

“Falling Out of Favor: Economic Sanctions and the Tenure of Leaders” \*

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Abstract: Despite their increasing popularity as tools of statecraft, the effectiveness of economic sanctions remains contested in both the political and academic community. Few attempts have been made to track this spotty track record back to the incentives of targeted leaders through direct statistical analyses of leader survival. With an updated measure of leader failure, which more carefully discriminates between leaders’ reasons for leaving power, the analyses here contradict previous findings. Economic sanctions do not consistently endanger the political prospects of their targets. To the contrary, leaders targeted with economic sanctions or the threat thereof face systematically lower risks of losing office through a mechanism indicative of failure than do those who have not been “punished” by another state or the international community.

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On the first of July, 2010, President Obama signed the Comprehensive Iran Sanctions, Accountability and Divestment Act, renewing and extending U.S. measures to slow down the growth of Iran's nuclear program. At the signing ceremony, President Obama summarized the policy goals of this newest round of trade restrictions:

We're showing the Iranian government that its actions have consequences. And if it persists, the pressure will continue to mount, and its isolation will continue to deepen. ... Finally, even as we increase pressure on the Iranian government, we're sending an unmistakable message that the United States stands with the Iranian people as they seek to exercise their universal rights (Obama 2010).

Sanctions, in other words, will create two sets of costs for the Iranian leadership: first, diminished capacity to achieve their own goals, and second, the provocation of political dissatisfaction in the population. This standpoint could be considered the conventional wisdom of optimists regarding the effectiveness of economic sanctions. By singling out the regime leadership and inflicting costs, often via domestic publics' willingness and ability to inflict punishment, foreign policy strategies like economic sanctions can bring about changes in policy and/or leadership (e.g. McGillivray and Smith 2000; McGillivray and Stam 2004; Marinov 2005; Allen 2008a).

Yet the perhaps more conventional wisdom is that these "optimists" are exactly that: overly optimistic.<sup>1</sup> The impact of economic sanctions on leaders' ability to stay in power constitutes the foundation of arguments on both sides of the sanction effectiveness debate (e.g. McGillivray and Smith 2000; Galtung 1967). Recent arguments contend that the accountability of democratic leaders to broad swathes of the population will make sanctions more politically painful than they would be for nondemocratic leaders (e.g. McGillivray and Smith 2000; McGillivray and Stam 2004; Allen 2008a, 2008b). Some even expect that authoritarian leaders may *benefit* from the distortions created in their embattled economies (Kaempfer, Lowenberg and Mertens 2004; Wood 2008).

Understanding the conditions under which substantive results may be achieved through this policy tool requires a better understanding of the incentives of targeted leaders. I address this need

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<sup>1</sup> Two of Robert Pape's most widely read contributions on the subject are titled "Why Sanctions Do Not Work" and "Why Sanctions Still Do Not Work" (Pape 1997, 1998).

through several theoretical and empirical advancements over recent analyses. First, my theory expands the concept of institutional differences to encompass the opposing dynamics of leader risk inside democratic and nondemocratic regimes. Rather than treating democracy/authoritarianism as creating constant incentives over the course of leaders' careers, I expect the patterns in risk of losing office over time to vary across regime type and likewise to produce different dynamics in the conditional impact of institutions on sanction effects.

Second, we require a sensitive measure of what we may reasonably assume most of these leaders are fighting to avoid: *failure*. The most widely known direct analysis of leader stability and economic sanctions utilizes a basic dichotomous variable: each leader either does/does not lose office in the given year (see Marinov 2004).<sup>2</sup> A multitude of circumstances give rise to this series of zeros and ones, and only a fraction of these correspond to leader destabilization. "False positives" (i.e. concluding that sanctions destabilize leaders when they do not) thus become very likely, particularly in the case of sanctioned democratic leaders, whose time in office is inherently limited. To guard against over-estimating the impact of sanctions, I have coded a measure of leader failure which differentiates between leaders peacefully replaced by challengers versus heirs/protégés.<sup>3</sup>

Finally, the analysis and theory herein acknowledge the inherently strategic nature of sanction targeting. The formal literature has long argued that analyses of economic sanctions face a major selection problem, as those sanctions most likely to be effective are probably never imposed.<sup>4</sup> Moreover, if a primary mechanism of sanction success is the ignition of public dissent, then two propositions may follow. First, threats themselves could be destabilizing as the population interprets these negative signals

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<sup>2</sup> In fact, I am aware of only two direct tests of the impact of sanctions on leader survival: that of Marinov (2004) and a piece recently published by Escriba-Folch and Wright (2010) which looks exclusively at nondemocratic leaders. This second piece does test for differences across "regular" vs. "irregular" exits, finding consistently significant effects only for regular exits. Rather than alleviating the issue I am raising, this finding highlights it: if sanctions are only affecting loss of office through regular means then the existence of "false positives" is particularly problematic.

<sup>3</sup> This data builds on the Archigos v2.9 dataset, adding detail to the exits of leaders which were ruled to be "regular" (i.e. legal) by the investigators (see Goemans, Gleditsch and Chiozza 2009). The coding will be described in more detail below.

<sup>4</sup> A few examples of work utilizing this premise: Hovi, Huesby and Sprinz (2005); Lacy and Niou (2004); Drezner (1999, 2003); Morgan and Meiers (1999); Morgan and Schwebach (1997); Smith (1996)

from the international community as a policy failure. Second, potential sending states may take existing levels of dissent into account when choosing which policy to utilize in their influence attempt. A leader who is already unpopular may be viewed as more easily destabilized through threats and imposed sanctions than one who enjoys high levels of approval. A related point may be illustrated by the relative speed and unanimity with which the UNSC acted to impose freezes on Muammar Qaddafi's assets at the end of February. When human rights violations correspond with mass protests, democratic states will likely see economic sanctions as an obvious policy choice. I address these theoretical issues and the potential threats they pose to successful empirical analyses by jointly modeling the processes of sanction targeting and leader failure. I also utilize the newly collected Threat and Imposition of Economic Sanctions (TIES) data, which, as the name suggests, identify threats as well as enacted sanctions (Morgan, Krustev and Bapat 2009). This data allowed me to construct a dataset of 4,409 leader years from 1971-2004, including 885 individual leaders 39% of which were either threatened or sanctioned in the given year.

The more cautious coding of leader turnover, tenure dynamics, and selection effects undertaken herein reverse earlier findings. Economic sanctions do not destabilize leaders. They are systematically aimed at leaders experiencing elevated domestic instability, but their independent impact is insullatory. Threats and imposed sanctions significantly decrease the likelihood of being replaced by a challenger for both democratic and authoritarian leaders across the range of their tenure in office. The effect is substantively notable, with democrats' risks reduced by 82% and authoritarians' by 95%.

While the mechanism behind this benefit for sanctioned leaders may/may not be the rallying effect proposed by Pape (1997, 1998) these findings should put a damper on our faith in the likelihood of populations adopting external assessments of their governments' legitimacy. Establishing the insulation provided by economic sanctions, interestingly, does not mean that they can never function as an effective policy tool. Leaders do sometimes give in to sanctions, and they are more likely (it appears) to do so at the threat stage (e.g. Drezner 2000). Their reasons for doing so, however, may be quite different than we generally believe. If leaders do not fear destabilization as a result of sanctions, and in fact, have reason to

believe they will become safer if targeted, what makes them concede? Future theories of sanction effectiveness must focus in on more complex dynamics: choosing to give policy concessions cannot be the product of a simple, single-dimension, cost-benefit analysis.

Below, my argument proceeds in five parts. I will first provide a brief review of pertinent literature. Then, I will present my theoretical framework and hypotheses. This will be followed by a discussion of the dataset and modeling choices. The results of the statistical analysis will be displayed and analyzed in the fourth section. Finally, I will provide concluding remarks about the implications of these results for the analysis of economic sanctions, leader survival and foreign policy effectiveness.

### ***Sanctions and Leader Survival in the Literature***

Economic sanctions, most commonly defined as the threatened or actual restriction of normal economic relations by one or more states in the interest of altering or reversing the behavior of another state (Hufbauer, Schott and Elliott 1990), have long held a place in the repertoire of foreign policy strategists. They are a classic example of the exercise of power in the international system, and, due to their perceived lower cost in human terms, a particular favorite among democratic states (Cox and Drury 2006; Hart 2000). Sanctions may be levied for a variety of reasons: the desire to alter terms of trade or economic regulations (Drezner 2003), outrage over human rights abuses (e.g. sanctions against the Burmese government), security concerns (e.g. U.S. sanctions against Pakistan following testing of nuclear weapons), or the desire to destabilize “rogue” regimes (e.g. sanctions against Saddam Hussein’s Iraq, Kim Jong Il’s North Korea, and Qaddafi’s Libya prior to 2003).

The most basic interpretation of the sanctions process holds that the restriction of economic relations imposes a cost on the targeted state. Deprived of goods or assistance, the target must give in to the demands of the sending state(s) in order to get out from under the sanctions. Consistent with this simple logic, the economic impact of sanctions on the target have been frequently noted as significantly increasing both probability of success and magnitude of concessions (Allen 2008b; Nooruddin 2002;

Drezner 2003; Hufbauer et al. 1990). Cost, however, falls far short of a perfect predictor for either aspect of sender victory in the sanctions “game”.

