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Sanctions and Signals: How International Sanction Threats Trigger Domestic Protest in Targeted Regimes

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Western powers often turn to international sanctions in order to exert pressure on incumbent governments and signal their support for the opposition. Yet whether, and through what mechanisms, sanctions trigger protest remains unclear. We argue that sanction threats work as an international stamp of approval for would-be protesters; they encourage collective action against governments. Moreover, sanction threats send particularly clear and coherent signals if multiple senders issue them and if they focus on human rights, which makes such sanctions threats more effective in sparking social unrest. Using count models of protest activity, we find strong support for our arguments. We corroborate our findings with qualitative evidence from the case of Zimbabwe.

Introduction

The Arab Spring revived scholarly and public interest in the drivers of mass protest against governments (see Beinin and Vairel 2013). Both recent developments and past research suggest that external support emboldens protesters (Kriesi 2004; Schock 2004, 142–72). This makes international sanctions a key foreign policy tool for stopping the suppression of internal opposition and, more generally, for instigating democratization abroad (von Soest and Wahman 2015). Yet, almost all studies on social movements and collective action ignore the possible role of sanctions in stimulating protest in targeted regimes (for an exception, see Stephan and Chenoweth 2008).

However, scholars repeatedly identify internal opposition as a key factor influencing the effectiveness of international sanctions. Sanctions may increase previously existing dissent within the targeted regime (Wallensteen 2000, 126; Weiss 1999, 502). They may also create new opposition to the regime by mobilizing dissatisfied—but previously uncommitted and passive—elements of the population (Blanchard and Ripsman 1999; Kaempfer and Lowenberg 1999, 48–51). In a nutshell, protest activity appears to increase in sanctioned regimes (Allen 2008).

The connection between economic sanctions and internal dissent remains puzzling. According to the "punishment theory" (Lektzian and Souva 2007, 850), sanctions' economic harm directly translates into political pressure; deprivation induces citizens to challenge the regime. However, international sanctions also sparked antigovernment protest *before*, or *without*, hurting the targeted country's economy (Arya 2008, 37–38; Blanchard and Ripsman 1999, 245).

In this article, we move beyond deprivation-based explanations of protest in regimes under sanctions. We argue that the messages of regime disapproval—and opposition support—conveyed by sanctions provide the key mechanism through which sanctions encourage protest. Sanction threats create *perceived* opportunities for protesters, because they constitute an international stamp of approval for antiregime activity. We disentangle the signaling dimension of economic sanctions from their economic consequences by focusing on the stage in which outside powers threaten, but have yet to implement, sanctions. If sanctions increase mobilization against the regime via signaling mechanisms, rather than by increasing economic deprivation, then increasing protest activity should occur at this stage.

We combine the new version of the Threats and Imposition of Sanctions (TIES) dataset (Morgan, Bapat,

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and Kobayashi 2014) and the Mass Mobilization dataset (Clark and Regan 2016) to test our expectations via models of protest counts. Our results demonstrate that sanction threats, rather than imposed sanctions, increase the probability of antigovernment protests. In addition, we present a nuanced picture, highlighting the disparate consequences of different types of threats. Sanction threats send particularly credible signals that encourage domestic dissent if they (1) refer to human rights violations or (2) come from multiple senders. Narrative evidence from Zimbabwe illustrates the causal mechanisms postulated in our theoretical argument and confirms the findings of the statistical analysis. Sanction threats against President Mugabe increased the *perceived* opportunity for voicing dissent, thereby driving a wave of protests even in the face of likely repression.

Overall, our theoretical approach integrates work on social movements, particularly antigovernment protest, with sanctions scholarship. In doing so, we move away from the predominant focus on state actors that currently prevails in sanctions research. Our analysis also provides new insights into the broader question—in both international relations and comparative politics research—of how external factors influence the domestic politics of states (Gourevitch 1978; Levitsky and Way 2006).

The Sanctions–Protest Nexus

Traditional theories of protest suggest that a sense of (relative) deprivation motivates citizens to lash out at the government (Gurr 1970). Accordingly, socioeconomic downturns relate positively to the mobilization of dissent (see Urdal and Hoelscher 2012). This approach suggests that sanctions prompt protest activity by inflicting economic hardship on the population. The targeted society acquiesces to the deprivation caused by economic sanctions only up to a certain threshold, above which it reacts with antigovernment protest (Kerr and Gaisford 1994). Numerous sanctions scholars, therefore, use the economic pain inflicted upon the target to predict the effectiveness of international sanctions (Drury 1998; Hufbaueret al. 2007).

Yet such deprivation-based explanations fail to fully account for the link between sanctions and domestic protest. Overall, economic dislocation following economic sanctions rarely correlates with antigovernment protest (Allen 2008). Sanctions that do not affect the society as a whole—for example, financial restrictions targeted at the leadership—appear to inspire antigovernment behavior to the same extent as more painful measures (Selden 1999). Moreover, sanctions sparked antigovernment protest *prior to or without* hurting the targeted country economically. For example, student demonstrations in Nepal spiraled into large-scale antigovernment protest before India's economic sanctions could impose hardship on the population (Blanchard and Ripsman 1999). Similarly, sanctions against South Africa gave heart to anti-apartheid activists even though the regime mitigated their financial impact (Arya 2008; Crawford and Klotz 1999).

Our theoretical argument builds on an existing strand of literature that links third-party advocacy to domestic dynamics of contention. In addition to creating deprivation, sanctions send signals that may influence citizens' decisions to engage in collective action. Signals from third parties can drastically alter organizers' calculations of the challenges to and opportunities for staging a protest at home. Several case studies indicate that signals conveyed by sanctions encourage dissent (see Baldwin 1985, 193; Crawford and Klotz 1999). Research on international human rights nongovernmental organizations (NGOs) shows that these organizations create attention and offer support for protesters (Risse and Ropp 1999). As a result, NGO activities increase domestic antigovernment protest (Murdie and Bhasin 2011).

