Appendix

(Not for Print Publication)

This appendix includes summary statistics for our data, discussion of our measure, additional substantive results, and a series of robustness checks not included in the main text. Our robustness checks show the following:

- Our results are robust to including only those IGOs that have the relevant mandate and required capacity to enforce trade-related sanctions.
- Our results are mostly robust to looking only at the effect of primary sanction senders.
- Our results are robust to controlling for the sanctioning coalition's export portfolio variety.
- Our results are robust to including in our sample sanctions that involve suspension of economic agreements.
- Our results are robust to including in our sample only these sanctions that are over "high politics" issues.
- Our results are robust to controlling for smart sanctions, US-led sanctions and IGO involvement.
- The substantive effects of our new independent variables are comparable to established predictors of sanction success.
- Our results are robust to using an alternative definition of sanction success (our dependent variable).
- Our results remain robust when including a group of "status quo" observations and accounting for selection.
- Our results are robust across most types of sanctions such as import or export sanctions.

- Our results are generally stronger during the post-Cold War era (after 1990) than during the earlier period.
- Our results are mostly robust to calculating "market power" using only high-RCA exports.
- Our results for imposed versus threatened sanctions generally hold when using our non-parametric estimator.
- Our results provide a better fit than models that conceptualize power in terms of trade dependence.
- Our *market power* measures are conceptually related to elasticities based on Armington equations, but have significant advantages for empirical work.

Summary Statistics

Table A1: **Summary statistics**

Variable	Mean	Std. Dev.	Min.	Max.	N
Sanctions Success	0.52	0.50	0	1	851
Target's Total GDP	19.50	1.99	12.93	23.02	1012
Target's GDP per capita	8.96	1.01	5.85	10.65	1012
Target's CINC	0.03	0.05	0	0.36	1048
Target's Democracy	4.25	7.05	-10	10	1075
Cold War Dummy	0.46	0.50	0	1	1140
Security Issue	0.31	0.46	0	1	1140
Number of Sanctioners	9.92	25.47	0	154	877
Sender Market Power over Target	2.44	2.93	0	15.48	877
Target Market Power over Sender	2.53	9.49	0	139.63	877
Target Export Variety	66.30	10.60	9	79	867
Target Portfolio Concentration	0.08	0.16	0.007	0.96	866

Alternative Definitions of a "Sanction Coalition"

In our baseline analysis a "sanction coalition" includes the primary senders and members of all IGOs listed for a given sanction episode in the TIES dataset (Morgan, Bapat and Kobayashi, 2014). IGO involvement in sanctions is important, because they can require their members to implement sanctions and monitor members' compliance with this order (Martin, 1992). However, we are aware that IGOs vary in their mandates and their level of institutionalization, which means that their effectiveness in implementing trade-related sanctions may vary as well (Boehmer, Gartzke and Nordstrom, 2004; Early and Spice, 2015). For this reason we present the following analyses where we define a sanction coalition in more restrictive ways.

First, we exclude from the sanction coalition IGOs (and their members) that do not have a trade-related mandate and a high level of institutionalization. For this purpose we have obtained an updated version of the dataset of Boehmer, Gartzke and Nordstrom (2004). Based on their data we include in sender coalitions only those IGOs that have an economic mandate *and* a medium or high level of institutionalization. Second, we adopt an even more restricted definition and include only primary senders in a sanction coalition. Both cases analyze *all* available sanctions episodes, including both IGO-sponsored and non-IGO-sponsored sanctions. The difference between these models (and between these and the models presented in the main text) is the way that we treat the other member states in the organization (i.e., the way that we calculate target and sender power). Our model specifications in both cases are identical to Model 3 of Table 2. We present Logit analyses, which are easier to compare to the regression tables in the main text.

The results appear in Table A2. In Model 1 (where we only include trade-relevant and institutionalized IGOs), our findings are almost identical to the original findings, which suggests that this subset of IGOs may be driving our main results, and that IGOs with unrelated mandates or with a low level of institutionalization probably do not have much effect on sanction effectiveness. In Model 2 (where we exclude all IGO members from the sanction coalition) there are two differences: *Sender Market Power over Target*'s coefficient is 25% smaller and *Target Mar-*

ket Power over Sender is both statistically insignificant and has the wrong sign. In other words, excluding all IGOs from the analysis makes our variables related to bilateral trade seem less important. Our results in Table A2 support the literature's cautious optimism about IGOs. On the one hand, Model 2 in Table A2 shows that at least some IGOs matter and running an analysis as if IGOs do not exist will result in missing important determinants of sanction success. On the other hand, Model 1 in Table A2 shows that not all IGOs are equally effective and our evidence for IGO effectiveness actually may be driven by IGOs that have the relevant mandate and institutionalization.

Table A2: **Logit Analysis with Alternative "Sender Coalition" Definitions**

	Only trade-relevant & institutionalized IGOs	Only primary senders
	(1)	(2)
Sender Market Power over Target	0.137***	0.102***
	(0.048)	(0.031)
Target Market Power over Sender	−0.020*	0.060
	(0.012)	(0.066)
Target Export Variety	−0.053***	−0.049***
	(0.018)	(0.018)
Target Portfolio Concentration	−0.913	−0.532
	(0.806)	(0.746)
Target Portfolio Concentration	0.215**	0.234**
× Target Democracy	(0.103)	(0.100)
Target's Total GDP	0.091	0.063
	(0.139)	(0.133)
Target's GDP per capita	0.211	0.228
	(0.136)	(0.144)
CINC	2.423	−0.237
	(2.804)	(3.277)
Target Democracy	−0.066***	−0.075***
	(0.021)	(0.020)
Cold War	0.011	−0.104
	(0.226)	(0.209)
Security Issue	−0.428*	−0.397*
	(0.244)	(0.235)
Coalition Size	−0.003	0.221***
	(0.005)	(0.075)
Constant	−0.166	−0.216
	(2.128)	(2.063)
N	594	594
Log-Likelihood	−380.039	−379.279
AIC	786.077	784.558

Robust standard errors clustered on target in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All tests are two-tailed tests.

Alternative Model Specifications

As discussed in the main text our baseline specification does not include the sender coalition's export variety because this measure exhibits little empirical variation. Moreover, it is unclear theoretically how a large number of countries can help each other in product substitution, given the substantial transaction costs that come with bargaining.[1] However, we present here the results of a model specification that includes *Sender Coalition Export Variety* as a sensitivity analysis. This variable is calculated as the number of unique commodities produced by all countries in a sender coalition in a given sanction episode.

We also examine the sensitivity of our results to our definition of sanctions episodes. Since we analyze the effects of trade relations on sanction effectiveness we only look at cases where trade-related sanctions were threatened. An ambiguous case is that in which "suspension of economic agreements" were threatened. Such sanctions (and threats) were excluded from the main analysis. Below we present the results when these sanctions are also included in the the sample. This change increases our sample from 592 to 619.

