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Leader similarity and international sanctions

Abstract

Political leaders matter, but statistical evidence for their relevance in international politics is scarce. We estimate panel probit models with data for the period 1970 to 2004 and sender-year and dyad fixed effects to evaluate whether more similar leaders are less likely to sanction each other. We find that higher leader similarity significantly reduces the likelihood of sanction imposition. The effect is especially pronounced when UN and EU sanctions are excluded, that is, when focusing on sanctions imposed through unilateral political decisions. In this case, going from no correlation to perfect correlation in the characteristics of the leader pair lowers the likelihood of sanctions by 5.7 pp. Moreover, leader similarity seems to matter especially for sanctions aimed at democratic change or human rights improvements, where political leaders are expected to enjoy more discretion.

JEL Codes: D70, F51, K33.

Keywords: International sanctions, Leader similarity, Political leaders.

1 Introduction

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

"I knew Putin very well. I got along with him great. He liked me. I liked him."
(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 success 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 less attention has been paid to whether such traits of politicians matter for international diplomacy 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. 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 leaders. Foster and Keller (2023), for example, provide some ev-

idence 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.

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. The literature on international sanctions has, thus far, ignored individual leader characteristics as a cause of sanction imposition or effectiveness, despite their fast-growing importance in the broader international political economy literature. Most similar to our work is the study by DiLorenzo and Rooney (2023) who find that pairs of countries whose leaders share a similar background are less likely to engage in an interstate conflict. Our focus on sanctions instead of conflict implies some notable differences between our study and that of DiLorenzo and Rooney (2023). First, sanctions occur much more frequently than 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. 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 (2023). It covers 179,507 observations corresponding to 143 sender countries, 152 target countries, and 10,839 directed country-pairs over the period from 1970 to 2004. As many sanctions are imposed by international organizations consisting of a group of countries, an individual country and its leader might have a limited impact on these decisions. To account for this, we also estimate models excluding these multilateral sanctions.

Our main finding suggests that higher leader similarity significantly reduces the likelihood of sanction imposition. This result holds across various specifications that also control for the primary reasons for imposing sanctions. The effect is especially pronounced when UN and EU sanctions are excluded, that is, when focusing on sanctions imposed through unilateral political decisions. In this case, going from no correlation to perfect correlation in the characteristics of the leader pair lowers the likelihood of sanctions by 5.7 pp. Moreover, we find that 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. These are the types of sanctions for which political leaders are expected to enjoy most discretion regarding their imposition.

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

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 (Ertug et al. 2020). In-group bias describes the tendency to trust 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.[1] Both homophily and in-group bias can be motivated either by taste or by expectations about the behav-

1. Falk and Zehnder (2013) show in a field experiment that in-group bias even exists among inhabitants of the same city district.

ior 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 (2023), 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.

To develop our theoretical argument, we first have to take a step back and explain why countries use international sanctions. 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) 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).

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 cases where sanctions would not be deployed in equilibrium, even if the target made no concession, the sender government would not even bother to formulate a threat. Therefore, implemented international sanctions are a second-best instrument for a world of incomplete information. Transaction costs concern the costliness of the bargaining process, but also whether information on the sender's and the receiver'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, sanctions can 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 human rights or international law, although it is not able to change the target government's policy choice (Hovi et al. 2005).

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 transaction costs and, thereby, widens the scope for voluntary agreements between the two nations. 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. 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. Finally, if more

similar leaders have more trust in each other, the use of sanctions could potentially erode that trust and thereby reduce their ability to conclude unrelated mutually beneficial agreements in the future. In other words, sanctions might be less likely to be imposed between more similar leaders, because their opportunity costs are higher. 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 Estimation Strategy and Data

3.1 Estimation Strategy

To evaluate the effect of leader similarity on sanction imposition, we estimate panel probit models.[2] These allow modeling binary outcomes while accounting for unobserved heterogeneity and non-linear time trends. The general model specification can be described as follows:

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

$y^*_{i,j,t}$ 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) leader 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

2. Hahn and Newey (2004) show that the bias resulting from the incidental parameters problem in panel probit models is small. Moreover, the estimation procedure calculates coefficients for the fixed effects only if there is variation in the dependent variable within a data spell. Spells without this variation are excluded from the estimations.