The agent-specific punishment model forwarded by McGillivray and Smith (2000) suggests that a different kind of cost may be the missing link: “Agents that violate international norms not only expose their nation to the wrath of others but also lose their jobs in the process” (811). If threats and punishments from the outside hold leaders rather than peoples responsible, then international disapproval is easily reversed by the sacking of the leadership. The ability of domestic publics to fire the offending leaders, however, must be conditioned by the costs of doing so, which are necessarily higher when there is no established, peaceful mechanism of turnover. The agent-specific punishment approach anticipates differential effects of sanctions across regime types, with democratic publics more able and willing to appropriately punish their leaders. This knowledge should prompt leaders vulnerable to punishment to avoid incurring international disapproval (McGillivray and Smith 2000).

The public choice perspective, exemplified by the work of Kaempfer and Lowenberg (2000), also acknowledges the importance of politics inside the targeted state. Despite the generally depressive effects of sanctions on an economy, the distortions created by cutting off normal trade can produce profitable opportunities for narrow groups within the target state (i.e. “sanctions rents”). The potential payoffs increase with the severity of the distortion in the overall economy (Kaempfer and Lowenberg 1999). Depending on the breadth of the sanction and the strength of the opposition in the target state, the government may be able to use these sanction rents to pay off loyalists or to enhance its own power by developing monopolies (Kaempfer and Lowenberg 1999, 2000). Both these theoretical orientations, then, point us toward analysis of the domestic political incentives of targeted leaders.

Cutting edge research picks up on this trail. Marinov (2005) emphasizes the micro-story underlying the effectiveness of sanctions: if leaders are career-driven, they will not give in to politically costly concessions from outsiders unless failing to do so hurts their ability to stay in power. Sanctions, if they are likely ever to work, must be capable of destabilizing leaders. The empirical investigation uncovers first a universal destabilization and then a conditional finding, wherein democratic leaders are

more destabilized than nondemocratic leaders. Spurred on by these findings, Allen (2008a) travels further down the causal chain to assess whether domestic upheaval follows sanction onset. Consistent with the pattern for leaders' risks, she finds riots and other violent expressions of discontent increase dramatically only in at least partially democratic targets.

Together the findings regarding leader stability and domestic turmoil create a compelling underpinning for the broadly established tendency of democratic targets to make bigger concessions and to concede more often (see Allen 2008a; Cox and Drury 2006; Nooruddin 2002; Bolks and Al-Sawayel 2000). In the face of domestic outrage for the international punishment, democratic leaders, reliant upon a majority of the public for their place in power, have little choice but to make the concessions necessary to mollify the sanctioning community.

Autocratic leaders, according to Bolks and Al-Sawayel (2000), enjoy a broader set of possible strategies. Kaempfer, Lowenberg and Mertens (2004) hypothesize a complex relationship between sanctions and political incentives in autocratic regimes. Depending on the strength and orientation of the opposition, sanctions may either increase or decrease the relative "prices" of repressing the people versus purchasing loyalty; some dictators, therefore, will gain and others will lose power following onset of sanctions. Wood (2008) argues instead that dictators will always fear destabilization in the face of sanctions, and that they will act to counter this potential by increasing repression; he finds sanctioned nondemocratic leaders significantly likely to more severely repress their people. Allen's (2008b) finding of significantly increasing variance in sanction outcome as the target's regime becomes less democratic could be consistent with either account, as repression itself sometimes results in further instability.<sup>5</sup> The careful analysis carried out by Escriba-Folch and Wright (2010) assesses the impact of foreign pressure on different authoritarian survival strategies. Unlike militaristic and single-party states, they find that personalist leaders may be destabilized by economic sanctions due to their over-reliance on private-goods provision and inability to effectively implement strategies of co-optation and repression.

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<sup>5</sup> The massive protests events spreading across northern African states in the winter of 2011 underscore this observation.

While findings appear to be compiling smoothly in this area, some snags persist. The simple story that democratic leaders concede for fear of losing their jobs should be complemented by evidence that democratic leaders ousted during a sanction episode are replaced by leaders more willing to proffer concessions. Quite to the contrary, it appears democratic turnover has no effect at all on the likelihood of sanction termination, while autocratic turnover hastens it (McGillivray and Stam 2004). If robust, this pattern throws serious doubt on the simple domestic sensitivity argument: if sanctioned democratic leaders are being thrown out for their failure to maintain international favor, why do their successors maintain the same policies?

Before concluding that we have found the answer regarding one of the most basic causal mechanisms behind sanctions' potential success, we must dig deeper into domestic political incentives and the data utilized to assess their impact. Most democratic shifts in power occur through means generally considered to be peaceful and legal, which usually occur at regularly scheduled intervals. Indeed, this is one of the chief benefits of democracy. In democratic systems loss of office cannot be safely equated with loss of public support. Without a more satisfactory scheme for measuring leader failures as evident or non-evident of destabilization, we risk over-stating the impact of economic sanctions on leader survival. Essentially, treating every exit as a failure must be counting some instances of leaders who simply had finished their terms as casualties of foreign policy failure. These "false positives" do not simply introduce noise into statistical analyses: they could bias tests *in favor* of the hypothesis that sanctions "work" and should thus be ardently avoided. This issue applies also to the limited range of sanction effectiveness uncovered in the Escriba-Folch and Wright (2010) analyses; when separated by type of exit, the authors found that their findings hold consistently only for exits considered "regular". According to my coding from 1970-2004, 25% of the regular turnovers of personalist leaders placed regime insiders or protégés in power.<sup>6</sup>

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<sup>6</sup> Within this time period, there were 32 "regular" turnovers in personalist regimes, eight of which featured heirs/protégés taking power. These include, for example, the death of Francois Duvalier and instatement of his son, the infamous "Baby Doc" Duvalier in 1971.



Another snag in the research on leader survival and sanctions arises from a key issue in the broader literature. If economic sanctions promise to inflict costs which cannot be outweighed by the benefits of maintaining status quo policies and they are likely to be levied, then a rational target should concede ahead of time. Likewise, if economic sanctions will not inflict enough damage to prompt concession, then a rational sender will back down before incurring any of the costs associated with disrupting economic relations (Hovi, Huseby and Sprinz 2005; Lacy and Niou 2000; Drezner 1999, 2003; Morgan and Meiers 1999; Morgan and Schwebach 1997; Smith 1996). The likelihood of selection processes holds important implications for analyses of the impact of sanctions on leaders' survival. There may be systematic differences between cases which terminate at the threat stage and those which advance to imposed sanctions, but differences between those threatened in the first place and those not may also exist.

#### ***Domestic Political Incentives and International Disapproval***

The agent-specific punishment and public choice theories direct our attention to the changes in domestic political incentives caused by foreign influence attempts. Success depends upon the domestic institutions through which the policy must filter. Severe economic sanctions, for example, may aim to destroy the targeted leader. But, if they succeed, the actual hand upon that leader's throat will be that of a compatriot not of the foreign government. We must ask not just how the influence attempt is *supposed* to work, but also how the institutions of the target are likely to insulate or expose the leader to the process.

Like many of the key theoretical approaches in international relations, I begin with the dual assumptions that leaders are beholden to some portion of the population for their continued tenure in office, and that institutions of representation determine the relative size of that "winning coalition" vis-à-vis the rest of the population (see Bueno de Mesquita, Smith, Siverson and Morrow 2005; Kaempfer and Lowenberg 2000; McGillivray and Smith 2000; McGillivray and Stam 2004). The broad expectation regarding the higher sensitivity of democratic, or large winning coalition leaders, to economic sanctions –

a tool known to produce broad societal “bads” – was presented in the literature review above.<sup>7</sup> The fairly mainstream hypothesis regarding economic sanctions effectiveness, then, would be simply:

*H1: Economic sanctions will be more likely to destabilize democratic, or large winning coalition, leaders than nondemocratic, or small winning coalition leaders.*

Beneath the overall conditioning effects of institutions, temporal dynamics may factor into the sensitivity of leaders to external intervention (see Wright 2008; Licht 2010). Leaders’ risks of failure do not remain constant over time. To utilize the popular framework of selectorate theory (Bueno de Mesquita et al. 2003), winning coalitions do not spring into existence as essentially cohesive and loyal groups of supporters and then suddenly cease to exist when leaders fail. Support and loyalty within even small cadres takes time, takes work. A democratic leader engages in this work over the course of a campaign, which culminates in a jubilant effusion of support not just for the new leader, but for the electoral process itself (see Mueller 1973, 211-212; Brody 1992, 27-44; Lockerbie, Borrelli and Hedger 1998; Brace and Hinckley 1992, 21-44 and 60-74). For the democratically elected, holding on to office will never again be as easy as in these first few “honeymoon” months (Warwick 1992; Light 1999, 36; Altman 2000).

The story reverses for leaders who rule without the inherently legitimate process of popular elections. For autocrats the riskiest days are the first. The work of securing patronage networks, buying off the important, and dominating the weak, takes place largely *after* the seizure of power. If successful in maintaining support through the initial instability, however, the job security benefits of the small winning coalition system kick in. Loyalty increases over time as private benefits accumulate and each member becomes certain of their place in the winner’s circle – a place which they are unlikely to maintain should their benefactor be overturned. This overall pattern of decreasing risks over time emerged in many studies of leader survival, due to the heavy proportion of nondemocratic leaders in sample (e.g. Chiozza and Goemans 2004; Bueno de Mesquita and Siverson 1995; Bienen and Van de Walle 1991).