Our argument differs from these studies. Instead of examining tangible commitments to a domestic population, we contend that signals of outside support provided by sanctions often suffice to increase domestic protest. For example, US sanctions against the Dominican Republic under Trujillo encouraged the internal opposition to intensify their fight against the authoritarian government (Kirshner 1997, 56). In Rhodesia, the resistance movement likewise felt that sanctions validated their fight against the Ian Smith government (Baldwin 1985, 193). At the most basic level, international sanctions communicate the sending states' disapproval of targeted governments' policies, implying support for the opposition. The US and European sanctions on Burma in 2007 following the military government's brutal repression of large-scale demonstrations, for instance, constituted "a show of international support for the protesters and a strong symbolic condemnation of the regime" (Wood 2008, 489). Sanctions reassure the opposition, even though they may not lead to concrete interventions on behalf of the opposition. The perceived possibility of success or the usefulness of their activity influences citizens' decision to engage in collective action (Ginkel and Smith 1999). Sanctions influence these perceived opportunities for voicing and enacting dissent in two ways.

First, sanctions show that the international community takes note of a certain situation. International signals of support for domestic dissent draw attention to norm-violating states. This process empowers and mobilizes protesters by establishing that an international audience exists for their demands (Risse and Sikkink 1999). "Movement activists are media junkies" who care deeply about media reporting (Gamson 1995, 85). News coverage shapes their perception of opportunities and risks. International human rights NGOs often actively seek the attention of foreign powers to support their cause (Murdie and Peksen 2014). Such attention works as a "stamp of approval," suggesting to (potential) protesters that the international community supports the antiregime activity.

Second, international sanctions enable opposition movements to mobilize domestically, because they point to a potential ally. Sanctions signal to opposition groups that other states seek policy change from the target (Peksen and Drury 2010). Antiregime activists regularly interpret third-party advocacy as providing additional legitimacy for their demands and activity (Della Porta and Diani 2006, 223; Schock 2004; Stephan and Chenoweth 2008). International sanctions that convey signals of disapproval of the regime and support for the opposition lead dissident groups to see themselves as more powerful and leaders as more vulnerable. As a result, they influence dissenters' strategic calculations. We contend that external support strengthens potential protesters in regimes under sanctions and reinforces their morale (Arya 2008; Hovi, Huseby, and Sprinz 2005; Nossal 1989).

Perceptions of opportunity created by sanction signals may even trigger collective protest in the absence of structural conditions favorable to collective action (see Peksen and Drury 2010). For instance, citizens took to the streets in Iran in 1977, because they saw greater opportunities for successful protest than before, even though the Shah regime was in no way more vulnerable (Kurzman 1996). For signals of opposition support to reach the (potential) protesters, however, the targeted regimes must be characterized by a minimum degree of political openness (Meyer and Minkoff 2004) and, particularly, by freedom of the press (Osa and Corduneanu-Huci 2003).

International sanctions shock domestic politics, fundamentally changing the potential protesters' perceived room to maneuver. As described above, this shift in perceived opportunity could stem from the material impact of sanctions, or from the intangible signal of outside support to the opposition. Empirically, disentangling the signaling impact of sanctions from their potential deprivation dimension is difficult. Threatened sanctions, though, offer a means of exploring the effect of messages in isolation from deprivation. Threats, by their nature, send a signal without imposing immediate hardship on the broader population.

In contrast, imposed sanctions may trigger countervailing processes. By inflicting hardship on the society as a whole, they can prompt a "rally-'round-the-flag effect," if the ruling elite manages to stoke nationalist feelings (Allen 2005; Galtung 1967; Wood 2008). This effect increases with the financial damage inflicted upon civilians (Tostensen and Bull 2002, 376). In addition, the ruling elite may prevent key constituencies from voicing dissent by shifting economic pressure from support groups to political opponents (inter alia Escribà-Folch and Wright 2010). Former president Milosevic employed such a strategy in Yugoslavia in the 1990s when he made access to sanction rents contingent on support for the regime (Woodward 1995). Finally, regimes often use sanctions as scapegoats for political and economic problems (Allen 2005; Nincic 2005). This strategy undermines opposition mobilization against the status quo. Rally-'round-the-flag effects, the strategic reallocation of resources to the disadvantage of opposition actors, and blame shifting counteract the signals sent by sanctions. Thus, we argue that, in contrast to sanction threats, imposed economic sanctions generally do not trigger increased collective action against governments.

This discussion leads to the following testable hypotheses:

H1: *Threats of sanctions increase protest activity.*

H2: *Imposed sanctions do not increase protest activity.*

Disaggregating Sanction Threats

Thus far we have discussed economic sanctions as a uniform tool of policy. Sanctions, however, differ widely in their goals and implementation. We draw upon research on the signaling dimension of sanctions (Giumelli 2011; Crawford and Klotz 1999), as well as on framing and social movements (Benford and Snow 2000), to identify heterogeneity in the mobilizing power of sanctions signals. The clarity, coherence, and credibility of the threat affect the strength of sanctions' "communication factor" (Doxey 1972, 535) and shape behavioral outcomes (Peterson 2013).

As Western nations increased their advocacy for democracy after the end of the Cold War, an increased readiness to react to human rights violations globally followed (Jentleson 2000). Sanctions that explicitly aim to improve the level of human rights protection may also open up the potential for increased political rights through democratization (Davenport and Armstrong 2004; von Soest and Wahman 2015). The general effectiveness of such human rights sanctions is contested (Drury and Li 2006; Peksen 2009). They can, nevertheless, convey particularly strong signs of disapproval, because they explicitly demand changes to state–society relations in targeted regimes. Measures specifically issued because of human rights violations provide clearer signals for domestic protesters, as they correspond to demands for rights and protections.[1] They relate more coherently to protest goals, and clearly express a salient cause.

Second, we expect sanction threats to exert a stronger impact if multiple senders issue them. Policymakers and scholars alike suggest that larger coalitions of states send particularly strong and credible signals to targeted regimes (Bapat and Morgan 2009, 1075; Martin 1993, 431). This contrasts with earlier findings, using the Hufbauer et al. (2007) dataset, that unilateral sanctions are more likely to succeed than multilateral ones (Miers and Morgan 2002). Other studies conclude that the bargaining and enforcement advantages of international organizations eliminate this counterintuitive finding (Drezner 2000; Drury 1998). Newer research establishes that sanctions success becomes more likely as international cooperation increases, even without institutionalization (Bapat and Morgan 2009; McLean and Whang 2010).

