# The Erosion of Institutional Comparative Advantage[*]

*International Organization* — conditionally accepted

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**Abstract**

We develop a framework of institutional comparative advantage and its erosion, linking changes in the global institutional environment to changes in trade patterns and political cleavages. For much of the 20th century, countries such as the U.S. provided superior property rights institutions, which advantaged firms enmeshed in domestic production networks. With the global convergence toward stronger property rights, this institutional comparative advantage eroded. This shifted trade patterns and created a distinct source of discontent with globalization – rooted in institutional change abroad and concentrated in institution-dependent production. Several pieces of evidence paint a broad picture consistent with our framework. Developing measures of the institution-dependence of production, we show that U.S. imports of institution-dependent goods increased in response to institutional change abroad, and that trade competition is more pronounced, 2016 vote shares for Donald J. Trump are higher, and attitudes toward trade are more negative in institution-dependent local economies. We report similar patterns for trade flows and support for protectionist parties in Western Europe. Highlighting the role of domestic production networks, we establish a link from global institutional dynamics to domestic politics and describe a novel source of globalization backlash. Our framework also creates a conceptual and empirical bridge across the literatures on institutions, economic development, and political cleavages over trade.

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Over the past decades, institutional reforms across countries have strengthened the rights of firms in their interactions with each other and with governments. The result has been a pattern of institutional convergence. On standard measures, such as the Varieties of Democracy property rights index (Coppedge et al. 2016), the median country now reaches unprecedented levels of property rights. At the same time, differences across countries have dropped to the lowest levels since 1945, with a dense clustering of countries near the top of the distribution.

For individual countries, stronger property rights have many benefits. By facilitating exchange and contracting between firms, property rights are central to economic development and often part of reforms toward democratization (North and Thomas 1973; Acemoglu, Johnson and Robinson 2001; Frye 2004). We argue that the global convergence toward stronger property rights has another consequence in the context of global markets: it reshapes trade patterns and therefore has domestic redistributive consequences in countries that in the past provided a superior institutional environment, fomenting opposition to globalization there. This opposition is concentrated among constituents that in the past benefited from their country's institutional comparative advantage, and are now hurt from its erosion. The diffusion of the very institutions that facilitate economic exchange and open markets – and in many ways underpin the liberal international order – contributes to contemporary opposition to globalization in established democracies.

Our argument proceeds in several parts. Building on an interpretation of institutions as a source of comparative advantage (Hall and Soskice 2001; Nunn 2007; Levchenko 2007), we first highlight the role of institutional differences across countries in shaping trade patterns. We start from the premise that firms are embedded in domestic production networks, which differ in the nature of the relationships among customers and suppliers. Some inputs can be sourced from thick markets. Others are specific to a customer and supplier, which heightens the risks and costs of establishing such relationships in the first place. Inputs, in turn, may rely on relationship-specific inputs as well. Stronger property rights institutions – by facilitating exchange, resolving disputes, and enforcing contracts – make it easier to develop and sustain such layered production networks. They confer on firms a comparative

advantage in producing *institution-dependent* goods, for which input sourcing requires a network of relationship-specific ties, and for which alternative firm-specific protections remain insufficient.

This institutional comparative advantage is contingent on differences in institutions across countries. It eroded with the global convergence in property rights. With fewer institutional differences, institutional comparative advantage weakens. This places new import pressures on firms and voters in countries with previously superior institutions – not because of technological change or the removal of trade barriers, but because institutional change abroad has undermined their comparative advantage in institution-dependent production.

Embedding a Ricardian model of comparative advantage into a model of economic geography, we delineate the political consequences of these shifts. We treat institutional change abroad as a common pressure across the U.S. and focus on differences in the exposure of local economies (see, e.g., Rickard 2020; Scheve and Serlin 2025; Han and Milner 2026). We describe several mechanisms through which pressures on institution-dependent industries differ from generic import pressures and become politically particularly salient: they affect those previously sheltered by institutional differences; they have a broader impact on the local community because they travel along closely knit networks; they apply to industries that, by being tied into customer-supplier relationships, cannot easily adjust production processes in response to trade pressures; and they create indirect pressures that are likely to remain uncompensated. As a consequence, opposition to globalization becomes concentrated in local economies that in the past were more reliant on institution-dependent production.

We present several pieces of evidence that together paint a picture broadly consistent with our framework. We focus on the U.S., which for a long time provided a superior environment for contracting among domestic firms. Developing measures of the institution-dependence of production across industries and local economies, we first demonstrate that the composition of U.S. trade has shifted over time, with an increase in imports concentrated in institution-dependent products from countries that experienced institutional strengthening. As a consequence, institution-dependent local economies in the U.S. are now exposed to more extensive import pressures.

We then show that past institution-dependence of local economies predicts support for protectionism. As well-established empirical context, we turn to votes for Donald J. Trump in the 2016 U.S. Presidential election as indicator of support for a protectionist candidate. We document that vote shares for Donald J. Trump were higher in counties that had more institution-dependent economies. We find similar patterns in 2016 primary vote shares, differentiating this effect from anti-incumbent voting, and for individual-level attitudes toward trade in a nationally representative survey.

These results remain suggestive: we cannot separate past institution-dependence from long-run economic structures. At the same time, we focus on an attribute that maps directly onto our framework and captures a specific dimension of economic structures. The patterns we report are also not explained by the China shock, offshoring, or automation, and unobserved confounders would have to be implausibly strong to invalidate our results. We further present results that draw an explicit link from institutional change abroad to political outcomes; we offer evidence that pressures on institution-dependent local economies have stronger effects than generic trade pressures; we show using original data that the indirect effects throughout production networks are less likely to be compensated by trade adjustment assistance; and we discuss parallels to familiar shift-share designs in a causal framework.

We establish the broader applicability of our framework with evidence from Western Europe. There, trade patterns followed a similar pattern as they did in the U.S., and individuals in occupations associated with institution-dependent industries support more protectionist parties. Our results uncover the remarkable imprint of global institutional change in domestic politics.

The argument that global markets affect domestic politics is, of course, not new. Gourevitch (1986) and Rogowski (1987) articulated it prominently. The recent literature on the globalization backlash describes related patterns (Jensen, Quinn and Weymouth 2017; Owen and Walter 2017; Colantone and Stanig 2018*b*; Broz, Frieden and Weymouth 2020; Milner 2021). Existing frameworks explain comparative advantage with differences in factor endowments, productivity, or occupational structure, and attribute shifts in trade to shifts in technology or trade policy. We identify a fundamentally different perspective on the origins and the nature of political cleavages over trade, rooted in institutional

differences across countries. We also depart from a literature that has coalesced around two dominant themes: individual firms, isolated from each other, and global production networks, connected across borders (Kim and Osgood 2019). In our framework, connections among firms in the domestic economy are a key feature. This focus on domestic production networks allows us to bridge work on institutions, economic development, and comparative advantage to political cleavages over trade. Comparative advantage is not immutable. It responds to institutional, and ultimately political, choices not only in a country but also among its trading partners.

Our paper makes several additional contributions. First, we highlight a tension between the simultaneous move toward democracy, which is typically bundled with stronger property right institutions (Olson 1993), and economic globalization. The global diffusion of democracy is often understood as a driver of open markets (Milner 1999; Mansfield, Milner and Rosendorff 2000). Yet, as countries catch up and converge toward stronger institutions, they erode the comparative advantage of countries with already strong institutions, generating constituencies hostile to globalization there. Economic globalization and democracy on a near global scale might be surprisingly difficult to reconcile.[1]

Second, we contribute to a growing literature on the political implications of economic ties among firms (Mizruchi 1992; Johns and Wellhausen 2016; Cory, Lerner and Osgood 2021; Cruz and Graham 2022; Betz and Hummel 2025). Similar to Kim and Pelc (2026), who describe the political dynamics when industries embedded in local economic clusters come under pressure, we connect a main theme from this literature to political and economic geography. Links among firms also take a prominent place in the Varieties of Capitalism literature (Hall and Soskice 2001). In liberal market economies, dissolving ties among firms should be easier than in coordinated market economies. Our work offers opportunities for understanding differences in economic resilience – at a moment of major upheavals from geo-political risk, a resurgence of policy uncertainty, and challenges to established institutions.

[1]We thus echo the conclusion of Verdier (1998), but arrive at it by viewing institutions as commitment devices rather than aggregation mechanisms. Verdier observes that similar political institutions lead to the political empowerment of similar groups, which will support free trade in some countries and oppose it in others. We instead consider how institutional convergence reshapes more immediately who supports and opposes free trade.

# 1 Institutions, Production Networks, and Trade

Our framework places front and center the context in which economic activity takes place: firms draw on inputs from many other firms throughout *domestic production networks*; and these interactions are underpinned by *institutions* that sustain relationships among firms. Incorporating these elements, we first outline an account of institutional comparative advantage, building on work by Hall and Soskice (2001), Nunn (2007), and Levchenko (2007). We then advance three key claims. First, property rights institutions differed in the past, but over time converged across countries. Second, as long as institutional differences were pronounced, these differences sustained an institutional comparative advantage; as institutional differences became more muted, the comparative advantage of countries with already strong institutions eroded, resulting in shifting trade patterns. Third, this shift has uneven geographic consequences within countries, such that individuals in institution-dependent local economies have become opposed to international trade.

**Institutions and Production Networks.** The production of many goods relies on domestic production networks, where firms source inputs directly from suppliers and indirectly from suppliers of suppliers. These customer-supplier relationships have different qualities. Some inputs are relatively easy to source and can be purchased, largely interchangeably, from other suppliers. For example, many raw materials can be purchased on thick markets with alternative suppliers, and suppliers can relatively easily find alternative customers: the input has ample use outside this specific relationship. Other inputs are specific to the production process and tailor-made, such as some electrical parts in the production of cars, and the involved firms are dependent on this specific exchange.

These latter types of linkages expose both sides to risks. For the supplier, a part made to the specification of a customer is difficult to sell to a different firm or to re-purpose it for other customers. For the customer, if the part is of inadequate quality or not delivered on time, the production process stalls. If problems occur later on, it can also be difficult to hold the supplier liable. Such relationships therefore require investments upfront in establishing a business relationship, including reaching agreement

on the specification of the required parts and on compensation if specifications are not met. Firms may seek to guard against these risks by writing contracts accordingly, specifying details of the terms of delivery and payment, and penalties if contractual obligations are not met. For example, in its 2018 SEC filings, Ford Motor Company highlighted its liabilities arising from safety recalls and warranty actions, and noted that some of these costs can be recovered from suppliers; but that such recovered costs can only be accounted for "when specifics of the recovery have been agreed with our supplier and the collection of the recovery is virtually certain."[2]

It is difficult to specify contracts in sufficient detail to address all contingencies. This leaves contracts by necessity incomplete. Even where firms can specify and agree on contract terms, disagreements over their interpretation have to be resolved and enforced. Whether contracts between firms can be enforced effectively, efficiently, and impartially depends on domestic institutions: because contracts allocate property rights, the ability to write adjudicable and enforceable contracts ultimately depends on property rights institutions (North and Thomas 1973).