Lastly, we restrict our analysis to the subset of sanctions that concern "high politics," or more serious issues. The TIES dataset lists 15 issue types over which sanctions may be threatened and/or imposed, and some of these are traditionally considered less crucial for a state's well-being than others. For instance, "containing military behavior" (*Issue* 1) and "solving territorial disputes" (*Issue* 5) are more central to a state's security concerns than "improving environmental politics" (*Issue* 12) or "implementing trade reform (*Issue* 14). It is possible that the dynamics of high politics sanctions are different than others. To explore this possibility, we restrict our analysis to sanctions in categories 1-10, which are respectively: "containing political influence," "containing military behavior," "destabilizing regime," "releasing citizens, property or material," "solving territorial dispute," "denying strategic materials," "retaliating for alliance or alignment choice," "improving human rights," "ending weapons/materials prolifer-

[1] For comparison, the standard deviation of *Target Export Variety* is 11 with a mean of 66, whereas for *Sender Coalition Export Variety* the standard deviation and mean are 8 and 72.

ation," and "terminating support for non-state actors." This list excludes the following issue types: "deterring or punishing drug trafficking practices," "improving environmental policies," "trade practices," "implementing economic reform," and "other issues". This restriction reduces our sample from 592 to 222.

Table A3 presents the results from these three different specifications. In all cases, we continue to find that target market power over sender and target export variety reduce sanction success whereas sender power increases it. In Models 1 and 2 we find a positive significant effect for *Target Portfolio Concentration* × *Target Democracy*, but in Model 3 the effect is attenuated and is no longer statistically significant. This change may help to explain the puzzling finding regarding the positive effect of portfolio concentration in authoritarian states, as it suggests that variable has a notable impact only for sanctions that involve low-politics issues.

Table A3: **Logit Analysis with Different Specifications**

	Control for sanctioners' export variety (1)	Include sanctions on "econ agreement sanctions" (2)	Only sanctions over high politics issues (3)
Sender Market Power over Target	0.140**	0.117**	0.103*
	(0.055)	(0.051)	(0.057)
Target Market Power over Sender	−0.019*	−0.020*	−0.038**
	(0.011)	(0.010)	(0.016)
Target Export Variety	−0.054***	−0.055***	−0.055***
	(0.019)	(0.019)	(0.020)
Target Portfolio Concentration	−0.516	−0.517	−1.893
	(0.771)	(0.757)	(1.321)
Target Portfolio Concentration	0.281***	0.281***	0.133
× Target Democracy	(0.102)	(0.101)	(0.153)
Sender Coalition Export Variety	−0.002		
	(0.011)		
Target's Total GDP	0.090	0.084	0.260
	(0.137)	(0.138)	(0.179)
Target's GDP per capita	0.290**	0.334**	−0.070
	(0.139)	(0.146)	(0.215)
CINC	1.960	1.834	−4.488
	(2.722)	(2.813)	(3.953)
Target Democracy	−0.076***	−0.074***	−0.066**
	(0.020)	(0.021)	(0.033)
Cold War	0.024	0.084	0.548*
	(0.232)	(0.221)	(0.331)
Security Issue	−0.210	−0.197	0.349
	(0.242)	(0.238)	(0.463)
Coalition Size	−0.005	−0.002	−0.004
	(0.007)	(0.007)	(0.007)
Constant	−0.637	−0.960	−1.265
	(2.058)	(2.041)	(2.743)
N	592	619	222
Log-Likelihood	−382.739	−402.453	−137.213
AIC	793.478	830.906	300.425

Robust standard errors clustered on target in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All tests are two-tailed tests.

Additional Controls and Alternative Dependent Variable

Table A4 reports results from two robustness checks. In the first column, we estimate the standard model from the main text, and include three additional control variables: whether "smart" sanctions (asset freezes, travel bans, etc.) were used or threatened, whether the U.S. was among the primary senders in the coalition, and whether sanctions were brought to bear under the aegis of an international governmental organization. In general, the substantive results are the same as those reported in the main text. We also find that sanctions are significantly more likely to succeed when an IGO is involved.

In the second column, we use an alternate specification of our dependent variable. In our main analysis, we include all sanctions episodes, marking those that ultimately fizzle out as failed attempts at sanctioning the target. Here, we drop those observations, looking only at the observations for which we have definitive termination years. This ensures that our results are not driven by ambiguous cases. When we run this specification, we find that our results are very similar to those in the primary model, as well as the results in column 1, suggesting that they are robust to these alternative specifications.

Table A4: **Additional Controls and Alternative DV**

	Additional Controls (1)	Alternative Dependent Var (2)
Sender Market Power over Target	0.095**	0.113**
	(0.042)	(0.053)
Target Market Power over Sender	−0.034*	−0.021*
	(0.018)	(0.011)
Target Export Variety	−0.053***	−0.059***
	(0.020)	(0.021)
Target Portfolio Concentration	−0.989	−0.856
	(0.729)	(0.878)
Target Portfolio Concentration × Target Democracy	0.233**	0.264**
	(0.101)	(0.109)
Target's Total GDP	0.106	0.083
	(0.143)	(0.141)
Target's GDP per capita	0.250*	0.253*
	(0.143)	(0.130)
CINC	1.715	1.575
	(2.927)	(2.814)
Target Democracy	−0.072***	−0.075***
	(0.021)	(0.021)
Cold War	0.038	0.071
	(0.213)	(0.248)
Security Issue	−0.125	−0.524**
	(0.269)	(0.244)
Coalition Size	−0.010	−0.003
	(0.007)	(0.007)
Smart Sanctions	−0.387	
	(0.389)	
US Among Primary Senders	0.274	
	(0.251)	
IGO Involved	1.255***	
	(0.314)	
Constant	−0.977	0.432
	(2.074)	(2.073)
N	594	539
Log-Likelihood	−372.590	−344.353
AIC	777.179	714.707

Robust standard errors clustered on target in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All tests are two-tailed tests.

Additional Substantive Results

Our primary analysis focuses on four variables of interest: sender market power, target market power, target export variety, and target portfolio concentration. However, other variables, for which we control, may also be of interest to the reader. In Figure A1, we present results from Model 1 in Table A4, comparing the four variables of interest to the effects of target Polity score and IGO involvement, as each of the variables changes from its minimum to its maximum value. We hold continuous variables at their means and discrete variables at their medians (dichotomous variables are set to zero), unless otherwise indicated. To facilitate comparison, we place all results on a common y-axis, ranging between 0 and 1.

Across the different variables, the largest absolute difference comes from target portfolio variety, for which the probability of success declines from 0.9 for targets that export only nine goods to 0.18 for targets that export 79. In general, the effects of democracy and IGO involvement are roughly similar to sender and target power, changing the probability of success by about 0.3–0.4.