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<sup>7</sup> On the tendency of sanctions to produce costs for the general public, see: Peksen and Drury (2010); Andreas (2005); Hufbauer and Oegg (2003); Joyner (2003); Cortright and Lopez (2002); Garfield (2002); Heine-Ellison (2001); Garfield, Devin and Fausey (1995); Hufbauer et al. (1990).

Even as institutional accountability conditions the ability of publics to punish their leaders for incurring international disapproval, the domestic mood conditions the publics' propensity to interpret international disapproval as a punishable offence. A very popular leader who has been threatened with sanctions, for example, may be perceived at home as a champion of national interests, or as a victim of hypocritical foreign powers. This perception would be more likely to create stability and longevity for the leader. At the other end of the spectrum, a leader whose hold on power is already shaky will be more likely to be undermined by international disapproval. If the winning coalition has already been split by policy failure, the international signal will conform to rather than directly contradict the population's existing opinions regarding the leader's job-performance and legitimacy. This line of thinking produces the first hypothesis regarding the ability of economic sanctions to tap into temporal domestic political dynamics:

*H2: Economic sanctions will be most likely to destabilize leaders during times when their extant political risks are high and vice versa.*

An even more pessimistic prediction for the capacity of sanctions to destabilize their targets would contend that even unpopular leaders are unlikely to be blamed and punished by their people. International intervention may always provide leaders with at least an initial means of unifying public opinion against the external "aggressor" (e.g. Galtung 1967). In democracies this may take as little as the leader framing the sanctions-inducing policies as a matter of central national interest. In autocracies it may mean restructuring the economy or taking advantage of monopolies on information. For either, the hypothesis here would be:

*H3: Economic sanctions will not destabilize and may actually insulate leaders from internal turnover.*

These three potential dynamics in the impact of sanctions on leader "survival" will be analyzed with sensitivity to the fact that sanctions do not spring randomly from the conduct of international politics. Sanctions are foreign policy tools, an essentially strategic phenomenon. Sending states utilize them to alter the policy behavior of others, and are likely to aim them at those they believe to be particularly susceptible to their effects. Formal models of sanction onset and effectiveness often argue

that this strategic selection process creates a sample of observed sanctions which are unlikely to succeed, as the best are those which terminate prior to imposition (Hovi, Huesby and Sprinz 2005; Lacy and Niou 2004; Drezner 1999, 2003; Morgan and Meiers 1999; Morgan and Schwebach 1997; Smith 1996).

The prospect of strategic targeting is particularly important in an analysis of sanctions' impact on leader tenure. Theories of sanction efficacy, and also the statements of those who employ this foreign policy tool, tell us that sanctions should work at least partially through the creation of domestic dissent. If we combine this observation with the fact that these tools are particularly popular for the value-laden foreign policies of democratic states, then we should expect sanctions to be aimed at leaders facing high *ex ante* probabilities of failure.

Leaders who already have only a shaky hold on power should be considerably easier to destabilize than those whose rule is firm and unquestioned. The input of the external actor will confirm rather than contradict what the population (or at least some significant portion thereof) feels about the leader's performance. Credible negative signals about the leadership could empower opposition groups and force compromises or turnover in leadership. The tendency of powerful democracies to utilize sanctions more readily than other types of states (see Cox and Drury 2006; Hart 2000), means they are also often utilized in pursuit of democratic foreign policy goals: protecting against/punishment for human rights abuses, development or utilization of weapons of mass destruction, etc. Large scale, highly publicized human rights violations often occur in concert with internal discontent and instability. The course of events in Libya provides ample illustration of this tendency. If sanctions are meant to punish the crack-downs of governments facing serious internal threats, then they are clearly likely to be aimed at leaders whose risks are already high. Regarding the selection process, then, we may expect:

*H4: Heightened domestic risks of failure will make sanction targeting more likely.*

To test these hypotheses I model targeting and leader failure as endogenous processes with linked errors. Several sticky empirical and theoretical issues have impeded the accumulation of empirical work on economic sanctions, particularly the type of empirical work which aims for a more nuanced approach than the "they work/they do not work" dichotomy. The recent contribution of Morgan, Krustev and

Bapat's (2009) Threat and Imposition of Economic Sanctions (TIES) dataset to the common pool has alleviated two of the most pressing issues – selection effects and the separation of strategic- and trade-based sanctions. Combined with my refined measure of leader turnover, which more carefully designates the change in power of democratic leaders as “failures”, these data will allow for the most refined test of sanctions' impact on leader tenure to date. Below I describe the statistical model as well as the data involved in this test.

### ***Research Design***

My dataset begins with a sample of leaders consistent with TIES's temporal range of 1970-2004. The data include 4,409 leader years and 885 individual leaders. Some of these leaders rose to power through institutions that rewarded principled politics and the provision of public goods; others utilized force and ruthlessness to make it to the top. Similarly, some of them helmed their countries successfully for many years before peaceably retiring, while others crashed and burned in a display of more blood than of glory. And the political ideological goals of some these leaders lived on after their exits through the administrations of hand-picked successors, protégés and political kindred spirits, while others saw their life's work systematically dismantled following a crushing opposition victory. Each leader, in other words, trod a unique route of rise and fall. While we obviously cannot do justice to the variegated experiences of these 885 men and women in a large N statistical analysis, we can be sensitive to major features indicative of their (in)ability to maintain the support of their people up until the end.

To consider the exit of any one of these leaders a failure, we must consider the coincidence of two features of the exit: the means by which the leader leaves office and their political relationship to the successor. The Archigos v2.9 (Goemans, Gleditsch and Chiozza 2009) dataset provides an excellent base to begin collecting this information. Their codings of leader exit, however, tend to homogenize as “regular” means as disparate as term limits, resignation, impeachment and electoral defeat. To capture destabilization of leaders by foreign policy tools, we need a variable which can differentiate between those leaders whose platforms were dismissed and those who simply handed power over to their chosen successor. My data includes this vital bit of information, using party affiliation, historical and media

accounts, and notes in the Archigos v2.9 Codebook to label the relationship between the exiting and entering leaders as one of three types: heir/successor, challenger, or neutral.<sup>8</sup> In this analysis, a leader's legal exit will be counted as a "failure" *only* if the subsequent leader is not of the heir/successor variety. Figure 1 displays the percentage of the types of successors for the legal means of exit. Handing power over to a political protégés constitutes a significant portion of these exits, most commonly about 20% as in the sudden regime breakdowns which have been coded as unclear, resignations, and term limited exits. When leaders die in office, they are replaced by their chosen successors more than 40% of the time.<sup>9</sup> The dependent variable utilized in the leader failure aspect of this analysis then is a binary indicator which equals 1 if turnover is irregular or next leader is not an heir/successor; 0 otherwise.

As noted above, this analysis has two dependent variables. The second outcome of interest is whether/not a leader is targeted with economic sanctions or the threat thereof. To obtain this information, I turn to the TIES dataset. The unit of analysis for this data is the sanctioning event. For my purposes, I have created a simple indicator of whether or not a threat was issued or standing or a sanction was imposed against the current leader in any given year.

I model the processes of targeting and failure jointly using a bivariate probit regression, which assumes that the link between these events can be captured by jointly normally distributed error terms. This technique assumes a relationship between the two events, but does not treat it as a traditional "selection" process. All leaders fail, eventually, not just those who have been targeted by economic sanctions. The trick here is to capture the potentially higher (*ex ante*) risk of politics within sanctioned polities, without setting up a test that will be biased in the direction of finding significant effects of sanctions. The bivariate probit allows us to analyze the potential strategic selection of sanction targets without dismissing the valid comparison of failure probabilities between those that face sanctions and

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<sup>8</sup> Neutral codings usually indicate the emplacement of a "national unity", "technocratic" or transitional government following serious instability. There are significant proportions of neutral codings in other exit types due to a steep threshold for coding "challenger" and "heir/successor". If I was unable to find significant documentation of either a protégé type or opposition type relationship between the exiting and entering leader I coded neutral.

<sup>9</sup> An added benefit to coding this data is that we do not need to automatically reject all cases of natural death, as some 60% of these events actually feature political systems taking advantage of the opportunity to forsake the political agenda of the ailing leader.

those who do not. Within each equation of the bivariate probit, I address the theoretical and statistical needs of the particular analysis. I discuss these each in turn below.

### ***Leader Failure***

In any model of leader failure, duration dependence must be carefully modeled. This is especially true in a pooled model containing many different forms of government with their varied norms of length of tenure. The importance redoubles again when hypotheses directly reference temporal dynamics, as is the case here. If duration dependence itself is not accounted for, we may end up interpreting the results of specification bias as evidence of support for our hypotheses. Interactions of variables with time may end up looking significant simply because the impact of time has not been sufficiently incorporated independent of the variable(s) with which it is interacted. There are two main aims regarding duration dependence in this analysis then: 1) model different duration processes across regime types, 2) enable assessment of changing impacts of covariates over time.