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 various time-invariant standard control variables, such as common borders, shared languages, and cultural, genetic, geographic, and political proximity. 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. For comparison, DiLorenzo and Rooney (2023) control "only" for dyad fixed effects and year fixed effects. Our specification does not include target-year fixed effects, as target country leaders do not choose to be targeted by sanctions.[3] We do, however, account explicitly for a number of potentially confounding time-varying target country characteristics. $X_{j,t}^{pol}$ represents political and $X_{j,t-1}^{econ}$ represents one-year lagged economic control variables. They are described in more detail in Section 3.2. $\epsilon_{i,j,t}$ is an idiosyncratic error term.

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 $\Phi(\cdot)$:

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

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

3. 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 only serve to explain the composition of those sender countries. Observations corresponding to targets of UN sanctions or countries not sanctioned in a given year would be dropped. This would substantially reduce our sample size and discard most of the variation in the sanction imposition variable, ultimately causing the probit models to fail to converge.

Since these estimates describe the effects on the latent variable, we have to calculate marginal effects to quantify the effects 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 ($S_{i,j,t}$) is calculated as follows:

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

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 first calculate the marginal effects for each observation and then take the simple average. Finally, we use the delta method to estimate the standard errors of the marginal effects:

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

All reported standard errors are clustered at the dyad level.

3.2 Data

Our dataset contains 179,507 observations covering 143 sender countries, 152 target countries, and 10,839 directed country-pairs for the period from 1970 to 2004. Lists of sender and target countries can be found in Tables A1 and A2. Our dependent variable is a binary indicator showing 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 in the case of 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 sam-

ple (21,406 dyad-years with sanctions in place), (ii) excluding observations with UN sanctions (10,432 dyad-years with sanctions in place), and (iii) excluding observations with UN and EU sanctions (9,021 dyad-years with sanctions in place). 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).

Our main independent variable is a leader similarity index introduced by DiLorenzo and Rooney (2023). They calculate Pearson's R correlation coefficient over 58 leader-level characteristics, separately for every leader pair. Data on leader characteristics comes from the LEAD dataset by Ellis et al. (2015), covering personal background information and life experiences (e.g., military experience, education, occupational history, political experience). To deal with leadership changes within a given year, we construct a leader similarity score for years of leadership transition, which is the average of the 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. The economic situation is captured by the country's real GDP per capita (in logs and lagged by one year to mitigate potential reverse causality). In addition, our models include the population size (also in logs and lagged by one year) and a globalization index (KOFGI, lagged by one year, 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), latent human rights scores by Fariss (2019), and dummy variables for minor and major conflicts (Gleditsch et al. 2002).

Table A3 provides a list of variables alongside their definitions and the underlying data sources. Table A4 shows descriptive statistics, also separately for 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 experience minor and major conflicts at a much higher frequency.

4 Empirical Results

4.1 Baseline Results

Table 1 shows the average marginal effects 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).[4] 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 9.1–12.5 percentage point (pp) decrease in sanction occurrences. Since the similarity variable is based on Pearson's correlation coefficient, the marginal effect can be interpreted as the shift from no correlation (0) to perfect correlation (1) in the characteristics of the leader pair. The empirical distribution of the similarity

4. 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 80%–90% of the original sample.

variable varies – depending on the sample – with minimum values between –0.08 and –0.09 and maximum values equal to 1. The sample mean is in the range of 0.61 and 0.66 and the standard deviation is between 0.35 and 0.37. Hence, the effect remains substantial even when it is judged by a one standard deviation increase in leader similarity, which is still associated with a change in the sanction probability ranging from –3.2 to –4.4 pp.

