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The Hegemon's Purse: No Economic Peace Between Democracies*

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Cox & Drury broaden the democratic peace literature from the domain of militarized conflict to economic sanctions. Their analysis of economic sanctions data from 1978 through 2000 finds that democracies are more likely to enact sanctions but are less likely to do so against other democracies. In this article, their analysis is extended in three different ways: first, their methodology and sample size are improved; second, interactions between variables are examined; and, third, additional hypotheses are tested. This article finds that the substantive effects of joint democracy on the likelihood of sanctions disappear after accounting for the disproportionate role of the United States (and correcting for method); the United States has a significantly different pattern of implementing sanctions than other states; and the trade dependence of a potential sender plays a significant role in determining the likelihood of sanctions.

Introduction

Cox & Drury (2006), following Lektzian & Souva (2003), extend the democratic peace argument from the domain of militarized conflict to economic sanctions. Their claims are both reasonable and of significant consequence: democracies, they argue, sanction more often than other types of governments but are less apt to sanction each other; trade flows increase the likelihood of sanctions; and the United States imposes sanctions more often than any other government and frequently targets allies. In their analysis, the world of economic sanctions conforms to the laws of the democratic peace writ large.

We argue that economic sanctions, however, are not like armed conflict (Pape, 1997). While states and increasingly non-state actors of all kinds use armed force to pursue political interests with considerable consequence, economic sanctions are mainly available to a small subset of international actors – states with large market power relative to others (those states that feature a sizeable GDP per

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capita relative to potential targets, coupled with low trade dependence on those targets). This asymmetry in capacity to use sanctions is plainly evident: 60% of sanctions are initiated by the United States against other governments (Hufbauer et al., 1997).[1] And, as Cox & Drury and others rightly argue, the USA's privileged role as market hegemon accords it particular advantages in foreign policy and creates incentives for sanctioning behavior that other governments do not have (Marinov, 2003). This effect also extends, to a more limited extent, to other states with large relative market power, such as states in the European Union.

Our focus in this article is on the effects of US dominance in market power on economic sanctions. Overpowering US market hegemony brings with it a particular capacity to use sanctions against other states; high trade flows with most of the world, combined with a powerful domestic economy, mean that sanctions are readily available to US policymakers and often costly for other states, while remaining relatively manageable for the United States. Moreover, the US government's ideological commitment to democracy abroad, coupled with its willingness to use sanctions for the purposes of redressing human rights violations (mainly taking place inside autocratic states), creates the clear expectation that the United States will not only sanction often, but sanction more selectively than other states, targeting mostly non-democracies. Yet, if this hypothesis is correct – that the USA will sanction more often than other states and be more likely to sanction autocrats when it does – then economic sanctions may not follow the logic of a democratic peace at all, but instead reflect US hegemony and ideology. The rest of the world has neither the same market capacity to impose sanctions nor, as a result, the same foreign policy goals in using sanctions. Consequently, other liberal democracies may not be any more or less likely to sanction other democracies; the liberal economic peace may simply be an artifact of the hegemon's purse.

We aim to understand how relative disparities in market power shape economic sanctions more generally, and we test the robustness of regime type results by fully accounting for US dominance. Our purpose is not to criticize Cox & Drury's study specifically but to build upon their research in a constructive way, by exploring the role that these variables play in the initiation of economic sanctions. We accordingly extend Cox & Drury's analysis in three ways: first, by improving their methodology and sample size; second, by creating interactions between key variables to examine whether the USA is not only more likely to sanction other states but also chooses its targets differently; and, third, by testing additional hypotheses regarding trade dependence and shedding light on Cox & Drury's findings. After increasing the sample size, making methodological improvements, and accounting for the disproportionate role of the United States in initiating sanctions, we find that democratic senders are no more or less likely to target either autocratic or democratic states; the United States sanctions a different set of states (in particular, non-democracies and governments with less relative power); and trade dependence plays a significant role in determining the likelihood of sanctions.

