Political Distance and International Trade*

Knut Niemann

This version: 3 September 2026

Abstract

Rising geopolitical tensions have revived concerns about trade fragmentation and a "New Cold War." Using disagreement in UN General Assembly voting, I measure bilateral political distance and estimate its relationship with trade in a structural gravity model covering 166 countries from 1966 to 2020. An increase equal to the mean within-pair standard deviation is associated with 3.36 percent lower trade, about one quarter of the effect of removing a regional trade agreement. Dynamic estimates show little prior trade movement and subsequent declines. Adding sanctions attenuates the coefficient by 30 percent, while security-related disagreement predicts trade more strongly than human-rights disagreement, patterns more consistent with a trade-cost channel than a demand shifter. In a counterfactual return to 1962 political alignments, global welfare effects are small, but the Americas gain 2.2 percent while the former Warsaw Pact and other European countries lose 2.1 and 1.5 percent, respectively.

Keywords: Political distance, gravity, trade fragmentation, geopolitics
JEL-Codes: F12, F13, F14, F51

*Niemann: University of Tübingen, knut.niemann@uni-tuebingen.de. I thank participants at the Macro Brown Bag Seminar (Tübingen), the XXVII Conference on International Economics and the EEA-ESEM Congress, as well as Anna Gumpert, Gernot Müller, Ralph Lütticke, Felix Strobel, Inga Heiland, Martina Magli, Andrea Ariu, Claudia Steinwender, Ina Jäkel, and Simon Gilchrist, for their useful comments. I also thank Selami Doğan Akansu for excellent research assistance. I gratefully acknowledge financial support from the Friedrich-List-Stiftung.

1 Introduction

Geopolitical tensions have intensified in recent years, prompting concerns that trade could fragment along political lines, with some foreseeing a 'New Cold War'. Yet systematic long-run evidence on how strongly political frictions affect trade remains scarce. In this paper, I quantify this impact.

To fix ideas, I use a simple trade model to show that political frictions can enter either as demand shifters or as trade costs. As an empirical proxy for political frictions, I calculate political distance based on countries' votes in the United Nations General Assembly (UNGA) on, for example, the status of Palestine or nuclear disarmament. Despite some early uses, these data have only recently found their way into the broader international economic literature and policy discussions.[1] UNGA votes are useful for measuring political distances because of near-universal membership and frequent roll calls (around 80 per year, on average) on a wide range of topics. The main measure of political distance relies on the squared difference in countries' votes (Cohen 1968). It widens during geopolitical confrontation, narrows during cooperation, and fluctuates with domestic political change.

For the empirical analysis, I use a panel of international and domestic trade for 166 countries from 1966 to 2020. I estimate a gravity equation and find that an increase in political distance by the average within-pair standard deviation decreases trade by 3.36 percent, roughly one quarter the impact of removing an RTA. The dynamics of this effect indicate no robust evidence that trade declines beforehand, while higher political distance consistently predicts subsequent declines in trade.

Controlling for sanctions attenuates the political-distance coefficient by 30 percent, consistent with sanctions accounting for an economically meaningful part of the relationship. Political distance on security resolutions has the strongest and most robust relationship with trade, whereas political distance on human-rights resolutions is statistically insignificant, also more suggestive of the trade-cost channel than of the demand shifter channel.

Finally, political distance matters for real income. I consider a New Cold War in 2020 that sets political distances to their 1962 values. On average, the resulting general-equilibrium change in real income is small, at -0.1 percent. This global average, however, conceals substantial regional differences: countries in the Americas gain 2.2 percent, while countries of the former Warsaw Pact and other European countries lose 2.1 and 1.5 percent, respectively.

Together, the results show that political distance is a quantitatively meaningful trade friction whose aggregate effects conceal substantial regional exposure.

[1] See, for example, Wang (1999), Alesina and Dollar (2000), and Dreher et al. (2008) for earlier uses and Kleinman et al. (2024), Javorcik et al. (2024), and Gopinath et al. (2025) for more recent ones.

In more detail, I first use a standard one-sector Armington (1969) model to fix ideas. Political frictions can reduce bilateral trade via two conceptually distinct channels. They may operate as a demand shifter, lowering consumers' valuation of goods, for example, due to political consumerism. Political frictions, however, may also enter the model as a trade cost by reflecting sanctions or contract enforcement risk. In the resulting gravity equation that characterizes bilateral expenditures, the coefficient on political frictions captures the combined demand- and cost-side effect. The framework also provides the general-equilibrium structure used in the counterfactual analysis.

To bring this framework to the data, I construct a measure of political frictions from countries' voting behavior on final-passage resolutions in the United Nations General Assembly United Nations (2026) and refer to it as political distance. Political distance is continuous, bilateral and time-varying. It takes a quadratically weighted Cohen's κ (Cohen 1968) and transforms it into distance. From κ (see also Häge 2011; Fernandez-Villaverde et al. 2025), political distance inherits two useful features. First, yes–no disagreement receives more weight than disagreement involving an abstention. Second, the measure adjusts observed voting disagreement for expected disagreement given both countries' marginal voting distributions. Political distance equals zero under identical voting and has a theoretical upper bound of two.

Political distance reflects major geopolitical episodes and moves with changes in government. Political distance between the US and Russia (and the Soviet Union), for example, was particularly high in the 1970s but reached its historical low briefly after the end of the Cold War. Since then, however, it has steadily increased. In 2020, it remains well below Cold-War levels.

Data on intra-national and international trade flows are from the CEPII Trade and Production database (Tradeprod Mayer et al. 2023). Its International trade flows in TradeProd are from COMTRADE. Intra-national trade flows in TradeProd, which account for around three-quarters of global expenditures and are crucial for theory-consistent estimation, are computed by subtracting total exports from industrial production in nine sectors. I aggregate the nine sectoral flows to the country-pair level and use these trade flows throughout.

Trade agreements (from Egger and Larch 2008), sanctions (from the Global Sanctions Database, Felbermayr et al. 2020; Yalcin et al. 2025), and tariffs (from Feodora Teti's Global Tariff Database, Teti 2024) enter as potential channels through which political frictions may operate. Because these policies may themselves respond to political distance, they are excluded from the baseline and introduced subsequently as potentially endogenous channels.

The combined estimation panel covers the years from 1966 to 2020 and averages around 18,000 observations per year. It features 166 countries as exporters and importers. On average, country pairs are observed for 39 years, with a median of 40 years. The empirical framework strongly relies on within-country-pair variation over time. While political distance varies over time for virtually all international country pairs, RTAs and sanctions vary over time for 25 and 41 percent of country pairs, respectively. Tariffs are available only from 1988 onward but vary over time in 96 percent of covered country pairs. International and intra-national trade covered in the estimation panel sum to approximately 92 percent of world manufacturing output on average and 98 percent in 2020.

The empirical framework brings the gravity equation of the model to the data. It relates the value of goods traded from the exporter to the importer to political distance and fixed effects at the origin-year, destination-year, country-pair and international-trade-by-year level (Yotov et al. 2016; Head and Mayer 2014). The baseline specification intentionally does not condition on tariffs, trade agreements, or sanctions, as these variables may represent mechanisms through which political frictions operate. The coefficient for political distance is then estimated from changes within a country pair over time, conditional on fixed effects. The estimation uses the Poisson pseudo-maximum likelihood estimator (PPML, see Silva and Tenreyro 2006; Correia et al. 2020).

The baseline empirical result is that an increase in political distance of one within-pair standard deviation is associated with a 3.36 percent decrease in trade.

In the specification controlling for RTAs, this effect is approximately one quarter of the estimated effect of removing an RTA. This magnitude is robust to alternative measures and to controlling for militarized interstate disputes that may jointly impact political frictions and trade. I also document that this average magnitude hides substantial heterogeneity over time. The relationship is strongest during the late Cold War and after 2016, but statistically insignificant between the end of the Cold War and the Great Financial Crisis.

To examine whether changes in trade precede or follow changes in political distance, I augment the baseline specification with leads and lags of political distance (Baier and Bergstrand 2007; Wooldridge 2008). Overall, coefficients for leads of political distance are not significant, neither individually nor jointly, with joint p-values above 5 percent in all specifications. Lag coefficients are negative and jointly significant at conventional levels. Local projections similarly show little systematic movement before changes in political distance and increasingly negative responses after approximately two to three years. Overall, the results provide no robust evidence that trade declines precede increases in political distance, while higher political distance consistently predicts subsequent trade declines.

The dynamic patterns are consistent with causality running from political distance to trade rather than the reverse. However, they cannot reveal if the mechanism runs through a demand shifter or a trade cost. For this, I re-estimate the baseline specification and sequentially include potential channels as a descriptive attenuation exercise. The main text considers RTAs and sanctions as potential channels, as they are available for the entire sample period. While controlling for RTAs leads to little attenuation of the political distance coefficient, controlling for sanctions reduces the coefficient by 30 percent. This attenuation is consistent with sanctions accounting for an economically meaningful part of the relationship, although it cannot be interpreted as a causal mediation effect. Additional results in the appendix show that tariffs (Teti 2024) induce little attenuation in the political distance coefficient, suggesting tariffs are less likely to be part of the mechanism.

Additionally, I compute political distance for specific topics that are plausibly indicative of different mechanisms. Based on the textual descriptions of resolutions, I tag resolutions as human rights-related, economic, or security-related. Human-rights disagreement, then, is more closely associated with the demand shifter mechanisms, while security or economic disagreement is more closely associated with the trade-cost mechanism. Estimating the baseline specification with these measures of political distance yields significantly negative effects only for economic and, in particular, security political distance. Human-rights distance, on the other hand, is statistically insignificant, both individually and in a horse-race specification. In sum, these results are more consistent with political distance operating as a trade cost rather than a demand shifter.

Having estimated the combined effect of political distance on bilateral expenditures, I use the one-sector Armington model introduced earlier to quantify its general-equilibrium welfare consequences. Specifically, I consider a New Cold War (Gopinath et al. 2025) in 2020, in which political distances are (approximately) reset to the levels observed in 1962. Using the model, I compute how these changes in political distance affect real income around the world in general equilibrium. To reflect the time-heterogeneity, I use the post-2016 political-distance coefficient of -0.38 and an estimated trade elasticity of 3.02. On average, a New Cold War would have minor welfare effects (-0.1 percent). Regionally, however, there is large heterogeneity. Countries in the Americas gain 2.2 percent in real income, while countries of the former Warsaw Pact (-2.1 percent) and other European countries (-1.5 percent) experience real income losses. For the rest of the world, real income gains are small ($+0.1$ percent). Holding sanctions fixed reduces the magnitudes of the regional gains and losses by approximately 15–20 percent, although the small global average remains essentially unchanged.

The remainder of the paper is organized as follows. In the remainder of the introduction, I place the contribution in the context of the literature. 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.

Contribution to the literature This paper relates most directly to the literature on the political determinants of international trade. Early work shows that commerce responds to political cooperation, strategic interests, and military alliances (Pollins 1989a; Pollins 1989b; Gowa and Mansfield 1993; Morrow et al. 1998). More recent studies introduce measures of foreign-policy similarity into gravity models: political differences and distance in United Nations General Assembly voting predict lower bilateral trade, with effects that vary across institutions and sectors (Umaña Dajud 2013; Mityakov et al. 2013; Kastner 2007; Chen and Zhou 2021). I extend this literature by using a continuous, chance-corrected measure of political distance for 166 countries over more than five decades. The estimation uses a gravity model and is fully theory-consistent.

A related literature studies the underlying mechanisms. Bilateral trust, public opinion, and politically motivated boycotts can shift consumer demand (Disdier and Mayer 2007; Guiso et al. 2009; Michaels and Zhi 2010; Pandya and Venkatesan 2016; Heilmann 2016). Governments may instead influence procurement and state-owned firms or restrict transactions through sanctions and export controls (Berger et al. 2013; Fuchs and Klann 2013; Davis et al. 2019; Lin et al. 2019; Li et al. 2021). Political tensions may also raise the cost of financing and securing transactions (Crozet and Hinz 2020), whereas sunk relationships can insulate trade from minor, short-lived disputes (Davis and Meunier 2011; Du et al. 2017). My dynamic and issue-specific estimates help distinguish these channels. The gradual and persistent response, its concentration in security and economic-policy disagreement, and its partial attenuation after controlling for sanctions are more consistent with a trade-cost channel than with a transitory demand shift.

Finally, the paper contributes to the literature on geoeconomic fragmentation. Recent work documents trade reallocation toward politically aligned blocs and quantifies uneven welfare losses from fragmentation (Gopinath et al. 2025; Campos et al. 2023; Javorcik et al. 2024). Closest to this paper, Chen et al. (2025) study geopolitics along value chains and Jiang et al. (2025) examine value-added exports. I instead estimate a continuous bilateral friction from long-run within-pair changes. The chance-corrected measure does not impose a common geopolitical axis and can be decomposed into security, economic, human-rights, and other issues. Embedding this friction in an Armington model links the mechanism evidence to the distributional welfare consequences of a historically grounded New Cold War realignment.

Taken together, the paper connects these literatures by treating political distance as a measurable bilateral trade friction. It combines long-run global evidence, diagnostics on mechanisms and the direction of causality, and structural general-equilibrium quantification to show how political disagreement shapes both trade flows and the welfare consequences of geopolitical realignment.

2 Gravity and political distance

In this section, I provide a brief review of the theoretical background of gravity equations and the data used for their estimation.