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 marginal effects 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 only reach significance – at the 10% level in Column (5) and at the 5% level in Column (6), that is, when at least UN sanctions are excluded. 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 5.7 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.

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	–0.0462***	–0.0427***	–0.0246***	–0.0908***	–0.1045***	–0.1245***
	(0.0029)	(0.0031)	(0.0029)	(0.0186)	(0.0293)	(0.0335)
lag log(GDP pc)^T	0.0110***	–0.0013	0.0013	–0.3354***	–0.1451***	–0.0731**
	(0.0011)	(0.0013)	(0.0013)	(0.0192)	(0.0281)	(0.0370)
lag log(Population)^T	0.0108***	0.0114***	0.0098***	0.0619**	0.9341***	0.6346***
	(0.0006)	(0.0005)	(0.0005)	(0.0294)	(0.0823)	(0.1062)
lag Globalization^T	–0.0053***	–0.0017***	–0.0014***	–0.0103***	–0.0030*	–0.0025
	(0.0001)	(0.0001)	(0.0001)	(0.0013)	(0.0018)	(0.0018)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE				Yes	Yes	Yes
Observations	179,507	122,690	121,161	33,087	13,504	10,032
Std. Dev. of Similarity	0.3402	0.3374	0.3360	0.3536	0.3669	0.3503

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	–0.0363***	–0.0329***	–0.0170***	0.0021	–0.0454*	–0.0567**
	(0.0029)	(0.0030)	(0.0028)	(0.0178)	(0.0249)	(0.0280)
lag log(GDP pc)^T	0.0136***	–0.0006	0.0018	–0.2967***	–0.1344***	–0.0720**
	(0.0010)	(0.0013)	0.0012)	(0.0198)	(0.0251)	(0.0355)
lag log(Population)^T	–0.0031***	0.0120***	0.0117***	–0.0340*	0.7026***	0.4535***
	(0.0009)	(0.0007)	(0.0007)	(0.0178)	(0.0774)	(0.1027)
lag Globalization^T	–0.0030***	–0.0007***	–0.0005***	–0.0035***	0.0008	–0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0010)	(0.0016)	(0.0017)
Democracy^T	–0.0337***	–0.0539***	–0.0516***	–0.2831***	–0.2164***	–0.1898***
	(0.0020)	(0.0024)	(0.0025)	(0.0231)	(0.0274)	(0.0261)
Human Rights^T	–0.0356***	–0.0047***	–0.0017*	–0.1332***	–0.0801***	–0.0693***
	(0.0013)	(0.0011)	(0.0010)	(0.0086)	(0.0143)	(0.0120)
Minor Conflict^T	0.0111***	–0.0017	–0.0015	0.0140	0.0814***	0.0913***
	(0.0020)	(0.0022)	(0.0022)	(0.0123)	(0.0174)	(0.0189)
Major Conflict^T	0.0395***	–0.0518***	–0.0568***	0.1231***	–0.0108	0.0279
	(0.0031)	(0.0040)	(0.0043)	(0.0165)	(0.0285)	(0.0320)
Sender-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Dyad FE				Yes	Yes	Yes
Observations	179,507	122,690	121,161	33,087	13,504	10,032
Std. Dev. of Similarity	0.3402	0.3374	0.3360	0.3536	0.3669	0.3503

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.

Turning briefly to the control variables themselves, we find that a higher (lagged) GDP per capita reduces the likelihood of sanctions. Globalization shows a similar negative effect, though not in the smallest samples. Sanctions are also less likely used against democratic countries and those with greater respect for human rights. While conflicts 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, as shown in Columns (1)–(3). 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.

4.2 Extensions and Robustness Tests

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.[5] Table 3 presents the results under the more conservative setting with dyad fixed effects as well as the broader set of control variables used in 2. In line with our theoretical considerations and empirical evidence provided above (cf. Table 2), we focus here on results based on samples without UN sanctions and without both UN and EU sanctions.

