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Abstract

It is well-established that political leaders matter for domestic outcomes, but statistical evidence for their relevance in international politics is still scarce. Here, we ask whether the personal relationship between political leaders can change the propensity for nonviolent conflict between nation-states in the form of sanctions. Panel probit models with data for the period 1970 to 2004 are estimated to evaluate whether more similar leaders are less likely to sanction each other. Our results indicate that higher leader similarity significantly reduces the likelihood of sanction imposition. The effect is most pronounced for sanctions imposed through unilateral political decisions. The probability of such sanction imposition ranges from 4.9% at the highest observed leader similarity in the sample to 13.0% at the lowest. Leader similarity seems to matter especially for sanctions aimed at democratic change or human rights improvements, for non-trade sanctions, and when at least one autocracy is involved. Finally, leader similarity has become more important after the Cold War.

JEL Codes: D70, F51, K33.

Keywords: Geoeconomics, International sanctions, Leader similarity, Political leaders.

1 Introduction

"I believe him, and I'm convinced that he is [a flawless democrat]."
(German chancellor Gerhard Schroeder on Vladimir Putin, 2004).

"I knew Putin very well. I got along with him great. He liked me. I liked him."
(Former US President Donald Trump, 2022).

In recent years, economists and political scientists have abandoned the idea that political leaders can be characterized solely by a policy platform, which has long dominated political economy research. It has, for example, been shown that political leaders' ideology, education, and professional background matter for policy decisions (Dreher and Yu 2020; Dreher et al. 2009; Funke et al. 2023; Gutmann et al. 2024; Hayo and Neumeier 2016; Mehmood and Seror 2023; Neumeier 2018; Peveri 2022). It has also been demonstrated that whether politicians are, based on their looks, perceived as attractive or trustworthy determines their electoral success, their political work, and the performance of their national economy (e.g., Berggren et al. 2010; François et al. 2023; Gründler et al. 2024). Most of these studies are concerned with domestic politics, while little attention has been paid to whether such traits of politicians matter for international cooperation and conflict. Still, some recent studies have emphasized that leader characteristics and leaders' interactions with each other can shape foreign policy behavior and international outcomes (Byman and Pollack 2001; Saunders 2011; Dube and Harish 2020; Horowitz and Stam 2014; Ellis et al. 2015; Holmes and Yarhi-Milo 2017; Holmes 2018; Yarhi-Milo 2018; Holmes and Wheeler 2020; Wheeler 2018).

In international relations, it is natural to think of political leaders not just as isolated decision-makers, but as being continuously involved in interactions with other leaders. This leads to the question of which of these leader pairs cooperate more successfully or are more likely engaged in conflict with each other. At the country level, it has been demonstrated that the intensity of bilateral economic exchange is determined by countries' shared history and cultural similarities (Guiso et al. 2009). Only few empirical studies have started to generate comparable insights for political lead-

ers. Foster and Keller (2023), for example, provide some evidence suggesting that pairs of cognitively simple leaders experience more interstate conflicts than pairs of cognitively complex leaders or asymmetric pairs that consist of both types of leaders. At the same time, cases where personal relationships between leaders have affected those between their nations exist in abundance (e.g., Zurcher and Murphy 2025). When Mark Carney replaced Justin Trudeau as Canada's prime minister, relations with US president Trump and his government improved instantly. Carney is not only older than Trudeau (i.e., closer to Trump's age), he also had a successful career in international banking and running financial institutions, allowing him to accumulate significant personal wealth.

Here, we seek to determine whether international sanctions are imposed less often between political leaders who share greater similarity (measured as described in Section 3.2). The literature on international sanctions has, thus far, ignored individual leader characteristics as a factor in sanction imposition and effectiveness, despite the attention they receive in the broader international political economy literature. Most similar to our study is DiLorenzo and Rooney (2025). They find that pairs of countries whose leaders share a similar background are less likely engaged in an interstate conflict with each other. Our focus on sanctions instead of conflict implies some notable differences between our study and DiLorenzo and Rooney (2025). First, international sanctions occur much more frequently than military interstate disputes. The number of imposed sanctions has increased dramatically since the end of the Cold War (Felbermayr et al. 2020). If leader similarity has a significant effect on international sanctions, it is of great importance to the day-to-day business of international politics, and not just to the most severe escalations of interstate conflict. Moreover, the larger number of international sanctions compared to military conflicts facilitates the study of the heterogeneity of causes of different types of sanctions. Second, sanction imposition is typically a more legalistic process than the decision to start an interstate conflict (see, e.g., Eaton and Sykes 2002). It is, therefore, more difficult for leader traits to influence the decision to impose sanctions than that to start a war.

In our empirical analysis, we estimate panel probit models with sender-year and dyad fixed effects. Our dataset combines data from the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020) with data on leader similarity by DiLorenzo and Rooney (2025). It covers 293,594 observations corresponding to 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs (or dyads) over the period from 1970 to 2004. As many sanctions are imposed by international organizations consisting of a group of countries, an individual political leader has only limited influence on these decisions. Thus, we estimate most models without these multilateral sanctions.

We find that higher leader similarity significantly reduces the likelihood of sanction imposition. This result holds across various model specifications and estimation techniques. The effect is especially pronounced when United Nations (UN) and European Union (EU) sanctions are excluded, that is, when sanctions are imposed through unilateral political decisions. The predicted probability of such sanctions being imposed ranges from 4.9% at the highest observed leader similarity in the sample to 13.0% at the lowest. Moreover, the influence of leader similarity is strongest in sanctions aimed at political or human rights improvements – compared to those related to conflict, terrorism, regime destabilization, or policy change. Political leaders can be expected to enjoy most discretion regarding the imposition of these types of sanctions. Finally, leader similarity has become more important after the Cold War; and the imposition of non-trade sanctions (e.g., arms and military sanctions) and of sanctions from or against autocracies depends most on the similarity of the involved political leaders.

The remainder of this article is structured as follows. Section 2 briefly outlines the theoretical rationale for why sanctions are more likely imposed between leaders with a less similar background. Section 3 describes our estimation strategy and the data used in our empirical analysis. We discuss our empirical results, including extensions and robustness tests, in Section 4. Section 5 concludes.