Thus, we propose two additional hypotheses regarding the effect of different types of economic sanctions:

H3: *Human-rights-related sanction threats are more likely to increase protest activity than threats connected to other demands.*

H4: *Multi-sender sanction threats are more likely to increase protest activity than other sanction threats.*

Research Design

We construct a dataset of country-months from 1990 to 2005. It includes information about ongoing and new sanctions from the Threat and Imposition of Sanctions (TIES) dataset, version 4 (Morgan, Bapat, and Kobayashi 2014). For the occurrence of protest activity, we consult the Mass Mobilization dataset, phase 2 (Clark and Regan 2016). This new resource contains information on antigovernment protests and regime responses in Latin America, the Middle East and North Africa, Asia, and the Americas (excluding the United States).[2] The dataset holds distinct advantages over other, commonly used datasets on antigovernment protest. It provides more detailed information at a finer temporal grain than the Cross-National Time-Series Data Archive (Banks and Wilson 2012). Moreover, its spatial coverage is broader than the Social Conflict in Africa Dataset (Salehyan et al. 2012). The case illustration on Zimbabwe presented later shows that the mechanisms identified in the quantitative analysis also apply to other world regions. The data allow us to establish the temporal ordering so vital to our claim that threats prompt domestic responses. Below, we discuss the construction of the base dataset and the dependent variable, before turning to the independent and control variables.

[1]On the related issue of democratic sanctions, see von Soest and Wahman (2015).

[2]This data includes any event with 50 or more people who make a demand against the state, either peacefully or using violence. Armed resistance (for example, a rebel attack) is not included.

Modeling Change in Protest Activity

Our dataset leverages the most fine-grained information available. This constitutes a contribution to the study of the domestic consequences of foreign intervention. Previous work on sanctions and civil dissent utilized protest data aggregated to the annual level. This limitation required scholars both to discount any protests occurring in the same year as a sanctions imposition and to assume an enduring ability of sanctions to provoke domestic unrest. Furthermore, the studies could not account for the potential effect of the threat stage on domestic unrest. Our fine-grained data strategy sidesteps these issues by offering a better defense against four threats to validity: measurement error, simultaneity bias, spuriousness, and endogeneity.

It is unlikely that every protest event observed in our sample stems from sanction threats and/or the imposition of sanctions. To count every protest in the month or year of a threat would risk introducing a substantial amount of measurement error and simultaneity bias. We guard against both threats by carefully lining up the dates of sanctions with the dates of reported protest events. First, we converted the TIES and the Mass Mobilization datasets to episode-months. Second, we coded whether protest events took place before or after a new threat or imposition of sanctions occurred. Third, we allowed only protests that *followed* the threat or imposition to contribute to counts of protest activity when aggregating this data to country-months. For example, if a country threatened another one with sanctions on November 15, we did not allow a protest event from November 10 to count toward our calculation of change in protest level in November. An event on November 17, however, would count. In sum, carefully lining up extremely fine-grained data enables us to develop a model that captures more precisely whether protest events follow sanction threats.

This strategy also allows us to guard against potential spuriousness in our analyses. An episode of political violence may cause *both* the threat of sanctions and an increase in protest activity. If common, this coincidence could pose a threat to the validity of inference regarding the effect of sanctions on protest increases. The relative paucity of fine-grained data on human rights violations, political scandals, and other potential confounders makes this a tricky issue to control for. However, we employ the best available variables: an indicator for recent atrocities taking place within the country (Ulfelder and Schrodt 2009), as well as an indicator for increased use of violence against protesters in the previous month (Clark and Regan 2016). Atrocities and violence against protesters exemplify the kind of events that trigger both protests and sanctions. Controlling for these events minimizes the risk of spurious correlation.[3]

In addition, our dataset provides a much better guard against confounding variables than more temporally aggregated data. Consider, for example, a generic cause of spuriousness between sanctions (X) and protest increase (Y), which we will call Z. This factor, Z, is positively, but imperfectly, related both to sanction threats and to protests. Our data, then, will contain some observations where Z and Y occur with X, where the problem of spuriousness is best illustrated. Because the international community's response rate to Z is imperfect, the data also contain many instances where Z and Y occur absent X. Recall that our measure of Y discounts all protests that occur prior to X. This practice essentially advantages such cases: if Z causes protest increase, then cases with $Z = 1$ and $X = 0$ should have higher values of Y overall than cases where $Z = 1$ and $X = 1$. Unless the international community responds very quickly and nearly every time that Z occurs, our empirical strategy sets up a very hard test for the effect of X. This reduces concerns about spurious causation in our analysis.

Related to this, endogeneity is a concern if the value of Y in a previous time period affects the probability of sanctioning. Specifically, a round of protests itself, or the regime's reaction to it, could prompt a threat of sanctions. A positive relationship between threats of sanctions and protest activity could partially stem from serial autocorrelation in protest levels rather than from the theorized signaling dynamic. However, our strategy substantially limits this possibility in two ways compared to annualized data. Working with monthly, rather than yearly, data introduces more variation in the dependent variable, which makes it less sticky over time. The practice of discounting protests that occur prior to the threat or imposition creates an additional disruption in any autocorrelation process.[4] To further assuage concerns, we also present a model with a lagged dependent variable.

Our hypotheses focus on the probability of increases in protest activity in response to sanction threats. We tap into this concept using familiar count-model techniques to capture variation in protest levels between sanctioned and unsanctioned observations as well as *within* cases over time. The monthly counts of protest that we employ include a large proportion of zeros, as well as some units where an antigovernment protest of 50 people or more—the criterion for the Mass Mobilization dataset's coding of a protest event—never occurs. The count-model framework suggests two methods for dealing with unit heterogeneity and excess zeros in count data: zero-inflated models and fixed effects estimators. Zero-inflated models estimate the probability of a case falling into the all-zero population as well as the effect of covariates on the expected count of protests. Fixed effects estimators, in contrast, leverage only the within-case variation. They drop any countries that lack variation in count over time and allow each remaining unit its own unique intercept. We report results from both models.[5]

Independent Variables: Threat and Imposition of Sanctions

We expect that threatened sanctions will affect the behavior of protesters differently than imposed sanctions. Threats rarely fall upon states not already under imposed

[3]Some recent papers utilize a measure of media attention to human rights abuses as a control for spuriousness by way of identifying the visibility of a regime's bad behavior (Nielsen 2013; Peksen, Peterson, and Drury 2014). We much prefer to control for recent use of violence and atrocities, for two reasons. First, as both of our data sources use media searches to identify their events, these occurrences are, by necessity, visible both internally and externally. Second, the available media attention variables code yearly variation in coverage of human rights violations in the United States only. A measure that would be constant across every one of our observations in every year could perform better as a guard against spuriousness than indicators that capture variation in regime behavior from month to month. To capture another reasonable dimension of the visibility of regime behavior, though, we also include measures of media freedom in the potential target states.

