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Private incentives, public outcomes: the role of target political incentives in the success of foreign policy

Amanda Abigail Licht
University of Iowa

PRIVATE INCENTIVES, PUBLIC OUTCOMES: THE ROLE OF TARGET POLITICAL INCENTIVES IN FOREIGN POLICY SUCCESS

by

Amanda Abigail Licht

An Abstract

Of a thesis submitted in partial fulfillment
of the requirements for the Doctor of
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Thesis Supervisor: Associate Professor Brian H. Lai

ABSTRACT

When do foreign influence attempts succeed in obtaining concessions from targeted states, and why do they so often fail? Powerful states employ a broad range of foreign policy tools in their dealings with other countries, but their ability to successfully exert power varies. This project seeks an explanation for the patchy record of foreign aid and economic sanctions in the political incentives of targeted leaders. Understanding the process of foreign policy success and failure requires considering both the effect of intervention on leader survival and the domestic cost of providing concessions. In both respects, the type of sanction interacts with targets' domestic context. Dynamic trends in leadership experience and political support, strength of political opposition, and regime type condition both the probability of sanctions' effectively tapping into target incentives and the difficulty of providing concessions.

My framework and analyses push beyond standard conceptualizations of leader incentives and foreign policy in several ways. The theory unites positive and negative strategies rather than treating them as divergent phenomena. I also break the traditional dichotomy of democratic and autocratic regimes, modeling dynamic political processes and explicitly incorporating the political opposition. I pursue a multi-stage modeling technique which more faithfully represents the strategic encounters between sending and targeted states and furthers our understanding of the interplay between external demands and domestic political incentives.

The findings suggest many strategies utilized for targeting aid and economic sanctions may be faulty. Sending states' best bet for achieving concession may be to target leaders whose place in office is very secure, yet empirically they pursue the opposite strategy. Contrary to much theory in the literature, I also find that even ineffective negative sanctions can achieve success provided the target faces few domestic challenges. The probability of concession also increases when states demand concessions of a diffuse and symbolic nature, rather than changes to the status quo which would hurt a

private domestic interest. A strong political opposition magnifies the relative ease of public-costs concessions, suggesting that challenging parties compete for the favor of elites rather than championing the public interest.

Abstract Approved: _____
Thesis Supervisor

_____
Title and Department

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Date

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Iowa City, Iowa

CERTIFICATE OF APPROVAL

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PH.D. THESIS

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This is to certify that the Ph.D. thesis of

Amanda Abigail Licht

has been approved by the Examining Committee
for the thesis requirement for the Doctor of Philosophy
degree in Political Science at the July 2010 graduation.

Thesis Committee: _____
Brian H. Lai, Thesis Supervisor

_____
Sara McLaughlin Mitchell

_____
Frederick Boehmke

_____
Cameron Thies

_____
Michaela Hoenicke-Moore

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CHAPTER 1

POWER IS AS POWER DOES: AN INTRODUCTION TO INFLUENCE ATTEMPTS AND TARGETED LEADERS' INCENTIVES

In the spring of 2009, the Pakistani military began pursuit of Al-Qaeda-related targets in the Swat Valley. These operations prompted a serious humanitarian crisis, as refugees streamed from the region. In response, Secretary of State Hillary Clinton held a press conference on the 19th of May, announcing the allocation of an additional $100 million in assistance to Pakistan to alleviate the hardships of displaced persons. Secretary Clinton's explanation for the increased aid flows shifted seamlessly from the suffering of civilians on the ground to the overarching strategic importance of supporting Pakistan's decision to undertake the politically costly maneuvers. "Providing this assistance," the Secretary proclaimed, "is not only the right thing to do, but we believe it is essential to global security and the security of the United States, and we are prepared to do more as the situation demands" (White House 2009). This outlay of funds hardly represented the United States' first attempt to "support" the Pakistani government in an effort to tackle the problem of Al-Qaeda and Taliban camps within its territory.

From the time when he pledged support for the U.S. "War on Terror" to his exit under threat of impeachment in 2008, the United States allocated a total of $665.562 trillion in military and economic assistance to the preceding regime of President Pervez Musharraf (USAID 2010). Compared to aid received in the preceding two years, U.S. aid to Pakistan in fiscal years 2001-2002 increased by 923%. The Bush administration made clear the purpose of this payoff and President Musharraf began suffering the political consequences of his alliance choice almost immediately.

As early as September 23, 2001 large scale protests and marches in opposition to the decision emerged (Burke 2001). Internal dissent increased in breadth and depth as Musharraf attempted to make good on his commitments to clear the northwestern tribal

regions of Al-Qaeda and Taliban strongholds. Military actions in the regions spurred armed retorts (Ricks and Khan 2002), rocket attacks against Peshawar (Lynch 2004) and full-scale battles (Garcia 2004). While the U.S. praised Musharraf's military maneuvers, mounting civilian casualties and growing anti-American sentiments led to a resurgence of support for fundamentalist political parties (Lancaster 2003; Lancaster and Khan 2004; Garcia 2004b).

In 2006, Musharraf backed away from the battle, signing a truce with local tribal leaders. The agreement restored the autonomy of the tribal regions in exchange for a commitment to peaceably discourage foreign infiltrators in their locale. Musharraf, a military man, declared that violence only creates entrenched positions and advocated for negotiations with the Taliban (The Globe and Mail 2006).