5. 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.

Table 3: Effect Heterogeneity for Sanction Objectives: Broad Set of Controls

	Excl. "Democracy"		**Excl. "Human Rights"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0053	–0.0062	–0.0297	–0.0122
	(0.0325)	(0.0339)	(0.0266)	(0.0285)
Observations	8,759	7,043	10,453	8,860
	Excl. "Destab. Regime"		**Excl. "Policy Change"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0515**	–0.0686**	–0.0806**	–0.0855**
	(0.0243)	(0.0268)	(0.0317)	(0.0425)
Observations	13,016	9,544	7,883	5,161
	Excl. "Prevent War"		**Excl. "End War"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0797**	–0.0703*	–0.0382	–0.0531*
	(0.0310)	(0.0418)	(0.0249)	(0.0301)
Observations	8,666	5,681	13,135	9,035
	Excl. "Territ. Conflict"		**Excl. "Terrorism"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0493**	–0.0657**	–0.0429*	–0.0578**
	(0.0249)	(0.0278)	(0.0253)	(0.0281)
Observations	12,943	9,471	12,568	9,596
	Excl. "Other Objectives"		**Full Dataset**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0684***	–0.0872***	–0.0454*	–0.0567**
	(0.0240)	(0.0263)	(0.0249)	(0.0280)
Observations	13,045	9,552	13,504	10,032

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 and EU sanctions, (ii) the absorption of data spells by the fixed effects, and (iii) the exclusion of sanctions of a particular objective. Models include control variables (lag log(GDP pc)^T, lag log(Population)^T, lag Globalization^T, Democracy^T, Human Rights^T, and two conflict indicators), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from Columns (5) and (6) of Table 2.

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. This suggests that leader similarity plays most into the decision whether to impose sanctions in situations where demo-

cratic 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 (Coyne 2008; Steinbach et al. 2023, see, e.g.,). 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 baseline, indicating that leader similarity is less relevant to sanction imposition when these objectives are concerned.

Table A5 in the Appendix shows the marginal effects without political control variables. The results for leader similarity are comparable to those in Table 3. The main difference is that the effect of leader similarity remains marginally significant when sanctions aimed at improving human rights are excluded.

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 179,507 observations corresponding to 143 sender countries, 152 target countries, and 10,839 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, also when controlling for the primary reasons for sanction imposition (human rights violations, conflict involvement, and democratic deficits). 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. A one-unit increase in leader similarity, that is, going from no correlation to perfect correlation in the char-

acteristics of the leader pair, lowers the likelihood of sanctions by 5.7 pp. 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.

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.

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Appendix

Data Description

Table A1: Sender Countries

Sender Countries (number of observations)