2 Theory

Sanctions are an instrument of foreign influence that is supposed to induce a policy change by the target country's government. In a globally interconnected world with abundant political and economic international externalities, sanctions offer an alternative policy instrument for the sender country's government where voluntary (self-enforcing) international agreements fail to internalize the most significant externalities (Aidt et al. 2021; Gutmann et al. 2025). How much compliance can be extracted via sanctions depends on the cost of sanctions to the sender and the target, as well as both parties' patience (Eaton and Engers 1992).[1]

In practice, sanctions are costly to both senders and targets (Gutmann et al. 2023a, 2023b) and a first-best outcome, thus, cannot be achieved when they are employed. In a world of complete information, that is, zero transaction costs, sanctions would never need to be used, as credible sanction threats would be anticipated and the target would adjust its behavior in advance (Coase 1960; Eaton and Engers 1999). In the remaining cases, sanctions would also not be deployed in equilibrium, although the target makes no concession, and the sender government would, therefore, not even bother to formulate a threat in the first place. Therefore, implemented international sanctions are a second-best instrument for a world of incomplete information. Transaction costs include the cost of the bargaining process and determine, for example, whether information on the sender's and the target's type is available to both parties. If the costliness of sanctions to the other party or the other party's patience is private information, bargaining can fail and sanctions may be used in equilibrium (Hovi et al. 2005; Spaniel and Smith 2015). Sanctions may also be used because of asymmetric information between the sender government and an audience, such as voters or the international community, if the sender government wants to be seen as doing something, for example, about a violation of international law, even though it is not able to change the target government's policy choice (Hovi et al. 2005).

1. Li (2025) demonstrates that leaders with a reputation for madness are less likely to back down when targeted with sanctions.

Based on this theory of sanction imposition, the level of similarity of two political leaders may affect the likelihood that one is imposing sanctions against the other via different channels. First, leader similarity reduces the cost of contracting and, thereby, widens the scope for voluntary agreements between the two nations (see also Gerber et al. 2013). Many international externalities are accordingly internalized without the need for sanction threats. Second, by improving communication and promoting trust, leader similarity can reduce the likelihood that sanctions are imposed when there is uncertainty about the other leader's policies or their intentions. Arms sanctions could, for example, be avoided if the sender trusted the target government to not use acquired arms to wage war or commit atrocities (Efrat and Yair 2024). Third, if leader similarity allows one leader to better judge the type of the other, that is, how patient they are or how costly sanctions would be to them, fewer or no sanctions would be imposed in equilibrium. This is because potential targets would know when to acquiesce, and potential senders would know that imposing sanctions in the remaining cases would not yield results.[2] Finally, if more similar leaders have more trust in each other, using sanctions could potentially erode that trust and thereby reduce their ability to conclude unrelated mutually beneficial agreements in the future. In other words, sanctions would be less likely to be imposed between more similar leaders, because their opportunity costs are higher.

To understand why similarity is expected to enhance trust, communication, and mutual understanding, as we have assumed thus far, we have to integrate non-rational-choice based theories of social interaction into our framework of analysis (see Akerlof and Kranton 2000, on integrating identity into rational choice models). Social interactions are frequently characterized by homophily and in-group bias (Currarini and Mengel 2016), two of the most widely observed social phenomena. Homophily describes the tendency to associate with similar others and leads to smoother coordination and communication as well as enhanced trust (Békés and Ottaviano 2025; Currarini et al. 2009; Ertug et al. 2020). In-group bias describes the tendency to trust

2. Sanctions might still be used to deter transgressions of third-party leaders who are less similar to the potential sender, but this incentive is independent of the similarity of the sender and target at hand.

and give favorable treatment to members of one's perceived in-group. Both similarity to others and the delineation of one's in-group can be based on a variety of personal traits, including people's age, gender, ethnicity, religion, social class, nationality, education, and occupation.[3] Both homophily and in-group bias can be motivated either by taste or by expectations about the behavior of others. Cetre et al. (2024), for example, find that 80% of the ethnically motivated in-group bias they observe in the United States and Germany is taste-based and only 20% is attributable to anticipated differences in trustworthiness. Psychological research has shown that perceived similarity plays an important role in the evolution of cooperation in a prisoner's dilemma (Fischer 2009), and that perceived similarity in a trust game is conducive to trust (Clerke and Heerey 2021). Similarity can be expected to matter for trust when trust is largely localized, rather than generalized (see Tabellini 2008). Or, in the terminology of Enke (2019, 2020), similarity is more conducive to trust, the more individuals have communal rather than universalistic values.

These insights about general human behavior have rarely been applied to the interactions of leaders in international politics. Their decisions are often assumed to be guided by strict rationality, also because of the support by professional administrative bodies and the existence of institutional guardrails. This assumption, however, has been challenged. DiLorenzo and Rooney (2025), for example, argue that leaders' shared life experiences facilitate social bonding and the emergence of trust, which in turn reduces information and commitment problems that could lead to military disputes. Foster and Keller (2023) argue that conflict is more likely when cognitively simple leaders interact with each other.

The main implication of our theoretical arguments can be summarized in the following hypothesis:

Hypothesis 1. *Pairs of more similar leaders are less likely to impose sanctions against each other than leader pairs with fewer similarities.*

3. Falk and Zehnder (2013) show in a field experiment that in-group bias even exists among inhabitants of the same city district. Even-Tov et al. (2023) find favoritism simply due to a shared first name.

3 Estimation Strategy and Data

3.1 Estimation Strategy

To evaluate the effect of leader similarity on sanction imposition, we estimate panel probit models. These allow modeling binary outcomes while accounting for unobserved heterogeneity and non-linear time trends. The general model specification can be described as follows:

$$y^*_{i,j,t} = \beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j} + \epsilon_{i,j,t} \tag{1}$$

$y^*_{i,j,t}$ is the latent (unobserved) variable that corresponds to the observed binary outcome variable $y_{i,j,t}$. In this notation, country i (the sender) imposes sanctions on country j (the target) in year t ($y_{i,j,t} = 1$) or not ($y_{i,j,t} = 0$). $S_{i,j,t}$ represents the (weighted)[4] similarity of the leader in sender country i and the leader in target country j. $\tau_{i,t}$ represents sender-year fixed effects that capture all (time-varying) political, economic, and social conditions in the sender country. Importantly, these fixed effects account for a sender government's general (and potentially time-varying) inclination to employ international sanctions in disputes with other countries. Including these fixed effects is vital, as it separates leader similarity from the characteristics of the political leader imposing the sanctions. Moreover, these fixed effects nest the less granular year fixed effects, which capture any global (non-linear) time trend in the use of sanctions, such as their increased use since the end of the Cold War. Dyad fixed effects $\alpha_{i,j}$ absorb factors such as shared languages and history as well as cultural, genetic, geographic, and political proximity, to the extent that they are time-invariant (Guiso et al. 2009, e.g., show that bilateral exchange depends on shared history and culture). In addition, dyad fixed effects nest the less granular sender fixed effects and target fixed effects. Thus, they account for all time-invariant sender and target country characteristics. Since dyad fixed effects reduce the scope for potential bias in the estimation at the cost of significantly reduced efficiency (only variation within dyads over time is analyzed),

4. The weighting procedure is explained in Section 3.2 below.

we present estimation results with and without dyad fixed effects. We do not include target-year fixed effects, as target country leaders do not choose to be targeted by sanctions.[5] We account explicitly for a number of potentially confounding time-varying target country characteristics. $X^{pol}_{j,t-1}$ and $X^{econ}_{j,t-1}$ represent one-year lagged political and economic control variables. These are described in more detail in Section 3.2. $\epsilon_{i,j,t}$ is an idiosyncratic error term for which we assume clustering on the dyad level.

