Political Distance and Trade*

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Abstract

I quantify how trade responds to political distance across geopolitical environments and what this variation implies for the welfare effects of political realignment. Using UN General Assembly voting disagreement, I estimate a structural gravity model of manufacturing trade for 167 countries over 1966–2020. A one-standard-deviation increase in political distance within a country pair is associated with 3 percent lower trade, a quarter of a regional trade agreement's effect. Trade subsequently declines; prior declines lack robust support. The responsiveness peaked in the Cold War, was weak and insignificant during 1995–2007, and strengthened again by 2016–2020 without reaching its peak. Under a trade-cost interpretation, I assess the welfare effects of political realignment by imposing 1962 political distances in 2020. This brings some countries closer and pushes others apart. The average welfare loss is half a percent under 2016–2020 trade responsiveness, but 40 percent larger under Cold-War responsiveness.

Keywords: Political distance, UN General Assembly voting, gravity, sanctions, trade fragmentation, geopolitics

JEL-Codes: F13, F14, F51, F52

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1 Introduction

Renewed great-power rivalry has raised the prospect of a world once again organized around rival political blocs (Gopinath et al. 2025). A growing literature documents the effects of political realignment on trade and welfare. However, evidence remains limited on how trade responsiveness to political distance varies across geopolitical environments. If political disagreement constrains trade more strongly under bloc confrontation than during post-Cold-War integration, estimates from recent periods may understate the costs of renewed fragmentation. I ask how trade responds to political distance across geopolitical environments and what this variation implies for the welfare effects of political realignment.

I combine political distance, measured by disagreement in UN General Assembly votes, with domestic and international manufacturing trade data for 167 countries over 1966–2020 in a structural-gravity framework.

First, I document substantial historical variation in trade responsiveness to political distance. Responsiveness was strongest during the Cold War, weak and statistically insignificant during 1995–2007, and strengthened again during 2016–2020, but remained below its Cold-War peak. Over the full period, an increase in political distance equal to the mean within-pair standard deviation is associated with three percent lower trade, around one quarter of the effect of removing a trade agreement. Second, I provide evidence favoring a trade-cost interpretation of political frictions over shifts in consumer demand, which matters for welfare comparisons. Controlling for sanctions attenuates the responsiveness, while security-related disagreement predicts trade more strongly than human-rights disagreement. Third, under a trade-cost interpretation, I quantify how historical differences in trade responsiveness change the welfare consequences of a fixed political realignment. I impose 1962 political distances on the 2020 economy. The responsiveness estimated for 2016–2020 implies that average welfare is half a percent lower, with losses concentrated in Europe, while the Americas gain on average. Holding the baseline economy and political realignment fixed, using late-Cold-War responsiveness increases the average welfare loss by approximately 40 percent. The welfare effects of fragmentation therefore depend on both political realignment and trade responsiveness.

I first set up a one-sector Armington (1969) model that guides both the estimation and the counterfactual analysis. Political frictions can reduce consumers' valuation of a partner's goods or raise the cost of supplying them, for example through sanctions or contract-enforcement risk. Both channels enter the gravity equation as a common bilateral friction, so aggregate trade flows alone cannot distinguish them. Their welfare implications nevertheless differ. Under a trade-cost interpretation, calculated real-income changes measure welfare changes. Under a demand-shifter interpretation, those changes also reflect changing preferences and do not provide a standard welfare comparison.

I measure political distance using disagreement in countries' votes in the United Nations General Assembly. I use a distance measure based on Cohen's (1968) quadratically weighted κ, which weights outright opposition more heavily than differences involving abstention and adjusts for agreement expected from countries' voting patterns. It captures changes in political disagreement both within and across geopolitical regimes: for example, US–Soviet disagreement was high during the late Cold War, declined sharply around its end, and subsequently rose again between the United States and Russia.

I combine political distance with domestic and international manufacturing trade from the CEPII Trade and Production database (Mayer et al. 2023). The panel covers approximately 92 percent of world manufacturing output on average. Including domestic trade captures substitution between domestic and foreign suppliers, which is important for estimating the effects of international trade costs (Dai et al. 2014; Larch and Yotov 2024). Domestic expenditure shares also connect the estimation to the counterfactual analysis.

I estimate the gravity equation using Poisson pseudo-maximum likelihood (Silva and Tenreyro 2006; Correia et al. 2020). Identification uses changes in political distance within country pairs over time, conditional on exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects (Yotov et al. 2016; Head and Mayer 2014). A causal interpretation requires changes in political distance, conditional on these fixed effects, to be unrelated to otherwise unobserved bilateral trade shocks. I initially exclude regional trade agreements, sanctions, and tariffs because they may transmit the effects of political distance to trade. I then introduce these controls to assess potential channels, interpreting coefficient changes descriptively rather than as causal mediation effects.

The baseline estimate implies that an increase in political distance equal to the mean within-pair standard deviation is associated with 3 percent lower bilateral trade. In the specification controlling for regional trade agreements, the estimated one-SD effect is approximately one quarter of the effect of removing an agreement. Similar one-SD effects arise from alternative UN voting agreement statistics. On a common sample with the preferred fixed effects, ideal-point distance (Bailey et al. 2017) yields a smaller negative one-SD point estimate, which is statistically insignificant.

A causal interpretation faces two threats: time-varying bilateral confounding and reverse causality. Military conflict may simultaneously increase political distance and reduce trade (Glick and Taylor 2010; Korovkin and Makarin 2023), potentially confounding the estimated relationship. The negative relationship nevertheless persists when I control for militarized interstate disputes or exclude observations during and around them. Cultural and religious distance may influence both foreign-policy alignment and bilateral trade through trust and preferences. Adding time-varying measures of these distances (Pellegrino et al. 2025), separately or jointly, leaves the estimate essentially unchanged. Political alignment may itself respond to economic relationships, as discussed in the literature on foreign aid (Alesina and Dollar 2000) and economic dependence (Kleinman

et al. 2024). This creates a potential reverse channel from trade to political distance. Leads and lags of political distance (Baier and Bergstrand 2007; Wooldridge 2010) and complementary local projections (Jordà 2005; Boehm et al. 2023) show a gradual decline in trade following political divergence, with no robust evidence of preceding declines. Taken together, these findings are consistent with a causal interpretation, although unobserved time-varying bilateral factors cannot be ruled out.

The average responsiveness conceals substantial variation across geopolitical environments. I estimate annual political-distance coefficients without imposing predefined geopolitical periods. In the unrestricted sample, responsiveness peaks in the late 1980s, becomes statistically insignificant during 1995–2007, and subsequently strengthens, reaching about half its late-Cold-War peak during 2016–2020. The annual pattern is robust to excluding country pairs observed for fewer than 28 years, suggesting that short panels do not drive it. The recent strengthening represents a partial return to historically high trade responsiveness.

I assess the trade-cost and demand-shifter interpretations using policy controls and disagreement on different UN resolution topics. Controlling for sanctions reduces the political-distance coefficient by approximately one third, while controlling for regional trade agreements produces little attenuation. Human-rights disagreement may affect consumers' willingness to purchase a partner's goods. Security disagreement may affect firms through regulatory scrutiny, financing conditions, or the risk of future restrictions. The relationship is strongest for security-related disagreement; the human-rights coefficient is small and statistically insignificant. Security disagreement also remains negatively and significantly associated with trade when observations with active sanctions are excluded. Together, these findings favor trade costs over a predominantly consumer-demand explanation, without separately identifying the channels.

Finally, under a trade-cost interpretation, I quantify the welfare effects of renewed political bloc confrontations by imposing 1962 political distances on the 2020 economy. I reconstruct missing political distances for 1962, the year of the Cuban Missile Crisis, using predecessor states and the historical relationships of countries that are politically similar in 2020. The realignment brings some country pairs politically closer while moving others farther apart. I translate changes in political distance into trade frictions using the 2016–2020 responsiveness estimated without policy controls, allowing the shock to incorporate these potential channels. In the model calibrated to manufacturing trade and production, welfare falls in about 70 percent of countries, with an unweighted average loss of 0.55 percent. Losses are concentrated in Europe, while the Americas gain on average. Holding the estimation specification, baseline economy, and realignment fixed, late-Cold-War responsiveness amplifies both gains and losses and increases the average welfare loss by approximately 40 percent.

Contribution to the literature First, I contribute to the literature on political relations and trade by documenting how trade responsiveness to political distance changes across geopolitical environments. Early work shows that military alliances have stronger trade effects under bipolarity than multipolarity (Gowa and Mansfield 1993). Recent research documents geopolitical fragmentation and estimates trade responses to political alignment and bilateral tensions (Gopinath et al. 2025; Bosone and Stamato 2024; Fan et al. 2026; Chevalier et al. 2026). Historical studies examine evolving trade barriers across the Iron Curtain and the effects of Cold War political realignments (Campos et al. 2026; Meyer and Wesseler 2026). I connect these strands by tracing annual trade responsiveness to political distance in a common structural-gravity framework over 1966–2020. This provides a common basis for comparing the strength of political frictions across Cold War confrontation, post-Cold-War integration, and renewed fragmentation.

Second, I contribute evidence on the mechanisms linking political relations to trade. One strand of the literature documents consumer-demand responses to political tensions (Michaels and Zhi 2010; Pandya and Venkatesan 2016; Heilmann 2016). Another examines how trade policies shape bilateral trade costs, including the effects of sanctions (Felbermayr et al. 2020) and regional trade agreements (Baier and Bergstrand 2007; Larch and Yotov 2024). Controlling jointly for sanctions and regional trade agreements leaves more than 60 percent of the baseline political-distance coefficient intact, suggesting that the relationship extends beyond observed policy restrictions and agreements. I combine these policy controls with disagreement on different UN resolution topics to assess the relative support for trade-cost and consumer-demand explanations. Although these exercises do not separately identify the channels, they provide evidence favoring a trade-cost interpretation.

Third, I contribute to the quantitative literature on the welfare effects of political fragmentation (Bekkers and Góes 2022; Javorcik et al. 2024; Campos et al. 2023). This literature considers both political realignment and increased sensitivity of trade barriers to geopolitics (Hakobyan et al. 2026). Bonadio et al. (2026) show that recent decoupling combined rising cross-bloc trade costs with falling within-bloc costs, generating net real-income gains for the median country in their model. I contribute a historical empirical benchmark for trade responsiveness by comparing the welfare consequences of a fixed political realignment under recent and late-Cold-War responsiveness. Holding the baseline economy, political realignment, and trade elasticity fixed isolates the role of the estimated trade response. Historical estimates thus provide an empirical reference for assessing how sensitive the welfare costs of renewed fragmentation are to the responsiveness used in the counterfactual.

The remainder of the paper is organized as follows. Section 2 introduces the empirical framework and data, Section 3 presents the results, Section 4 quantifies the welfare effects of a New Cold War, and the final section concludes.

2 Gravity and political distance

Political frictions cannot be observed directly, and they can reduce trade through demand or through costs. This section sets out how both channels enter structural gravity and how I measure the frictions behind them.

2.1 Conceptual framework

To fix ideas, consider a standard one-sector Armington model (Armington 1969). I add a bilateral demand shifter, as in Yotov et al. (2016), allowing political frictions to affect consumers' relative valuation of goods independently of trade costs. Political frictions may then reduce bilateral trade by lowering consumers' relative valuation of goods from a politically distant country (Pandya and Venkatesan 2016; Heilmann 2016) or by increasing the costs of supplying those goods (Crozet and Hinz 2020; Antràs and Foley 2015; Felbermayr et al. 2020).

Each country produces a distinct variety. Let p_i denote its factory-gate price, Y_i the value of production, and E_j aggregate expenditure in destination j. For expositional simplicity, I suppress time indices throughout this subsection. Preferences over country-varieties are homothetic and have a common CES elasticity $\sigma > 1$. Let $a_{ij} > 0$ denote an inverse demand shifter for goods from i in destination j: a larger value lowers their relative utility weight. Accordingly, the representative consumer in country j solves the problem

$$\max_{\{q_{ij}\}_i} U_j = \left(\sum_i a_{ij}^{\frac{1-\sigma}{\sigma}} q_{ij}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \quad \text{s.t.} \quad \sum_i p_i \tau_{ij} q_{ij} \leq E_j,$$

where $p_i \tau_{ij}$ is the delivered price of the good from origin i in destination j and $\tau_{ij} \geq 1$ denotes the iceberg trade cost.

The inverse demand shifter a_{ij} and bilateral trade costs τ_{ij} consist of a persistent component and one that depends on latent bilateral political frictions $pf_{ij} \geq 0$:

$$\begin{aligned} \ln a_{ij} &= \ln \bar{a}_{ij} + \gamma_d pf_{ij}, \qquad \gamma_d \geq 0, \\ \ln \tau_{ij} &= \ln \bar{\tau}_{ij} + \gamma_s pf_{ij}, \qquad \gamma_s \geq 0. \end{aligned}$$

Here, γ_d captures demand-side effects of political frictions such as country-of-origin aversion and consumer boycotts, and γ_s reflects cost-side effects such as contracting, insurance, financing, or compliance risks and trade-policy restrictions. I normalize $pf_{jj} = 0$, $a_{jj} = 1$, and $\tau_{jj} = 1$. Bilateral frictions are therefore measured relative to domestic sourcing.

Bilateral expenditures at delivered prices on the good from i shipped to j are

$$X_{ij} = q_{ij} p_i \tau_{ij} = \left(\frac{a_{ij} p_i \tau_{ij}}{P_j} \right)^{1-\sigma} E_j,$$

where $P_j = \left(\sum_i (a_{ij} p_i \tau_{ij})^{1-\sigma} \right)^{1/(1-\sigma)}$ is the price index in j. Market clearing for goods from each origin implies $Y_i = \sum_j X_{ij}$. The composite bilateral friction $\omega_{ij} \equiv a_{ij} \tau_{ij}$, with $\omega_{jj} = 1$ by the normalization above, combines the inverse demand shifter and the iceberg trade cost. Under CES preferences, these channels enter bilateral expenditure through the same composite bilateral friction and therefore cannot be separately identified from aggregate bilateral expenditures alone. As in Anderson (1979), bilateral expenditures can then be written as

$$X_{ij} = \frac{Y_i E_j}{Y} \left(\frac{\omega_{ij}}{\Pi_i P_j} \right)^{1-\sigma},$$

where $Y = \sum_i Y_i = \sum_j E_j$ denotes total world output and expenditure. The multilateral-resistance terms satisfy

$$\Pi_i^{1-\sigma} = \sum_j \left(\frac{\omega_{ij}}{P_j} \right)^{1-\sigma} \frac{E_j}{Y}, \quad P_j^{1-\sigma} = \sum_i \left(\frac{\omega_{ij}}{\Pi_i} \right)^{1-\sigma} \frac{Y_i}{Y}.$$

A change in a bilateral friction therefore affects trade directly through ω_{ij} and indirectly through equilibrium changes in multilateral resistance (Π_i and P_j), output, and expenditure. Because $\omega_{ij} = a_{ij} \tau_{ij}$, $\ln \omega_{ij} = \ln \overline{\omega}_{ij} + (\gamma_d + \gamma_s) p f_{ij}$ with $\overline{\omega}_{ij} \equiv \overline{a}_{ij} \overline{\tau}_{ij}$. The structural gravity equation then reads

$$X_{ij} = \exp\left(A_i + B_j + C_{ij} + \delta p f_{ij} \right), \tag{1}$$

with $\delta = (1-\sigma)(\gamma_d + \gamma_s) \leq 0$. $A_i = \ln Y_i - \ln Y - (1-\sigma) \ln \Pi_i$ is an origin-specific term, $B_j = \ln E_j - (1-\sigma) \ln P_j$ is destination-specific, and $C_{ij} = (1-\sigma) \ln \overline{\omega}_{ij}$ is pair-specific. When time indices are restored, A_i and B_j become origin-year and destination-year components, while C_{ij} remains pair-specific. In the empirical specification in Section 3, the structural components are absorbed by corresponding fixed effects, and the coefficient β on observed political distance is estimated from its changes within country pairs over time. Because observed political distance proxies for latent political friction, β need not equal δ. The distinction between the two channels matters for interpreting real-income changes as welfare changes. If political frictions operate through trade costs, changes in E_j / P_j measure welfare. If they operate through demand shifters, the same changes partly reflect changes in preferences, so comparing them across equilibria is not a standard welfare comparison. Section 3 therefore assesses which channel the data favor.

2.2 Measurement of political distance

Political frictions are not directly observable. As a proxy, I use disagreement in countries' UNGA voting positions and refer to the resulting index as political distance. It captures the extent to which two countries take different positions on the issues considered by the General Assembly; it does not imply that UN voting disagreement itself constitutes the economic friction affecting trade.

The starting point is the combined voting record of United Nations (2026). It spans 1946–2025 and contains 5,694 recorded decisions. Excluding one non-resolution decision leaves 5,693 final-passage resolutions for the analysis. Building on these records, I compile a panel of measures of political distance. Relative to Häge (2017), the panel extends the temporal coverage and provides both measures computed from all resolutions and topic-specific measures.[1] The panel covers only recorded final-passage votes, while amendments and paragraph votes are excluded.

The baseline measure uses all retained resolutions. I use alternative agreement statistics for robustness and topic-specific resolution samples to investigate mechanisms. Figure 1a shows the total and topic-specific counts over the 1966–2020 estimation period. During these years, 4,785 resolutions underwent final-passage voting, an average of 87 per year. Resolutions on security and human rights account for approximately 31 percent and 24 percent, respectively, averaging the annual topic shares. For every resolution, countries can vote yes, no, or abstain. Countries may also be absent, which, in line with the literature, I treat as equivalent to abstaining.

To quantify political distance based on countries' votes at the UNGA, I build upon existing measures of political similarity. I construct political distance for each country pair and year from quantitative Cohen's κ, an agreement statistic for ordered vote categories. For a pair i, j in year t, $\mathcal{R}_{ijt}$ denotes the set of resolutions on which both countries are eligible and have a recorded vote or absence. Following Häge (2011), I code votes as 1 for yes, 2 for abstention or recorded absence, and 3 for no. Missing and ineligible country-resolution observations are excluded.