2.1 Conceptual framework

To fix ideas, consider a standard one-sector Armington model and augment it with a bilateral demand shifter as in Yotov et al. (2016) Political frictions may 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). 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.

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 $\sigma > 1$. Accordingly, the representative consumer in country j solves the problem

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

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. E_j denotes total expenditure in country j, while a larger $a_{ij} > 0$ lowers the relative utility weight attached to goods from i.

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 $\tau_{jj} = 1$, so that $\omega_{jj} = 1$. 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 $\omega_{ij} \equiv a_{ij}\tau_{ij}$ combines the inverse demand shifter and the iceberg trade cost. Political friction may affect both components. Hence, bilateral expenditures can 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, $\Pi_i^{1-\sigma} = \sum_j \left(\frac{\omega_{ij}}{P_j}\right)^{1-\sigma} \frac{E_j}{Y}$, and $P_j^{1-\sigma} = \sum_i \left(\frac{\omega_{ij}}{\Pi_i}\right)^{1-\sigma} \frac{Y_i}{Y}$. The outward multilateral resistance Π_i summarizes the average incidence of trade frictions on exports from i, while the inward multilateral resistance P_j summarizes destination j's access to alternative suppliers. A change in a bilateral friction therefore affects trade directly through ω_{ij} and indirectly through equilibrium changes in multilateral resistance, output, and expenditure. Because $\omega_{ij} = a_{ij}\tau_{ij}$,

$$\ln \omega_{ij} = \ln \overline{\omega}_{ij} + (\gamma_d + \gamma_s) pf_{ij},$$

where $\overline{\omega}_{ij} \equiv \overline{a}_{ij} \overline{\tau}_{ij}$. Substituting into structural gravity then yields

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

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, the structural components are absorbed by corresponding fixed effects, and β is estimated from changes in observed political distance within country pairs over time. Because observed political distance proxies for latent political friction, β need not equal δ, and aggregate bilateral expenditures cannot separately identify the demand- and cost-side components. For the counterfactual quantification, I express the estimated effect in iceberg-trade-cost-equivalent units. This conventional representation does not imply that the entire relationship arises from literal resource costs.

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 covers 5,694 final-passage resolutions. 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.[2] The panel covers only recorded final-passage votes, while amendments and paragraph votes are excluded.

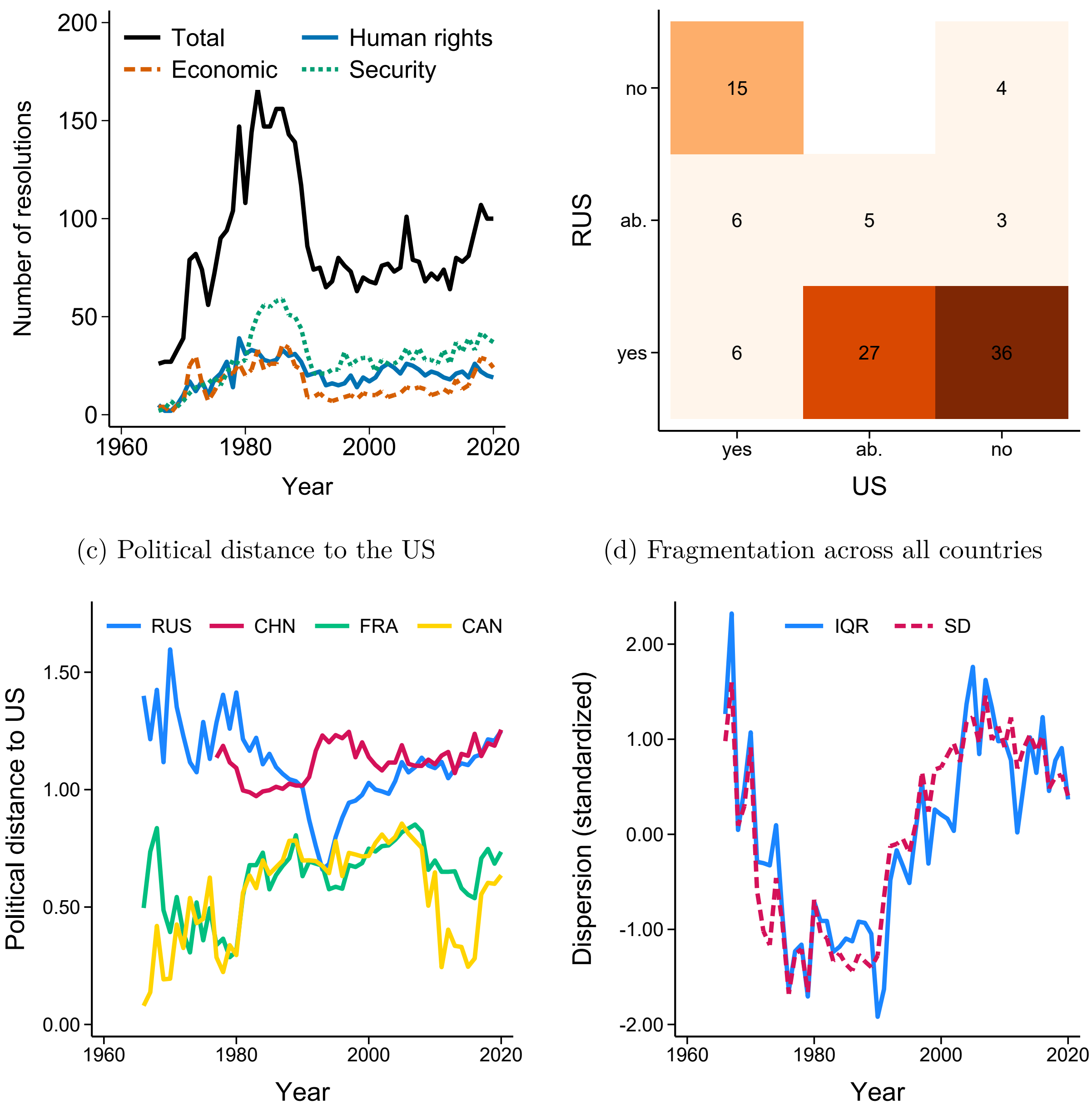
Figure 1a shows the total number of resolutions each year. On average, around 80 resolutions undergo final-passage voting per year. They generally cover a wide range of topics. Resolutions on conflicts and security, or human rights, however, are relatively frequent and are covered in 25 percent and 15 percent of resolutions, on average. The baseline measure uses all retained resolutions. I use alternative agreement statistics for robustness and topic-specific resolution samples to investigate mechanisms. 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.

Figure 1b shows the joint distribution of Russian and US votes in 1980 on 108 resolutions with recorded votes. Both countries cast the same vote on 15 percent of resolutions. On the remaining 85 percent of resolutions, they disagree, strongly so on more than half of all resolutions, where one country votes yes and the other no.

To quantify political distance based on countries' votes at the UNGA, I build upon existing measures of political similarity. For each country pair and year, I construct political distance from quantitative Cohen's κ, an agreement statistic for ordered vote categories. For each country pair i, j and year t, I use the set $\mathcal{R}_{ijt}$ of resolutions on which both countries are eligible and have a recorded vote. 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

[2] 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



(a) Resolutions at the UNGA

(b) Russian and US votes in 1980

(c) Political distance to the US

(d) Fragmentation across all countries

Notes: Panel (a): annual totals of the number of resolutions voted on in the UNGA and the number of resolutions on conflict (keyword in subject, title, or agenda title: conflict, ceasefire, peace, disarmament, proliferation, nuclear, chemical, biological, weapons, security) and human rights (human rights, discrimination, equality, minority, indigenous people). Panel (b): joint distribution of votes of the US and the Soviet Union (labelled as Russia) 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.

(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. Political distance is undefined when the common votes contain insufficient variation to compute κ; this occurs in only 0.09% (812 of 953,454) of country-pair-years. 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.

The median country-pair-year is based on 78 common resolutions. To validate the measure of political distance, I compare them with Häge (2017) for the overlapping sample. The preferred distance correlates at 0.988 with the corresponding measure in the FPSIM data. These comparisons validate the underlying records and implementation of the measure; the historical patterns below provide complementary evidence on its substantive interpretation. Appendix Section A.1 documents the construction, validation, and limitations in detail.

Figure 1c illustrates the resulting time-series. It shows the political distance of China, Russia, France, and Canada towards the US from 1966 to 2020. France and Canada are politically closer to the US than China and Russia, particularly during the Cold War. While the distance between the US and China remains relatively stable during the sample period, there is variation in the US distances to Russia, France, and Canada. The US-Russia distance is highest in 1970 at 1.70. Towards the end of the Cold War, it decreased by 60 percent, reaching its historical low (0.66) in 1993. Since then, it has increased steadily, reaching 1.25 in 2020, well below its historical high.[3]

The dispersion of these distances was high during the Cold War and rather low after the Cold War. Figure 1d shows that this pattern also holds more generally when considering the dispersion of political distance (measured by its standard deviation or interquartile range in a given year) across all country pairs. Dispersion was high during the Cold War, reached its minimum at the end of the Cold War, and has since then increased again.

[3]This behavior is not shared by another frequently used measure of political distance: the ideal-point distance of Bailey et al. (2017). Figure A.1 compares that distance and my κ-based measure. It shows that for the distances between the US and Russia, as well as the US and China, only the κ- based measures have increased since 2015, during a period of heightened geopolitical tensions. In contrast, ideal-point distances have remained flat or decreased moderately over the same period.

2.3 Trade and trade policy

For intra-national 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 165 countries and 9 industrial 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. Specifically, domestic trade is computed by subtracting sectoral exports from gross production. Observations on domestic trade are set to missing in case the trade and production data imply negative domestic trade. For the main analysis, I sum nominal values reported at the sectoral level to the bilateral level, matching both the unit of observation in the model and for political distance. For a complementary heterogeneity exercise, however, I also use the sectoral data. Observations are identified by exporter, importer, sector and year. Each observation covers the values reported by the exporter and the importer, as well as a combined variable that is based on importer-reported values complemented with exporter-reported values when importer-reported values are missing. Throughout, I use the combined measure.

While trade flows and political distance are essential components of the baseline specification, policy variables (RTAs, sanctions, and tariffs) are potential mechanisms or mediators. For trade agreements, I use Mario Larch's Regional Trade Agreements Database from Egger and Larch (2008), which is also used by 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 among different types of sanctions, such as trade, travel, and arms sanctions. I use the dyadic version of the database and construct a simple indicator for any active sanctions between the exporter and the importer. Tariff data with sufficient coverage is only available since 1988. I use Feodora Teti's Global Tariff Database (v_beta1-2024-12) from Teti (2024). It covers 200 countries as exporters and importers from 1988 until 2021 and corrects for measurement error in commonly used data provided by World Integrated Trade Solution (WITS). I use the unweighted average bilateral tariff.

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

2.4 Descriptive statistics

The combined panel covers trade within and between UN member states, political distance, and trade costs. Table 1 reports descriptive statistics for the dimensions of the panel in Panel (a) and the main variables in Panel (b).

The panel spans 55 years, from 1966 to 2020, averaging more than 18,000 observations per year, with a higher number of observations available in recent years. See Figure A.2a for details. Countries are identified by dynamic codes from TradeProd, which account for territorial changes, distinguishing, for example, the Soviet Union and Russia, as well as Germany before and after the reunification in 1990. Overall, the panel comprises 166 countries, with an average of more than 6,000 observations per country as an importer and as an exporter. The available number of directed country pairs (26,112) is quite close to, but below the fully-square benchmark ($27,556 = 166^2$). Hence, a very small number of country pairs are never observed trading, likely due to data availability or embargoes. The panel is unbalanced: country pairs are observed for 39 years on average and for 40 years at the median, with an interquartile range of 28 to 55 years. More than one quarter of all country pairs are observed throughout the full 55-year sample period. See Figure A.2b for a histogram of observations per country pair.

Overall, the panel features 1,011,431 observations at the exporter-importer-year level, as shown in Panel (b) of Table 1. Trade flows include intra-national and international transactions, measured in millions of current US dollars (USD). The distribution is strongly right-skewed, with a mean of 920 million USD and a median of 0.18 million USD. Around 30 percent of all observations record zero trade. Cross-sectional variation is substantial: the overall standard deviation is around 50 billion USD, far exceeding the average within-country-pair standard deviation of 630 million USD. Political distance averages 0.82. For example, this corresponds to the political distance between the US and Russia in the early 1990s, after the signing of the START II treaty limiting nuclear arsenals. Political distance is zero for domestic trade, which accounts for one percent of all observations. Variation is sizable both across and within pairs, with the within-pair standard deviations around half of the total. The mean within-pair SD is 0.16, which I will use later to assess the magnitude of the impact of political distance on trade. This standard deviation closely matches that of the US and Poland. See Figure A.5 for the full distribution of within-pair SDs of political distance and other trade costs.