Stronger institutions facilitate all kinds of economic activity (Acemoglu, Johnson and Robinson 2001). But they are especially important to industries that require relationship-specific investments along the production network. Institutions facilitate the establishment of such relationships, and thus the production of these goods. Where institutions are lacking, by contrast, it is difficult to establish such relationships in the first place. Production processes therefore differ in what we call *institution-dependence*, which combines two features: the extent to which firms in an industry source inputs that are relationship-specific, building on Nunn (2007); and the depth of an industry's production network upstream, which reflects whether inputs of inputs (and inputs of inputs of inputs, etc.) are also relationship-specific. Our notion of institution-dependence captures the reliance on relationship-specific investments along an industry's production network, analogous to measures of the carbon content of production, which incorporate not just the carbon content of a single production stage, but

[2]Ford Motor Company, 2018 Quarter 1 SEC Filing Form 10-Q, available at `https://www.sec.gov/Archives/edgar/data/37996/000003799618000036/f0331201810-q.htm`. Last accessed April 25, 2025.

the carbon content along the upstream supply chain (Shapiro 2021). We provide more details on how we measure institution-dependence in the empirical section. Conceptually, institution-dependence applies at the level of the purchasing industry: it aggregates the relationship-specificity of its upstream production network onto an industry.

Property rights institutions, with broad and even coverage across firms, are crucial for such production processes. Firm-specific alternatives are poor substitutes when firms need to rely on an entire production network upstream. For example, a firm may seek out political connections to improve its standing vis-a-vis business partners. But this creates an asymmetry. Unconnected business partners now may hesitate entering into a business relationship (Bhandari 2022), because politically connected firms cannot commit to not exploit their connections. Indeed, when firms form new political connections, suppliers are affected adversely (Hsu and Faccio 2025). These effects compound throughout production networks.

Similarly, while firms and their business partners may feel confident in their interaction with each other – for example because of board interlocks or a dense customer-supplier relationship – they have less influence over the relationships of suppliers with their business partners. Yet, interruptions to customer-supplier relationships have enormous potential to hold up production processes further downstream. Carvalho et al. (2021) document the downstream consequences of a regionally concentrated earthquake in Japan throughout the country's production network.[3]

Our focus on production networks also suggests limits to alternative institutional sources of property rights that remain firm-specific. Notably, bilateral investment treaties grant firms far-reaching protections. While these treaties only extend protections to a limited set of foreign firms, domestic firms can share into these through financial ties with foreign firms and through round-tripping (Betz and Pond 2019). While we focus on domestic institutions as the source of this global improvement in property rights, we do not preclude other sources of property rights from strengthening. Ultimately,

[3]Vertical integration allows firms to internalize immediate customer-supplier relationships, but this does not easily extend further up in the production network. It remains more efficient to source products from specialized firms, rather than incorporating the entire production network into one single entity (Antràs 2015).

however, these alternatives remain firm-specific, reflecting an 'atomization' of firm privileges (Thrall 2025). They protect a firm against predation from the government and its business partners, but they do little to sustain relationships among the business partners of a firm's business partners – and especially so within the domestic economy. While a firm may find avenues to overcome concerns relative to its immediate business partners, this becomes more challenging for indirect business relationships throughout the production network. Sustaining business relationships in these networks requires institutions that apply across layers of domestic firms. This layered need for protections is what sets institution-dependent production apart from other types of supply-chain relationships, which may remain more shallow and less reliant on institutions.

**Comparative Advantage.** Because institutions underpin economic exchange, differences in institutions shape comparative advantage. Where institutions facilitate economic exchange, the production of institution-dependent goods is more efficient, providing firms in these countries with a relative advantage in producing such goods – and a relative disadvantage in the production of other goods. Just like technology or factor endowments can create a comparative advantage in the production of some goods over others, property rights institutions facilitate the production and export of some goods over others (Nunn 2007; Levchenko 2007; Costinot 2009). This interpretation follows Ricardian comparative advantage. Stronger institutions raise productivity across the board, but unevenly. The industries that benefit most are those that rely on relationship-specific inputs throughout their production network. The extent of this comparative advantage depends on differences in institutions: with larger differences, institutional comparative advantage is more pronounced.

Figure 1 presents evidence of such a pattern, based on a cross-section of countries for a single year, 1998, similar to the cross-sectional evidence in Nunn (2007). Each panel corresponds to one measure of institutions: property rights from the Varieties of Democracy project (Coppedge et al. 2016); the share of contract-intensive money in total money supply (following Clague et al. 1999); and polity scores (Marshall, Gurr and Jaggers 2017). The vertical axis measures the average institution-

dependence of a country's trade with the U.S. The figure depicts the positive association between institutional quality and trade patterns: Countries with stronger institutions export products that are, on average, more institution-dependent.

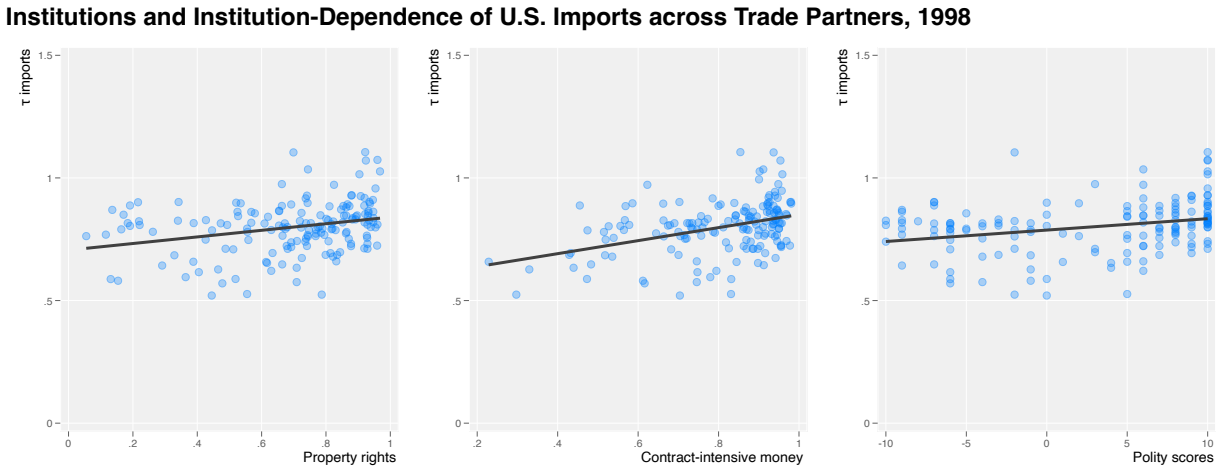



**Figure 1** Quality of institutions (horizontal axis) and average institution-dependence of bilateral U.S. imports (vertical axis) across countries, 1998. Scatter plot and linear fit. Left panel: VDEM property rights. Middle panel: Contract-intensive money. Right panel: Polity scores.

# 2 Global Institutional Convergence, Trade Patterns, and Politics

This institutional comparative advantage is contingent on enduring differences in institutions across countries. In the following, we detail three core claims. First, property rights institutions have converged across countries. Second, this institutional convergence has eroded the comparative advantage of countries with already strong institutions, resulting in shifting trade patterns. Third, this shift has uneven geographic consequences within countries, such that individuals in institution-dependent local economies have become more opposed to international trade.

## Global Institutional Convergence

Our first claim is based on a simple descriptive observation: recent decades saw a steady strengthening of property rights. This strengthening reflects, in part, a growing recognition among economists, policy-makers, and international organizations that institutions are key to economic performance and development (North 1989). Investments in property rights are frequently also part of broader institutional reforms toward democratization (Olson 1993). Competition for capital together with ratings and benchmarking exercises have further incentivized institutional convergence (Kelley and Simmons 2019). For example, the World Bank annually assesses a country's institutions with the Country Policy and Institutional Assessments, providing for straightforward comparisons across countries, exerting upward pressures on institutional quality from domestic audiences and international markets.[4]

These trends are reflected in empirical measures of the quality of institutions. For example, the World Bank notes, in discussing its Doing Business Indicators, how "The global diffusion of legal and institutional reforms—such as specialized commercial courts, electronic case management, and alternative dispute resolution—has strengthened contract enforcement and property rights worldwide".[5] These institutional reforms underpin economic exchange among firms throughout entire production

[4] `https://ida.worldbank.org/financing/resource-management/ida-resource-allocation-index`. Accessed April 29, 2025.

[5] `https://subnational.doingbusiness.org/en/data/exploretopics/enforcing-contracts/good-practices`. Last accessed April 29, 2025.

networks. They are broader than firm-specific protections derived from, for example, political connections and cross-border financial links.

**Evidence of institutional convergence.** Figure 2 documents the extent of these changes over time, using the property rights index from the Varieties of Democracy Project (Coppedge et al. 2016). The index is based on expert surveys and therefore on perceptions of institutional quality, rather one being a de facto measure of one specific institution. The figure displays a series of histograms for years between 1945 and 2015, in increments of five years. The figure shows how over time, the distribution across countries became increasingly tightly packed near the top. By 2015, property rights scores at the 75th and the 95th percentile have become nearly indistinguishable – the difference shrunk to .06, less than the difference between the United States and Canada, and within the average margin of error of the property rights coding. In Appendix SI.1, we provide systematic assessments of this convergence based on quantile regressions. Even small perturbations in a country's property rights score now result in sharp swings in a country's rank in the distribution.

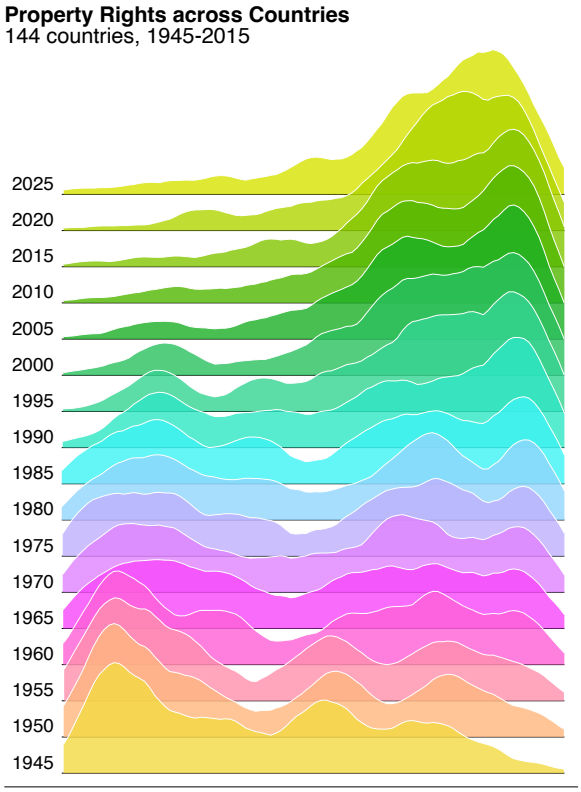



**Figure 2** Distribution of property rights index, from Varieties of Democracy Project (Coppedge et al. 2016), across 144 countries from 1945 to 2015 in five-year increments. Over time, the distribution became increasingly heavily concentrated near the top.