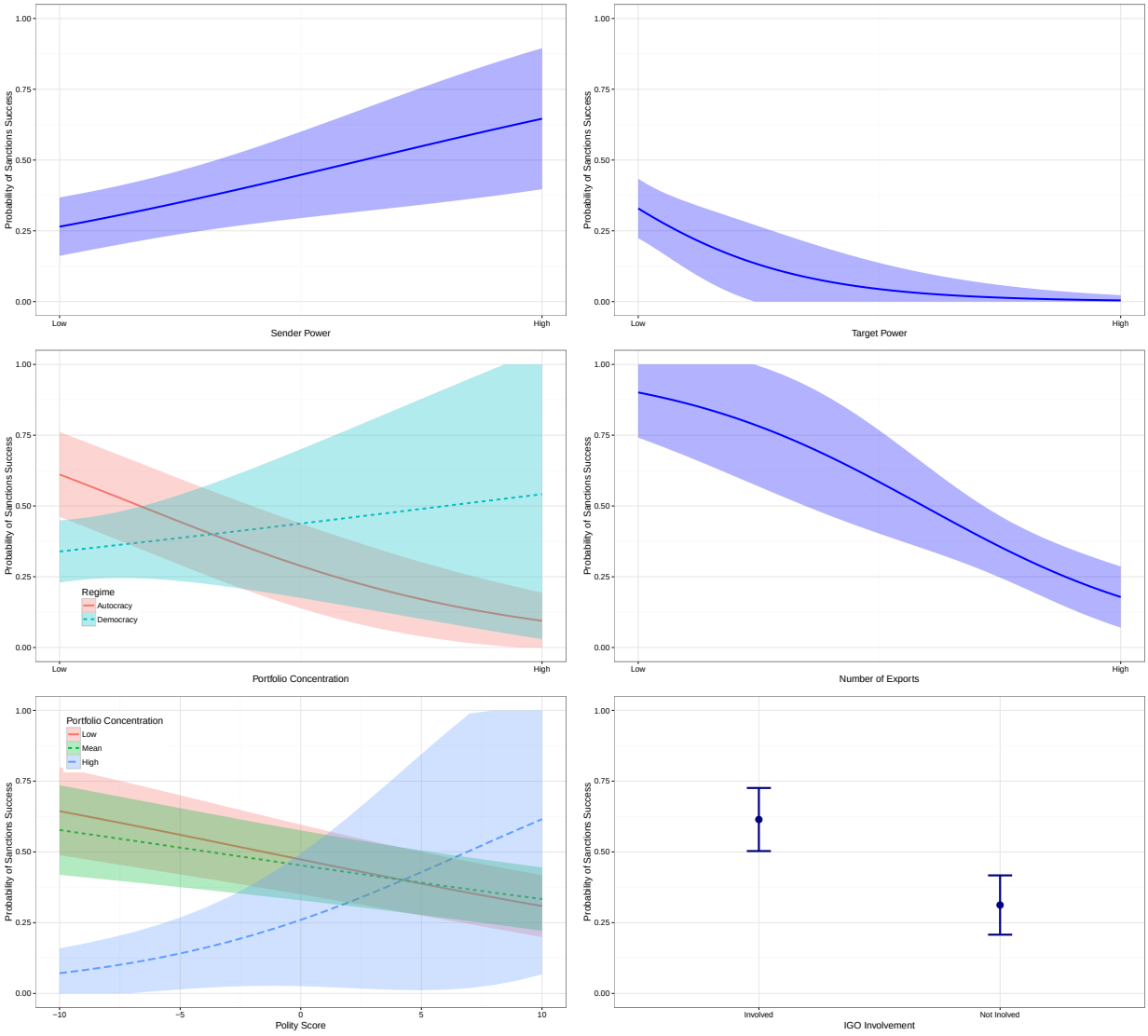

Figure A1: Comparison of predicted probabilities

Selection and the Status Quo

Our application of a local likelihood logit regression to our full sample in the main text uses a highly-flexible, fully non-parametric estimator to avoid problems caused by specification bias. The benefit of this approach is that it is robust to the type of specification error (i.e., whether the true data generating process is simple non-random selection, or whether it is better described by strategic interaction). Other authors have dealt with this problem in other ways. In particular, Whang, McLean and Kuberski (2013), following examples from the conflict literature (Huth and Allee, 2002; Lewis and Schultz, 2005), generate a set of hypothetical status quo cases to include in their structural analysis. In this section, we employ their procedure to do the same.

The first step in the generation of status quo cases is the identification of the set of relevant dyads and the time frame. Our data span the period 1945–2004, and cover 417 unique dyads.[2] This gives us significantly greater coverage than Whang et al., who were dealing with a shorter time span. Following their specification, we divide the period into decades, grouped by the third digit of the year (i.e., 1940s, 1950s, 1960s, etc.). For any sender-target dyad with no sanctions threats in a decade, we code one status quo observation, assigned randomly to a year in that decade. Doing so provides us with 357 status quo cases, bringing our total number of observations to 1,497.[3] In line with standard practice, status quo observations are coded zero for both imposition and victory.

We analyze these data in two ways. First, we run parametric Heckman selection models. Table A5 presents two Logit and two Heckman models. In the two Heckman models we identify the model using two sets of variables that are included in the selection stage, but excluded from the outcome stage. In one model we have *UN Voting Similarity* included in the selection stage. In the second Heckman model we have a set of *Decade* dummies in the selection stage. In both Heckman models in the first stage the *Market Power* measures are calculated between the target and the primary sender, because at the status quo stage it is unclear to the primary disputants

[2] Of these, 394 include two actors with valid COW country codes.

[3] Unfortunately, due to missing data on control variables for some cases, we cannot use all of the extra observations.

what kind of a sender coalition will form if a sanction threat is made. In the outcome stage these variables are calculated between the target and the whole sanction sender coalition since at that stage we know from the TIES dataset the members of a sender coalition.

Our original results are in column 1. Because *UN Voting Similarity* is missing for some of these observations, column 2 shows a similar Logit estimated on the subset of observations for which *UN Voting Similarity* is not missing. Note that this change in the sample makes the coefficient for *Target Portfolio Concentration* × *Target Democracy* smaller.

In the outcome stage both Heckman models produce results that are mostly consistent with each other and with our original findings. The one exception is that in column 3, *Target Portfolio Concentration* × *Target Democracy* is not statistically significant, but this could be due to the change in the sample induced by missing data (as seen in column 2).

In addition to the parametric estimates, we replicate our local logit analysis, using the full data set, which includes status quo cases.[4] The plots in Figure A2 depict the results from the local likelihood logistic analysis, with status quo cases included in the data set. While there are some minor differences between the plots here and those in the main text, they are substantively similar in most ways. This provides further evidence that our results are not driven by the characteristics that led to the dispute in the first place. Table A5 and Figure A2 demonstrate that market power has a significant impact on sanction success, even when we account explicitly for non-random selection.

[4]Non-parametric analyses that include status quo observations exclude, by necessity, the variables accounting for number of sanctioners and the presence of a security issue. Since there is no sanction threat, these would be coded as missing.

Table A5: **Heckman Selection Models**

	Logit	Logit	Heckman		Heckman	
	Original Model	*UN Vote* nonmissing	Outcome Stage	Selection Stage	Outcome Stage	Selection Stage
	(1)	(2)	(3)	(4)	(5)	(6)
Sender Coalition's Market Power over Target	0.128**	0.151**	0.0911**		0.0749**	
	(0.05)	(0.06)	(0.04)		(0.03)	
Target Market Power over Sender Coalition	−0.0195*	−0.0168*	−0.0106*		−0.0116**	
	(0.01)	(0.01)	(0.01)		(0.01)	
Target Export Variety	−0.0539**	−0.0623**	−0.0377**	0.00447	−0.0235**	0.0124*
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Target Portfolio Concentration	−0.544	−1.400	−0.927	0.426	−0.203	0.627
	(0.77)	(1.01)	(0.61)	(0.52)	(0.48)	(0.42)
Target Portfolio Concentration × Target Democracy	0.287**	0.105	0.0878	−0.206**	0.114*	−0.174**
	(0.10)	(0.13)	(0.10)	(0.08)	(0.07)	(0.06)
Primary Sender's Market Power over Target				0.0289		0.0440
				(0.08)		(0.08)
Target Market Power over Primary Sender				0.00		0.00
				(0.00)		(0.00)
Target's Total GDP	0.0876	0.183	0.0926	0.0842	0.0618	0.0236
	(0.14)	(0.15)	(0.10)	(0.07)	(0.08)	(0.06)
Target's GDP per capita	0.291**	0.254*	0.143	0.00970	0.176**	0.0458
	(0.14)	(0.15)	(0.10)	(0.10)	(0.08)	(0.09)
CINC	1.975	1.886	1.131	1.205	0.989	0.824
	(2.69)	(3.12)	(1.84)	(1.70)	(1.48)	(1.65)
Target Democracy	−0.0759**	−0.0589**	−0.0433**	0.0534**	−0.0307**	0.0452**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Cold War	0.0502	0.205	0.105	0.137	0.0576	
	(0.23)	(0.27)	(0.18)	(0.14)	(0.12)	
Security Issue	−0.231	−0.467*	−0.279*		−0.162	
	(0.24)	(0.28)	(0.17)		(0.13)	
Coalition Size	−0.00352	−0.00930	−0.00560		−0.00264	
	(0.01)	(0.01)	(0.00)		(0.00)	
UN Voting Similarity				0.406**		
				(0.17)		
Decade Dummies						Yes
ρ			-0.418		0.917*	
			(1.18)		(0.49)	
N	594	455	561 (455 uncens'd)		724 (594 uncens'd)	
Log-Likelihood	−384.3	−290.3	-527.2		-689.0	

Target-clustered s.e. are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All tests are two-tailed tests.