I address these two needs by incorporating separate duration dependence parameters for large and small winning coalition leaders. In autocratic systems, theory and empirics suggest that a monotonic function should be sufficient to model the relationship between time and risk of failure. I utilize the natural log of nondemocratic leaders' cumulative tenure in office to model this in the leader failure equation below.<sup>10</sup> Intuition and empirics should lead us to believe that the relationship between risks and time differ considerably in democratic systems. I use the more flexible cubic polynomial approach for these leaders (Carter and Signorino 2010).

Figure 2 contains Kaplan Meier smoothed estimates of hazard rates across winning coalition size and targeting. These plots largely affirm the use of the monotonic function for small coalition systems and the more complicated function for large coalition systems. There is an upturn in the small winning coalition systems hazard rates at around the 27 year mark, but this applies to quite a small percentage of

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<sup>10</sup> I tested this specification against both the quadratic and cubic polynomial forms of cumulative time in office. Tests of Bayesian Information Criteria indicated that the simpler specification of nondemocratic leaders' duration dependence was superior.

leaders. Bayesian Information Criteria tests suggest that adding in a quadratic term for small winning coalition leaders is not justified.<sup>11</sup> The second major trend illustrated in these plots is the difference in hazard rates and functional forms for targeted leaders across regime types. In the small winning coalition panel, targeted leaders face a consistently higher likelihood of failing at any point in time, but the *form* of their risks is almost perfectly parallel to those of non-targeted leaders of their regime type. To the left, the big winning coalition panel displays a difference in both level of risk and in trends in risk over time by targeting. Early on, targeted democratic leaders face higher risks, but this reverses with the sharp increase in risks for the non-targeted leaders corresponding to most electoral cycles. Targeted leaders look safer than their counterparts for several years, until the slopes of the risks for both types reverses at around the ten year mark.

The nonparametric estimates of trends in risks suggest that the over-time impact of sanctions should also be modeled with different forms. For the nondemocratic set, sanctions seem to be related to a monotonically decreasing risk of losing office, and are thus modeled by a single interaction between threats and sanctions and the natural log of time in office. For democratic cases, I used interactions between the sanction and threat variables and time and time squared to model the changing form over time.<sup>12</sup>

Aside from temporal trends, I also incorporate variables to account for differences in type of sanction, variation in stability within the blocks of small vs. big winning coalition systems, and internal instability. As many have argued that trade disputes differ from actual sanctions events (see for example Pape 1998), I include an indicator for trade-based sanctions using information from the TIES data. I also measured the average credibility of sending states in each year using a transformation of the TIES sender commitment variable. This variable describes the level of specificity with which the primary sending state details the conditions under which threats/imposed sanctions will be removed from weak (1) to

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<sup>11</sup> The difference in Bayesian Information Criteria was  $3425.692 - 3409.691 = 16.002$ , which indicates “very strong” support for the monotonic modeling of duration dependence.

<sup>12</sup> I tested this formulation against the more complicated interaction with the full cubic polynomial. Again, the Bayesian Information Criteria test supported the simpler formulation:  $3425.659 - 3409.691 = 15.968$ .



strong(3). As leaders face multiple sanctions/threats in any given year, I averaged this variable across each year. One might also expect, under the leader specific punishment framework, there to be differences in the impact of sanctions specifically targeted at the current leader versus ones inherited from a predecessor. I mark inherited sanctions/threats with an indicator variable. Finally, along this vein, I include the estimated costs of sanctions using the TIES variables *targetcosts* and *anticipatedtargetcosts*. These are qualitative variables ranging from 1 (low) to 3 (high). I averaged these across all current threats and sanctions, and added the resulting averages together.

To get at different aspects of regime type, I include an indicator for personalist regimes (Geddes 1999), as these are often the least stable type of authoritarian systems and also give the Esriba-Folch and Wright (2010) finding that these types of leaders are susceptible to sanctions. I also use the state party and legislative coalitions variables from the Banks Cross Sectional Time Series data (Banks 2011).

### ***Sanction Targeting***

Serial autocorrelation is a prime concern in this equation. I included lagged variables going from one to ten years, and excluded those which were not significant. The significant lags were those at t-1, t-6 and t-10. I believe this sufficiently handles the tendency for previously targeted leaders to be targeted again. I also included a host of regional variables to account for spatial effects, finding that only a Sub-Saharan African location significantly altered targeted propensity (Hensel and Diehl 2004).

More theoretically, this model needs to account for the H4 argument that higher-risks will be associated with higher probability of targeting. I model this using variables that tap into domestic instability at various levels. The first of these is a civil war indicator, from the Uppsala Conflict Data Program (Harbom and Wallensteen 2010). This obviously taps only very high levels of civil instability. As a measure of less intense situations, I also include the count of anti-government demonstrations from the Banks dataset (Banks 2011).

The remaining variables in this equation tap into factors likely to relate to targeting: anti-system behavior, strategic interests, and power. First, provocative behavior is measured using an indicator for

whether/not the state initiated a revisionist militarized interstate dispute in the current year (Ghosn, Palmer and Bremer 2004). Strategic factors include regime type, measured with the winning coalition scale as operationalized by Bueno de Mesquita et al. (2005) and also an indicator for monarchy (Banks 2011) which taps into the oil-rich monarchies of the Middle East. Total international trade (lagged and logged) indicates the interconnectedness of the state, and thus opportunity for sanctions-worthy events to emerge (Barbieri, Keshk and Pollins 2008). Power is measured by military personnel and expenditures from the National Military Capabilities Version 4 dataset (Singer, Bremer and Stuckey 1972), and also by (logged) population size (Gleditsch 2002).

### ***Results***

Table 1 contains the full results of the bivariate probit model described above. The first half of the table gives coefficient estimates for the leader failure equation, and the second half does the same for the targeting equation. The estimate for the correlation between errors in these two equations is highly significant and positive. The test of independence of equations produces a highly significant  $\chi^2$  statistic, confirming the appropriateness of the joint modeling technique. Leaders with higher risks of losing office are also more likely to be targeted by sanctions.

Hypothesis 4 argued that leaders with domestic troubles would be more likely to be targeted by sending states, as it would be reasonable to expect sanctions to destabilize leaders whose populations already distrust them. The positive and highly significant coefficients for both civil war and anti-government demonstrations lend strong support to this hypothesis. The control variables also take on expected values, with revisionist conflict, trade, military expenditures and population size increasing targeting likelihood. Winning coalition size does not reach significance, however. The only other slightly aberrant result is the small negative coefficient on military personnel, but I believe this reflects the “threat” potential of the military. Very large armies which are poorly equipped do not attract attention in the same way as weapons programs, for example.

To explore the substantive importance of these significant effects, I used a simulation procedure to draw 100,000 sets of  $\hat{\beta}$  taking the estimates in Table 1 as the mean of a joint Normal distribution with variance being the variance-covariance matrix. For each set of coefficients drawn from this simulated distribution, I calculated the relative difference in probability of targeting given high versus low values of each variable. For continuous variables the high value is one standard deviation above the mean; the low, one standard deviation below the mean. During the calculation, all other variables were held at their mean or modal values. The exception to this rule is the lagged targeting variables, which were switched off. Turning of the lags means that these first differences refer to the probability of a *new* sanctioning event against a previously non-targeted leader. Figure 3 displays the median difference and the 2.5<sup>th</sup> and 97.5<sup>th</sup> percentiles from this sample. The results are separated across regime types. For more democratic systems, the results are slightly less significant for demonstrations, civil war and population size. Each of these achieves significance at the 90% threshold, but not 95%.

For both types of leaders, then, Figure 3 displays the strong substantive importance of domestic instability. The risk of targeting increases by 56% and 33% in small and big winning coalition systems respectively, as anti-government demonstrations increase. The impact of civil war is analogous, with senders 52% and 32% more likely to send new threats or sanctions. These impacts are not the biggest in the model. Understandably, revisionist interstate conflict exerts an effect nearly twice the size of domestic instability. Norms in the international community are much more consistent regarding the use of violence against another state as opposed to one's own population. Similarly, high military expenditures, likely related to weapons programs and arms-races increase the chances of targeting. The effect here is so large in the democratic community that including it makes observation of the other effects very difficult.

With the selection hypothesis satisfactorily examined, we can now move on to discuss the more complicated results regarding sanctions' ability to destabilize leaders. The interactions between time, sanctions and regime type make the coefficient estimates at the top of Table 1 highly conditional. The coefficient for large winning coalition systems, for example, refers only to the effect of democratic institutions in the first year in office if no threats or sanctions were levied. Extracting meaningful

information from such a highly interactive specification requires extra work (Brambor, Clark and Golder 2006; Kam and Franzese 2007). Following a simulation procedure like the one described above for Figure 3, I calculated predicted probabilities and first differences across a range of tenure times up to the 98<sup>th</sup> percentile of in-sample durations for each regime type.