## Global Institutional Change and Trade Patterns

Our second claim is that this institutional convergence reshapes global trade. As countries become more similar, differences in institutions become a weaker explanation of comparative advantage – we observe an erosion of comparative advantage as a result of institutional convergence. With institutions strengthening on a global scale, the quality of U.S. institutions declines *relative* to other countries, undermining the competitiveness of U.S. firms in institution-dependent industries and increasing import pressures on those firms. The increasing convergence in institutions is key to this pattern. If institutions had been similar all along, institution-dependent industries in the U.S. would not have benefited from a superior institutional environment in the past. That institutions differed in the past allowed for institutional comparative advantage; that institutions are similar now leads to the erosion of this institutional comparative advantage.

We note a key difference to models of input offshoring and the expansion of global value chains (Osgood 2018). Stronger institutions facilitate input sourcing from abroad (Li and Resnick 2003; Pinto and Weymouth 2016), because they alleviate risks in cross-border transactions (Greif 1993). In our framework, stronger institutions abroad facilitate the production of institution-dependent products themselves, and institutional *differences* matter because they shape the relative cost of producing different goods. Input offshoring in the context of global value chains and the erosion of institutional comparative advantage therefore imply different empirical patterns. The former suggests increasing imports of inputs (including, but not exclusively, inputs to institution-dependent goods). The latter suggests increasing imports of institution-dependent goods that create indirect pressure across the network of input suppliers. These indirect pressures are not necessarily reflected in trade flows. From the perspective of a supplier, input offshoring implies losing business because a customer outsources production; the erosion of institutional comparative advantage implies losing business because an institution-dependent customer comes under import pressure.

To illustrate, consider automobile manufacturing. One of the most institution-dependent industries, it relies on a complex upstream production network. With institutional change abroad, produc-

tion networks around automobile manufacturing become more efficient in other countries, increasing pressures on automobile manufacturers in the U.S. As demand for automobiles manufactured in the U.S. declines, so does demand for many of the component parts that are incorporated into the final product, thus creating spillover pressures for firms in industries that are adjacent in the production network. While we expect to observe an increase in imports of automobiles from abroad, we would not expect a concomitant increase in the inputs to automobile manufacturing – demand for these parts by U.S. manufacturers decreased, and foreign automobile imports already incorporate these components, obviating the need to import them separately.

Institution-dependent industries might spread supply chains across borders, just as other industries do. However, the benefits from doing so are more limited for institution-dependent industries. Inputs in institution-dependent industries rely on the entire production network and are specific to the customer and supplier, and thus are more difficult to separate geographically. Huang et al. (2023) and Juhász et al. (2026) document the challenges of finding alternative suppliers of inputs relying on firm-to-firm connections, even for multinationals with established global production networks. The benefits from input sourcing should not accrue disproportionately to institution-dependent industries.

**Evidence of institutional change and trade patterns.** To pin changes in trade to changes in institutions, Appendix SI.2 provides evidence from U.S. bilateral trade flows and institutional change abroad over three decades. We demonstrate that institutional strengthening abroad increases imports in industries that score high on institution-dependence, and decreases it for others, resulting in a shift in trade patterns. This pattern in granular trade data holds in specifications with country and year fixed effects, ruling out most plausible explanations of bilateral trade. The pattern does not extend to inputs to institution-dependent products, which differentiates our results from input offshoring and the expansion of global value chains (Appendix SI.4.10). Finally, we show that institutions become worse predictors of institution-dependent trade over time, consistent with our argument emphasizing the erosion of institutional comparative advantage.

We also address two potential concerns. First, institutional change abroad may be endogenous to U.S. interests that also shape trade patterns. In countries where U.S. firms have substantial commercial interests, we might simultaneously observe larger trade flows and U.S. support for institutional improvements. This would suggest a correlation between trade flows and institutional change, but not necessarily a shift of trade patterns toward institution-dependent products.[6] We also provide several additional results. We capture pre-existing, pre-period U.S. interests with country fixed effects, and we show that the results are not explained by U.S. related-party trade.

Second, we present evidence suggesting that these patterns are indeed driven by domestic institutions, not by firm-specific protections. We find no similar effects for bilateral investment treaties, which provide firm-specific protections but cannot sustain ties among domestic suppliers throughout production networks. This further supports our interpretation around the crucial role of domestic institutions, by providing broad-based rather than firm-specific protections.

## Global Institutional Convergence and Political Consequences

Our third claim is that the consequences of the erosion of institutional comparative advantage are geographically clustered within countries. We interpret the global strengthening of property rights as a common national-level pressure with heterogeneous consequences across the U.S., driven by the pre-existing institution-dependence of local economies. The heterogeneous geographic consequences of trade are well-established. Because local economies vary in their industry composition, they are differentially exposed to global market pressures (Autor, Dorn and Hanson 2016; Rickard 2020).

Previous work suggests several pathways through which import pressures affect individuals in local economies. As industries face increased competition, employees in these industries experience downward wage pressure, layoffs, and reduced job security (Acemoglu et al. 2016), leading them to question the benefits of trade liberalization (Colantone and Stanig 2018*b*). Additionally, voters not

[6]To the extent that U.S. support for institutional change abroad comes in recognition of the effects on institution-dependent production and the resulting increase in institution-dependent trade flows, the pattern remains consistent with our framework.

only experience the negative effects of trade directly, but also indirectly through local market conditions (Rickard 2020) – for example, through downward pressure on home prices (Scheve and Slaughter 2001) and spillovers to other industries (Flaherty 2023).

Such an economic geography model, which presumes that production is specific to a location, is pertinent for institution-dependent production. Customers and suppliers tend to co-locate (Ellison, Glaeser and Kerr 2010). At its core, institution-dependent production relies on relationship-specific ties between customers and suppliers. These relationship-specific ties make the co-location forces plausibly even more pronounced, such that the production of these goods tends to become geographically clustered and limits opportunities for adjustment (Porter 1998; Kim and Pelc 2026; Juhász et al. 2026). Thus, location choices become sticky and production geographically unevenly distributed. Production in any single industry cannot easily be moved, because it is enmeshed in the local economic context. The consequence is that import pressures become specific to local economies and at the same time, within local economies, broader by traveling along production networks.

In short, then, we expect that trade pressures rooted in global institutional change are concentrated on institution-dependent local economies. Four inter-related attributes suggest import pressures on institution-dependent local economies might elicit a particularly strong response, even compared to generic trade pressures. First, institution-dependent industries in the U.S. used to be protected by a superior institutional environment and by the high entry barriers created by the need to establish supplier networks. In the past, these industries remained sheltered from global competition, even in the face of trade liberalization. They therefore have limited experience with available market strategies to fend off such pressures, and have not invested in the political connections to obtain relief either. Additionally, while governments may compensate firms for higher input costs or output market competition at the firm-level, it is more difficult to compensate for a change in the relative institutional environment, because it affects the entire domestic production network. The very dependence on institutions, which cover broad parts of the economy, makes it difficult to find viable alternatives in firm-specific policy instruments.

Second, institution-dependent industries face more challenges than other industries when adjusting to trade pressures on other margins: altering production processes, adopting new product lines, or upgrading technology. These adjustments are particularly challenging for firms that rely on dense customer-supplier linkages and thus coordination among firms, because adjustments have to be coordinated across firms. The Varieties of Capitalism literature has long noted related concerns (Hall and Soskice 2001), such as the importance of inter-firm relations in adjusting to market pressures, but focused on cross-country differences in the speed with which firms can make such adjustments. We note complementary differences in adjustment costs across industries. Our argument is not that coordination constraints and investment costs outweigh the benefits from adjustments to production technology or quality upgrading. Rather, coordination constraints and investment costs are more pronounced in institution-dependent industries than in other industries.

The contrast between the broad effects of institutions and firm-specific adjustments again underscores this point. The erosion of institutional comparative advantage places pressures on industries and their production networks. These broader effects are difficult to counter with firm-specific adjustment strategies, that by design remain narrowly confined. For example, quality upgrading for institution-dependent products necessitates a series of bilateral adjustments along the production networks, co-ordinated by a lead firm. In deeper or more dispersed production networks, this becomes more challenging to coordinate for any single firm. In deeper production networks, moreover, a type of weakest-link dynamic unfolds: if any one supplier cannot move along with the demands of a firm seeking to upgrade technology or products, a lead firm needs to find alternative suppliers or refrain from these adjustments. In a parallel to the literature on asset specificity (Alt and Gilligan 1994), their ties to specific suppliers thus make institution-dependent industries particularly vulnerable.

These same attributes also make institution-dependent industries more prone to suffer from a variant of economic sclerosis (Olson 1982). Being relatively shielded from global competition by a superior institutional environment reduced pressures to innovate pre-emptively. At the same time, institution-dependent industries face particularly stiff competition from abroad. While some established U.S. in-

dustries remain stuck in existing technologies and relationships, newly emerging foreign industries – empowered by institutional catch-up – can adopt modern production technologies. This effect is plausibly most pronounced for older firms and industries and those stuck in established customer-supplier relationships, and thus for institution-dependent industries.[7] Competition in institution-dependent industries, driven by institutional change abroad, thus differs from competition in industries with already established foreign firms that enter the U.S. market in response to trade liberalization or a drop in transportation costs.

Upgrading products and production technology also is particularly challenging in response to tightened budgets in the wake of import pressures. Declining revenues are correlated with firms dropping more products from their portfolios and adding fewer new products (Bernard, Redding and Schott 2010). Similarly, firms may be able to rely more on automation to save labor costs in the long term, but this necessitates overcoming the high fixed costs of labor-replacing technologies. Indeed, while trade liberalization can spur innovation and productivity, the underlying mechanisms do not apply to the pressures on institution-dependent industries that are our focus. Innovation tends to happen in response to increased market size and thus export opportunities (Bustos 2011; Coelli, Moxnes and Ulltveit-Moe 2022), not because firms face more competition in their domestic market. Where firms do innovate more, it is in part reflecting a selection effect: innovation happens among surviving firms. Firms operating with outdated technology are more likely to exit the market in response to import pressures (Bloom, Draca and Van Reenen 2016), such that import pressures overall depressed innovation among U.S firms (Autor et al. 2020). In short, the combination of losing an institutional shield, the rigidity of existing relationships in the U.S., and the emergence of new firms abroad at the technological frontier reinforces pressures on institution-dependent U.S. industries.

