Economic Peace Revisited:

Coercion and Democracy

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Abstract

Scholars have argued whether democratic peace also holds in the realm of economic sanctions — whether there is an economic peace. Substantial amounts of evidence have been gathered both for and against economic peace and findings have been extremely sensitive to changes in research design. This article provides a new insight, with the use of the updated TIES data set and improved methodology, into the topic of economic peace. It find that democracies are more likely to issue economic sanctions and that there is no economic peace. In fact, democracies are more likely to sanction one another. The article indicate that lack of economic peace is consistent with the public choice approach to economic sanctions. It also argue that the exercise of power among democracies has been rechannelled to economic coercion.

1 Introduction

Democracies do not go to war with one another (Bueno de Mesquita et al., 1999). However, does this special relation between democracies extend beyond the military domain, to economic sanctions?[1] Although researchers have argued that domestic structural constraints that democratic leaders face (Lektzian and Souva, 2003), or norms that they follow (Cox and Drury, 2006) ought to make democracies less likely to sanction one another, empirical findings on the presence of economic peace are mixed (Lektzian and Souva, 2003; Cox and Drury, 2006; Hafner-Burton and Montgomery, 2008), and the relationship between economic and democratic peace remains unclear.[2]

The expectation of democratic peace is based on the theoretical premises that political leaders are voted out of office in case of a war that is lost, and that democratic societies are resilient targets of military interventions (Bueno de Mesquita et al., 1999). The argument is that interaction between these structural characteristics of democracies makes war between them unlikely. Scholars also go beyond the structural argument and point to normative factors underlying democratic peace, for example, a common value system shared by democratic societies (Dixon, 1994). These structural and normative approaches to democratic peace are mirrored in the theoretical work on economic peace, which argues that the same set of constraints that restrains democratic leaders from engaging in war ought to diminish the prospects for economic sanctions (Lektzian and Souva, 2003; Cox and Drury, 2006). However, the theoretical frameworks on economic peace, derived from democratic peace literature, contrast with the recent empirical research on economic sanctions. First, scholars find that voters favour economic coercion regardless of the outcomes of the policy and the democracy level of the target state (Whang, 2011). Second, there is no evidence that democracies are more resilient targets of economic sanctions, nor that democratic leaders are less likely to impose sanctions on important economic partners (Bapat and Kwon, 2015). This would suggest that the building blocks of the democratic peace theory are not empirically supported with respect to economic peace and, consequently, the relationship between democratic and economic peace is not straightforward.

This article shows that democracies are more likely to issue economic sanctions and there is no economic peace between democratic states. In fact, democracies are more likely to sanction one another — thus, the opposite of an economic peace seems to exist in international relations. The empirical findings of this article indicate that the public choice theoretical framework on the use of economic sanctions (Kaempfer and Lowenberg, 1988) appears most accurate in depicting the relation between economic coercion and democracy. In the public choice framework, democracies are more likely to issues sanctions because the objective of elected leaders is to respond to special interest groups and build broad domestic support, and through economic sanctions they address both

[1] I define economic sanctions, following Morgan et al. (2014), as "actions that one or more countries take to limit or end their economic relations with a target country in an effort to persuade that country to change its polices".

[2] I define economic peace as a propensity of "democratic states to be less likely to sanction one another compared to other regime types" (Wallace, 2013).

foreign policy and protectionist demands from voters. What is more, as democracies are unlikely to engage in war with one another, yet democratic leaders do experience pressure from interest groups towards conflict with other democratic states, coercion between democracies may be rechannelled to economic sanctions — potentially explaining the higher propensity of democracies to sanction one another. This argument addresses a recent call in international relations, as current economic sanctions literature "primarily looks to explain the success or failure of direct sanctions" (Farrell and Newman, 2019) and does not engage in the study of why sanctions come about, and, more broadly, how power is exercised in international relations.

Work on economic peace, apart from theoretical concerns, also raises a number of empirical questions. The results on the presence of economic peace are sensitive to every new data set and to methodological choices of researchers (Hafner-Burton and Montgomery, 2008; Wallace, 2013). With respect to the data, in this article I study the prospect of economic peace using the updated TIES data set (Morgan et al., 2014), the Polity IV data set (Marshall et al., 2018), and the Political Regimes data set (Boix et al., 2013). While the most recent work on economic peace (Wallace, 2013) is based on the first edition of the TIES data set, the updated TIES contains 59% more cases and covers additional years (Morgan et al., 2014).

Unlike past studies, this article (i) conducts the empirical analysis with a logistic regression, (ii) treats threats-only of sanctions as a counterfactual to imposed sanctions, and (iii) offers an improved specification and interpretation of the interaction effects. Previous research employed a rare event logit model that is highly sensitive to changes in the non-event (i.e. no sanctions dyad) section of the sample. However, the updated TIES data set offers both information on imposed sanctions and on threats of sanctions, allowing for a change of the statistical method from a rare-event logit to a logistic regression.[3] What is more, scholars of economic peace have not distinguished between relevant counterfactual events to a sanction imposition (i.e. an unrealised sanction threat) and alternative means of coercion (e.g. war or diplomacy) — combining all outcomes as non-sanctions — what may result in a biased estimate. An attempt to address this problem in previous research — with identifying conditions for a "potential conflict" — is not explicitly specified in the literature and does not allow for replication. Hence, in this article, a clear strategy to identify a counterfactual event to an imposed sanction is offered; an approach in line with recent research on economic sanction (Walentek et al., 2021; Gutmann et al., 2021).

In the next section I discuss the literature related to democratic and economic peace and identify the main tenets in the current scholarship. After that, in Section 3, I outline the research design for this study, discussing the data, variables and the econometric model employed. Finally, the results of the regression analysis are presented in Section 4 and conclusions in Section 5.

[3]The original TIES data set also offered cases of sanction threats only; however, Wallace (2013) decided to remove these from the analysis and censor the sample, from 888 to 585 cases. The argument for studying only imposed sanctions was to keep the research design as close to previous studies as possible. The HSE data set used in previous research, unlike the TIES data, offers no information on threats.

2 What drives peace

2.1 Democratic peace literature

Democratic peace, one of the major tenets in political science, rests on the argument, and repeated empirical evidence, that democracies do not wage war against one another. It emerged in its current form nearly 200 years after Kant's work on perpetual peace, where a similar argument is presented (Russett et al., 1998), as a field of research focused on establishing a statistical relation between democracy and peace (Babst, 1972; Small and Singer, 1976). After establishing the presence of this relation, scholars went on to assess the mechanisms underpinning the apparent democratic peace, focusing predominantly on the structural (Bueno de Mesquita et al., 1999) and normative (Dixon, 1994) constraints that prevent democratic leaders from engaging in war.

The structural approach to democratic peace emphasises two aspects: the resilience of democratic states in face of conflict and lack of appetite among voters for war. With respect to the first argument, democratic states are considered resilient targets of military interventions, because of the rally-round-the-flag effect. Citizens in democracies strongly resist a foreign intervention, making a successful military campaign against a democracy unlikely (Mueller, 1970; Bueno de Mesquita et al., 1999). And, as voters in democracies tend to punish leaders who lose a war, democracies are less likely to target other democracies with military intervention (Bueno de Mesquita et al., 1999). With respect to the latter, citizens being the ones bearing the burden of a military confrontation, in terms of both economic cost and human loss, makes war efforts unlikely to be popular with voters. This makes war a difficult platform to build political capital on, further reducing the prospects of a war between democracies (Morgan and Campbell, 1991). Thus, following the structural democratic peace argument, a political leader interested in preserving power will be less likely to engage in military conflict with another democracy, fearing a prolonged war that eventually fails and the popular discontent that accompanies a military intervention, both of which are likely to remove a politician from office.

With respect to the normative approach, scholars argue that, as a result of shared norms and liberal values, democracies are less inclined to engage in military conflict with other democracies. The argument is that democratic states have developed a sense of community, and also frequently institutionalise this communal sentiment. In turn, these institutions allow the non-violent resolution of conflict between democracies (Dixon, 1994; Maoz and Russett, 1993).[4]

[4]Literature on the democratic peace is broader and more in-depth than the discussed publications; however, scholars engaged in economic peace research relate to the concepts discussed in the listed articles. For an excellent overview of the democratic peace literature see the work of Hayes (2012).