The Democratic Peace of Economic Sanctions

Cox & Drury (2006) studied 115 instances of sanctions onset between a sender and target from 1978 and 2000. Democratic governments, they argue, by and large share common values, place constraints on their leaders and political institutions, and engage in high levels of commerce with one another, all of which discourage war. For these same reasons, democracies should also avoid sanctioning one

[1] Of the 115 sanctions episodes observed in the data, 69 were initiated by the United States.

another. Their analysis confirms previous findings by Lektzian & Souva (2003) that democracies frequently resort to economic sanctions but rarely sanction each other, although they frequently sanction non-democracies.

For the purpose of replication and knowledge accumulation, we follow Cox & Drury (2006) and Lektzian & Souva (2003) in the use of sanctions onset ($Sanctions_{ij}$) data coded by Elliott et al. (forthcoming). Onset occurs in the first year that a sanctions threat from official recorded sources occurs or a sanctioning event is recorded (e.g. 1991 is the first year if a threat or event is recorded at any point in 1991).[2] We employ these data because they are ubiquitous in the study of economic sanctions and are the basis for the research we build upon. We also acknowledge that they suffer considerable limitations. Sampled on the availability of media coverage, the data were non-randomly selected. Moreover, there is debate over their coding; the set includes instances of two other types of economic instruments that may be distinct from economic sanctions: commercial negotiations and economic warfare.[3]

Increasing Sample Size

We make several improvements to Cox & Drury's dataset and methodology in order to test the robustness of their hypotheses. Owing to missing values and intentional selection of a subset of dyads for analysis, they test their hypotheses on only about 150,000 directed-dyad-years from 1978 to 2000 out of a potential total of 670,000. By replacing the trade and GDP data in their dataset with the latest version of Gleditsch's data[4] and recalculating the relevant variables, we increase the total number of dyad-years in their base model from 150,000 to 374,198.

This still leaves almost 300,000 potential observations out of the analysis. Cox & Drury argue that only 'sanctions relevant dyads' – made up of pairs of states that trade – should be considered potential candidates for economic sanctions, drawing an analogy with the 'politically relevant dyads' sometimes used in studies of militarized conflict. There are two principal reasons why all dyads, rather than sanctions-relevant dyads, should be used as the basis for analysis. The first reason is that the assumption that only certain states have the ability to sanction each other is false. While it may be more difficult for certain states to sanction each other effectively when they do not trade, weak economic linkages do not prevent states from initiating sanctions for symbolic reasons. Evidence confirms this fact: ten instances of economic sanctions in the dataset occur between states that have zero or missing values for trade data in Gleditsch's dataset (see Table I). Excluding these cases, therefore, excludes nearly 10% of the total sanctions cases. The second reason is that cutting the sample to sanctions-relevant dyads amounts to non-random sampling on the population. Arguments about the effects of trade on sanctions can and should be evaluated, but by including appropriate control variables on the right-hand side of the equation, rather than by removing observations. Consequently, in our analysis, we include all dyad-years that are not missing GDP, trade, alliance, or regime-type data, further increasing the number of observations to 617,122.

[2] Hufbauer, Schott & Elliot (1990: 2,3) define economic sanctions 'to mean the deliberate, government-inspired withdrawal, or threat of withdrawal, of customary trade or financial relations'. They include sanctions only in support of explicit foreign policy goals, while excluding 'the normal realm of economic objectives sought in banking, commercial, and tax negotiations between sovereign states'.

[3] In particular, Pape (1997) identifies a number of potentially suspect episodes. However, these episodes all occur before the 1978 start date of Cox & Drury's dataset.

[4] Cox & Drury's analyses use Gleditsch's (2002) data; by updating the trade and GDP variables to the latest version (4.1, 20 August 2004 release), we decrease the occurrence of listwise deletion and increase their sample. It appears that directed dyads where the target state had a COW code less than the sender were missing trade data, leaving 15 of the 115 episodes out of the analysis; eight additional episodes were eliminated, owing to missing GDP data.