Third, pressures on institution-dependent industries travel through tightly knit production networks, tying together local communities and creating a shared sense of exposure to these pressures. This channel operates through the economic spillovers described earlier: when institution-dependent

[7] In data from the Census Business Dynamics, firms in more institution-dependent industries are older.

industries experience import pressures and demand decreases, demand for inputs to these industries also decreases. Politically, spillovers can unfold their own political dynamic. Pressures on key industries in the local economy have pronounced political consequences, because the effects travel through social networks and elicit a shared sense of loss (Kim and Pelc 2026). Similarly, pressures on industries with stronger supplier linkages throughout the local economy create a stronger sense of place among voters (Betz, Binder and Geus 2026). Import pressures on institution-dependent industries thus have not just broader effects through spillovers. They are more likely to result in a shared understanding that globalization threatens the local community.

Finally, trade pressures on institution-dependent labor markets are more likely to remain uncompensated by trade adjustment assistance (TAA). Compensation through TAA was able to offset some of the backlash to the negative labor market effects of trade. This includes reduced support for Donald J. Trump in the 2016 election (Kim and Pelc 2021*b*; Ritchie and You 2021; Lee and Balcazar 2024). The flipside is that *uncompensated* trade pressures create an environment conducive to a protectionist turn. Such uncompensated trade pressures are more likely to emerge in institution-dependent local economies, where indirect trade pressures from spillovers to suppliers accumulate. For indirect trade pressures, TAA is available in principle: TAA rules – at various times – explicitly covered petitions based on indirect trade exposure, where (1) workers in a downstream customer firm had been certified to be eligible for TAA or (2) imports of a product have increased and workers producing an input to that product have lost their jobs.

However, both routes are likely to result in uncompensated trade pressures. The first route requires TAA certification for workers of a downstream customer. Because only a subset of eligible workers based on direct exposure files for TAA (Kim and Pelc 2021*a*), at least some workers that experience indirect exposure by necessity remain uncompensated. The second route leads to uncompensated indirect exposure because of the informational and evidentiary burden. Establishing eligibility requires information on the trade exposure of downstream firms. Moreover, because of multiple revisions to TAA, this route was not available before 2009 at all, and after 2009 only with interruptions. Partially

as a result, workers face an informational barrier: they may not even know that TAA offers eligibility based on indirect exposure, and newly eligible groups of workers tend to be slow to pick up access to TAA (D'Amico et al. 2012). Indeed, the documents petitioners need to complete do not even list indirect exposure, based on this second route, as an example in the instructions. The consequence are uncompensated import pressures on institution-dependent counties, where suppliers agglomerate. Between 2010 (when this route first became available for the full year) and 2015, less than 60 out of over 7,000 petitions were certified via this route – less than .1% of certified petitions.

**Opposition to trade.** We expect that these negative consequences of trade pressures translate into politics. Together with the well-developed literature on the globalization backlash (for a review, see Walter 2021), and standard open economy politics frameworks that deduce attitudes and behavior from material consequences (Lake 2009), several pieces of evidence suggest this to be the case. Local economic conditions shape voter evaluations of trade policy (Guisinger 2017), and being embedded into the local context blurs the line between ego-centric and socio-tropic attitude formation (Fordham and Kleinberg 2012). In response to broader economic decline at the local level, voters might suspect that globalization benefits might never translate into tangible gains for them or their communities. Psychological and economic factors intertwine, fostering opposition to trade in areas experiencing import exposure.

As empirical context, we focus on the 2016 U.S. Presidential election. Explicitly and from the outset of his campaign, Donald J. Trump advocated for anti-globalization policies, such as the imposition of higher tariffs on imports. If protectionist sentiment in the electorate is in part explained by the erosion of institutional comparative advantage, we should observe that the vote share for Donald J. Trump in 2016 was higher in counties historically more dependent on institution-dependent industries.

The candidacy of Donald J. Trump offered a sudden supply of protectionism in politics that responded to previously unmet demand. As Rodrik (2018) observes, opposition to globalization did not have to result in support for right-wing populism and economic nationalism, as was the case in West-

ern Europe and the U.S., but it evidently did. For our empirical context, we therefore take it as a given that this is how opposition to globalization has manifested, and lean on explanations from prior work. For example, Frieden (2020) notes that the sudden shift in the supply of protectionist candidates, policies, and party platforms not just in the U.S., but also in Western Europe, appears to reflect a "failure of compensation" accompanied by a "failure of representation" – an inability and unwillingness of the political system to accommodate simmering, but increasing discontent with globalization. The dislocation from trade also plausibly resulted in heightened economic anxiety, status concerns, and racial resentment, all of which ultimately fed into support for Donald J. Trump (Rodrik 2021; Ballard-Rosa, Jensen and Scheve 2022).

Prior work also suggests some explanations for why import pressures may not elicit an immediate response in politics, and only surface with relatively dramatic shifts as in the 2016 election. One reason is the accumulation of trade pressures over time, which has led to sudden breaks with precedent, as was the case in the Brexit referendum in the United Kingdom (Colantone and Stanig 2018*a*). In a two-party system like the U.S., the supply of protectionist policies by established parties and candidates may have been particularly limited. Politics may also have adjusted on other margins, such as changes in legislative voting behavior (Feigenbaum and Hall 2015) or a movement toward representatives who can more credibly respond to local economic distress (Betz, Binder and Geus 2026), that ultimately proved insufficient to address the resulting grievances.

Another reason is that governments – and established, mainstream parties in particular – were slow to respond to discontent with globalization because they were enmeshed in institutional constraints and norms of neo-liberal policy-making, offering increasing space for protectionist, anti-globalization candidates over time (Goldstein and Mansfield 2026). For example, the 2012 Presidential election in the U.S. arguably happened already in a context characterized by economic displacement from globalization pressures. Yet, the two major-party candidates were largely supportive of global trade: Barack Obama as incumbent had signed several high-profile trade agreements, including the Trans-Pacific Partnership and a (renewed) trade agreement with South Korea; Mitt Romney as Republican party can-

didate was widely perceived as market-friendly and champion of globalization (and portrayed himself as proponent of free trade in his Presidential campaign).

For these reasons, we view the 2016 election as a useful empirical context that allows us to describe suggestive patterns: global institutional change leaves a systematic, observable imprint in domestic politics, concentrated in institution-dependent local economies. At the same time, this empirical context comes with limitations. While Donald J. Trump ran on a platform that clearly opposed globalization, the platform also included a host of other issues that might be correlated with institution-dependent local economies and that we cannot enumerate and confidently rule out. To focus on attitudes toward trade specifically, below we additionally rely on individual-level survey evidence: we expect that individuals in local economies that in the past were more institution-dependent voice more negative attitudes toward trade.

**Proposition 1.** *Support for protectionist candidates and policies increases in the past institution-dependence of local economies.*

# 3 Evidence from U.S. Politics

We begin with describing how we measure institution-dependence. We build on Nunn's work on contract-intensity (Nunn 2007), which captures an industry's reliance on relationship-specific inputs, and combine it with the depth of an industry's production network. Our measure reflects that an industry is more dependent on institutions if the production of its inputs also sources relationship-specific inputs. Developing such production networks across multiple stages is facilitated by institutions that reduce transaction costs, adjudicate disputes impartially, and limit biased government interference. Any individual firm might find alternative ways to address such issues with its immediate suppliers, for example through vertical integration or through firm-specific protections from political connections, international financial ties, or bilateral investment treaties. Firms cannot do the same for the relationships of other firms with their suppliers, which are outside its control but nonetheless impor-

tant for its production process.

We construct our measure at the industry-level. Firm-level data on domestic production networks are not available on a systematic scale, except for a few countries with highly restricted data access.[8] The industry-level represents an adequate level of aggregation. We need to assume that production technologies are similar across firms within industries in terms of the required input mix. For example, we need to assume that different manufacturers of washing machines use a similar proportion of inputs from different industries. This assumption does not rule out that some manufacturers might be more productive or use higher-quality components. It also appears both plausible and not too limiting. The literature on, for example, input-output linkages in global value chains operates with similar assumptions about the proportionality of input sourcing, as does the Bureau of Economic Analysis in compiling some of its statistics of input-output linkages (Caliendo and Parro 2015; Johnson 2018).

We start with products defined by Harmonised System 6-digit codes, and use Broad Economic Categories, BEC Rev. 5, to identify products used as inputs. For each input, we code whether it is specific to the relationship between supplier and customer, based on the distinction between products that are differentiated (and thus specific to firm-to-firm exchange) and generic (with many alternative buyers and sellers), using Rauch (1999) and BEC Rev. 5. We then calculate the average relationship-specificity, $R_j$, for each BEA industry as the proportion of goods and services relying on firm-to-firm exchange produced by industry $j$ and used as inputs in other industries.

We then calculate the extent to which an industry relies on relationship-specific inputs, using the BEA Input-Output Tables from the 1997 benchmark survey. This is the first year based on industry classifications derived from NAICS, facilitating later concordances. Using this information, we calculate for industry $i$ a weighted average of inputs,

$$\xi_i = \sum_j \frac{u_{ij}}{u_i} R_j, \tag{1}$$

[8] Existing data sources for the U.S. are limited to major suppliers of listed companies, and even these only have to disclose a small percentage of their suppliers: those that account for more than 10% of their revenues and whose loss would present a material disruption.

where $u_{ij}$ is the value of input $j$ used by industry $i$ and $u_i$ is the total value of all inputs, including labor, used in industry $i$. This measure captures an industry's inputs, analogous to contract-intensity as defined by Nunn (2007). We construct this measure for all industries covered by BEA and NAICS codes, including agricultural products and services. In the next step, we consider inputs of inputs and so on, throughout the entire production network of an industry. To do so, we re-write the above equation as $\xi = \mathbf{AR}$, where $\mathbf{A}$ is a matrix collecting $u_{ij}/u_i$ and $\mathbf{R}$ is a vector collecting $R_j$. With $\mathbf{I}$ as the identity matrix, we calculate *institution-dependence* as

$$\tau = \mathbf{AR} + \mathbf{A}^2\mathbf{R} + \mathbf{A}^3\mathbf{R} + \ldots = \mathbf{A}(\mathbf{I} - \mathbf{A})^{-1}\mathbf{R}. \tag{2}$$

The top panel of Figure 3 displays the values of institution-dependence across all NAICS two-digit sectors. The bottom panel homes in on manufacturing industries, NAICS 31-33, and plots the distribution across NAICS six-digit industries, grouped by three-digit sub-sectors, showcasing substantial differences even within manufacturing. Among the manufacturing industries scoring particularly high on institution-dependence are *automobile manufacturing*, *electronic computer manufacturing*, and *audio and video equipment manufacturing*, but also industries such as *irradiation apparatus manufacturing* or *leather and hide tanning and finishing*; in contrast, *industrial mold manufacturing*, *pottery, ceramics, and plumbing fixture manufacturing*, and *explosives manufacturing* are at the bottom of the distribution.