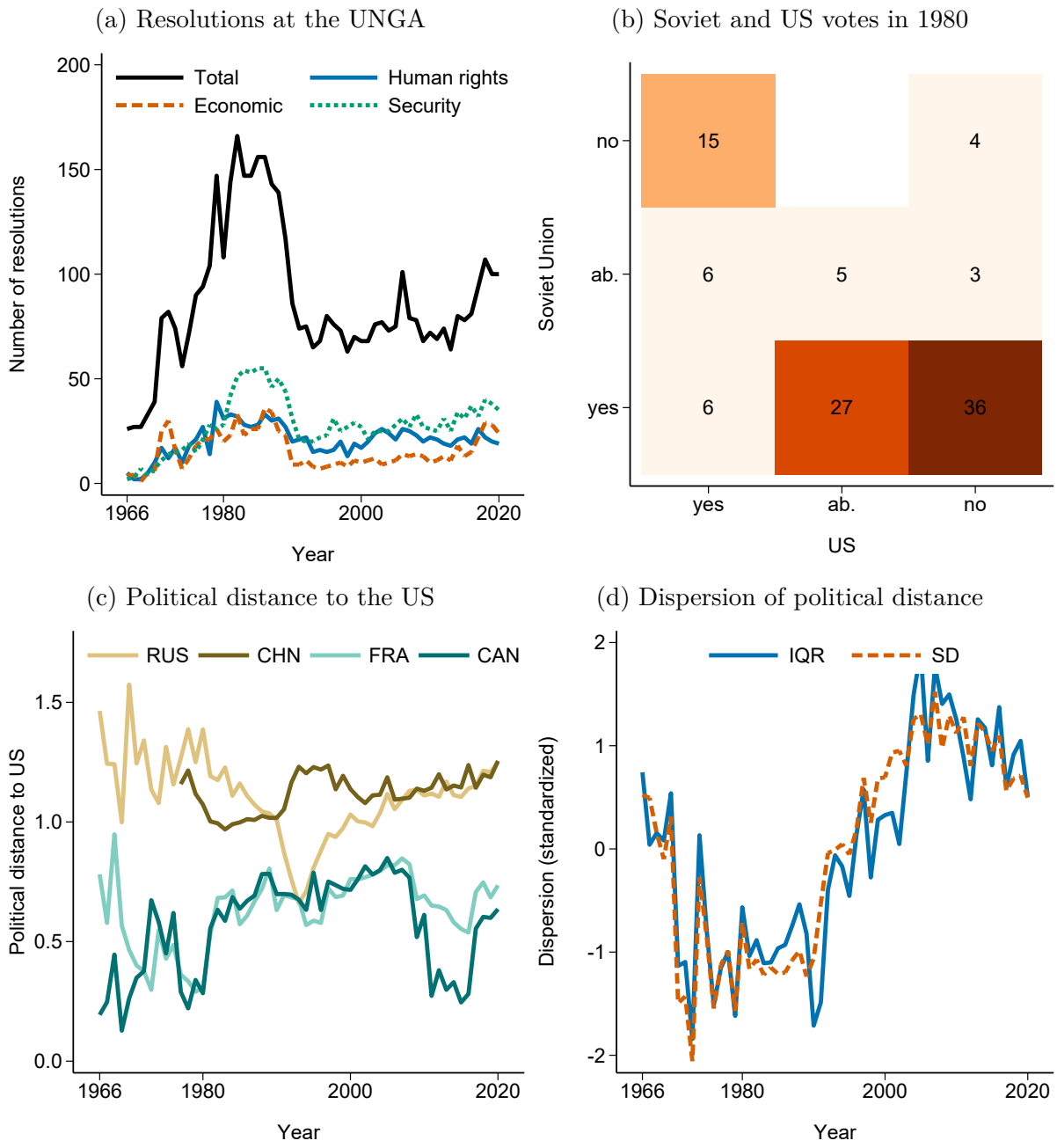
I define political distance as one minus the quantitative Cohen's κ (Cohen 1968) agreement coefficient:

$$\begin{aligned} \text{political distance}_{ijt} &= 1 - \kappa_{ijt} \\ &= \frac{\sum_r (v_{ir} - v_{jr})^2}{\sum_r (v_{ir} - \overline{v}_{i,ijt})^2 + \sum_r (v_{jr} - \overline{v}_{j,ijt})^2 + R_{ijt}(\overline{v}_{i,ijt} - \overline{v}_{j,ijt})^2}. \end{aligned} \tag{2}$$

All sums are over $r \in \mathcal{R}_{ijt}$, and $R_{ijt} = |\mathcal{R}_{ijt}|$ denotes the number of common resolutions. The terms $\bar{v}_{i,ijt}$ and $\bar{v}_{j,ijt}$ denote the countries' mean vote codes over this set. The

[1] The estimation sample is restricted by the availability of data on bilateral and domestic expenditures, which are available from 1966 to 2020.

Figure 1: Resolutions at the UNGA, votes, and political distance



Notes: Panel (a): annual totals of the number of resolutions voted on in the UNGA and the number of resolutions on security, human rights, and economic issues, classified using the subject tags, titles, and agenda titles. Panel (b): joint distribution of votes of the US and the Soviet Union in 1980. Numbers give the frequency in percent of every possible combination of votes (yes, abstain/absent, no). Panel (c): political distances of (People's Republic of) China, Russia (or the Soviet Union), France, and Canada from the US. Panel (d): annual standard deviation and interquartile range of political distance between all country pairs. Both time series are standardized. Results are similar when conditioning on dyads observed in every year.

numerator is a sum of squared vote differences, and the denominator is positive whenever the measure is defined, so political distance is non-negative.[2] The measure inherits two useful properties from quantitative κ. First, it recognizes the ordering of votes: a yes–no disagreement receives greater weight than disagreement involving an abstention. Second, it adjusts observed disagreement for the countries' marginal voting tendencies. It equals zero under identical voting and has a theoretical upper bound of two.

Figure 1b shows the joint distribution of Soviet and US votes in 1980 on 108 resolutions with recorded votes. The two countries cast the same vote on only about 14 percent of resolutions, and on more than half of all resolutions one votes yes and the other no. This voting pattern implies a political distance of approximately 1.4.

In the full historical panel, the median country-pair-year is based on 78 common resolutions. Appendix Section A.1 documents the construction and limitations of the measure in detail and validates it against the Foreign Policy Similarity (FPSIM) data of Häge (2017).

Figure 1c shows political distance from the US for China, Russia, France, and Canada. France and Canada are generally closer to the US than China and Russia, but their distances also vary substantially over time. France's relatively high distance in the late 1960s coincides with de Gaulle's pursuit of greater independence from Washington, illustrated by France's withdrawal from NATO's integrated military command in 1966. Canada's distance falls markedly during the later years of Stephen Harper's government and rises again following Justin Trudeau's accession in 2015. Soviet–US distance declines around the end of the Cold War and subsequently rises again between Russia and the US.[3]

Figure 1d describes the dispersion of political distance across all country pairs. Its historical pattern differs from the selected US relationships: dispersion is relatively low during much of the Cold War and higher in the 2000s. Changes in the size of observed political differences must therefore be distinguished from changes in their trade consequences. Section 3.4 does so by comparing annual coefficients and effects scaled by each year's dispersion.

[2]For a nonempty common-resolution set, the denominator is zero when both countries cast the same constant vote on every common resolution. Political distance is left undefined in these cases; this occurs in fewer than 0.1 percent (812 of 953,454) of unordered country-pair-years in the full 1946–2025 panel.

[3]Figure A.1 compares the κ-based distance with the ideal-point distance of Bailey et al. (2017). For the US–Russia and US–China pairs shown, the κ-based distances increase after 2015, whereas ideal-point distances remain flat or decrease moderately. These selected trajectories illustrate differences between the measures rather than establish their relative accuracy. More generally, the two measures are only moderately correlated (0.6 across international observations, 0.9 for pairs involving the US), and their year-to-year changes within country pairs are nearly uncorrelated (0.2). Appendix Table A.1 compares the two measures in the gravity model.

2.3 Trade and trade policy

For domestic and international trade flows, I use the CEPII Trade and Production (TradeProd) database (V202401, Mayer et al. 2023).[4] It covers international and domestic trade at the bilateral level for nine manufacturing sectors between 1966 and 2020. The database consists of separate files for estimation (TPe) and counterfactual analysis (TPc). The main estimates use TPe; Section 4 describes TPc used for the quantitative exercise.

The database combines data on international trade from COMTRADE with production data from the UNIDO Industrial Statistics database. For each sector, domestic trade equals gross production minus exports. Sectoral domestic trade is missing where production data are unavailable or imply negative domestic trade. For the main analysis, I sum the available nominal sectoral values to the bilateral level, matching the unit of observation of both the model and the political-distance measure. This aggregation does not require complete coverage of all nine sectors. International flows in the main sample have all nine sectors observed, whereas domestic flows can be based on fewer sectors. If all sectoral values are missing, aggregate trade is left missing. A complementary heterogeneity exercise uses the sectoral data. Throughout, I use TradeProd's combined trade variable, which relies on importer-reported values and fills gaps with exporter-reported values.

Trade-policy variables (RTAs, sanctions, and tariffs) serve as potential channels. For trade agreements, I use Mario Larch's Regional Trade Agreements Database (Egger and Larch 2008); see also Heid and Larch (2016) and Lee et al. (2023). It covers 570 multilateral and bilateral regional trade agreements (RTAs) as notified to the World Trade Organization from 1950 to 2022. Throughout, I use an RTA indicator equal to 1 if there is any type of RTA in place between both countries. Data on sanctions are from the Global Sanctions Database (GSDB-R4, see Felbermayr et al. 2020; Yalcin et al. 2025). It reports sanctions cases from 1950 to 2023 and distinguishes different types of sanctions, such as trade, travel, and arms sanctions. I use the dyadic version of the database and construct an indicator equal to one if either country imposes at least one active sanction on the other in a given year. Tariffs are from the Global Tariff Database (v_beta1-2024-12, Teti 2024), which covers 200 countries from 1988 to 2021 and corrects for measurement error in data from the World Integrated Trade Solution (WITS). I use the source's unweighted average bilateral tariff across HS6 products, as reported in its country-pair file, covering the GTD's full product range.

The main comparisons use a common sample with the required political-distance and policy data available and retain zero-valued aggregate trade flows. Tariff specifications use a shorter sample beginning in 1988; sample restrictions are detailed in the corresponding table notes.

[4]For other uses, see Bergstrand et al. (2015) and Larch et al. (2025).

Table 1: Descriptive statistics

(a) Panel dimensions

Variable	Unique values	Mean	Median	1. Quartile	3. Quartile
Year	55	18,491	20,955	14,449	23,263
Origin country	167	6,090	7,041	4,328	7,232
Destination country	167	6,090	7,204	4,496	7,695
Country pairs	26,427	38	40	28	55

(b) Panel variables and identifying variation

Variable	Mean	Median	SD Total	SD Within	$\frac{\text{Within}}{\text{Total}}$ (%)	Pairs w. SD>0 (%)
Trade	914.63	0.19	50,286.56	619.82	1.23	100.00
Political distance	0.83	0.92	0.29	0.15	52.52	99.38
Any trade agreement	0.15	0.00	0.36	0.11	29.65	25.22
Any sanction	0.14	0.00	0.35	0.16	46.42	40.46

Notes: The table summarizes the dimensions and variables of the estimation sample, which contains 1,017,000 country-pair-year observations, including both domestic and international trade. Panel (a): Countries are identified by dynamic codes that account for territorial changes. Mean, median, and the first and third quartiles are computed across the number of observations for each unique value of the respective variable. See Table A.2 for detailed coverage by country.
Panel (b): Trade denotes aggregate domestic and international trade flows in millions of current USD. Political distance is the preferred quantitative Cohen's κ measure. The trade-agreement indicator equals one if any RTA is in force between the countries. The sanctions indicator equals one if the country pair is subject to at least one sanction. SD Total is the standard deviation over the entire sample. SD Within is the average of the within-pair standard deviations. Within/Total reports the within-pair standard deviation as a percentage of the total standard deviation. The final column reports the percentage of country pairs for which the variable changes over time and thus summarizes how many pairs can contribute to identification of its coefficient.

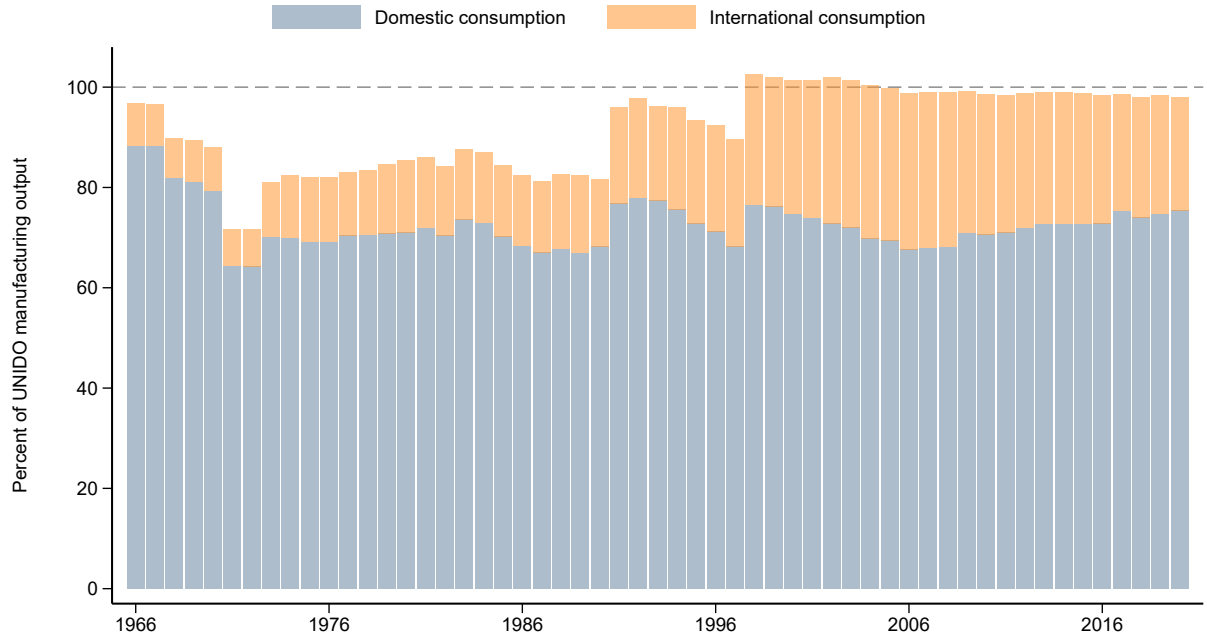
2.4 Descriptive statistics

The unbalanced panel combines domestic and bilateral trade, political distance, RTAs, and sanctions for 167 country identities, with coverage varying by country and year. Table 1 reports descriptive statistics for the dimensions of the panel in Panel (a) and the main variables in Panel (b).

The panel spans 1966–2020 and contains 26,427 directed country pairs. Its 167 country identities use dynamic TradeProd codes that distinguish territorial changes, including the Soviet Union and Russia and Germany before and after reunification. Of the pairs missing relative to the square benchmark, 390 involve countries never observed in the same year; others lack trade observations. Country pairs are observed for 38 years on average and 40 at the median, with an interquartile range of 28–55 years. More than one quarter are observed throughout all 55 years (Appendix Figure A.2b); annual coverage increases over time (Appendix Figure A.2a).

Panel (b) of Table 1 summarizes 1,017,000 exporter-importer-year observations. Trade

Figure 2: World manufacturing output and world trade



Notes: Coverage of world manufacturing output in the estimation sample. The stacked bars show domestic and international consumption in the estimation sample as shares of manufacturing output reported by UNIDO. The dashed line denotes 100 percent coverage. Differences from 100 percent reflect variation in country coverage, sample restrictions, and valuation across the two data sources.

is strongly right-skewed: its mean is 915 million USD and its median 0.2 million USD, while about 29 percent of observations record zero trade. Political distance averages 0.8, approximately the US–Russia distance in 1995; it is zero for domestic flows, which account for less than one percent of observations. I use its mean within-pair standard deviation to assess trade effects (Appendix Figure A.5).

Political distance varies over time for virtually all international pairs, whereas RTAs and sanctions vary for only 25 and 40 percent of country pairs, respectively. Within-pair variation in political distance, combined with the large number of country pairs, allows the estimator to exploit changes within pairs rather than cross-sectional differences.

The estimation sample covers approximately 92 percent of UNIDO-reported world manufacturing output on average and 98 percent in 2020 (Figure 2). Domestic trade accounts for most of this coverage: across years, its aggregate value represents roughly 64–88 percent of reported world output. Including domestic flows captures substitution between domestic and foreign suppliers, the sourcing margin emphasized by Yotov (2022). The early-1970s coverage decline reflects Soviet output entering the UNIDO aggregate in 1971 without corresponding domestic flows in TradeProd and West Germany entering the UN sample only in 1973. For identical country-years, TradeProd totals closely track UNIDO output, averaging approximately 99 percent (Appendix Figures A.3 and A.4).

3 Results

This section introduces the estimation framework and estimates the overall relationship between political distance and trade in bilateral data. I then present robustness checks, assess the identifying assumptions using military-conflict and timing evidence, examine heterogeneity across geopolitical environments, and investigate channels.

3.1 Estimation framework

To estimate structural gravity equations, such as Equation 1, several studies established a set of best practices (see, for example, Baldwin and Taglioni 2007; Head and Mayer 2014; Yotov et al. 2016). Adopting these best practices yields the baseline for the empirical gravity equation:

$$\mathbb{E}(X_{ij,t}|\mathbf{Z}_{ijt}) = \exp\left(\pi_{i,t} + \pi_{j,t} + \pi_{i\neq j,t} + \pi_{ij} + \beta \,\text{political distance}_{ij,t}\right). \tag{3}$$

Here, $X_{ij,t}$ denotes the value of goods traded from exporter i to importer j in year t from TradeProd, including domestic flows ($i = j$) and reported zero flows, and $\mathbf{Z}_{ijt}$ contains conditioning information, that is, political distance and fixed effects. Fixed effects at the exporter-year and importer-year levels, $\pi_{i,t}$ and $\pi_{j,t}$, account for multilateral resistances (Anderson and Van Wincoop 2003; Fally 2015). Fixed effects for international-trade-by-year account for unobserved changes in international trade costs relative to domestic trade (Bergstrand et al. 2015; Larch and Yotov 2024), and (directed) pair fixed effects account for unobserved, time-constant trade costs.

The baseline intentionally does not condition on tariffs, trade agreements, or sanctions. These policies may transmit the demand- and cost-side consequences of political distance to trade. The baseline coefficient therefore captures their contribution to the overall relationship without conditioning on these potential policy channels. Subsequent specifications introduce these variables as potential (and endogenous) channels.

The coefficient is identified from changes in political distance within a country pair over time, net of contemporaneous exporter- and importer-specific shocks and common changes in international trade costs relative to domestic trade. A causal interpretation requires that the remaining variation in political distance be uncorrelated with other time-varying bilateral determinants of trade that are not themselves consequences of political distance. The subsequent subsections assess this assumption.

The estimation uses the Poisson pseudo-maximum likelihood (PPML) estimator (Silva and Tenreyro 2006; Correia et al. 2020), which is consistent in gravity models with three-way fixed effects (Weidner and Zylkin 2021). Standard errors are clustered at the undirected country-pair level, since political distance is undirected as well.

3.2 Baseline results

Table 2 reports the baseline result using the preferred quantitative Cohen's κ measure and two alternative agreement statistics.

Column (1) uses the preferred quantitative Cohen's κ measure defined in Equation (2). The coefficient is approximately -0.2 and statistically significant at the 1 percent level. An increase in political distance equal to its mean within-pair standard deviation is associated with a 3.3 percent decrease in bilateral trade.