[4]See the online appendix for residual plots assessing the extent of any remaining autocorrelation.

[5]In the online appendix, we also present results from a bivariate probit model with an equation for increase and decrease in protests. This method, which produces results consistent with those of our main models, allows us to entertain the possibility that covariates' ability to provoke protest is not symmetrical with their potential to stifle protest. See Table A17 and Figure A9.

sanctions from a different party or regarding a different issue. We code not only whether a new threat or a new imposition takes place, but also whether other threats or imposed sanctions are currently ongoing for each country-month.[6] This produces indicator variables for *new threat, new imposition, other threats ongoing*, and *other impositions ongoing*.

As a foreign policy tool, economic sanctions address target behavior across a wide range of issues, from militarization and alliance behavior to tariff levels and environmental protections. Sanctions also take on different forms, from the freezing of individuals' assets to comprehensive trade embargos (Morgan, Bapat, and Kobayashi 2014). Finally, different types of senders threaten (and implement) sanctions. We account for this heterogeneity across sanction episodes in three ways. First, to test H3 and H4, we control for human rights and multi-sender sanctions. Indicators tag episodes that emerge over human rights violations in the target state, or in cases with more than one sender. Second, we separate sanctions that might hurt the general population from those that target elites (Tostensen and Bull 2002; Brooks 2002; Lektzian and Souva 2007). The variable *targeted* denotes asset freezes and travel bans. These indicators should be understood as interactions with the *other ongoing* variables. They switch on only in cases of ongoing imposition or threat and thus allow sanctions episodes involving multiple senders, human rights, or targeted measures to differ in effect. Too few observations exist for separate effects to be reliably estimated across types of new impositions/threats.

The third strategy we employ acknowledges the asserted difference between trade-related sanctions—or what Pape (1997) labels "trade wars"—and other types of sanctions (see Morgan, Bapat, and Krustev 2009). Similarly, sanctions leveled over environmental protection could have less relevance for domestic contestation. We code a second slate of indicators for sanctioning activity that exclude any new or ongoing sanctions about either trade or environmental protection. These cases are identified using the TIES issue variables.

Controls

We estimate models with a slate of controls that may affect antigovernment protest. First, we tap into the opportunity for protest and coordination via measures of regime type and information availability. Previous research shows that democracies, in general, are more likely to concede (inter alia Allen 2005). Targeted sanctions such as asset freeze and travel ban may work better against authoritarian leaders (Brooks 2002; Lektzian and Souva 2007). Even more importantly, the predicted likelihood of demonstrations after the imposition of sanctions is smaller for authoritarian and democratic regimes than for hybrid regimes (Allen 2005). Accordingly, we model regime type using polity2 and polity2 squared (Marshall, Gurr, and Jaggers 2014). We employ Freedom House's Freedom of the Press Index (FOTP) to proxy information availability (Freedom House 2013), because signaling effectiveness depends on freedom of speech and open media (Choi and James 2007). Specifically, we include indicators for their ratings of "free" and "partly free," leaving "not free" as a reference category. Past studies showed that linkage—understood as the density of ties and cross-border flows—increases the prospects of external support (inter alia Levitsky and Way 2006). Hence, we include a measure of globalization and openness in the form of Dreher, Gaston, and Martens's (2008) Overall Globalization Index. These variables tap into both the plausibility and effectiveness of signaling across national borders and domestic coordination potential.

The cost of protest potentially constrains collective action. State repression has strong repercussions for both protest and sanction threats (inter alia DeNardo 1985). Alternatively, state-sponsored violence against dissidents may radicalize individuals and generate or facilitate large-scale mobilization (Hess and Martin 2006). We measure repression in two ways. First, the Physical Integrity scale controls for general levels of repression in each state year (Cingranelli, Richards, and Clay 2014). Second, we use the Mass Mobilization dataset's state response variable to code indicators that summarize recent trends in the regime's treatment of protesters. Specifically, we calculate the average number of violent or accommodative responses to protest in each month and code whether either type of response increased or decreased relative to the prior month.[7]

Finally, the capacities of citizens to mobilize, and of the regime to suppress such mobilization, must be taken into account. As regards the former, research on protest shows that population size positively impacts mobilization (inter alia Eisinger 1973). In addition, greater state capacity is associated with lower levels of protest (inter alia Cunningham 2013). We include the variables population size and real gross domestic product, both lagged one year and logarithmically transformed (Gleditsch 2002).

Results

Table 1 presents regression results for the monthly count of antigovernment protests across an array of specifications. Column (1) gives the count portion of the zero-inflated negative binomial. For clarity of presentation, the results for the inflate equation can be found in the online appendix. Columns (2) through (4) present coefficients and robust standard errors for fixed effects Poisson models with slight variations.[8] The second model controls for country-fixed effects. The third introduces fixed effects for the years that residuals analysis suggested may be unusual. The fourth model includes a lagged dependent variable, which provides an additional buffer against serial autocorrelation. As such a strategy may not be considered appropriate in the fixed effects Poisson (see Cameron and

[6] Our data contain 62 country-months of threats against states not already under sanction (see online appendix).

[7] The variable for increased violence against protesters was mentioned earlier during our discussion of how the modeling strategy would address the threat of spuriousness.