2.2 Economic peace literature

The idea of democratic peace and the particular behaviour of democracies in conflict situations has prompted a search for parallel trends for economic sanctions. Following the structural democratic peace argument and borrowing from the public choice approach to economic sanctions (Kaempfer and Lowenberg, 1988), Lektzian and Souva (2003) propose that the presence of democratic institutions makes democracies less likely to sanction each other but more likely to issue sanctions relative to non-democracies. Both characteristics are a result of the constraints that democratic leaders face. First, following the democratic peace argument, due to high costs of a failed foreign policy — removal from the office — incumbents prefer weak targets. Consequently, as democracies are known for their resilience, democratic leaders are more likely to select nondemocracies as targets of economic sanctions, and are less inclined to sanction one another. Second, relating to the public choice approach, winning coalitions in democracies tend to be broad and encompass a large variety of interest groups, concerning, for example, security, human rights or protectionist demands. Consequently, democratic leaders are more prone to use sanctions in order to stay in office because they have to satisfy a broader audience than their autocratic counterparts, for whom a number of concerns, like championing human rights abroad, are not relevant to staying in power.

Lektzian and Souva (2003) find empirical support for their structural economic peace argument, and observe that democracies are both more likely to issue sanctions and less likely to sanction one another. Nevertheless, other recent empirical evidence suggests that, regardless of the policy outcome, democratic leaders receive a domestic audience benefit from the use of economic sanctions (Whang, 2011), and that there is no empirical evidence for democracies being more resilient in face of economic coercion (Bapat and Kwon, 2015). Furthermore, the structural democratic peace and public choice approach to sanctions — two frameworks that, according to Lektzian and Souva, work together — may hold independently. If the benefit to a democratic leader from pursuing a sanction policy, for example by additionally sheltering a domestic industry (Pond, 2017), is greater than the cost resulting from a failed policy attempt, then we will not observe economic peace, but there will be a higher propensity among democracies to issue sanctions (regardless of the target) — in line with the public choice approach. On the other hand, we may observe economic peace only, because of the potentially large (audience) cost for the democratically elected leader associated with losing a sanction episode — under the assumption that democracies are resilient targets of economic coercion. This may occur while sanctions generate a coalition broad enough to boost popularity for a democratic leader and result in an inclination to restore more frequently to economic coercion; however, only against non-democracies.

Although Cox and Drury (2006) provide empirical evidence on economic democratic peace through methodological improvements, they highlight the effects of norms, rather than institutions, on the relations between democracies. This follows the normative argument in the democratic

peace literature (Dixon, 1994) that democracies are more likely to pursue a norms-based foreign policy. Since democracies advocate human rights and democratisation with economic sanctions, they exhibit a higher propensity to target non-democracies with economic coercion. Cox and Drury (2006) further argue that the fact that democracies do not sanction each other is a result of shared values. This contrasts with Lektzian and Souva (2003), who argue that only strong economic ties and structural incentives drive economic peace between democracies. However, recent scholarship on economic sanctions contradicts the normative economic peace framework. Rather than supporting the argument that economic sanctions serve the purpose of human rights promotion, it finds that they are oriented towards the domestic audience of the sender state (Whang, 2011).[5] If the normative account on economic sanctions holds true, we should at least observe that democracies are less likely to sanction one another and more likely to issue economic sanctions in general.

Consequently, we can summarise the existing theoretical work on economic peace in four sets of observable implications:

- First, if both the structural economic peace and the public choice argument hold, we ought to observe that democracies are less likely to sanction one another, more likely to issue sanctions, and less likely to be a target of economic sanctions;
- Second, if only the structural economic peace argument holds, we ought to observe that democracies are less likely to sanction one another and less likely to be a target of economic coercion;
- Third, if only the public choice approach argument holds, we ought to only observe that democracies are more likely to issue economic sanctions, regardless of the target;
- Finally, if only the normative approach holds, we ought to observe that democracies are less likely to sanction one another and more likely to issue sanctions.

In order to assess these theoretical arguments, this article tests the following three hypotheses:

H1: Democracies are less likely than non-democracies to sanction one another.

H2: Democracies are more likely than non-democracies to issue economic sanctions.

H3: Democracies are less likely than non-democracies to be the target of economic sanctions.

Besides broader theoretical frameworks, scholars of economic peace have arrived at a number of puzzling empirical conclusions. To begin with, Hafner-Burton and Montgomery (2008) suggest that the findings of Cox and Drury (2006) and, indirectly, those of Lektzian and Souva (2003) are the result of limited data and methodological weaknesses. Hafner-Burton and Montgomery show that democracies indeed issue sanctions more often (public choice argument), but are *not* less likely to sanction one another. They argue that, in the previous studies, it is the specific behaviour of the United States (US) that drives the presence of economic peace among democracies. They propose that the theoretical argument of public choice on economic sanctions holds, but that economic peace

[5] In fact, economic sanctions show a poor record with respect to addressing human rights issues (Peksen, 2009).

— whether driven by norms or structure — is only present because of the US. If Hafner-Burton and Montgomery are correct, democracies are more likely to issue economic sanctions and the US is the only democracy less likely to sanction other democracies:

H4: The US is the only democracy less likely to sanction other democracies rather than non-democracies.

What is more, Wallace (2013) proposes that the work of Hafner-Burton and Montgomery (2008) suffers itself from a data bias, so that, while it shows that democracies are more likely to issue sanctions (public choice argument) and are less likely to sanction one another (economic peace argument), this is only true for security issues. For non-security issues (e.g. trade or environmental policy), following Wallace, there is no economic peace between democracies. Furthermore, in Wallace, the special role of the US proposed by Hafner-Burton and Montgomery is absent, suggesting that, with respect to economic coercion, the US may act just as other democracies do. Wallace therefore argues that all sides of the argument on economic peace are partially correct: there is an economic peace, but it only holds for security issues. So, if Wallace's argument holds, there should be a different dynamic with respect to imposition of economic sanctions, subject to the type of the issue:

H5: Economic peace between democracies holds only for sanctions in the security domain.

3 Research design

3.1 Data

Threat and Imposition of Sanctions The TIES (v4.0) data set (Morgan et al., 2014) is currently the most complete collection of data on economic sanctions; it draws on 1,412 cases and covers the period from 1945 to 2005.[6] The key contribution of this data set is information on sanction threats, for 1,053 cases. This allows researchers to distinguish between imposed sanctions and threats only, creating scope for a counterfactual analysis. The HSE (Hufbauer et al., 2007) data set does not incorporate information on sanction threats. In the TIES data set 48% of sanction are in the trade domain. The remaining 52% are sanctions related to non-trade issues, for example non-proliferation. The US is the most active actor with respect to economic coercion, and has participated in 48% of the cases in the data set. If a negotiated settlement outcome and an on-going case are treated as failures, the effectiveness of economic sanction in the TIES data is 27%. If negotiated settlement is treated as a success but the on-going cases still as a failure, the success rate of sanctions increases to 40%. In this study, I employ the latter definition of success, as is common in research using the TIES data (Bapat and Kwon, 2015; Bapat and Morgan, 2009).

[6] Available at: http://sanctions.web.unc.edu. Nb: Although the coders of the TIES data set do not treat policies aimed at protection of a domestic industry as sanctions, they do record sanctions with the objective of changing a trade policy of another state.

POLITY IV The Polity IV data set (Marshall et al., 2018) provides information about the level of democratisation of states over time.[7] The observations, from 1800 to 2017, offer insight into the quality of democracy among 167 states. I use the democracy score (*DEMOC*) variable, which varies from 0 to 10, a numerical score for the number of democratic institutions that a country has, where 0 is a full autocracy, where citizens have no influence on the government, and 10 stands for a fully democratic society, with a complete array of democratic institutions. However, the democracy score is only available for 1,221 sender states and 1,249 target states and for 1,100 sender-target pairs.[8] Focusing solely on cases where a public threat was issued decreases the number for the sender-target democracy dyad further, to 807 cases.

Political Regimes The Political Regimes (PR) data set (Boix et al., 2013) allows us to test the robustness of the findings.[9] This data set contains information about the democracy level of 219 countries between 1800 and 2007, focusing not only on institutions, as in the Polity IV data set, but also on political contestation and popular participation. This allows testing of the findings from a different perspective on democracy and autocracy. The authors of the PR data set use a dichotomous coding, where states are either a democracy or an autocracy. I observe the PR democracy score for 1,239 sender states, 1,323 target states and 1,165 sender-target dyads in the sample.