The withdrawal of the Pakistani military presence allowed a resurgence of both Al-Qaeda and Taliban operations amidst the rough terrain of the tribal provinces. The U.S. shifted its strategy. Rather than promise and praise, American representatives began to threaten and criticize Musharraf (Sanger and Mazzetti 2007; Loudon 2007). The new tactic culminated in the strategic release of intelligence reports locating Al-Qaeda's most prominent officers in mountain enclaves within Pakistan's borders (DeYoung and Warrick 2007). These claims publically questioned the capacity of President Musharraf's regime to enforce law and order within its borders, an embarrassing development for an executive whose traditional support base lay in the military. This embarrassment, combined with the rising domestic political threat of Muslim extremism prompted Musharraf to renew his military campaigns in the tribal regions in July of 2007 (DeYoung and Warrick 2007).

For Musharraf, however, the political damage had been long done. Serious irregularities in his 2008 electoral victory prompted claims of tampering and threats of impeachment proceedings from the opposition. Roughly one year after his renewed efforts to fulfill obligations to the U.S., President Musharraf resigned to avoid further

disgrace. And so, the United States began its efforts to convince the new government to undertake the extreme political costs of turning the state's weapons upon the most politically radical and geographically entrenched of its own citizens.

This timeline provides concrete illustration of several of the key theoretical and practical problems with which this dissertation plans to grapple. The ups and downs of concerted anti-terrorism efforts in Pakistan reflect the varying ability of the United States to influence the domestic political choices of the Pakistani leadership. Such attempts by external actors to influence the behavior of other states fall firmly within the realm of international relations' most central queries: what is power, who has it, and how does it work? Through foreign policy tools, such as the foreign aid and economic sanctions leveled against Pakistan in the example above, states attempt to exercise power over each other. I will address variation in the success of such endeavors through a focus on the incentives of the political elites whose decisions these policy tools purport to impact.

The most comprehensive understanding of power defines it as the ability of one actor to alter the behavior of another. Power, then, is less something possessed than something accomplished. States exercise power through varied tools of foreign policy, which can be divided broadly into positive sanctions and negative sanctions. In other words, states employ both rewards and punishments in the pursuit of influence over others. The United States' dealings with President Musharraf illustrate specific tools within each category. Grants of economic aid, military assistance, special alliance status, and public praise all qualify as positive tools of influence; while threats of economic sanction (in this case, the threatened withdrawal of aid) and public criticism fall under the rubric of negative sanctions.[1] Beyond giving concrete examples of each type of sanction,

[1] The clear distinction between positive and negative sanctions is questioned by some theories of power (e.g. Dahl 1963; Schelling [1960] 1980). This particular example of foreign aid awards functioning first as positive and "then" as negative when their withdrawal is threatened forms the basis of this protestation. As will be detailed in the literature to come, I find that this position blurs both temporal processes and the mechanics of influence.

the Pakistan case, then, also shows the willingness of states to shift and mix these strategies.

Secretary Clinton's press conference comments illustrate a second important element of the exercise of power. States attempt to influence the domestic policies of others for a variety of reasons. The goals may range widely from the purely humanitarian to the purely strategic, and positive and negative tools may be used in pursuit of the full spectrum. Though ostensibly the emergency aid allocated to Pakistan in May of 2009 addressed the humanitarian crisis and reflected a concern for the wellbeing of actual human beings, there was little doubt in the minds of journalists in attendance that the overarching goals of the U.S. were to encourage the continuation of an incredibly costly anti-terrorism campaign by offsetting the human costs and limiting anti-American blowback.[2]

No matter the professed or implied aim of influence attempts, the desired outcome almost always involves obtaining a policy concession from the targeted state. Concessions may involve: changes in existing undesirable behaviors which are domestically preferred, or avoidance of future undesirable behaviors which would be domestically preferred; continuation of current desirable behaviors which have become domestically costly, or commitment to adopt desirable behaviors in the future despite likely domestic costs. Power cannot be observed if the targeted actor does not change their behavior to do something that otherwise would not be done.

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[2] The briefing and questioning can be viewed online through the White House website at <http://www.whitehouse.gov/video/Secretary-of-State-Clinton-Briefs-Press-on-Aid-to-Pakistan>. The most pointed question was this:

> "Madam Secretary, are you worried the Pakistanis might abandon the fight against the Taliban without this aid? And is the lack of this kind of aid, in your opinion, the reason that former President Musharraf did not prosecute the war against the Taliban as efficiently as the current government?"

Secretary Clinton, as might be expected, answered indirectly.

Concessions require action by key decision-makers within the target state. Compliance with the U.S. terms of assistance required Musharraf to eliminate the long-standing policy of the border regions' de facto autonomy, a state of affairs which not even the British imperial army chose to upset. Succumbing to U.S. influence in this case, carried domestic political costs of the most immediate kind: following his cooperation, disgruntled fundamentalists twice attempted to take Musharraf's life. Not surprisingly, the president's willingness to continue providing this concession declined in the face of the mounting domestic backlash. In order to renew his commitment, the Americans were forced to switch policy tools.

This variability in Musharraf's concessionary behavior points us toward the most important lesson in the case of the United States' attempts to exercise power over Pakistani politics: external influence attempts often fail. In aggregate terms, the two key foreign policy tools utilized in this case – foreign aid and economic sanctions – obtain shockingly low success rates. Optimists estimate only one in three economic sanctions episodes returns policy concessions (Hufbauer, Schott and Elliott1990). And prominent international financial institutions (IFIs) report a maximum of about 60% compliance with conditions imposed on aid monies, a figure which they recognize as inflated by some very easy terms (World Bank 1992). In the following chapters, I address this vital issue in international politics, seeking to answer the questions, "When do foreign influence attempts succeed in obtaining concessions from targeted states, and why do they so often fail?"