[8] In contrast to concerns that Poisson models are vulnerable to overdispersion, which may be a problem in this data, Poisson fixed effects estimators with robust standard errors are remarkably robust (see Cameron and Trivedi 2013, 341–57; Wooldridge 2010, 755–58). In particular, the alternative to Poisson in this case would be the Negative Binomial distribution. This distribution, however, cannot be modified effectively for fixed effects estimation without extremely unrealistic assumptions (see Allison and Waterman 2002; Cameron and Trivedi 2013, 357–58; Guimaraes 2008). Readily available estimators for "fixed effects Negative Binomial"—notably Stata's xtnbreg command—are not truly fixed effects estimators (Allison and Waterman 2002; Guimaraes 2008). Given the number of individual countries in our analysis and the relatively short panel, the only viable alternative to the fixed effects Poisson strategy—dummy variable negative binomial regression—may induce an incidental parameters problem. The Poisson is one of the only distributions that is invulnerable to this problem (Cameron and Trivedi 2013, 353–55; c.f. Allison and Waterman 2002; Greene 2004).

Table 1. Regression Results for Monthly Count of Antigovernment Protest

	(1) Zero-inflated negative binomial	*(2) Fixed effects (FE) Poisson*	*(3) FE Poisson w/time dummies*	*(4) FE Poisson w/lagged DV*
New threat	**0.465*****	**0.206****	**0.199****	**0.205****
	(0.134)	(0.0851)	(0.0871)	(0.0831)
New imposition	−0.034	−0.097	−0.090	−0.064
	(0.107)	(0.126)	(0.122)	(0.114)
Human rights sanction	−0.585**	0.188	0.217	0.172
	(0.296)	(0.241)	(0.234)	(0.239)
Targeted sanction	−0.072	−0.014	−0.002	−0.030
	(0.137)	(0.107)	(0.095)	(0.107)
Multisender sanction	0.147	0.678***	0.658***	0.663***
	(0.182)	(0.181)	(0.162)	(0.179)
Other threats ongoing	0.847***	0.361*	0.333*	0.341*
	(0.166)	(0.191)	(0.182)	(0.185)
Other impositions ongoing	0.434*	0.581	0.542	0.582
	(0.228)	(0.388)	(0.394)	(0.386)
Overall globalization$_{(t-12)}$	0.041***	0.077***	0.072***	0.069***
	(0.006)	(0.019)	(0.017)	(0.021)
Polity2	0.035	−0.145	−0.121	−0.113
	(0.048)	(0.101)	(0.086)	(0.092)
Polity2 squared	−0.002	0.006	0.005	0.004
	(0.002)	(0.005)	(0.004)	(0.004)
Physical integrity	0.055	0.006	0.002	−0.000
	(0.046)	(0.038)	(0.031)	(0.038)
FOTP Free	0.657***	0.179	0.0355	0.166
	(0.199)	(0.161)	(0.147)	(0.154)
FOTP Partly free	0.123	0.200	0.0945	0.184
	(0.107)	(0.177)	(0.172)	(0.168)
Logged population$_{(t-12)}$	0.563***	−0.669	−1.295	−0.384
	(0.0702)	(1.321)	(1.509)	(1.319)
Atrocity$_{(t-1)}$	−0.243*	−0.051	−0.083	−0.045
	(0.125)	(0.047)	(0.051)	(0.047)
Increased state violence$_{(t-1)}$	−0.03	−0.001	−0.015	−0.034
	(0.092)	(0.081)	(0.067)	(0.082)
Increased state accommodation$_{(t-1)}$	0.071	−0.093	−0.091	−0.091
	(0.102)	(0.093)	(0.087)	(0.086)
Decreased state violence$_{(t-1)}$	0.161	−0.015	−0.051	−0.028
	(0.107)	(0.144)	(0.130)	(0.137)
Decreased state accommodation$_{(t-1)}$	0.264**	0.116	0.102	0.121
	(0.134)	(0.180)	(0.176)	(0.181)
Antigovernment protests$_{(t-1)}$	0.015*			0.012*
	(0.008)			(0.006)
Constant	−8.607***			
	(0.688)			
Ln/Alpha	0.806***			
	(0.172)			
Combined coefficients				
Other threats + Human rights	0.263	0.550*	0.550*	0.513*
	(0.329)	(0.307)	(0.300)	(0.306)
Other threats + Targeted	0.776***	0.348*	0.331	0.311
	(0.245)	(0.202)	(0.197)	(0.200)
Other threats + Multisender	0.995***	1.04***	.991***	1.005***
	(0.251)	(0.255)	(0.239)	(0.25)
Other imposed + Human rights	−0.151	0.770	0.758	0.754
	(0.477)	(0.470)	(0.471)	(0.468)
Other imposed + Targeted	0.362	0.568	0.539	0.552
	(0.308)	(0.383)	(0.385)	(0.38)
Other imposed + Multisender	0.581*	1.26***	1.200***	1.245***
	(0.301)	(0.424)	(0.419)	(0.42)
Observations	12204	7968	7968	7968
Countries	73	47	47	47
−2 Loglikelihood	−6922	−12453	−12365	−12405
Model χ^2	729.3	1098	1478	3428

Note: In all cases, reported standard errors in parentheses are robust and clustered on country. The unit of observation is the country-month. Fixed effects specifications necessarily omit all cases in which there is no variation in the dependent variable. The subscript $t-12$ indicates, for variables only available in yearly increments, a lag of one year; $t-1$ indicates a lag of one month. Combined coefficients are calculated using Stata's lincom utility, which obtains standard error estimates via the Delta Method. See online appendix for further specifications and reporting, including: inflate equation from the zero-inflated negative binomial, coefficients from dummy variables on selected years in Model 3, and a dummy-variable Poisson for intercept estimates by country. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

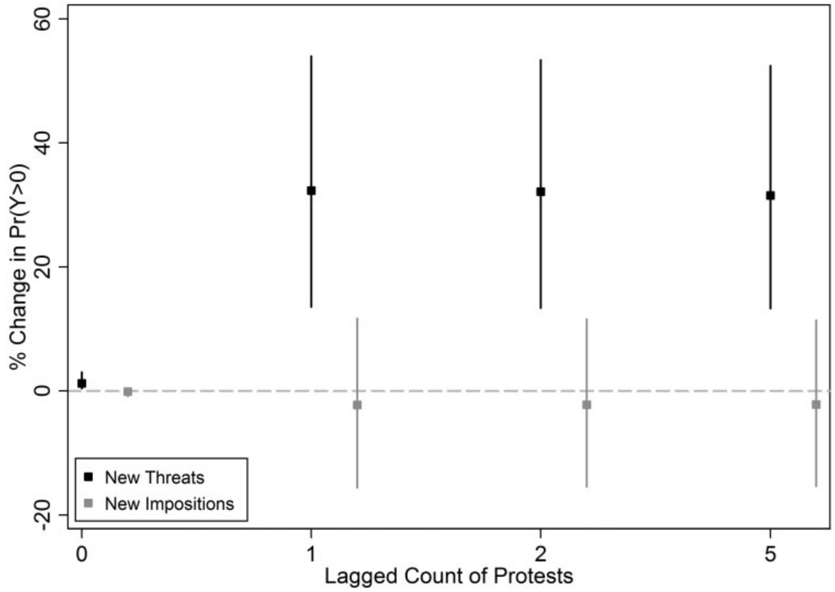

Figure 1. Relative differences in predicted probability of nonzero count of antigovernment protests by new sanctions and previous levels