Columns (2) and (3) instead use qualitative Cohen's κ and quantitative Scott's π. Because the measures have different scales, their raw coefficients are not directly comparable. Their standardized effects are nevertheless similar: an increase equal to the corresponding mean within-pair standard deviation is associated with 3.2 percent lower trade using qualitative Cohen's κ and 3.0 percent lower trade using Scott's π. The two Cohen's κ estimates are statistically significant at the 1 percent level, and the Scott's π estimate at the 5 percent level. Thus, the estimated magnitude is robust to alternative ways of measuring disagreement in UNGA voting. Appendix Table A.1 reports complementary estimates using alternative measures. Political distance constructed from alliance membership also shows a significant and negative association with trade. The table also compares the preferred measure with the ideal-point distance of Bailey et al. (2017) on a common sample. With the preferred international-trade-by-year fixed effects, a one-SD increase is associated with 3.2 percent lower trade for the preferred measure and 2.0 percent lower trade for ideal-point distance; the latter estimate is statistically insignificant. Omitting these fixed effects increases the magnitudes to 4.6 and 2.8 percent, respectively. In the joint specification, the preferred estimate remains essentially unchanged, while the ideal-point effect is small (-0.1 percent) and statistically insignificant.

Appendix Table A.3 examines alternative estimators, outcome transformations, and sampling intervals in specifications that condition on RTAs. The estimated one-SD effect remains negative and statistically significant when I use OLS transformations, restrict the sample to positive flows, or retain observations at three-, four-, or five-year intervals.

Appendix Table A.4 examines the role of domestic flows more directly. In the RTA-controlled specification in Panel (b), omitting international-trade-by-year fixed effects changes the estimated trade effect from -3.1 to -3.9 percent. Without policy controls (Panel a), using the same mean within-pair standard deviation across specifications, the estimated trade effect changes from -3.3 percent to -4.7 percent when international-trade-by-year fixed effects are omitted, but falls to -0.5 percent and becomes statistically insignificant when domestic flows are excluded. Such attenuation when domestic trade is omitted is also documented for other trade-cost variables (Dai et al. 2014; Larch and Yotov 2024). These authors attribute the larger estimates obtained with domestic flows to capturing substitution between domestic and foreign suppliers. Restricting domestic trade

Table 2: Baseline results

	(1)	(2)	(3)
Political distance			
Quantitative Cohen's κ	-0.221^{***}		
	(0.074)		
Qualitative Cohen's κ		-0.244^{***}	
		(0.082)	
Scott's π			-0.156^{**}
			(0.065)
Mean within-pair SD	0.153	0.134	0.193
One-SD effect on trade (%)	-3.33	-3.23	-2.97
Observations	1,017,000	1,017,000	1,017,000

Notes: The dependent variable is bilateral exports, including domestic and reported zero flows. Columns (1)–(3) use quantitative Cohen's κ, qualitative Cohen's κ, and Scott's π, respectively. All specifications use the same observations, are estimated by PPML, and include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors in parentheses are clustered by undirected country pair. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance, calculated separately for each measure on the common estimation sample. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

observations to those with all nine manufacturing sectors reported yields a one-SD effect of -2.5 percent, significant at the 1 percent level. The negative relationship therefore persists when domestic flows based on partial sector coverage are excluded.

The baseline result also survives excluding each country in turn: all 167 leave-one-country-out estimates remain negative and statistically significant at the five percent level. Holding the baseline mean within-pair standard deviation fixed, the implied trade effects range from -5.2 to -2.8 percent, compared with -3.3 percent in the full sample (Appendix Table A.5). Excluding the United States or Canada strengthens the negative effect, indicating that neither country drives the baseline finding.

The negative relationship is not confined to trade across geopolitical, institutional, or geographic divides (Appendix Table A.6). It is similar for pairs with and without an RTA and for pairs within and across broad geographic regions; neither equality test rejects. For geopolitical blocs, the relationship is negative and statistically significant within aligned blocs and for pairs involving non-aligned countries, whereas the estimate for pairs in opposing US- and China-aligned blocs is positive and insignificant. If anything, the point estimates suggest a stronger negative relationship within geopolitical blocs than across them, although the difference is significant only at the 10 percent level.

Overall, the results imply that an increase in political distance equal to its mean within-pair standard deviation is associated with approximately 3 percent lower trade. For the preferred estimate, the 95 percent confidence interval covers declines of 1.2 to 5.5 percent. Because the specifications include country-pair fixed effects, the coefficient is

identified from changes in political distance within a given pair rather than from persistent differences between politically close and distant pairs. The estimates should therefore be interpreted as a robust reduced-form relationship; the following subsection examines military conflict, cultural and religious distance, and the timing of the response to assess the identifying assumptions.

3.3 Identification

A causal interpretation faces two main threats: omitted time-varying bilateral factors affecting both political distance and trade, and reverse causality from trade to political alignment. The fixed effects absorb persistent bilateral characteristics and country-specific annual shocks.

I first examine observed bilateral confounders, specifically, militarized interstate disputes (MIDs) and cultural and religious distance. MIDs are extreme manifestations of bilateral political friction, but their direct physical and economic consequences could disproportionately drive the estimated relationship. This concern is particularly relevant because armed conflict can itself sharply reduce bilateral trade (Glick and Taylor 2010; Korovkin and Makarin 2023; Thoenig 2024), while trade in turn shapes the incentives for conflict (Martin et al. 2008). I therefore assess whether the results depend on these episodes in Panel (a) of Table 3. To identify MIDs, I use the MID5 dataset (Palmer et al. 2022), which covers the sample period through 2014. It reports observations (country pair and time) with any form of dispute, ranging from threats to use force to an open war. Restricting the sample to observations covered by MID5 has little effect on the estimates; see Column (1). A one-SD increase in political distance is associated with 3.3 percent lower trade, essentially the same magnitude as in the baseline. Column (2) controls for ongoing MIDs, which may constitute either an omitted variable or a channel through which political distance impacts trade. Compared to Column (1), the one-SD effect is essentially unchanged. The indicator for ongoing MIDs itself is not significant.

Despite their overall rarity, observations with MIDs may nonetheless have large leverage in the estimation. To assess this, I omit observations (country pair $\times$ year) during MIDs and in the two years before and after them, and re-estimate the baseline specification. The results are in Column (3), which excludes approximately 1.1 percent of the MID-coverage sample relative to Column (1); this difference includes the two-year buffers and any additional observations removed by PPML. While the one-SD impact is about 14 percent larger than the baseline, it is nonetheless well within the baseline confidence interval. Overall, there is little evidence that MIDs account for the estimated relationship.

Cultural and religious distance may influence both foreign-policy alignment and bilateral trade through trust and preferences. I therefore augment the baseline specification with the time-varying measures of cultural and religious distance from Pellegrino et al.

Table 3: Identification checks: observed bilateral confounders

(a) Militarized interstate disputes

	(1) MID coverage 1966–2014	(2) Control for ongoing MID	(3) Exclude MID episodes
Political distance	−0.217***	−0.218***	−0.249***
	(0.081)	(0.080)	(0.079)
Ongoing MID		0.011	
		(0.027)	
Mean within-pair SD	0.155	0.155	0.155
One-SD effect on trade (%)	−3.31	−3.32	−3.78
Ongoing MID observations	3,479	3,479	0
Observations	872,157	872,157	862,961

(b) Cultural and religious distance

	(1)	(2)	(3)	(4)
Political distance				
Preferred κ	−0.176**	−0.168**	−0.165**	−0.160**
	(0.079)	(0.080)	(0.077)	(0.079)
Additional controls				
Cultural distance		4.053*		3.596
		(2.219)		(2.197)
Religious distance			0.574*	0.530
			(0.335)	(0.340)
Mean within-pair SD	0.130	0.130	0.130	0.130
One-SD effect on trade (%)	−2.25	−2.16	−2.12	−2.05
Observations	368,111	368,111	368,111	368,111

Notes: PPML estimates of bilateral exports, including domestic and reported zero flows, using the preferred quantitative Cohen's κ measure. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects and omit policy controls. Standard errors in parentheses are clustered by undirected country pair. Panel (a): Column (1) restricts the baseline to MID coverage, 1966–2014; Column (2) adds an ongoing-opponent-MID indicator; Column (3) excludes opponent pairs during disputes and for two years before and after, symmetrically in both trade directions. Each column uses its own realized estimation sample to calculate s. Panel (b): all four columns use the same 368,111 observations from the baseline sample with cultural and religious distance available, covering 1984–2020. Column (1) re-estimates the baseline on this common sample; Columns (2) and (3) separately add cultural and religious distance; Column (4) includes both. The same s is used in all four columns. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

(2025). Whereas pair fixed effects absorb persistent bilateral differences, the controls account for within-pair variation in cultural and religious distance over time. Panel (b) of Table 3 compares the baseline with specifications adding cultural distance, religious distance, and both jointly. All columns use a common 1984–2020 sample with both measures observed and omit trade-policy controls. Adding these controls leaves the estimated effect of political distance essentially unchanged.

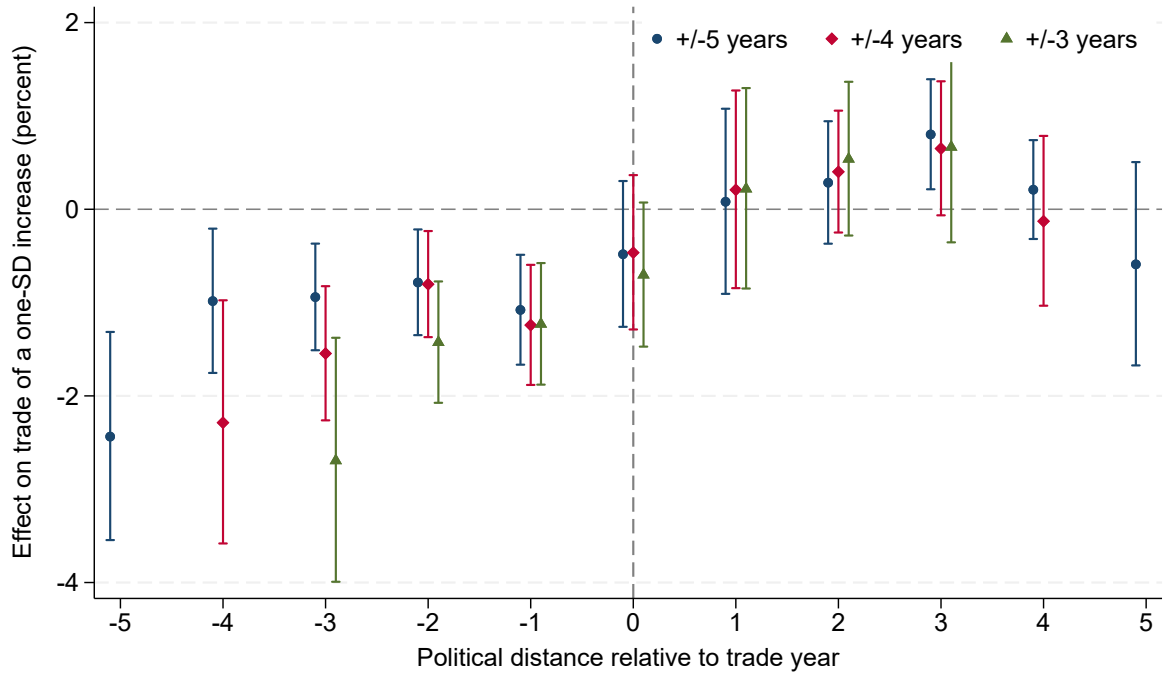
A reverse channel is economically plausible: political alignment may itself respond to economic relationships, as discussed in the literature on foreign aid (Alesina and Dollar 2000) and economic dependence (Kleinman et al. 2024). Simple specifications with lagged political distance retain a negative relationship (Appendix Table A.7), but cannot rule out persistent reverse causality or time-varying bilateral confounding. To examine whether changes in trade precede changes in political distance, I augment the baseline specification (3) with leads and lags of political distance, following Baier and Bergstrand (2007). The lags allow for the gradual adjustment documented for trade agreements (Baier et al. 2014; Anderson and Yotov 2016). This provides a Granger-style timing test (Wooldridge 2010). Conditional on current and lagged political distance, coefficients on its future values indicate whether current trade predicts subsequent changes in political distance. Negative lead coefficients would be consistent with trade declining before political distance increases. Such a pattern could reflect reverse causality, although anticipation, omitted dynamics, and measurement error could produce similar results.

All lead- and lag-augmented specifications are estimated on the sample where all five leads and lags are non-missing. Figure 3a reports specifications with five, four, and three leads and lags; Appendix Table A.8 also reports the shorter specifications. Across all three plotted specifications, past political distance is negatively associated with current trade, and the lag coefficients are jointly significant at the 1 percent level. The lead estimates do not show the negative pattern consistent with trade declining before political distance rises; individually significant leads are instead positive. Summed over the current and five preceding years, a one-SD increase in political distance is associated with 6.5 percent lower trade, compared with 3.2 percent in the static specification on the same sample.

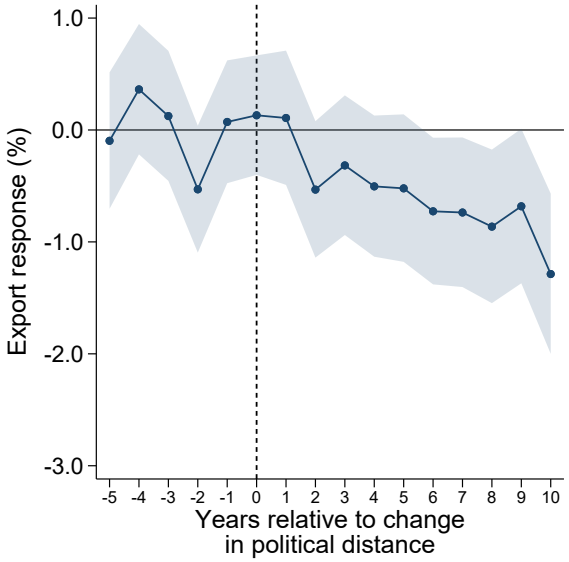
To assess the direction of the pre-period relationship formally, I test the sum of the lead coefficients against the one-sided alternative that it is negative. The sum is positive in all specifications, and none of these tests reject. This differs from testing whether all lead coefficients are jointly zero: that null is rejected at the 5 percent level in the five-year specification, but not in the other specifications. Despite their significance, the positive signs do not support the hypothesized negative pre-period relationship. Estimating the specification on three- and five-year intervals, which smooths transitory year-to-year fluctuations in voting, yields the same pattern: political distance in the previous interval is strongly negatively associated with trade, whereas the coefficient on the next interval is small, positive, and insignificant (Appendix Table A.9).

Figure 3: Timing of the relationship between political distance and trade

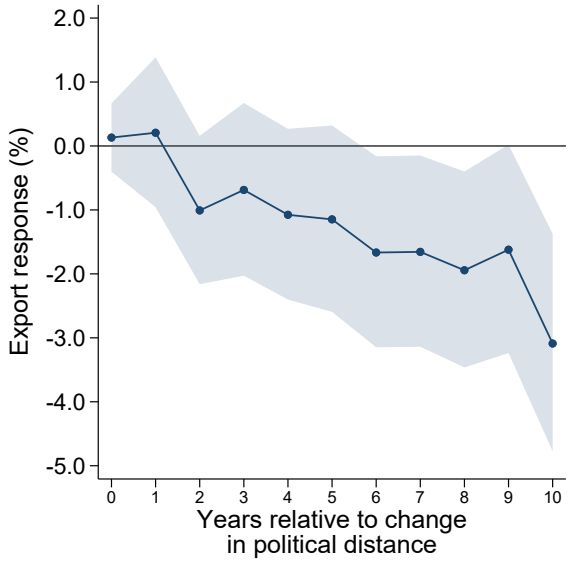
(a) Leads and lags in the gravity equation



(b) Local projection: impulse response



(c) Persistence-adjusted response



Notes: Panel (a) augments the baseline specification (3) with $K \in \{5, 4, 3\}$ leads and lags of political distance, estimated by PPML on the sample of the five-year specification. Panel (b) shows local projections of log exports on a one-year change in political distance at t: for $h \geq 0$ the cumulative change since $t-1$, for $h < 0$ the annual change in year $t+h$. Panel (c) divides the trade response by the response of political distance itself, using jointly estimated equations on a common sample at each horizon. All effects are expressed as the percentage change in trade for an increase in political distance equal to its mean within-pair standard deviation. Whiskers in panel (a) and shading in panels (b) and (c) show pointwise 95 percent confidence intervals based on standard errors clustered by undirected country pair; panel (c) uses the delta method with the covariance between the two responses. See Appendix Tables A.8, A.10, and A.11 for additional results.

For a more direct estimate of the impulse response of trade to political distance, I estimate complementary local projections (Jordà 2005; Boehm et al. 2023). For each horizon $h \geq 0$, I regress the change in log exports from $t-1$ to $t+h$ on the one-year change in political distance at t, controlling for its change at $t-1$ and including horizon-specific exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. For $h < 0$, the outcome is the annual change in log exports in year $t+h$, so these coefficients test whether trade moves before political distance changes. Figure 3b shows no systematic movement in exports before the change in political distance and a gradual decline afterwards, of about 0.5 percent after five years and 1.3 percent after ten years.

These responses are smaller than the gravity estimates because they trace the response to a one-year change in political distance, much of which is transitory. Estimating the same local projection with political distance as the outcome shows that about half of a one-year change persists after one year and about 40 percent after ten years (Appendix Figure A.7), consistent with transitory year-to-year fluctuations in voting. Dividing the trade response at each horizon by this persistence gives a persistence-adjusted response, analogous to the horizon-specific elasticities of Boehm et al. (2023). The trade and political-distance responses are estimated jointly on the same observations at each horizon, and the delta-method confidence intervals account for their covariance. Figure 3c shows little immediate response and a larger negative response at longer horizons, reaching -3.1 percent after ten years (95 percent confidence interval: -4.8 to -1.4; Appendix Table A.11). The corresponding fixed-sample estimate is -2.2 percent (-3.9 to -0.5; Appendix Figure A.8).

These magnitudes are of the same order as the gravity estimates: on the sample of Figure 3a, the static effect is -3.2 percent and the cumulative effect over the current and five preceding years is -6.5 percent. The estimates are not directly comparable because the local projections are estimated by OLS on log exports of positive flows, whereas the gravity estimates use PPML and include zero flows. For IHS-transformed exports, the persistence-adjusted ten-year response is smaller but remains negative and statistically significant: -1.2 percent (-1.7 to -0.7) with horizon-specific samples and -1.2 percent (-1.8 to -0.7) with a fixed sample. Interpreting the ratio as the effect of a lasting increase requires the additional assumption that the trade response scales with the persistence of political distance.