Regional trade agreements (RTAs) occur in 15 percent of observations, with most variation concentrated across pairs rather than over time, although within-pair variation remains non-negligible. Tariffs average ten percent but are only available starting in 1988. As with RTAs, most variation reflects cross-sectional differences, but there is also meaningful within-pair

Table 1: Descriptive statistics

(a) Panel dimensions

Variable	Unique values	Mean	Median	1. Quartile	3. Quartile
Year	55	18,390	20,690	14,469	23,217
Origin country	166	6,093	7,032	4,434	7,216
Destination country	166	6,093	7,194	4,482	7,684
Country pairs	26,112	39	40	28	55

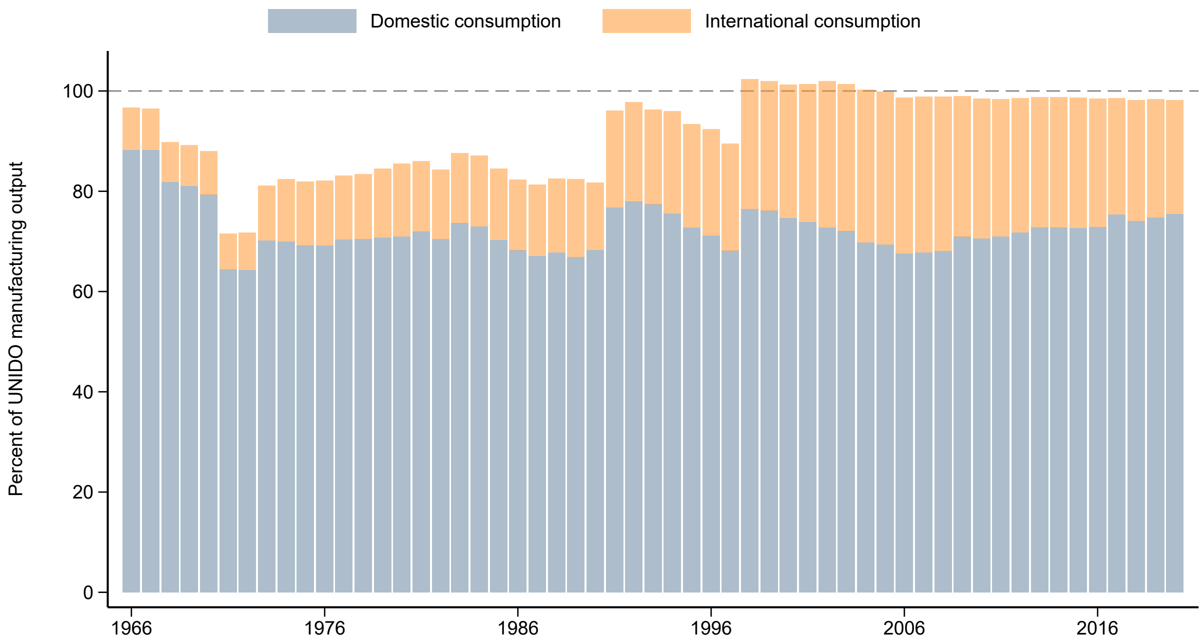
(b) Panel variables and identifying variation

Variable	Mean	Median	SD Total	SD Within	$\frac{\text{Within}}{\text{Total}}$ (%)	Pairs w. SD>0 (%)
Core variables						
Trade	919.31	0.18	50,424.79	630.24	1.25	100.00
Political distance	0.82	0.90	0.30	0.16	53.36	99.37
Potential channels						
Any trade agreement	0.15	0.00	0.36	0.11	29.67	25.25
Any sanction	0.14	0.00	0.35	0.16	46.60	40.66
Tariffs	0.10	0.08	0.10	0.03	35.10	95.75

Notes: The table summarizes the dimensions and variables of the estimation sample, which contains 1,011,431 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.1 for detailed coverage by country.
Panel (b): Tariff statistics are based on 697,300 observations because coverage begins in 1988. 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. Tariffs are the unweighted average bilateral tariff. 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.

movement over time. Sanctions occur in 14 percent of country-pair-year observations and vary over time for 41 percent of country pairs; their mean within-pair standard deviation amounts to 47 percent of their total standard deviation. The final column of the table reports for each variable the share of country pairs with some time-series variation in that variable. For trade flows, there is variation in all panels. For trade costs, political distance varies over time for virtually all international pairs. For other trade costs, fewer panels contribute to identification: over time, tariffs vary in 96 percent of covered country pairs and RTAs in only 25 percent. The considerable within-country pair variation in all trade costs (see the last two columns of Panel (b) in Table 1), combined with the substantial number of country pairs, is central for identification. In particular, it enables the estimators to exploit changes within

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 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.

country pairs rather than cross-sectional differences.

Figure 2 assesses the global coverage of the estimation sample and highlights the importance of domestic trade for theory-consistent gravity estimation. Structural gravity describes how total output is allocated across domestic and foreign destinations. Domestic flows therefore provide the natural benchmark against which international trade costs are identified. Excluding them would condition the analysis on exports alone and omit the largest destination for manufacturing output. In the sample, domestic consumption accounts for roughly 70–90% of world manufacturing output, while international consumption accounts for a considerably smaller, though increasing, share. Together, the included flows cover approximately 92% of UNIDO-reported manufacturing output on average and about 98% in 2020. The pronounced decline in coverage during the early 1970s primarily reflects differences in historical country coverage: Soviet manufacturing output enters the UNIDO aggregate in 1971 without corresponding domestic flows in TradeProd, while West Germany is excluded until 1973 when it joins the UN. Appendix Figures A.3 and A.4 compare the sources for identical country-years and show that TradeProd totals closely track matched UNIDO output, averaging approximately 99%.

3 Results

This section introduces the estimation framework and establishes the total effect of political distance on bilateral trade. I then examine the timing of the trade response, investigate channels and competing explanations, and present additional robustness checks.

3.1 Estimation framework

To estimate 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 and $\mathbf{Z}_{ijt}$ contains conditioning information, that is, political distance and fixed effects. Because observed political distance proxies for latent political friction, the reduced-form coefficient β need not equal the structural coefficient δ. The data include domestic and international trade flows and retain reported zero-valued flows. Fixed effects at the exporter-year and importer-year levels, $\pi_{i,t}$ and $\pi_{j,t}$, account for multilateral resistances (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. They constitute mechanisms through which political distance may affect trade and are therefore part of the (average) total effect that combines demand- and cost-side consequences of changes in political distance. 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). Standard errors are clustered at the undirected country-pair level, since political distance is undirected as well.

3.2 Main results

Table 2 presents the baseline results. Columns (1) to (3) use alternative measures of political distance, while Columns (4) to (6) explore the impact of militarized interstate disputes (MID) on the results.

Column (1) uses the preferred quantitative Cohen's κ measure defined in Equation (2). The estimated coefficient is -0.216 with a standard error of 0.072 and is statistically significant at the 1 percent level. An increase in political distance equal to its mean within-pair standard deviation of 0.158 is associated with a 3.36 percent decrease in bilateral trade. The delta-method standard error of this percentage effect is 1.10 percentage points.

Columns (2) and (3) instead use qualitative Cohen's κ and quantitative Scott's π, respectively. Because the measures have different scales, their raw coefficients are not directly comparable. Their standardized effects are nevertheless very similar: an increase equal to the corresponding mean within-pair standard deviation is associated with 3.21 percent lower trade using qualitative Cohen's κ and 3.04 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.7 reports complementary estimates using political distance constructed from alliance membership, which also show a significant and negative association of that measure of political distance and trade.

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 (Thoenig 2024; Korovkin and Makarin 2023). I therefore assess whether the results depend on these episodes in Columns (4) to (6). 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 (4). It shows that the impact of a 1-SD increase in political distance is four percent lower compared to the baseline. Comparing the number of observations, the column also shows that ongoing MIDs occur in only 3,482 bilateral observations, approximately 0.4 percent of the MID-coverage sample. Column (5) controls for ongoing MIDs, which may constitute either an omitted variable or a channel through which political distance impacts trade. Compared to Column (4), the 1-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) two years before

Table 2: Main results

	(1)	(2)	(3)	(4)	(5)	(6)
Quantitative Cohen's κ	-0.216***			-0.202***	-0.203***	-0.240***
	(0.072)			(0.078)	(0.077)	(0.077)
Qualitative Cohen's κ		-0.244***				
		(0.083)				
Scott's π			-0.156**			
			(0.065)			
Ongoing MID					0.013	
					(0.027)	
Mean within-pair SD	0.158	0.134	0.198	0.162	0.162	0.161
One-SD effect on trade (%)	-3.36	-3.21	-3.04	-3.21	-3.23	-3.80
Delta-method SE	1.10	1.08	1.24	1.22	1.21	1.19
Observations	1,011,431	1,011,431	1,011,431	866,261	866,261	857,115

Notes: The dependent variable is bilateral exports. All specifications are estimated by PPML and include origin-year, destination-year, origin-destination-pair, and international-trade-year fixed effects. Standard errors, reported in parentheses, are clustered by undirected country pair. Columns (1)–(3) separately use quantitative Cohen's κ, qualitative Cohen's κ, and Scott's π as measures of political distance. Column (4) uses quantitative Cohen's κ and restricts the sample to the temporal coverage of the MID 5.0 data (1966–2014 in the estimation sample). Column (5) uses the same sample and additionally controls for an indicator equal to one when the origin and destination are opponents in an ongoing militarized interstate dispute in the year of observation. Column (6) excludes bilateral observations between MID opponents during the dispute and for two years before and afterward. Opponent exclusions are applied symmetrically to both directions of trade. The mean within-pair standard deviation is calculated separately using each column's realized estimation sample. The one-SD effect is $100[\exp(\widehat{\beta} \times SD) - 1]$; its standard error is calculated using the delta method. Observation counts reflect any singleton or separated observations removed by PPML. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

and after any MIDs from the estimation sample and re-estimate the baseline specification. The results are in Column (6). While the 1-SD impact is about 18 percent larger than the baseline, it is nonetheless well within the baseline confidence interval. Overall, there is little evidence that MIDs affect the results, either as an omitted variable or as high-leverage observations.

Appendix Table A.2 examines alternative estimators, outcome transformations, sampling intervals, and fixed-effect structures in specifications that condition on tariffs and RTAs. The estimated one-standard-deviation effect remains negative and statistically significant when I use PPML or OLS, retain or exclude zero trade flows, and sample the data annually or at three-, four-, or five-year intervals. Omitting the international-trade-by-year fixed effects also leaves the estimate essentially unchanged. By contrast, when I discard domestic trade and identify the coefficient only from international flows, the estimate falls to -0.3 percent and is statistically insignificant. This qualification underscores the role of domestic trade as the theory-consistent benchmark for identifying changes in international trade costs.

Appendix Table A.5 additionally groups country pairs according to the geopolitical blocs of Gopinath et al. (2025). A one-standard-deviation increase in political distance is associated with 3.47 percent lower trade within aligned blocs, 2.50 percent lower trade across the US and China blocs, and 3.76 percent lower trade for pairs involving at least one non-aligned country. I cannot reject equality of these effects ($p = 0.859$). Thus, the average relationship is not driven exclusively by trade across opposing blocs and also holds within broad geopolitical groupings.

The average relationship may conceal substantial heterogeneity across time and sectors, particularly given changes in the international political and trading system over the sample period and differences in the composition of traded goods. To investigate these dimensions, I allow the coefficient for political distance to vary across major political episodes and across TradeProd sectors. The results are in Figure 3.

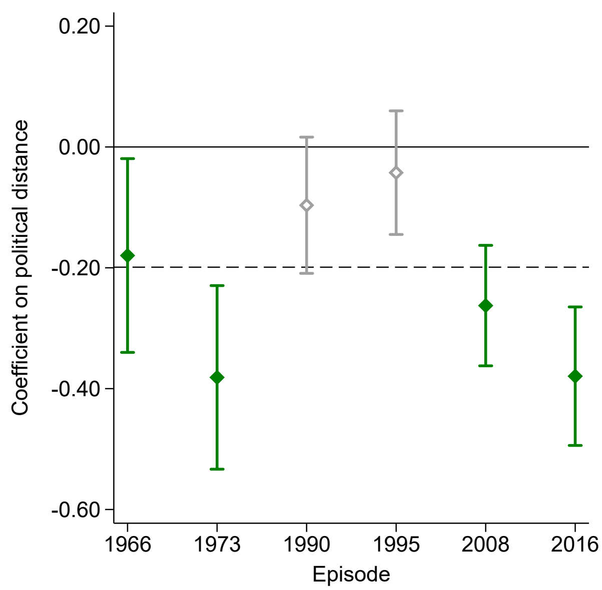
Panel (a) shows the heterogeneity across time. From 1966 until 1972, the coefficient is -0.18, close to the overall estimate, and significant at the five percent level. During the Cold War, the coefficient reaches -0.38, the most negative point estimate. After 1990, the coefficient quickly becomes insignificant until the onset of the Great Financial Crisis in 2008. In the post-GFC period, the coefficient again turns significant and reaches -0.27. After 2016, a period characterized by economic fragmentation and the pandemic, the coefficient becomes more negative and, at -0.38, reaches levels comparable to those of the Cold War.

Panel (b) shows negative coefficients in all nine sectors, ranging from -0.35 for food and -0.34 for machines to -0.12 for wood and paper. A joint Wald test rejects equality of all sector coefficients ($p = 0.002$), showing that the aggregate estimate masks significant sectoral heterogeneity. Given TradeProd's nine broad categories, however, this pattern is descriptive and does not by itself identify a specific demand- or trade-cost mechanism.

