)$ and $Z_2 = 0.5X_1 + 0.25X_2 + Q$ with errors $P, Q \sim N(0,1)$ and independent of V_T, V_U, V_R. Unobservables V_T, V_U, V_R are correlated, which induces endogeneity. $V_T, V_U, V_R \sim N(0, \Omega)$ with Ω a 3×3 matrix with variances equal to 1 and covariances equal to 0.5. Changing values for θ_1 and θ_2 and θ_3 allows the creation of DGPs with zero NDEs and NIEs. The 'naive OLS' model uses the product of coefficients approach to estimate the indirect effect.

Panel (B), simulated from DAG B, studies the setting where there is no direct effect (i.e., the treatment effect of T on U is fully mediated by M). We again, find that the instrumental variable mediation model recovers the absence of a direct effect fairly well (the Monte Carlo estimate is nonzero but rather small, especially compared to the OLS estimate). Similarly, the instrumental variable mediation model recovers the indirect effect quite well, while the OLS model estimates are twice as large as the true value.

H. 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. It is thus germane to ask if our results hold when partialling out the influence of public unions in a given district.

Table H.1
Accounting for public unions. IV estimates of local union membership effects adjusting for the number of public unions members

	Compensation			Trade		
	(1)	(2)	(3)	(4)	(5)	(6)
Union membership	0.204	0.195	0.232	−0.370	−0.380	−0.412
	(0.055)	(0.072)	(0.066)	(0.084)	(0.100)	(0.097)
Invalid IV robust p^a	0.002	0.020	0.008	0.000	0.001	0.000
Weak IV robust p^b	0.000	0.000	0.000	0.000	0.000	0.000
State & time FE	✓	✓	✓	✓	✓	✓
District characteristics						
Slavery & immigration history	✓	✓	✓	✓	✓	✓
Demographics & ideology		✓	✓		✓	✓
Technological change			✓			✓
N	5458	5454	5380	4243	4243	4183

Note: 2SLS estimates of effect of logged union membership on roll call votes with cluster-robust standard errors in parentheses. Union density instrumented by share of mining employment in the 1950s (see text for detailed discussion). The smallest robust first stage F statistic for compensation votes is 36.6, for trade votes it is 21.8. Analyses include the number of members in public unions based on our classification of likely union filings. To reduce skewness, the variables is cube-root transformed. For details on other district-level controls see Table II.

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

b Test p-value of union coefficient allowing for weak instruments.

Again, the administrative forms used 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. Based on the data from Becher, Stegmueller, and Kaeppner (2018), 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 include the number of employees in public union members in a district. While clearly being an approximation, our public union measure captures 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 and

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

In Table H.1 we report IV estimates of the effect of union membership on roll call votes (cf. Table II in the main text) after adjusting for public union membership.[14] 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 largely indicate a significant relationship between union strength and roll call votes on free trade and economic compensation.

I. Full table of IV estimates

In this section, we present a full table of estimates of our instrumental variable models (using the specification with all covariates included). Table I.1 shows estimates for the key treatment variables (trade exposure and union membership), as well as coefficients for all control variables. The bottom half of the table shows p-values for significance tests of the treatment variables while allowing for arbitrary weak instruments (Moreira 2009; Andrews, Stock, and Sun 2019) and for locally invalid instruments, i.e., a local-to-zero violation of the exclusion restriction (Wang et al. 2018).

[14]We include public unions membership 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 show that our main results change very little—without making further statements about public union estimates.

Table I.1
Full table of IV estimates

	Union membership	Trade roll calls	Compensation roll calls
Trade exposure	−0.363 (0.035)		
Union membership		−0.463 (0.124)	0.246 (0.074)
District characteristics			
Hist. share black	−0.519 (0.282)	−0.520 (0.262)	0.425 (0.133)
Hist. share foreign	−2.175 (0.375)	−1.699 (0.387)	1.350 (0.232)
Share foreign born	2.016 (0.415)	0.742 (0.316)	0.519 (0.256)
Share black	1.539 (0.425)	0.119 (0.288)	0.173 (0.151)
Share college degr.	−0.120 (0.511)	0.540 (0.414)	−0.032 (0.199)
Share empl. manuf.	5.383 (0.907)	−0.706 (0.638)	0.054 (0.403)
Share urban	3.084 (0.777)	0.263 (0.740)	0.106 (0.364)
Automation risk	−0.053 (0.031)	0.044 (0.027)	−0.071 (0.011)
IV-violation robust inference			
Weak instrument[a]	0.000	0.000	0.000
Local-to-zero exclusion[b]	0.000	0.003	0.021
N	2554	4183	5380

Note: 2SLS estimates with state-congress fixed effects and cluster-robust standard errors. Chinese imports per worker is instrumented by imports to eight other highly industrialized countries. Historical data on share of blacks and foreign born based on 1910 Census. District's socio-economic composition variables based on 1990 Census. Technological change measured by risk of automation (RSH_d).

a Test of treatment coefficient allowing for arbitrary weak instrument (Moreira 2009).

b Test of treatment coefficient allowing for violation of IV exclusion restriction, H_0: $\beta = 0$ w. $Cov(Z, e) \approx 0$, (Wang et al. 2018). See Appendix E.

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