## 3.1 2016 U.S. Presidential Election

Our outcome variable is the county-level vote share of Donald J. Trump in the 2016 presidential election, from the MIT Election Data and Science Lab (2018). We calculate institution-dependence at the level of counties as the extent to which the local economy relies on institution-dependent industries.

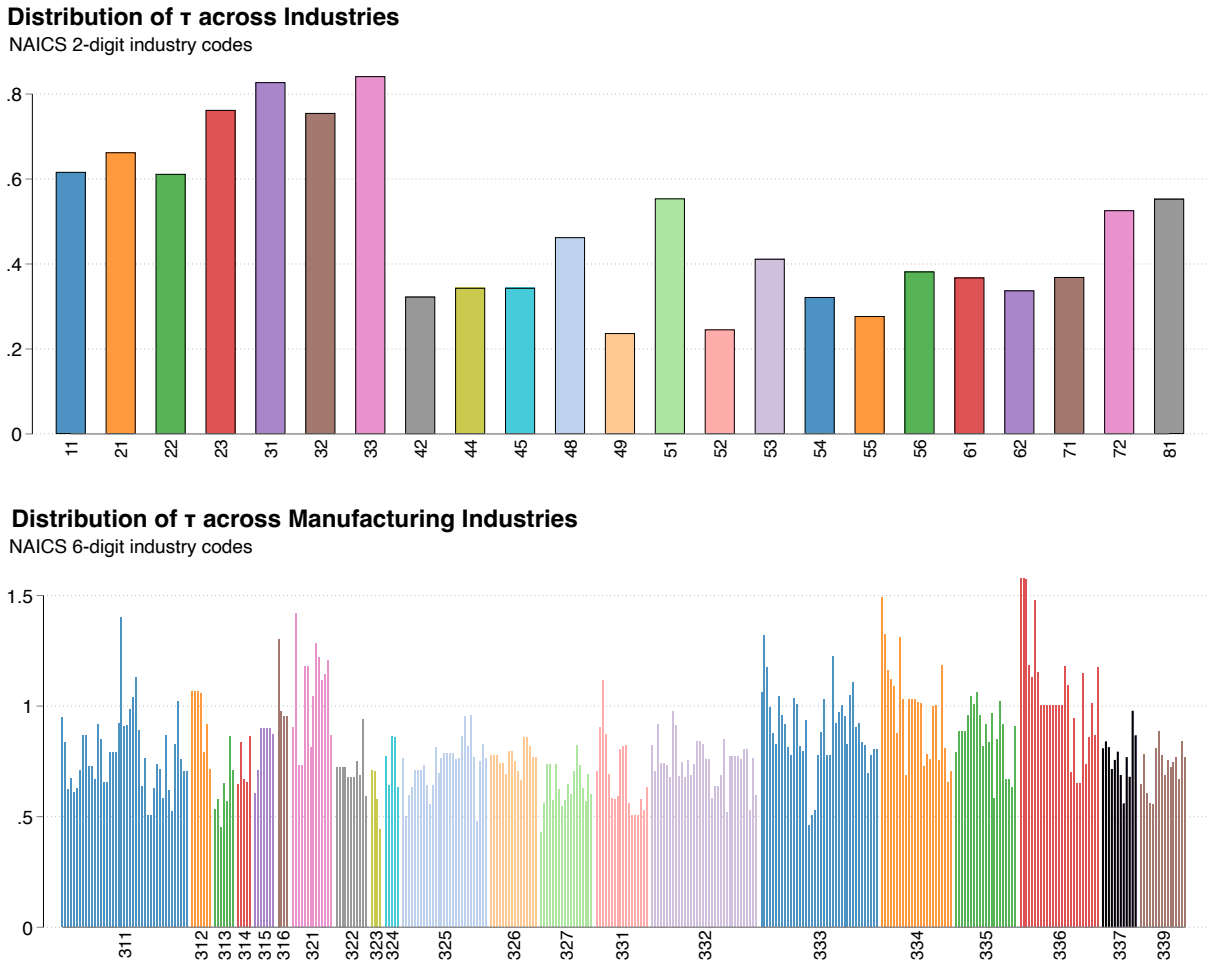



**Figure 3** Top panel: Values of $\tau$ across NAICS two-digit sectors. Bottom panel: Values of $\tau$ across NAICS six-digit manufacturing industries (sectors 31-33), grouped by three-digit subsectors.

### County-level institution-dependence

We create an index combining institution-dependence with the industry composition of counties. Our main results are based on a county's institution-dependence in 1998, which is the earliest year for which production data in the NAICS format were released for each county. The correspondence between NAICS and the previously used SIC codes would introduce additional noise into our measure.[9]

[9]See notes from the Census Bureau, `https://www.census.gov/programs-surveys/economic-census/guidance/historical-data.html`. Accessed February 4, 2026.

We present results from earlier years, based on the institution-dependence of each county as early as 1975, in Appendix SI.4.2.

County-level institution-dependence, $\chi_c$, represents the average institution-dependence of employment in each county. With employment in industry $i$ in county $c$ as $q_{ic}$ and total employment in that county as $q_c$, we calculate

$$\chi_c = \sum_i \frac{q_{ic}}{q_c} \tau_i. \tag{3}$$

County-industry employment data are from the County Business Patterns compiled by Eckert et al. (2020), which we augment with farming employment from the American Community Survey. We track changes in county boundaries over time based on Ferrara and Zhou (2022). Our definition of $\chi_c$ assumes a specific form of homogeneity of industry-level supply chain relationships across regions: industry $i$ uses the same inputs with the same proportions, regardless of where it is located.




**Figure 4** County-level institution-dependence index in 1998, by percentiles, from least institution-dependent (lighter shades) to most institution-dependent (darker shades).

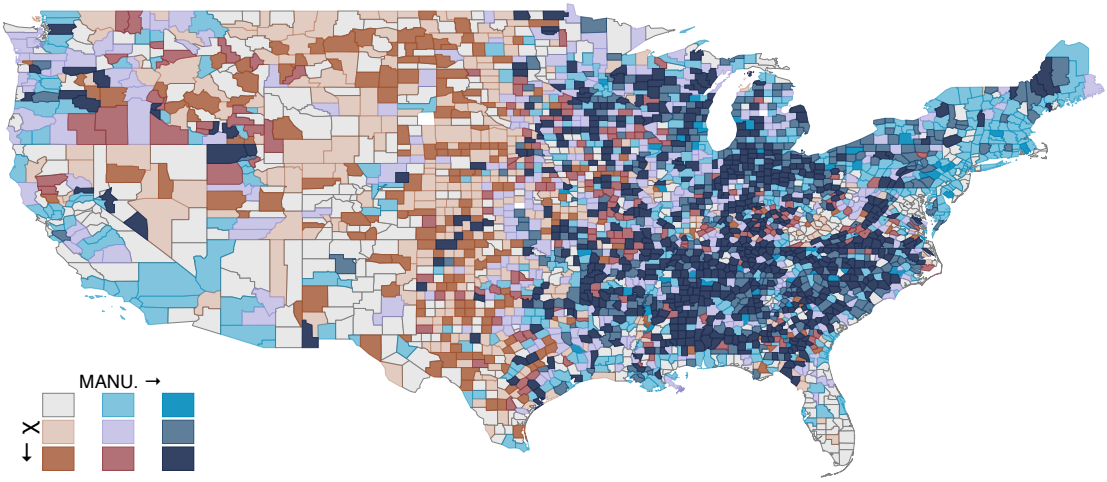



**Figure 5** County-level institution-dependence and manufacturing employment in 1998. Stronger hues of blue indicate higher shares of manufacturing employment at low shares of institution-dependent employment. Stronger hues of red indicate higher shares of institution-dependent employment at low shares of manufacturing employment. Stronger hues of purple indicate higher shares of both institution-dependent and manufacturing employment.

**Comparison to manufacturing.** Figure 4 displays the institution-dependence of each county in 1998, from the least (in lighter shades) to the most institution-dependent (in darker shades). At the county-level, institution-dependence differs from manufacturing, as shown in Figure 5. Counties with high shares of manufacturing but low shares of institution-dependent employment are depicted in stronger hues of blue; high shares of institution-dependent but a low share of manufacturing employment in stronger hues of red; high shares of both institution-dependent and manufacturing employment in purple. Stronger hues of red and blue thus indicate a divergence between institution-dependent and manufacturing employment. We control for manufacturing employment in all of the empirical models below. We note that between 1998 and 2016, four out of five counties saw a decline in institution-dependent employment. For about a quarter of counties, the loss in institution-dependent employment exceeded 10%.

**Research Design**

We estimate linear regression models, predicting the 2016 vote share with a county's past institution-dependence, while controlling for several variables that plausibly correlate both with 2016 Republican vote shares and the composition of the local economy. These include the percentage of manufacturing workers in 1998, the percentage of the population with a high school diploma or above, and the percentage of the population that identifies as white. We obtain these from the BEA Regional Economic Accounts. We also control for whether the majority of the county population lives in urban areas, based on the 2010 Census. Urbanization also is a strong predictor of local access to legal services within states (U.S. Department of Justice 2025).

We include state fixed-effects in all model specifications. The state fixed effects capture main differences in legal institutions within the U.S., which might correlate with institution-dependent production. Adjudicating disputes over customer-supplier contracts ultimately rests with courts that operate state-wide or at the federal level. Companies can appeal decisions from lower-level courts to state courts, narrowing differences across counties within states. Similarly, reports by the U.S. Chamber Institute for Legal Reform, an affiliate of the U.S. Chamber of Commerce, acknowledge differences within states but note that in surveys of general counsels and senior executives, 89% point to the "litigation environment in a state" as important for business decisions (U.S. Chamber Institute for Legal Reform 2019). In transactions that cross state boundaries and in disputes pertaining to federal laws, adjudication can take place in federal courts, again narrowing within-state differences in legal institutions. Empirical work on legal systems and property rights also tends to suggest limited differences within countries, given the established legal system of the U.S.[10] That being said, within-state differences in access to legal services and institutional quality remain (Murphy 2020). In the U.S., some rural counties amount to 'legal deserts' with limited access to attorneys and large distances to the nearest court house (Pruitt et al. 2018). Local courts also vary in their experience and expertise in handling

[10]For example, in the 2022 report on its Economic Freedom Index, the Fraser Institute notes that "these are primarily areas of national policy and would vary little from [...] state to state" (Stansel et al. 2022, p. 3), and accordingly assigns uniform values across U.S. states.

business-to-business disputes (Bardash 2025). We address these differences in Appendix SI.4.9.

Our research design exploits historical levels of a county's institution-dependence to predict the 2016 election outcome, while controlling for (1) manufacturing employment and (2) differences across states. This design offers a parallel to a body of recent work, which conceives of globalization pressures as uniform across the U.S. and examines how county-level differences in access to railroads and ports shape exposure and the political response to globalization (Scheve and Serlin 2025; Han and Milner 2026). Our design can be similarly understood as interpreting global institutional change as uniform pressure across the U.S. that gets absorbed to different degrees by local industries.