The observed binary outcome $y_{i,j,t}$ is linked to the latent variable $y^*_{i,j,t}$ as follows:

$$y_{i,j,t} = \begin{cases} 1 & \text{if } y^*_{i,j,t} > 0 \\ 0 & \text{if } y^*_{i,j,t} \leq 0 \end{cases} \tag{2}$$

Hence, the probability of observing $y_{i,j,t} = 1$ is given by the cumulative distribution function of the standard normal distribution, which is represented by $\Phi(\cdot)$:

$$P\left(y_{i,j,t} = 1 \mid S_{i,j,t}, X^{pol}_{j,t-1}, X^{econ}_{j,t-1}, \tau_{i,t}, \alpha_{i,j}\right) = \Phi\left(\beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j}\right) \tag{3}$$

The parameters of the probit model are obtained using maximum likelihood estimation.

As these estimates describe the effects on the latent variable, we have to calculate marginal effects to quantify the effect of a change in an independent variable on the probability of the observable binary outcome occurring. The marginal effects are obtained by taking the partial derivative of the probability function in Eq. (3) with respect to the variable of interest. For example, the marginal effect for leader similarity

5. Target-year fixed effects would fully account for the number of countries by which a target is sanctioned in any given year, and leader similarity would then only serve to explain the composition of those sender countries. Moreover, all observations of targets of UN sanctions or countries not sanctioned in a given year would be dropped. This would substantially reduce our sample size to 2,676 observations and discard most of the variation in the sanction imposition variable, ultimately causing the probit models to fail to converge in light of 1,920 fixed effects to be estimated (1,312 sender-year, 341 dyad-year, and 267 target-year fixed effects). For comparison, DiLorenzo and Rooney (2025) control for dyad fixed effects and year fixed effects.

$(S_{i,j,t})$ is calculated as follows:

$$\frac{\partial P\left(y_{i,j,t}=1 \mid S_{i,j,t}, X^{pol}_{j,t-1}, X^{econ}_{j,t-1}, \tau_{i,t}, \alpha_{i,j}\right)}{\partial S_{i,j,t}} = \phi\left(\beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j}\right) \cdot \beta \tag{4}$$

Since the marginal effects in probit models vary across observations, due to the non-linearity of the link function, we report average marginal effects (AME), that is, we calculate the marginal effects for each observation and then take the mean value of these marginal effects. We use the delta method to estimate the standard errors corresponding to the AME:

$$\begin{aligned} \text{SE(AME)} &= \sqrt{\text{Var}(\beta)} \cdot \frac{1}{N} \sum_{i=1}^{N} \phi\left(\beta S_{i,j,t} + \gamma X^{pol}_{j,t-1} + \delta X^{econ}_{j,t-1} + \tau_{i,t} + \alpha_{i,j}\right) \\ &= \sqrt{\text{Var}(\beta)} \cdot \text{AME} \end{aligned} \tag{5}$$

Hahn and Newey (2004) demonstrate in Monte Carlo studies that the bias from the incidental parameters problem in panel probit models is small. Nevertheless, we present estimates in Section 4.2 that are based on the bias correction method by Fernández-Val (2009) to address remaining concerns about incidental parameter bias. Finally, we also test the robustness of our results to OLS estimation (Angrist 2001).

3.2 Data

Our dataset contains 293,594 observations covering 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs for the period from 1970 to 2004. A list of countries can be found in Table A1. Our binary dependent variable indicates whether a potential target country is sanctioned by a potential sender country in a given year. Many sanctions are imposed by international organizations, wherein decisions are made by a group of countries. The effect of an individual country and its leader on the imposition of UN sanctions is – except for the five veto powers – very limited. The same applies to a lesser extent to sanctions imposed by the EU.

Accordingly, we estimate all models (i) for the full sample, (ii) excluding observations with UN sanctions, and (iii) excluding observations with UN and EU sanctions. Data on sanctions is taken from release 3 of the Global Sanctions Data Base (GSDB, see Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024). In the full dataset, 30,793 out of 293,594 dyad-years (10.5%) are subject to sanctions. In the most restrictive estimation sample, this number drops to 1,099 out of 11,787 dyad-years (9.3%), as observations with UN and EU sanctions are excluded, and many observations are absorbed by dyad fixed effects due to a lack of within-dyad variation in the dependent variable.

Our main independent variable is a leader similarity index introduced by DiLorenzo and Rooney (2025). They calculate Pearson's correlation coefficient over 58 leader characteristics, separately for every leader pair.[6] The data on leader characteristics comes from the well-established but discontinued LEAD dataset by Ellis et al. (2015). It covers personal background information and life experiences in seven categories (military experience, education, family variables in childhood, family variables in adulthood, occupation, political experience, and others).[7] Observed values of the index range from –0.09 to 1.00. For example, the similarity score of Gerhard Schroeder and Vladimir Putin is 0.95. As part of our robustness tests, we re-calculate the leader similarity index by leaving out one of the seven categories at a time (see Section 4.2 below). Similarity scores are calculated for leader pairs, not for dyad-years. To deal with leadership changes within a given year while aggregating scores to the dyad-year

6. As part of our robustness tests, we also calculate a different leader similarity metric based on the reversed Gower's distance (Gower 1971). This measure can directly handle different types of data (nominal, ordinal, and continuous) and might be more appropriate than the Pearson correlation coefficient, which is designed for continuous data.

7. The individual variables are **Military experience:** Military service (combat/noncombat), military education, rebel participation, national war participation (victory/loss), and rebel war participation (victory/loss). **Education:** Primary education, boarding school, and education level. **Family variables, childhood:** Only/firstborn/middle-born/"illegitimate" child, first son/daughter, parental status, orphan, royalty, father occupation, mother in labor force, and childhood health. **Family variables, adulthood:** Married (ever/in power), divorced (ever), number of spouses in life, number of sons/daughters, number of children/adopted children, and children died. **Occupation:** Education, journalism, law, engineering, medicine, science, agriculture, military career, religion, labor, activist, career politician, writer, film/music, aristocrat/landowner, police, and interpreter. **Political experience:** Years in politics before becoming leader and puppet leader. **Others:** Physical/mental health and gender. For further details, we refer to Ellis et al. (2015).

level, we construct the leader similarity score in years of leadership transition as the average similarity of the different leader pairs who are in office in that year, weighted by the number of days each leader pair is in office.