If we want to understand the success and failure of external influence attempts, I argue we must turn our attention to the leaders of targeted states. In Chapter 3 I will present a theory of influence which recognizes the centrality of targeted leaders' political incentives. It is ultimately the decisions of these men and women which determine, if, when, and how fully concessions will be provided. A focus on the targeted decision makers requires us to consider the effects of external influence attempts on the people in

power and the likelihood of these effects putting them in a concessionary mood. In so doing, we must remember two points: first, all targets are not created equal; second, pressure comes from below as well as above.

Emphasizing these points, we should expect that influence attempts intersect with leaders' incentives in two ways. The first, which I term the "effectiveness" of a foreign policy tool, is the ability of the sanction to affect the targeted leaders' ability to hold on to power. The effectiveness of any given policy tool varies across the range of its potential and actual targets. Some tools are appropriate for the targeting of leaders who rely on the support of the privileged few; others work better when the public interest determines leaders' continued stay in power. Moreover, the susceptibility of leaders to external attempts to insulate or destabilize them (depending on the choice of positive or negative sanctions) varies over time. Experience in office brings expertise and competence, but also changes in the loyalty and fervor of supporters. Effectiveness, as conditioned both by the appropriateness of the chosen policy tool and the targeted leader's experience in office, affects the baseline probability of influence success. When sanctions backfire, hurting leaders they should help or helping leaders they should hurt, successful influence becomes unlikely.

The costs which would be produced by the demanded concessions generate the second impact of external influence attempts on leader incentives. Though appearing to make decisions based on the interests of other states rather than their own will almost never ingratiate leaders to their populace, some concessions are easier to provide than others. Targeted leaders' resolve to resist influence deepens in response to concessions likely to trigger strong domestic responses. The highest political barriers to concession arise when the required policy changes would cause intense costs to focus on a particular element of the population. The theory of collective action tells us political mobilization occurs most often in the face of such private costs (Olson 1971). The relative costliness of private-costs producing concessions, however, will also vary across targets. The

presence of opposition groups whose incentives drive them to publicize even relatively cheap concessions constitutes the first likely source of this variance. The second stems, again, from the time which the leader has accumulated in office. Experienced leaders may have the skills necessary to force through controversial policy changes, or they may have expended, too much of their political capital in earlier policy debates to affect any further changes. The magnitude of the domestic political barriers to concession, conditioned by the demands, the strength of the opposition, and the targeted leaders' experience, place limits on the probability of successful influence.

Together, sanction effectiveness and domestic hurdles to concession represent the process of influence from the perspective of the targeted leader. By definition, power requires the shift of a target's behaviors from those which align with its own preferences and incentives to those which align with the external actors'. This may be accomplished through utter destruction at very high costs to both parties. But, more commonly, major states apply rewards and punishments which, if accurately targeted and performed, change the underlying incentives of the target. Beginning with the perspective of the targeting leader, as my approach recommends, thus constitutes the foundation for understanding the success and failure of influence attempts.

Modeling the outcome of foreign policy rewards and punishments in terms of their intersection with leader incentives requires several steps. In Chapter 4 I will outline the details of a design which allows me to tap directly into the impact of influence attempts on targeted leaders' survival and model the outcome in terms of both effectiveness and domestic hurdles. I begin by selecting foreign aid and economic sanctions as model cases of foreign policy rewards and punishments. These represent some of the most commonly utilized positive and negative sanctions in international relations, and are closely linked practically by their economic nature. Indeed, some analysts believe them to be two sides of the same coin (e.g. Schelling [1960] 1980; Dahl 1963).

After selecting these policy tools, the next step of analysis is estimating their impact on targeted leaders' tenure in office. I assess effectiveness through event history analyses of leader failure following targeting with foreign aid and economic sanctions in Chapter 5 and 7 respectively. This modeling choice allows me to directly estimate the impact of sanctions on leader survival. From these regressions, a prediction of sanctions' impact on survival will be created. This improves upon the traditional tactic of proxying sanctions' impact on leaders through regime interactions, tapping more directly into the proposed dynamic of survival-driven behavior. The predicted measure will be utilized in models of concession to foreign policy rewards and punishments in Chapters 6 and 8.

The theory and empirical analyses proposed herein push beyond the conceptualizations of leader incentives and foreign policy in the extant literature. Firstly, this framework emphasizes the dual pressures acting upon targeted leaders. While positive and negative sanctions represent attempts to influence from above, domestic processes of power-sharing and competition constantly influence elites from below. The competing pressure from external and internal sources ensures that costs accrue from compliance as well as noncompliance. Rather than assuming the key to foreign policy success lies in the sending states' ability to generate steep enough costs for recalcitrance, I acknowledge the presence of domestic groups capable of inflicting costs for submission.

In pursuit of an understanding of the domestic political pressures which impinge upon external influence attempts, I break open the frequently relied upon dichotomous blocks of democratic/nondemocratic in two ways. First, I consider and explicitly model the impact of dynamic political processes on leaders' vulnerability to external influence. Second, I incorporate the political opposition – whether democratic or not – as a true player in the game of policy change and concession. Incorporating these details recognizes politics itself, in all its fickle confusion, as an explanation of international politics. Rather than a single input which identically determines the behavior of all

leaders within a certain class, political institutions produce complex and competing dynamics which directly impact incentives.

The two-stage modeling process which I pursue also contributes better to our understanding of the interplay between external demands and domestic incentives. The traditional approach proxies the ability of sanctions to impact leaders using an interaction between sanction and regime type. This method conflates regime type with effectiveness, creating two problems for hypothesis testing. First, these proxies ignore variation within the blocks of democratic and not which condition the ability of institutions to muffle or to exacerbate the external intervention's impact. If some types of autocratic leaders are less vulnerable to external influence than others, their behavior will muffle the estimated impact of sanction-effectiveness on influence outcomes.