The impulse responses are similar for IHS-transformed exports when the analysis is restricted to international trade, and when every horizon is estimated using only observations for which the transformed outcome is observed throughout the window from six years before to ten years after the change in political distance (Appendix Figure A.6). Across all eight outcome, trade-sample, and horizon-sample combinations, the pre-period coefficients are jointly insignificant at the 10 percent level, while the post-period coefficients are jointly significant at the 5 percent level (Appendix Table A.10). Overall, the results

indicate no robust evidence that trade declines beforehand, while higher political distance consistently predicts subsequent declines.

The relationship is robust to the observed bilateral confounders considered here, and higher political distance consistently predicts subsequent trade declines. The timing exercises provide no robust evidence of prior declines, although some lead coefficients are positive and significant. Together, these findings strengthen a causal interpretation without establishing it conclusively.

Having assessed identification of the pooled relationship, I next examine how its magnitude varies across geopolitical environments before turning to the underlying mechanisms.

3.4 Variation across geopolitical environments

The pooled coefficient of approximately -0.2 may conceal substantial heterogeneity across geopolitical environments. To investigate this variation, I allow the coefficient for political distance to vary annually.

Figure 4 shows a pronounced late-Cold-War peak in the negative relationship. In the unrestricted sample, the coefficient reaches approximately -0.7 in 1986–1989. It moves toward zero during the 1990s and is statistically insignificant during 1995–2007 before becoming more negative again, reaching about -0.4 in 2016 and remaining between roughly -0.3 and -0.4 thereafter. The recent strengthening thus brings the relationship back to about half of its late-Cold-War magnitude.

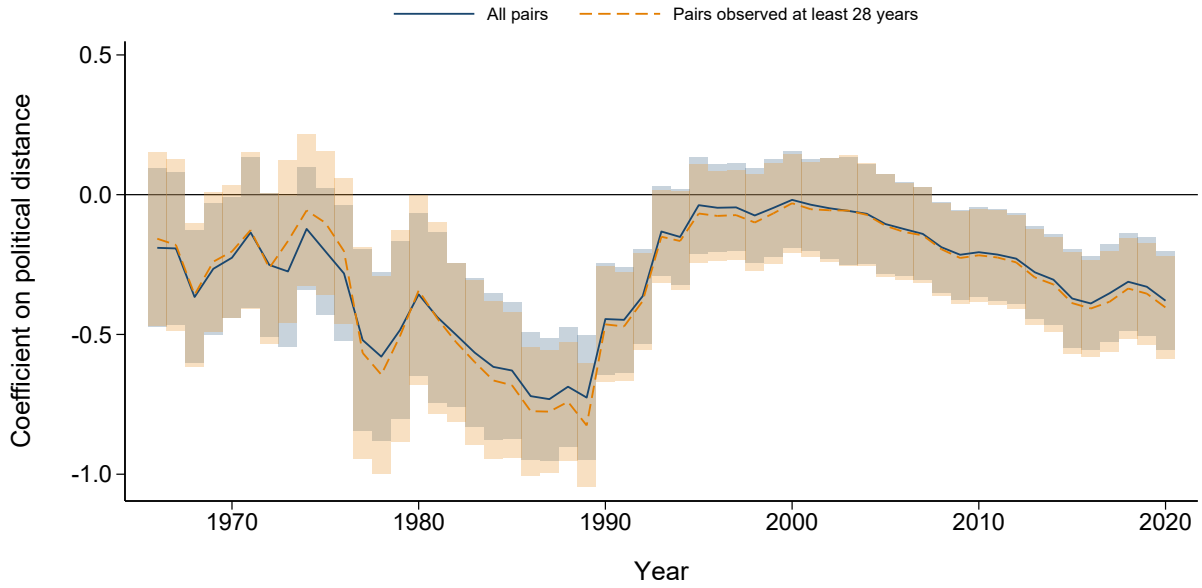
The average annual coefficient during 1983–1990 is -0.640, compared with -0.352 during 2016–2020. The difference is statistically significant at the 1 percent level ($p = 0.007$). Thus, the stronger late-Cold-War relationship in the unrestricted sample is also supported by a comparison of period averages.

To assess robustness to short panels, I exclude country pairs observed for fewer than 28 years, the 25th percentile of pair-level coverage in the unrestricted sample (Appendix Figure A.2b). This retains about 88 percent of the unrestricted observations. The annual estimates in Figure 4 closely track the unrestricted series, preserving the late-Cold-War peak, the weak relationship during 1995–2007, and renewed strengthening after 2007. This suggests that country pairs with short observation histories do not drive the annual pattern.

Scaling the unrestricted annual coefficients by each year's dispersion of political distance preserves the late-Cold-War peak but narrows the gap to recent years: a one-standard-deviation increase is associated with 14.8 percent lower trade in 1987 and 11.7 percent lower trade in 2016 (Appendix Figure A.9).

The recent strengthening is consistent with Bosone and Stamato (2024), who estimate increasingly negative episode-specific relationships between UNGA-based political distance and manufacturing trade over 2012–2022. In their preferred specification excluding energy,

Figure 4: The relationship between political distance and trade over time



Notes: Annual coefficients on political distance from PPML regressions for the unrestricted sample ("All pairs"; 1,017,000 observations) and pairs observed for at least 28 years (893,317 observations after PPML exclusions). The cutoff is the unweighted 25th percentile of years observed across directed country pairs in the unrestricted 1966–2020 sample, including domestic pairs; all cutoff ties are retained. Both specifications include domestic and zero trade flows and exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Filled bars show pointwise 95 percent confidence intervals with standard errors clustered by undirected country pair. Coefficients are per fixed one-unit increase in political distance.

the relationship is insignificant during 2012–2017, turns negative and significant at the ten percent level in 2018–2020, and becomes more negative and precisely estimated in 2021–2022. My recent annual estimates therefore accord with their evidence that political distance has become a more important trade friction in recent years, although my sample ends before their final episode. The coefficient magnitudes are not directly comparable: they use logged ideal-point distance and condition on tariffs and RTAs, whereas this exercise uses the level of the chance-corrected distance measure without those policy controls. The longer historical perspective places this recent strengthening alongside the substantial trade responsiveness observed during the late Cold War.

The delayed trade response also motivates examining the underlying mechanisms. Evidence that bilateral trust and public opinion shape trade, and that politically motivated boycotts can generate sharp responses, points to a role for demand (Disdier and Mayer 2007; Guiso et al. 2009; Michaels and Zhi 2010; Fuchs and Klann 2013; Pandya and Venkatesan 2016; Heilmann 2016). Gradual adjustment is also consistent with changes in firms' trading relationships. Timing alone cannot distinguish these mechanisms, since consumer responses can persist and trade costs can affect trade immediately. The next subsection therefore examines complementary sectoral, policy, and topic-specific evidence.

3.5 Channels

The previous sections documented a statistically significant negative relationship between political distance and trade. Recall from the conceptual framework that the reduced-form coefficient β captures the combined demand- and cost-side consequences of observed political distance for trade. Distinguishing these channels matters because interpreting counterfactual real-income changes as welfare changes requires a trade-cost interpretation. Sectoral variation offers limited guidance, so I complement it with policy controls and topic-specific measures of political distance. Overall, the evidence is more consistent with the trade-cost channel.

I first examine whether the aggregate relationship is concentrated in particular manufacturing sectors. Coefficients are negative in all nine TradeProd sectors and differ significantly, ranging from approximately -0.4 to -0.1, with the most negative estimates for food and machines and the least negative for wood and paper (Appendix Table A.12). The negative relationship thus extends across a broad range of goods. With only nine broad sectors, however, these differences offer limited scope for distinguishing demand shifts from trade costs. I therefore turn to policy channels and the substance of political disagreement.

Under the trade-cost channel, political distance may affect trade through trade policy, particularly regional trade agreements and sanctions. I augment the baseline specification with these variables, separately and jointly. Because these policies may themselves respond to political distance, coefficient changes are interpreted as descriptive attenuation rather than causal mediation shares. The results are in Table 4.

The first column reports the specification without these potential channels. Controlling for RTAs in Column (2) attenuates the one-SD effect from -3.3 to approximately -3.1 percent, or by about 6 percent. Controlling for sanctions in Column (3) reduces it to -2.3 percent, an attenuation of about 32 percent. With both controls in Column (4), the effect is -2.1 percent. These changes are consistent with sanctions accounting for an economically meaningful part of the relationship.

Tariff data begin only in 1988, so Appendix Table A.13 repeats the exercise on a common 1988–2020 sample. The political-distance estimate is smaller and statistically imprecise even before adding policy controls: its one-SD effect is -1.5 percent. Because this baseline already differs from the full-sample estimate, the exercise assesses attenuation within the shorter sample rather than explaining the cross-sample difference. Adding tariffs alone changes the estimate only modestly, to -1.4 percent, whereas adding sanctions reduces it to -0.6 percent; with tariffs, RTAs, and sanctions together, it is also -0.5 percent. The tariff coefficient itself is negative and precisely estimated. Within this sample, tariffs therefore have little influence on the political-distance point estimate, while sanctions produce a more pronounced attenuation.

Table 4: Classical trade-policy channels

	(1)	(2)	(3)	(4)
Political distance				
Preferred κ	-0.221^{***}	-0.206^{***}	-0.149^{**}	-0.138^{*}
	(0.074)	(0.068)	(0.075)	(0.070)
Potential channels				
Any RTA		0.132^{***}		0.126^{***}
		(0.037)		(0.038)
Any GSDB sanction			-0.127^{***}	-0.123^{***}
			(0.024)	(0.023)
Mean within-pair SD	0.153	0.153	0.153	0.153
One-SD effect on trade (%)	-3.33	-3.12	-2.26	-2.09
Observations	1,017,000	1,017,000	1,017,000	1,017,000

Notes: All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair, and all columns use the same sample. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance, calculated on the common estimation sample and held fixed across columns. RTAs and sanctions are potentially endogenous policy channels. Changes in the political-distance coefficients are therefore descriptive attenuation exercises rather than causal mediation estimates. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

I next exploit the differences in the topics of UNGA resolutions from which I compute political distance to distinguish between the mechanisms through which political distance may affect trade. Disagreement over human rights is especially likely to influence consumers' willingness to purchase goods from politically dissimilar countries and is therefore more closely associated with a demand-shifter channel. By contrast, disagreement over security issues is more likely to affect the institutional and political conditions under which bilateral trade takes place—including enforcement, regulatory scrutiny, financing, and the risk of future restrictions—and is therefore more closely associated with a trade-cost channel.

Table 5 accounts for classical trade-policy channels and uses the topic-specific measures of political distance. Some country pairs are not jointly observed voting on specific topics in a small number of years. Hence, the number of available observations is lower compared to the baseline results and the results on classical trade-policy channels. All four columns use a common sample and condition on RTAs, sanctions, and fixed effects. Columns (1)–(3) use security, human-rights, and economic distance separately; Column (4) includes all three topic distances jointly.

Security distance in Column (1) has the largest one-SD effect, associated with approximately 2.7 percent lower trade, and is significant at the 1 percent level. Human-rights distance in Column (2) is negative and statistically insignificant. Economic distance in Column (3) is associated with 1.8 percent lower trade and is significant at the 5 percent level.

Table 5: Political distance for specific topics

	(1)	(2)	(3)	(4)
Political distance				
Security	−0.134***			−0.128***
	(0.038)			(0.038)
Human rights		−0.005		0.054
		(0.051)		(0.053)
Economic			−0.074**	−0.040
			(0.029)	(0.029)
Potential channels				
RTA	0.125***	0.127***	0.126***	0.127***
	(0.038)	(0.038)	(0.038)	(0.038)
Any GSDB sanction	−0.116***	−0.137***	−0.125***	−0.117***
	(0.024)	(0.024)	(0.024)	(0.024)
Mean within-pair SD	0.201	0.230	0.242	
One-SD effect on trade (%)	−2.65	−0.12	−1.78	
Observations	949,783	949,783	949,783	949,783

Notes: Results are for aggregate manufacturing trade from country i to country j in year t, including domestic trade and reported zero trade flows. Political distance is calculated separately from UNGA resolutions classified as concerning security, human rights, or economic issues. The RTA indicator equals one if any economic integration agreement is in force; the sanctions indicator equals one if the country pair is subject to at least one sanction. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance, calculated separately for each topic-specific measure on the common estimation sample. Column (4) includes all three topic distances, so no single SD or one-SD effect is reported. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses below the point estimate. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

In the joint specification, security distance remains negative and significant at the 1 percent level, while the other topic coefficients are insignificant. The three topic coefficients are jointly significant ($p < 0.001$). The standardized security-distance effect differs from the human-rights effect ($p = 0.012$), but not from the economic-distance effect ($p = 0.178$). The human-rights and economic effects do not differ significantly ($p = 0.135$).

Appendix Table A.14 shows that sanctions attenuate the security-distance estimate, but the relationship remains negative and significant when sanctioned observations are excluded. An interaction specification does not reject equal coefficients across sanctions statuses.

Taken together, the channel exercises point to political frictions affecting the conditions under which firms trade. The relationship extends across manufacturing sectors, is attenuated by sanctions controls, and is strongest for disagreement over security issues even after conditioning on observed trade policies. This combination is more consistent with a role for trade costs than with an explanation based predominantly on consumer

demand. It does not, however, identify a unique mechanism: security disputes may also affect consumers, human-rights disagreements may induce restrictions, and the policy controls do not measure all costs of trading. Nor can coefficient attenuation be interpreted as a causal decomposition.

This distinction matters for the quantitative exercise that follows. Treating the estimated relationship as a change in trade costs permits a welfare comparison under unchanged preferences. The channel evidence supports this interpretation without establishing that the entire response operates through costs. The counterfactual welfare results should therefore be read as conditional on that interpretation, rather than as a welfare accounting of every possible consequence of political disagreement.

4 A counterfactual New Cold War

The previous section quantified the negative relationship between political distance and bilateral trade. Using the model introduced in Section 2.1, I assess the general-equilibrium welfare consequences of counterfactual political realignment under the trade-cost interpretation supported by the preceding evidence.

Specifically, I consider a *New Cold War*, inspired by Gopinath et al. (2025), who show that recent trends in trade fragmentation resemble those of the early Cold War. I impose the bilateral political distances observed in 1962, the year of the Cuban Missile Crisis, on the 2020 economy, approximating historical distances where they are not directly observable. This realignment brings some countries politically closer and pushes others farther apart, both within and across geopolitical blocs.

I translate these changes in political distance into a composite bilateral trade-friction shock using the estimated responsiveness for 2016–2020 and 1983–1990 from the same gravity regression without tariff, RTA, or sanctions controls. Holding the 2020 economy, political-distance shock, and trade elasticity fixed isolates the implications of historical differences in responsiveness.

4.1 Counterfactual political distances

The starting point is the 2020 cross-section of UNGA members for which trade data are available, supplemented by a rest-of-world aggregate. The resulting square matrix contains 158 economies and more than 24,000 ordered pairs. Political distances involving the rest-of-world aggregate remain unchanged.

Table 6 summarizes the hierarchical assignment rules and their coverage in the 2020 trade matrix. Observed 1962 distances are used where available; otherwise, the rules use Soviet or legal-predecessor relationships and the historical alignments of modern-day political neighbors. Domestic distances and distances involving the rest-of-world aggregate

Table 6: Construction of counterfactual political distances

Rule	Pair category	Assigned distance	Share (%)
–	Rest-of-world pairs	Unchanged	1.3
–	Domestic pairs	Zero distance; unchanged	0.6
1	Two former Soviet republics	Zero distance	0.8
2	One former Soviet republic	1962 Soviet distance to the other partner	11.1
3	Two observed UN members	Observed 1962 bilateral distance	33.5
4	Legal predecessors	1962 distance using legal predecessors	1.6
5	One historical non-member	Historical distances of ten modern-day neighbors	36.1
6	Two historical non-members	Historical distances between both sets of neighbors	15.0
Fallback	Unassigned historical distance	Unchanged; zero-shock fallback	< 0.1
	All pairs		100.0

Notes: Mutually exclusive assignment categories in the 2020 TPc trade matrix. Shares refer to all 24,964 ordered pairs, including domestic observations and the rest-of-world aggregate. After leaving rest-of-world and domestic pairs unchanged, historical assignment rules are applied in the numbered order: the first applicable rule determines the assignment. Unassigned pairs receive zero shocks as a final fallback. The six unassigned international pairs link India, Iran, and the Philippines in both directions and receive zero shocks. Shares are rounded.

remain unchanged. Six directed pairs without an assigned historical distance also receive no shock, consistent with the solver's treatment of missing shocks.

I define the counterfactual change in political distance relative to 2020 as

$$\Delta_{ij}^{cf} = \text{political distance}_{ij,2020}^{cf} - \text{political distance}_{ij,2020}.$$

Across international pairs excluding the rest-of-world aggregate, political distance increases by 0.049 units on average, approximately 6 percent of its mean in 2020. This net increase masks changes in both directions: political distance rises for about 55 percent of pairs and falls for about 45 percent. The median absolute change is 0.218 units. Appendix Figure A.10 shows the full distribution.

4.2 General equilibrium effects

The change in trade costs implied by the counterfactual political distances is given by

$$\hat{\tau}_{ij}^{-\theta} = \exp\left(\beta^{cf} \cdot \Delta_{ij}^{cf}\right),$$

where $\theta = \sigma - 1$ is the trade elasticity implied by the CES preferences in Section 2.1. As Figure 4 shows, there is sizable heterogeneity across time in trade responsiveness to political distance. For a like-for-like comparison, I estimate a gravity regression over 1966–2020 with episode-specific political-distance slopes and no tariff, RTA, or sanctions controls. It retains the baseline sample, domestic trade, and the same fixed effects, with standard errors clustered by undirected country pair. The 2016–2020 coefficient is

approximately -0.35, compared with -0.64 for 1983–1990.

Holding other gravity terms fixed, the implied percentage change in bilateral trade is $100[\exp(\beta^{cf}\Delta_{ij}^{cf})-1]$. Using 2016–2020 responsiveness, this change averages approximately -0.8 percent across international pairs excluding the rest-of-world aggregate, giving each pair equal weight. The realignment therefore generates offsetting bilateral trade gains and losses even before general-equilibrium adjustment.

As a salient illustration, the US–Russia assignment raises political distance from 1.249 to 1.599, an increase of 0.350 units, or approximately 28 percent. At recent responsiveness, this implies an 11.6 percent decline in bilateral trade before general-equilibrium adjustment. This distance change is larger than the median absolute change but lies below the upper quartile.

I estimate the trade elasticity separately from a regression that includes log gross tariffs from Teti (2024) alongside episode-specific political-distance slopes, retaining the gravity fixed effects and omitting RTA and sanctions controls. This regression uses the unweighted bilateral tariff, excludes missing tariffs, and retains zero domestic tariffs within the coverage of the source. The tariff coefficient implies a trade elasticity of approximately 3.04, which I use for both political-distance coefficients.