Afghanistan (1711), Albania (1436), Algeria (4147), Angola (1416), Argentina (3592), Armenia (964), Australia (4246), Austria (3975), Azerbaijan (984), Bahrain (3194), Bangladesh (2103), Belarus (1293), Belgium (3826), Benin (1541), Bhutan (182), Bolivia (2499), Botswana (1143), Brazil (2868), Bulgaria (2056), Burkina Faso (1230), Burundi (1439), Cambodia (1205), Cameroon (2142), Canada (3499), Central African Republic (842), Chad (1187), Chile (2857), China (2351), Colombia (2523), Comoros (550), Costa Rica (2231), Croatia (1098), Cuba (1473), Cyprus (2194), Czech Republic (929), Denmark (3424), Djibouti (1392), Dominican Republic (1555), Ecuador (2083), Egypt (3189), El Salvador (243), Equatorial Guinea (441), Eritrea (222), Estonia (969), Fiji (1515), Finland (3092), France (3066), Gabon (1480), Gambia (1373), Georgia (627), Ghana (1877), Greece (2903), Guatemala (1050), Guinea (1271), Guinea-Bissau (558), Guyana (1546), Haiti (1554), Honduras (417), Hungary (1412), India (1936), Indonesia (1554), Iran (1122), Iraq (1420), Ireland (2194), Israel (1284), Italy (2537), Jamaica (1840), Japan (2566), Kazakhstan (730), Kenya (1364), Kuwait (1877), Kyrgyzstan (438), Laos (681), Latvia (603), Lebanon (1911), Lesotho (586), Liberia (601), Libya (1803), Lithuania (686), Luxembourg (1903), Malawi (1096), Mali (769), Mauritania (1530), Mauritius (1213), Mexico (1065), Moldova (514), Mongolia (559), Morocco (1675), Mozambique (697), Myanmar (863), Namibia (430), Nepal (493), Netherlands (1757), New Zealand (1178), Nicaragua (940), Niger (748), Nigeria (1059), North Macedonia (361), Norway (1633), Pakistan (899), Panama (970), Papua New Guinea (550), Paraguay (874), Peru (1055), Philippines (759), Poland (753), Portugal (1381), Qatar (801), Romania (693), Russia (446), Rwanda (506), Saudi Arabia (969), Senegal (635), Serbia (187), Sierra Leone (524), Singapore (659), Slovakia (371), Slovenia (301), Somalia (99), South Africa (398), South Korea (354), Spain (1104), Sudan (705), Suriname (261), Swaziland (258), Sweden (931), Syria (499), Tajikistan (149), Tanzania (399), Thailand (398), Togo (341), Tunisia (500), Turkey (669), Turkmenistan (118), UAE (328), Uganda (258), UK (721), Ukraine (139), Uruguay (265), USA (559), Uzbekistan (38), Venezuela (168), Zambia (114).

Table A2: Target Countries

Target Countries (number of observations)

Afghanistan (852), Albania (443), Algeria (89), Angola (971), Argentina (91), Armenia (480), Australia (134), Austria (191), Azerbaijan (490), Bahrain (204), Bangladesh (193), Belarus (217), Belgium (288), Benin (306), Bhutan (73), Bolivia (236), Botswana (194), Brazil (347), Bulgaria (740), Burkina Faso (337), Burundi (503), Cambodia (406), Cameroon (467), Canada (522), Central African Republic (255), Chad (289), Chile (602), China (1092), Colombia (572), Comoros (260), Costa Rica (504), Croatia (741), Cuba (1032), Cyprus (696), Czech Republic (276), Denmark (784), Djibouti (367), Dominican Republic (332), Ecuador (633), Egypt (1057), El Salvador (90), Equatorial Guinea (491), Eritrea (218), Estonia (389), Ethiopia (447), Fiji (943), Finland (957), France (980), Gabon (817), Gambia (839), Georgia (295), Ghana (959), Greece (1102), Guatemala (324), Guinea (922), Guinea-Bissau (515), Guyana (896), Haiti (849), Honduras (234), Hungary (1326), India (1684), Indonesia (1300), Iran (903), Iraq (1037), Ireland (1274), Israel (1434), Italy (1387), Jamaica (966), Japan (1512), Jordan (1463), Kazakhstan (565), Kenya (1291), Kuwait (1444), Kyrgyzstan (371), Laos (1029), Latvia (539), Lebanon (1572), Lesotho (779), Liberia (1205), Libya (2105), Lithuania (677), Luxembourg (1245), Madagascar (1415), Malawi (1311), Malaysia (1734), Mali (1182), Mauritania (1374), Mauritius (1629), Mexico (1164), Moldova (622), Mongolia (1094), Morocco (1841), Mozambique (1313), Myanmar (1617), Namibia (537), Nepal (977), Netherlands (1727), New Zealand (2070), Nicaragua (1198), Niger (1587), Nigeria (2025), North Macedonia (862), Norway (2095), Oman (1728), Pakistan (2157), Panama (1566), Papua New Guinea (1289), Paraguay (1691), Peru (2063), Philippines (2349), Poland (2289), Portugal (2351), Qatar (1612), Romania (2377), Russia (1088), Rwanda (1925), Saudi Arabia (2467), Senegal (2365), Serbia (1342), Sierra Leone (2016), Singapore (2518), Slovakia (1081), Slovenia (1019), Solomon Islands (32), Somalia (285), South Africa (1437), South Korea (1282), Spain (2543), Sri Lanka (2654), Sudan (2410), Suriname (1417), Swaziland (1476), Sweden (2696), Syria (2297), Tajikistan (691), Tanzania (2413), Thailand (2891), Togo (2398), Tunisia (2879), Turkey (2751), Turkmenistan (764), UAE (2442), Uganda (2246), UK (2869), Ukraine (1232), Uruguay (2453), USA (1262), Uzbekistan (594), Venezuela (2609), Yemen (1178), Zambia (2748), Zimbabwe (2250).