To account for the various causes of sanction imposition in the target country, we control for a number of political and (socio-)economic factors, lagged by one year to mitigate potential reverse causality. The economic situation is captured by the country's real GDP per capita (in logs). In addition, our models include the population size (also in logs) and a globalization index (KOFGI, see Dreher 2006; Gygli et al. 2019). The target country's political conditions – and thus the likely causes of sanctions – are described by a democracy dummy (indicating a *polity2* score by Marshall and Gurr 2020 above five), a latent human rights score by Fariss (2019), and dummy variables for a successful coup (Bjørnskov and Rode 2020) as well as minor and major conflicts (Gleditsch et al. 2002).

Table A2 provides a list of variables alongside their definitions and the underlying data sources. Table A3 shows descriptive statistics for the full dataset (Panel A) and the dataset on which most of the estimations are based (Panel B), that is, without dyad-years subject to UN or EU sanctions and without data spells that are absorbed by the two-way fixed effects. In both panels, we also distinguish between non-sanctioned and sanctioned observations. Leader similarity is lower in dyad-years subject to sanctions. Moreover, sanctioned countries tend to be less globalized and have a lower GDP per capita. They show more serious infringements of human rights, are less often democratic, and more often experience a successful coup. In the full dataset, countries subject to sanctions experience minor and major conflicts at a much higher frequency. However, this no longer holds once UN sanctions are removed from the dataset – presumably because (major) conflicts are one of the primary reasons for the UN Security Council to reach agreement on imposing sanctions.

4 Empirical Results

4.1 Baseline Results

Table 1 shows the AMEs from our baseline panel probit estimations. In a first step, we include only control variables that are undoubtedly exogenous to economic sanctions, that is, the lagged (socio-)economic conditions in the target country. Columns (1)–(6) include sender-year fixed effects and Columns (4)–(6) additionally account for dyad fixed effects. Columns (1) and (4) are based on the full sample, Columns (2) and (5) exclude those observations where UN sanctions are in place, and Columns (3) and (6) exclude all observations with UN or EU sanctions in place. As explained above, we exclude these observations gradually. The following discussion focuses on the more conservative estimates in Columns (4)–(6).[8] The substantial differences in the number of observations underlying the different regression models result from two factors: (i) the exclusion of cases with UN or both UN and EU sanctions, and (ii) the absorption of data spells by the fixed effects.

Leader similarity consistently has a negative effect on the likelihood of sanction imposition that is significant at the 1% level across all three specifications. The estimated effect increases when UN sanctions are excluded, and even further when also EU sanctions are excluded from the sample. This would suggest that the impact grows as a leaders' say in imposing sanctions increases. A one-unit increase in leader similarity is associated with a 5.8–10.9 percentage point (pp) decrease in sanction occurrences. Since the similarity variable is based on Pearson's correlation coefficient, the AME can be interpreted as the average observed shift from no correlation (0) to perfect correlation (1) in the characteristics of the leader pair.

8. These model specifications are indeed very conservative. All dyads in which we observe no (or only) sanctions are dropped from our sample, although such a pattern could, according to our theory, be the consequence of permanently high (low) leader similarity between two countries. This concerns about 82%–93% of the original sample.

Table 1: Explaining Sanctions with Leader Similarity and Socio-Economic Variables

	(1) All Obs.	(2) w/o UN	(3) w/o UN/EU	(4) All Obs.	(5) w/o UN	(6) w/o UN/EU
Leader Similarity	–0.0241***	–0.0338***	–0.0203***	–0.0577***	–0.0937***	–0.1088***
	(0.0021)	(0.0024)	(0.0023)	(0.0133)	(0.0271)	(0.0287)
lag log(GDP pc)^T	0.0200***	0.0038***	0.0063***	–0.2514***	–0.1125***	–0.0632**
	(0.0008)	(0.0011)	(0.0011)	(0.0123)	(0.0247)	(0.0318)
lag log(Population)^T	0.0133***	0.0092***	0.0083***	0.0416*	0.8581***	0.5012***
	(0.0004)	(0.0004)	(0.0004)	(0.0218)	(0.0904)	(0.1136)
lag Globalization^T	–0.0055***	–0.0016***	–0.0015***	–0.0218***	–0.0056***	–0.0060***
	(0.0001)	(0.0001)	(0.0001)	(0.0011)	(0.0017)	(0.0017)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE				Yes	Yes	Yes
Observations	293,594	167,012	165,364	52,007	15,425	11,787
Unique Dyads	13,242	11,990	11,801	1,961	697	583
Std. Dev. of Similarity	0.3505	0.3508	0.3504	0.3621	0.3668	0.3517

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the exclusion of UN sanctions or UN and EU sanctions and (ii) the absorption of data spells by the fixed effects.

Table 2: Explaining Sanctions with Leader Similarity, Socio-Economic Variables, and Reasons for Sanction Imposition

	(1) All Obs.	(2) w/o UN	(3) w/o UN/EU	(4) All Obs.	(5) w/o UN	(6) w/o UN/EU
Leader Similarity	–0.0164***	–0.0259***	–0.0143***	0.0181	–0.0388	–0.0450*
	(0.0020)	(0.0023)	(0.0022)	(0.0130)	(0.0236)	(0.0250)
lag log(GDP pc)^T	0.0250***	0.0040***	0.0062***	–0.2306***	–0.0887***	–0.0497
	(0.0008)	(0.0011)	(0.0011)	(0.0140)	(0.0229)	(0.0309)
lag log(Population)^T	–0.0055***	0.0065***	0.0067***	–0.0017	0.6743***	0.3645***
	(0.0007)	(0.0006)	(0.0006)	(0.0135)	(0.0848)	(0.1034)
lag Globalization^T	–0.0032***	–0.0008***	–0.0007***	–0.0113***	–0.0021	–0.0039**
	(0.0001)	(0.0001)	(0.0001)	(0.0007)	(0.0015)	(0.0016)
lag Democracy^T	–0.0189***	–0.0307***	–0.0290***	–0.2902***	–0.2116***	–0.1800***
	(0.0016)	(0.0020)	(0.0020)	(0.0199)	(0.0295)	(0.0263)
lag Human Rights^T	–0.0433***	–0.0080***	–0.0052***	–0.1103***	–0.0529***	–0.0486***
	(0.0010)	(0.0009)	(0.0009)	(0.0068)	(0.0138)	(0.0133)
lag Successful Coups^T	–0.0030	0.0085	0.0018	0.0090	0.0512***	0.0370**
	(0.0027)	(0.0039)	(0.0040)	(0.0083)	(0.0152)	(0.0188)
lag Minor Conflict^T	0.0218***	0.0127***	0.0132***	0.0112	0.0654***	0.0660***
	(0.0014)	(0.0018)	(0.0017)	(0.0079)	(0.0149)	(0.0158)
lag Major Conflict^T	0.0450***	–0.0331***	–0.0359***	0.1319***	0.0205	0.0456
	(0.0019)	(0.0032)	(0.0034)	(0.0098)	(0.0253)	(0.0295)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE				Yes	Yes	Yes
Observations	293,594	167,012	165,364	52,007	15,425	11,787
Unique Dyads	13,242	11,990	11,801	1,961	697	583
Std. Dev. of Similarity	0.3505	0.3508	0.3504	0.3621	0.3668	0.3517

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to (i) the exclusion of UN sanctions or UN and EU sanctions and (ii) the absorption of data spells by the fixed effects.