With respect to the relation between the PR and Polity IV data, I observe the following. The (dichotomised) democracy score of the sender state based on the Polity IV data set (a state is a democracy for a score equal to or higher than 7) is strongly correlated with the corresponding score in the PR data set (*r=0.92*). The (dichotomised) democracy score of the target state resulting from the Polity IV and the PR data set is less, yet still strongly, correlated (*r=0.86*). The dichotomous democracy score of the sender-target dyad based the Polity IV and the PR data set are also strongly correlated (*r=0.87*). While the PR data set also provides information on democratic transition or breakdown, too few of the observations in the TIES data set are states in transition (< 25), so the information cannot be used in the econometric analysis.

Correlates of War Trade Data The Correlates of War (COW) Trade Data set (Barbieri and Keshk, 2016) allows us to combine data on economic sanctions with trade data in order to assess the role of trade dependency on economic peace.[10] The COW Trade Data offers information on trade flows for the years 1870 to 2014, both bilateral and total trade figures. Given the scarcity of bilateral trade data in this study, and substantial scope for bias in this type of data (Linsi and Mügge, 2019), I use the total trade figures. This limits the ability to assess the trade dependency between the sender and the target, but allows study of the general dependency on trade and openness toward global markets (Gartzke, 2007). I observe total trade (in current USD) for the sender of the economic

[7] Available at: http://www.systemicpeace.org/inscrdata.html.

[8] If an economic sanction is multilateral, I use the democracy score of the primary sender of the sanction, as identified by the TIES data coders.

[9] Available at: https://journals.sagepub.com/doi/suppl/10.1177/0010414012463905.

[10] Available at: http://correlatesofwar.org.

sanctions in 1,238 cases.[11] However, taking into account public threats of economic sanctions and information on the democracy level of the sender and the target reduces the sample to 780 cases.

3.2 Variables

Misspecifications in previous research To start, scholars (Hafner-Burton and Montgomery, 2008; Cox and Drury, 2006; Lektzian and Souva, 2003; Wallace, 2013) use the Polity IV data to identify the level of democratic institutions present in a particular sender or target state in a particular year. Polity IV offers the *DEMOC* variable that ranges from 0 to 10, where 0 means complete lack of democratic institutions. Polity IV also offers a negative part of the scale, *AUTOC*, that informs us how authoritarian the regime is and ranges from 0 to -10. While scholars of economic peace use the combined score *POLITY* (ranging from -10 to 10) for the estimations, if a state scores 7 or more on the democracy (i.e. *DEMOC* or *POLITY*) score, they transform the dependent variable into a dummy that is equal to 1 (meaning democracy) in the analysis.

The approach to data transformation discussed above leads to two concerns. First, given that the studies of economic peace focus on how the degree of democracy affects interaction between states, the negative part of the variation, present in *AUTOC*, may lead to unnecessary bias. If a country scores zero on the democracy score (*DEMOC*), then neither the structural nor the normative mechanism for democratic peace can be realised. Rather than being interested in how variation in democracy and authoritarianism affect economic peace, we want to assess how an increasing presence of democratic institutions affects the behaviour of states in relation to economic sanctions. Consequently, we are only interested in the part of the variation offered by the *DEMOC* score of the Polity IV data set.

The second concern is that the transformation of a continuous variable into dummies brings risks. Dichotomising observations reduces the prospects of finding statistically significant relations between variables because it has the same effect as removing up to a third of the data (MacCallum et al., 2002). This is particularly relevant here because research on democratic peace already grapples with a small sample problem (Hafner-Burton and Montgomery, 2008). Besides, the attribution of a particular numerical score to a level of democracy in a country-year observation risks being the coder's arbitrary choice. Following the continuous character of the data could help hedge against a potential bias resulting from coding differences. Another problem related to dichotomising continuous variables is that it increases the risk of type I error (Austin and Brunner, 2004), which is highly relevant for research on economic peace, because recent scholarship primarily focuses on indicating a type I error in the literature: correcting for the wrongly assigned effect of democracy on interstate economic coercion (Hafner-Burton and Montgomery, 2008; Wallace, 2013).

The second concern appears most pressing because the frequent change in the significance level of

[11] In case of a multilateral sanction, I report the total trade figure for the primary sender — as reported by the TIES data set authors.

the key independent variables in research on economic peace may be partially due to dichotomisation. Consequently, in this study, to account for potential bias, I use the continuous score on the *DEMOC* variable from the Polity IV data set. Still, in the results section of this article, I offer a robustness check with dichotomised variables that identify a country as a democracy for a score of 7 or higher on the *DEMOC* variable — an approach replicating past research on economic peace. The results from this robustness check do not yield different findings compared to the main results based on a continuous democracy score.[12]

Dependent variable *Imposition* is a binary variable that allows us to observe whether the sender decides to move from the threat level to actual imposition of economic sanctions. The variable is generated from the TIES data set.

Independent variables *Democracy score sender* and *Democracy score target* are variables that, based on the Polity IV data set, identify the level of democracy of the sender state and of the target state, respectively.[13] I use these two variables to study whether democracies are more or less likely to issue and receive sanctions, and to generate the interaction effect necessary for testing the economic peace hypotheses. The two scores vary from 0 to 10, where 10 is a full democracy and 0 an autocracy. The *Dyad Democracy* variable is an interaction (product) between the democracy levels of the sender and the target. The higher the value of the variable, the more democratic the sender-target pair, up to a maximum possible score of 100.[14]

Interaction effect An additional concern related to research design choices made by authors on the economic peace (Wallace, 2013; Hafner-Burton and Montgomery, 2008; Cox and Drury, 2006; Lektzian and Souva, 2003) is the use of the interaction term. In order to assess whether joint democracy decreases the prospects of economic sanction incidents, they multiplied the (dummy) democracy score of the sender with the (dummy) democracy score of the target. While this is a plausible approach, they have also interpreted the main effects in the regression models as if the interaction term were not present. However, once variables are interacted, the main effects cannot be interpreted as unconditional (in an additive manner), with some exceptions (Afshartous and Preston, 2011), and ought to be ignored (Brambor et al., 2006). Consequently, because the interaction term is present in all cases, the results on the propensity of democracies to issue, or to be a target of, economic sanctions cannot be correctly interpreted from the models presented in the literature on

[12]Scholars of democratic peace have already called for the use of a continuous democracy variable so that the findings are not merely an artefact of data separation (Bennett, 2006).

[13]In order to allow for easier cross-study comparison and more meaningful interpretation of the results, I standardise the democracy score of the sender and the target state; what results in the partial effects being reported at the sample mean, rather than at 0 (Afshartous and Preston, 2011). In the summary table and the regression models, I refer to the variables with an (std) prefix to indicate the standardisation. I standardise the variables to a standard deviation of 1 and a mean of 0. This operation does not have any effect on the significance level or the sign in the regression results.

[14]I treat the *Democracy score sender* and *Democracy score target* variables, based on the *DEMOC* score, as continuous; thus, an interaction term here is a product of the two variables (Angrist and Pischke, 2009). In fact, *DEMOC* is a discrete variable that summarises the number of democratic institutions. However, if we assume that the discrete variable behaves monotonically (i.e. quality of democracy increases with each additional institution), we can use a continuous approximation. A violation of this assumption would appear in the distribution of the standard errors, but we do not observe it int the data (Figure A.1). The continuous character of the *DEMOC* score us also a common assumption in the literature (Jeong and Peksen, 2019; Bapat and Kwon, 2015; Bermeo, 2016).

economic peace.[15]

The difficulties with interaction effects are also evident in research design when Hafner-Burton and Montgomery (2008) and Wallace (2013) test for the role of the US on democratic peace. Since this is a three-way interaction term consisting of the democracy score of the sender, target and a dummy variable for the US, not all interactions are present in the regression model. That is likely to substantially bias the results (Brambor et al., 2006) and makes interpreting the findings on the role of the US with respect to economic peace difficult.

In addition, the strategy in Wallace (2013) to divide the sample into subsamples in order to assess the role of issue salience could be solved through a three-way interaction; although, as argued in the previous paragraph, all the interaction terms need to be specified in the regression. In this study, I choose to provide a three-way interaction in the regression for issue salience so as to be able to compare my findings with those of Wallace, while reducing potential bias.