These simulations allow me to chart the probability of failure across regime types for leaders who were not targeted with economic sanctions. The predicted probabilities in Figure 4 can be thought of as the baseline risks, from which sending states hope to create elevated likelihood of losing office by imposing threats and sanctions. For more democratic leaders, baseline risks are nonmonotonic – increasing initially but reversing in slope after about five years. If this function was plotted out beyond the 98<sup>th</sup> percentile of in-sample duration times, risks would again turn up around the 20 year mark, as basically every large winning coalition system leader exits prior to this time. For nondemocratic leaders, the logarithmic form has imposed a monotonic function, which took on a negative slope. Consistent with many prior studies, the estimates demonstrate a decreasing risk of losing office over time as autocratic systems solidify.

If sending states hope to prompt policy changes through heightening the domestic risks of targets, their mission has gone awry. Figure 5 charts the difference in probability of failure for leaders facing sanctions, threats, or a combination of the two, holding all other variables at their mean or modal values. Across the range of plausible duration times, economic sanctions and threats only *reduce* the risk of losing office for targeted leaders. Over time, the raw amount of insulation decreases, but this is due to the drop in baseline risks not an increase in effectiveness. Across the length of reasonable tenure durations, the relative drop in risks is fairly consistent. The median estimated decrease begins at 82% for first year democrats and ends at 65% in the 15<sup>th</sup> year. The 2.5<sup>th</sup> percentile in that year, however, is an 87% drop, thus the 95% confidence bounds at the end include the initial median estimate. The same is true for the nondemocratic leaders, though their insulation ranges from 95% to 80%. The hypothesized role of extant temporal dynamics of risk then, do not gain much support from these trends. Indeed, the larger estimated effect in the early years of nondemocratic leaders is exactly the opposite of what we should expect under

H2. These findings could perhaps be interpreted as a sort of perverse support for H1, that democratic leaders are more “vulnerable” to the impact of economic sanctions. However, a 15 point smaller drop in risks does not make a great case for the potential efficacy of sanctions against democrats.

At least initially, then, it appears that the most pessimistic expectations for the efficacy of sanctions hit closest to the general rule. Before giving over entirely to H3, though, we should look into the potential capacity of other contextual factors to increase the ability of sanctions to destabilize. If sending states look for already shaky leaders as a way to increase the chances of success, then we should evaluate the impact of increasing instability on the probability of sanctioned leaders’ failure. In Table 1, civil war and anti-government demonstrations achieved strong positive coefficients in both phases of the analysis. To assess their role in further destabilizing sanctioned leaders, I have calculated the probability of failure for sanctioned leaders at high and low levels of these variables and arrayed the results against the baseline probability of failure for non-sanctioned leaders. For nondemocratic leaders, the heightened level of domestic instability produces no significant shifts in the probability of sanctioned leader failure. The same is not true for democratic leaders, however.

Figure 6 charts the 95% confidence bounds of the probability of failure of democratic leaders under three conditions. The gray shaded band indicates the probability of failure for non-targeted, big winning coalition leaders given a low level of anti-government demonstrations (one standard deviation below the mean level). The black dashed caps give the corresponding probability of failure for either threatened or sanctioned leaders with the same level of demonstrations. The red caps chart the probability of failure for sanctioned or threatened leaders facing higher than average anti-government demonstrations (one standard deviation above the mean. The results here are subtle: the influence of anti-government demonstrations is not large enough to produce a significant difference in the predicted probability of failure for threatened/sanctioned leaders; the 95% confidence bands always overlap. But, the demonstrations do play a role. Compared to a targeted leader with low anti-government activity, those with more unstable domestic conditions become statistically indistinguishable from the baseline between two and four years earlier depending on whether the sanction is actualized. Sanctions and threats may

accidentally provide leaders with the means of rallying their people and making themselves safer than they otherwise would have been, but at least this policy backfire is less severe when the population is already out in the streets protesting the actions of an entrenched elected official.

Civil war exerts a similar influence. Figure 7 displays the same information, but using the low value as no civil war and the high value as civil war. Threats against an administration which is waging large scale violence against some portion of its own population can trigger domestic integration only for “first-term” leaders. After six years in office, the threats provide no insulation. If allowed to escalate to actual sanctions, the range of backfiring policy is longer, up to the 10<sup>th</sup> year of leader tenure. This indicates also, some mild support for the argument that threats should be more effective than the body of sanctions which are imposed.

Figure 8 shows another interesting dynamic in the model. Oftentimes, sanctions extend beyond the term of a single leader. McGillivray and Stam (2004) found, in fact, that democratic leader turnover does not significantly increase the likelihood of sanction termination. My analysis demonstrates a potential reason for this: if it is good politics to continue domestically-motivated policies in the face of international disapproval, then sanctioned democrats are likely to be replaced by their protégés rather than the opposition. These incoming heirs, however, do not benefit as much as their predecessors. Heirs to international disapproval benefit only for about four years from threats and seven from actual sanctions. These ranges include a significant portion of observed survival times for democratic leaders, of course, but the principle is interesting in that it suggests they may be unable to hand down the benefits to the next generation of their political family.

### ***Conclusions***

Despite attempts to leave behind the “they work/they don’t work dichotomy”, in the end, this analysis has come up with a relatively stark conclusion: sanctions and threats do not significantly destabilize targeted leaders. When using even a relatively mild threshold for determining political failure, given that a neutral take-over is not a complete defeat but here is counted as one, we find that our previous empirical conclusions regarding the efficacy of sanctions reverse. The likelihood of being

replaced by a neutral or unfriendly politician decreases substantially when the outside world attempts to shame and punish a leader. While the relatively familiar arguments of the public choice school explain this finding for autocrats, the finding is more surprising for democratic leaders. Perhaps democratic publics are less internationally minded than political scientists have been wont to assume. Whereas the leader-specific punishment model assumes that the public will perceive the costly signals of other countries as evidence of foreign policy incompetence, it seems that Galtung's and Pape's suspicion that integration will follow external punishment may hit closer to home. As democratic policies are designed in the first place to please a large portion of the population, being asked from the outside to change them may mean defining a new policy which will benefit fewer people. Elected officials with a reasonable level of skill should be able to make this clear to their constituents.

The failure of economic sanctions to directly contribute to domestic destabilization does not necessarily mean that they will never "work" in terms of gaining policy concessions, however. While by all accounts, the success rate of sanctions on this front is also low, it is not zero. This becomes a particularly interesting puzzle in light of these findings. If leaders benefit from being targeted, why would they not act so as to maintain their status as champions of the national interest? Perhaps the inefficacy of sanctions on the destabilization front creates an interesting bargaining opportunity, opening the door for issue linkage and negotiation. Or, perhaps, the heirs of sanctions may benefit more from settling these disputes than from continuing them? We know now that sanctions help, but this is not the same as knowing that ending sanctions hurts. In future work I hope to tackle this possibility directly.

Another important line of research will be to travel down from this highly aggregated level to look at individual leaders' responses to being sanctioned. Are leaders expressing their policy positions in terms of national interests, painting sanctions as unjustified foreign intervention? Are leaders ignoring the sanctions? Framing them as technical issues? Blaming them on the opposition? There are many potential strategies for leaders to take, and textual analysis of their political statements may give us insight into the foundations of the general insulation effect uncovered by this statistical study.

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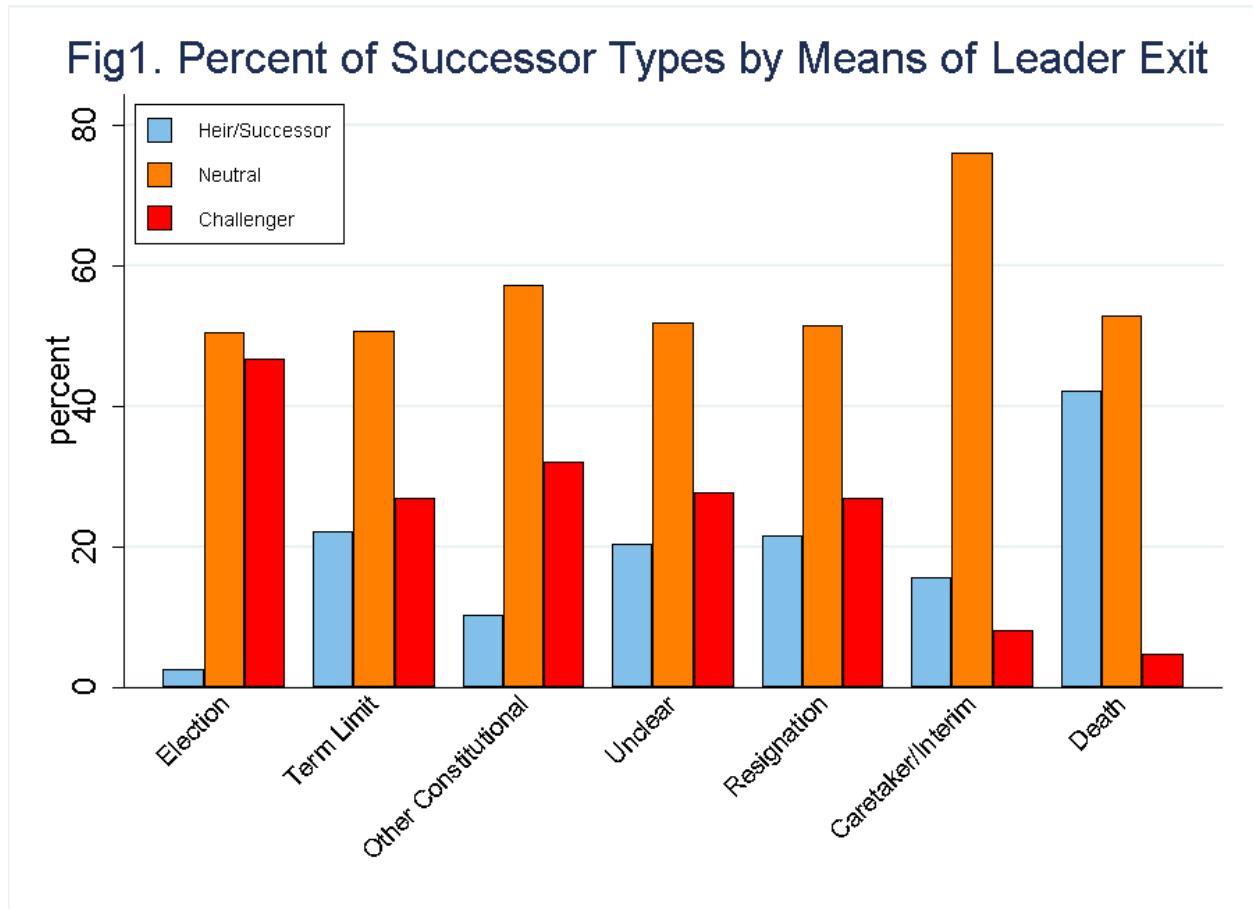