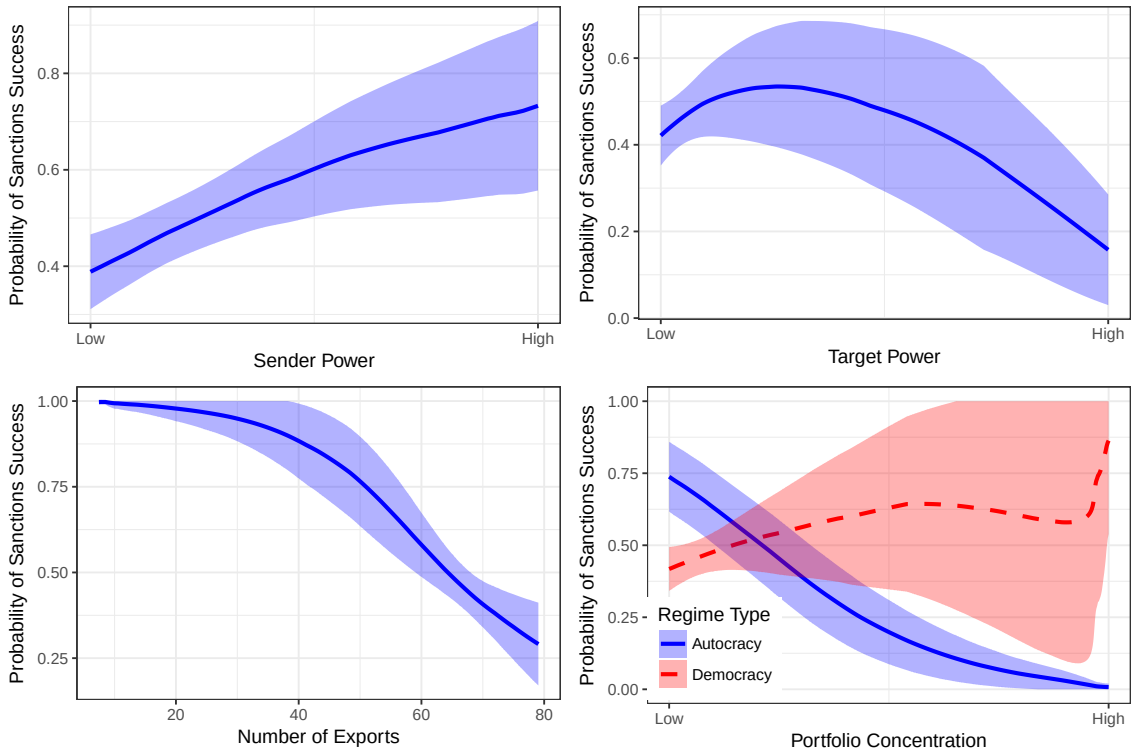

Figure A2: Local logit results including status quo cases

Sanction Threat Type

We also analyze the extent to which our results change if we subset our sample by type: import restrictions vs. export restrictions. We subset the data using the *Sanction Threat Type* variable in the TIES dataset.

Our primary analysis treats each sanction as threatening the total trade between target and sender countries regardless of what senders say at the beginning of a sanction episode. Our argument is that this is because sanctions can (and often do) grow as the crisis evolves, and therefore neither state can be certain which commodities will ultimately be left alone. However, suppose that we take the sender's pronouncements seriously and look only at the scope of the sanctions threatened. Do our variables have different effects in sanctions with different types of restrictions or different scopes? We examine these questions in Table A6.

Here, Model 1 is our baseline model, which uses the whole sample. Model 2 looks only at those sanction threats that involve import restrictions (i.e., the sender threatens to ban the purchase of some goods from the target country). Sanction types that fall into this category are "total economic embargo", "partial economic embargo," "import restriction," and "blockade." In Model 3, we look only at threats involving export restrictions (i.e., the sender threatens to forbid the sale of certain goods to the target country). This subset comprises the following categories: "total economic embargo," "partial economic embargo," "export restriction," and "blockade." The estimates in models 1–3 are quite similar, particularly in terms of sign and significance. The only notable change is GDP per capita, which loses significance in the export restriction model. However, the coefficient estimate is similar to that of the baseline model, and the loss of significance may be due to the larger standard errors induced by the smaller set of observations.

Table A6: **Subsetting Sample by Sanction Type**

	Baseline Model	Import Restrictions	Export Restrictions
	(1)	(2)	(3)
Target's Total GDP	0.088	0.221	0.213
	(0.137)	(0.150)	(0.152)
Target's GDP per capita	0.291**	0.510***	0.309
	(0.139)	(0.143)	(0.212)
CINC	1.975	−0.246	−2.910
	(2.692)	(3.064)	(4.077)
Target Democracy	−0.076***	−0.106***	−0.081**
	(0.020)	(0.020)	(0.033)
Cold War	0.050	−0.063	0.044
	(0.225)	(0.240)	(0.295)
Security Issue	−0.231	0.043	−0.053
	(0.240)	(0.305)	(0.275)
Coalition Size	−0.004	0.002	−0.010
	(0.007)	(0.009)	(0.007)
Sender Market Power over Target	0.128**	0.133***	0.142***
	(0.050)	(0.052)	(0.054)
Target Market Power over Sender	−0.020*	−0.033*	−0.023**
	(0.011)	(0.019)	(0.011)
Target Export Variety	−0.054***	−0.082***	−0.051**
	(0.019)	(0.025)	(0.020)
Target Portfolio Concentration	−0.544	0.227	−0.414
	(0.772)	(0.794)	(1.048)
Portfolio Concentration × Democracy	0.287***	0.484***	0.216*
	(0.102)	(0.106)	(0.129)
Constant	−0.695	−3.280	−3.192
	(2.023)	(2.325)	(2.396)
N	594	521	298
Log-Likelihood	−384.342	−326.367	−191.890
AIC	794.684	678.733	409.780

Target-clustered robust s.e. in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Time Periods

There is a distinct possibility that the purpose and means of implementing sanctions has changed over the last few decades. In particular, we know that smart sanctions are more common in recent years than they were in the past. Thus, it may be the case that our variables are more effective at one point in our data set than in another. Table A7 examines subsets of our data based on when a sanction began. For a number of reasons, we opt to use the Cold War (whether the sanction began before or after 1990) as our cut point. Model 1 provides estimates for our variables when applied to sanctions that began during the Cold War. Model 2 looks only at post-Cold War sanctions. In Model 3, we examine the entire data set but look at differences in the two periods by interacting our main variables with a dummy for the Cold War.