In a second step, we control for the political conditions in the target country that are commonly considered the primary causes of sanction imposition (Hufbauer et al. 2009). Table 2 shows the AMEs derived from these estimations. Again, we pay particular attention to the more conservative model specifications corresponding to Columns (4)–(6). The estimates are smaller than those in Table 1 and reach significance at the 10% level only in Column (6), that is, when UN and EU sanctions are excluded.[9] As before, the effect is strongest when both UN and EU sanctions are excluded: here, a one-unit increase in leader similarity corresponds to a 4.4 pp decrease in sanction occurrence.

This reduction in effect size and significance is expected, given the correlation between political control variables and the target country's leader characteristics. One could, however, argue that by adding the political control variables, we are already underestimating the effect of leader similarity on sanctions. If political leaders in target countries have some idea what they can get away with without getting sanctioned by a particular potential sender country, their decision to enter conflicts or commit human rights violations itself is affected by how similar they are to other political leaders who may consider sanctioning them. In that sense, the estimates in Tables 1 and 2 could be considered reasonable upper and lower bounds.

Turning briefly to the (lagged) control variables, we find that a higher GDP per capita reduces the likelihood of sanctions. Globalization shows a similar negative effect, though not in all samples. Sanctions are also less likely used against democratic countries and those with greater respect for human rights. While conflicts (and successful coups) generally increase the likelihood of sanctions, this effect is not uniform across models; in particular, the results for major conflicts may reflect the absorption of numerous spells and the exclusion of UN sanctions. Comparing the effect of leader similarity with that of the main causes of sanction imposition suggests that leader similarity's impact is – not too surprisingly – smaller than that of the standard control variables. Yet, the effect of leader similarity is empirically relevant, as demonstrated

9. It is worth noting that the estimate in Column (5) is only marginally insignificant, with a p-value of 10.3%.

by the predicted probability of sanction imposition across its observed empirical range (from –0.09 to 1). In the most conservative model (Column 6 of Table 2), this probability increases from 4.9% at the highest observed leader similarity to 13.0% at the lowest.[10]

4.2 Extensions and Robustness Tests

In line with our theoretical considerations and empirical evidence provided above (and to conserve space), all the following results focus on estimation samples without UN and EU sanctions.

Alternative Estimation Methods. To test the robustness of our results, we first apply alternative estimation techniques. Table A4 presents the marginal effects from probit estimations that incorporate the incidental parameter bias correction method proposed by Fernández-Val (2009).[11] The absolute values of the AMEs with the narrow set of controls are slightly smaller than those reported in Column (3) of Table 1, yet they remain highly statistically significant. Estimates derived from the broad, more conservative set of controls are nearly identical to those in Column (3) of Table 2. These results suggest that our estimations are not substantially affected by a incidental parameter problem.

Next, we re-estimate our baseline models using least squares, applying linear probability models. The results are presented in Table A5. In these models, we also include linear and quadratic time trends for the target countries (Columns 5 and 6) to further control for unobserved factors affecting these countries.[12] With the exception of Column (1), the absolute values of these estimates are even larger and exhibit higher levels of statistical significance compared to the baseline probit estimations (see Columns 3

10. This probability range is slightly larger than the product of the AME (4.4 pp) and the range of the similarity variable (1.09). The difference arises because the AME reflects the average change in probability when shifting from no correlation to perfect correlation for each observation. In contrast, the predicted probability range is estimated while holding all other covariates at their sample means.

11. These estimations converge when using sender-year fixed effects but fail to do so when dyad fixed effects are also included. Accordingly, we cannot employ a bias correction as the default estimation method.

12. It has to be noted that probit models with these trends face convergence issues similar to those with target-year fixed effects (see footnote 5).

and 6 of Tables 1 and 2). This provides further evidence to interpret our baseline results as a conservative lower bound for the effect of leader similarity on the likelihood of sanction imposition. Furthermore, our results remain robust when incorporating target country time trends.

Effect Heterogeneity. The GSDB records nine frequently coinciding categories of objectives or reasons for imposing sanctions (democracy, human rights, destabilize regime, policy change, prevent war, end war, territorial conflict, terrorism, and "others") based on information in official documents. To test if the effect of leader similarity depends on the situation in which sanctions are to be imposed, we re-estimate our models, excluding sanction cases with one objective at a time.[13] Table 3 presents the results under the more conservative setting with dyad fixed effects.

While our findings are in general robust to this jackknife-style test based on sanction objectives, it is striking that excluding sanctions aimed at promoting democracy or improving human rights in the target country causes the estimated effects to become much smaller and statistically insignificant (or just marginally significant when excluding human rights sanctions in the estimations using the narrow set of controls). This suggests that leader similarity plays most into the decision whether to impose sanctions in situations where democratic change or improvements in human rights are sought. These are arguably the most controversial types of foreign interventions, due to legitimacy concerns and their questionable track record (see, e.g., Coyne 2008; Steinbach et al. 2023). It is, therefore, not surprising that political leaders appear to have more discretion in these decisions than with regard to sanctions that target, for example, supporters or instigators of conflict and terrorism. Consistent with that interpretation, excluding conflict- or terrorism-related sanctions from the sample does not alter the results. The same holds for sanctions aiming at regime destabilization, policy changes, and other objectives. In fact, some coefficients are even larger than the

13. The majority of sanctions have multiple objectives. When we exclude sanctions one objective from the estimation, these sanctions, therefore, may have also had any number of other objectives. Our exercise here should, thus, be considered a robustness test rather than a way to cleanly separate sanctions with particular goals.

baseline, indicating that leader similarity is less relevant to sanction imposition when these objectives are concerned.