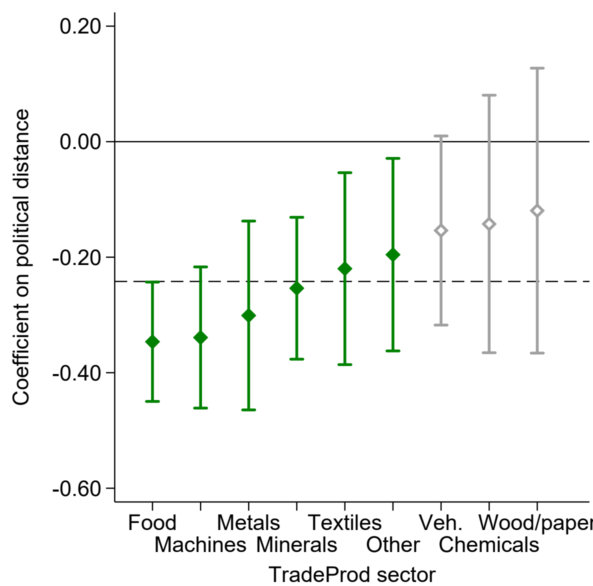
Overall, the results imply that an increase in political distance equal to its mean within-pair standard deviation is associated with approximately 3.0–3.4 percent lower trade. For the preferred estimate, the 95 percent confidence interval covers declines of 1.2 to 5.5 percent. Because the specifications 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 section examines the timing of the response and the extent to which it supports a causal interpretation.

Figure 3: Heterogeneity across time and sectors

(a) Across time



(b) Across sectors



Notes: Panel (a) interacts political distance in the baseline specification with major political episodes; its x-axis reports the starting year of each episode: 1966–1972 (late Bretton Woods), 1973–1989 (late Cold War), 1990–1994 (post-Cold War), 1995–2007 (globalization), 2008–2015 (Great Financial Crisis), and 2016–2020 (geoeconomic fragmentation). Panel (b) uses sector-level TradeProd observations and allows the coefficient for political distance to vary across its nine sectors; sectors are ordered from the most negative to the least negative point estimate. It includes exporter–year–sector, importer–year–sector, country-pair–sector, and international-trade–year–sector fixed effects. Both panels show point estimates and 95 percent confidence intervals based on standard errors clustered by undirected country pair. Coefficients significant at the 5 percent level are green; all others are grey. Dashed lines show the relevant pooled estimate: the aggregate baseline estimate from Column (1) of Table 2 in Panel (a), and the pooled estimate using sector-level observations from Table A.4 in Panel (b). Joint Wald tests reject equality of the coefficients across periods ($p < 0.001$) and sectors ($p = 0.002$).

3.3 Timing and dynamic responses

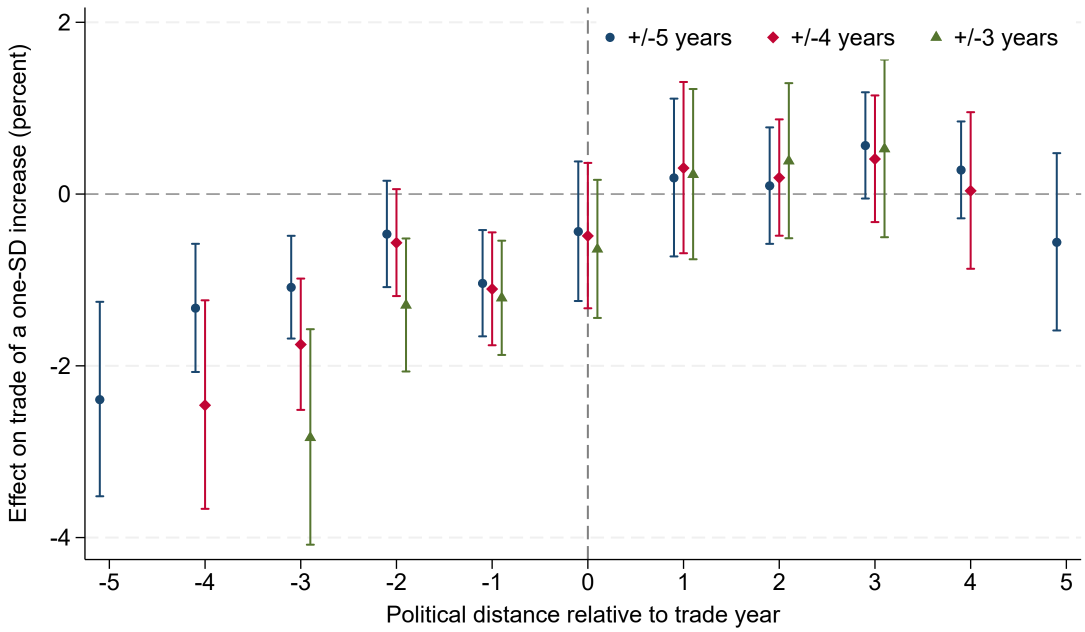
A reverse channel is economically plausible: Kleinman et al. (2024) show that greater economic dependence can foster political alignment. To examine whether changes in trade precede changes in political distance, I augment the baseline specification (3) with leads and lags of political distance, following Baier and Bergstrand (2007). This provides a Granger-style timing test (Wooldridge 2008). 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 4 shows the results from including $K \in \{5, 4, 3\}$ leads and lags. Additional specifications in Table A.8 also use $K \in \{2, 1\}$. For $K = 5$, coefficients on leads are jointly insignificant at the five percent level ($p = 0.08$). For all other specifications, lead coefficients are jointly insignificant at all conventional levels ($K = 4$: 0.636; $K = 3$: 0.794; $K = 2$: 0.472; $K = 1$: 0.579). Lagged coefficients, on the other hand, are jointly significant at all conventional significance levels ($p < 0.001$) and negative in all specifications.

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), shown in Figure A.6. The response of both ln-transformed and IHS-transformed exports suggests generally little and insignificant movement before the change in political distance. Afterwards, the estimated responses quickly become increasingly negative after approximately two to three years and remain negative throughout the ten-year horizon. After five years, the point estimates imply a decline of approximately 0.4 percent using the IHS transformation and 0.7 percent using logs. By year ten, the corresponding declines are approximately 0.4–0.5 and 1.0–1.2 percent.

The results are similar when the analysis is restricted to international trade and when every horizon is estimated using only observations for which the transformed trade outcome is observed throughout the complete window from six years before to ten years after the change in political distance. Appendix Table A.9 reports joint tests for these specifications. Across all eight outcome, trade-sample, and horizon-sample combinations, the joint pre-period p-values range from 0.072 to 0.309, so none reject at the five percent level. The post-treatment coefficients are jointly significant at the five percent level in all IHS and fixed-sample log specifications; in the horizon-specific log specifications they are jointly significant at the ten percent level ($p = 0.086$ for all trade and $p = 0.097$ for international trade). Overall, the results indicate no robust evidence that trade declines beforehand, while higher political distance consistently predicts subsequent declines.

Figure 4: Lags and leads of political distance



Notes: I augment the baseline specification (3) with leads and lags of political distance. Coefficients are converted into the percentage change in trade associated with an increase in political distance equal to the mean across country pairs of its within-pair standard deviation. All specifications use the estimation sample of the five-year specification. Whiskers show pointwise 95 percent confidence intervals based on standard errors clustered by undirected country pair. See Table A.8 for additional results.

The delayed and persistent response provides suggestive evidence about the underlying channels. This timing complements evidence that bilateral trust and public opinion shape trade and that politically motivated boycotts can generate sharp, sometimes short-lived responses (Disdier and Mayer 2007; Guiso et al. 2009; Michaels and Zhi 2010; Pandya and Venkatesan 2016; Heilmann 2016). It is less consistent with a purely contemporaneous demand shock and more consistent with mechanisms involving gradual adjustment of firms and bilateral trading relationships. However, timing alone cannot distinguish between these explanations: consumer responses can persist, while changes in trade costs can also affect trade immediately. I therefore investigate the underlying channels more directly in the following section.

3.4 Channels

The previous sections showed that political distance has a significant and negative effect on 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. In this section, I provide evidence on whether the results are more consistent with the demand-shifter channel or the trade-cost channel. Specifically, I first account for the trade-cost channel explicitly and augment the baseline specification with classical trade-policy variables. Second, I use topic-specific measures of political distance. Overall, the evidence is more consistent with the trade-cost channel.

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. The results are in Table 3.

The first column reports the specification without these potential channels. Controlling for RTAs in Column (2) attenuates the one-standard-deviation effect from -3.36 to -3.13 percent, or by about 7 percent. Controlling for sanctions in Column (3) reduces it to -2.33 percent, an attenuation of about 31 percent. With both controls in Column (4), the effect is -2.15 percent. These changes are consistent with sanctions accounting for an economically meaningful part of the relationship, but they are not causal mediation shares.

Tariff data begin only in 1988, so Appendix Table A.3 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-standard-deviation effect is -1.41 percent. Adding tariffs alone changes it only modestly, to -1.28 percent, whereas adding sanctions reduces it to -0.57 percent; with tariffs, RTAs, and sanctions together, it is -0.47 percent. The tariff coefficient itself is negative and precisely estimated. Because the shorter common-sample baseline already differs from the full-sample estimate, these comparisons are informative about attenuation within that sample rather than about the source of the cross-sample difference.

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 4 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

Table 3: Classical trade-policy channels

	(1)	(2)	(3)	(4)
Political distance				
Preferred κ	-0.216***	-0.201***	-0.149**	-0.137**
	(0.072)	(0.066)	(0.073)	(0.069)
Potential channels				
Any RTA		0.134***		0.129***
		(0.037)		(0.037)
Any GSDB sanction			-0.119***	-0.115***
			(0.024)	(0.023)
Mean within-pair SD	0.158	0.158	0.158	0.158
One-SD effect on trade (%)	-3.36	-3.13	-2.33	-2.15
Delta-method SE	1.10	1.02	1.13	1.07
Observations	1,011,582	1,011,582	1,011,582	1,011,582

Notes: All specifications include exporter–year, importer–year, country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by country pair, and all columns use the same sample. Reported effect sizes are the exact percentage change in expected exports associated with an increase in the preferred measure of political distance equal to its mean within-pair standard deviation; standard errors are calculated using the delta method. RTAs and sanctions are potentially endogenous policy channels. Changes in the political-distance coefficients are therefore descriptive attenuation exercises rather than causal mediation estimates.

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. The first column in Table 4 shows that in the sample for which all topic-specific political distances can be computed, the baseline result is virtually identical to the conditional results from the overall sample (see Column (4) in Table 3): a one-standard-deviation increase is associated with 2.24% less trade, conditional on RTAs, sanctions, and fixed effects.

In Column (2), I measure political distance using UNGA votes on human rights resolutions. Its coefficient is negative and insignificant. For Column (3), political distance is based on economic resolutions only. Here, the coefficient is negative and highly significant. Its magnitude (effect of a 1-SD increase) is around 25% lower compared to the overall measure in Column (1). Column (4) uses only security resolutions. The coefficient is negative and highly significant. Its magnitude (-2.32%) is the largest among all measures.

The final column simultaneously considers all topic-specific distances in a horse race. I test the hypothesis that topic-coefficients are equal and reject this H_0 with a p-value of 4.4%.

In line with the topic-specific regressions, the impact of human rights distance and economic distance is small (and not significant in this case). Security distance, however, again turns out to have the strongest effect, which is highly significant as well.

Table 4: Political distance for specific topics

	(1)	(2)	(3)	(4)
Political distance				
Security distance	-0.131***			-0.127***
	(0.039)			(0.039)
Human-rights distance		-0.007		0.050
		(0.052)		(0.054)
Economic distance			-0.070**	-0.034
			(0.031)	(0.030)
Potential channels				
RTA	0.129***	0.130***	0.130***	0.131***
	(0.038)	(0.038)	(0.038)	(0.038)
Any GSDB sanction	-0.109***	-0.129***	-0.118***	-0.109***
	(0.024)	(0.025)	(0.024)	(0.024)
One-SD effect (%)	-2.59	-0.16	-1.69	
Observations	940,741	940,741	940,741	940,741
Topics jointly significant: p-value				0.002
Human rights = security: p-value				0.020
Human rights = economic: p-value				0.193

Notes: Results are for aggregate goods 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 reports the percentage change in expected trade associated with a one-mean-within-pair-standard-deviation increase in the relevant topic-specific distance, $100 \cdot (\exp(\bar{\sigma}_{pd} \cdot \beta_{pd}) - 1)$; its significance is calculated using the delta method. All specifications include fixed effects for importer-year, exporter-year, pair, and international-trade-by-year. Standard errors are clustered at the importer–exporter level and listed in brackets below the point estimate. *** p<0.01, ** p<0.05, * p<0.1.

The stronger security-distance relationship is more consistent with firm- and trade-cost-related channels than with a predominantly consumer-demand explanation.

Appendix Table A.6 probes whether this security-distance result merely reflects sanctioned dyad-years. On the matched sample, a one-standard-deviation increase in security distance is associated with 3.92 percent lower trade without the sanctions control and 2.85 percent lower trade after controlling for sanctions. The estimate remains negative and statistically significant when sanctioned observations are excluded (-1.55 percent). An interaction specification yields effects of -2.74 percent without an active sanction and -3.39 percent when a sanction is active; a joint-model Wald test does not reject equality of these effects ($p = 0.465$). Thus, sanctions attenuate the average security-distance coefficient, but the negative association is not confined to observations under active sanctions, and there is no evidence that its slope differs by sanctions status.

4 A counterfactual New Cold War

The previous section quantified how political distance reduces bilateral trade. This section expands the analysis by quantifying the general equilibrium impact of counterfactual changes in political distances on real income, while other trade costs remain unchanged.