The estimated effect of our *Sender Market Power* power is positive and significant in both eras. *Target Market Power* attains significance only in the Cold War model (though the coefficients in each of the three models are nearly identical). Our measure of *Target Export Variety* shows significance in Model 1, though not in Model 2. When we look at the interaction with the Cold War dummy, we find significant effects in both eras, though they are stronger during the Cold War than after, suggesting that export variety has begun mattering less in recent years. Finally, our *Portfolio Concentration* measure is a bit more complex. The main effect of concentration (i.e., portfolio concentration for non-democratic states) is significant only in Model 1. When we move to the interaction model, it loses significance. The results for its interaction with democracy are just the opposite. We find significance in Model 1, and we find that it mattered in both eras. Interestingly, however, the effect of concentration for democracies switches after 1990, making it similar to that of autocracies (i.e., in the later era, greater portfolio concentration makes sanctions against democracies less likely to succeed).

Table A7: **Sanctions During and After the Cold War**

	Cold War	Post-Cold War	Interaction
	(1)	(2)	(3)
Sender Market Power over Target	0.138**	0.161**	0.199***
	(0.056)	(0.082)	(0.075)
Target Market Power over Sender	−0.026*	−0.023	−0.029
	(0.014)	(0.017)	(0.022)
Target Export Variety	−0.121***	−0.023	−0.035*
	(0.028)	(0.022)	(0.019)
Target Portfolio Concentration	−2.066**	0.152	0.624
	(1.031)	(1.771)	(1.839)
Target Portfolio Concentration × Target Democracy	0.228	0.584***	0.681***
	(0.144)	(0.177)	(0.174)
Cold War			5.188***
			(1.574)
Cold War × Sender Market Power over Target			−0.095
			(0.081)
Cold War × Target Market Power over Sender			0.009
			(0.025)
Cold War × Target Export Variety			−0.072***
			(0.024)
Cold War × Target Portfolio Concentration			−2.956
			(2.120)
Cold War × Target Democracy			0.029
			(0.033)
Cold War × Target Portfolio Concentration × Target Democracy			−0.517**
			(0.206)
Target's Total GDP	0.178	−0.005	0.098
	(0.164)	(0.171)	(0.132)
Target's GDP per capita	0.344*	0.257	0.300**
	(0.206)	(0.184)	(0.140)
CINC	4.669	1.042	2.400
	(3.337)	(3.565)	(2.562)
Target Democracy	−0.078***	−0.084**	−0.093***
	(0.027)	(0.036)	(0.031)
Security Issue	−0.086	−0.319	−0.201
	(0.301)	(0.459)	(0.246)
Coalition Size	−0.008	0.000	−0.003
	(0.008)	(0.009)	(0.006)
[0.2em] Constant	1.659	−0.665	−2.436
	(2.889)	(2.954)	(2.036)
N	295	299	594
Log-Likelihood	−181.671	−193.670	−377.719
AIC	387.341	411.341	793.437

Target-clustered robust s.e. in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Only Countries with High RCA

Here we calculate our “market power" measures in a different way. Our original operationalization takes advantage of the fact that RCA is a continuous measure and uses that to weight trade volume to arrive at our market power measure. Here we assume that unless a country has an exceptionally high RCA in a given commodity, then its trade partners can easily switch to alternative exporters of that good. Specifically, for each commodity-year we calculate the five countries with the highest RCA and create a binary variable that is coded 1 for that exporter-commodity-year, and 0 otherwise. We then use this binary *High RCA* variable to weight trade volume.

Table A8 presents our estimates using this alternative measure. Most of our results, with one exception, are robust. We no longer find *Target Market Power over Sender* to have a significant effect even though the coefficient has the expected sign. This change is not surprising since this alternative operationalization is very restrictive. The lack of an effect for *Target Market Power* suggests that the original continuous measure of RCA appears to be more appropriate for the purpose of estimating market power.

Table A8: **Logit Analysis with Only High RCA Countries**

	(1)	(2)
Sender Market Power over Target	0.161**	0.154**
	(0.070)	(0.068)
Target Market Power over Sender	−0.010	−0.010
	(0.034)	(0.035)
Target Export Variety	−0.043**	−0.046**
	(0.018)	(0.019)
Target Portfolio Concentration	−1.163	0.218
	(0.736)	(0.755)
Target Portfolio Concentration × Target Democracy		0.264**
		(0.107)
Target's Total GDP	−0.045	0.016
	(0.130)	(0.135)
Target's GDP per capita	0.181	0.289*
	(0.140)	(0.151)
CINC	3.062	2.195
	(2.698)	(2.704)
Target Democracy	−0.037**	−0.067***
	(0.018)	(0.021)
Cold War	0.075	0.098
	(0.214)	(0.218)
Security Issue	−0.470**	−0.434*
	(0.239)	(0.247)
Coalition Size	0.003	0.003
	(0.005)	(0.005)
Constant	2.129	0.259
	(1.974)	(2.062)
N	554	554
Log-Likelihood	−366.619	−363.930

Target-clustered s.e. are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. All tests are two-tailed tests.

Analyzing the Subsamples of Threatened and Imposed Sanctions

Figures A3 and A4 provide local likelihood logistic estimates for threatened and imposed sanctions, respectively. These analyses are analogous to those reported in Table 2 Models 4 and 5 in the main text. In Figure A3, we look at all sanction threats, and code them as successes if the target acquiesces without sanction imposition and failures otherwise. We then use our non-parametric estimator to examine the effect of each of four key independent variables while holding other variables constant at their means (for continuous and polytomous variables) or at zero (for binary variables). Using the flexible, non-parametric estimator allows us to recover effects without worrying about structural misspecification. The results are comparable to those in the main text in many respects. With respect to target power and number of exports, we tend to see significant effects only for imposition and not for threats. For portfolio concentration, however, the difference between democracy and autocracy is more pronounced with threats than with imposed sanctions. This, again, is consistent with our main findings. The only major difference is in our estimate of sender power. Here, we estimate a large and significant effect for *both* models using the local logit, while the parametric logit found no effect of sender power on threat effectiveness. One possibility is that assumptions about model specification behind normal logit result in the non-finding for *Sender Power*, whereas the local logit is more capable of dealing with these issues and recovers the effect. Another explanation for the discrepancy is the weighting of observations for the local logit. While parametric modesl derive estimates using all observations with no weighting scheme, the non-parametric estimator weights according to distance from the specified profile of values. We would likely find other effects if we were to alter this profile. In general, however, our estimates are findings are consistent with those in the main text.