Table 3: Effect Heterogeneity for Sanction Objectives (w/o UN/EU Sanctions)

	Excl. "Democracy"		**Excl. "Human Rights"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.0233	0.0048	–0.0496*	–0.0062
	(0.0358)	(0.0307)	(0.0273)	(0.0255)
Observations	8,495	8,495	10,365	10,365
Unique Dyads	464	464	521	521
	Excl. "Destab. Regime"		**Excl. "Policy Change"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1250***	–0.0575**	–0.1132***	–0.0781**
	(0.0267)	(0.0232)	(0.0391)	(0.0372)
Observations	11,268	11,268	5,874	5,874
Unique Dyads	579	579	307	307
	Excl. "Prevent War"		**Excl. "End War"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1005***	–0.0550*	–0.1138***	–0.0411
	(0.0374)	(0.0332)	(0.0312)	(0.0265)
Observations	6,835	6,835	10,675	10,675
Unique Dyads	291	291	572	572
	Excl. "Territ. Conflict"		**Excl. "Terrorism"**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1257***	–0.0562**	–0.1074***	–0.0472*
	(0.0302)	(0.0256)	(0.0286)	(0.0245)
Observations	10,662	10,662	11,307	11,307
Unique Dyads	576	576	565	565
	Excl. "Other Objectives"		**Full Dataset**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1361***	–0.0441*	–0.1088***	–0.0450*
	(0.0264)	(0.0249)	(0.0287)	(0.0250)
Observations	11,724	11,724	11,787	11,787
Unique Dyads	583	583	583	583

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the exclusion of sanctions of a particular objective. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from (6) of Tables 1 and 2, respectively.

A second set of jackknife-style tests examines additional effect heterogeneity. Table 4 first presents results when sanctions from specific periods are excluded. When post-Cold War sanctions are omitted, the effects of leader similarity are smaller than in the baseline estimations (see the bottom right panel of Table 4). In contrast, excluding Cold War sanctions amplifies these effects. This suggests that leader similarity has played a greater role since 1991. This finding is unsurprising, as – at least within our sample period, which ends in 2004 – the world was no longer strictly divided into two rival blocs. As a result, leaders had greater discretion in shaping foreign policy.

Table 4: Effect Heterogeneity for Further Categories (w/o UN/EU Sanctions)

	Excl. Post-Cold War Sanctions		**Excl. Cold War Sanctions**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.0675**	–0.0369	–0.1218***	–0.0671*
	(0.0325)	(0.0477)	(0.0353)	(0.0344)
Observations	4,092	4,092	5,055	5,055
Unique Dyads	208	208	459	459
	Excl. Sanctions ag. Democracies		**Excl. US Sanctions**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.2087***	–0.0616***	–0.1160***	–0.0960***
	(0.0330)	(0.0232)	(0.0329)	(0.0310)
Observations	7,933	7,933	8,141	8,141
Unique Dyads	446	446	414	414
	Excl. Trade Sanctions		**Full Dataset**	
	Narrow	Broad	Narrow	Broad
Leader Similarity	–0.1766***	–0.1341***	–0.1088***	–0.0450*
	(0.0377)	(0.0503)	(0.0287)	(0.0250)
Observations	4,028	4,028	11,787	11,787
Unique Dyads	268	268	583	583

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the exclusion of sanctions of a particular category. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from (6) of Tables 1 and 2, respectively.

Next, the estimations exclude sanctions imposed on democratic countries, which strengthens the effect of leader similarity on sanction decisions, suggesting that targeting autocratic countries is more influenced by leader similarity. This aligns with the

findings in Table 3 where sanctions aimed at promoting democratic change largely drive the overall results. A similar pattern emerges when US sanctions are excluded. Since this subsection already excludes UN and EU sanctions, the share of non-democratic sanctioning countries naturally increases. Autocratic leaders typically have greater discretion in imposing sanctions, likely explaining the stronger effect observed. Finally, removing trade sanctions – the most common type – also amplifies the effect of leader similarity. This suggests that trade sanctions are often driven by a country's economic interests rather than leader similarity, unlike non-trade sanctions (e.g., arms, military, financial, travel, and "other" sanctions).[14]

General Country Affinity. Our baseline models do not include indicators for the general affinity between two countries, as such variables could be heavily influenced by leader similarity and would thus be a post-treatment confounder (i.e., a bad control variable). However, controlling for country affinity remains a useful robustness check to determine whether leader similarity continues to significantly impact sanction imposition. We use a relatively crude binary country affinity measure from Tomashevskiy (2024), which is quite stable over time and unlikely to be immediately affected by shifts in leader characteristics. As shown in Table A6, the results after controlling for country affinity remain virtually unchanged from those in Columns (3) and (6) of Table 2. This reinforces our confidence that the effect of leader pair characteristics on sanction imposition extends beyond country affinity, which is also partly captured by the dyad fixed effects.

Leader Characteristics. Our next modification to the baseline model addresses variations in the leader similarity variable. As explained in Section 3.2 and footnote 7, this indicator is based on the correlation of two leaders across 58 characteristics spanning seven categories. To test its robustness, we recalculate the index for each leader pair, omitting one category at a time. These jackknife-style leader similarity indices then serve as explanatory variables in the models corresponding to Column (6) of Ta-

14. It should be noted that excluding sanctions against non-democracies, non-US sanctions, and non-trade sanctions – combined with the absorption of spells with no within-dyad variation – results in treatment groups that are too small for meaningful estimations.

bles 1 and 2. Table A7 presents the results. Excluding any single category leaves the results virtually unchanged, with the exception of political experience, where both the absolute point estimates and standard errors increase slightly. Nevertheless, this modification suggests that no single category of characteristics disproportionately drives the effect of leader similarity on sanction imposition.

An Alternative Similarity Metric. Our final robustness check concerns an alternative measure of leader similarity, which – due to a lack of more recent comprehensive data on leader characteristics – is also based on the LEAD Dataset (Ellis et al. 2015). Using the same 58 characteristics, Gower's distance is calculated, which can directly account for nominal and ordinal variables. To simplify its comparison with the correlation-based similarity indicator, we subtract Gower's distance metric from 1. Table A8 presents the results of estimating probit models (Columns 1 and 2) and linear probability models (Columns 3 and 4) while using our alternative leader similarity indicator. Gower-based leader similarity itself also has a significant negative effect on sanction imposition. The marginal effects are larger than those in Tables 1, 2, and A5, which can be explained by the lower standard deviation in Gower's similarity (0.06) compared to the correlation-based measure (0.35).

5 Conclusion

In this study, we seek to determine whether international sanctions are less likely to be imposed between pairs of political leaders who share greater similarity. To answer this question, we estimate panel probit models with sender-year and dyad fixed effects as well as different sets of target country control variables. Our dataset covers 293,594 observations corresponding to 156 potential sender countries, 155 potential target countries, and 13,242 directed country-pairs over the period from 1970 to 2004.

Our main finding suggests that higher leader similarity significantly reduces the likelihood of sanction imposition. This result holds across various specifications and estimation techniques, also when controlling for the primary reasons for sanction im-

position. The effect is especially pronounced when UN and EU sanctions are excluded, that is, when focusing on sanctions imposed following a unilateral political decision-making process. In this case, the probability of sanction imposition rises from 4.9% at the highest observed leader similarity in the sample to 13.0% at the lowest. Moreover, we find that the influence of leader similarity is strongest in the decision to impose sanctions aimed at democratic change or human rights improvements, where we argue that political leaders have some of the highest levels of discretion. Finally, leader similarity has become more important after the Cold War and the imposition of sanctions from or against autocracies and of non-trade sanctions (e.g., arms and military sanctions) depends most on the similarity of the involved political leaders.