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. To explore the potential impact of a continuation of this trend, I set political distances to the values observed in 1962, the year of the Cuban Missile Crisis (or approximate those values when they are not directly computable). To isolate the impact of these changes in political distances on real income, I leave RTAs and tariffs unchanged. Hence, these estimates approximate the actual effects, where these other trade costs would likely change as well.

I evaluate this counterfactual using the computable general equilibrium features of the gravity model and the TPc file of the TradeProd database. Unlike the TPe file used for estimation, TPc provides complete (squared) bilateral trade matrices by industry and year, together with a more restrictive series that is squared across all industries within each year. To complete these matrices, TPc extrapolates missing domestic sales using country-specific relationships between exports and gross output where available and industry averages otherwise. Domestic sales that would be negative based on reported production are treated as missing and extrapolated in the same way. These complete matrices are required to solve the model's market-clearing conditions in the counterfactual equilibrium.

4.1 Counterfactual political distances

The starting point is the 2020 cross-section of UNGA members, for which trade data are available. This cross-section features 158 countries or more than 24,000 country pairs.

To construct counterfactual distances, I apply a set of hierarchical rules. For pairs within the former Soviet Union, political distances are set to zero. Pairs with exactly one former Soviet Republic use the historical 1962 distance between the Soviet Union and the non-Soviet partner. When both modern-day countries (or their legal ancestors) were UN members during the Cold War as well, I use the observed distance during the Cold War for the counterfactual. Finally, when one or both countries were not UN members during the Cold War, I impute distances based on the Cold War alignments of their ten closest modern-day political neighbors. For around one-third of modern-day country pairs, both countries were UN members during the Cold War. For another third of all modern-day country pairs, I impute values based on their modern-day political neighbors, and the remaining third of country pairs falls into the other cases.

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

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

The changes in political distances are generally centered around 0 and less than 1 in absolute value for 97 percent of all bilateral pairs. On average, political distance decreases by 0.05 units. For example, the distance between the US and Russia in the counterfactual scenario increases by $\Delta^{cf} = 1.67 - 1.25 = 0.42$ units of political distance — about one third of its actual value in 2020. Conditional on the baseline estimate, bilateral trade would then be expected to drop to $\exp(-0.38 \cdot 0.42) = 0.85$ or 85 percent of its actual value in 2020 in partial equilibrium.

4.2 General equilibrium effects

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

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

As Figure 3 shows, there is sizable heterogeneity across time in the effect of political distance, δ. To best reflect the effect of political distance in 2020, I use the coefficient estimated for the post-2016 period: $\delta = -0.38$.[5] This estimate captures the total effect of political distance, including the part associated with sanctions. I repeat the counterfactual using the corresponding estimate from a specification that includes sanctions as a control. This second exercise holds the sanctions channel fixed and captures the remaining association between political distance and trade; the difference between the specifications should not be interpreted as a causal mediation effect. Hence, 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 my estimate for the trade elasticity of 3.02 and the `ge_gravity` procedure of Baier et al. (2019), which solves a one-sector Armington-CES model.[6] To account for estimation uncertainty, I bootstrap welfare effects and average across 5 000 draws from the joint distribution of δ and θ in each specification. While most of the results focus on welfare effects, I also quantify the degree of trade reshuffling by the standard deviation of the relative change in exports by country. This measure captures the dispersion of bilateral trade adjustments, with higher values indicating stronger export reorientation.

Table 5 reports the welfare effects. The average effect across all countries is small

[5] Using the overall estimate of $\delta = -0.2$ reduces welfare effects roughly by half.

[6] The results are qualitatively similar with θ equal to 4 or 10, as in Simonovska and Waugh (2014), with average welfare effects 15 to 65 percent lower.

Table 5: Welfare effects of a New Cold War

Country group	N	Total effect			Sanctions held fixed		
		Mean	Min	Max	Mean	Min	Max
Americas	29	2.21	-0.79 (BHS)	6.56 (SLV)	1.80	-0.66 (BHS)	5.32 (SLV)
Former Warsaw Pact	22	-2.05	-6.58 (HUN)	3.33 (TJK)	-1.81	-5.65 (HUN)	2.69 (TJK)
Other Europe	25	-1.50	-7.87 (SVN)	3.31 (IRL)	-1.28	-6.65 (SVN)	2.68 (IRL)
Rest of the world	82	0.12	-2.66 (CYP)	2.60 (COG)	0.09	-2.25 (CYP)	2.15 (COG)
All countries	158	-0.06	-7.87 (SVN)	6.56 (SLV)	-0.08	-6.65 (SVN)	5.32 (SLV)

Notes: Welfare changes are defined as $100(\widehat{W}_i - 1)$ and reported in percent. Means are unweighted country averages; minima and maxima are country-level extrema, with the corresponding country shown in parentheses. Country groups are mutually exclusive and exhaustive; the final row summarizes the full sample. *Total effect* uses the political-distance estimate obtained without controlling for sanctions. *Sanctions held fixed* uses the estimate obtained with sanctions included as a control. All other counterfactual inputs are identical across specifications.

under both specifications: -0.06 percent when the counterfactual incorporates the total estimated effect of political distance and -0.08 percent when sanctions are held fixed. These global averages, however, conceal substantial regional redistribution. Under the total-effect specification, welfare increases by 2.21 percent on average in the Americas, while it falls by 2.05 percent in the former Warsaw Pact and by 1.50 percent in the rest of Europe. The largest country-level changes range from a loss of 7.87 percent in Slovenia to a gain of 6.56 percent in El Salvador.

Holding sanctions fixed reduces the magnitude of both regional gains and losses: the average gain in the Americas falls to 1.80 percent, while average losses fall to 1.81 percent in the former Warsaw Pact and 1.28 percent in the rest of Europe. The corresponding country-level range narrows to -6.65 percent in Slovenia and 5.32 percent in El Salvador. The slightly more negative global mean arises because gains as well as losses shrink when sanctions are held fixed. The comparison is consistent with sanctions accounting for part of the total effect of political distance, but it is not a causal mediation exercise. Figure A.7 in the appendix shows the geographic distribution of welfare changes and trade reshuffling.

5 Conclusion

This paper quantified the impact of political distance on trade using a structural gravity framework with more than 50 years of bilateral data. Using UNGA voting disagreement as a revealed proxy for latent bilateral political friction, I estimate that a one-standard-deviation deterioration in measured political alignment is associated with more than 3% lower trade.

The relationship is robust to alternative measures and specifications, and the dynamic estimates show little evidence that trade declines before political distance rises. Sectoral estimates reveal significant heterogeneity across broad product groups, although they do not by themselves identify the underlying mechanism. The counterfactual shows that political realignment alone can have a meaningful impact on welfare, even when formal trade policies remain unchanged.

Overall, these findings suggest that political distance has long been a salient determinant of trade, not merely a potential future risk. The counterfactual *New Cold War* scenario shows that welfare losses would be highly concentrated in Europe, affecting both EU members and non-members alike. In this scenario, former Soviet Republics, such as Latvia, Estonia, and Lithuania, are politically realigned with Russia and away from their main modern-day trading partners in Europe, leading to substantial welfare losses. These results highlight the economic importance of political cohesion in Europe: sustaining the independence and stability of Eastern European democracies benefits not only their welfare but also the broader region's welfare.

Future research could examine consumers' and firms' responses in more detail to further disentangle the mechanisms.

References

Alesina, Alberto and David Dollar (2000). “Who Gives Foreign Aid to Whom and Why?” *Journal of Economic Growth*, 5(1): 33–63.

Antràs, Pol and C. Fritz Foley (2015). “Poultry in Motion: A Study of International Trade Finance Practices”. *Journal of Political Economy*, 123(4): 853–901.

Armington, Paul S. (1969). “A Theory of Demand for Products Distinguished by Place of Production”. *Staff Papers (International Monetary Fund)*, 16(1): 159–178.

Baier, Scott L. and Jeffrey H. Bergstrand (2007). “Do free trade agreements actually increase members’ international trade?” *Journal of International Economics*, 71(1): 72–95.

Baier, Scott L., Yoto V. Yotov, and Thomas Zylkin (2019). “On the widely differing effects of free trade agreements: Lessons from twenty years of trade integration”. *Journal of International Economics*, 116: 206–226.

Bailey, Michael A., Anton Strezhnev, and Erik Voeten (2017). “Estimating Dynamic State Preferences from United Nations Voting Data”. *The Journal of Conflict Resolution*, 61(2): 430–456.

Baldwin, Richard and Daria Taglioni (2007). “Trade Effects of the Euro: a Comparison of Estimators”. *Journal of Economic Integration*, 22(4): 780–818.

Berger, Daniel, William Easterly, Nathan Nunn, and Shanker Satyanath (2013). “Commercial Imperialism? Political Influence and Trade during the Cold War”. *American Economic Review*, 103(2): 863–896.

Bergstrand, Jeffrey H., Mario Larch, and Yoto V. Yotov (2015). “Economic integration agreements, border effects, and distance elasticities in the gravity equation”. *European Economic Review*, 78: 307–327.

Boehm, Christoph E., Andrei A. Levchenko, and Nitya Pandalai-Nayar (2023). “The Long and Short (Run) of Trade Elasticities”. *American Economic Review*, 113(4): 861–905.

Campos, Rodolfo G., Julia Estefania-Flores, Davide Furceri, and Jacopo Timini (2023). “Geopolitical Fragmentation and Trade”. *Journal of Comparative Economics*, 51(4): 1289–1315.

Chen, Qin and Yi Zhou (2021). “Whose Trade Follows the Flag? Institutional Constraints and Economic Responses to Bilateral Relations”. *Journal of Peace Research*, 58(6): 1207–1223.

Chen, Renjing, Chenxin Jin, Wei Jin, Bin Sheng, and Guanxiang Wang (2025). “Geopolitics along the Value Chains”. *China Economic Review*, 90: 102341.

Cohen, Jacob (1968). “Weighted kappa: Nominal scale agreement provision for scaled disagreement or partial credit.” *Psychological bulletin*, 70(4): 213.

Correia, Sergio, Paulo Guimarães, and Thomas Zylkin (2020). "Fast Poisson estimation with high-dimensional fixed effects". *Stata Journal*, 20(1): 95–115.

Crozet, Matthieu and Julian Hinz (2020). "Friendly fire: the trade impact of the Russia sanctions and counter-sanctions". *Economic Policy*, 35(101): 97–146.

Davis, Christina L., Andreas Fuchs, and Kristina Johnson (2019). "State Control and the Effects of Foreign Relations on Bilateral Trade". *Journal of Conflict Resolution*, 63(2): 405–438.

Davis, Christina L. and Sophie Meunier (2011). "Business as Usual? Economic Responses to Political Tensions". *American Journal of Political Science*, 55(3): 628–646.

Disdier, Anne-Célia and Thierry Mayer (2007). "Je t'aime, moi non plus: Bilateral Opinions and International Trade". *European Journal of Political Economy*, 23(4): 1140–1159.

Dreher, Axel, Peter Nunnenkamp, and Rainer Thiele (2008). "Does US Aid Buy UN General Assembly Votes? A Disaggregated Analysis". *Public Choice*, 136(1/2): 139–164.

Du, Yingxin, Jiandong Ju, Carlos D. Ramirez, and Xi Yao (2017). "Bilateral Trade and Shocks in Political Relations: Evidence from China and Some of Its Major Trading Partners, 1990–2013". *Journal of International Economics*, 108: 211–225.

Egger, Peter H. and Mario Larch (2008). "Interdependent preferential trade agreement memberships: An empirical analysis". *Journal of International Economics*, 76(2): 384–399.

Fally, Thibault (2015). "Structural gravity and fixed effects". *Journal of International Economics*, 97(1): 76–85.

Felbermayr, Gabriel, Aleksandra Kirilakha, Constantinos Syropoulos, Erdal Yalcin, and Yoto V. Yotov (2020). "The global sanctions data base". *European Economic Review*, 129: 103561.

Fernandez-Villaverde, Jesus, Tomohide Mineyama, and Dongho Song (2025). *Are We Fragmented Yet? Measuring Geopolitical Fragmentation and Its Causal Effects*. Working paper, February 4, 2025.

Fuchs, Andreas and Nils-Hendrik Klann (2013). "Paying a Visit: The Dalai Lama Effect on International Trade". *Journal of International Economics*, 91(1): 164–177.

Gopinath, Gita, Pierre-Olivier Gourinchas, Andrea F. Presbitero, and Petia Topalova (2025). "Changing global linkages: A new Cold War?" *Journal of International Economics*, 153: 104042.

Gowa, Joanne and Edward D. Mansfield (1993). "Power Politics and International Trade". *American Political Science Review*, 87(2): 408–420.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales (2009). "Cultural Biases in Economic Exchange?*". *Quarterly Journal of Economics*, 124(3): 1095–1131.

Häge, Frank M. (2011). “Choice or Circumstance? Adjusting Measures of Foreign Policy Similarity for Chance Agreement”. *Political Analysis*, 19(3): 287–305.

— (2017). *Chance-Corrected Measures of Foreign Policy Similarity (FPSIM Version 2)*. Version V2.

Head, Keith and Thierry Mayer (2014). “Chapter 3 - Gravity Equations: Workhorse, Toolkit, and Cookbook”. *Handbook of International Economics*. Ed. by Gita Gopinath, Elhanan Helpman, and Kenneth Rogoff. Vol. 4. Handbook of International Economics. Elsevier, 131–195.

Heid, Benedikt and Mario Larch (2016). “Gravity with unemployment”. *Journal of International Economics*, 101: 70–85.