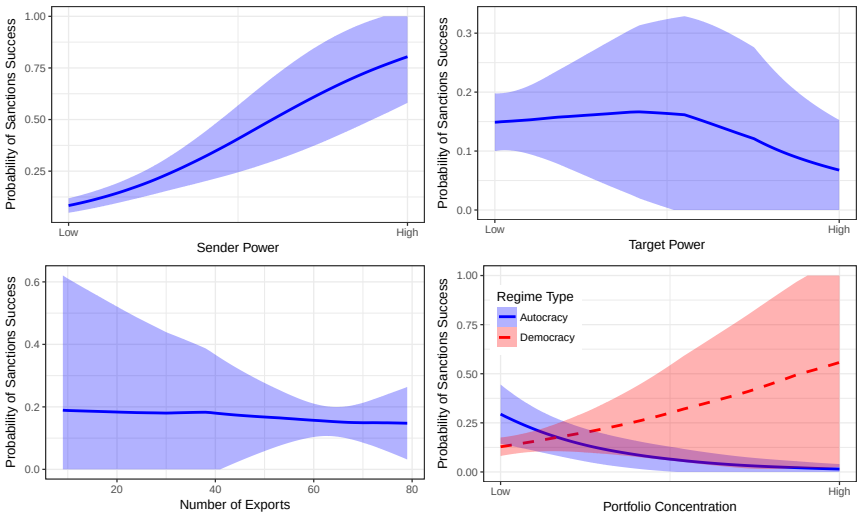

Figure A3: Predicted probabilities for threat effectiveness

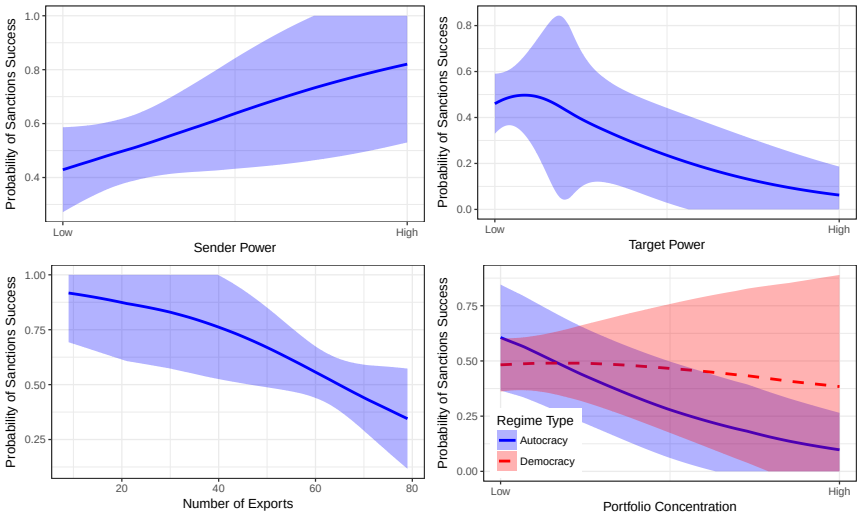

Figure A4: Predicted probabilities of sanction success for imposed sanctions

Model Fit Comparison of *Market Power* and *Trade Dependence*

An important empirical contribution of our work is to offer a number of new measures that are related to the costliness of economic sanctions for target and sender countries. It is natural to ask whether these conceptualizations provide an improvement over previous measures. In Table 2 of the main text, we directly compare our models with a measure of the target's total trade dependence upon the sender. We show that our measures substantially improve model fit when they are incorporated into the analysis. In Table A9, we go a bit deeper, examining not only total trade dependence, but disaggregated import and export dependence as well.

Model 1 is our baseline model, which uses our new measures (this is simply a replication of Model 3 in Table 2 of the main text). In the other models we use variants of trade dependence. Model 2 uses Target Export Dependence and Target Import Dependence, which are calculated by taking the ratio of target exports to the sender and target imports from the sender respectively to target GDP. As with all of our variables, these are measured for the year preceding sanctions. In none of the models in Table A9 do any of the measures of raw trade dependence attain significance. In addition, these models show higher AIC values and smaller areas under the ROC curve than Model 1, suggesting that the latter is superior in terms of model fit.

One might argue that these trade dependence measures are simply close substitutes for our "market power" measures. However, they should be significantly different from both Portfolio Variety and Portfolio Concentration. Therefore, Model 3 adds these two variables to the analysis. The estimated coefficients for Variety and Concentration are similar to those in Model 1. While Model 3 shows improved AIC and AUC values, they are still inferior to the values in Model 1.

In Models 4 and 5, we switch to the conventional Trade Dependence measure, which is calculated as total target trade with sender divided by target GDP. This measure is positive and significant in Models 4 and 5, as expected. However, the fit for Models 4 and 5 remains inferior to that of Model 1. Moreover, this specification obscures the important lesson that total trade does not have a unidirectional effect on sanction outcome. Depending on each side's comparative advantages, greater trade can be an advantage for the target or the sender coalition. More

nuanced measures—such as our market power variables—are necessary to capture this insight.

Table A9: **Comparison of Market Power and Trade Dependence**

	Baseline Model	Unweighted Dependence	Add Variety and Concentration	Unweighted Total Dependence	Add Variety and Concentration
	(1)	(2)	(3)	(4)	(5)
Target's Total GDP	0.088	−0.137	0.096	−0.142	0.080
	(0.137)	(0.112)	(0.136)	(0.114)	(0.137)
Target's GDP per capita	0.291**	−0.027	0.218	−0.018	0.231
	(0.139)	(0.150)	(0.156)	(0.147)	(0.154)
CINC	1.975	4.964**	1.059	4.962**	1.143
	(2.692)	(2.408)	(2.715)	(2.380)	(2.718)
Target Democracy	−0.076***	−0.022	−0.078***	−0.022	−0.078***
	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)
Cold War	0.050	−0.115	0.064	−0.113	0.063
	(0.225)	(0.200)	(0.219)	(0.198)	(0.217)
Security Issue	−0.231	−0.450**	−0.295	−0.457**	−0.308
	(0.240)	(0.221)	(0.241)	(0.221)	(0.240)
Coalition Size	−0.004	0.004	0.001	0.004	0.002
	(0.007)	(0.004)	(0.005)	(0.004)	(0.005)
Sender Market Power over Target	0.128**				
	(0.050)				
Target Market Power over Sender	−0.020*				
	(0.011)				
Target Export Variety	−0.054***		−0.055***		−0.053***
	(0.019)		(0.019)		(0.019)
Target Portfolio Concentration	−0.544		−0.556		−0.642
	(0.772)		(0.814)		(0.811)
Portfolio Concentration × Democracy	0.287***		0.316***		0.316***
	(0.102)		(0.104)		(0.105)
Target's Export Dependence		4.697	7.417		
		(6.559)	(5.273)		
Target's Import Dependence		14.108	22.409		
		(18.554)	(18.433)		
Target's Trade Dependence				6.913*	10.833***
				(4.040)	(4.070)
Constant	−0.695	2.832	0.023	2.843	0.144
	(2.023)	(1.858)	(2.105)	(1.850)	(2.121)
N	594	595	594	595	594
Log-Likelihood	−384.342	−401.781	−388.130	−401.867	−388.322
AIC	794.684	823.562	802.259	821.735	800.644
Area under ROC Curve	0.68	0.62	0.67	0.62	0.67

Target-clustered robust s.e. in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We can also compare the models graphically. Figure A5 compares the results in our primary model (Column 1 of Table A9) to the results from a full model that uses trade dependence instead of market power (Column 5 of Table A9). While all goodness-of-fit measures (including log-likelihood, AIC, and area under curve) suggest a better model fit for the former, it is wortwhile to view the results graphically. Figure A5 plots the proportion of observations correctly predicted by each model as we vary the threshold for a prediction of successful sanc-