Fig 2. Kaplan Meier Estimates of Smoothed Hazard Rates

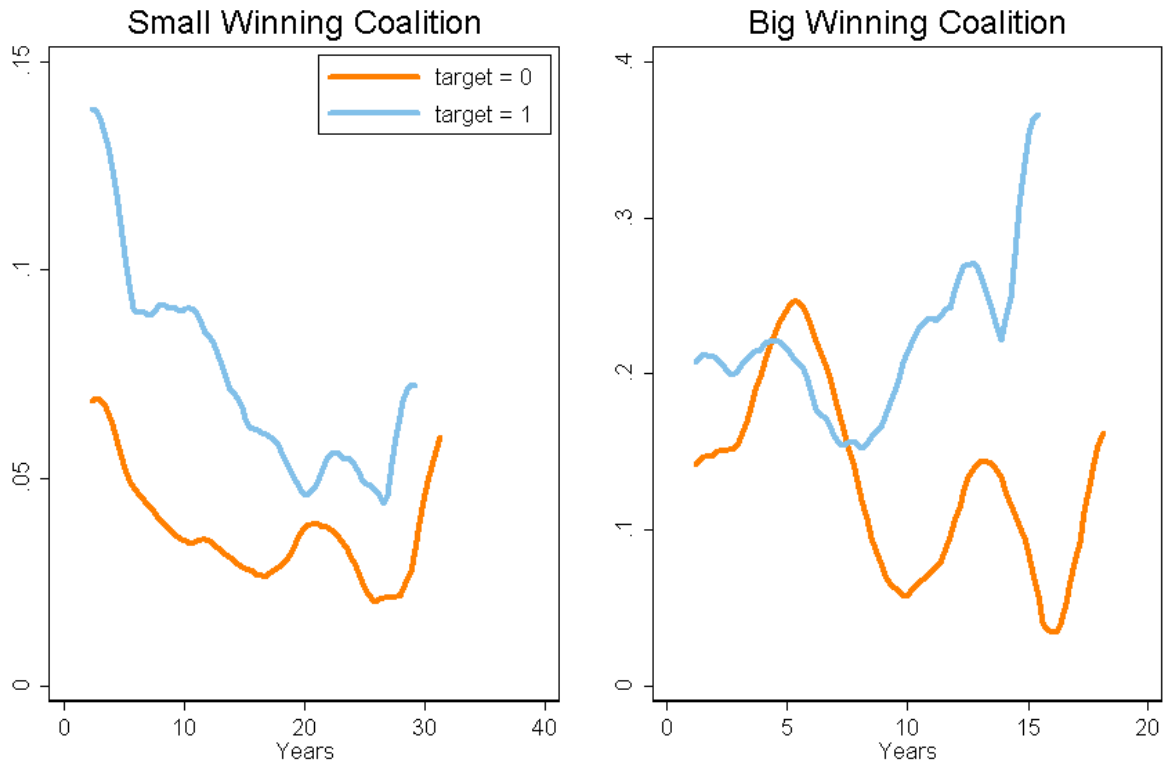


Table. 1 Bivariate Probit Model of Sanction Targeting and Leader Failure

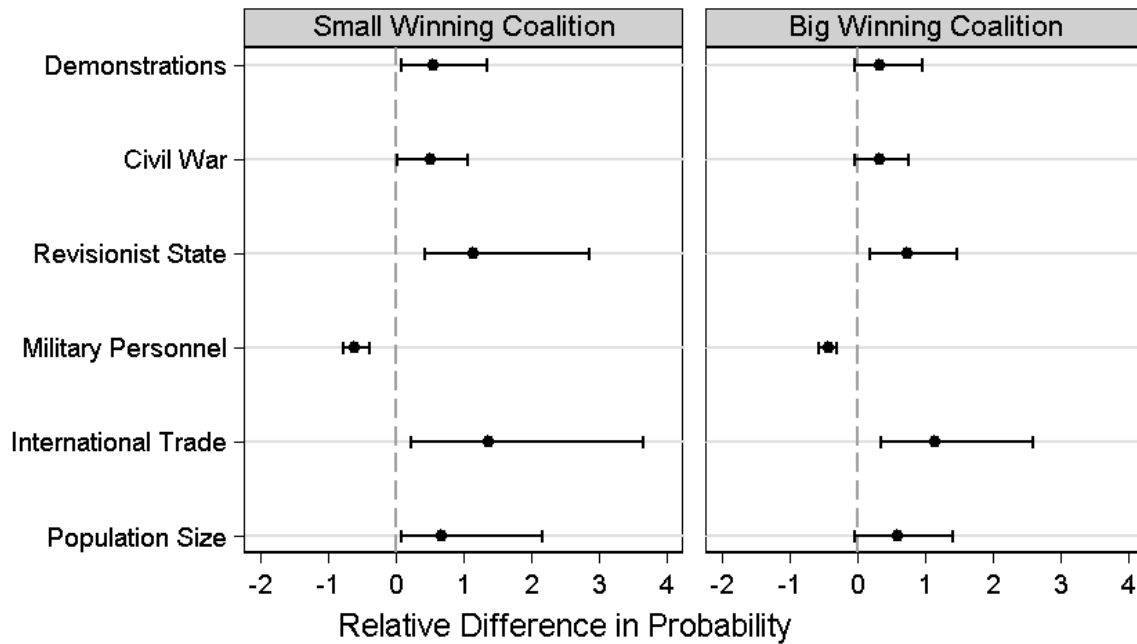
<i>Variables</i>	Coef. (std.error)	95% Confidence Intervals
<i>Sanction</i>	-1.0577*** (0.3732)	-1.7892 - -0.3262
<i>Threat</i>	0.6847 (0.4571)	-0.2113 - 1.5806
<i>Big Winning Coalition</i>	-0.5840** (0.2434)	-1.0612 - -0.1069
<i>Big Winning Coalition Leaders' Time Trend 1(t)</i>	0.2416* (0.1323)	-0.0177 - 0.5009
<i>Big Winning Coalition Leaders' Time Trend 2(t<sup>2</sup>)</i>	-0.1078** (0.0426)	-0.1913 - -0.0243
<i>Big Winning Coalition Leaders' Time Trend 3(t<sup>3</sup>)</i>	0.0099*** (0.0035)	0.0031 - 0.0167
<i>Small Winning Coalition Leaders' Time Trend (lnt)</i>	-0.1571*** (0.0290)	-0.2140 - -0.1002
<i>lnt × Sanction</i>	0.1319*** (0.0505)	0.0329 - 0.2308
<i>lnt × Threat</i>	-0.1055* (0.0638)	-0.2306 - 0.0197
<i>Big Winning Coalition × Sanction</i>	0.0835 (0.2042)	-0.3167 - 0.4838
<i>t × Big Winning Coalition × Sanction</i>	-0.1767 (0.1886)	-0.5464 - 0.1929
<i>t<sup>2</sup> × Big Winning Coalition × Sanction</i>	0.0367 (0.0358)	-0.0336 - 0.1069
<i>Big Winning Coalition × Threat</i>	-0.3967* (0.2099)	-0.8081 - 0.0147
<i>t × Big Winning Coalition × Threat</i>	0.4457*** (0.1538)	0.1442 - 0.7473
<i>t<sup>2</sup> × Big Winning Coalition × Threat</i>	-0.0656*** (0.0181)	-0.1011 - -0.0302
<i>Trade-Based Dispute</i>	0.0658 (0.0848)	-0.1004 - 0.2320
<i>Sender Credibility</i>	-0.0515 (0.1011)	-0.2497 - 0.1468
<i>Inherited Threats or Sanctions from Prior Leader</i>	0.9375*** (0.1053)	0.7312 - 1.1438
<i>Estimated Target Costs from Sanctions</i>	.01365 (.0458)	-.0762 - .1035
<i>Personalist Regime</i>	-0.0334 (0.0865)	-0.2029 - 0.1362
<i>State Party</i>	-0.2983*** (0.0864)	-0.4677 - -0.1290
<i>Legislative Coalition</i>	-0.1418*** (0.0368)	-0.2140 - -0.0696
<i>Civil War</i>	0.1368** (0.0652)	0.0091 - 0.2646
<i>Anti-Government Demonstrations</i>	0.0373*** (0.0128)	0.0123 - 0.0624
<i>Constant</i>	-0.1544 (0.2164)	-0.5786 - 0.2698