Absent a causal interpretation, this design allows us to describe where opposition to globalization should be most pronounced. The advantage of focusing solely on past industry structure is that we do not need to settle on a specific measure of institutions, a starting and end point of institutional change, or the time it takes for production and trade flows to respond. The downside is that we do not incorporate institutional change abroad explicitly – being constant across countries, it effectively drops out. Below, we provide results from a measure that additionally incorporates institutional change abroad as an industry-specific shock (which, we note, might still be endogenous to U.S. politics).[11]

This design makes causal identification difficult. We have no obvious candidates to explain county-level differences in institution-dependence in an instrumental variable model, and the cross-sectional setup is not amenable to a difference-in-differences design. To appreciate the difficulties of establishing a causal interpretation, it helps to consider our research design in the context of shift-share instruments. In the literature on the consequences of trade exposure, these instruments usually rely on the exogeneity of the past composition of local economies as exposure shares (Goldsmith-Pinkham, Sorkin and Swift 2020). In our case, institution-dependence captures these exposure shares. This implies that a causal interpretation rests on assumptions similar to those entertained in work drawing on shift-share instruments, including work on U.S. Presidential elections in response to economic

[11] Institutional change abroad is plausibly driven by U.S. foreign economic and security interests, and thus not exogenous to U.S. politics. But the allocation of economic activity across U.S. counties is not obviously related to institutional change abroad in a way that co-varies with differential changes in voting in the 2016 election.

decline (e.g., Baccini and Weymouth 2021) and populist and right-wing voting in response to globalization pressures (Colantone and Stanig 2018*b*; Milner 2021). In this interpretation in the tradition of shift-share instruments, our design corresponds to reduced-form regressions.

More importantly, this parallel underscores that our design inherits the weaknesses inherent to shift-share designs based on share exogeneity. A causal interpretation has to rely on the exogeneity of institution-dependence. Yet, institution-dependence as an attribute of past economic structure is inseparable from other long-run economic attributes that predict the 2016 election outcome. We control for the most plausible confounders based on prior literature, and discuss below, drawing on sensitivity analysis (Cinelli and Hazlett 2020), that unobserved confounders would have to be implausibly strong to account for the associations we report. Ultimately, however, this remains a design that does not lend itself easily to a causal interpretation. Hence, we interpret the results as suggestive.

**Results**

We present results in Table 1. In column 1, we estimate the baseline model. The results indicate a strong, statistically significant association between a county's institution-dependence in 1998 and the vote share of Donald J. Trump. Moving from the 10th percentile to the 90th percentile on the county-level institution-dependence measure corresponds to a change in the vote share of Donald J. Trump of about 8.5 percentage points.

In column 2, we include the Republican party vote share in the 2012 Presidential election. This variable captures county-specific support for Republican candidates, and thus absorbs a large number of confounders. The coefficient estimate decreases in size, but the association remains positive and statistically significant at the 5% level. We obtain similar results, reported in Appendix SI.4.6, when instead using the difference in vote shares between 2016 and 2012 for the outcome variable. In Appendix SI.4.4, we additionally offer results that replace the outcome variable with the change in the Republican party vote share between 2016 and 2000, allowing for an interpretation around shifts in party support for a time period that matches our exposure variable, and for two elections that featured

no incumbent candidate.

In column 3, we distinguish our explanation from two other main threads in the literature: the China shock and automation. We control for the China shock, the surge in manufacturing goods imports from China after the U.S. granted permanent normal trade relations to China in the context of its WTO entry in 2001, using data from Autor, Dorn and Hanson (2016) that we match to counties. In Appendix SI.4.5, we control alternatively for the change in trade policy uncertainty toward China at the product-level (Handley and Limão 2017), which is plausibly exogenous to local industry dynamics in the U.S. To account for automation, we control for the adoption of industrial robots, using industry-level data from the International Federation of Robotics between 2004 and 2015 (following Acemoglu and Restrepo 2020), and matching these to the county-level.[12] In Appendix SI.4.5, we show that we obtain similar results when using an alternative measure from Acemoglu and Restrepo (2020), based on data from five European economies, of the propensity to adopt industrial robots. We also obtain similar results when controlling for the share of jobs in the local economy at risk of automation, based on routine task intensity from Autor and Dorn (2013), or for imports of industrial robots drawn from transaction-level maritime trade data, sourced from Panjiva, that we match to the location of importing firms (the U.S. imports most industrial robots; for evidence of the strong correlation between robot adoption and robot imports, see Brynjolfsson et al. 2026).

While the coefficients on these control variables reported in Table 1 remain insignificant, the models do not seek to describe the effects of the China shock or of robot adoption. Indeed, much of the literature suggests a downward bias in the China shock variable, and positive coefficients on the variable are often contingent on the use of instrumental variables. The concerns in that literature are largely rooted in differential patterns in imports from China. Our main variable does not incorporate imports and thus is not affected by those concerns to the same extent. Similarly, when using other variants of the China shock variable (such as a variable based on trade policy uncertainty), we do uncover the expected positive effects. The endogeneity of imports – but not of local economic composition

[12] 2004 is the earliest year in which these data are available for the U.S.

– suggests why we recover the expected results for the China shock when using the arguably more exogenous measure of changes in trade policy uncertainty instead of the change in observed imports.

**Incorporating institutional change.** In a variant of our main measure, we incorporate institutional change abroad directly. We first calculate an industry-level measure of institutional change as the average change in the Varieties of Democracy Property Rights Index for each U.S. trade partner, weighted by the trade partner's share in U.S. imports in that industry.[13] With $\Delta\rho_l$ as the change in the strength of institutions of country $l$, $m_{i,l}$ as imports of industry $i$ from country $l$, and $m_i$ as total imports of industry $i$, industry-specific institutional change is $\widehat{\rho}_i = \sum_l \Delta\rho_l m_{i,l}/m_i$.

From there, we obtain a composite county-level measure, which we label *Institution-shift exposure*,

$$\phi_c = \sum_i \frac{q_{ic}}{q_c} \tau_i \widehat{\rho}_i, \tag{4}$$

which captures the coincidence of industry-specific institutional change abroad with industries that are sensitive to such institutional change – or, put differently, whether local industries absorb institutional change. $\phi_c$ resembles a standard shift-share variable, with industry-specific institutional change as the shift. Columns 4-6 of Table 1 show that our results remain robust when relying on this variable. We reiterate that institutional change abroad is plausibly driven in part by U.S. multinationals (for a possible mechanism, see Thrall 2025), such that we caution against an interpretation of institutional change as exogenous to U.S. politics.

In Appendix SI.4.12, we offer additional results by dividing industries at the median into high and low institution-dependence. We then calculate two separate variables that reflect whether institutional change abroad coincides with industries in the local economy that score high and low, respectively, on institution-dependence. Institutional change abroad has positive and statistically significant effects

[13] This approach necessitates choosing a starting point for institutional change. Because our measure of institution-dependence is based on 1998 (which, in turn, we chose because it reduces the number of required crosswalks), for the following results we consider the difference between the three-year average of the property rights score between 1998 and 2015. The results are not sensitive to this choice, as reported in Appendix SI.4.2.

**Table 1 Institution-dependence and 2016 Election**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Exposure | | | Exposure + Shift | | |
| *Dependent variable: 2016 vote share Donald J. Trump* | | | | | | |
| Institution-dependence | .51 | .14 | .14 | | | |
| | (.048) | (.018) | (.018) | | | |
| Institution-dependence + shift | | | | 3.77 | 1.04 | 1.07 |
| | | | | (.579) | (.239) | (.245) |
| Urban county | -5.42 | -3.11 | -3.10 | -7.14 | -3.54 | -3.53 |
| | (.448) | (.163) | (.163) | (.454) | (.156) | (.156) |
| % manufacturing | -.12 | -.015 | -.015 | -.009 | .014 | .008 |
| | (.019) | (.008) | (.008) | (.016) | (.007) | (.008) |
| % high school | -.12 | -.21 | -.21 | -.15 | -.22 | -.22 |
| | (.067) | (.017) | (.017) | (.068) | (.018) | (.018) |
| % white | .62 | .100 | .099 | .63 | .098 | .097 |
| | (.018) | (.008) | (.008) | (.018) | (.008) | (.008) |
| China shock | | | .021 | | | .010 |
| | | | (.018) | | | (.017) |
| Robot adoption | | | -.051 | | | .070 |
| | | | (.039) | | | (.037) |
| 2012 vote share | | .91 | .91 | | .92 | .92 |
| | | (.010) | (.010) | | (.010) | (.010) |
| Constant | -2.16 | 12.6 | 12.3 | 23.0 | 19.4 | 19.4 |
| | (6.15) | (1.69) | (1.71) | (5.30) | (1.45) | (1.45) |
| State FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Number Obs. | 3,106 | 3,106 | 3,106 | 3,106 | 3,106 | 3,106 |

Coefficient estimates and robust standard errors. Linear regression models, state fixed effects included. Columns 1-3 use county-level institution-dependence. Columns 4-6 combine county-level institution-dependence with institutional change abroad.

only when it coincides with industries in the local economy that score high on institution-dependence; for industries that score low on institution-dependence, institutional change abroad has small and statistically insignificant effects. In Appendix SI.4.1.a, we also report that this variable predicts subsequent import pressures, analogously to our measure of institution-dependence.

**Supplier offshoring.** An alternative explanation is that firms in institution-dependent industries offshored inputs as institutions abroad strengthened. The associations we report might then not be driven by import pressures on institution-dependent industries, but instead by the offshoring of suppliers co-located with institution-dependent industries. Several pieces of evidence, discussed in Appendix SI.4.10, suggest our results are not driven by such input offshoring. First, in the trade analyses, we find no evidence that imports increase for suppliers to institution-dependent industries. Second, transportation costs of inputs *increase* in an industry's institution-dependence. This suggests that these industries are unlikely to engage in more input offshoring compared to other industries. Third, we control for imports of inputs of industries located in a county, thus accounting directly for supply-chain dependence and offshoring. Fourth, we control for measures of the offshorability of jobs in the local economy, capturing the potential for offshoring based on Owen (2017).

**Additional Evidence and Mechanisms**

We present several pieces of evidence to assess key parts of our argument: that the results are not explained by anti-incumbent voting; that import pressures on institution-dependent industries have stronger effects than other import pressures; that pressures on institution-dependent industries are politically distinct because they create spillovers throughout the local economy; and that indirect trade pressures are more likely to remain uncompensated by trade adjustment assistance. We also assess the potential impact of unobserved confounders with sensitivity analyses.