Second, the regime-interactions lump together a host of possible causal factors which may contribute to concession provision. Consider, for example, the role of democracy in an evaluation of compliance with conditionality agreements. If a culture of legality compels democratic leaders to uphold contractual commitments, we should see a regime interaction achieving significance whether the sanctions significantly impacted survival or not. Shift focus to evaluation of concession outside of contractual obligation, and the democracy-sanction interaction may drop out of significance or flip in sign for the same reason rather than any impact of the sanction on survival prospects.

My framework also unites expectations regarding the success of both positive and negative sanctions, rather than treating them as entirely divergent species. While hostile policies aim to communicate disapproval and friendly policies intimate the opposite, both function as tools of influence in the international system and sending states wield both in pursuit of the full spectrum of goals. These similarities produce analogs in their operation and malfunction. Political factors which condition the success of negative sanctions also condition the success of positive sanctions (though not necessarily in exactly the same way).

My approach also holds value for practical policymaking. States expend considerable resources on foreign policy endeavors such as development assistance and economic sanctions, while receiving little in terms of influence success. My focus on targeted leaders' incentives will highlight the conditions under which policy tools succeed in tapping appropriately into leaders' base motivations and when they fail. When influence attempts backfire at the effectiveness stage, many unexpected and undesirable occurrences may follow. Sanctions intended to disrupt a despot may instead insulate, leading to continued human rights abuses or escalated domestic violence (e.g. Saddam Hussein's Iraq). Rather than ameliorating international interventions may exacerbate many sticky domestic situations. A better understanding of these potential backfires may assist in more effective and efficient foreign policy targeting.

Similarly, my attention to the feasibility of concession by the difficulty of changes to the status quo and the changing domestic political environment holds practical value. I will assess the relative power of the difficulty of offering and willingness to provide concessions by modeling the processes together. If the goal of foreign policy is successful influence rather than simply the appearance of action, then intervening states may need to level more measured and carefully timed requests.[3] My analyses will outline what types of concession are most likely to be successful, at what point in leaders' careers these concessions are easiest to provide and under what level of political opposition leaders tend to be more vulnerable to external demands.

The argument presented herein contributes to a long line of research in international relations. Before outlining my own ideas on the subject, I must acknowledge the impact which the extant literature has had on their production. Below, I proceed by

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[3] With respect to economic sanctions in particular, some theories argue that the endeavor is more about sender states signaling to their own population than about obtaining results (e.g. Eland 1995; Daoudi and Dajani 1983). Further, Nossal (1989) claims sanctions inflict punishment for punishment's sake.

reviewing some influential accounts of power's nature and application in the international sphere. This general account will be followed by consideration of the literature on exemplary positive and negative tools of influence, beginning with foreign aid and moving on to economic sanctions. Finally, I will discuss the emerging theoretical focus on leaders' incentives in international relations research as fertile ground for progress in our understanding of the productive use of foreign policy. Chapter 2's review of leading ideas in the field thus sets the tone both for the elaboration of the ideas introduced above and the series of empirical tests carried out in Chapters 5 through 8.

CHAPTER 2

POWER AND INFLUENCE IN THE LITERATURE

Power permeates the theory and practice of international politics. Morgenthau (1949, 13) opened his famous text by asserting, "International politics, like all politics, is a struggle for power." While mainstream paradigms of the discipline now acknowledge the vital role of other goals and ideas, leaving power out of the narrative makes the story difficult to follow. This is especially so when we consider power not as a list of resources, but as a relationship:

> Political power is a psychological relation between those who exercise it and those over whom it is exercised. It gives the former control over certain actions of the latter through the influence which the former exert over the latter's minds. That influence may be exerted through orders, threats, persuasion, or a combination of any of these (Morgenthau 1949, 14).

Morgenthau's definition – which bears remarkable resemblance to that often used today – highlights the centrality of power to the project at hand.[4] I seek to identify the conditions under which targeted leaders prove vulnerable to external demands. This is analogous to identifying the conditions under which external actors may successfully influence targets. Economic sanctions and foreign aid fall under the rubric of "orders, threats, [and/or] persuasion" as means of projecting power.[5] Given this connection, it is important to review the theoretical foundations of power in international relations scholarship before moving on to the specifics of any individual policy tool.

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[4] Credited to Dahl (1957), the most commonly cited definition of power is the ability of A to get B to do something B otherwise would not do. Like Morgenthau's, this is a "relational" conceptualization as opposed to "possessional" (e.g. Baldwin 1985, 22-24). Power inheres in the relationship between two or more actors, not in the gold, missiles or other resources amongst them. The language of "psychological" pull has been dropped in favor of terms more conducive to analysis and hypothesis testing.

[5] Morgenthau (1962), however, was very skeptical of foreign aid as an effective tool of influence. His argument foreshadowed the findings of many economists, that sending money to status quo powers is not conducive to changes in the status quo .

Sophisticated theories of power analysis developed during the Cold War. Below, discussion covers two very influential approaches from that time period, when successful influence attempts were often considered a matter of global survival. The first of these is Schelling's ([1960] 1980, 1966) groundbreaking strategic analysis of behavior in "mixed-motive games". Though the logic transfers to other areas of interaction, Schelling speaks explicitly to military force and coercion. The second scholar pursues a different class of policy tools. Baldwin (1971, 1985) insists on the importance of promises as well as threats in the pursuit of influence, and focuses on economic rather than military tools. Despite this difference in focus, Baldwin shares with Schelling the tendency to focus in on the skill and credibility of the sending state rather than political realities within the target. In following sections, I will present evidence from the literature on sanctions and aid, suggesting this is a major area for development.