Table A3: 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 2023), 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).
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. *Source:* Polity5 dataset (Marshall and Gurr 2020).
Human Rights^T	Latent human rights variable for the target country with higher values indicating a better protection of human rights. *Source:* Human Rights Protection Scores (Fariss 2019).
Minor Conflict^T / 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. *Source:* UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002; Davies et al. 2022).

Table A4: Descriptive Statistics

	All Observations ($N = 179,507$)	**No Sanctions** ($N = 158,409$)	**Sanctions** ($N = 21,098$)
Continuous Variables (Mean Values)			
Leader Similarity	0.64	0.64	0.58
lag log(GDP pc)^T	8.42	8.49	7.86
lag log(Population)^T	9.20	9.19	9.34
lag Globalization^T	48.03	49.08	40.15
Human Rights^T	–0.01	0.10	–0.83
Binary Variables ($X = 1$)			
Democracy^T	70,204	66,396	3,808
Minor Conflict^T	27,264	22,386	4,878
Major Conflict^T	10,951	7,836	3,115

Notes: The upper panel of the table shows mean values and the lower panel the frequency of non-zero observations for the key explanatory variable and all control variables.

Additional Results

Table A5: Effect Heterogeneity for Sanction Objectives: Narrow Set of Controls

	Excl. "Democracy"		**Excl. "Human Rights"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.0400	–0.0423	–0.0657**	–0.0546*
	(0.0375)	(0.0390)	(0.0290)	(0.0308)
Observations	8,759	7,043	10,453	8,860
	Excl. "Destab. Regime"		**Excl. "Policy Change"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.1125***	–0.1395***	–0.1202***	–0.1321***
	(0.0289)	(0.0320)	(0.0341)	(0.0451)
Observations	13,016	9,544	7,883	5,161
	Excl. "Prevent War"		**Excl. "End War"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.1178***	–0.1255***	–0.1008***	–0.1303***
	(0.0339)	(0.0473)	(0.0298)	(0.0365)
Observations	8,666	5,681	13,135	9,035
	Excl. "Territ. Conflict"		**Excl. "Terrorism"**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.1115***	–0.1361***	–0.1097***	–0.1215***
	(0.0298)	(0.0344)	(0.0305)	(0.0337)
Observations	12,943	9,471	12,568	9,596
	Excl. "Other Objectives"		**Full Dataset**	
	w/o UN	w/o UN/EU	w/o UN	w/o UN/EU
Leader Similarity	–0.1289***	–0.1554***	–0.1045***	–0.1245***
	(0.0283)	(0.0297)	(0.0293)	(0.0335)
Observations	13,045	9,552	13,504	10,032

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 and EU sanctions, (ii) the absorption of data spells by the fixed effects, and (iii) the exclusion of sanctions of a particular objective. Models include control variables (lag log(GDP pc)^T, lag log(Population)^T, and lag Globalization^T), sender-year fixed effects, and dyad fixed effects. Estimates are available on request. "Full Dataset" replicates the results from Columns (5) and (6) of Table 1.