We study sanction imposition, but leader similarity may also influence the severity of imposed sanctions. It has been argued that weak sanctions are imposed to be perceived as doing something. The argument is that they are used where stronger sanctions would not be effective, but the leader might simply not want to impose serious sanctions. This warrants further inquiry, but more and better data on sanction intensity is needed with a similar time and country coverage as in the Global Sanctions Database.

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Appendix

Data Description

Table A1: Countries

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Benin, Bhutan, Bolivia, Botswana, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Costa Rica, Croatia, Cuba, Cyprus, Czech Republic, Denmark, Djibouti, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Estonia, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, Hungary, India, Indonesia, Iran, Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Laos, Latvia, Lebanon, Lesotho, Liberia, Libya, Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Moldova, Mongolia, Morocco, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, North Macedonia, Norway, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Romania, Russia, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, Slovakia, Slovenia, Solomon Islands, Somalia, South Africa, South Korea, Spain, Sri Lanka, Sudan, Suriname, Swaziland, Sweden, Switzerland, Syria, Tajikistan, Tanzania, Thailand, Togo, Tunisia, Turkey, Turkmenistan, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States of America, Uruguay, Uzbekistan, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe.

Table A2: Definitions of Variables and Data Sources

Variable	**Definition & Source**
Leader Similarity	Continuous measure of leader similarity calculated by applying Pearson's R correlation on similarity data of the LEAD dataset. Weighted by the number of overlapping days if more than one leader pair exists within a dyad-year. *Source:* Leader Similarity (DiLorenzo and Rooney 2025), LEAD Dataset (Ellis et al. 2015).
Gower's Leader Similarity	Continuous measure of leader similarity calculated by applying reversed Gower's Distance (Gower 1971), or Gower-based similarity, on similarity data of the LEAD dataset. Weighted by the number of overlapping days if more than one leader pair exists within a dyad-year. *Source:* LEAD Dataset (Ellis et al. 2015).
Sanctions	Binary indicators for country-years with sanctions in place. *Source:* GSDB (Felbermayr et al. 2020; Kirikakha et al. 2021; Syropoulos et al. 2024).
lag log(GDP pc)^T	Natural logarithm of real GDP per capita in USD of the target country, lagged by one year. *Source:* Trade and GDP Data (Gleditsch et al. 2002).
lag log(Population)^T	Natural logarithm of the population size of the target country, lagged by one year. *Source:* World Development Indicators (World Bank 2023).
lag Globalization^T	Index measuring the economic, social, and political dimensions of globalization in the target country, lagged by one year. *Source:* KOF Globalisation Index (Dreher 2006; Gygli et al. 2019).
lag Democracy^T	Binary democracy indicator for the target country based on the polity2 index that ranges from strongly democratic (+10) to strongly autocratic (–10). Coded as "1" if the index is larger than 5, lagged by one year. *Source:* Polity5 dataset (Marshall and Gurr 2020).

Table A2: Definitions of Variables and Data Sources (Continued)

Variable	**Definition & Source**
lag Human Rights^T	Latent human rights variable for the target country with higher values indicating a better protection of human rights, lagged by one year. *Source:* Human Rights Protection Scores (Fariss 2019).
lag Successful Coups^T	Binary indicator for target country-years in which successful coups have occurred, lagged by one year. *Source:* Coup Dataset (Bjørnskov and Rode 2020).
lag Minor Conflict^T / lag Major Conflict^T	Armed conflicts resulting in 25 to 999 / at least 1,000 battle-related deaths in a given year in the target country, lagged by one year. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).

Table A3: Descriptive Statistics

Panel A: Full Dataset

	All Observations ($N = 293,594$)	No Sanctions ($N = 262,801$)	Sanctions ($N = 30,793$)
Continuous Variables (Mean Values)			
Leader Similarity	0.62	0.62	0.60
Gower's Leader Similarity	0.78	0.78	0.78
lag log(GDP pc)^T	8.32	8.37	7.88
lag log(Population)^T	9.06	9.03	9.29
lag Globalization^T	45.43	46.14	39.30
lag Human Rights^T	–0.07	0.03	–0.94
Binary Variables ($X = 1$ in percent)			
lag Democracy^T	34.21	36.14	17.68
lag Successful Coups^T	2.56	2.45	3.54
lag Minor Conflict^T	15.24	13.97	26.14
lag Major Conflict^T	6.12	4.95	16.05

Panel B: Main Estimation Dataset (w/o UN/EU Sanctions and w/o Absorbed Spells)

	All Observations ($N = 11,787$)	No Sanctions ($N = 10,688$)	Sanctions ($N = 1,099$)
Continuous Variables (Mean Values)			
Leader Similarity	0.62	0.62	0.60
Gower's Leader Similarity	0.79	0.79	0.79
lag log(GDP pc)^T	8.29	8.31	8.10
lag log(Population)^T	9.83	9.83	9.85
lag Globalization^T	46.32	46.85	41.18
lag Human Rights^T	–0.07	–0.03	–0.43
Binary Variables ($X = 1$ in percent)			
lag Democracy^T	38.13	39.91	20.75
lag Successful Coups^T	2.47	2.28	4.28
lag Minor Conflict^T	17.28	17.38	16.74
lag Major Conflict^T	4.79	5.08	2.00

Notes: Panel A shows descriptive statistics for the full dataset, Panel B for the dataset used in the estimations with two-way fixed effects and without sanctions imposed by the UN or the EU.

Additional Results

Table A4: Bias Corrected Probit Estimations (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad
Leader Similarity	–0.0104***	–0.0148***
	(0.0020)	(0.0020)
lag log(GDP pc)^T	0.0027***	0.0047***
	(0.0008)	(0.0008)
lag log(Population)^T	0.0038***	0.0068***
	(0.0004)	(0.0005)
lag Globalization^T	–0.0006***	–0.0007***
	(0.0001)	(0.0001)
lag Democracy^T		–0.0273***
		(0.0017)
lag Human Rights^T		–0.0039***
		(0.0007)
lag Successful Coups^T		0.0009
		(0.0037)
lag Minor Conflict^T		0.0112***
		(0.0018)
lag Major Conflict^T		–0.0357***
		(0.0033)
Sender-Year FE	Yes	Yes
Observations	165,364	165,364
Unique Dyads	11,801	11,801
Std. Dev. of Similarity	0.3504	0.3504

Notes: Marginal effects of bias-corrected panel probit models according to Fernández-Val (2009). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level.