The partial effect on trade is negative for countries that move further apart in the counterfactual ($\Delta_{ij}^{cf} > 0$). To account for general equilibrium effects, I use the `ge_gravity` procedure of Baier et al. (2019), which solves the one-sector Armington–CES model introduced in Section 2.1, holding preferences fixed and representing political realignment through trade costs.[5] The economy is calibrated to manufacturing trade and production, so the results describe real-income changes within this one-sector model. I hold each economy's nominal trade imbalance, $D_i = E_i - Y_i$, fixed, following the solver's default closure, with world nominal output as the numeraire. Real-income changes are $\widehat{W}_i = \widehat{E}_i/\widehat{P}_i = (\widehat{E}_i/\widehat{Y}_i)\widehat{\lambda}_{ii}^{-1/\theta}$, where λ_{ii} is the domestic expenditure share and hats denote counterfactual-to-baseline ratios. When expenditure and income change proportionally, this reduces to the domestic-share formula of Arkolakis et al. (2012); with fixed nominal trade imbalances, the expenditure-to-income adjustment also matters. I solve each counterfactual at the parameter point estimates and report these outcomes throughout. To quantify parameter uncertainty, I use a parametric bootstrap with 5 000 draws per specification from the estimated normal distributions of the political-distance coefficient and trade elasticity, approximating their cross-regression covariance as zero.[6] For each

[5] In the tariff-conditioned specification, using alternative trade elasticities of 4 or 10 (Eaton and Kortum 2002; Simonovska and Waugh 2014) preserves the regional pattern of gains and losses. Holding the political-distance coefficient at its point estimate, the unweighted mean absolute real-income change across countries is about 24 and 69 percent lower, respectively, than with the estimated trade elasticity.

[6] The political-distance coefficients and trade elasticity come from separate regressions. Their cross-regression covariance has not been estimated. The recent episode overlaps the tariff sample, while the Cold War episode overlaps it only in 1988–1990. Drawing independently is an approximation, not a claim of exact sampling independence.

Table 7: Welfare effects of a New Cold War

		2016–2020 responsiveness				1983–1990 responsiveness			
Country group	N	Mean	Median	SD	Losing	Mean	Median	SD	Losing
Americas	29	0.72	1.06	2.53	20.69	1.48	2.09	4.82	17.24
Former Warsaw Pact	22	−1.81	−1.52	2.45	72.73	−2.49	−1.75	4.14	63.64
Other Europe	25	−1.56	−1.22	1.47	96.00	−2.54	−1.88	2.53	92.00
Rest of the world	82	−0.36	−0.37	0.79	76.83	−0.58	−0.64	1.42	76.83
All countries	158	−0.55	−0.36	1.82	68.99	−0.78	−0.63	3.21	66.46

Notes: Welfare changes are in percent; SD is in percentage points. Statistics are unweighted across countries, including the rest-of-world aggregate. Losing is the percentage of countries with welfare losses. All means differ from zero at the 5% level under the parameter-uncertainty calculation described in the text. Appendix Table A.15 reports the 95% confidence intervals.

draw, I solve the model and calculate regional means; their 2.5th and 97.5th percentiles give 95 percent confidence intervals conditional on the model, baseline data, and counterfactual.

A third scenario converts the Cold War one-standard-deviation effect into 2020 units by multiplying its coefficient by the ratio of the mean annual political-distance SD in 1983–1990 to the SD in 2020, measured across international pairs in the estimation sample. The SD-scaled, alternative-elasticity, and sanctions-controlled scenarios are evaluated only at point estimates.

Table 7 compares the welfare effects of the same political realignment using recent and late-Cold-War trade responsiveness. With 2016–2020 responsiveness, real income falls in about 69 percent of countries. The unweighted mean welfare loss is 0.55 percent, the median change is −0.36 percent, and the cross-country standard deviation is 1.82 percentage points. Losses are concentrated in Europe: real income falls by 1.8 percent on average in the former Warsaw Pact and by 1.6 percent in other European countries, where all countries except Ireland lose. The Americas are the only region with an average gain, at 0.7 percent, although one in five countries there loses. The rest of the world loses 0.4 percent on average.

Using 1983–1990 responsiveness while holding all other counterfactual inputs fixed increases the mean welfare loss to 0.78 percent, about 41 percent larger than under recent responsiveness. The median change falls to −0.63 percent and the cross-country standard deviation rises to 3.21 percentage points; about 66 percent of countries lose. The regional pattern persists with larger gains and losses: the average gain in the Americas roughly doubles to 1.5 percent, while average losses in the former Warsaw Pact and other European countries reach 2.5 percent each. Historical differences in trade responsiveness therefore affect both the average cost and the distribution of welfare effects. The larger effects persist, somewhat attenuated, after scaling the historical coefficient to account for the change in political-distance dispersion (average change −0.7 percent; Appendix

Table A.15).

For the tariff-conditioned 2016–2020 specification, Figure A.11 in the appendix shows the geographic distribution of welfare changes. Controlling additionally for sanctions reduces the magnitude and cross-country dispersion of real-income changes while preserving the regional pattern of gains and losses (Appendix Table A.16).

5 Conclusion

This paper provides evidence on how trade responds to political distance across geopolitical environments. Using a structural-gravity framework with domestic and international trade for 167 countries over 1966–2020, I find that an increase in UNGA voting disagreement equal to its mean within-pair standard deviation is associated with approximately 3 percent lower trade. In the unrestricted sample, responsiveness is strongest during the late Cold War, weakens during the intervening period of globalization, and subsequently strengthens again without returning to its historical peak. Policy and topic-specific evidence favors a trade-cost interpretation, although it does not separately identify causal channels.

This historical variation matters for the welfare consequences of political realignment. Imposing 1962 political alignments on the 2020 economy allows bilateral frictions to rise or fall and produces uneven regional gains and losses. Under a trade-cost interpretation, the model calibrated to manufacturing trade and production implies welfare losses for about 70 percent of countries, while the Americas gain on average. In a separate comparison holding the estimation specification fixed, evaluating the same realignment with late-Cold-War responsiveness amplifies both gains and losses and increases the average welfare loss by approximately 40 percent. Estimates from recent years can therefore provide an incomplete guide to the consequences of renewed political fragmentation.

Political distance varies over time for virtually all country pairs, whereas regional trade agreements and sanctions change for only a minority. Understanding how political frictions affect trade therefore requires attention to mechanisms beyond changes in these formal policies. A fruitful direction for future research is to examine whether political disagreement alters contracting, financing, regulatory treatment, or sourcing decisions even when formal trade policy remains unchanged. Such evidence would help policymakers assess how far stable trade agreements can sustain economic integration amid political divergence.

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Online Appendix –
Political Distance and Trade

Knut Niemann

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A.1 United Nations Voting Data and the Construction of Political Distance

This appendix documents how I construct the country-pair–year measures of political distance used in the paper. The underlying country–resolution records come from the General Assembly Voting Data compiled and maintained by the United Nations Dag Hammarskjöld Library (United Nations 2026, version 5, released 6 February 2026, UN Digital Library record 4060887).

A.1.1 Source and scope

This release contains 947,434 member-state–decision observations for 5,694 recorded decisions, from resolution 1 (11 December 1946) through resolution 80/246 (30 December 2025). Each row records a member state's vote and provides the UNDL control number and link, the resolution symbol and date, session and meeting identifiers, the title, agenda title, subjects, and reported vote totals.

The UN file covers adopted General Assembly resolutions decided by recorded vote in regular, special, and emergency special sessions. It excludes votes on individual paragraphs, amendments, and draft resolutions that failed to be adopted. Decisions adopted without a recorded vote are therefore also outside the data. These source-defined restrictions yield the final-passage concept used throughout the paper and in established UNGA voting data.

I exclude the single non-resolution decision, A/DEC/80/506, leaving 947,241 member-state–resolution observations for 5,693 resolutions. The official file extends beyond the estimation sample. I retain its full historical coverage when constructing the political-distance panel; the trade regressions use 1966–2020 because bilateral and domestic expenditure data determine the estimation period.

A.1.2 Cleaning and vote coding

The source identifies member states by a three-letter code and records the choice as yes (`Y`), no (`N`), abstention (`A`), or non-voting (`X`). I harmonize the source codes with the country identifiers used in the trade data and extract the calendar year from the vote date. The recorded choice is coded

$$x_{ir} = \begin{cases} 1 & \text{yes,} \\ 2 & \text{abstain or absent/non-voting,} \\ 3 & \text{no,} \end{cases} \tag{A.1}$$

where i indexes countries and r resolutions. Treating abstention and non-voting as the intermediate category follows the valued-vote approach and preserves the ordering from

support to opposition. The original four-way vote variable is retained in the intermediate data, so abstention and non-voting remain distinguishable. A country pair's calculation uses only resolutions for which both countries have a recorded source row and a valid coded choice.

A.1.3 Construction of political distance

Let $\mathcal{R}_{ijt}$ denote the resolutions in year t on which both countries i and j have a valid coded choice, and let n_{ijt} be the number of such resolutions. All statistics are calculated only on this common set. The simplest similarity measure is the observed agreement share,

$$A_{ijt} = \frac{1}{n_{ijt}} \sum_{r \in \mathcal{R}_{ijt}} \mathbf{1}\{x_{ir} = x_{jr}\}. \tag{A.2}$$

The paper's main measure uses quantitative Cohen's κ for ordered, valued votes. Write $\bar{x}_i$ and $\bar{x}_j$ for the two countries' mean vote codes over $\mathcal{R}_{ijt}$. I calculate

$$\kappa_{ijt} = 1 - \frac{\sum_r (x_{ir} - x_{jr})^2}{\sum_r (x_{ir} - \bar{x}_i)^2 + \sum_r (x_{jr} - \bar{x}_j)^2 + n_{ijt}(\bar{x}_i - \bar{x}_j)^2}. \tag{A.3}$$

This statistic adjusts observed disagreement for the countries' marginal voting tendencies while using the ordered distance between vote categories. For robustness, I also report quantitative Scott's π,

$$\pi_{ijt} = 1 - \frac{\sum_r (x_{ir} - x_{jr})^2}{\sum_r (x_{ir} - \bar{x})^2 + \sum_r (x_{jr} - \bar{x})^2}, \qquad \bar{x} = \frac{\bar{x}_i + \bar{x}_j}{2}, \tag{A.4}$$

and conventional qualitative Cohen's κ, which uses category matches rather than the numerical distance between categories. If D_o is the observed probability of disagreement and D_e is the disagreement probability implied by the two countries' marginal vote distributions, qualitative κ is $1 - D_o/D_e$.

For every similarity statistic $S \in \{A, \kappa, \pi, \kappa^q\}$, political distance is

$$d^S_{ijt} = 1 - S_{ijt}. \tag{A.5}$$

The paper's main measure is d^κ_{ijt}. Zero denotes identical voting on the common resolutions. Unlike the raw disagreement share, chance-corrected distances need not lie in the unit interval: negative agreement coefficients imply distances above one, with a theoretical upper bound of two.

The constructed panel supplies both i, j and j, i observations to facilitate merges with directed trade flows. Quantitative κ is left undefined when the common votes contain insufficient variation to calculate its denominator.

A.1.4 Validation

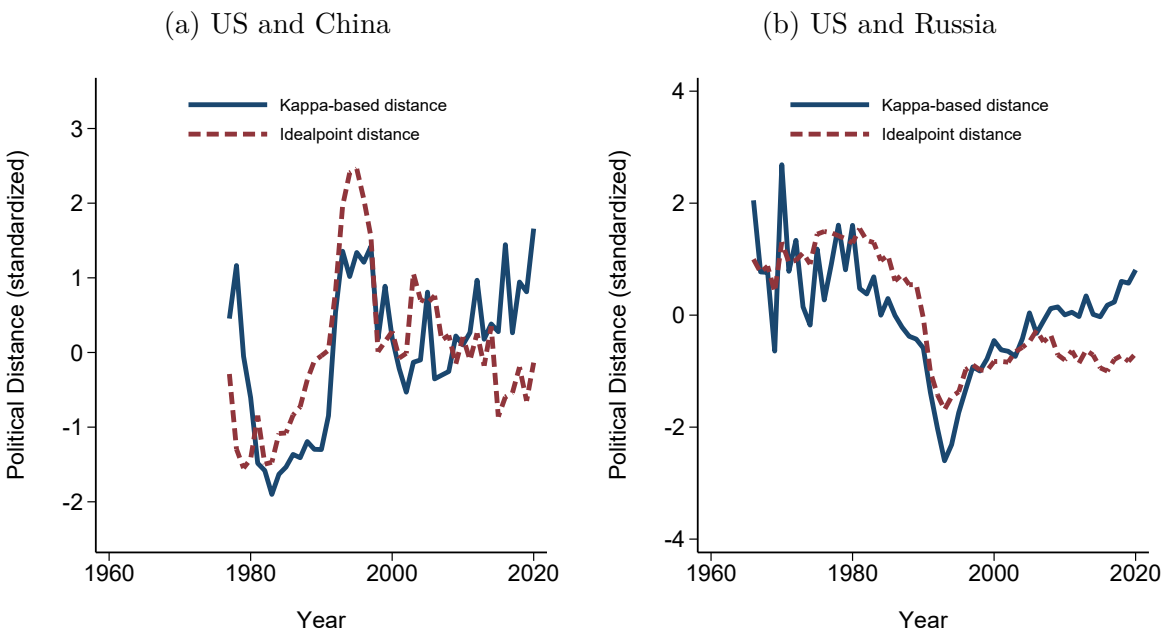
To validate the measure, I compare it with the Foreign Policy Similarity (FPSIM) data of Häge (2017) for the overlapping sample. Over the overlapping years of the estimation period (1966–2015), the preferred distance is strongly correlated with the corresponding FPSIM measure based on quantitative κ. The correlation is particularly close to one from 1991 onward and lower before, when the official records used here and the voting data underlying FPSIM differ more. This comparison validates the underlying voting records and the implementation of the measure; the historical patterns in Section 2 provide complementary evidence on its substantive interpretation.

A.1.5 Interpretation and limitations

UNGA voting similarity is a revealed-alignment measure, not a complete measure of bilateral relations. It gives equal weight to retained resolutions and reflects the General Assembly's changing agenda. Strategic abstention, non-voting, bloc voting, and the increasing membership of the United Nations can all affect the series. Chance correction addresses countries' marginal vote tendencies but does not remove changes in issue composition. The constructed data therefore retain common-resolution counts and alternative measures, permitting minimum-count rules and issue-specific distances. The UN source also provides titles, agenda titles, and subject tags that I use to form the topic-specific samples discussed in the paper.

A.2 Additional data and measurement results

Figure A.1: Measures of political distance



Notes: Both panels show standardized values of two measures of political distance between the US and China (Panel a) and the US and Russia (Panel b). The κ-based distance is the measure of political distance used in the paper, one minus quantitative Cohen's κ (Equation (2)). Ideal-point distance is from Bailey et al. (2017).

Table A.1: Alternative measures of political distance

(a) Alliance membership

	(1)	(2)	(3)
Political distance			
Preferred κ (UNGA)	-0.227^{***}		
	(0.077)		
Cohen's κ (alliances)		-0.470^{***}	
		(0.097)	
Scott's π (alliances)			-0.446^{***}
			(0.095)
Mean within-pair SD	0.155	0.025	0.025
One-SD effect on trade (%)	−3.45	−1.17	−1.13
Observations	822,392	822,392	822,392

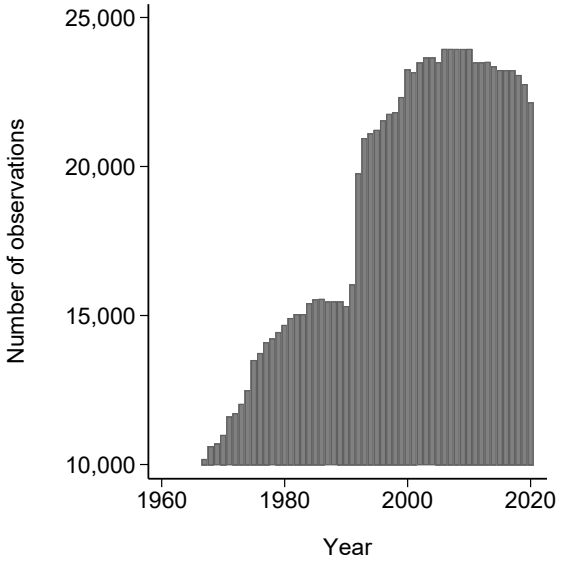
(b) Ideal-point distance

	(1)	(2)	(3)	(4)	(5)
Political distance					
Preferred κ (UNGA)	-0.308^{***}		-0.214^{***}		-0.211^{***}
	(0.091)		(0.074)		(0.074)
Ideal points (UNGA)		-0.079^{**}		−0.056	−0.003
		(0.040)		(0.035)	(0.037)
Mean within-pair SD: κ	0.153		0.153		0.153
Mean within-pair SD: ideal-point		0.358		0.358	0.358
One-SD effect on trade (%), κ	−4.60		−3.23		−3.17
One-SD effect on trade (%), ideal-point distance		−2.79		−1.98	−0.09
International-trade-by-year FE	No	No	Yes	Yes	Yes
Observations	997,407	997,407	997,407	997,407	997,407

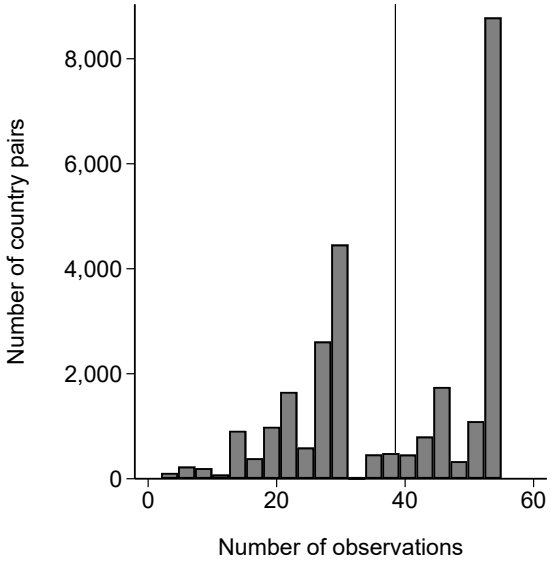
Notes: PPML estimates for aggregate manufacturing trade, using identical observations across columns within each panel. Panel (a) covers 1966–2012: Column (1) uses the preferred UNGA-based measure; Columns (2)–(3) use alliance-based Cohen's κ and Scott's π (Häge 2017). Panel (b) covers 1966–2020: Columns (1)–(2) separately use the preferred measure and the absolute difference in UNGA ideal points (Bailey et al. 2017), without international-trade-by-year fixed effects; Columns (3)–(4) add these effects; Column (5) includes both measures with these effects. All specifications include exporter-year, importer-year, and directed country-pair fixed effects. Panel (a) also includes international-trade-by-year effects. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance, calculated separately for each measure on the common estimation sample within each panel. Domestic flows enter with zero distance. Standard errors in parentheses are clustered by undirected country pair. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.2: Panel dimensions

(a) Observations per year



(b) Observations per country-pair



Notes: Number of observations per year and a histogram with 20 bins for observations per country-pair. The average number of observations per country-pair is 38, and the median is 40.