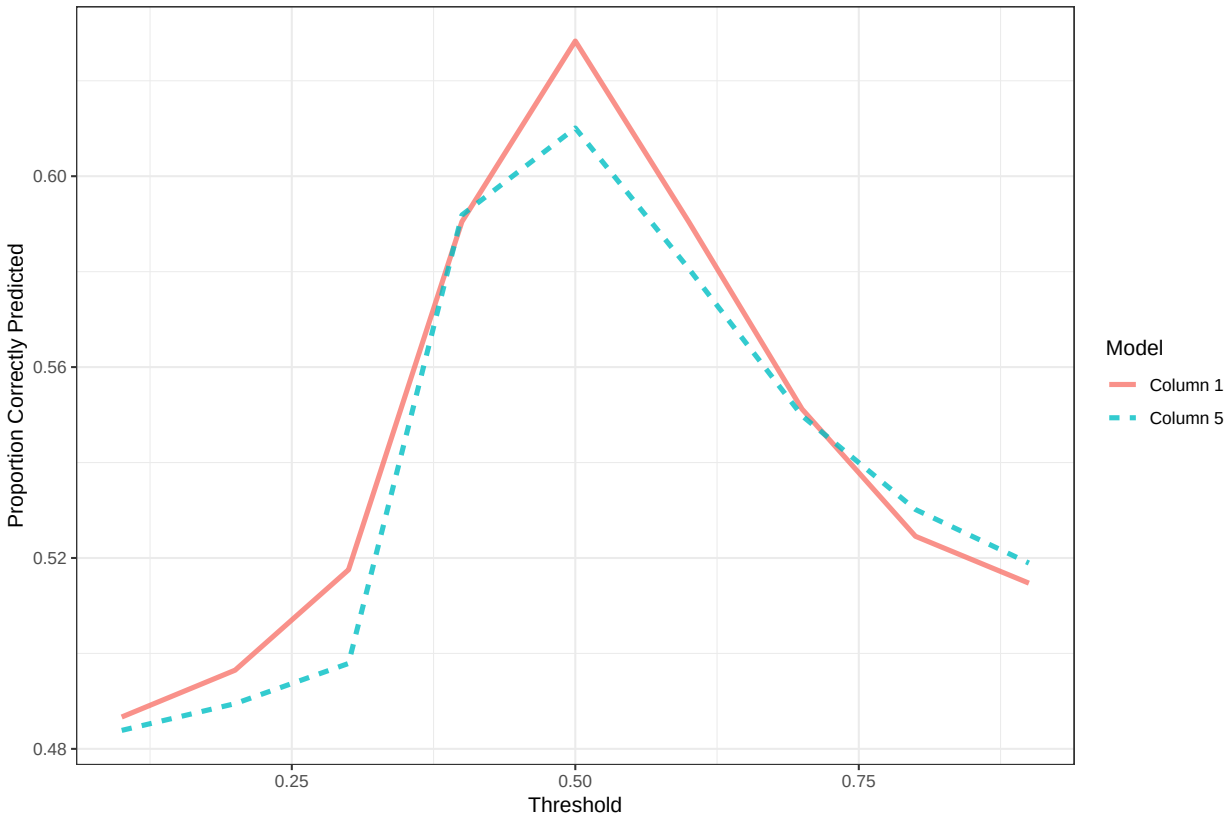

Figure A5: Comparison of market power and aggregate trade models

tions, from 0.1 to 0.9. While our model is not dominant across the *entire* range of thresholds, it generally predicts a larger percentage of observations correctly than does the aggregate trade model. Importantly, this difference is especially pronounced around the crucial 0.5 mark. In this region—where we would generally place the threshold for a positive prediction—we see some of the largest gaps between the two models. The largest advantage for one model over the other is around 0.3, where our model provides an improvement of about 4%. At the 0.5 threshold, we improve upon the aggregate trade model by approximately 3%. In general, this demonstrates a clear advantage to using measures of market power over measures of aggregate trade.

Our primary analysis also analyzed the effect of market power on sanction success for imposed versus threatened sanctions, finding that our measures were particularly important when

Table A10: **Comparison of Market Power and Trade Dependence (Only Imposed Sanctions)**

	Baseline Model	Unweighted Dependence	Add Variety and Concentration	Unweighted Total Dependence	Add Variety and Concentration
	(1)	(2)	(3)	(4)	(5)
Sender Market Power over Target	0.228***				
	(0.065)				
Target Market Power over Target	−0.037**				
	(0.015)				
Target Export Variety	−0.084***		−0.090***		−0.089***
	(0.029)		(0.030)		(0.030)
Target Portfolio Concentration	−2.421		−2.387		−2.732*
	(1.556)		(1.544)		(1.545)
Target Portfolio Concentration × Target Democracy	0.014		0.076		0.059
	(0.189)		(0.171)		(0.170)
Target's Export Dependence		−0.475	4.152		
		(7.498)	(7.773)		
Target's Import Dependence		45.959*	46.756*		
		(27.802)	(27.390)		
Target's Trade Dependence				9.027	12.942**
				(6.816)	(6.120)
Target's Total GDP	0.037	−0.201	0.090	−0.210	0.067
	(0.180)	(0.150)	(0.177)	(0.149)	(0.175)
Target's GDP per capita	0.059	−0.241	−0.012	−0.201	0.020
	(0.204)	(0.227)	(0.244)	(0.222)	(0.243)
CINC	3.551	5.467	0.736	5.111	0.523
	(3.838)	(3.976)	(4.086)	(3.887)	(4.047)
T's Democracy	0.005	0.027	−0.013	0.029	−0.012
	(0.033)	(0.025)	(0.032)	(0.025)	(0.032)
Cold War	0.335	0.146	0.433	0.141	0.429
	(0.343)	(0.303)	(0.329)	(0.302)	(0.326)
Security Issue	−0.100	−0.234	−0.089	−0.243	−0.105
	(0.378)	(0.374)	(0.369)	(0.375)	(0.371)
Coalition Size	0.009	0.019***	0.016**	0.021***	0.018***
	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
Constant	3.511	5.266**	3.860	5.163**	4.068
	(2.777)	(2.381)	(2.802)	(2.340)	(2.811)
N	312	312	312	312	312
Log-Likelihood	−182.157	−199.332	−188.494	−200.222	−189.193
AIC	390.314	418.663	402.989	418.443	402.386
Area under ROC Curve	0.75	0.66	0.74	0.66	0.74

Target-clustered robust s.e. in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

sanctions were actually carried out. Therefore, it is also useful to compare the effectiveness of our measures to that of the total trade dependence model for imposed sanctions specifically. We do this in Table A10. In general, our results show that Model 1 exhibits the best fit by far, in terms of AIC scores. In terms of area under the ROC curve, Model 1 again shows the best performance, though here Models 3 and 5 (which include trade dependence and our *export variety* and *export concentration* measures) perform only slightly worse.