Control variables For control variables, I refer to the findings on the effectiveness of economic sanctions and sanction threats and the indicators associated in the scholarship with probability of interstate conflict. I account for the trade dependence and market openness (Gartzke, 2007) of the sender of economic sanctions by controlling for the (natural logarithm of) total exports of the sender state, based on the COW Trade Data. I expect that part of the variation in the decision of states to engage in economic coercion is determined by the strength of the trade ties between the sender and the target of economic coercion. I also control for the reputation effect (Peterson, 2013) by accounting for the commitment of the sender in past sanction episodes, based on the sender's commitment indicator in the TIES data set. Threats of sanctions from senders that have a poor record of commitment to past imposed sanctions may be treated differently by targets, as the eventual cost of conflict may be negligible. I also control for the objective of the sanction, following the specification offered by the TIES data set. I introduce the *Trade* variable, which separates economic sanctions with a trade and economic liberalisation objective from other sanctions (Morgan et al., 2014). Following Wallace (2013), I control for security objectives, and offer a control variable that separates economic measures with a security objective from other sanctions.[16] This follows from the expectation that part of the trend in the sample can be explained by the issue type of the sanction regime. Next, I control for whether the sanction is multilateral (Bapat and Morgan, 2009), based on the information on sanction senders from the TIES data set. A higher number of senders is likely to systematically affect the decision to engage in economic coercion. I also control for the role of the US (Wallace, 2013; Hafner-Burton and Montgomery, 2008; Haas, 1997) with a

[15]Thus, in a model $y = b1x + b2z + b3xz$ there is no unconditional effect of z on y, because of the presence of the interaction effect. This becomes clear when we take the first order derivative of y with respect to x: $dy/dx = b1 + z$; the effect of x on y is conditional on the value of z. Mere interpretation of the $b1$ coefficient becomes insufficient.

[16]I identify the following categories from the TIES data set as security-related: "Contain Political Influence"; "Contain Military Behavior"; "Destabilize Regime"; "Release Citizens, Property, or Material"; "Solve Territorial Dispute"; "Deny Strategic Materials"; "Retaliate for Alliance or Alignment Choice"; "End Weapons/Materials Proliferation" and "Terminate Support of Non-State Actors".

dichotomous variable that takes a value of one if the US participated in the sanction regime as a sender, based on the TIES data set. This responds to the suggestion that US involvement drives findings with respect to economic coercion. By introducing a squared term of the dyadic democracy score, I also test whether the dyadic relation between the sender's and the target's democracy level and sanction imposition is non-linear. This is because scholars find that similar regime types — both democracies and autocracies — are less likely to engage in conflict, suggesting that there is not only a democratic peace but also an authoritarian one (Bennett, 2006). Finally, I offer a robustness test of the results with the Political Regimes (Boix et al., 2013) data set, which I use to obtain an alternative to the Polity IV measure of democracy of the sender and the target and the dyadic sender-target democracy score.

Role of success Part of the variation in the sample can be explained by success of threats: senders do not follow up with an imposition of economic measures because the policy demand has been met at the threat stage. In fact, the crisis bargaining literature suggests that those economic sanctions most likely to succeed should end at the threat stage (Drezner, 2003), that democracies ought to be more likely to succeed at the threat stage (Schultz, 1999), and that threats are more successful for economically interdependent states (Whang et al., 2013). Thus, based on the crisis bargaining literature, democracies should be less likely to impose sanctions, a result partially arising from the success rate at the threat stage. In addition, this mechanism should also apply to democratic dyads, resulting in what scholars identify as economic peace, the propensity of democracies not to issue economic sanctions against one another.

The objective of this study is to assess whether democracies exhibit different behaviour with respect to sanction imposition, in general and against one another. Hence, cases that succeeded at the threat stage may appear beyond the scope of this study, as the sender has no reasons to impose the sanction. Even so, the crisis bargaining literature suggests that it is precisely the high effectiveness of democracies at the threat stage that may drive economic peace, offering an alternative theoretical underpinning for this empirical phenomenon. Consequently, removing the successful cases of economic coercion from the sample could lead to biased results, as we could overlook a potential powerful driver of economic peace, namely, success at the threat stage. I therefore do not remove successful threats from the sample.[17]

3.3 Econometric model

Difficulties with the rare-event logit Scholars of economic peace (Hafner-Burton and Montgomery, 2008; Cox and Drury, 2006; Lektzian and Souva, 2003) employ a rare event logit model (King and Zeng, 2001) in their empirical analyses.[18] However, this approach raises a number of

[17] I conducted a test on a censored sample, removing cases of successful threats, and the results are consistent with the findings reported in this article.

[18] Note that Lektzian and Souva (2003) do not report the model type used for the analysis in their article, but given the sample size and the distribution of economic sanctions they probably do use a rare event logit.

concerns. While the rare event logit is useful when studying events that occur with relatively low frequency, its estimator suffers from a bias if the event occurs rarely in *absolute* terms (Leitgob, 2013). For example, the rare event model provides efficient estimates if an event occurs once in a thousand times and the rare event was observed happening a hundred times. However, as the number of observations decreases in the less frequent category, so, does the efficiency of the estimator. This quality of the rare-event model raises concern about its applicability to research on economic peace. In the less frequent category (democracies that sanction one another), researchers observe only five cases of economic sanctions so, despite the use of a rare-event logit model in the analysis, the predictions on economic peace presented by Hafner-Burton and Montgomery (2008), Lektzian and Souva (2003) and Cox and Drury (2006) may suffer from bias.

Furthermore, the research design correction of Cox's article proposed by Hafner-Burton and Montgomery (2008) may also suffer from a research design problem. The solution that Hafner-Burton offers for Cox and Drury (2006) is to increase the number of non-events (i.e. cases of no economic sanctions), adding more country-pair years. Consequently, authors only increase the number of observations in the more frequent category (i.e. no sanctions). However, this does not solve the fundamental problem for the source of bias in the rare event logit; namely, that there are only a few observations in the less frequent category. In addition, the model proposed by King and Zeng (2001) has the propensity to overcorrect bias as the sample size decreases (Leitgob, 2013). Consequently, the change in the significance of the variables in the regression after the correction offered by Hafner-Burton to Cox's work may result from the rare-event model specification, rather than actual data improvement.

Wallace (2013), in an attempt to address previous research design misspecifications, bases his analysis on the TIES data set, which has more observations than the HSE data set used in previous research on economic peace. However, that study conducts the analysis only on the 585 cases of implemented sanctions available in the TIES data set, removing the 303 cases of sanction threats from the study. It does not report how many cases of sanctions involving two democracies are in the full and the restricted samples reported in the article. This makes it difficult to assess whether it also suffers from too few observations in the less frequent group (i.e. two democracies sanctioning one another), and what type of information is foregone by censoring the sample to imposed sanctions only. Besides, Wallace shows that when the data is separated into two subsets (i.e. security and non-security economic sanctions), the coefficients for democratic peace are only significant for the former. However, this result could be driven by the bias resulting from decreasing the number of the less frequent category and an uneven split between the two categories. Given that the number of observations of two democracies issuing sanctions against one another has been low in previous studies, a narrow difference between the two samples may have led to Wallace's different results.[19]

[19]For example, we have a sample with an event that occurred 5 times and 100 non-event observations. The event can be split into two categories, let us say category (a) with 3 events and category (b) with 2 events. Wallace (2013)

Potential remedies The updated TIES data set offers a potential remedy to the problems associated with the rare event logit. Its data provides information about the use of threats of economic sanctions and their actual imposition.[20] Due to the temporal relation between the two, as threats come before imposition, the observed threats-only can be used as a counterfactual observation in this sample. Since, in the TIES data set, the coders could observe a public threat for 75 per cent of the cases (1,053 of 1,412), the dependent variable becomes imposition of sanctions (escalation from a threat to an economic sanction), and I can conduct the analysis with a logistic regression. Besides, the argument that threats-only offer a counterfactual for imposed sanctions has been acknowledged in the literature on economic sanctions (Drezner, 2003; Smith, 1995; Eaton and Engers, 1999; Lacy and Niou, 2004) and war (Schultz, 1999), and is present in the recent empirical work too (Schmid et al., 2021; Walentek et al., 2021). The use of threats as counterfactuals is also relevant from the perspective of the mechanism potentially underlying economic peace: the higher propensity of success experienced by democracies at the threat stage in an interstate conflict, as suggested in the crisis bargaining literature (Schultz, 1999; Whang et al., 2013; Walentek et al., 2021).