Table A.2: Country coverage

Country	Range	Country	Range
AFG	1966–2019 (2002–2019)	AGO	1976–2020 (2014–2020)
ALB	1986–2020	ARE	1977–2020
ARG	1966–2020 (1984–2020)	ARM	1994–2020
AUS	1966–2020	AUT	1966–2020
AZE	1992–2020	BDI	1966–2020 (1971–2015)
BEL	1966–2020	BEN	1966–2020 (1974–1981)
BFA	1966–2020 (1974–1983)	BGD	1974–2020
BGR	1980–2020	BHR	1971–2020 (1992–2018)
BHS	1974–2020 (1977–1998)	BIH	2003–2020 (2010–2020)
BLR	1998–2020 (2005–2020)	BLZ	1981–2020 (1989–1992)
BOL	1966–2020 (1970–2019)	BRA	1966–2020 (1990–2020)
BRB	1967–2020 (1970–1997)	BRN	1984–2020 (2010–2012)
BTN	1991–2012 (none)	BWA	2000–2020
CAF	1966–2020 (1973–1993)	CAN	1966–2020
CHE	2002–2020	CHL	1966–2020 (1966–2019)
CHN	1977–2020	CIV	1966–2020 (1966–1997)
CMR	1966–2020 (1970–2008)	COG	1966–2020 (1968–2008)
COL	1966–2020	CPV	1978–2020 (1997–2019)
CRI	1966–2020	CSK	1966–1991
CUB	1975–2018 (1975–2008)	CYP	1966–2020
CZE	1993–2020 (1995–2020)	DEU.1	1973–1989
DEU.2	1991–2020	DNK	1966–2020
DOM	1966–2020 (1966–1984)	DZA	1966–2017 (1967–2017)
ECU	1966–2020	EGY	1966–2020
ERI	1993–2020	ESP	1966–2020
EST	1992–2020	ETH.1	1966–1992
ETH.2	1993–2020 (1993–2015)	FIN	1966–2020
FJI	1970–2020	FRA	1966–2020
GAB	1966–2020 (1966–1995)	GBR	1966–2020
GEO	1996–2020 (1998–2020)	GHA	1966–2019 (1966–2015)
GMB	1966–2020 (1975–2004)	GRC	1966–2020
GTM	1966–2020 (1968–1998)	HND	1966–2020 (1966–1996)
HRV	1992–2020	HTI	1970–1997 (1988–1997)
HUN	1966–2020	IDN.1	1966–2000 (1970–2000)
IDN.2	2001–2020	IND	1966–2020
IRL	1966–2020	IRN	1966–2020
IRQ	1966–2020	ISL	1966–2020 (1968–2020)
ISR	1966–2020	ITA	1966–2020 (1967–2020)
JAM	1966–2020 (1966–1992)	JOR	1966–2020
JPN	1966–2020	KAZ	1993–2020
KEN	1966–2020	KGZ	1992–2020
KHM	1966–2020 (1993–2000)	KOR	1991–2020

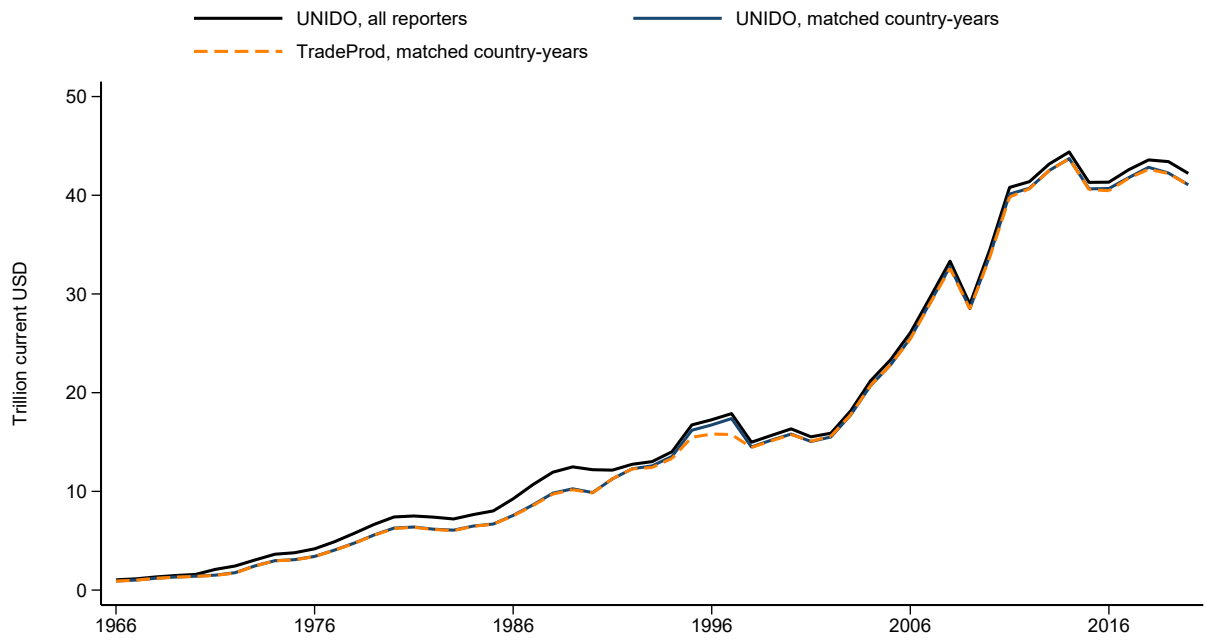
Notes: Each listed country appears in every year within the reported range. Where domestic-trade coverage differs from international-trade coverage, the domestic range is shown in parentheses; "none" indicates that no domestic trade flow is available. International coverage requires both imports and exports. Numerical suffixes distinguish countries across territorial changes. Predecessor states are listed only if they have at least one year of data in the estimation sample.

Table A.2: Country coverage, continued

Country	Range	Country	Range
KWT	1966–2020	LAO	1966–2020 (1999–2017)
LBN	1967–2020 (1998–2014)	LBR	1966–1985 (1972–1985)
LBY	1966–2019 (1966–1980)	LCA	1979–2020 (1993–1997)
LKA	1966–2020	LTU	1992–2020
LUX	1999–2020	LVA	1992–2020
MAR	1966–2020 (1976–2020)	MDA	1992–2020
MDG	1966–2020 (1967–2006)	MDV	1995–2020 (2013–2015)
MEX	1966–2020 (1984–2020)	MHL	1992–2020 (none)
MKD	1993–2020	MLT	1966–2020
MMR	1966–2020 (1989–2020)	MNE	2006–2020 (2010–2020)
MNG	1990–2020	MOZ	1975–2020 (1975–1998)
MUS	1968–2020	MWI	1966–2020 (1966–2019)
MYS.2	1966–2020 (1968–2020)	NAM	2000–2020 (2007–2020)
NER	1966–2020 (1990–2018)	NGA	1966–2020 (1966–1996)
NIC	1966–2020	NLD	1966–2020
NOR	1966–2020	NPL	1974–2020 (1986–2020)
NZL	1966–2020	OMN	1975–2020 (1993–2020)
PAK.1	1966–1970	PAK.2	1971–2020 (1971–2018)
PAN	1966–2020	PER	1966–2020 (1982–2020)
PHL	1966–2020	PNG	1975–2020 (1975–2001)
POL	1982–2020	PRT	1966–2020
PRY	1966–2020 (1966–2014)	QAT	1972–2020 (1987–2020)
ROU	1985–2020	RUS	1992–2020
RWA	1996–2020 (1999–2020)	SAU	1968–2020 (1989–2020)
SCG	1992–2004 (1994–2002)	SDN.1	1966–2010 (1966–2007)
SEN	1966–2020 (1974–2020)	SGP	1966–2020
SLE	1966–2018 (1981–1986)	SLV	1966–2020 (1966–1998)
SOM	1966–1986 (1967–1986)	SRB	2006–2020
SUN	1966–1991 (none)	SUR	1975–2020 (1975–2007)
SVK	1993–2020	SVN	1992–2020
SWE	1966–2020	SWZ	2000–2020 (none)
SYR	1966–2010 (1966–2005)	THA	1966–2020 (1968–2018)
TJK	1992–2020 (1992–2019)	TKM	1997–2000 (none)
TON	1999–2014 (1999–2004)	TTO	1966–2020 (1966–2006)
TUN	1966–2020	TUR	1966–2020
TZA	1966–2020	UGA	1966–2020 (1966–1989)
UKR	1992–2020	URY	1968–2020
USA	1966–2020	UZB	2013–2020
VEN	1966–2013 (1966–1998)	VNM.2	1997–2020 (1998–2020)
YEM.2	1991–2019 (1991–2014)	YUG	1966–1990 (1970–1989)
ZAF	1966–2020	ZMB	1966–2020 (1966–2017)
ZWE	1984–2020 (2009–2018)		

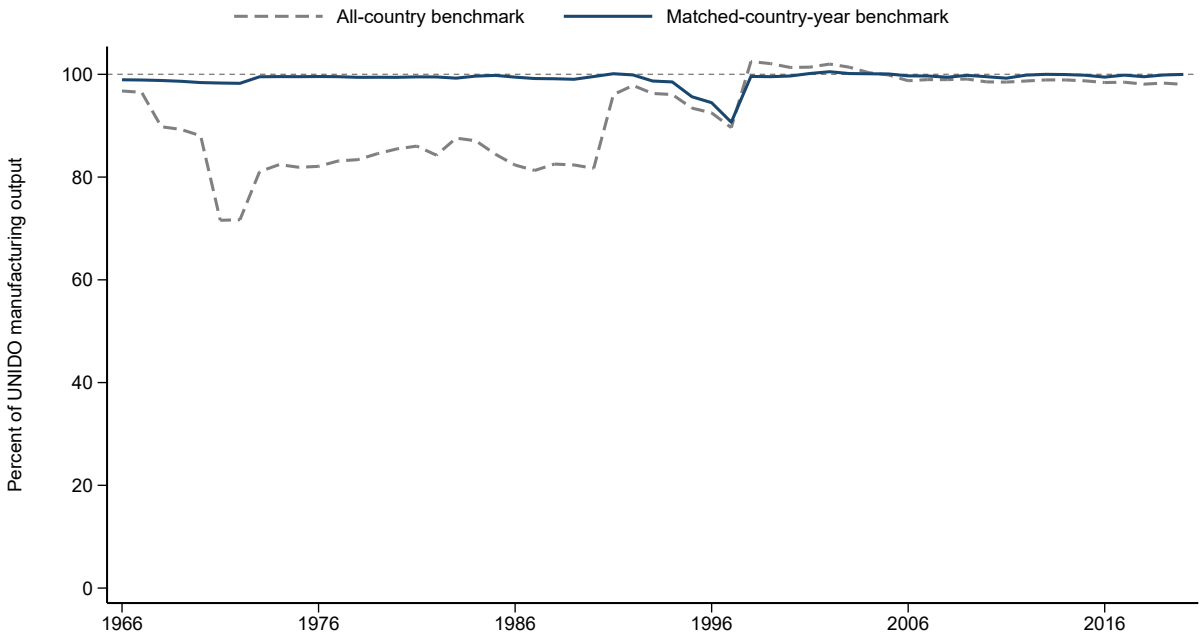
Notes: Each listed country appears in every year within the reported range. Where domestic-trade coverage differs from international-trade coverage, the domestic range is shown in parentheses; "none" indicates that no domestic trade flow is available. International coverage requires both imports and exports. Numerical suffixes distinguish countries across territorial changes. Predecessor states are listed only if they have at least one year of data in the estimation sample.

Figure A.3: Manufacturing output in UNIDO and TradeProd



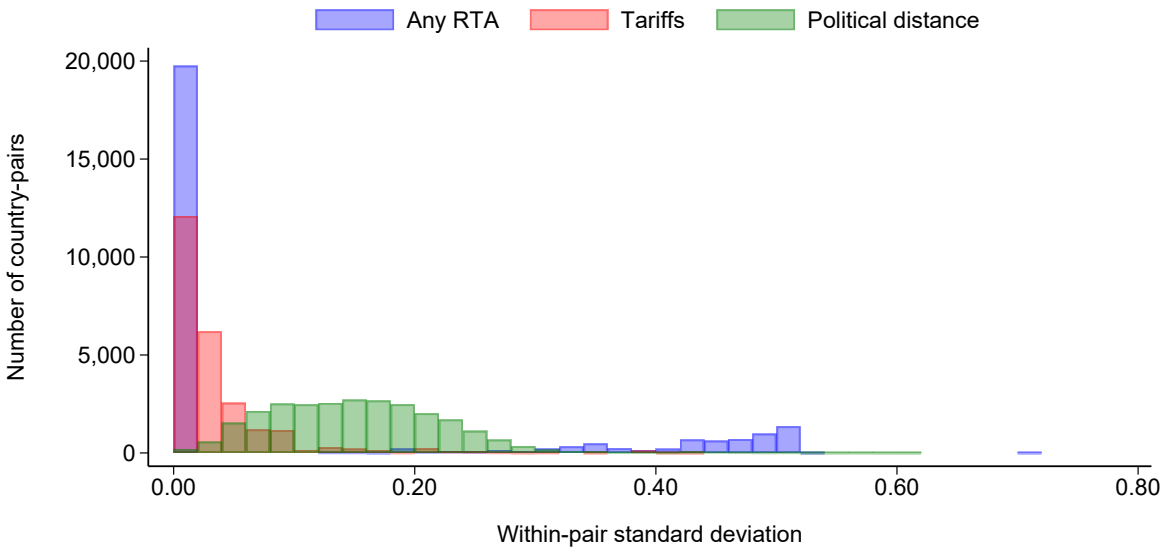
Notes: The figure compares manufacturing output reported by UNIDO for all reporting countries with UNIDO output and total TradeProd flows for matched country-years. Matched country-years have both UNIDO manufacturing output and a domestic TradeProd flow in the common estimation sample. TradeProd totals combine domestic and international flows. Country identities account for territorial changes. Values are in trillion current USD.

Figure A.4: Coverage of manufacturing output: all and matched countries



Notes: The all-country benchmark divides total domestic and international TradeProd flows in the common estimation sample by manufacturing output reported by all countries in UNIDO. The matched-country-year benchmark restricts both sources to country-years with UNIDO manufacturing output and a domestic TradeProd flow in the common estimation sample. The horizontal line denotes 100 percent. Country identities account for territorial changes.

Figure A.5: Histograms of within-pair SDs of trade costs



Notes: Figure shows the distribution of within-pair standard deviations of the main trade costs.

A.3 Additional empirical results

Table A.3: Structural variations

(a) Estimator, transformations and zero flows

	(1) PPML	(2) PPML, positive	(3) IHS-OLS	(4) IHS-OLS, positive	(5) Log-OLS, positive
Political distance	−0.206***	−0.181***	−0.138***	−0.188***	−0.188***
	(0.068)	(0.068)	(0.015)	(0.018)	(0.026)
Mean within-pair SD	0.153	0.143	0.153	0.143	0.143
One-SD effect on trade (%)	−3.12	−2.55	−2.12	−2.69	−2.65
Observations	1,017,000	723,743	1,017,000	723,743	723,743

(b) Interval data

	(1) Annual baseline	(2) 3-year	(3) 4-year	(4) 5-year
Political distance	−0.206***	−0.241***	−0.250***	−0.236***
	(0.068)	(0.066)	(0.075)	(0.065)
Mean within-pair SD	0.153	0.156	0.150	0.155
One-SD effect on trade (%)	−3.12	−3.68	−3.68	−3.58
Observations	1,017,000	329,770	255,220	199,909

Notes: The dependent variable is bilateral exports. All specifications condition on an RTA indicator. Panel (a) compares PPML using all flows, PPML restricted to positive flows, and OLS estimates using the inverse hyperbolic sine or log of exports. Panel (b) compares annual estimation with samples retaining every third, fourth, or fifth calendar year. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance on each model's realized estimation sample. PPML and log OLS use this exponential transformation; IHS OLS instead reports the large-positive-flow approximation $100\widehat{\beta}s$, which has no exact outcome-independent percentage interpretation. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.4: Domestic trade and estimated trade-cost effects

(a) Political-distance-only specification

	(1) Domestic included	(2) No international-trade–year FE	(3) International only	(4) Complete domestic only
Political distance	−0.221***	−0.315***	−0.036	−0.166***
	(0.074)	(0.091)	(0.032)	(0.045)
Domestic flows	Yes	Yes	No	Complete
International-by-year FE	Yes	No	No	Yes
Common within-pair SD	0.153	0.153	0.153	0.153
One-SD effect on trade (%)	−3.33	−4.72	−0.54	−2.52
Observations	1,017,000	1,017,000	983,755	999,631
Additional PPML exclusions	0	0	27,490	14,581

(b) Joint specification with RTAs

	(1) Baseline	(2) No international-trade–year FE	(3) International only
Political distance	−0.206***	−0.260***	−0.028
	(0.068)	(0.084)	(0.031)
Mean within-pair SD	0.153	0.153	0.151
One-SD effect on trade (%)	−3.12	−3.92	−0.43
Observations	1,017,000	1,017,000	983,755

Notes: The dependent variable is bilateral exports. All models use PPML with exporter-year, importer-year, and directed country-pair fixed effects. Column (1) of each panel and Column (4) of Panel (a) additionally include international-trade-by-year fixed effects. Standard errors in parentheses are clustered by undirected country pair. Panel (a) re-estimates the baseline specification without policy controls; columns (1) and (2) use the same 1,017,000 observations. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance. Panel (a) uses s from Column (1) in all columns; Panel (b) calculates s on each specification's realized estimation sample. Panel (b) reports the existing structural-variation specifications, jointly including political distance and an RTA indicator. Within each panel, column (3) starts from the international subset of columns (1) and (2); additional PPML exclusions are 27,490 observations in Panel (a) and 27,490 in Panel (b). Column (4) of Panel (a) excludes 2,788 domestic observations for which TradeProd reports fewer than nine sectors, retains all international observations as eligible, and then loses a further 14,581 observations through PPML singleton and separation exclusions. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.5: Leave-one-country-out robustness

Omitted country	One-SD effect on trade (%)	95% CI	Change (pp)
Full sample	−3.33	[−5.45, −1.16]	—
A. Five most negative effects			
USA	−5.22	[−7.01, −3.39]	−1.89
CAN	−4.78	[−6.25, −3.29]	−1.45
CHN	−3.95	[−6.66, −1.17]	−0.62
RUS	−3.53	[−5.75, −1.26]	−0.20
DEU.2	−3.52	[−5.67, −1.33]	−0.19
B. Five least negative effects			
HUN	−2.77	[−4.87, −0.62]	+0.56
POL	−2.78	[−4.83, −0.70]	+0.55
ESP	−2.82	[−4.88, −0.72]	+0.51
JPN	−2.94	[−5.20, −0.62]	+0.39
ROU	−2.98	[−5.07, −0.85]	+0.35

Notes: The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance on the full baseline estimation sample, held fixed across country exclusions. Each row re-estimates the baseline after omitting the named country. The 95 percent confidence intervals transform coefficient confidence limits using the same formula and fixed s, with standard errors clustered by undirected country pair. Change is relative to the full-sample one-SD effect, in percentage points.