Power through Coercive Means

Schelling ([1960] 1980, 1966) considers the problem of gaining influence in "strategic situations", those characterized neither by complete animosity nor by complete harmony. The coercive and preventative means of "winning" such "mixed-motive games" dominated thought during the Cold War (Davis 2000, 4-6). Schelling's (1966, 3 emphasis original) opening argument in *Arms and Influence* demonstrates this tendency:

> It is the *threat* of damage or of more damage to come, that can make someone yield or comply. It is *latent* violence that can influence someone's choice – violence that can still be withheld or inflicted….

Influence, under this frame, is accomplished by use of deterrent and compellent threats.

Deterrence refers to A's prevention of an action by B. A successful deterrent threat convinces B: (1) that the benefits of pursuing a certain policy would be outweighed by A's threatened punishment, and (2) that A will carry out the threat if challenged. Deterrence situations are characterized by the demand for *nonaction*: "Stay

within your sovereign borders, *or else*", "Do not develop WMD, *or else*", "Maintain free trade, *or else.*"

Economic sanctions could be termed deterrent. They have become a standard solution to conflicts short of war, and they may be directly specified in foreign aid or trade agreements. Under these conditions the threat of sanctions holds prior to any fault being committed by the object of influence. Often, however, economic sanctions are threatened in response to, rather than anticipation of, bad behavior.

Compellence refers to those situations when B has already undertaken an undesirable behavior which A must stop or reverse. Compellent threats require the imposition of punishment and the pledge to continue until concession is forthcoming. This type of threat presents added difficulties. To be effective, any threat must be clearly understandable to the target; the terms for compliance must be ascertainable and observable. In compellence situations, however, states face problems communicating the terms which will bring about the end of punishment. The ambiguity of goals and lack of assurance that matters will return to the status quo make it difficult for targets to concede. Moreover, the reputation costs for compliance with a compellent threat are higher; complying with a deterrent threat requires no action whereas compellent threats require a real change in policy (Schelling 1966, 69-86).

Another key concept developed by Schelling can be illustrated by the legal concept of "last clear chance", the standard for assigning fault in automobile accidents. If one player can lock in a strategy – by burning the metaphorical bridge or throwing the metaphorical steering wheel out the window – the other is forced to make the decision whether to back down or inflict mutual pain. This logic flows from the bargaining framework, wherein the actor with the most restricted preference set can manipulate more flexible actors. As Schelling describes, in a two-player bargaining game a player only cedes, "Because he thinks the other will not. 'I must concede because he won't. He won't because he thinks I will. He thinks I will because he thinks I think he thinks

so…'" ([1960] 1980, 21-2). If this is the reason for sub-optimal outcomes, then a strategy of "tying hands" is likely to be very effective. Actually being incapable of giving in must be more effective than just saying one is so. This suggests an interesting flip in the power relationship between donors and recipients of foreign aid:

> The government that cannot control its balance of payments, or collect taxes, or muster the political unity to defend itself, may enjoy assistance that would be denied it if it could control its own resources (Schelling [1960] 1980, 23).

For an aid-receiving leader, then, the best way to keep getting aid may be to remain incompetent rather than to achieve the goals set out by donors.[6]

Schelling's influence has weighed heavily upon the development of military deterrence and bargaining theory in international relations. In particular, his emphasis on the construction of credible threats has formed the basis for most theories and empirical tests of deterrence situations. Within the rational choice literature on the subject, four principle variables are forwarded as central to a sender's ability to credibility threaten (and thus achieve deterrence success): bargaining/signaling behavior, reputations, relative military capabilities, and interests at stake (Huth 1999).

Bargaining and signaling behavior as an explanation of strategic success has recently focused on the "audience cost" argument forwarded by Fearon (1994, 1994b). Domestic political audiences which have the potential to restrain and punish leaders who back down after issuing threats provide leaders with a means of making their threats costly. This contributes to success by assuring potential challengers to the deterrent threats with a signal that the threatening state is not bluffing. An older, alternative explanation of credible threats focuses on the *ability* of the sender state to follow-through on the threat. Strong states appear generally more capable of deterring new challenges (Mearsheimer 1983; Shimshoni 1988; Lieberman 1994, 1995; Huth 1988, 1996; Bueno

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6 This "moral hazard" problem has been investigated and found some support in the literature on IMF lending (Dreher 2006).

de Mesquita, Morrow, and Zorick 1997). Strength, however, cannot always prevent low-level "probes" through which challenging states evaluate their opponents' resolve (George and Smoke 1974; Lieberman 1994,1995; Shimshoni 1988; Bar-Joseph 1998; Morris 1993; Huth, Bennett and Gelpi 1992; Huth and Russett 1993). Similarly, reputations for resolve on the part of the threatening state contribute to deterrence success only conditionally. Only when reputations refer to geographically proximate issues and have been recently demonstrated do they carry enough weight to prevent challenges (Huth 1999; Huth, Gelpi and Bennett 1993; Orme 1992; Lieberman 1994; Shimshoni 1988; Stein 1996).

Finally, researchers have considered the import of the issues over which deterrent threats have been conveyed as a predictor of their success. Selection bias plagues evaluation of the hypothesis that highly salient issues will contribute to deterrence failure (Danilovic 2001; Smith 1996). Threatening states are likely able to signal strong commitments regarding alliance partners' security and territorial disputes. Yet, we observe only cases where precisely the high salience which makes that threat credible also drives the challenging state to act despite the credible threat of retaliation (Hensel 1996).