Table A5: Linear Probability Models (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad	(3) Narrow	(4) Broad	(5) Broad	(6) Broad
Leader Similarity	–0.0145***	–0.0198***	–0.1393***	–0.0812**	–0.1288***	–0.1320***
	(0.0020)	(0.0028)	(0.0370)	(0.0330)	(0.0248)	(0.0219)
lag log(GDP pc)^T	0.0042***	0.0053***	–0.0804*	–0.0677*	–0.0970***	–0.1575***
	(0.0009)	(0.0013)	(0.0340)	(0.0346)	(0.0361)	(0.0285)
lag log(Population)^T	0.0032***	0.0090***	0.7100***	0.5050***	0.1082	–0.1617**
	(0.0004)	(0.0007)	(0.1150)	(0.1035)	(0.0747)	(0.0725)
lag Globalization^T	–0.0010***	–0.0008***	–0.0093***	–0.0068***	–0.0063***	–0.0118***
	(0.0001)	(0.0001)	(0.0024)	(0.0024)	(0.0019)	(0.0016)
lag Democracy^T		–0.0281***		–0.2657***	–0.1478***	–0.0853***
		(0.0017)		(0.0294)	(0.0211)	(0.0204)
lag Human Rights^T		–0.0003		–0.0372**	–0.0997***	–0.0486**
		(0.0008)		(0.0165)	(0.0165)	(0.0202)
lag Successful Coups^T		0.0001		0.0688***	0.0926***	0.0967***
		(0.0050)		(0.0256)	(0.0225)	(0.0186)
lag Minor Conflict^T		0.0104***		0.0790***	0.0089	0.0110
		(0.0021)		(0.0232)	(0.0175)	(0.0159)
lag Major Conflict^T		–0.0325***		0.0654*	–0.0408*	–0.0424*
		(0.0028)		(0.0353)	(0.0278)	(0.0243)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE			Yes	Yes	Yes	Yes
Linear Target Country Trends					Yes	Yes
Quadratic Target Country Trends						Yes
Observations	165,364	165,364	11,787	11,787	11,787	11,787
Unique Dyads	11,801	11,801	583	583	583	583
Std. Dev. of Similarity	0.3504	0.3504	0.3517	0.3517	0.3517	0.3517

Notes: Coefficients of linear probability models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The differences in the number of observations are due to the absorption of data spells by the fixed effects.

Table A6: Controlling for Country Affinity (w/o UN/EU Sanctions)

	(1)	(2)
Leader Similarity	–0.0155***	–0.0459*
	(0.0021)	(0.0250)
Country Affinity	0.0484***	–0.0251
	(0.0025)	(0.0187)
lag log(GDP pc)^T	0.0071***	–0.0506
	(0.0010)	(0.0311)
lag log(Population)^T	0.0052***	0.3630***
	(0.0006)	(0.1029)
lag Globalization^T	–0.0009***	–0.0037**
	(0.0001)	(0.0016)
lag Democracy^T	–0.0302***	–0.1786***
	(0.0020)	(0.0261)
lag Human Rights^T	–0.0068***	–0.0476***
	(0.0008)	(0.0133)
lag Successful Coups^T	0.0007	0.0383**
	(0.0040)	(0.0188)
lag Minor Conflict^T	0.0122***	0.0665***
	(0.0018)	(0.0160)
lag Major Conflict^T	–0.0367***	0.0383
	(0.0033)	(0.0296)
Sender-Year FE	Yes	Yes
Dyad FE		Yes
Observations	165,364	11,787
Unique Dyads	11,801	583
Std. Dev. of Similarity	0.3504	0.3517

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. The difference in the number of observations is due to the absorption of data spells by the fixed effects.

Table A7: Jackknife-Style Exclusion of Leader Characteristics (w/o UN/EU Sanctions)

	(1) Narrow	(2) Broad
excl. Military Experience	–0.1071***	–0.0453*
	(0.0292)	(0.0252)
excl. Education	–0.0999***	–0.0411*
	(0.0258)	(0.0228)
excl. Childhood Family	–0.1060***	–0.0441*
	(0.0281)	(0.0245)
excl. Adult Family	–0.1059***	–0.0442*
	(0.0280)	(0.0244)
excl. Occupation	–0.1033***	–0.0421*
	(0.0271)	(0.0235)
excl. Political Experience	–0.1214***	–0.0522
	(0.0465)	(0.0475)
excl. Other	–0.1086***	–0.0441*
	(0.0285)	(0.0249)
All Characteristics	–0.1088***	–0.0450*
	(0.0287)	(0.0250)
Observations	11,787	11,787
Unique Dyads	583	583

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5). The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level. Models include control variables (Narrow: lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T; Broad (additionally): lag Democracy^T, lag Human Rights^T, lag Successful Coups^T, and two lagged conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "All Characteristics" replicates the results from (6) of Tables 1 and 2, respectively.

Table A8: Gower's Leader Similarity (w/o UN/EU Sanctions)

	(1)	(2)	(3)	(4)
	Probit Models		Linear Probability Models	
	Narrow	Broad	Narrow	Broad
Gower's Leader Similarity	–0.4420***	–0.2452*	–0.6001***	–0.3685**
	(0.1287)	(0.1263)	(0.1605)	(0.1533)
lag log(GDP pc)^T	–0.0603*	–0.0468	–0.0774**	–0.0653*
	(0.0336)	(0.0315)	(0.0336)	(0.0343)
lag log(Population)^T	0.5215***	0.3762***	0.6874***	0.4890***
	(0.1134)	(0.1042)	(0.1117)	(0.1010)
lag Globalization^T	–0.0069***	–0.0041**	–0.0101**	–0.0072***
	(0.0018)	(0.0016)	(0.0024)	(0.0024)
lag Democracy^T		–0.1856***		–0.2695***
		(0.0273)		(0.0299)
lag Human Rights^T		–0.0497***		–0.0380**
		0.0133		(0.0166)
lag Successful Coups^T		0.0426**		0.0760***
		0.0184		(0.0251)
lag Minor Conflict^T		0.0649***		0.0782***
		(0.0154)		(0.0228)
lag Major Conflict^T		0.0418		0.0594*
		(0.0300)		(0.0359)
Sender-Year FE	Yes	Yes	Yes	Yes
Dyad FE	Yes	Yes	Yes	Yes
Observations	11,787	11,787	11,787	11,787
Unique Dyads	583	583	583	583
Std. Dev. of Similarity	0.0625	0.0625	0.0625	0.0625

Notes: Marginal effects of panel probit models according to Eqs. (1)–(5) and coefficients of linear probability models. The LHS variable indicates whether sanctions of sender country i against target country j are in place in year t. Standard errors in parentheses are clustered at the dyad level. ***/**/* indicates significance at the 1%/5%/10% level.