Note: Spikes provide 95 percent credible intervals from simulated sampling distribution of 100,000 draws from the parameter matrices of the negative binomial reported as Model (1) in Table 1. The relative difference is the $(\Pr(Y > 0|\text{sanction}) - \Pr(Y > 0| \sim \text{sanction}))/\Pr(Y > 0| \sim \text{sanction})$.

Trivedi 2013, 368–75), we consider the results of Model (3) the most reliable.

The results remain remarkably similar across these three specifications. None of our included covariates are time invariant, but many move very rarely. When restricted to within-case variation (i.e., in the fixed effects models), few of the control variables achieve statistical significance. The globalization indicator is positively related to protest and statistically significant, which confirms the importance of cross-border ties (Levitsky and Way 2006). The inconsistent performance of all measures of state repression—physical integrity score, atrocity, and the monthly changes in violence—squares with previous research, which indicates that repression may either inhibit or encourage mobilization.[9] Population size and free media exert large positive effects in the zero-inflated framework. However, they are extremely inefficient in Models (2) through (4), where their impact is restricted to describe the effect of changes within a country.[10] This is a general feature of fixed effects models, where loss of data from comparisons between units of analysis is abandoned in order to increase confidence in control of unit heterogeneity.

Despite the variation in modeling assumptions, our key explanatory variable, new threats, performs as hypothesized in all specifications. As suggested in H1 and H2, threats of sanctions, not newly imposed sanctions, increase antigovernment protests. All else being equal, a new threat increases the incident rate of protests by 59 percent, compared to a case without threats looming.[11]

The relative difference in the probability of a nonzero count for cases experiencing new threats or new impositions versus those without new sanctions or threats provides another intuitively appealing confirmation of H1 and H2. Figure 1 presents the differences in probability across a range of possible counts in the previous month. The spikes in Figure 1 span the range between the 2.5th and 97.5th value in a simulated sampling distribution of 100,000 estimated differences in probability. Black spikes give the effect for new threats; gray, for new impositions.[12] New threats exert a significantly positive effect, while new imposed sanctions do not. The probability of a nonzero count of protests increases by 1.22 percent even if the prior month featured no such activity. If a single protest occurred in the previous month, the effect of threats is much larger, increasing the probability of some dissent by over 32 percent.

In Models (2) through (4), which eliminate between-unit differences, new threats still exert a positive and significant effect. According to Model (3), a target hit with a new threat will experience a 22 percent greater incidence of dissent, all else constant. Even this very conservative test supports H1 and H2. The findings regarding ongoing sanctions episodes also support our signaling argument: ongoing threats may also contribute to protest mobilization. Though the results in the fixed effects models do not reach the 5 percent threshold, the coefficient for ongoing threats is consistently sized across the specifications. In Model (1), which allows differences between units to contribute to the effect, ongoing threats appear to be very destabilizing for the regime. These findings suggest that

[9]More details on the role of regime type are available in the Robustness Check section below.

[10]See Table A1 in the online appendix for descriptive statistics, including the within and between variance of each covariate.

[11]We calculated this quantity from Model (1). It is obtained by a simple transformation of the coefficient into an incident rate ratio: $(e^{\beta} - 1) \times 100$. The average marginal effect of new threats in this model is 0.40 ($p < .001$). The marginal effect in this, as in most nonlinear models, depends on the values of all other covariates. This figure was obtained via Stata's margins command with the dydx() option, which averages over all the predicted marginal effects and provides standard errors via the Delta Method.

[12]We obtained these figures and confidence bounds through a simulation procedure, which took 100 draws of 1,000 vectors of coefficients from the variance–covariance matrix of the zero-inflated negative binomial.

Table 2. Regression Results for Monthly Count of Antigovernment Protest, Censoring Sanctions Episodes from Trade and Environmental Disputes

	(5) *Zero-inflated negative binomial*	*(6)* *Fixed effects (FE) Poisson*	*(7)* *FE Poisson w/time dummies*	*(8)* *FE Poisson w/lagged DV*
New threat	**0.372*****	**0.208*****	**0.195*****	**0.205*****
	(0.109)	(0.063)	(0.063)	(0.062)
New imposition	0.011	−0.088	−0.078	−0.053
	(0.107)	(0.128)	(0.125)	(0.117)
Human rights sanction	−0.565*	0.580	0.592	0.551
	(0.296)	(0.394)	(0.389)	(0.386)

Note: Full results available in the online appendix, Tables A13 and A14. ***p<0.01, **p<0.05, *p<0.10.

there may be some over-time trends in the effect of threats, which should be investigated in future work.[13]

We include several variables to capture likely sources of variation in the effect of sanctions. At the bottom of Table 1 we report the combined coefficients describing the effect of different types of ongoing threats or imposed sanctions, which potentially exert varying impact on protest (narrow sanctions/threats, multilateral sanctions/threats, and human rights sanctions/threats). Since the human rights and multi-sender indicators switch on only if there is an ongoing sanction or threat, they represent interaction terms, allowing sanctions with these characteristics to affect the outcome differently. The combined coefficients evaluate the conditional hypotheses about types of sanctions/threats. The most consistent finding across our specifications is the destabilizing effect of sanctions disputes that involve more than one sender. Across all four models, multi-sender threats contribute to a sizeable increase in the count of protests, increasing the conditional mean by about one unit. This result lends strong support to H4, which predicts that multi-sender threats and sanctions produce a bigger effect than unilateral ones, because they indicate a broader base of international support for the opposition. Moreover, the finding that multi-sender sanctions raise the conditional mean by even more than one unit when actually imposed strongly confirms previous research (Bapat and Morgan 2009).