Table I. Non-'Sanctions-Relevant' Dyads with Sanctions Episodes

Sender	*Target*	*Year*
South Africa	Lesotho	1982
Saudi Arabia	Yemen	1990
United States	Yemen	1990
Azerbaijan	Armenia	1991
Russia	Turkmenistan	1991
Turkey	Armenia	1991
Russia	Estonia	1992
United States	Libya	1992
Russia	Ukraine	1993
Greece	Yugoslavia	1994

Improving Methodology

In addition to increasing the sample size, we modify Cox & Drury's method. We follow their choice of rare events logistic regression and directed dyads for analysis, while making four methodological improvements. First, we cluster estimation by dyad. By clustering, we assume that the observations are independent across different dyads but not within a dyad over time, and we adjust the standard errors accordingly. This is the appropriate assumption to make, given the nature of the data at hand, which are certainly dependent temporally within dyads. Second, we drop Cox & Drury's year variable from our analyses, because it is unnecessary, given corrections for temporal dependence; however, we follow their approach of including a correction for the time since last sanction as well as cubic splines. Third, and by convention, we lag all of the independent variables by one year in order to ensure causal integrity. Fourth, we correct an error in their model by including the democracy status of the target; since they include both an interaction term (their democratic dyad variable) and one of the lower-order terms (their democratic sender variable), excluding the other lower-order variable (democratic target), as they do, is equivalent to assuming that the value of this coefficient is zero, muddling interpretation.[5]

[5] On the interpretation of lower-order coefficients, see Braumoeller (2004).

An Analysis of Hegemony

Finally, we use their existing variables and additional trade and GDP data from Gleditsch's (2002) dataset to extend their analysis and test the robustness of the effects of democracy (which they measure as a dummy variable, using the Polity scale on democracy)[6] on economic sanctions in two ways. We contend that the observed effect – that democracies do not sanction other democracies – is in part an artifact of particular US foreign policy capacity, derived from market dominance, to implement the US agenda of spreading democracy and curbing human rights abuses, rather than a feature of the democratic peace more generally. We use interaction terms to test our argument; because the lower-order variables are binary, interpretation of dependence is straightforward (Braumoeller, 2004). First, we generate interaction variables between the USA as a sender and two of Cox & Drury's independent variables: democratic dyads[7] and relative economic power. With these interaction variables, we perform two tests: (1) whether the original finding, that democracies do not sanction one another, is in part an artifact of US behavior,

[6] Full details of all data coding are available in Cox & Drury (2006).

[7] Interacting the USA as sender with democratic dyads is the same as interacting the USA as sender with democratic targets, since the USA is a democracy for all years in the dataset.

as we hypothesize, and (2) whether relative economic power, measured in terms of relative GDP per capita, operates in the same way for the United States as it does for all senders. Next, and following Nooruddin (2002) and Lektzian & Souva (2003), we generate trade-dependence variables for the sender (measured as the total trade with the potential target, divided by the sender's GDP) to test more directly the liberal hypothesis that the more dependent a sender is, the less likely that sender is to enact sanctions, a separate test of market power. Both relative power and trade dependence are tested with their means set to zero, in order to generate lower-order coefficients for these variables that fall within the dataset (Braumoeller, 2004).

Analyses

We analyze the same base model and use the same procedures for estimation as Cox & Drury do, while making improvements to sample size and methodology. We first report exact replications of their four models, incorporating all of our methodological improvements, as well as the increase in sample size. These estimates are reported in Table II.

We then extend Cox & Drury's analysis in Table III; we base our models in this table on their second model, which is reproduced in the first column for easy reference. The remaining columns of Table III explore how US patterns of economic sanctions differ from those of other states and how market power shapes sanctioning behavior. The second column of Table III tests the hypothesis that the likelihood of sanctions among democratic dyads and among dyads with substantial disparities in GDP per capita depends on whether the USA is the sender; the third column additionally tests whether trade dependence shapes economic sanctions.