The distribution of the observations in the TIES data set on imposition, success and democracy of the sender and the target of economic sanctions has the following structure. In the part of the sample where threats are made public and are observed by the coders, threats succeed in 48% of the cases, and imposed sanctions succeed in 38%. Unsuccessful threats are followed by imposition in 62% of the cases.[21] In 287 cases, the threat of economic sanctions is followed by an imposition where both the sender and the target were democracies.[22] This is a substantial increase compared to previous research on economic peace, where there were only five cases of two democracies sanctioning one other (Cox and Drury, 2006; Hafner-Burton and Montgomery, 2008; Lektzian and Souva, 2003). Finally, in the TIES data set there are 486 cases of threats followed with a sanction and 567 cases of threats-only. In 617 cases, the conflict (i.e. either a threat-only or imposition of economic coercion) involved a democratic dyad, and in 436 cases at least one party in the conflict was not a democracy. There are also 117 cases of a democracy following up on a sanction threat to a non-democracy, and 50 cases of a non-democracy pursuing a sanction threat against a democracy.

The frequency of the observations offered by the TIES data set and the presence of the threat-only counterfactual to an imposed sanction make it possible to use a logistic regression, with *imposition* as a dependent variable in place of the rare-event logit. Neither the absolute frequency of democratic dyads sanctioning one another (287 cases), nor its relation to the complete sample (1,053 if threats

proposes to conduct two separate regressions, one for category (a), where we have 3 events and 100 non-events (because the non-events cannot be categorised), and then another regression with 2 events and 100 of the same non-events. Given the sensitivity of the rare-event logit, it is possible that the former regression will show significant coefficients but not the latter. However, this is an artefact of the model.

[20] Wallace (2013) already indicated that the presence of sanction threats in the TIES data set offers opportunities for further research and improved research design.

[21] We can also assume that threats were also issued in the 359 cases where coders could not find a public record. In the Appendix (Table A.4), I provide a study with the non-threat events coded as failed threats. The results from this analysis are consistent with the findings presented in the main body of the article.

[22] If we assume that countries that score seven or more on the Polity IV *DEMOC* score are democracies, so following the common approach in research on economic peace (Wallace, 2013).

were made public, 1,412 otherwise) motivate the use of a rare-event logit for the empirical analysis. However, there are two concerns related to the logistic regression and the research design advocated in this article; selection bias and inconsistency with past research. I address both in the next subsections.

Selection bias The study of threats and imposition suffers from a potential bias, a selection problem. This misspecification occurs when the observations in the sample are non-randomly selected (Vance and Ritter, 2014). In this data, I only observe cases of threats that either (i) escalate to an imposed sanction or (ii) where the sender settles for the status quo and does not follow up on the threat. However, I do not observe (iii) instance when there was a conflict, but the sender has not issued a threat. Hence, the outcome variable (to impose a sanction or issue a threat only) is subject to a type of a selection process — issuing of a threat of economic sanctions — so may produce a non-random sample. As a result, since the error term may be correlated with the independent variables, there is a risk that the estimator is biased (Vance and Ritter, 2014; Brandt and Schneider, 2007).

Nevertheless, there are two arguments against the use of a selection model in this study and in research on economic coercion more broadly. First, the variable of interest may be (i) whether a threat of economic coercion is pursued with a sanction policy or not, rather than (ii) whether a conflict escalates to a threat of economic coercion or not. A selection problem is present only in the latter case. Studies that employ a selection model cannot observe a *clear-cut* counterfactual (e.g. research on foreign aid) and must design strategies to address this issue (Vance and Ritter, 2014). The TIES data set, unlike the HSE data on economic sanctions, allows researchers to avoid this problem through the inclusion of the threat stage and a clear specification of the outcome variable. This approach is employed in the most recent literature on economic sanctions (Walentek et al., 2021; Schmid et al., 2021) and in line with the earlier calls for inclusion of threats and the use of threats-only as a counterfactual observation (Lacy and Niou, 2004; Drezner, 2003; Eaton and Engers, 1999; Smith, 1995; Whang et al., 2013).

A second argument against the use of a selection model relates to assumptions about the data subject to censoring in selection models. The most common empirical strategy to address the selection problem is the use of a Heckman model in statistical software (Vance and Ritter, 2014). However, this model treats censored data as missing, in this case implying the presence of a latent threat that is not observable to the researcher (e.g. a threat of economic sanction communicated via diplomatic channels and not announced publicly or leaked). Consequently, the use of a Heckman model does not address the problem of non-random selection in the TIES data set, because the underlying assumption (i.e. censored observations are latent threats) is not fully consistent with the expectations about the empirical world. While some of the potential non-events (non-sanction country-pair per year) are certainly latent threats, many (if not most) country dyads are either not

in conflict or are involved in another type of conflict, for example a public diplomatic row.

Consistency Another reason to question the use of threats-only as counterfactuals is the limited consistency between this article and previous research on economic peace. The empirical strategy of previous scholars (Cox and Drury, 2006; Hafner-Burton and Montgomery, 2008; Wallace, 2013; Lektzian and Souva, 2003) was to test whether democracies are more likely to send, receive or target one another with economic coercion if part of a "potential economic conflict" dyad. The empirical strategy in this article is to assess whether democracies send, receive or target one another with economic coercion if there is a prospect of conflict and the use of sanctions (i.e. in the case when a threat of economic sanctions has been issued). This research could be argued to be a special case of the broader work on economic peace and part of a common effort to establish whether it exists. However, this reasoning may be misleading for two reasons.

The first reason is that research on economic peace treated all non-sanction years for country-pairs identified as having potential for economic coercion as counterfactual. While this was treated as sufficient to allow for broad claims about whether there is economic peace in general, the consequences of unclear specification of "potential conflict" were overlooked. The combining of conflict dyads with non-conflict dyads and the treatment of both as equally counterfactual to onset of economic sanctions is an empirical choice that results in a confounding effect of the true counterfactual (i.e. sender settling for the status quo in face of actual conflict) in cases where no conflict was present. This leads to an inefficient regression model, where, due to the confounding effect of the relevant and the not relevant non-events, the results of the statistical analysis are difficult to interpret. This is particularly important in this research because the mere significance and sign of the explanatory variables are of key interest.

Second, other types of conflict, diplomatic or military, are overlooked, as if assuming that all non-sanction year dyads are also non-conflict dyads. This results in an aggravation of the confounding effect and further undermines the validity of the empirical analysis conducted on economic democratic peace. There might have been cases where there was the prospect of an actual conflict and the use of economic sanctions but where the issue was eventually resolved through military or diplomatic means (Pape, 1997).

In this article, I am faced with a trade-off between consistency with previous research and improvements in research design. I have decided to prioritise the latter, because a misspecified empirical strategy would neither inform the reader about the accuracy of the past research on economic peace in the light of the new data (i.e. TIES), nor, given the problems in the empirical strategy associated with past studies, would it provide a scientific contribution on its own. In addition, given that researchers of economic peace were, in fact, concerned with "potential economic conflict" dyads, as they specify in their research design, rather than all possible dyads, part of this trade-off is, in fact, only apparent and this article is consistent with both the theory and declared

research design in past studies of economic peace.

4 Results

4.1 Economic peace and uniqueness of democracies

Table 1 presents the results of the logistic regression of the continuous and standardised variable of the democracy score (*DEMOC* based on Polity IV data set) of the sender and the target of economic sanctions (Model (1)) and an interaction term between the two (Model (2)). I also test for potential non-linear relation between the democracy of sender-target dyad and probability of sanctions imposition (Model (3)), and for the role of the US (Model 4) and the impact of security as the issue motivating sanctions imposition (Model (5)). In Model (4) and Model (5), I use a three-way interaction model.

In Model (1), I show that the level of democracy is positively and significantly related to the prospects of sanction imposition (*OR=1.452*, *p=0.01*). This supports the public choice theoretical framework for economic sanction of Kaempfer and Lowenberg (1988), who argue that democratic leaders serve broader domestic constituencies and, consequently, are more likely to engage in economic coercion. The article finds no evidence for democracies being less likely to receive sanctions, as is also argued by scholars of economic peace, based on the rally-round-the-flag effect found in literature on democratic peace (Mueller, 1970), which suggests that democracies are more resilient and, consequently, less appealing targets of coercion. Hence, I accept H2, that democracies are more likely to issue economic sanctions, and I reject H3, that democracies are less likely to be a target of economic sanctions.