Heilmann, Kilian (2016). “Does political conflict hurt trade? Evidence from consumer boycotts”. *Journal of International Economics*, 99: 179–191.

Javorcik, Beata, Lucas Kitzmüller, Helena Schweiger, and Muhammed A. Yıldırım (2024). “Economic costs of friendshoring”. *World Economy*, 47(7): 2871–2908.

Jiang, Hui, Churen Sun, and Qing Qin (2025). “Bilateral Political Relations and Trade Gains between Countries: Perspective from Value-Added Exports”. *Pacific Economic Review*, 30(2–3): 91–120.

Jordà, Òscar (2005). “Estimation and Inference of Impulse Responses by Local Projections”. *American Economic Review*, 95(1): 161–182.

Kastner, Scott L. (2007). “When Do Conflicting Political Relations Affect International Trade?” *Journal of Conflict Resolution*, 51(4): 664–688.

Kleinman, Benny, Ernest Liu, and Stephen J. Redding (2024). “International Friends and Enemies”. *American Economic Journal: Macroeconomics*, 16(4): 350–85.

Korovkin, Vasily and Alexey Makarin (2023). “Conflict and Intergroup Trade: Evidence from the 2014 Russia-Ukraine Crisis”. *American Economic Review*, 113(1): 34–70.

Larch, Mario, José-Antonio Monteiro, Roberta Piermartini, and Yoto V. Yotov (2025). “On the trade effects of GATT/WTO membership: They are positive and large after all”. *Canadian Journal of Economics*, 58(1): 281–328.

Larch, Mario and Yoto V. Yotov (2024). “Estimating the effects of trade agreements: Lessons from 60 years of methods and data”. *World Economy*, 47(5): 1771–1799.

Lee, Woori, Alen Mulabdic, and Michele Ruta (2023). “Third-country effects of regional trade agreements: A firm-level analysis”. *Journal of International Economics*, 140: 103688.

Li, Yuhua, Ze Jian, Wei Tian, and Laixun Zhao (2021). “How Political Conflicts Distort Bilateral Trade: Firm-Level Evidence from China”. *Journal of Economic Behavior & Organization*, 183: 233–249.

Lin, Faqin, Cui Hu, and Andreas Fuchs (2019). "How Do Firms Respond to Political Tensions? The Heterogeneity of the Dalai Lama Effect on Trade". *China Economic Review*, 54: 73–93.

Mayer, Thierry, Gianluca Santoni, and Vicent Vicard (2023). *The CEPII Trade and Production Database*.

Michaels, Guy and Xiaojia Zhi (2010). "Freedom Fries". *American Economic Journal: Applied Economics*, 2(3): 256–281.

Mityakov, Sergey, Heiwai Tang, and Kevin K. Tsui (2013). "International Politics and Import Diversification". *The Journal of Law and Economics*, 56(4): 1091–1121.

Morrow, James D., Randolph M. Siverson, and Tressa E. Tabares (1998). "The Political Determinants of International Trade: The Major Powers, 1907–1990". *American Political Science Review*, 92(3): 649–661.

Palmer, Glenn et al. (2022). "The MID5 Dataset, 2011–2014: Procedures, coding rules, and description". *Conflict Management and Peace Science*, 39(4): 470–482.

Pandya, Sonal S. and Rajkumar Venkatesan (2016). "French Roast: Consumer Response to International Conflict—Evidence from Supermarket Scanner Data". *The Review of Economics and Statistics*, 98(1): 42–56.

Pollins, Brian M. (1989a). "Conflict, Cooperation, and Commerce: The Effect of International Political Interactions on Bilateral Trade Flows". *American Journal of Political Science*, 33(3): 737–761.

— (1989b). "Does Trade Still Follow the Flag?" *American Political Science Review*, 83(2): 465–480.

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

Simonovska, Ina and Michael E. Waugh (2014). "The elasticity of trade: Estimates and evidence". *Journal of International Economics*, 92(1): 34–50.

Teti, Feodora A. (2024). *Missing Tariffs*. Tech. rep. 11590. Feodora Teti's Global Tariff Database (v_beta1-2024-12). CESifo Working Papers.

Thoenig, Mathias (2024). "Chapter 8 - Trade in the shadow of war: A quantitative toolkit for geoeconomics". Ed. by Oeindrila Dube, Massimo Morelli, and Debraj Ray. Vol. 1. Handbook of the Economics of Conflict. North-Holland, 325–380.

Umaña Dajud, Camilo (2013). "Political Proximity and International Trade". *Economics & Politics*, 25(3): 283–312.

United Nations (2026). *United Nations General Assembly Voting Data: Resolutions 1 (11 December 1946) to 80/246 (30 December 2025)*. Version 5. Dataset, version 5 (February 2026). New York.

Wang, T. Y. (1999). “U.S. Foreign Aid and UN Voting: An Analysis of Important Issues”. *International Studies Quarterly*, 43(1): 199–210.

Wooldridge, Jeffrey M. (2008). *Econometric Analysis of Cross Section and Panel Data*. MIT Press, 644.

Yalcin, Erdal et al. (2025). *The Global Sanctions Data Base - Release 4: The Heterogeneous Effects of the Sanctions on Russia*. Working Papers 2025002. Center for Global Policy Analysis, LeBow College of Business, Drexel University.

Yotov, Yoto V., Roberta Piermartini, and Mario Larch (2016). *An Advanced Guide to Trade Policy Analysis: The Structural Gravity Model*. WTO iLibrary.

Online Appendix – Political Distance and International Trade

Knut Niemann

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).

A.1.1 Source and scope

I use version 5, published in February 2026. It contains 947,434 member-state–resolution observations for 5,694 resolutions, 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.

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

$$x_{ir} = \begin{cases} 1 & \text{yes,} \\ 2 & \text{abstain or absent/non-voting,} \\ 3 & \text{no,} \end{cases} \tag{A.4}$$

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,

$$A_{ijt} = \frac{1}{n_{ijt}} \sum_{r \in \mathcal{R}_{ijt}} \mathbf{1}\{x_{ir} = x_{jr}\}. \tag{A.5}$$

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

$$\kappa_{ijt} = 1 - \frac{\sum_r (x_{ir} - x_{jr})^2}{\sum_r (x_{ir} - \bar{x}_i)^2 + \sum_r (x_{jr} - \bar{x}_j)^2 + n_{ijt}(\bar{x}_i - \bar{x}_j)^2}. \tag{A.6}$$

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,

$$\pi_{ijt} = 1 - \frac{\sum_r (x_{ir} - x_{jr})^2}{\sum_r (x_{ir} - \bar{x})^2 + \sum_r (x_{jr} - \bar{x})^2}, \qquad \bar{x} = \frac{\bar{x}_i + \bar{x}_j}{2}, \tag{A.7}$$

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

$$d^S_{ijt} = 1 - S_{ijt}. \tag{A.8}$$

The main variable in the paper, `pd_kappa`, 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. Users should therefore not interpret `pd_kappa` as a percentage.

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.

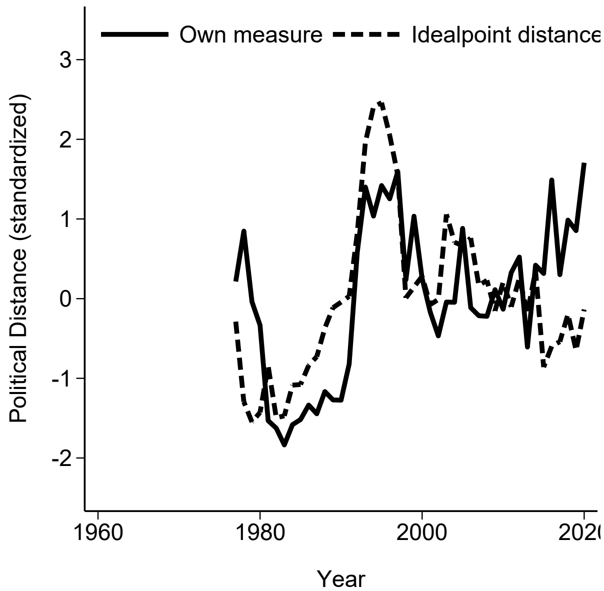
A.1.4 Interpretation and limitations

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.

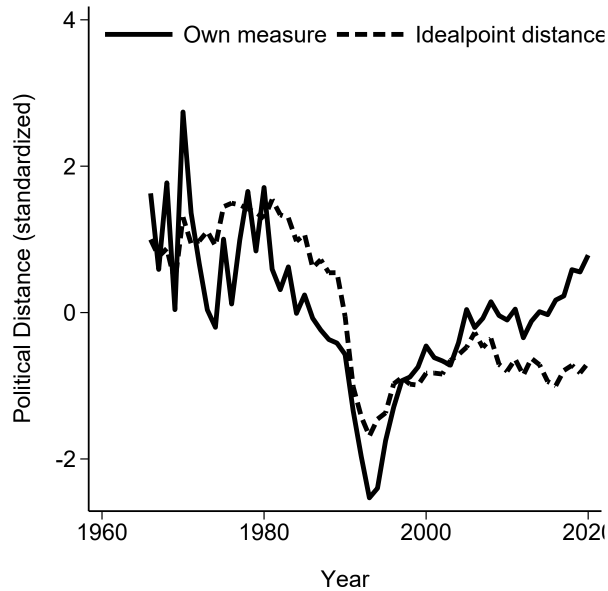
A.2 Additional descriptive statistics

Figure A.1: Measures of political distance

(a) US and China



(b) US and Russia



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). Own measure refers to the preferred κ-based measure used in the paper. Idealpoint distance is from Bailey et al. (2017).

Table A.1: Country coverage

Country	Range	Country	Range
AFG	1966-2020 (2002-2019)	AGO	1976-2020 (2014-2020)
ALB	1966-2020 (1986-2020)	ARE	1971-2020 (1977-2020)
ARG	1966-2020 (1984-2020)	ARM	1992-2020 (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	1966-2020 (1980-2020)	BHR	1971-2020 (1992-2018)
BHS	1973-2020 (1977-1998)	BIH	1992-2020 (2010-2020)
BLR	1992-2020 (2005-2020)	BLZ	1981-2020 (1989-1992)
BOL	1966-2020 (1970-2019)	BRA	1966-2020 (1990-2020)
BRB	1966-2020 (1970-1997)	BRN	1984-2020 (2010-2012)
BTN	1971-2020 (none)	BWA	2000-2020
CAF	1966-2020 (1973-1993)	CAN	1966-2020
CHE	2002-2020	CHL	1966-2020 (1966-2019)
CHN	1971-2020 (1977-2020)	CIV	1966-2020 (1966-1997)
CMR	1966-2020 (1970-2008)	COG	1966-2020 (1968-2008)
COL	1966-2020	CPV	1975-2020 (1997-2019)
CRI	1966-2020	CSK	1966-1992 (1966-1991)
CUB	1966-2020 (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	1966-2020 (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	1992-2020 (1998-2020)	GHA	1966-2020 (1966-2015)
GMB	1966-2020 (1975-2004)	GRC	1966-2020
GTM	1966-2020 (1968-1998)	HND	1966-2020 (1966-1996)
HRV	1992-2020	HTI	1966-2020 (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	1992-2020 (1993-2020)
KEN	1966-2020	KGZ	1992-2020

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.

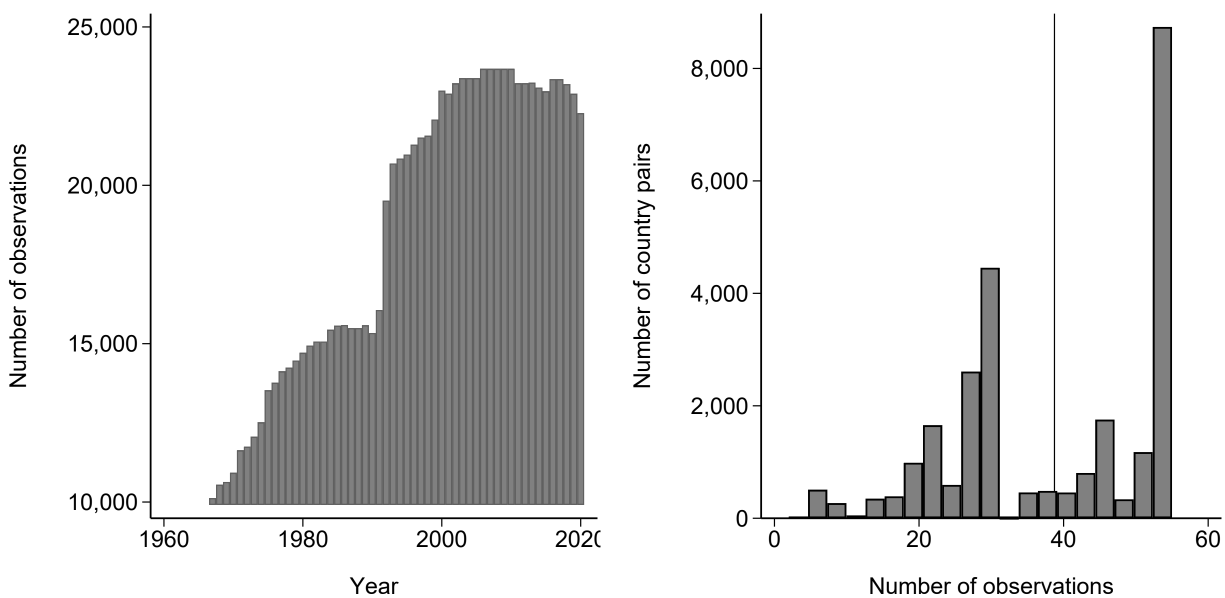
Table A.1: Country coverage, continued

Country	Range	Country	Range
KHM	1966-2020 (1993-2000)	KOR	1991-2020
KWT	1966-2020	LAO	1966-2020 (1999-2017)
LBN	1966-2020 (1998-2014)	LBR	1966-2020 (1972-1985)
LBY	1966-2020 (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	1969-2020 (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	1966-2020 (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	1966-2020 (1986-2020)
NZL	1966-2020	OMN	1971-2020 (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	1966-2020 (1982-2020)	PRT	1966-2020
PRY	1966-2020 (1966-2014)	QAT	1971-2020 (1987-2020)
ROU	1966-2020 (1985-2020)	RUS	1992-2020
RWA	1966-2020 (1999-2020)	SAU	1966-2020 (1989-2020)
SDN.1	1966-2010 (1966-2007)	SEN	1966-2020 (1974-2020)
SGP	1966-2020	SLE	1966-2020 (1981-1986)
SLV	1966-2020 (1966-1998)	SOM	1966-2020 (1967-1986)
SRB	2016-2020	SUN	1966-1991 (none)
SUR	1975-2020 (1975-2007)	SVK	1993-2020
SVN	1992-2020	SWE	1966-2020
SWZ	2000-2020 (none)	SYR	1966-2020 (1966-2005)
THA	1966-2020 (1968-2018)	TJK	1992-2020 (1992-2019)
TKM	1992-2020 (none)	TON	1999-2020 (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	1966-2020 (1968-2020)	USA	1966-2020
UZB	1992-2020 (2013-2020)	VEN	1966-2020 (1966-1998)
VNM.2	1977-2020 (1998-2020)	YEM.2	1991-2020 (1991-2014)
YUG	1966-1991 (1970-1989)	ZAF	1966-2020
ZMB	1966-2020 (1966-2017)	ZWE	1980-2020 (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.