Table A.6: Political-distance effects within and across blocs, RTAs, and regions

	(1) Gopinath blocs	(2) RTA	(3) M49 regions
Within	-3.57^{**}	-3.58^{***}	-3.30^{**}
Across	0.30	-2.60^{***}	-3.44^{***}
At least one non-aligned	-3.82^{***}		
Mean within-pair SD	0.153	0.153	0.153
Within = across (p-value)	0.09	0.40	0.94
All three coefficients equal (p-value)	0.09	–	–
Observations	978,701	978,701	978,701

Notes: The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance on the common covered sample, held fixed across groups and columns. For each group, $\widehat{\beta}$ is its political-distance slope. In Column (1), within and across refer to the same aligned bloc and opposing US–China blocs, respectively, using the bloc classification of Gopinath et al. (2025) based on proximity in UN General Assembly voting (top quartile of ideal-point proximity to the US or China); pairs involving at least one non-aligned country form a third group. In Column (2), within denotes an international pair with an RTA in force in the observation year, and across denotes a pair without an RTA. This specification also includes the RTA indicator's main effect. In Column (3), within and across refer to the same and different broad UN M49 regions: Africa, Americas, Asia, Europe, and Oceania. All columns use the same covered subset of the fixed Table 2 sample and the same SD. The USSR is excluded because its successor countries span Europe and Asia; Czechoslovakia and Yugoslavia are assigned to Europe. Domestic flows remain in estimation with zero political distance. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Equality tests concern the underlying political-distance coefficients. Effect standard errors are omitted. Significance stars use delta-method inference based on clustering by undirected country pair. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.7: Lagged political distance

	(1)	(2)	(3)	(4)
Political distance				
Contemporaneous	-0.227^{***}			
	(0.076)			
One-year lag		-0.258^{***}		
		(0.074)		
Two-year lag			-0.266^{***}	
			(0.072)	
Mean of lags 1–3				-0.346^{***}
				(0.093)
Mean within-pair SD	0.147	0.147	0.147	0.147
One-SD effect on trade (%)	−3.29	−3.73	−3.84	−4.96
Observations	946,712	946,712	946,712	946,712

Notes: PPML estimates of bilateral exports, including domestic and reported zero flows, using the preferred quantitative Cohen's κ measure. Contemporaneous political distance, its one- and two-year lags, and the mean of lags one to three enter separately in Columns (1)–(4). Calendar-year lags are constructed before sample restriction. All four columns use the same 946,712 observations and include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects, without policy controls. Standard errors in parentheses are clustered by undirected country pair. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of contemporaneous political distance, calculated on the common estimation sample and held fixed across columns. Lagged specifications alone do not establish causality. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.8: Full results for lead-lag augmented specifications

	(1) ±5 years	(2) ±4 years	(3) ±3 years	(4) ±2 years	(5) ±1 year	(6) No leads/lags
Political distance						
year $t-5$	-0.172^{***}					
	(0.041)					
year $t-4$	-0.069^{**}	-0.162^{***}				
	(0.028)	(0.047)				
year $t-3$	-0.066^{***}	-0.109^{***}	-0.191^{***}			
	(0.021)	(0.026)	(0.048)			
year $t-2$	-0.055^{***}	-0.056^{***}	-0.100^{***}	-0.200^{***}		
	(0.020)	(0.020)	(0.023)	(0.043)		
year $t-1$	-0.076^{***}	-0.087^{***}	-0.086^{***}	-0.139^{***}	-0.238^{***}	
	(0.021)	(0.023)	(0.023)	(0.030)	(0.048)	
year t	-0.034	-0.033	-0.049^{*}	-0.050^{*}	-0.095^{**}	-0.226^{***}
	(0.028)	(0.030)	(0.028)	(0.028)	(0.038)	(0.085)
year $t+1$	0.006	0.015	0.015	0.014	0.048	
	(0.035)	(0.038)	(0.038)	(0.042)	(0.063)	
year $t+2$	0.020	0.028	0.037	0.072		
	(0.023)	(0.023)	(0.029)	(0.045)		
year $t+3$	0.056^{***}	0.045^{*}	0.046			
	(0.021)	(0.025)	(0.036)			
year $t+4$	0.015	-0.009				
	(0.019)	(0.032)				
year $t+5$	-0.041					
	(0.039)					
Leads jointly zero (p)	0.03	0.11	0.58	0.14	0.44	–
Lags jointly zero (p)	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	–
One-SD effect on trade (%), sum of leads	0.78	1.13	1.43	1.25	0.70	–
Sum of leads $= 0$ (p)	0.53	0.34	0.22	0.29	0.44	–
Sum of leads < 0 (p)	0.74	0.83	0.89	0.86	0.78	–
One-SD effect on trade (%), cumulative	-6.53	-6.19	-5.93	-5.42	-4.66	–
Observations	767,159	767,159	767,159	767,159	767,159	767,159

Notes: Entries are raw PPML coefficients. Columns (1)–(5) include five through one leads and lags; Column (6) includes only contemporaneous political distance. All columns use the realized five-lead/five-lag estimation sample. Unlike the standardized effects in Figure 3, the entries here are coefficients per unit of political distance. Rows marked (p) report p-values: two-sided for the joint significance of the leads and of the lags and for the null that the sum of the lead coefficients is zero ($= 0$), and one-sided against the alternative that this sum is negative (< 0), the pattern implied by reverse causality (trade declining before political distance rises). The one-SD effects on trade are $100[\exp(Bs) - 1]$: B sums the lead coefficients for the sum-of-leads row and the current and lagged coefficients (years $t-K$ to t) for the cumulative row. Here s is the mean within-pair standard deviation of contemporaneous political distance across directed country pairs on the common five-lead/five-lag estimation sample, held fixed across columns. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses below the point estimate. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

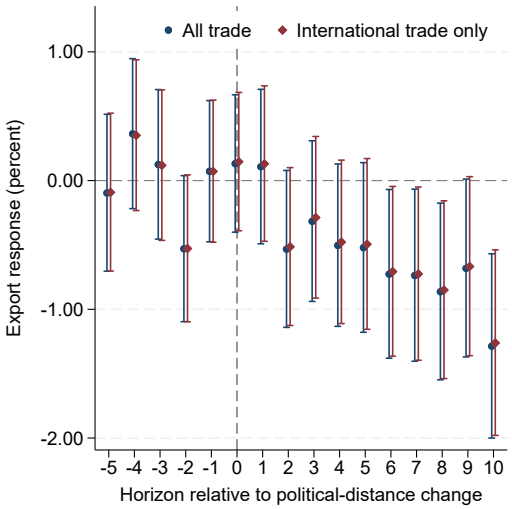
Table A.9: Leads and lags on three- and five-year intervals

	(1) 5-year intervals, averages	(2) 3-year intervals
Political distance		
Previous interval	-0.341^{***}	-0.255^{***}
	(0.093)	(0.051)
Current interval	-0.205^{**}	-0.116^{**}
	(0.092)	(0.054)
Next interval	0.018	0.014
	(0.089)	(0.056)
One-SD effect on trade (%), previous interval	−5.12	−3.87
One-SD effect on trade (%), current interval	−3.12	−1.77
One-SD effect on trade (%), next interval	0.27	0.22
Next interval equal to zero (p-value)	0.84	0.80
Observations	147,641	280,280

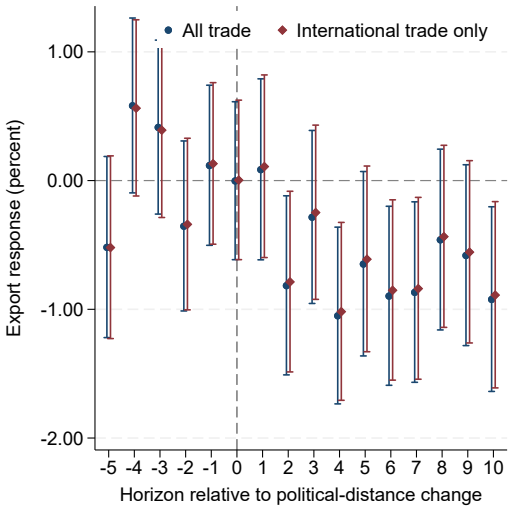
Notes: The dependent variable is bilateral exports. Column (1) uses observations in years divisible by five; political distance enters as its average over the current interval (years $t-4$ to t), the previous interval ($t-9$ to $t-5$), and the next interval ($t+1$ to $t+5$), each requiring at least three non-missing years. Column (2) uses observations in years divisible by three, with political distance in $t-3$, t, and $t+3$. Both columns start from the baseline sample and are estimated by PPML. The one-SD effect on trade is $100[\exp(\widehat{\beta}s)-1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance across international pairs in the annual baseline estimation sample, held fixed across intervals and columns. The p-value refers to the two-sided test that the next-interval coefficient is zero. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses below the point estimate. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.6: Local projections of trade on political distance

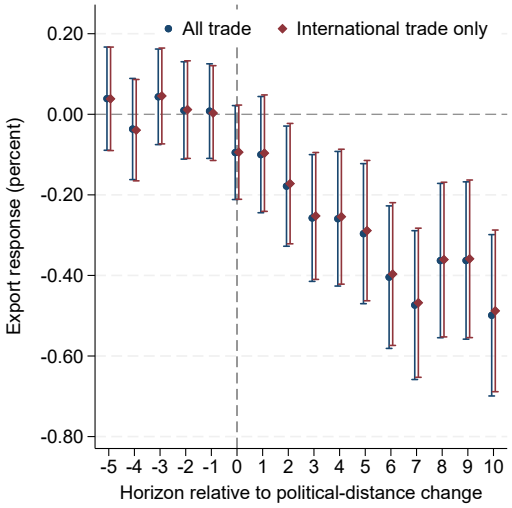
(a) Using ln Exports, full sample



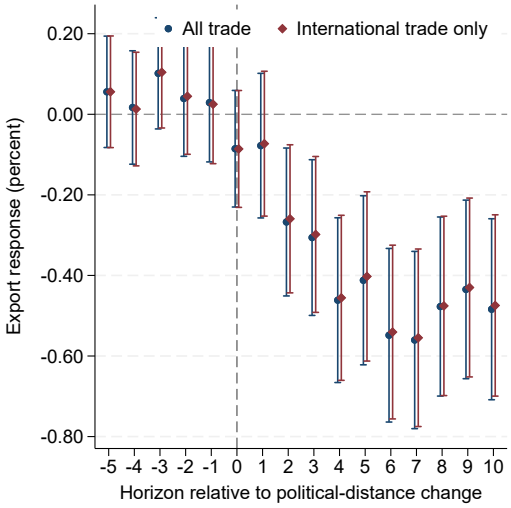
(b) Using ln Exports, fixed sample



(c) Using *asinh* Exports, full sample



(d) Using *asinh* Exports, fixed sample



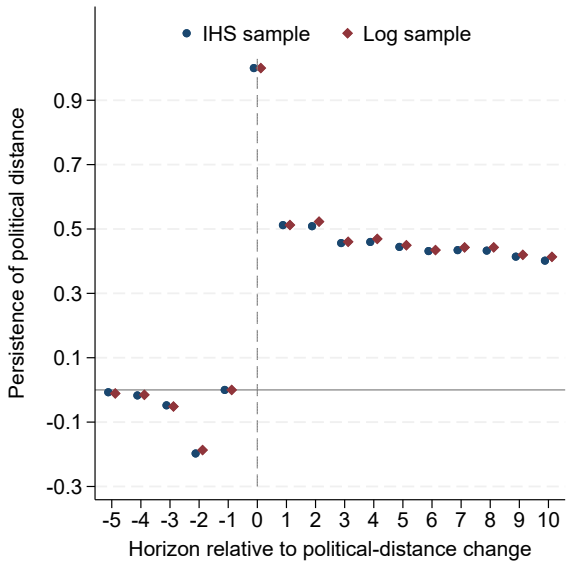
Notes: Impulse responses to an increase in political distance equal to the mean, across country pairs, of the within-pair standard deviation of political-distance levels. I estimate local projections (Jordà 2005; Boehm et al. 2023) of log- or inverse-hyperbolic-sine-transformed bilateral exports on the first difference of political distance. Denoting the transformed outcome by $y_{ij,t}$, the horizon-specific regressions are $\Delta_h y_{ij,t} = \beta_h \Delta \text{PD}_{ij,t} + \gamma_h \Delta \text{PD}_{ij,t-1} + \pi^h_{ij} + \pi^h_{i,t} + \pi^h_{j,t} + \pi^h_{i \neq j,t} + \varepsilon^h_{ij,t}$. For $h \geq 0$, the outcome is the long difference $\Delta_h y_{ij,t} \equiv y_{ij,t+h} - y_{ij,t-1}$. For $h < 0$, it is the one-period pre-shock change $\Delta_h y_{ij,t} \equiv y_{ij,t+h} - y_{ij,t+h-1}$. The full-sample specifications use all observations available at each horizon, whereas the fixed-sample specifications use the same observations at every horizon and require core-sample eligibility and a nonmissing transformed outcome throughout the window from $t-6$ to $t+10$. The figures report estimates using all bilateral flows and international flows only. Log responses are converted to percentages as $100[\exp(\widehat{\beta}_h s) - 1]$, where s is the political-distance increase defined above. IHS responses use the large-positive-flow approximation $100\widehat{\beta}_h s$. Bars report pointwise 95 percent confidence intervals. Standard errors are clustered by undirected country pair.

Table A.10: Local projections pretrend tests

Outcome	Trade sample	Horizon sample	Pre-period coefficients jointly zero (p-value)	Post-treatment coefficients jointly zero (p-value)
IHS	All trade	Horizon-specific	0.92	< 0.01
IHS	All trade	Fixed	0.14	< 0.01
Log	All trade	Horizon-specific	0.51	0.02
Log	All trade	Fixed	0.21	< 0.01
IHS	International	Horizon-specific	0.92	< 0.01
IHS	International	Fixed	0.13	< 0.01
Log	International	Horizon-specific	0.53	0.03
Log	International	Fixed	0.25	< 0.01

Notes: The table reports p-values for the joint nulls that all local-projection coefficients in the pre-period ($h = -5, \ldots, -1$) and post-treatment period ($h = 0, \ldots, 10$) equal zero. Local projections are estimated in a stacked regression with horizon-specific coefficients on contemporaneous and lagged changes in political distance. All specifications absorb horizon-specific directed-pair, exporter-year, importer-year, and international-status-year fixed effects. Inference is based on standard errors clustered by undirected country pair. IHS denotes the inverse-hyperbolic-sine transformation of exports and includes zero trade flows; Log denotes the natural logarithm and is defined only for positive trade flows. All trade includes domestic and international flows, while International restricts the sample to international flows. Horizon-specific allows the estimation sample to vary with outcome availability at each horizon; Fixed uses the common sample observed at every horizon from $h = -5$ through $h = 10$.

Figure A.7: Persistence of changes in political distance



Notes: Local projections of political distance on its own one-year change at t, with the controls, fixed effects, and horizon-specific samples of the local projections in Figure A.6. For $h \geq 0$, the outcome is the change in political distance from $t-1$ to $t+h$; for $h < 0$, it is the annual change in year $t+h$. Coefficients are per unit change at t. At $h = -1$, the outcome equals the lagged change, which is controlled for, so the coefficient is zero by construction. IHS and log samples refer to the observations used for the respective trade outcome. Bars show 95 percent confidence intervals based on standard errors clustered by undirected country pair.

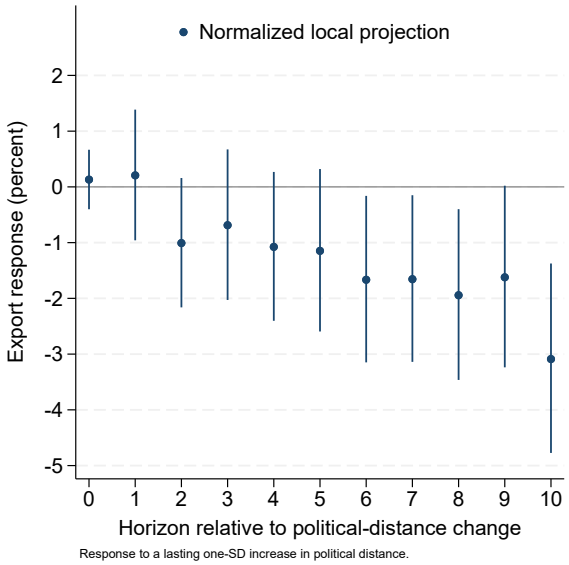
Table A.11: Local projections: political-distance persistence and persistence-adjusted trade responses

	IHS of exports		Log exports	
Horizon	Persistence	One-SD effect on trade (%)	Persistence	One-SD effect on trade (%)
0	1.000	−0.10	1.000	0.13
	(0.000)	[−0.21, 0.02]	(0.000)	[−0.40, 0.67]
1	0.512	−0.20	0.512	0.21
	(0.002)	[−0.48, 0.09]	(0.003)	[−0.96, 1.39]
2	0.509	−0.35	0.523	−1.01
	(0.002)	[−0.64, −0.06]	(0.002)	[−2.16, 0.16]
3	0.456	−0.57	0.460	−0.69
	(0.002)	[−0.91, −0.22]	(0.003)	[−2.03, 0.67]
4	0.460	−0.56	0.469	−1.08
	(0.002)	[−0.93, −0.20]	(0.002)	[−2.40, 0.27]
5	0.444	−0.66	0.450	−1.15
	(0.002)	[−1.05, −0.27]	(0.003)	[−2.59, 0.32]
6	0.431	−0.94	0.435	−1.67
	(0.002)	[−1.35, −0.53]	(0.003)	[−3.15, −0.16]
7	0.434	−1.09	0.443	−1.66
	(0.002)	[−1.51, −0.66]	(0.003)	[−3.14, −0.15]
8	0.433	−0.84	0.443	−1.94
	(0.002)	[−1.28, −0.40]	(0.003)	[−3.46, −0.40]
9	0.414	−0.88	0.420	−1.62
	(0.002)	[−1.35, −0.41]	(0.003)	[−3.24, 0.02]
10	0.402	−1.24	0.413	−3.09
	(0.002)	[−1.74, −0.74]	(0.003)	[−4.77, −1.37]

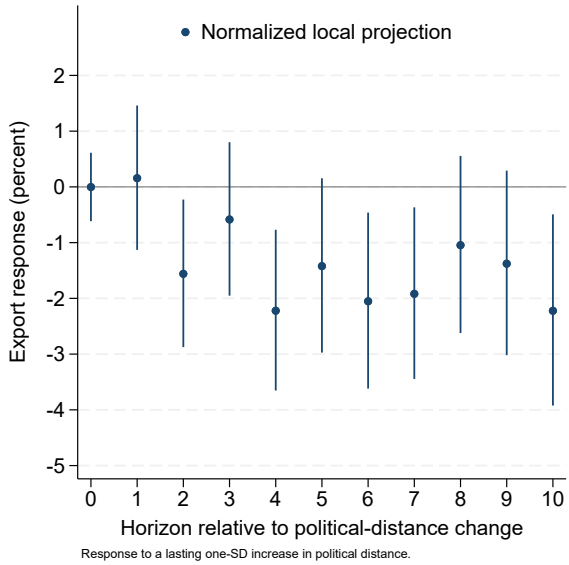
Notes: Horizon-specific samples, including domestic and international trade. Persistence is the political-distance response per unit change at t (standard errors in parentheses). The one-SD effect on trade is $100[\exp(r_h s) - 1]$ for log exports and the large-positive-flow approximation $100 r_h s$ for IHS exports. Here r_h is the trade response divided by the political-distance response at horizon h; s is the mean within-pair standard deviation of contemporaneous political-distance levels across directed country pairs in the common five-lead/five-lag gravity estimation sample, held fixed across horizons and outcomes. Brackets show pointwise 95 percent confidence intervals from delta-method intervals for the ratio, accounting for covariance between the jointly estimated responses. Equations share observations at each horizon and have outcome- and horizon-specific coefficients and fixed effects. Standard errors are clustered by undirected country pair. A lasting-change interpretation additionally requires the trade response to scale with persistence. Static and cumulative gravity effects on the lead-lag sample are −3.2 and −6.5 percent.