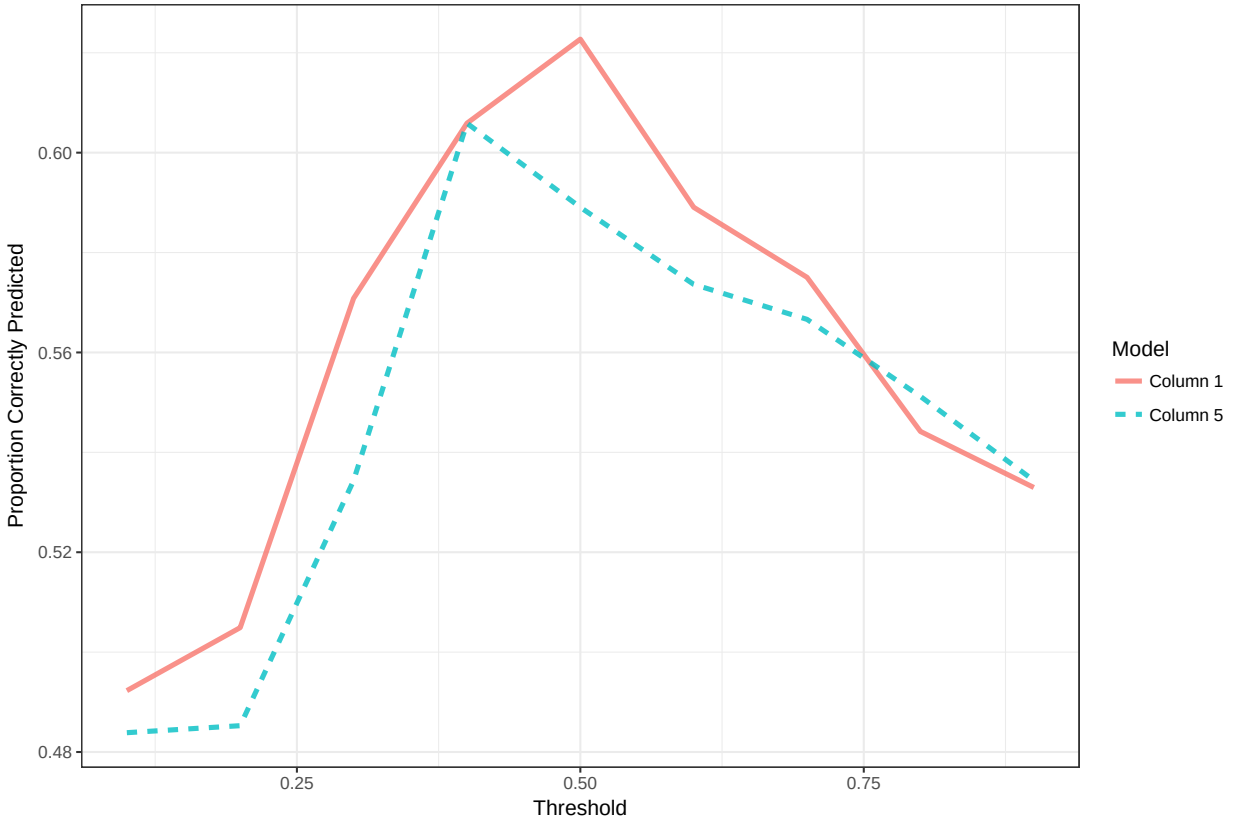

Figure A6: Comparison of market power and aggregate trade for imposed sanctions

As before, we can compare the models graphically. Figure A6 shows the comparison of correct predictions for the market power and trade dependence models, respectively. Once again, the figure demonstrates that our measures of sender and target power generally provide a better fit than aggregate trade dependence. Indeed, with imposed sanctions, the dominance of our preferred model is even stronger. As before, with the exception of very high thresholds, our

model generally outperforms the aggregate model, and the gaps are even larger than in Figure A5. Moreover, around the crucial threshold of 0.5, our model provides an especially large improvement. This figure provides further evidence of the usefulness of our measures of market power.

Revealed Comparative Advantage and Elasticity

Our measure of market power is conceptually similar in many ways to the concept of (inverse) elasticity. An elastic good is one for which there is a strong and immediate response to a change in price. Countries with inelastic export supply curves will tend to have greater market power than those with more elastic curves (Broda, Limao and Weinstein, 2008). Therefore, our measure of market power ought to be negatively correlated with measures of the elasticity of supply. To check this, we compare measures of commodity-level revealed comparative advantage to export supply elasticity. However, there are a few significant obstacles to using these elasticity measures in empirical work. In particular, it requires estimation of a system of simultaneous equations, which employs data that are often difficult to find (such as commodity price and share of the variety of a particular commodity) and generally requires the analyst to assume homogeneity of elasticity over time.

We use David Weinstein's calculation of export supply elasticity for the United States and 15 non-WTO countries over the 1994–2003 period as our measure of elasticity.[5] We then use our procedure to compute relative revealed comparative advantage for each country-commodity over this decade, using the six-digit HS scores (aggregated to four digits).[6]

The correlation between our measure and the measure of elasticity is weakly negative ($r \approx -0.02$), suggesting very little in common between the two measures, though what relationship exists is in the correct direction. However, plotting RCA against elasticity in Figure A7 sheds some light on the reasoning: there is significant clustering in the lower-left corner of the plot. Most commodities are highly inelastic. Although the observed export elasticity measure goes as high as 4350.415, nearly two-thirds of observations have an export elasticity value smaller than one. The 90th percentile is just over 5.5. The lack of variation reduces the level of correlation

[5] These data are available from `http://www.columbia.edu/~dew35/TradeElasticities/TradeElasticities.html` and are calculated according to the procedure given in Broda, Limao and Weinstein (2008).

[6] For data reasons, we are able to use only eleven of the countries involved. This gives us a total of 7,250 country-commodity-level observations for comparison.

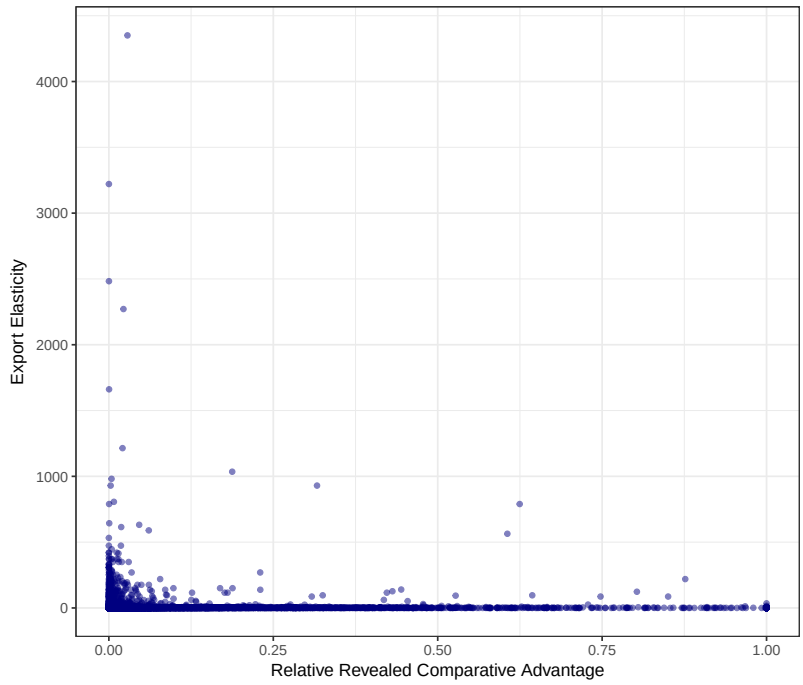

Figure A7: Comparison of RCA and elasticity

between the two measures. However, it is worth noting that all of the very large elasticity values have very small RCA values. Among those largest values, the relationship is much more strongly negative ($r \approx -0.26$ for elasticity over 500, for example).

It is worth noting, however, that this comparison is somewhat artificial. Our RCA measure is much more dynamic than traditional measures of elasticity, changing each year. Elasticity measures, by contrast, tend to be computed over much longer time frames. Moreover, our measure is far more feasible. Calculating elasticities (see Armington, 1969; Polachek, 1997) is difficult and requires analysts to acquire information about price and production variety, and then use this information to estimate information about *general* elasticities. The comparison above, for example, contains only fifteen countries because the requisite measures were not available more broadly. Our measure, by contrast, can easily be calculated with only annualized trade data at hand. We would argue that the fact that the two measures trend in similar directions, combined with the ease with which our measure can be calculated and its dynamic and global nature, makes revealed comparative advantage an ideal measure of market power.

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