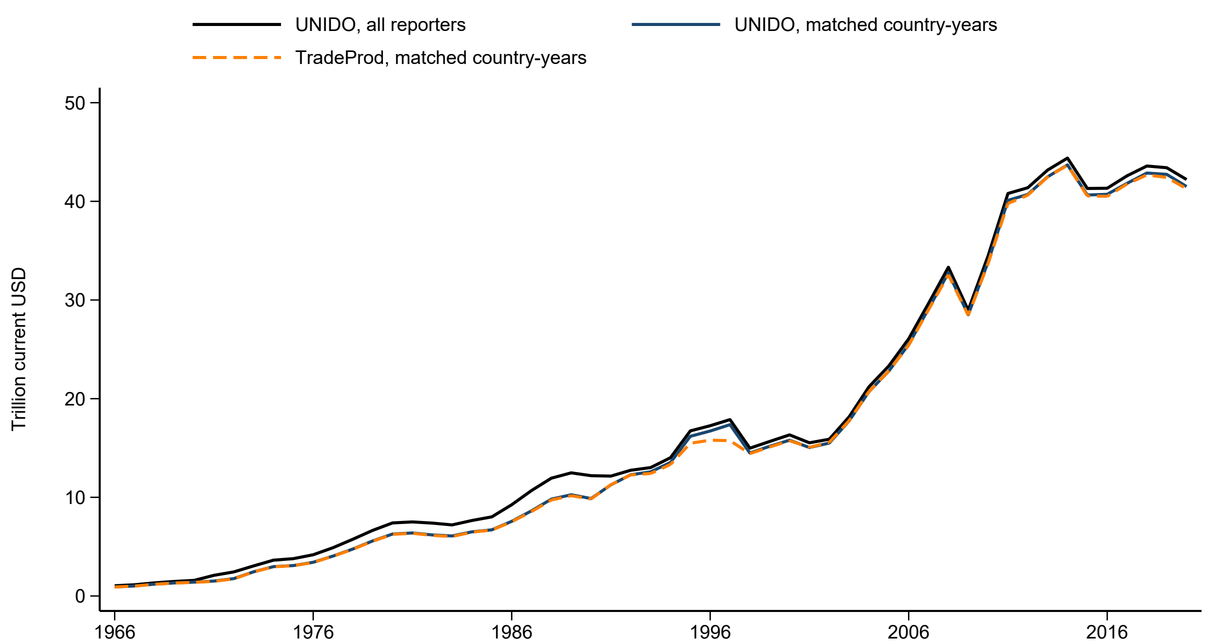
Figure A.2: Panel dimensions

(a) Observations per year (b) Observations per country-pair



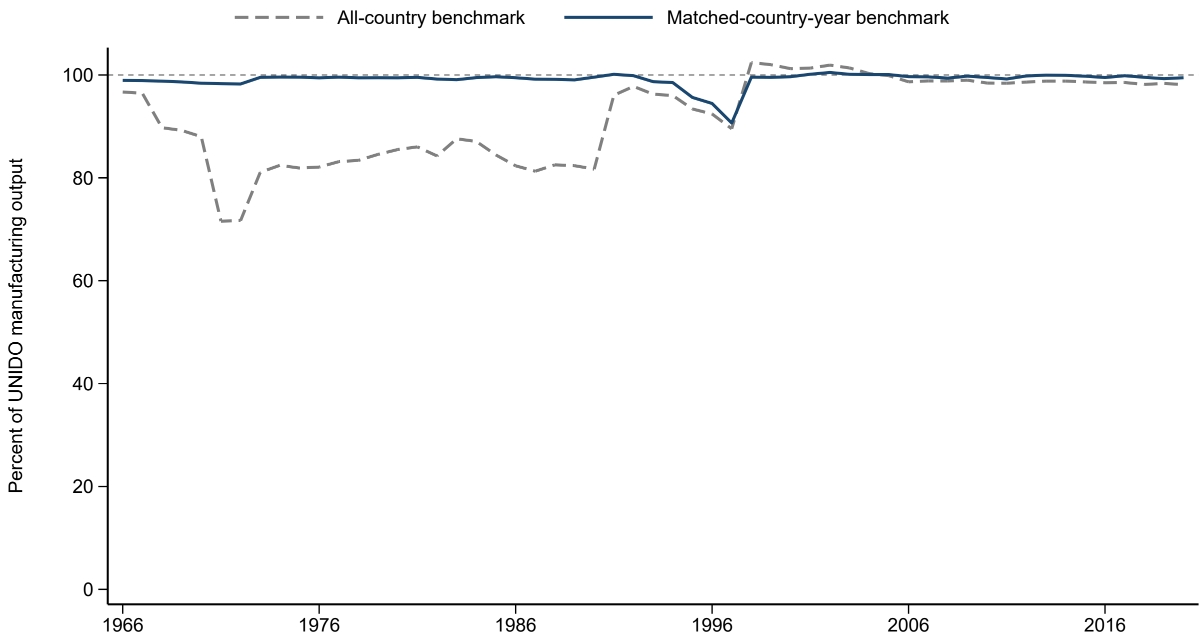
Notes: Number of observations per year and a histogram with 20 bins for observations per country-pair. The average number of observations per country-pair is 39, and the median is 40.

Figure A.3: Manufacturing output in UNIDO and TradeProd



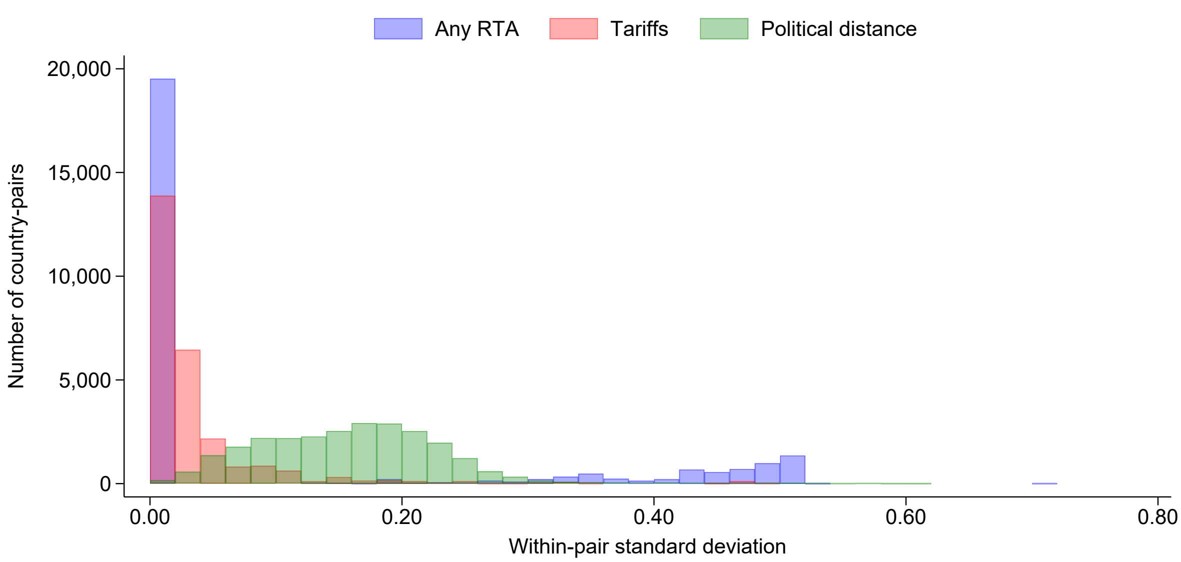
Notes: The figure compares manufacturing output reported by UNIDO for all reporting countries with UNIDO output and total TradeProd flows for matched country-years. Matched country-years have both UNIDO manufacturing output and a domestic TradeProd flow in the common estimation sample. TradeProd totals 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.2: 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***	-0.184***	-0.105***	-0.159***	-0.183***
	(0.049)	(0.049)	(0.013)	(0.016)	(0.024)
Mean pair-SD	0.16	0.15	0.16	0.15	0.15
Approx. effect (%)	-3.2	-2.7	-1.7	-2.3	-2.6
Observations	1,011,582	717,622	1,011,582	717,622	717,622

(b) Interval data

	(1) Annual baseline	(2) 3-year	(3) 4-year	(4) 5-year
Political distance	-0.206***	-0.257***	-0.248***	-0.246***
	(0.049)	(0.047)	(0.055)	(0.049)
Mean pair-SD	0.16	0.16	0.15	0.16
Approx. effect (%)	-3.2	-4.1	-3.7	-3.8
Observations	1,011,582	328,105	253,935	198,731

(c) Domestic trade and international-by-year fixed effects

	(1) Baseline	(2) No international-by-year FE	(3) International only
Political distance	-0.206***	-0.214***	-0.021
	(0.049)	(0.063)	(0.032)
Mean pair-SD	0.16	0.16	0.16
Approx. effect (%)	-3.2	-3.3	-0.3
Observations	1,011,582	1,011,582	976,514

Notes: The dependent variable is bilateral exports. All specifications condition on log tariffs and 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. Panel (c) compares the standard fixed-effect structure with a specification that omits international-trade-by-year fixed effects and one restricted to international flows; the latter cannot separately identify international-trade-by-year effects. The one-SD effect is $100[\exp(\widehat{\beta} \times SD) - 1]$ for PPML and log OLS and $100\widehat{\beta} \times SD$ for IHS OLS. All specifications include exporter–year, importer–year, and directed country-pair fixed effects unless the column heading states otherwise. Standard errors are clustered by undirected country pair and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Tariff attenuation on the common 1988–2020 sample

	(1)	(2)	(3)	(4)	(5)
Political distance					
Preferred κ	-0.103	-0.093	-0.095	-0.042	-0.034
	(0.070)	(0.062)	(0.068)	(0.072)	(0.065)
Potential channels					
Ln(1+tariff), Teti		-2.703***			-2.677***
		(0.312)			(0.327)
Any RTA			0.106***		0.001
			(0.040)		(0.041)
Any GSDB sanction				-0.110***	-0.106***
				(0.020)	(0.018)
Mean within-pair SD	0.138	0.138	0.138	0.138	0.138
One-SD effect on trade (%)	-1.41	-1.28	-1.30	-0.57	-0.47
Delta-method SE	0.95	0.85	0.92	0.99	0.89
Observations	696,293	696,293	696,293	696,293	696,293

Notes: PPML estimates for aggregate goods trade on the common sample for which the preferred political-distance measure, tariffs from Teti (2024), RTAs, and sanctions are jointly observed. Columns (1)–(4) report the baseline and add each potential policy channel separately; Column (5) includes all three channels. Reported effect sizes are exact percentage changes associated with an increase in political distance equal to its mean within-pair standard deviation, with delta-method standard errors. All specifications include fixed effects for importer-year, exporter-year, pair, and international-trade-by-year. Standard errors for coefficient estimates are clustered by country pair and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Political-distance effects by TradeProd sector

	(1) Pooled	(2) By sector
Political distance	-0.242***	
	(0.066)	
Political distance, by TradeProd sector		
Food		-0.346***
		(0.053)
Textiles		-0.220***
		(0.085)
Wood-Paper		-0.119
		(0.126)
Chemicals		-0.143
		(0.114)
Minerals		-0.254***
		(0.063)
Metals		-0.301***
		(0.083)
Machines		-0.339***
		(0.062)
Vehicles		-0.154*
		(0.084)
Other		-0.196**
		(0.085)
P-value: sector coefficients equal		0.002
Observations	8,523,708	8,523,708