In Table IV, we demonstrate the substantive significance of our full model (Table III, Column 3). Base risk calculations were performed by setting all binary variables to their medians (zero); variables that correct for time dependence were set to 0; the remainder were set to their means. First differences were calculated by setting binary variables to 1 and setting continuous variables to one standard deviation above their means.

When including all of our methodological improvements and increasing the sample size, the effects are striking; we see three primary implications of the effects listed in Tables III and IV.[8] First, and in strong support of Cox & Drury's findings, democracies are still significantly more likely than other types of governments to use economic sanctions; so, too, is the USA specifically. Moreover, dyads with substantial trade flows and relative disparities in GDP per capita are still more likely to experience sanctions. These findings are robust. They suggest that something fundamentally different is driving sanctioning and military conflict behaviors, and that democracies accordingly experience different incentives or capacities to engage in both. Like Cox & Drury, we believe these findings are substantively important to the study of international relations and deserve further exploration.

Second, and contrary to Cox & Drury's (2006) specific findings and Lektzian & Souva's (2003) more general argument, the democratic peace does not extend to the use of economic sanctions. The effects of democracy ($DemDyad_{ij}$) disappear when both methodology and sample size are improved; moreover, the effects of democracy and relative GDP differ, depending on whether the USA is the sanctions sender. Although the USA is more likely than other governments to use economic sanctions, it is not indiscriminate in which states it targets: the USA is more likely to sanction non-democratic regimes than democratic ones. There are several possible explanations for this finding. Market hegemony gives the United States unique power to pursue its ideological agenda to promote

[8] We also ran the models in Tables II and III on 'sanctions relevant' dyads only; all substantive results were the same.

Table II. Replication of Cox & Drury Table I with Improved Methods and Sample Size, 1978–2000

Variable	*Exports*	*Exports, US*	*Imports*	*Imports, US*
$DemDyad_{ij-1}$	−2.47 *	−1.81	−2.29 +	−1.79
	(1.21)	(1.16)	(1.17)	(1.16)
Dem_{i-1}	3.68 ***	2.82 **	3.70 ***	3.02 **
	(1.05)	(1.01)	(1.02)	(1.02)
Dem_{j-1}	1.29	1.10	1.17	1.16
	(1.22)	(1.15)	(1.18)	(1.16)
$LogExp_{i-1}$	0.53 ***	0.34 ***	0.45 ***	0.25 ***
	(0.04)	(0.05)	(0.04)	(0.05)
$RelPow_{ij-1}$	6.61E-02 ***	3.82E-02 ***	6.41E-02 ***	3.42E-02 ***
	(6.31E-03)	(8.77E-03)	(7.63E-03)	(9.34E-03)
$Allies_{ij-1}$	0.83 **	7.15E-02	1.00 **	0.25
	(0.30)	(2.91E-01)	(0.29)	(0.28)
US_{i-1}		3.31 ***		3.53 ***
		(0.30)		(0.34)
$SancYears_{ij}$	−0.49 *	−0.30 +	−0.50 *	−0.30 +
	(0.19)	(0.16)	(0.19)	(0.17)
Spline1	−8.92E-03	−4.88E-03	−8.92E-03	−4.55E-03
	(7.40E-03)	(6.65E-03)	(7.53E-03)	(6.75E-03)
Spline2	1.90E-04	−1.88E-03	2.73E-05	−2.28E-03
	(6.53E-03)	(6.05E-03)	(6.65E-03)	(6.12E-03)
Spline3	5.83E-03 +	5.84E-03 +	6.03E-03 +	6.08E-03 +
	(3.38E-03)	(3.26E-03)	(3.44E-03)	(3.28E-03)
Constant	−11.54 ***	−11.33 ***	−11.25 ***	−11.25 ***
	(1.13)	(1.05)	(1.09)	(1.05)
N	617,122	617,122	617,122	617,122

The numbers in parentheses are Huber standard errors. $+p<.10$, $*p<.05$, $**p<.01$, $***p<.001$.