The majority of work on this topic focuses in on characteristics of the state which issues the deterrent threat. The strategic domestic imperatives of targeted states have received much less attention. As Huth's (1999, 36-7) review of the literature reports, most arguments about states which fail to be deterred despite military imbalance attribute some kind of misperception or error in judgment to the offending leader. While cognitive bias could certainly be at work in some cases, this argument gives little credit to the complex sources of political pressure acting upon targeted leaders. "Rational choice analysts could argue," as Huth does (1999, 41), "that the risks of a crisis or war may be acceptable to decision makers when the alternative is the loss of political power or the anticipated weakening of their country's international security position."

Findings support the idea that tumultuous domestic situations may lead to international "acting out" under certain conditions (Huth and Russett 1993; Enterline 1998; Mansfield and Snyder 1995, 2002; Snyder 2005; Mitchell and Prins 2004). Domestic incentives, however, shape leaders' decisions constantly, not just in this dramatic fashion. Any time external actors attempt to alter target behavior, they should consider the likely domestic difficulties which will ensue. The interaction between domestic incentives and external demands merits further investigation.

Power through Diverse Means

Baldwin (1971,1971b, 1985) and Schelling ([1960] 1980, 1966) agree on many points, notably the added difficulty of compellent threats. The success of influence attempts, Baldwin insists, must be assessed with careful recognition of how baseline differences in the probability of success (such as the need for a compellent versus a deterrent threat) affect our conclusions. To compare the success rates of economic sanctions to threats of force, according to this view, is like comparing the ratings of a public TV special on deterrence to Sunday Night Football. This practice leads to the conclusion that sanctions do not work *as well as threats of force*, "Yet threats of force are usually used to deter while economic sanctions are usually used to compel" (1971b, 475).[7] If Schelling is correct that compellence is inherently more difficult than deterrence, a fair comparison cannot be made between sanctions and threats of force.[8] The process by which threats of force are made rather than economic sanctions is not

[7] If empirically accurate, this pattern may derive from the logic described by Schelling, that deterrent threats perform the bigger they are (up to a point) whereas compellent threats may lose credibility if they become too drastic.

[8] To get at this problem, analysis should differentiate cases of sanctions used as deterrents and threats of force used to compel, thereby holding the level of difficulty constant. Accomplishing this task in a large N analysis, however, requires information that is not widely available.

random. Policymakers evaluate a range of policy options when they seek to influence the actions of other states.

The breadth of selection available to policymakers may constitute Baldwin's departure from Schelling's theory. In order to elaborate on this point, we must first review a vital nuance of Baldwin's argument. Drawing heavily on sociological theories of power, Baldwin argues that an influence attempt could operate through a variety of potential mechanisms in the targeted state. These mechanisms are referred to as "power bases" (1971b, 474; 1985, 134-8). Consider, for example, a state targeted with sanctions following human rights abuses. If the state decides to provide concessions, the power base may be economic (if the strain of interrupted business prompts concessions) or social (if the shame of international disapproval drives the decision to renege). Concluding automatically, as Baldwin (1985, 136-7) claims many do, that a sanction which fails to inflict serious economic harm cannot be effective thus oversimplifies its potential power as a policy tool.[9]

Returning to the earlier point, alternative power bases highlight the existence of influence attempts outside the realm of threat and force. Within economic statecraft, Baldwin (1985, 42) locates at least nine positive foreign policy tools designed to alter targets' behavior through more civil means.[10] Countries targeted by positive sanctions may be compelled to good behavior either by the prospect of gain, or the recognition that assurances from outside negate their motivation to behave badly (e.g. Davis 2000). They may also be deterred by the implicit threat that "…what was once given may now be

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[9] Baldwin (1985, 136-7) lays much of the blame for this on Galtung's (1967) classic articulation of sanctions and its presentation of "the general theory of economic sanctions".

[10] Baldwin's positive economic sanctions include: favorable tariff discrimination, granting most-favored nation status, tariff reduction, direct purchase, subsidies, granting licenses, providing aid investment guarantees, encouragement of private capital, and favorable taxation. A complete list may also include disaster relief, technical assistance, transfer of military advisors, arms sales, defensive pacts, etc.

taken away" (e.g. Dahl 1963, 51). Baldwin holds, however that the use of these positive sanctions and the threat of negative sanctions should not be treated as two sides of the same coin.

Positive sanctions, like the provision of aid, can indeed be used to "set up" future deterrent threats (Schelling [1960] 1980). This, however, is not their primary function. Positive sanctions provide states with a means of expressing support and approval of other states, just as negative sanctions signal disapproval and hostility (Baldwin 1985, 135-6). Friendly gestures can ignite a reciprocal response, leading to the construction of a cooperative relationship. Consistent acts of this type construct stores of international good will – the "soft power" which Nye and Keohane (1977) tout as a powerful tool of influence. While fear and distrust are indeed powerful motivators of state behavior (e.g. Waltz 1979; Mearsheimer 2001; Walt 1987), so too are softer "feelings". Rather than creating fear of loss, positive policy tools focus on creating ties of affection and trust, transferring information and skills, cementing business relationships, affirming norms of behavior (Baldwin 1985, 135-6).