Leader Failure

<i>Table 1, Continued</i>		Coef. (std.error)	95% Confidence Intervals
	<i>Civil War</i>	0.1971** (0.0967)	0.0075 - 0.3867
	<i>Anti-Government Demonstrations</i>	0.0581*** (0.0219)	0.0151 - 0.1011
	<i>Revisionist State</i>	0.3521*** (0.1103)	0.1359 - 0.5683
	<i>Winning Coalition Scale</i>	0.1340 (0.1406)	-0.1415 - 0.4096
	<i>Total International Trade (Lagged and Logged)</i>	0.1098*** (0.0378)	0.0358 - 0.1838
<i>Sanction Targeting</i>	<i>Monarchy</i>	-0.2390*** (0.0919)	-0.4191 - -0.0588
	<i>Military Personnel</i>	-0.0005*** (0.0001)	-0.0007 - -0.0002
	<i>Military Expenditures</i>	0.0061*** (0.0019)	0.0024 - 0.0097
	<i>Population (Logged)</i>	0.0802** (0.0391)	0.0036 - 0.1567
	<i>Sub-Saharan Africa</i>	-0.2889*** (0.1103)	-0.5050 - -0.0728
	<i>Target[t-1]</i>	2.9014*** (0.0968)	2.7118 - 3.0911
	<i>Target[t-6]</i>	0.2431** (0.1042)	0.0389 - 0.4473
	<i>Target[t-10]</i>	0.2296** (0.1051)	0.0237 - 0.4356
	<i>Constant</i>	-3.4213*** (0.3337)	-4.0753 - -2.7674
		<i>Ath/Rho</i>	0.2713*** (0.0739)
$\chi^2$ test of independent equations, df=1:			13.311***
Wald $\chi^2$ , df=37			2152***
-2loglikelihood			-2193

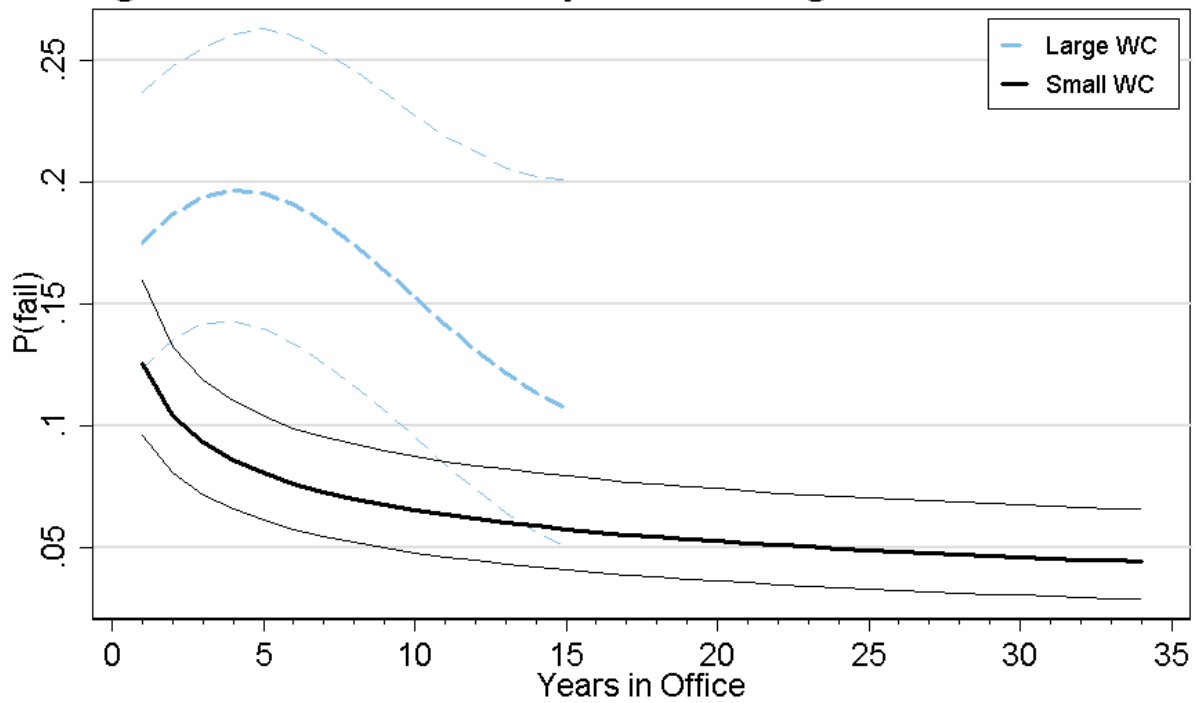
NOTE: N=4,409 leader years, clustered on 885 individual leaders. Robust standard errors in parentheses.  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Figure 3. Difference in Probability of Targeting



NOTE: Horizontal bars indicate 95% confidence interval derived from sample of 100,000 estimates drawn from the variance-covariance matrix of the bivariate probit reported in Table 1. Dots indicate the median relative difference from that simulated sample. For continuous covariates, high and low values were one standard deviation above and below the mean. For indicator and qualitative variables the minimum and maximum (i.e 0 and 1) were used. All other variables were held constant at means or modes for the calculations, with the exceptions of the lagged targeting variables, which were turned off. These probabilities, then, reflect the difference in likelihood of a new target being threatened or sanctioned. The relative difference was calculated as the difference of the probabilities at high and low values divided by the probability at the low value.

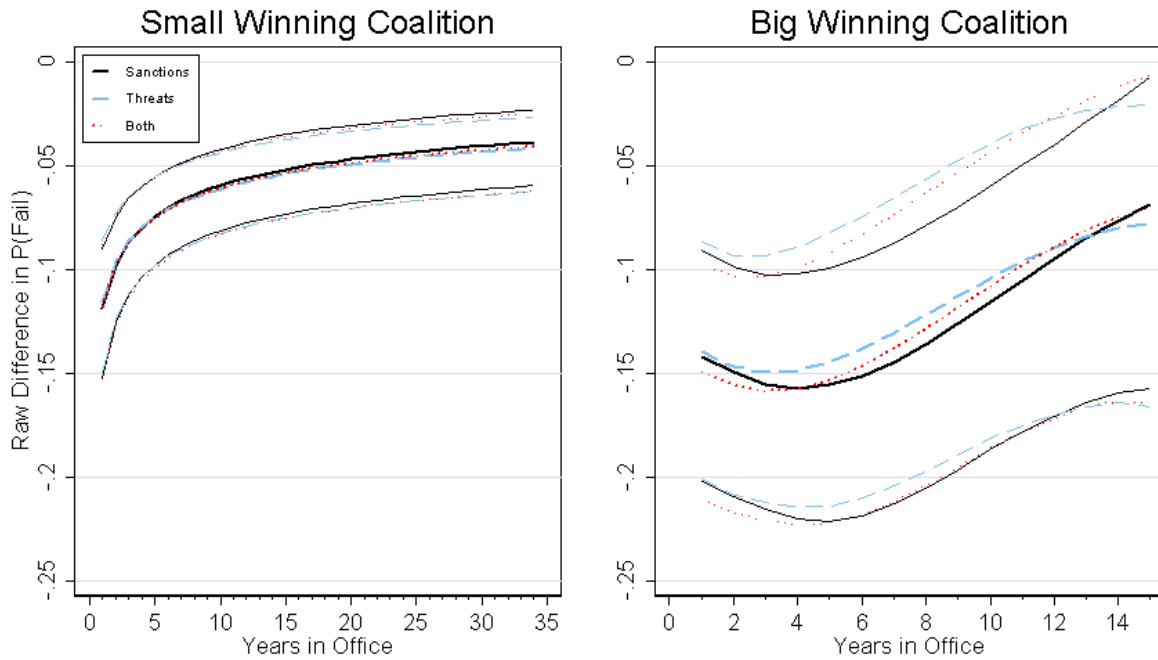
Fig.4 Predicted Probability of Non-Targeted Leader Failure



NOTE: Thick lines indicate median predicted probability of leader failure from a simulated sample of 100,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 1. The trend in probability of failure can be interpreted as the baseline duration dependence in the model. Thin lines of corresponding color and pattern provide the upper and lower bounds of the 95% confidence intervals from the simulated sample. Each curve is plotted up to the 98th percentile of in-sample survival times.