The results with respect to H3 are more mixed. In the zero-inflated specification, the indicator for human rights achieves significance, but with a negative sign. The combined effects, though, do not reach statistical significance. In our fixed effects specifications, ongoing human rights threats exert a positive combined effect, but fall short of the 5 percent confidence level. We believe this pattern of findings makes sense, given that threats over human rights abuses likely apply to those cases where protesters face high barriers to dissent. Thus, in the model that relies partially on between-unit variation, human rights sanctions appear to "wash out," and in the within-unit models, the effect is weakly significant. Overall, we interpret the results as modest evidence in favor of the extra signaling power predicted in H3 for human-rights-related threats and strong support for H4 with regard to multi-sender threats.

Our model also allows targeted sanctions to work differently than others. The results for this variable vary across model specification. This most likely means that some systematic differences between countries faced with targeted versus broad-based sanctions remain uncontrolled for in Model (1). The fixed effects specifications, then, provide a safer inference of the extent to which different types of ongoing sanctions episodes affect protest levels.

Robustness Checks

As a guard against the charge that sanctions over trade and environmental policy differ from others, we estimate our models using an alternative operationalization of sanctions. The results displayed in Table 2 exclude any target that is exclusively sanctioned over trade or environmental issues from the new and ongoing sanctions episodes. Clearly, our key finding—that new threats of sanctions increase protest—remains remarkably robust to this alternative conceptualization. The coefficient estimates for new threats are practically identical in the fixed effects models, and very similar in the zero-inflated model. The only notable change is that ongoing threats and impositions no longer achieve statistical significance.[14] The much larger proportion of ongoing sanctions and threats that were excluded by this coding rule drives this result. Only 17 of the 251 new threats in our sample stemmed from trade and environmental disputes. The proportion of cases excluded for ongoing threats and impositions was much higher, at 31 percent and 35 percent, respectively.

We also investigate the possibility of conditional effects across regime types, because the ability of economic sanctions to trigger domestic unrest could be mediated by the relationship between the regime and the people.[15] In particular, Allen (2008) finds an inverted-U relationship between regime type and protest. Accordingly, we separated our data into three clusters—democracies, anocracies, and autocracies—using conventional cut points of ±6 on the polity2 scale to separate the pure regime types from those in between. We then estimated our fixed effects models in each subclass. Table 3 reports the results.

We uncover a phenomenon similar to, but more pronounced than, the one noted by Allen (2008). New threats markedly increase the incidence of dissent in democracies and anocracies, by 30 percent and 81 percent, respectively.[16] In autocracies, however, we find a

[13]Model (1) also reports a marginally significant finding for ongoing impositions. The complete eradication of this finding in the fixed effects models suggests that it stems from between-case variation, and thus is more vulnerable to spuriousness or endogeneity concerns than is the finding regarding ongoing threats.

[14]The model estimated is identical to that reported in Table 1, but for the rules used to identify economic sanctions. Please see online appendix Tables A13 and A14 for full reporting of this model. We show only the most pertinent coefficients here.

[15]On the varying effectiveness of sanctions in destabilizing different types of leaders, see Licht (2015).

[16]Readers unfamiliar with the economic sanctions data may believe that sanctions against democracies are uncommon, or at least that "high politics" issues will almost never escalate to the point of economic coercion against a democratic target. This is not correct. In our sample, democracies are *more*

Table 3. Fixed Effects Poisson of Count of Antigovernment Protests, Subsample Analysis by Regime Type

	(9) *Democracies*	*(10)* *Anocracies*	*(11)* *Autocracies*
New threat	**0.260****	**0.591*****	**−0.239*****
	(0.109)	(0.148)	(0.030)
New imposition	−0.124	−0.468***	0.556***
	(0.089)	(0.131)	(0.169)
Human rights sanction	−0.508	−0.032	0.506
	(0.294)	(0.350)	(0.327)
Multisender sanction	1.209***	0.255	0.134
	(0.208)	(0.500)	(0.686)
Threats ongoing	0.309	0.329	0.245
	(0.301)	(0.432)	(0.903)
Sanctions ongoing	1.295***	0.552	1.169***
	(0.331)	(0.498)	(0.331)

Note: Please see online appendix, Table A15, for full reporting of results by regime type. Regime types are operationalized using ±6 as cut points on the Polity2 scale. ***p<0.01, **p<0.05, *p<0.10.

significant *decrease* of 21 percent in the protest incident rate following a new threat, all else constant. This finding could stem from the difficulty of the threat being received in fully closed systems (see also Osa and Corduneanu-Huci 2003). It is also consistent with the use of protest-dampening repression (Josua and Edel 2015; Davenport 1995). Sanctions spark less repression against antiregime actors in democracies than in autocracies (Peksen 2009). The finding for new impositions in autocratic regimes defies our expectations, indicating an increase in the incident rate.

Sanction Threats as Signals in the Zimbabwean Case

The sanctions episode in Zimbabwe illustrates how the signals conveyed by sanction threats create perceived opportunities for regime-critical forces, stimulating domestic antigovernment protest. In addition to tracing the causal mechanism, it demonstrates the applicability of the results obtained in the quantitative analysis for a case located in a region not covered by phase 2 of the Mass Mobilization dataset. Sanctions were repeatedly threatened and later imposed on Zimbabwe's long-time ruler Mugabe and key members of the government as well as on the ruling party, ZANU–PF (Zimbabwe African National Union–Patriotic Front), in response to widespread human rights violations.

The US Senate issued a highly publicized threat to impose sanctions and subsequently passed the Zimbabwe Democracy and Recovery Act (ZIDERA) on August 1, 2001. Likewise, the European Union issued various threats regarding so-called restrictive measures on Zimbabwe. That October, a meeting of ministers paved the way to sanctions and subsequently specified that the potential measures would include travel bans for the regime elite and development aid cuts (BBC 2002). The EU implemented sanctions on February 18, 2002.