The literature on compliance with international agreements notes a similar dynamic whereby desired outcomes can be reached without the use of coercion. States which desire coordinated action can create agreements or institutions which alter incentive structures by creating transparency, sharing information, and establishing new expectations (e.g. Keohane 1984; Keohane and Martin 1995). In the presence of a conducive culture of legality or of "norm entrepreneurs" committed to the cause, these new expectations for behavior, or "focal points", may lead states into deeper cooperation (e.g. Doyle 1983,1983b; Slaughter 2003; Dixon 1993; Mitchell and Powell 2009).[11]

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[11] Beth Simmons (1998) provides a helpful clarification of the difference between the relatively unilateral influence attempts upon which I focus here and the more cooperative relationships investigated in the compliance literature: "… [the compliance literature] discusses compliance with explicit rules or agreements, often of a legal character or of normative import, and not compliance with the demands of an adversary or the requests of an ally. The concern is

Positive sanctions, in short, operate through alternative, legitimate power bases. And, while some have argued that today's gift of assistance becomes tomorrow's threat to revoke, Baldwin (1971, 24) correctly notes that to lump these together ignores the passage of time, blurs the meaning of *both* concepts, and artificially diminishes the policy options available in times of crisis.

The decision process leaders embark on, according to Baldwin, involves consideration of the relative costs of alternative strategies of promises vs. threats, military vs. economic vs. diplomatic. Leaning again on Schelling ([1960] 1980, 177), Baldwin claims the baseline difficulty of an endeavor will influence selection of specific policy tools. Threats are costly when they fail; promises when they succeed. Threats, therefore, should be levied in the face of likely success; promises otherwise (Baldwin 1971, 30). Generally, this effect should create a more impressive track record for negative sanctions compared to positive sanctions, but it will say little about the variation in performance *within the same category of policy tool*.

To find out why some sanctions produce immediate concession and some produce quagmire, why some aid programs produce growth and others produce corruption, we must consider the specifics of these influence attempts. What do we know about their selection, their impact and their results? Below, I proceed with a review of the literature of exemplary positive and negative foreign policy tactics, beginning with foreign aid and moving on to economic sanctions. Then I will discuss how the emerging theoretical focus on leaders' incentives can further our understanding of the nature of influence attempts in the international system.

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typically with obligations that flow from authoritative agreements, widely held normative prescriptions, or authoritative interpretations of proper behavior, rather than acquiescence to unilateral political demands based on the exercise of power alone. In practice, of course, agreements among asymmetrically endowed actors are rarely perfectly voluntary, and the decision to conform to prescribed behavior might rest on an amalgam of obligation and felt coercion" (78).

Foreign Aid: Mixed Intentions and Mixed Outcomes

Wealthy and powerful states proffer foreign policy "carrots" – preferential trading agreements, military assistance and foreign aid – on a regular basis, sometimes with blatantly strategic aims. The U.S. promise of substantial economic and military assistance to Egypt's President Sadat through the Camp David Accords presents a notable historical example. And, in recent years, the United States has established beneficial relationships with many Central Asian and Middle Eastern states, providing aid in return for cooperation in the War on Terror. Even when donor nations hope to achieve more "liberal" goals, positive sanctions are meant to cause a change in the domestic politics of targeted states. These are power-plays, attempts to influence others in the international system. In this section, foreign aid, one of the most commonly utilized friendly foreign policy strategies, will be examined in depth.

Foreign aid provides an excellent embarkation point for investigating the success of positive sanctions as influence attempts. The advanced world invests heavily in development assistance, but its effectiveness is frequently called into question by empirical studies (Chenery and Strout 1966; Regan 1995; Kosack and Tobin 2005; Daalgard, Hansen and Tarp 2004; Brautigam and Knack 2004). In order to fairly evaluate the success of aid as a tool of influence, however, we must first consider the aims of donor states. Then, the findings of existing studies can be considered in light of donor intent.

Donor Goals

Dudley and Montmarquette (1976, 133) profess the conventional wisdom on foreign aid most succinctly: "…people usually give because they expect to get something in return. … in practice very few transfers are unilateral". The existence of a literature on the success rate or effectiveness of economic aid testifies to the fact that even humanitarian aid is allocated for a political purpose. Sometimes this political

purpose is spelled out directly in conditionality agreements. "Under conditionality", writes former regional economic policy and democracy/governance advisor for the U.S. Agency for International Development in Africa David F. Gordon, "external resources are made contingent upon the recipient government's undertaking a set of (in principle) mutually agreed-upon policy changes" (1992, 30). Such contracts, however, developed relatively recently. Due to their public nature, they are also limited in the goals which they can profess. Donor goals may include more than economic policies, human rights and environmental protections.

In their model of aid allocation, Dudley and Montmarquette (1976) enumerate three possible types of "return" for the investment of foreign aid: alignment of the targeted state with donor state national preferences, economic profit, and improved position of targeted civilians. Morgenthau (1962) would add stabilization of a friendly regime and preservation of the status quo to that list. More generally, we might think of donors as seeking either humanitarian or instrumental goals (McKinley and Little 1977). Humanitarian goals correspond to the improved position of targeted civilians, achieved through poverty reduction, human rights improvements, and democratization. The instrumental (i.e. strategic/political) aims of donor nations include the other two of Dudley and Montmarquette's (1976) list as well as the stabilization of regimes.

A long line of research continues to demonstrate the importance of strategic considerations in the post-Cold War allocation decisions of donor countries. Patterns in the allocation of aid monies indicate that states place heavy weight on strategic indicators, including: how effective their money will be in purchasing influence, post-colonial status, strategic importance (e.g. Egypt and Israel), proximity to threats, and commercial interests (Bueno de Mesquita and Smith 2007; Stone 2006; Lai 2003; Collier and Dollar 2002; Burnside and Dollar 2000; Alesina and Dollar 2000). A growing number of researchers contend that strategic goals are on the way out. Their studies indicate that in "the new millennium" donors may be: (1) funneling assistance toward

governments with better macroeconomic policies, more democracy and human rights; (2) altering their transmission mechanisms to limit corruption; and (3) more willing to cut aid off when it does not produce humanitarian results (e.g. Bermeo 2008; Bearce and Tirone 2008; Berthelemy 2006; Berthelemy and Tichit 2004; Burnside and Dollar 2004).