Results from Model (2), where the interaction term between democracy of the sender and the target state is introduced, suggest that there is no economic peace between democracies. In fact, the results point in the opposite direction, with democracies appearing to be more likely to sanction one another (*OR=1.248*, *p=0.1*). This dynamic is depicted in Figure 1, where I plot the predicted probability of sanction imposition and dyad democracy score. I therefore reject H1, that democracies are less likely to sanction one another. Moreover, given that I reject H1 and H3, but accept H2, I do not find evidence for either the structural or the normative economic peace argument, but I do find evidence for the public choice approach to the behaviour of democracies with respect to economic sanctions. Democracies are more likely to issue economic sanctions, regardless of the target, and there is evidence that they are actually more likely to sanction one another, what indicates that democratic leaders substitute war for sanctions when engaging in a conflict with another democracy.

Model (3) incorporates the squared dyad democracy score in order to identify a potential non-linear relation in the data. Although the squared term is not statistically significant, this does not exclude a potential non-linear relation. I conduct a joint significance test to assess whether both

the linear and quadratic dyad democracy score coefficients are zero and find evidence for a potential non-linear relation (*p=0.1*). I investigate this relation further and assess the location of the vertex of the function and estimate that it is located at the edge of the distribution (value of 99.2, where the maximum is 100). This suggests a semi-concave relation between probability of sanction imposition and dyad democracy score. Thus, I do not find evidence for an autocratic peace. This relation is graphically depicted in Figure 1.[23]

Model (4) provides no evidence for the proposition that the observable aggregated behaviour of democracies with respect to economic sanctions is driven by the policy of the US, as suggested by Hafner-Burton and Montgomery (2008). The three-way interaction term *US*(std) Demo Sender*(std) Demo Target* is not statistically significant. I therefore reject H4, that the US is the only democracy less likely to issue economic sanctions against other democracies. I find no evidence for particular behaviour of the US in relation to other democracies. Furthermore, in Model (5), I do not find evidence to support the proposition in Wallace (2013), that economic peace is subject to economic sanctions over security issues. The three-way interaction term *Security*(std) Demo Sender*(std) Demo Target* is not significant. Consequently, I can reject H5, that economic peace is driven by security issues.[24]

Finally, in order to study the detailed structure of the relation between democracy level of the sender and the target state and the probability of sanction imposition, I offer a contour plot in Figure 2. This visualisation of the results in Table 1 (Model (3)) allows disaggregation of the dyadic democracy score and taking greater advantage of the continuous variables used in the analysis. In Figure 2, I observe that states with a democracy score of 7 or higher show a large variation in their behaviour towards imposition of economic sanctions, i.e. they are not likely to impose sanctions against states with a low democracy scores, and increasingly likely to impose sanctions as the democracy score of the target increases. In contrast, senders with a democracy score of 3 or lower are largely indifferent in their sanctioning behaviour to the democracy level of the target state. This result suggests that a small number of democratic institutions have no constraining effect on leaders. While senders with a democracy score between 3 and 7 are more likely to impose sanctions against more democratic targets, the dynamic is not as strong as for states with a democracy score of 7 or higher. These findings are consistent with the traditional cut-off point for a state to be considered a democracy — a score above 6 (Jeong and Peksen, 2019) or above 7 (Wallace, 2013) on the Polity IV scale. These predictions are not present in the scholarship to date.[25]

[23] Figure A.1 in the Appendix provides a sensitivity analysis of the results from Table 1, Model (3).

[24] The Appendix (Table A.5) provides a three-way test with the *Trade* variable, following Morgan et al.'s (2014) suggestion that trade-related sanctions may follow a different dynamic to other sanctions, as they are often imposed "automatically" due to WTO rules or a Free Trade Agreement specification. However, I do not find evidence for this argument in the data.

[25] After removing cases of successful threats from the sample, the dynamics presented in Figure 2 are still consistent with the main results for senders with a democracy score equal to or higher than 7 and equal to or smaller than 3.

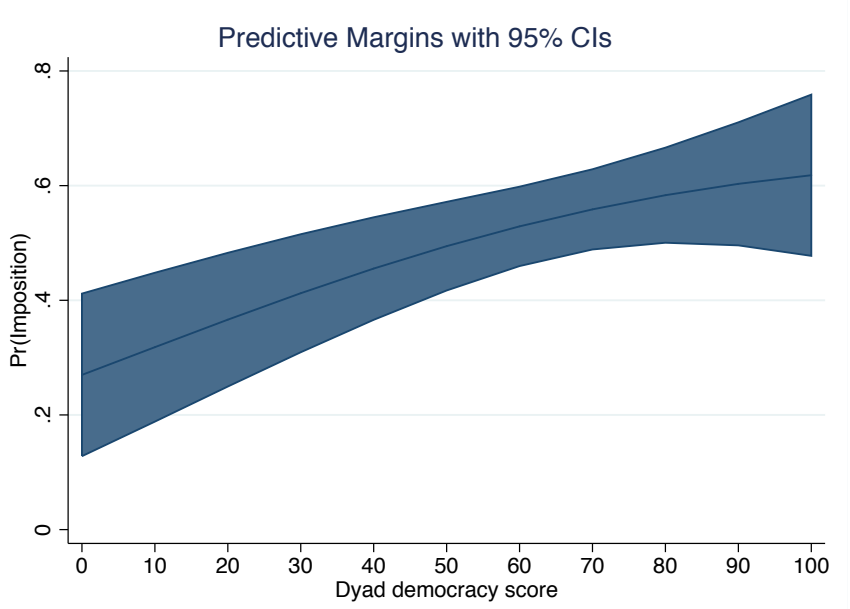

Figure 1: Impact of dyadic democracy score on predicted probability of sanction imposition.

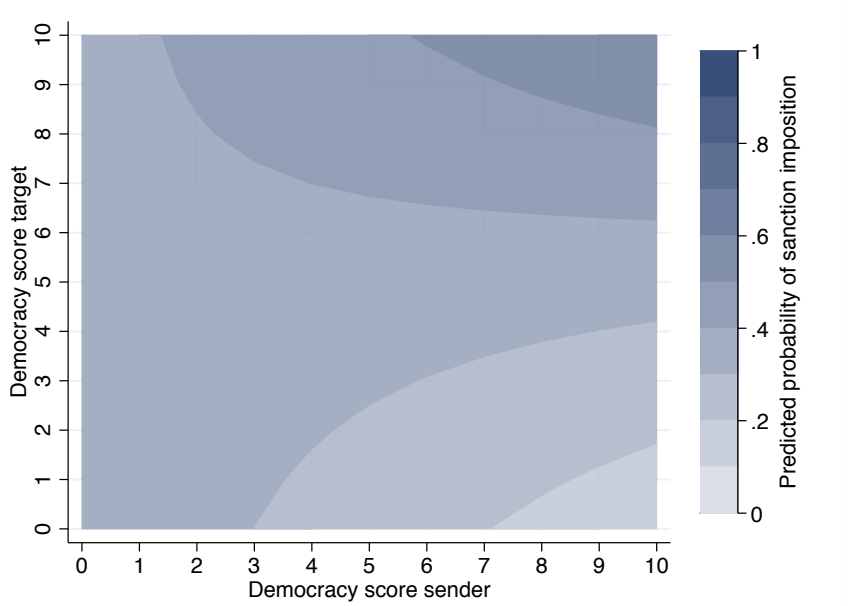

Figure 2: Contour plot of predicted probability of sanction imposition for democracy score of the sender and the target (DEMOC score).

The results are consistent with the public choice theoretical framework developed by Kaempfer and Lowenberg (1988) for the behaviour of democracies with respect to economic sanctions. This framework highlights that democratic leaders seek a broad support base in order to stay in power, and sanctions provide scope for addressing both foreign policy and domestic demands, resulting in a higher propensity among democracies to engage in economic coercion. I find no support for the economic peace theory: democracies do not appear less likely to sanction one another. In fact, I find evidence for an opposite dynamic. There is also no evidence for economic autocratic peace, that autocratic states are less likely to sanction one another.

These results are consistent with recent empirical research on economic sanctions, which shows that democracies are not more resilient targets of economic coercion relative to non-democracies (Bapat and Kwon, 2015). This contrasts with the fundamentals of economic peace theory. Scholars also suggest that economic coercion plays less of an instrumental role in the pursuit of foreign policy than a symbolic one (Whang, 2011; McLean and Whang, 2014), that democratic leaders experience a domestic audience benefit for imposing economic sanctions regardless of the policy outcome, the target state or the issue at stake. This overlaps closely with the "expressive" motivation for economic sanctions highlighted by Kaempfer and Lowenberg (1988).