democracy and human rights through economic sanctions, while foreign policies that sanction autocrats such as those in Myanmar or the Sudan have substantial domestic political appeal. Whereas the United States must reach agreement between two political parties in Congress in order to impose sanctions, the other main sanctioners, states in the European Union, must overcome the typically greater ideological and political divisions within or between the Union's many member-states, making sanctions harder to impose. Moreover, the USA often uses economic sanctions as a precursor to or during military interventions, which are far more likely against autocratic states. But the effect is not general: it is this targeted sanctioning behavior by the world's major market hegemon that explains the observation that democracies generally do not sanction one another. In fact, democratic senders, in general, are no more or less likely to target democracies or non-democracies with economic sanctions; only the USA is more likely to target non-democratic regimes. Additional robustness tests with EU dummies indicated that, while EU members are more likely than other states to enact sanctions, the same interaction effects are insignificant. Moreover, while sanctions are generally more likely to take place among dyads with substantial disparities in GDP per capita, this tendency is reduced for the United States.

Third, while the exports of a sender do affect its propensity to enact sanctions, a state's overall dependency has an even greater effect.

Table III. Estimates of the Effects of US Interactions, Trade Dependence on Economic Sanctions, 1978–2000

Variable	*(1) Base*	*(2) US interactions*	*(3) Trade dependence*
$DemDyad_{ij-1}$	−1.81	−1.36	−1.24
	(1.16)	(1.21)	(1.20)
Dem_{i-1}	2.82 **	2.64*	2.35 *
	(1.01)	(1.03)	(1.03)
Dem_{j-1}	1.10	1.28	1.40
	(1.15)	(1.18)	(1.17)
$LogExp_{i-1}$	0.34 ***	0.33 ***	0.49 ***
	(0.05)	(0.05)	(0.07)
$RelPow_{ij-1}$	3.82E-02 ***	6.51E-02 ***	6.12E-02 ***
	(8.77E-03)	(7.61E-03)	(8.67E-03)
$Allies_{ij-1}$	7.15E-02	0.11	0.25
	(2.91E-01)	(0.28)	(0.27)
US_{i-1}	3.31 ***	3.86 ***	3.34 ***
	(0.30)	(0.39)	(0.43)
US_{i-1}*$DemDyad_{ij-1}$		−0.98 +	−1.28 *
		(0.52)	(0.50)
US_{i-1}*$RelPow_{ij-1}$		−3.51E-02 **	−2.78E-02 *
		(1.20E-02)	(1.29E-02)
$TraDep_{i-1}$			−251.06 *
			(126.16)
$SancYears_{ij}$	−0.30 +	−0.29 +	−0.26 +
	(0.16)	(0.16)	(0.16)
Spline1	−4.88E-03	−4.68E-03	−4.02E-03
	(6.65E-03)	(6.58E-03)	(6.52E-03)
Spline2	−1.88E-03	−2.00E-03	−2.41E-03
	(6.05E-03)	(6.00E-03)	(5.96E-03)
Spline3	5.84E-03 +	5.90E-03 +	5.99E-03 +
	(3.26E-03)	(3.24E-03)	(3.23E-03)
Constant	−11.33 ***	−11.48 ***	−11.95 ***
	(1.05)	(1.07)	(1.06)
N	617,122	617,122	617,122

The numbers in parentheses are Huber standard errors. $+p<.10$, $*p<.05$, $**p<.01$, $***p<.001$.

Table IV. Predicted Probabilities

Variable	*Value*	*Percent change in risk (replication)*
$DemDyad_{ij-1}$	Maximum value	−42.76%
Dem_{i-1}	Maximum value	888.40% *
$LogExp_{i-1}$	Mean + 1 SD	138.24% *
$RelPow_{ij-1}$	Mean + 1 SD	37.85% *
$Allies_{ij-1}$	Maximum value	22.56%
US_{i-1}	Maximum value	2,684.60% *
$TraDep_{i-1}$	Mean + 1 SD	−93.06% *

* Denotes that zero falls outside the 95% confidence interval.