**Primary elections.** To distinguish votes for Donald J. Trump from votes driven by anti-incumbent voting, we collect data from 2016 primary election outcomes at the county level. These allow us to

evaluate whether more institution-dependent counties lean toward Donald J. Trump, as a particularly protectionist candidate, among the set of 2016 Republican party candidates. We scrape data on primary elections from the New York Times website,[14] which sources from the Associated Press. We replicate the previous models, replacing the outcome variable with the vote share of Donald J. Trump among Republican primary candidates in each county. We report results in column 1 of Table 2, replicating the model from column 3 in Table 1. Even among Republican primary voters, vote shares for Donald J. Trump were higher in counties that had more institution-dependent local economies in 1998.

**Spillovers through co-location.** We suggested that institution-dependent industries are distinct because they are tied into production networks. These create spillovers throughout the local economy, reinforcing the political consequences of import pressure. To capture spillovers emanating from institution-dependent industries, we return to the input-output tables to identify customer-supplier pairs. We calculate for each industry the weighted average institution-dependence of its customers, using as weights the share of an industry's output that is sold to each industry. We combine this variable with the County Business Patterns to identify the co-location of institution-dependent industries with their suppliers. Column 2 of Table 2 demonstrates that such co-location of suppliers predicts 2016 vote shares.

**Import pressures.** To compare import pressures on institution-dependent industries to generic import pressures, we calculate import exposure per worker as the increase in industry-level imports between 1998 and 2015 (in $1000 per worker), weighted by industry employment shares in each county, separately for the top half and the bottom half of institution-dependent industries. Our framework suggests a chain from past institution-dependence to 2016 vote shares through import exposure on institution-dependent industries. Accordingly, we implement a two-stage least square model, instrumenting for import exposure on institution-dependent industries with the past institution-dependence

[14] `https://www.nytimes.com/elections/2016/national-results-map`, last accessed April 17, 2025. In 2016, two states, North Dakota and Colorado, did not have Republican primaries. Two other states, Kansas and Minnesota, provide data at the Congressional District level, which we crosswalk to counties using Ferrara and Zhou (2022).

of the local economy. Our main objective is not to establish a causal effect, but to estimate a model that incorporates import pressures induced by a county's institution-dependence, mirroring our theoretical framework. We first estimate a two-stage least square model, replicating our previous analyses, reported in column 3. We then additionally control for import exposure on industries that score low on institution-dependence, reported in column 4.[15]

The results establish the expected pattern. Import pressures on institution-dependent industries have large and positive effects, whereas we obtain a small and statistically not significant coefficient for generic import pressures. The difference between the two coefficients is also statistically significant, with a $p$-value of $.0002$. Additionally, import pressures on institution-dependent industries are more extensive than generic import pressures, with a standard deviation of $11,680 per worker compared to $2,490 per worker. The first-stage relationship between institution-dependent import pressures and county-level institution-dependence also is strong, with a $t$-statistic of approximately 5 (and a corresponding first-stage $F$-statistic of 25), validating our proposed link between county-level institution-dependence and import exposure.

Manufacturing industries tend to be more institution-dependent and experience higher import pressures. When, instead of controlling for generic import pressures, controlling for import pressures on manufacturing and on non-manufacturing industries, the coefficient on institution-dependent imports quadruples in size (not reported). While on the surface reassuring, the considerable change in the coefficient estimates is in large part an artifact of the high correlation between institution-dependent and manufacturing import pressures in our our sample. In column 5, we therefore focus on manufacturing industries exclusively and recalculate both institution-dependent and generic import pressures *within* manufacturing industries only. Both types of manufacturing import pressures have positive and statistically significant effects: consistent with prior work, manufacturing import pressures increase the vote share of Donald J. Trump in 2016. The results moreover indicate substantially

[15]We drop a small number of outliers on the import pressure variables and omit the variable for the China shock. Neither choice affects the substantive interpretation.

(and, with a $p$-value for the difference of $.0001$, statistically significantly) larger effects for institution-dependent manufacturing imports than for generic manufacturing imports, nearly by a factor of four. The larger effects for institution-dependent imports therefore do not simply reflect a difference between manufacturing industries and other industries. They are present even among manufacturing industries. As before, institution-dependent manufacturing import pressures are on average nearly twice as large, with a roughly 40% larger standard deviation (of about $9,560 per worker compared to about $6,520 per worker). This supports an interpretation around both an extensive margin of institution-dependent import pressures (which are larger) and an intensive margin (with larger effects for a given import pressure). The results remain nearly identical when controlling for non-manufacturing imports (not reported).

**Uncompensated trade pressures.** In Appendix SI.4.11, we provide results that indirect trade pressures, driven by spillovers, are less likely to be compensated by TAA. At the industry-level, the number of workers certified by TAA petitions responds to direct import pressure, but not to indirect import pressure. The indirect exposure of suppliers to institution-dependent industries is more likely to remain uncompensated, plausibly reflecting limited access and informational barriers. Moreover, we hand-code the text of the petition determinations to identify petitions filed based on indirect exposure. Even among those petitions that are filed, which are only a select subset of possible cases (Kim and Pelc 2021*a*,*b*), petitions based on indirect exposure are less likely to be certified. This suggests that the high evidentiary burden and difficulty of establishing eligibility compounds the extent of uncompensated job losses. Finally, we observe corresponding patterns at the county-level: petitions originating from institution-dependent counties are rejected at higher rates.

**Sensitivity analysis.** To consider omitted variables, we use a benchmarking approach for sensitivity analysis (Cinelli and Hazlett 2020). In the literature, the share of white residents emerges as an established and important predictor of 2016 vote shares (e.g., Morgan and Lee 2018). We assess how much stronger than this variable an omitted variable would have to be to account for the associations

we report. In our baseline model, such an omitted variable would have to be a predictor of both 2016 vote shares and institution-dependence that is over five times stronger than the share of whites. Based on these results, and given the attention paid in the literature to explaining the 2016 election results, it appears implausible that an unobserved variable would explain the reported association between institution-dependence and 2016 vote shares.

**Table 2 Mechanisms: Primary Votes, Spillovers, Imports**

| | (1) Primary votes OLS | (2) Spillovers co-location OLS | (3) Imports split 2SLS | (4) Imports comparison 2SLS | (5) Imports manuf. 2SLS |
|---|---|---|---|---|---|
| Institution-dependence | .14 | .10 | | | |
| | (.027) | (.019) | | | |
| Spillovers | | .13 | | | |
| | | (.021) | | | |
| Imports institution-dependent | | | .65 | .66 | |
| | | | (.146) | (.148) | |
| Imports generic | | | | .063 | |
| | | | | (.078) | |
| Imports institution-dependent (manufacturing only) | | | | | .84 |
| | | | | | (.204) |
| Imports generic (manufacturing only) | | | | | .21 |
| | | | | | (.046) |
| % manufacturing | -.054 | -.005 | -.19 | -.19 | -.20 |
| | (.014) | (.008) | (.051) | (.052) | (.056) |
| % high school | -.40 | -.20 | -.28 | -.28 | -.33 |
| | (.030) | (.017) | (.031) | (.031) | (.038) |
| % white | .016 | .10 | .11 | .11 | .11 |
| | (.014) | (.008) | (.015) | (.015) | (.015) |
| Urban county | -2.37 | -2.96 | -4.57 | -4.58 | -5.43 |
| | (.266) | (.166) | (.362) | (.365) | (.506) |
| China shock | -.061 | .024 | | | |
| | (.031) | (.017) | | | |
| Robot adoption | -.28 | -.15 | -.62 | -.63 | -.83 |
| | (.078) | (.041) | (.195) | (.197) | (.259) |
| 2012 vote share | .018 | .91 | .92 | .92 | .95 |
| | (.016) | (.010) | (.016) | (.016) | (.016) |
| Constant | 72.4 | 6.34 | 20.4 | 20.2 | 26.3 |
| | (2.85) | (2.00) | (2.45) | (2.48) | (2.92) |
| State FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Number Obs. | 2,964 | 3,106 | 3,090 | 3,090 | 3,090 |

Coefficient estimates and robust standard errors. State fixed effects included.

## 3.2 Survey Evidence on Trade Attitudes

To substantiate the role of trade attitudes, we turn to survey data from the Nationscape + UCLA Survey (Tausanovitch and Vavreck 2021). The online survey was conducted in multiple waves from July 2019 through January 2021, for a total of nearly 500,000 respondents. Survey weights achieve representativeness relative to the U.S. adult population (Tausanovitch et al. 2021). The survey has unique attributes for our purpose: it includes an indicator for a respondent's Congressional District and questions about attitudes toward trade. This allows us to link institution-dependence to trade attitudes, with on average several hundred respondents per Congressional District.

We calculate the institution-dependence of each Congressional District using crosswalks from Ferrara and Zhou (2022). Our outcome variable is based on whether a respondent agrees or disagrees with "the following policy: limit trade with other countries." About 150,000 respondents were asked this question. About a quarter replied with 'not sure', a quarter with 'agree', and the remainder with 'disagree'. We code our variable, opposition to trade, as one when a respondent agrees with a policy limiting trade with other countries, and as zero when a respondent answers with 'disagree'. We omit respondents who answer 'not sure'. The results are robust to other treatments of this category, discuss below and in Appendix SI.5).

We include individual-level control variables mirroring previous models: whether a respondent has a high school degree or above and whether a respondent is white. We further control, at the district-level, for manufacturing employment and for whether the majority of the population lives in an urban area, together with state fixed effects. In additional models, we control for gender, age, household income (coded one above the median), and union membership as individual-level predictors of trade policy attitudes (e.g., Mayda and Rodrik 2005), as well as for the China shock and robot adoption at the county-level. We estimate binary logit models with survey weights, specifying a binomial family and a logit link and clustering standard errors by Congressional District.

The results in Table 3 show that respondents from more institution-dependent local economies are more opposed to trade. The results are statistically significant at the 5% level and substantively large.

Moving from the 10th to the 90th percentile along the district-level institution-dependence increases opposition to trade from 33.3% to 41.4%, by approximately 24%. The result is robust to including additional control variables in column 2.

In columns 3 and 4, we replicate the previous two models and interact institution-dependence with an individual's employment status. The effects of institution-dependence are strongest for those no longer employed full-time. This is consistent with an emphasis on the labor market effects of trade. In columns 5 and 6, we consider an answer of 'not sure' as support for trade. The effect size decreases, but remains positive and statistically significant.

In Appendix SI.5, we report results for two alternative outcomes. First, we replace the outcome with whether to "impose trade tariffs on Chinese goods". This question focuses explicitly on tariffs, but singles out a specific country, China. Second, we replace the outcome variable with whether "US trade with other countries has a mostly positive effect on jobs for US workers". This question inverts the wording, which ensures that results are not driven by a tendency to approve of statements. Moreover, the question focuses explicitly on workers, limiting price or consumption effects (Naoi and Kume 2015). Most respondents were asked only one variant of the three questions (our main results are based on the most frequently asked question of the three), thus ruling out that similarity across results is driven by voters' desires to give consistent answers.