Finally, the evidence that I present for a higher propensity among democracies to sanction and to sanction one another may indicate that democracies rechannel conflict among each other. As, due to normative and/or structural limitations on democratic leaders, war between democracies is rare (Bueno de Mesquita et al., 1999), sanctions may be a viable alternative for politicians to address the expectations of voters of an active foreign policy. And, as democratic leaders have to address a broader audience Kaempfer and Lowenberg (1988) and also obtain a domestic audience benefit for sanctioning (Whang, 2011), they may be increasingly tempted to engage in economic coercion. Lack of a similar dynamic among autocracies adds further weight to the argument. Consequently, this article suggests that the exercise of power may have been rechannelled to economic coercion as a result of democratisation and can offer a partial answer to the question of why do we see more economic sanctions since the end of the Cold War (Morgan et al., 2014).

It is still possible that democracies are generally less likely to experience (economic) conflict; and that they are more likely to issue a threat of sanctions and pursue it with an imposition only in the rare cases. However, given the rise in the frequency of economic sanctions (Morgan et al., 2014), it appears that the pacifying effect of rising democratisation (i.e. stemming from the shared norms and structural constraints) (Russett et al., 1998; Marshall et al., 2018) may not be sufficient to offset the drive of democracies to issue sanctions, which leads to a higher frequency of economic coercion in international relations — consistently with what we observe in TIES data. This suggests that democratisation does not reduce the propensity of conflict between states; it only changes the means of conflict in international relations.

Table 1: Democracy and economic sanctions — continuous score, all years. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1) Odds ratio	Model (2) Odds ratio	Model (3) Odds ratio	Model (4) Odds ratio	Model (5) Odds ratio
Imposition					
(Std) Demo Sender	**1.452*****	**1.349****	**1.665****	**1.340****	0.916
	(±0.195)	(±0.190)	(±0.364)	(±0.188)	(±0.345)
(Std) Demo Target	1.016	0.957	1.375	1.138	0.962
	(±0.0857)	(±0.0856)	(±0.407)	(±0.188)	(±0.237)
Dyad Democracy		**1.248***	**1.447****	**1.301****	1.755
		(±0.148)	(±0.239)	(±0.171)	(±0.860)
US*(Std) Demo Sender				0.747	
				(±0.581)	
US*(Std) Demo Target				0.586	
				(±0.220)	
US*(Std) Demo Sender*(Std) Demo Target				1.848	
				(±1.323)	
Security*(Std) Demo Sender					1.615
					(±0.615)
Security*(Std) Demo Target					0.729
					(±0.212)
Security*(Std) Demo Sender*(Std) Demo Target					0.595
					(±0.303)
US	0.698*	0.707	0.708	0.829	0.725
	(±0.151)	(±0.153)	(±0.153)	(±0.220)	(±0.160)
Security	1.419	1.390	1.421	1.391	0.957
	(±0.351)	(±0.345)	(0.353)	(±0.342)	(±0.284)
Past Commitment	0.961	0.971	0.965	0.971	0.960
	(±0.129)	(±0.130)	(±0.129)	(±0.131)	(±0.130)
Multilateral	**1.619****	**1.669****	**1.702****	**1.640****	**1.523***
	(±0.361)	(±0.375)	(±0.385)	(±0.371)	(±0.356)
(Ln) Total Exports Sender	0.994	0.986	0.984	0.994	0.963
	(±0.0551)	(±0.0557)	(±0.0558)	(±0.0566)	(±0.0552)
Trade	1.154	1.129	1.139	1.133	0.995
	(±0.240)	(±0.235)	(±0.236)	(±0.237)	(±0.211)
(Std) Dyad Democracy^2			0.640		
			(±0.222)		
Constant	1.065	1.365	1.440	1.063	3.192
	(±1.538)	(±2.006)	(±2.120)	(±1.574)	(±4.766)
Observations	715	715	715	715	715
Control variables	YES	YES	YES	YES	YES
Interaction term	NO	YES	YES	YES	YES
Three-way interaction	NO	NO	NO	YES	YES
Pseudo R2	0.0187	0.0222	0.0238	0.0250	0.0344
Log Lik	-485.4	-483.7	-482.9	-482.3	-477.7

4.2 Robustness of the results

I use two additional tests to assess the robustness of the findings. First, I replicate Wallace (2013), limiting the sample to the years 1971-2000. To start, I implement my methodological choices: I use the continuous variables instead of the dichotomous and treat the sanction threat as a counterfactual. In addition, I use a dichotomous specification of democracy, replicating past studies of economic peace. Following Wallace, I code countries as democracies if the Polity IV *DEMOC* score is equal to or higher than 7. Both the continuous and dichotomous results are consistent with the main findings: (i) democracies are more likely to issue economic sanctions; (ii) there is no evidence for democracies being less likely to receive economic sanctions; (iii) there is no evidence for economic peace; (iv) democracies appear more likely to sanction each other. What is more, the standard errors are larger in the study based on the dichotomised variables. This addresses the previous concerns about the consequences of dichotomising continuous variables. The regression results are presented in the Appendix in Table A.2.

Second, I provide an empirical test using the Political Regimes (PR) data set, which emphasises political competition and the role of suffrage in assessment of democracy more than the Polity IV

data set. The results from the robustness test with the PR data set are also consistent with the main results: democracies appear more likely to issue economic sanctions (Model (1)) and there is no economic peace; in fact, democracies seem more likely to sanction one another (Model (2)). Results are offered in the Appendix in Table A.3.

5 Conclusion

The main purpose of this article has been to provide insight into the behaviour of democracies with respect to economic sanctions. Drawing on an updated TIES data set on economic sanctions, I conclude that there is no economic peace between democracies, i.e. democracies are not less likely to sanction one another, even after accounting for a special role of the US or issue salience. This indicates that there is no direct relation between democratic peace and economic peace, which contrasts with past research (Hafner-Burton and Montgomery, 2008; Cox and Drury, 2006; Wallace, 2013; Lektzian and Souva, 2003). In fact, I find evidence that democracies are more likely to sanction one another, which may signal a rechannelling of the exercise of power from military intervention to economic coercion among democratic states. Besides, I find that, compared to non-democratic states, democracies are more likely to issue economic sanctions. This is in line with the public choice theoretical approach to economic sanctions (Kaempfer and Lowenberg, 1988), the democratic peace argument (Bueno de Mesquita et al., 1999) and recent empirical research on the effectiveness of economic sanctions (Whang, 2011; Walentek et al., 2021).

This research, apart from directly contributing to the literature on economic peace, also engages with the debate on the variation in the frequency of economic sanctions over time. Since the end of the Cold War, scholars have observed a major increase in the use of economic sanctions (Morgan et al., 2014), an increase contrary to the expectations of the academic community and coinciding with the post-Cold War wave of democratisation. The general expectation was that advances in the liberal economic and political order that accompany democratisation would make economic sanctions an obsolete tool in foreign policy (Hufbauer et al., 2007). However, as theory suggests (Kaempfer and Lowenberg, 1988), and empirical research shows (Whang, 2011), sanctions, being focused on addressing the domestic audience rather than on solving international conflict, may play an important symbolic role in democratic states. Thus, my findings suggest that the increase in the frequency of economic coercion may be a consequence of democratisation. Furthermore, the peace-building effect of democracy, as argued by democratic peace scholars (Bueno de Mesquita et al., 1999), may not be sufficient to offset the sanction-enhancing effect of democratisation. Combined with the propensity of democracies to not engage in war with one another, it may produce a rechannelling effect — economic sanctions for war — further contributing to the rise in the frequency of economic coercion and accounting for the key result of this article — that democracies are more likely to sanction

one another. This is a counter-intuitive outcome: advances of democracy might be expected to be a source of peaceful cooperation in international relations rather than of coercion (Keohane and Martin, 1995).

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Appendix

Table A.1: Summary statistics.