Figure A.8: Persistence-adjusted trade responses: horizon-specific and fixed samples

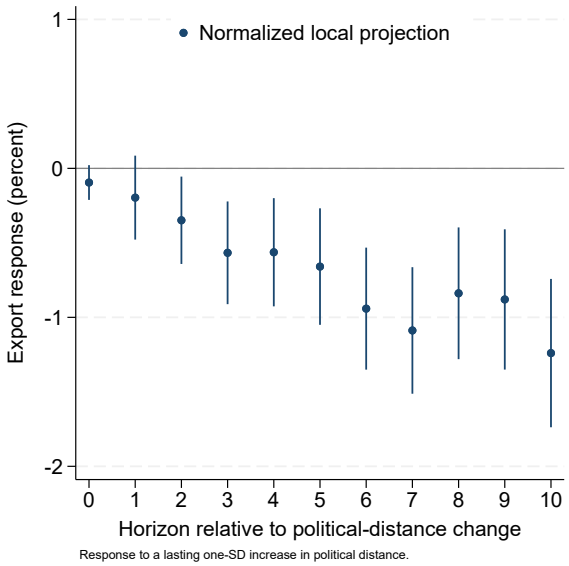
(a) Log exports, horizon-specific sample



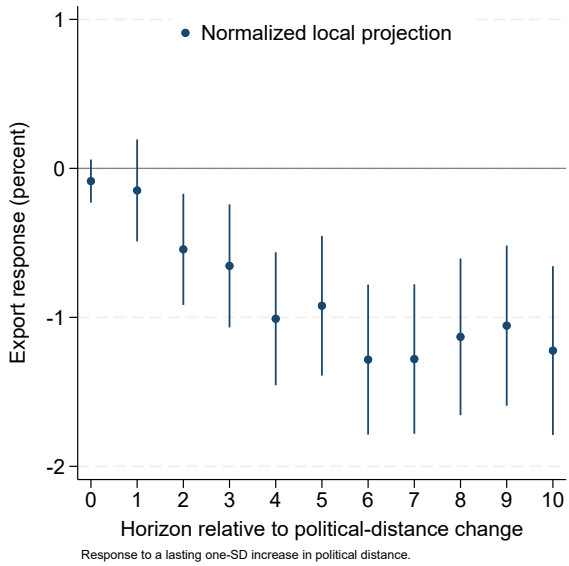
(b) Log exports, fixed sample



(c) IHS exports, horizon-specific sample



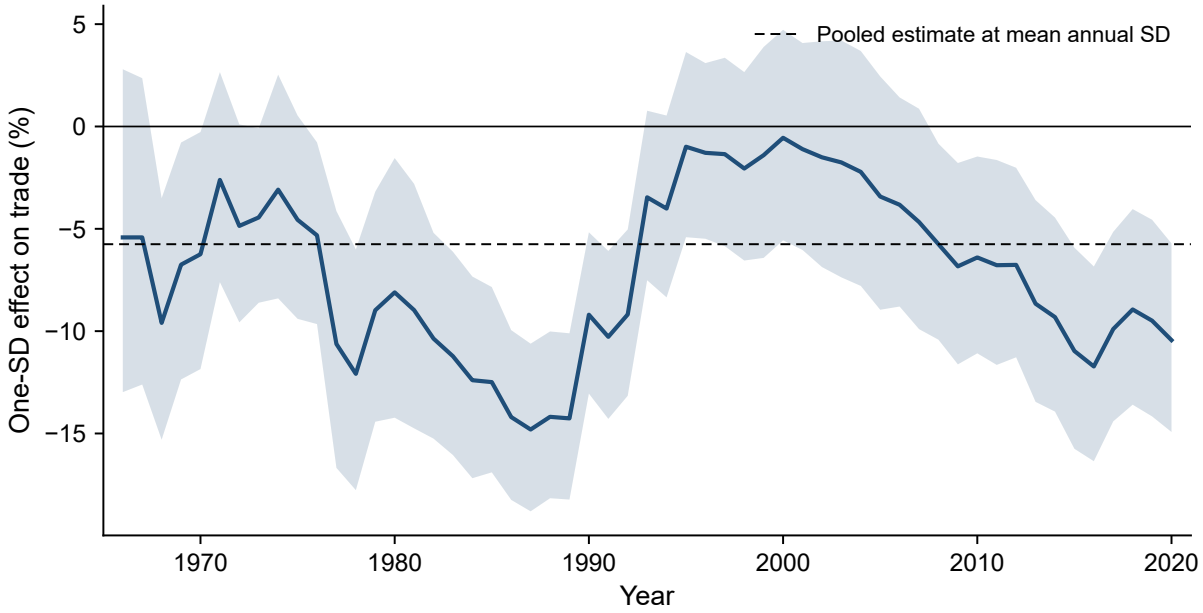
(d) IHS exports, fixed sample



Notes: Persistence-adjusted responses for domestic and international trade. At each horizon, the trade response is divided by the response of political distance, using jointly estimated equations on common observations. Effects and pointwise 95 percent confidence intervals are constructed as in Table A.11. Horizon-specific samples vary with the joint availability of both outcomes; fixed samples use observations available throughout the full estimation window. Standard errors are clustered by undirected country pair.

A.3.1 Scaling by the annual dispersion of political distance

Figure A.9: Trade effects of a year-specific one-standard-deviation increase in political distance



Notes: Effects are calculated as $100[\exp(\widehat{\beta}_t SD_t) - 1]$ from the unrestricted annual coefficients in Figure 4, using the corrected sample of 1,017,000 observations. The sample retains domestic and zero trade flows and the baseline fixed effects. SD_t is the unweighted annual standard deviation of political distance across unique unordered international pairs, included when at least one direction belongs to the sample. Domestic pairs are excluded from the SD calculation. Shading shows pointwise 95 percent confidence intervals, transforming coefficient confidence limits based on undirected-pair clustering and treating SDs as fixed. The dashed black line evaluates the unrestricted pooled coefficient at the mean annual SD.

A.3.2 Additional channel evidence

Table A.12: Political-distance effects by TradeProd sector

	(1) Pooled	(2) By sector
Political distance		
Pooled	−0.243***	
	(0.068)	
By TradeProd sector		
Food		−0.358***
		(0.056)
Textiles		−0.221***
		(0.086)
Wood-Paper		−0.113
		(0.127)
Chemicals		−0.141
		(0.117)
Minerals		−0.241***
		(0.064)
Metals		−0.298***
		(0.085)
Machines		−0.346***
		(0.063)
Vehicles		−0.147*
		(0.086)
Other		−0.204**
		(0.089)
Sector coefficients equal (p-value)		< 0.01
Observations	8,587,165	8,587,165

Notes: Column (1) reports the pooled political-distance coefficient. Column (2) allows the coefficient to vary across TradeProd sectors and reports sector-specific coefficient levels rather than deviations from an omitted sector. The equality p-value is from a joint Wald test that all sector-specific political-distance coefficients are equal. Both specifications omit tariffs and RTAs, use identical observations, and include exporter-year–sector, importer-year–sector, country-pair–sector, and international-trade–year–sector fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.13: Tariff attenuation on the common 1988–2020 sample

(a) Baseline and single channels

	(1) Baseline	(2) Tariffs	(3) RTAs	(4) Sanctions
Political distance				
Preferred κ	−0.109	−0.102	−0.102	−0.045
	(0.073)	(0.065)	(0.071)	(0.076)
Potential channels				
Ln(1+tariff), Teti		−2.664***		
		(0.311)		
Any RTA			0.104***	
			(0.039)	
Any GSDB sanction				−0.114***
				(0.020)
Mean within-pair SD	0.137	0.137	0.137	0.137
One-SD effect on trade (%)	−1.48	−1.39	−1.38	−0.61
Observations	697,954	697,954	697,954	697,954

(b) Combined channels

	(1) RTAs and sanctions	(2) Tariffs and RTAs	(3) Tariffs and sanctions	(4) All three
Political distance				
Preferred κ	−0.040	−0.102	−0.040	−0.040
	(0.074)	(0.065)	(0.068)	(0.068)
Potential channels				
Ln(1+tariff), Teti		−2.648***	−2.636***	−2.638***
		(0.331)	(0.305)	(0.326)
Any RTA	0.098**	0.005		−0.000
	(0.039)	(0.039)		(0.040)
Any GSDB sanction	−0.111***		−0.109***	−0.109***
	(0.020)		(0.018)	(0.018)
Mean within-pair SD	0.137	0.137	0.137	0.137
One-SD effect on trade (%)	−0.54	−1.38	−0.55	−0.55
Observations	697,954	697,954	697,954	697,954

Notes: PPML estimates for aggregate manufacturing trade on the common sample for which the preferred political-distance measure, tariffs from Teti (2024), RTAs, and sanctions are jointly observed. In the tariff-covered subset of the baseline sample (698,889 observations), the mean and median tariff are approximately 10 and 8 percent, respectively. Panel (a) reports the baseline and adds each potential policy channel separately; Panel (b) adds pairs of channels (Columns 1–3) and all three channels together (Column 4). All columns use the same observations. The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance, calculated on the common estimation sample and held fixed across columns. All specifications include exporter-year, importer-year, directed country-pair, and international-trade-by-year fixed effects. Standard errors for coefficient estimates are clustered by undirected country pair and reported in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.14: Security-related political distance and sanctions

	One-SD effect on trade (%)			
	(1) All dyad-years	(2) + Sanctions control	(3) Exclude sanctions	(4) Interaction model
Political distance				
Security	-3.89^{***}	-2.74^{***}	-1.52^{**}	
Security, no sanctions				-2.64^{***}
Security, sanctions active				-3.27^{***}
Equal security effects (p-value)	–	–	–	0.46

Notes: The one-SD effect on trade is $100[\exp(\widehat{\beta}s) - 1]$, where s is the mean across directed country pairs of their within-pair standard deviations of political distance for the security-related distance measure. In the interaction model, $\widehat{\beta}$ is the relevant sanctions-status-specific slope. The specifications sequentially use all dyad-years in the matched topic-policy sample, control for sanctions, exclude observations with active sanctions, and allow the effect to differ by sanctions status. Unlike the main topic table, these specifications do not control for RTAs. The no-sanctions subsample uses its own mean within-pair standard deviation; the other specifications use the common matched-sample scale. The reported equality p-value tests whether the interaction-model effects with and without an active sanction are equal. All specifications include the standard structural-gravity fixed effects. Effect standard errors are omitted. Significance stars use delta-method inference based on clustering by undirected country pair. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

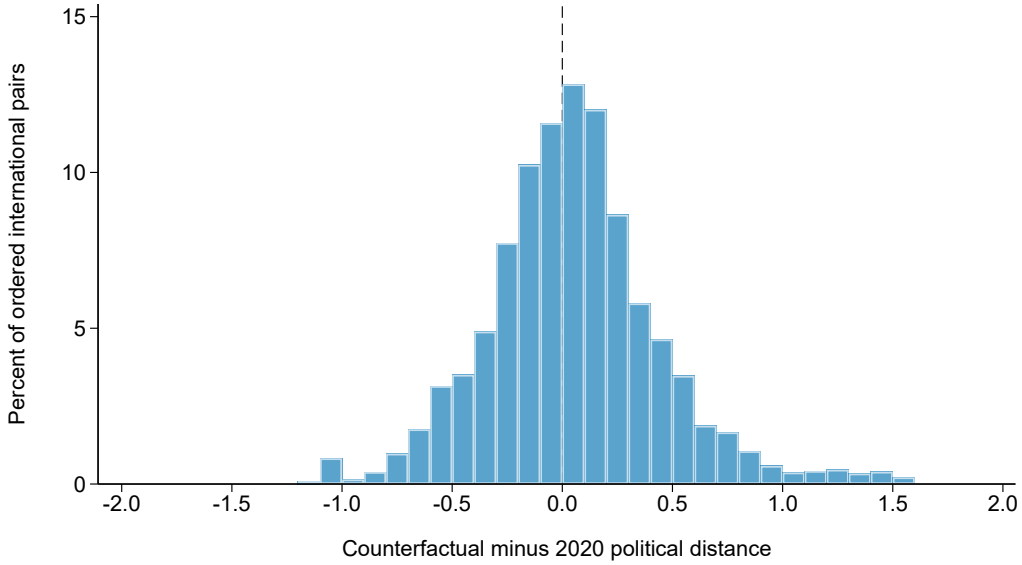
A.4 Additional counterfactual results

Table A.15: New Cold War welfare effects under historical trade responsiveness

Country group	N	2016–2020	1983–1990	Cold War, SD-scaled
Americas	29	0.72	1.48	1.00
		[0.34, 1.21]	[0.89, 2.25]	
Former Warsaw Pact	22	−1.81	−2.49	−2.16
		[−2.54, −1.06]	[−3.14, −1.91]	
Other Europe	25	−1.56	−2.54	−1.98
		[−2.35, −0.85]	[−3.44, −1.76]	
Rest of the world	82	−0.36	−0.58	−0.45
		[−0.54, −0.20]	[−0.78, −0.40]	
All countries	158	−0.55	−0.78	−0.66
		[−0.79, −0.32]	[−0.99, −0.59]	
Political-distance coefficient		−0.352	−0.643	−0.468

Notes: Entries are unweighted regional mean real-income changes in percent, evaluated at parameter point estimates. All columns use the same 2020 economy, 1962 counterfactual political distances, and estimated trade elasticity. The first two columns use episode-specific political-distance slopes from the same 1966–2020 gravity regression without tariff, RTA, or sanctions controls. The final column multiplies the 1983–1990 coefficient by the ratio of the mean annual political-distance SD in 1983–1990 to its 2020 SD, calculated across international pairs in the regression sample. Brackets in the first two columns report 95 percent parametric-bootstrap confidence intervals from 5 000 draws per specification, approximating covariance between each political-distance slope and the separately estimated trade elasticity as zero. Regional means are calculated within each draw before taking percentiles. The final column reports point estimates only.

Figure A.10: Counterfactual changes in political distance



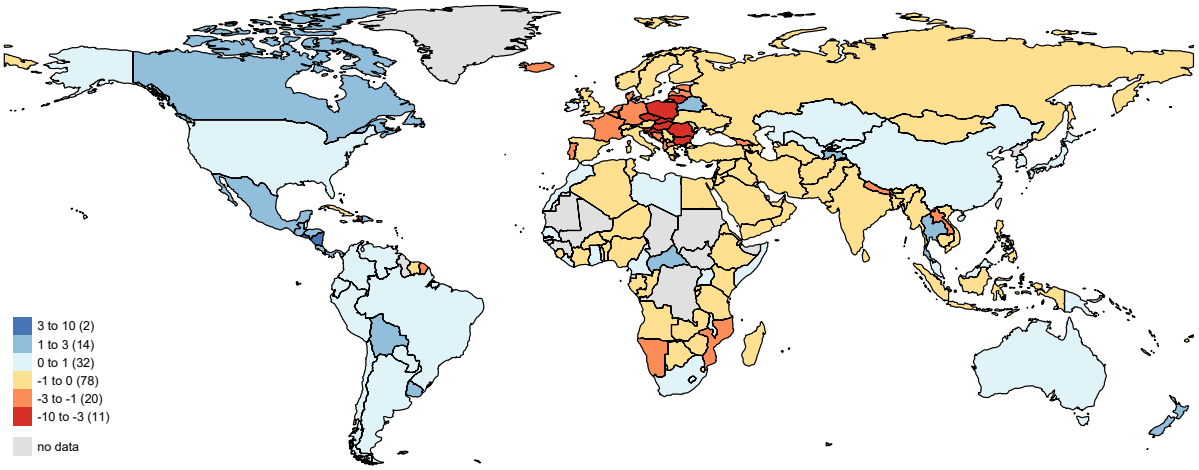
Notes: Counterfactual minus 2020 political distance across 24,492 ordered international pairs of the 157 individually represented economies, excluding domestic observations and the rest-of-world aggregate. The six pairs without an assigned historical distance receive zero shocks, matching the solver. The vertical line denotes no change; approximately 97 percent of changes are smaller than one in absolute value.

Table A.16: Welfare effects of a New Cold War: sanctions controlled

Country group	N	Mean	Median	SD	Losing (%)	Min	Max
Americas	29	0.42	0.64	1.54	20.69	−6.71 (HTI)	2.37 (SLV)
Former Warsaw Pact	22	−1.26	−1.08	1.59	77.27	−3.56 (HUN)	1.30 (TJK)
Other Europe	25	−1.02	−0.81	0.94	96.00	−4.26 (SVN)	0.04 (IRL)
Rest of the world	82	−0.23	−0.24	0.50	76.83	−2.42 (CYP)	1.04 (CAF)
All countries	158	−0.38	−0.24	1.15	69.62	−6.71 (HTI)	2.37 (SLV)

Notes: Real-income changes are reported in percent. Means, medians, and sample standard deviations (SD, in percentage points) are unweighted statistics across economies, including the rest-of-world aggregate. Losing reports the percentage with a strictly negative change. All statistics use country outcomes evaluated at parameter point estimates; extrema identify the corresponding economy in parentheses. The political-distance coefficient is estimated conditional on sanctions and Teti tariffs, omitting RTAs. The trade elasticity is held at its main point estimate, and other counterfactual inputs are unchanged.

Figure A.11: Geographic distribution of New Cold War effects



Notes: The map shows welfare changes evaluated at the parameter point estimates for the tariff-conditioned 2016–2020 specification, estimated with Teti tariffs and without RTA or sanctions controls. Welfare changes are reported in percent.