# 4 Evidence from Western Europe

We focused on the U.S., where disaggregated data on local economic conditions are available and the 2016 election featured a protectionist candidate. Our broader framework should apply to other established democracies as well. We offer evidence from trade flows and survey data for Western European countries, focusing on the member states of the European Union before its 2004 enlargement toward Central and Eastern Europe (the EU15), as a set of established democracies with strong institutions.

We first replicate our analyses of trade flows. We source bilateral product-level trade data from

**Table 3 Institution-dependence and Trade Attitudes**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Base model | | Employment Status | | 'Not sure' included | |
| *Dependent variable: Support for policy to limit trade with other countries* | | | | | | |
| Institution-dependence | 3.08 | 2.87 | 3.74 | 3.58 | 2.35 | 2.24 |
| | (.688) | (.689) | (.719) | (.728) | (.620) | (.617) |
| x full-time employed | | | -1.41 | -1.44 | | |
| | | | (.535) | (.555) | | |
| % manufacturing | -.13 | .38 | -.16 | .35 | -.03 | .43 |
| | (.452) | (.520) | (.452) | (.520) | (.389) | (.439) |
| High school | -.25 | -.26 | -.25 | -.28 | -.037 | -.11 |
| | (.038) | (.039) | (.038) | (.039) | (.033) | (.034) |
| White | .18 | .21 | .18 | .21 | .23 | .21 |
| | (.030) | (.032) | (.030) | (.031) | (.027) | (.028) |
| Urban CD | -.12 | -.099 | -.11 | -.095 | -.053 | -.067 |
| | (.053) | (.051) | (.052) | (.051) | (.048) | (.048) |
| Full-time employed | | | .68 | .76 | | |
| | | | (.249) | (.258) | | |
| China shock | | -.012 | | -.012 | | -.012 |
| | | (.012) | | (.012) | | (.010) |
| Robot adoption | | -.036 | | -.036 | | -.034 |
| | | (.030) | | (.030) | | (.028) |
| Man | | -.24 | | -.25 | | -.021 |
| | | (.023) | | (.023) | | (.020) |
| Age | | .004 | | .005 | | .007 |
| | | (.001) | | (.001) | | (.001) |
| > Median Household Income | | -.26 | | -.27 | | -.12 |
| | | (.026) | | (.026) | | (.023) |
| Union member | | .57 | | .54 | | .61 |
| | | (.039) | | (.039) | | (.036) |
| Constant | -1.48 | -1.11 | -1.80 | -1.47 | -1.98 | -2.07 |
| | (.301) | (.307) | (.318) | (.330) | (.276) | (.279) |
| State FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Survey Weights | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Number Obs. | 106,526 | 101,820 | 106,526 | 101,820 | 146,387 | 138,765 |

Coefficient estimates and robust standard errors clustered by Congressional District. Binary logit models (binomial family with a logit link), state fixed effects included, using survey weights.

COMEXT, yielding a sample of about 50 million observations. In Appendix SI.6, we report that trade patterns in Western European countries followed a similar pattern as in the U.S., with an increase in institution-dependent imports from countries that experienced institutional strengthening.

To assess the political implications, we turn to the European Social Survey, ESS (European Social Survey European Research Infrastructure 2023), a cross-country survey representative of the population aged 15 and over in each country. We again limit the sample to the EU15, which corresponds to twelve countries included in the ESS.[16] The survey asks respondents which party they voted for in the last national election. We link this information to an assessment of party support for protectionism versus trade liberalization on scale from 1 to 10 from the Chapel Hill Expert Survey, which was only asked in its 2019 edition (Rovny et al. 2025). In the CHES, over 400 political scientists and experts on political parties were asked to assess the issue-specific positions of the leadership of political parties. A major advantage, relative to the 2016 U.S. Presidential election, is that the measure provides a gradual assessment of support for protectionist parties, which is less coarse than support for one specific candidate in one election.

For Western European countries, publicly available data are not comparable to U.S. data in their granularity across industries and local economies.[17] We instead link the European Social Survey data based on the occupation of a respondent, which the ESS identifies with four-digit ISCO codes. To do so, we calculate the average institution-dependence of a respondent's occupation, based on the distribution of occupations across industries derived from Bureau of Labor Statistics occupation-industry matrices. We include control variables analogous to our previous analyses, accounting for the respondent's education level and whether the respondent was born abroad (replacing our measure of race in the U.S. samples), as well as whether the respondent works in manufacturing based on the provided NACE industry code. The resulting sample includes 11,838 individuals.

We estimate linear regression models that predict the protectionism of the party an individual

[16] Austria, Belgium, Germany, Denmark, Finland, France, Great Britain, Ireland, Italy, the Netherlands, Portugal, Spain.

[17] For example, in Germany, information on the county of a respondent is considered sensitive information, and cross-tabulations with detailed industry data are not available.

supports, using survey weights. The results, presented in column 1, corroborate the previous findings. The results are robust to including country fixed effects and control variables comparable to our earlier models: for the China shock (country- and industry-specific import exposure per worker) and automation (country- and industry-specific robot adoption per worker), and individual-level control variables for gender, age, household income, and union membership, reported in column 2.

The downside of the CHES measure is that the assessment of party positions on trade is only available for one year, corresponding to the ninth round of the European Social Survey. To capture party protectionism in earlier years, we turn to the Comparative Manifesto Project (Lehmann et al. 2025), which extends the sample back to 2002, the first round of the European Social Survey. The Comparative Manifesto Project scores the text of official party manifestos across topics. We calculate the logged difference between party support for protectionism and for free trade and use this as our outcome variable (adding .5 to handle zeros, following García-Viñuela, Motz and Riera 2024). As alternative, we code a dummy variable coded 1 for parties that make more statements supporting protectionism than statements supporting free trade. We then match the party-level data from the Comparative Manifesto Project with the European Social Survey across the nine waves from 2002 to 2018. We replicate our previous model specifications, adding survey-wave fixed effects. The results, in columns 3 and 4 for the continuous and columns 5 and 6 for the binary variable, corroborate the previous patterns.

Overall, these results point to a broader pattern that extends to Western European countries, is evident in a sample that includes earlier years, and assesses support for protectionist parties specifically.

**Table 4 Western Europe: Voting for Protectionist Parties**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | CHES (continuous) ESS 9 | | Manifesto (continuous) ESS 1-9 | | Manifesto (binary) ESS 1-9 | |
| *Dependent variable: protectionism of supported party* | | | | | | |
| Institution-dependence | 1.18 | 1.13 | .088 | .096 | .051 | .044 |
| | (.195) | (.252) | (.014) | (.014) | (.012) | (.013) |
| Manufacturing | -.30 | -.24 | -.019 | -.001 | -.012 | -.008 |
| | (.072) | (.098) | (.005) | (.005) | (.004) | (.005) |
| Education years | -.45 | -.38 | -.091 | -.009 | .005 | -.015 |
| | (.065) | (.083) | (.005) | (.005) | (.004) | (.005) |
| Born abroad | .23 | .076 | .0050 | .014 | .017 | .008 |
| | (.095) | (.127) | (.007) | (.007) | (.006) | (.006) |
| Urban area | | -.073 | | .006 | | .005 |
| | | (.070) | | (.004) | | (.003) |
| China shock | | -.12 | | .005 | | .004 |
| | | (.043) | | (.006) | | (.006) |
| Robot adoption | | .20 | | -.061 | | -.027 |
| | | (.243) | | (.017) | | (.016) |
| Man | | -.041 | | -.0003 | | .003 |
| | | (.060) | | (.003) | | (.003) |
| Age | | -.62 | | -.069 | | -.028 |
| | | (.080) | | (.005) | | (.005) |
| > Median Household Income | | -.31 | | -.050 | | -.030 |
| | | (.065) | | (.003) | | (.003) |
| Union member | | .26 | | .054 | | .045 |
| | | (.063) | | (.004) | | (.003) |
| Constant | 5.65 | 7.95 | .18 | .18 | .27 | .19 |
| | (.207) | (.441) | (.015) | (.028) | (.013) | (.026) |
| Survey Weights | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Country FE | | ✓ | | ✓ | | ✓ |
| Survey FE | | | ✓ | ✓ | ✓ | ✓ |
| Number Obs. | 11,838 | 10,040 | 101,256 | 74,337 | 101,256 | 74,337 |

Coefficient estimates and standard errors, using survey weights and, in columns 1 and 2, survey strata.

# 5 Conclusion

We presented a framework that highlighted the role of institutions in facilitating economic exchange within production networks. Institutional convergence across countries then erodes comparative advantage and creates new opposition to trade. Global institutional change thus leaves an observable imprint in the domestic politics of established democracies, which differs both in its origins and its nature from explanations rooted in changes in trade policy or technology.

Our paper suggests several avenues for future research. For example, our focus on domestic production networks offers new implications for the contours of cleavages over open markets, because trade pressures have *indirect* effects. We offered evidence for some of these effects, and highlighted the resulting challenges for traditional policy instruments, such as trade adjustment assistance. Accounting for indirect trade exposure has potentially broader implications. Economic ties between firms imply that firms across industries have a stake in each other's fortunes, resulting in new patterns of fragmentation and cohesion: within industries, firms differ in their access to specialized inputs and their competitiveness on international markets; across industries, firms tied together in production networks might share a common stance. Whether these ties are also recognized by employees, and are reflected in their attitudes toward open markets, remains a fruitful area for future research.

We also highlight a tension for foreign economic and security policy. Institutional reform abroad, bundled with global market integration, is an important component of foreign economic and security policy (Davis 2008). While promoting a stronger institutional environment in other countries may serve foreign policy goals around stability and security, it also undermines the competitiveness of firms and workers at home. Where institutional reforms and trade reform are linked, as is regularly the case in trade agreements (Büthe and Milner 2008), domestic constituents experience dual pressures. Sustaining domestic support for open markets in the U.S. might have been easier in a world dominated by non-democracies with weak institutions, where U.S. firms and workers enjoyed a privileged institutional environment.

Finally, the past years have seen the erosion of institutions in several established democracies. Previous work has interpreted this development as a downstream consequence of the globalization backlash and globalization pressures (Broz, Frieden and Weymouth 2020). This is notable in the context of our framework for two reasons. First, the same constituencies that for a long time benefited from superior institutions at home appear to have turned against those institutions once other countries caught up and the economic effects of superior institutions dissipated.

Second, this erosion of institutions is likely to place further pressures on institution-dependent industries, sparking more discontent. Viewed from a dynamic perspective that links systematic and domestic explanations (Chaudoin, Milner and Pang 2015; Ahlquist and Wibbels 2012), our account thus points to a pattern of joint waves of institutional change and global trade patterns. Convergence in institutions at a systemic level upsets domestic equilibria in other countries and, from there, begets further institutional change. This suggests a certain tragedy inherent to the global move toward democracy: the erosion of institutional comparative advantage, ignited by institutional strengthening abroad, can ultimately result in the erosion of institutions themselves.

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