Variables	N	Mean	SD	Min	Max
Start Year	1,412	1986	15.63	1945	2005
Threat	1,412	0.746	0.436	0	1
Imposition	1,412	0.598	0.490	0	1
Success	1,412	0.408	0.492	0	1
US	1,412	0.521	0.500	0	1
Trade	1,412	0.517	0.500	0	1
Security	1,412	0.305	0.461	0	1
Multilateral	1,412	0.262	0.440	0	1
Past Commitment	1,250	2.342	0.601	1	3
(Ln) Total Exports Sender	1,238	25.02	2.269	16.59	27.43
Democracy Score Sender	1,221	8.376	3.316	0	10
Democracy Score Target	1,249	6.272	4.093	0	10
(Std) Demo Sender	1,221	0	1.000	-2.526	0.490
(Std) Demo Target	1,249	0	1.000	-1.532	0.911
Dummy Demo Sender	1,221	0.835	0.372	0	1
Dummy Demo Target	1,249	0.622	0.485	0	1
Political Regime Demo Score Sender	1,239	0.829	0.377	0	1
Political Regime Demo Score Target	1,323	0.639	0.480	0	1

Table A.2: Democracy and economic sanctions — replication of Wallace's study. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1) Odds ratio (Continuous)	Model (2) Odds ratio (Continuous)	Model (3) Odds ratio (Dummy)	Model (4) Odds ratio (Dummy)
Imposition				
(Std) Demo Sender	**1.505****	1.254		
	(±0.256)	(±0.266)		
(Std) Demo Target	1.104	0.930		
	(±0.112)	(±0.120)		
Dyad Democracy		**1.705*****		
		(±0.352)		
Dummy Demo Sender			**3.642*****	0.345
			(±1.685)	(±0.376)
Dummy Demo Target			1.191	**0.0779****
			(±0.244)	(±0.0873)
Dummy Demo Dyad				**17.61****
				(±20.02)
Past Commitment	**0.669****	**0.696***	**0.667****	**0.685****
	(±0.126)	(±0.132)	(±0.126)	(±0.129)
Multilateral	1.404	1.426	1.399	1.469
	(±0.377)	(±0.389)	(±0.374)	(±0.399)
(Ln) Total Exports Sender	0.926	0.900	0.934	0.915
	(±0.0846)	(±0.0872)	(±0.0797)	(±0.0791)
US	0.629*	**0.644***	**0.624***	**0.642***
	(0.160)	(±0.164)	(±0.157)	(±0.162)
Trade	1.112	1.065	1.110	1.055
	(±0.286)	(±0.275)	(±0.285)	(±0.272)
Security	1.280	1.263	1.298	1.243
	(±0.407)	(±0.407)	(±0.413)	(±0.400)
Constant	17.37	37.87	4.164	**66.89***
	(±41.47)	(±95.70)	(±8.739)	(±157.7)
Observations	522	522	522	522
Control variables	YES	YES	YES	YES
Interaction term	NO	YES	NO	YES
Years 1971-2000	YES	YES	YES	YES
Pseudo R2	0.0296	0.0430	0.0328	0.0437
Log Lik	-350.7	-345.9	-349.6	-345.6

Table A.3: Democracy and economic sanctions — Political Regimes data set. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

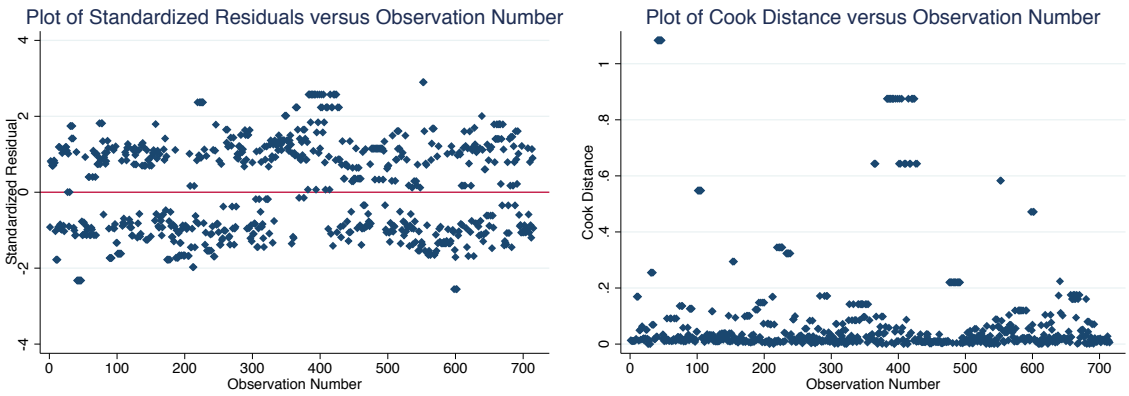
Variables	Model (1) Odds ratio	Model (2) Odds ratio
Imposition		
Democracy Score Sender	**2.772*****	0.909
	(±0.987)	(±0.627)
Democracy Score Target	0.959	**0.270***
	(±0.163)	(±0.189)
Dyad Democracy Score		**3.842***
		(±2.776)
Past Commitment	1.062	1.064
	(±0.136)	(±0.137)
Multilateral	**1.669****	**1.738****
	(±0.361)	(±0.380)
(Ln) Total Exports Sender	1.046	1.039
	(±0.0510)	(±0.0513)
US	0.709	0.723
	(±0.151)	(±0.154)
Trade	1.212	1.192
	(±0.239)	(±0.235)
Security	**1.708****	**1.670****
	(±0.410)	(±0.403)
Constant	**0.0918****	0.307
	(±0.108)	(±0.415)
Observations	762	762
Control variables	YES	YES
Interaction term	NO	YES
PR Data	YES	YES
Pseudo R2	0.0240	0.0272
Log Lik	-514.2	-512.5

Table A.4: TIES sample with absent threats coded as failed threats. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1) Odds ratio	Model (2) Odds ratio
Imposition		
(Std) Demo Sender	**1.302****	1.212
	(±0.138)	(±0.144)
(Std) Demo Target	0.975	0.934
	(±0.0750)	(±0.0761)
Dyad Democracy		**1.236****
		(±0.125)
Past Commitment	1.003	1.004
	(±0.118)	(±0.118)
Multilateral	1.212	1.244
	(±0.249)	(±0.256)
(Ln) Total Exports Sender	0.986	0.985
	(±0.0503)	(±0.0507)
US	**0.552*****	**0.557*****
	(±0.110)	(±0.111)
Trade	1.226	1.207
	(±0.235)	(±0.231)
Security	**1.540****	**1.545****
	(±0.339)	(±0.340)
Constant	2.407	2.508
	(±3.188)	(±3.343)
Observations	942	942
Control variables	YES	YES
Interaction term	NO	YES
Pseudo R2	0.0160	0.0200
Log Lik	-623.5	-621

Table A.5: TIES sample with a trade three-way interaction. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1) Odds ratio	Model (2) Odds ratio	Model (3) Odds ratio
Imposition			
(Std) Demo Sender	**1.452*****	**1.349****	**1.503*****
	(±0.195)	(±0.190)	(±0.229)
(Std) Demo Target	1.016	0.957	0.884
	(±0.0857)	(±0.0856)	(±0.101)
Trade*(Std) Demo Sender			0.606
			(±0.228)
Trade*(Std) Demo Target			1.151
			(±0.301)
(Std) Dyad Democracy		**1.248***	1.196
		(±0.148)	(±0.152)
Trade*(Std) Demo Sender*(Std) Demo Target			1.282
			(0.611)
Past Commitment	0.961	0.971	0.996
	(±0.129)	(±0.130)	(±0.134)
Multilateral	**1.619****	**1.669****	**1.632****
	(±0.361)	(±0.375)	(±0.371)
(Ln) Total Exports Sender	0.994	0.986	0.973
	(±0.0551)	(±0.0557)	(±0.0548)
US	**0.698***	0.707	0.716
	(±0.151)	(±0.153)	(±0.157)
Trade	1.154	1.129	0.511
	(±0.240)	(±0.235)	(±0.879)
Security	1.419	1.390	1.449
	(±0.351)	(±0.345)	(±0.363)
Constant	1.065	1.341	1.617
	(±1.538)	(±1.969)	(±2.381)
Observations	715	715	715
Control variables	YES	YES	YES
Interaction term	NO	YES	YES
Three-way interaction	NO	NO	YES
Pseudo R2	0.0187	0.0222	0.0281
Log Lik	-485.4	-483.7	-480.7



(a) (b)

Figure A.1: Diagnostics for the logistic regression of democracy dyad with continuous scores and all years: (a) standardised residuals and (b) the Cook Distance.