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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5: Political-distance effects by geopolitical-bloc relationship

	(1) Gopinath bloc definition
Within aligned bloc	-3.47**
	(1.43)
Across US–China blocs	-2.50
	(2.00)
At least one non-aligned	-3.76***
	(1.25)
Within = across: p-value	0.698
All three equal: p-value	0.859
Average within-pair SD	0.158
Observations	984,946

Notes: Entries are percentage trade effects of an increase in political distance equal to the average within-pair standard deviation. Bloc relationships follow the classification of Gopinath et al. (2025). The model uses the bloc-covered subset of the fixed Table 2 sample and includes exporter–year, importer–year, country-pair, and international-trade-by-year fixed effects. Standard errors are clustered by undirected country pair and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Security-related political distance and sanctions

	All dyad-years		+ Sanctions control		Exclude sanctions		Interaction model	
	b	se	b	se	b	se	b	se
Security distance	-3.92***	(0.84)	-2.85***	(0.80)	-1.55**	(0.77)		
Security distance: no sanctions							-2.74***	(0.84)
Security distance: sanctions active							-3.39***	(0.97)
P-value: effects equal							0.465	

Notes: Entries are percentage effects of an increase in security-related political distance equal to its average within-pair standard deviation. The specifications sequentially use all dyad-years, control for sanctions, exclude observations with active sanctions, and allow the effect to differ by sanctions status. 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. Standard errors are clustered by undirected country pair and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Alliance-based measures of political distance

	(1)	(2)	(3)
Political distance			
Preferred κ (UNGA)	-0.212***		
	(0.057)		
Cohen's κ (alliances)		-0.477***	
		(0.068)	
Scott's π (alliances)			-0.454***
			(0.066)
Mean within-pair SD	0.161	0.026	0.026
One-SD effect on trade (%)	-3.37	-1.21	-1.17
Delta-method SE	0.89	0.17	0.17
Observations	817,132	817,132	817,132

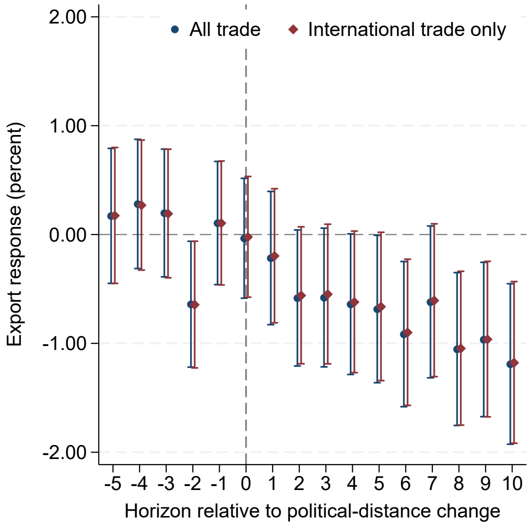
Notes: PPML estimates for aggregate goods trade using the common sample for which the UNGA- and alliance-based measures of political distance are jointly available. The sample covers 1966–2012. Column (1) uses the preferred UNGA-based measure of political distance, re-estimated on this restricted sample. Columns (2) and (3) use political distance constructed from alliance membership using Cohen's κ and Scott's π, respectively (Häge 2017). "Mean within-pair SD" reports the mean across country pairs of the within-pair standard deviation of the corresponding measure, $\bar{\sigma}_{pd}$. "One-SD effect on trade" reports $100 \cdot [\exp(\bar{\sigma}_{pd}\beta_{pd}) - 1]$; the following row reports its delta-method standard error. All columns contain exactly the same observations. All specifications include fixed effects for importer-year, exporter-year, pair, and international-trade-by-year. Standard errors for coefficient estimates are clustered by country pair and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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

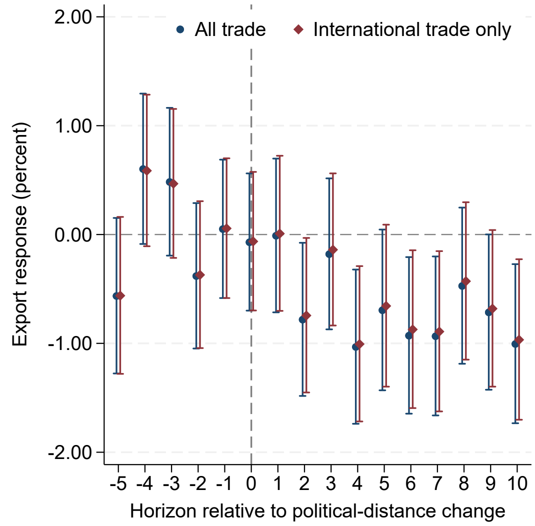
	(1) ±5 years	(2) ±4 years	(3) ±3 years	(4) ±2 years	(5) ±1 year	(6) No leads/lags
Political distance						
year $t-5$	-0.162***					
	(0.040)					
year $t-4$	-0.089***	-0.166***				
	(0.026)	(0.042)				
year $t-3$	-0.073***	-0.118***	-0.192***			
	(0.021)	(0.027)	(0.044)			
year $t-2$	-0.031	-0.038*	-0.087***	-0.173***		
	(0.021)	(0.021)	(0.027)	(0.044)		
year $t-1$	-0.070***	-0.074***	-0.081***	-0.140***	-0.212***	
	(0.021)	(0.023)	(0.023)	(0.030)	(0.046)	
year t	-0.029	-0.033	-0.043	-0.051*	-0.097**	-0.212***
	(0.028)	(0.029)	(0.028)	(0.027)	(0.039)	(0.082)
year $t+1$	0.013	0.020	0.015	0.018	0.032	
	(0.031)	(0.034)	(0.034)	(0.040)	(0.058)	
year $t+2$	0.006	0.013	0.026	0.046		
	(0.023)	(0.023)	(0.031)	(0.045)		
year $t+3$	0.038*	0.027	0.035			
	(0.021)	(0.025)	(0.035)			
year $t+4$	0.019	0.003				
	(0.019)	(0.031)				
year $t+5$	-0.038					
	(0.035)					
p-value: leads jointly zero	0.080	0.636	0.794	0.472	0.579	–
p-value: lags jointly zero	0.000	0.000	0.000	0.000	0.000	–
Observations	766,172	766,172	766,172	766,172	766,172	766,172

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

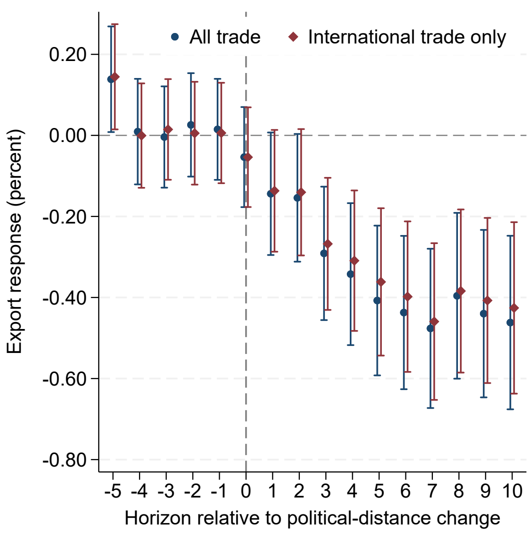
(a) Using ln Exports, full sample



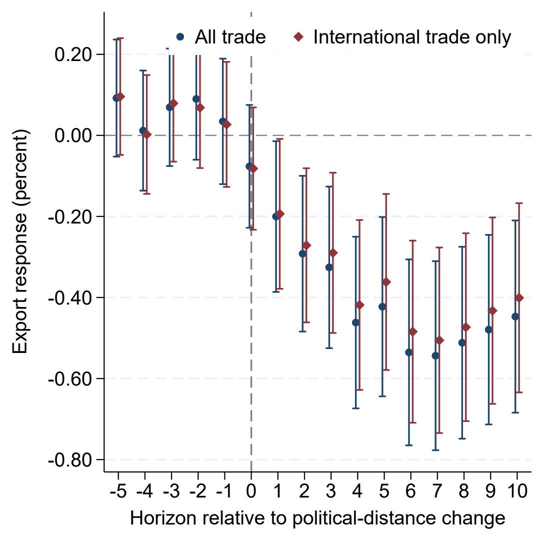
(b) Using ln Exports, fixed sample



(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 $\Delta_h y_{ij,t} = \beta_h \Delta \mathrm{PD}_{ij,t} + \gamma_h \Delta \mathrm{PD}_{ij,t-1} + \pi^h_{ij} + \pi^h_{i,t} + \pi^h_{j,t} + \pi^h_{i\neq j,t} + \varepsilon^h_{ij,t}$. 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 country-pair.

Table A.9: Local projections pretrend tests

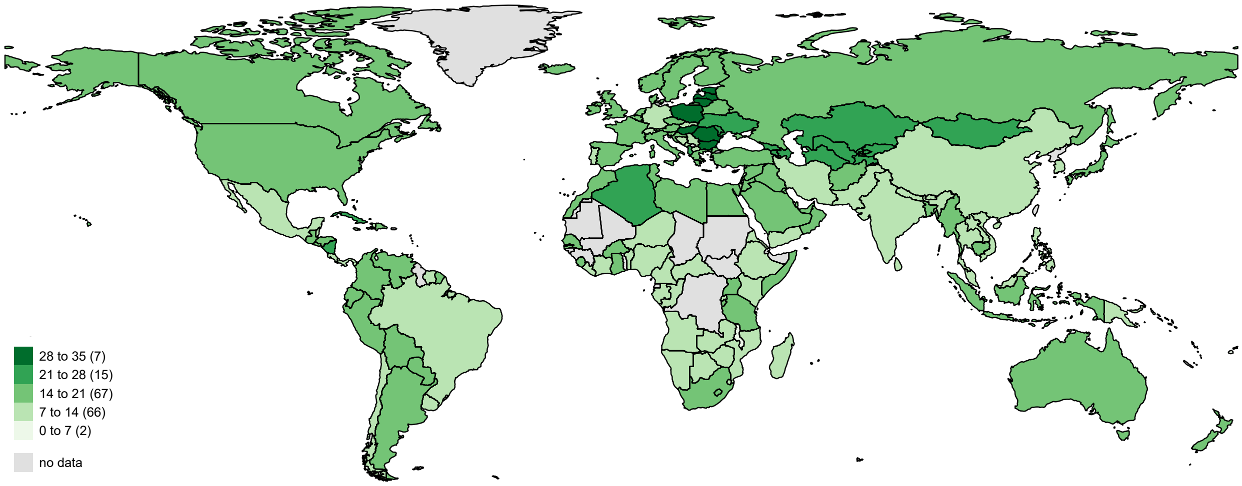
Outcome	Trade sample	Horizon sample	Joint pre-period p	Joint post-treatment p
IHS	All trade	Horizon-specific	0.255	0.001
IHS	All trade	Fixed	0.072	0.002
Log	All trade	Horizon-specific	0.300	0.086
Log	All trade	Fixed	0.163	0.007
IHS	International	Horizon-specific	0.244	0.002
IHS	International	Fixed	0.105	0.006
Log	International	Horizon-specific	0.309	0.097
Log	International	Fixed	0.195	0.011

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 $h = 10$.

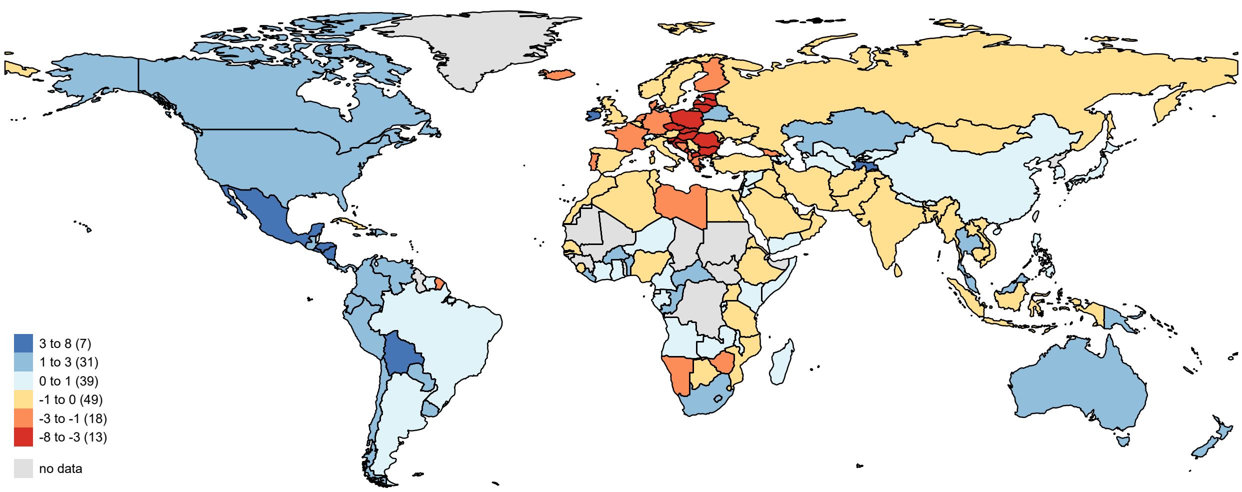
A.4 Additional counterfactual results

Figure A.7: Geographic distribution of New Cold War effects

(a) Trade reshuffling



(b) Changes in real income



Notes: The panels map average effects across the bootstrap draws for the total-effect specification. Trade reshuffling is the country-level standard deviation of relative changes in bilateral exports. Changes in real income are reported in percent.