The sanction threats led to increased antiregime activity. Regime-critical actors, spearheaded by the newly founded Movement for Democratic Change (MDC), welcomed the initial threats and particularly the senders' cautions against further human rights abuses (Chan 2010, 46). Unlike previous opposition parties, the MDC felt that it enjoyed the endorsement of the international community. This signal of support bolstered the cohesion and development of the Zimbabwean opposition forces (Alao 2012). Zimbabwe experienced an almost unprecedented wave of mass action, including a countrywide strike in 2001 and numerous demonstrations in downtown Harare and beyond throughout 2002 (Dansereau 2003).

Opposition actors envisioned international sanctions as part of their "final push" strategy, during which the domestic struggle would be complemented by external pressure.[17] The "opposition saw a glimmer of hope in sanctions"[18] at a time when the rigging of the 2002 presidential elections confirmed that the MDC was not contesting ZANU-PF on a level playing field. Despite widespread government repression, the signals of support sent by repeated sanction threats increased the perceived opportunity for protest. This perception was partly tied to an analogy made by the MDC between its struggle and that of the ANC against apartheid. The MDC believed that international sanctions against South Africa's apartheid regime helped the ANC gain political leverage and would likewise create opportunities for the antiregime struggle in Zimbabwe.[19]

Their nature made the international sanction threats particularly clear and coherent signals to the MDC. First, the senders explicitly referred to antigovernment protesters. The EU voiced "its serious concern about the…*intimidation of political opponents*" (Council of the European Union 2002, emphasis added). US criticism of Mugabe also referred to the hard stance he had taken against the MDC (Alao 2012, 184). In sum, the sanction threats sent the unequivocal signal that the West would not tolerate the shrinking of political space and that it was willing to support the democratic forces inside the country. This message served as an international stamp of approval for the protesters. The MDC stressed that "the eyes of the international community are still firmly fixed on Mugabe and his *illegitimate* regime [and]…that their violent and corrupt agenda is being documented and reacted to by the wider international community" (allAfrica.com 2002, emphasis added). These signals emboldened the MDC, helping drive the wave of protests against Mugabe despite serious threats of physical harm to individual protesters.

Second, these threats were issued by multiple senders rather than just stemming from the former colonial power Great Britain, in which case they could have been easily discredited. Instead, threats to implement targeted measures were conveyed by the EU in its entirety, the United States, Australia, Canada, and New Zealand (Eriksson 2007).

Conclusion: Sanction Threats as an Inducement to Domestic Protest

This article provides important insights into the connection between international sanctions, a key foreign policy tool, and mass protest within the targeted country. It highlights the role of nongovernment actors in shaping the political effects of sanctions. Most previous work incorporates domestic groups in one of two ways: there are helpless victims who suffer the consequences of international sanctions and domestic repression (for example Allen and

likely to have sanctions ongoing than to be untargeted. If we censor cases exclusively motivated by trade or environmental issues, we still see 35 percent of democratic cases under sanction and 27 percent under threat. Furthermore, our data contain 675 observations of human rights sanctions against democratic countries, including Israel, Haiti, Guatemala, Chile, India, Thailand, and Indonesia.

[17] Interview, Harare, January 20, 2014.

[18] Interview, Harare, January 21, 2014.

[19] Interview, Harare, January 28, 2014.

Lektzian 2013; Weiss 1999), and there are powerful interests already positioned to influence decisions and protect themselves from the fallout (Morgan and Schwebach 1995). Instead, we treat protesters as political actors in their own right. We present original evidence on the general question of how external factors shape domestic politics and, more specifically, dissent within states (Gourevitch 1978; Stephan and Chenoweth 2008).

By signaling disapproval of the incumbent regime and support for the opposition, sanction threats create perceived opportunities that encourage collective dissent; they constitute an international stamp of approval for the protesters. Our results thus support the notion that sanction threats can succeed in inducing political change in the target (Hovi, Huseby, and Sprinz 2005; Whang, McLean, and Kuberski 2013). They also shed light on the prior failure to find statistically significant relationships between sanctions' economic cost to the target and outcomes such as leadership change (Allen 2008; Marinov 2005; Licht 2015).

We show that the effect of sanction threats increases with clarity and consistency. First, sanction threats issued by multiple senders convey stronger signals than those that are only voiced by a single state. Second, we find some evidence that sanctions regarding a regime's human rights violations, rather than some arcane issue of trade law, imply stronger support for those critical of a regime.

Future research should aim for a more detailed understanding of the relationship between political protest in the face of external sanction threats and in the period after sanctions imposition. In this context, we need more information on *which* societal groups protest against targeted governments (see Allen 2008). We might also distinguish between differing protest strategies. Past studies highlight important differences between peaceful and violent protest tactics (Dudouet 2013; Murdie and Bhasin 2011; Stephan and Chenoweth 2008). We combined all types of opposition demonstrations. But scholars and policymakers alike should be interested to learn how external attempts to exercise influence affect the propensity for peaceful, as opposed to violent, reactions inside target states.

Most importantly, scholars need to unpack the dynamic interaction of sanction threats and responses by the opposition. In practical terms, the effects of new threats unfold over time. While some protests occur spontaneously, many require careful planning and organization. This means that would-be protesters on the ground require time to respond to international developments. Dissatisfaction with the regime may also build as the sanctions episode continues without successful settlement, which should potentially further reduce barriers to collective action. These arguments suggest that the medium- and long-term effects of threats vary in accordance with their clarity, coherence, and credibility.

Moreover, our findings provide additional evidence that sanctions undermine targeted governments. They are particularly valuable in the fight against human rights violations abroad. Overall, sanction threats foster action by would-be protesters—even in very adverse circumstances. Policymakers should, therefore, distribute their signal of regime disapproval widely—including through multiple channels, such as social media. Doing so increases the chances of the message reaching key target audiences. In addition, states should redouble efforts to issue sanction threats with a unified voice. Protesters seem to perceive coordinated threats that come from multiple states as particularly credible signals of external support. Our analysis also raises some questions about the efficacy of economic sanctions once they have been imposed. Sending states hoping to drum up domestic opposition may need to take further actions after sanctions have actually been imposed. Domestic civilian organizations may need assistance to operate once their governments begin scapegoating the states that have imposed sanctions and allocating remaining resources to shore up their position.

Supplemental Information

Supplemental information can be found at ISA Online.

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