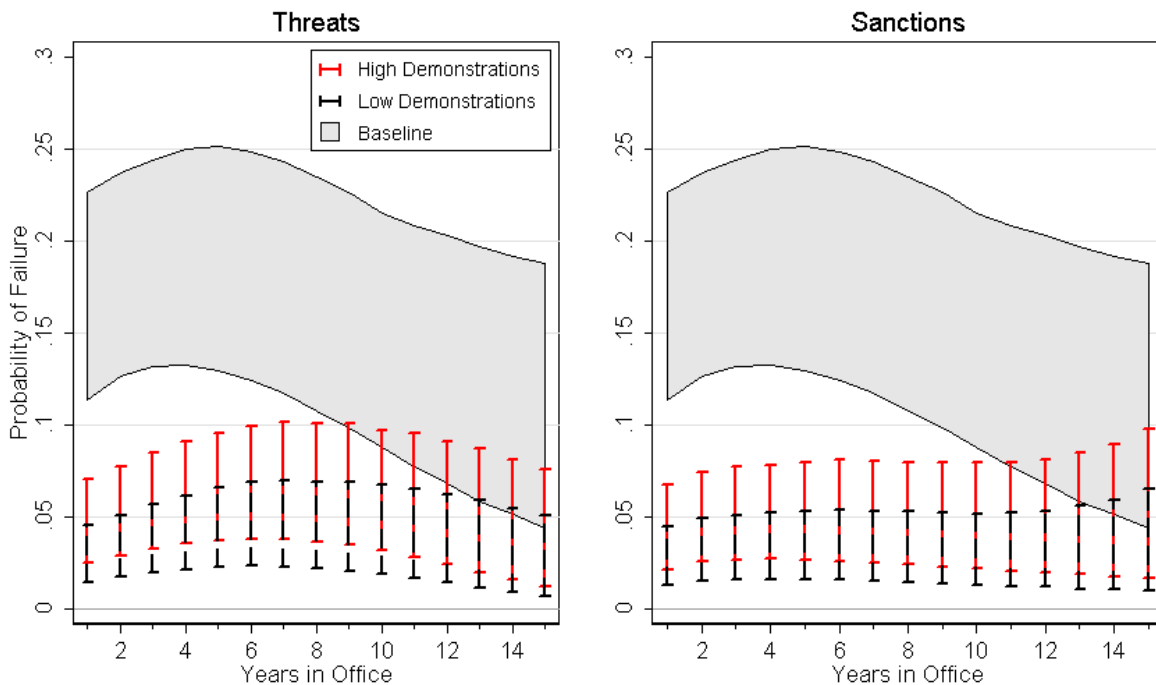


**Fig.5 Difference in Probability of Failure by Sanctioning**



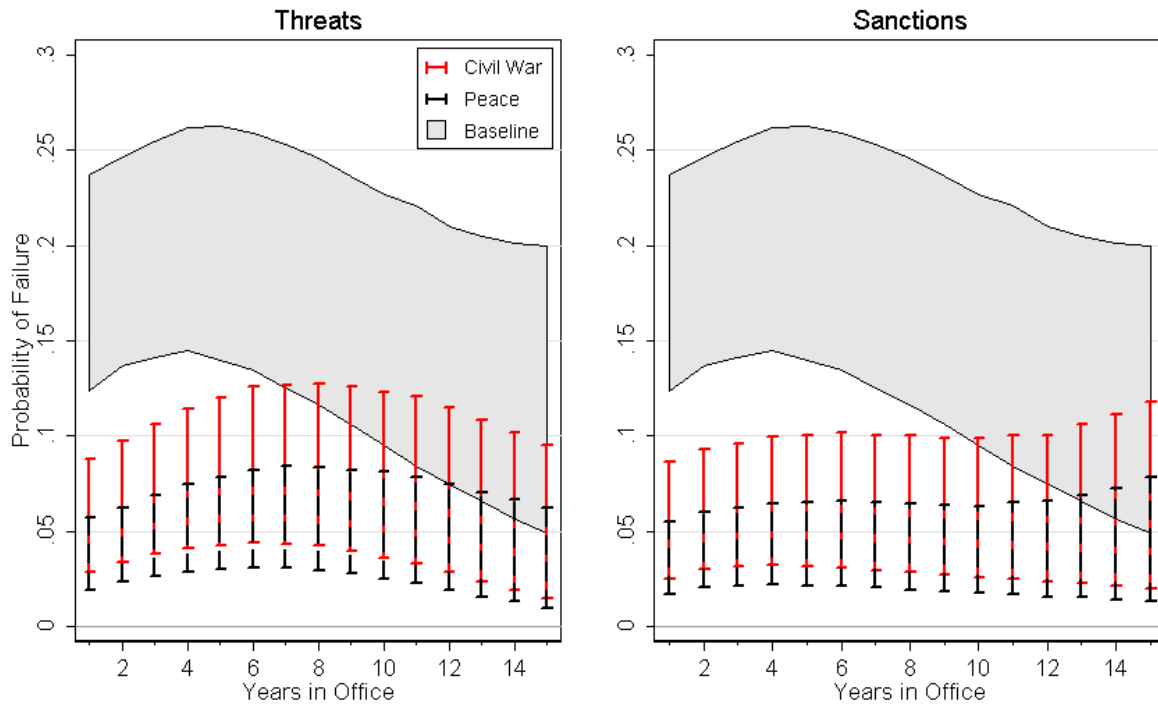
NOTE: Depicted effect is the median predicted relative difference in probability of failure from a sample of 100,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 1. The comparison is between the probability of failure given that the hypothetical leader was either sanctioned or threatened versus not having been targeted. Thin lines of corresponding color and pattern denote the 95% confidence bounds from the simulated sample. All other independent variables were held constant at means or modes as appropriate, with the exception of the indicators for lagged targeting, which were switched off.

Fig.6 Probability of Failure by Anti-Govt. Demonstrations



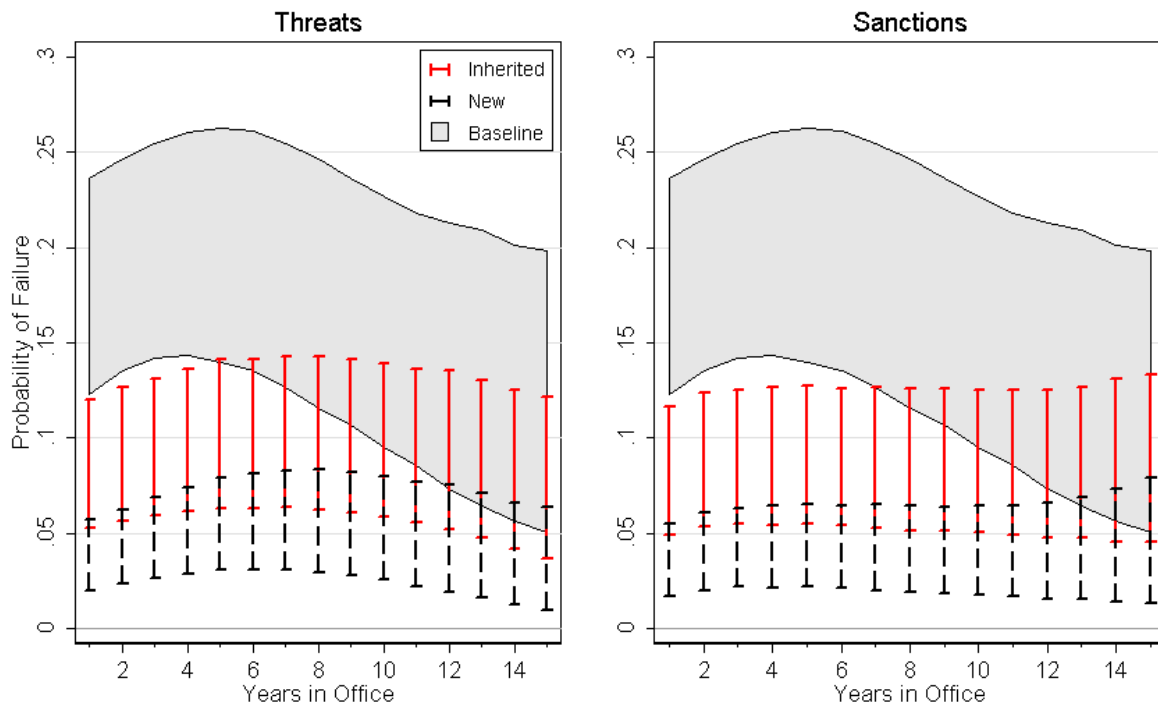
NOTE: Bands indicate the boundaries of the 2.5th and 97.5th percentiles of predicted probability of failure from a simulated sample of 100,000 draws of coefficients from the variance-covariance matrix of the bivariate probit in Table 1. The independent variable is set to its low value of one standard deviation below the mean for the calculation of the baseline probability of non-sanctioned/threatened failure. The high value is one standard deviation above the mean.

Fig.7 Probability of Failure by Civil War



NOTE: Bands indicate the boundaries of the 2.5th and 97.5th percentiles of predicted probability of failure from a simulated sample of 100,000 draws of coefficients from the variance-covariance matrix of the bivariate probit in Table 1. The independent variable is turned off for the calculation of the baseline probability of non-sanctioned/threatened failure.

Fig.8 Probability of Failure by Inheriting Sanction



NOTE: Bands indicate the boundaries of the 2.5th and 97.5th percentiles of predicted probability of failure from a simulated sample of 100,000 draws of coefficients from the variance-covariance matrix of the bivariate probit in Table 1. The independent variable is turned off for the calculation of the baseline probability of non-sanctioned/threatened failure.