While an increase in the logged exports of a sender by one standard deviation from the mean makes that state one-and-a-half times as likely to enact sanctions, an increase in dependency of a state by one standard deviation from the mean decreases the tendency of a state to enact sanctions by over nine times.

Discussion

Research on the democratic peace has become ubiquitous; mounting evidence supports the view that democracies do not go to war with other democracies (Russett & Oneal, 2001; Russett, Oneal & Davis, 1998). Some believe that this theory might also influence how and against whom states use economic sanctions. Evidence, however, largely suggests otherwise. When fully accounting for the predominance of the United States in sanctioning behavior, democracies in general are no more or less likely to target other democracies than non-democracies. Only the USA is less likely to target other democracies when implementing economic sanctions. Thus, the so-called democratic peace does not extend to economic sanctions generally; a US-selection bias for sanctioning non-democratic targets, however, is readily apparent. But other (mostly democratic) senders are not hesitating to sanction other democracies.

This discovery is consistent with recent findings that the second leg of the Kantian tripod (international governmental organizations, or IGOs) does not hold up well in the case of sanctions, either. While we have not explicitly considered the role of IGOs in this article, existing research shows that they have a mixed record. Scholars have argued that IGOs decrease militarized conflict, at least under certain circumstances.[9] However, other studies have shown that preferential trade agreements, which can prevent militarized disputes (Mansfield & Pevehouse, 2000; Powers, 2004) and repression of human rights (Hafner-Burton, 2005) and should be even more effective at preventing economic conflict, have no effect on the propensity of states to sanction each other (Hafner-Burton & Montgomery, 2008). These findings together suggest that the democratic peace hypothesis may not transfer so easily from war to sanctions.

However, the third part of the liberal peace (trade) is upheld in the case of economic sanctions. Although there is a modest increase in the likelihood of sanctions when trade increases, this is substantively small compared to the effects of trade dependence. The more dependent a state is on a potential target's trade to generate GDP, the less likely that state is to use sanctions. Conversely, when there is an asymmetry of market power between sender and target, sanctions become more likely.

These initial results call for further investigation into the causes of the clear differences between military actions and economic sanctions with respect to the Kantian triad, as well as the extension of these results to cover a longer time frame. They also raise the need to better analyze how market hegemony shapes the use of economic sanctions and warn against generalizing US behavior to the rest of the liberal world. Democracies may be using economic sanctions as a substitute for militarized conflict in some cases, or even as a prelude to war in other cases to satisfy public opinion. While IGOs may promote peace in some ways, they seem to be ineffective at eliminating all forms of conflict. However, trade itself does seem to be equally effective in suppressing both types of conflict. Ultimately, a combined model of economic sanctions and military conflict would be useful for exploring the complex relationship between regime type, interdependence, and different forms of interstate conflict.

[9] Recent work has found that only subsets of IGOs, or certain groups of states in the network created by IGOs, are less likely to conflict; see Boehmer, Gartzke & Nordstrom (2004); Hafner-Burton & Montgomery (2006); Pevehouse & Russett (2006).

Table V. Correlations

	Sanction	*DemDyad*$_{ij-1}$	*Dem*$_{i-1}$	*LogExp*$_{i-1}$	*RelPow*$_{ij-1}$	*Allies*$_{ij-1}$	*US*$_{i-1}$	*TraDep*$_{i-1}$
Sanction	1.00							
DemDyad$_{ij-1}$	0.01	1.00						
Dem$_{i-1}$	0.02	0.54	1.00					
LogExp$_{i-1}$	0.03	0.32	0.27	1.00				
RelPow$_{ij-1}$	0.02	−0.04	0.17	0.02	1.00			
Allies$_{ij-1}$	0.01	0.08	0.02	0.16	−0.06	1.00		
US$_{i-1}$	0.10	0.05	0.10	0.16	0.12	0.07	1.00	
TraDep$_{i-1}$	0.00	0.05	−0.01	0.26	−0.04	0.09	0.00	1.00

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