These more recent studies have demonstrated two possible types of variance in the apparent goals of donor states. First, cross-donor variance appears in the relative weight major donors placed on indicators of recipient need and strategic importance in the aid allocation process; some states appear more likely to pursue strategic goals than others (Berthelemy 2006; Stone 2006). Other projects claim that the variance is temporal, the break occurring with the shift from bipolarity to a unipolar system at the end of the Cold War (Bearce and Tirone 2008; Bermeo 2008; Dunning 2004).

To the extent that these studies allow room for donors to desire different outcomes from their aid investments, this research correctly harkens back to the spirit of the Dudley and Montmarquette (1976) framework of "returns". A teleological drift towards claims of the dawning replacement of strategic goals with humanitarian intent, however, leads the new line of argument away from the reality of international politics. We must acknowledge that the very obvious changes in the international system which came with the fall of the Soviet Union have corresponded to changes in the definition of strategic interests for the major donor states (Lai 2003). The line between "humanitarian" and "instrumental" goals may not be so clearly defined when terrorism, rather than Communism, tops the list of international security threats; it is therefore inappropriate to conclude that we have overcome the "strategic" tendencies of aid allocation just because Cold War indicators of strategic behavior no longer turn up quite as significant in regressions.

Researchers often blame the division between sincere humanitarian goals and strategic instrumental goals for the "failure" of donors to send aid where it is most likely to "work" (e.g. Collier and Dollar 2002; Devarajan, Dollar and Holmgren 2001;

McKinley and Little 1977). The problem of foreign aid for these scholars is that strategically interested donors cannot credibly commit to revoke aid allocations. Strategic goals trump liberal ideals. But, if the donor could remain strategically disinterested, threats to cut off the flow of money would increase in credibility (Stone 2006; Bearce and Tirone 2008).

From an influence perspective, this approach is misleading. If we assume that under some circumstances, donors have strategic interests which override the importance of recipients' economic or democratic reform it becomes strange to assess the "success" of aid with respect to humanitarian goals. What has happened to the instrumental forms of return? The donor country may be more interested in seeing a general drift of recipient's foreign policy into line with its international agenda, an increase in trade relations with the recipient, or a stabilization of political power within the recipient state. Does aid allocated for such purposes objectively fail as an influence attempt if it does not *also* produce general improvement of the welfare of target populations? We must evaluate aid effectiveness in light of the full range of donor interests, not just the politically correct ones. Thus, discussion below will consider findings regarding both humanitarian and strategic aims.

Aid Effectiveness

Alleviation of poverty – a humanitarian goal – is the most commonly professed purpose of aid allocation agencies. Economists and political scientists alike have long scrutinized the ability of aid to achieve this goal. Overwhelmingly, research demonstrates that aid produces positive macro-effects in targeted economies only under some combination of fairly stringent conditions. Regarding economic growth and stability, economists find that aid can be effective provided domestic resources gradually replace foreign funds (Chenery and Strout 1966), pre-existing domestic policies or institutions are favorable and stable (Burnside and Dollar 2000, 2004; Islam 2002;

Chauvet and Guillaumont 2002)[12], aid amounts are small to avoid diminishing returns effects (Burnside and Dollar 2004; Daalgard et al. 2004), receiving countries experience few climate-related drains on the work force (Daalgard et al. 2004)[13], and receivers enjoy high levels of existing economic development (Kosack and Tobin 2005). Clearly much of the aid-receiving world fails to meet these requirements, and there is considerable evidence that donors fail to take such factors into account in the allocation process (Stone 2006; McGillivray 2004; Lai 2003; Collier and Dollar 2002; Alesina and Dollar 2000).

Research on the ability of aid to promote human rights and democratization – also humanitarian aims – produces contradictory findings. Some insist that assistance produces no statistically significant improvements (Regan 1995; Knack 2004), others theorize that inflows of foreign money will damage democratic accountability and government capacity (Svennson 2000; Brautigam and Knack 2004). When utilizing a measure of U.S. aid specifically allocated to "democracy and governance assistance", Finkel, Perez-Linan, Seligson and Azpuru (2006) managed to uncover a significant, positive relationship; given the current magnitude of allocations through this mechanism, however, the substantive impact in practice is not very large.

The ability of foreign aid to produce strategic concessions appears more consistent than the democracy findings, but just as conditional as the growth results. Most work in this area focuses on voting patterns in the United Nations General

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12 The original Burnside and Dollar paper (2000) became something of a lightning rod. For direct contradictory opinion based on measurement choices, population, and specification see Hansen and Tarp (2000), Easterly, Levine and Roodman (2004). Or for a review of the debate see McGillivray, Feeny, Hermes and Lenskink (2006). In their 2004 piece, Burnside and Dollar successfully incorporate the criticism of Hansen and Tarp (2000) regarding the diminishing returns effect of aid and of Easterly et al. (2004) regarding sample robustness while utilizing a different measure of good policy. None of the detracting papers, as far as I can tell, posits an unconditional positive effect for aid. Most appear to be either more pessimistic in their evaluation of aid's potential success, or to simply prefer a different conditioning factor.

13 Malaria is an example of climate-related drains on productivity.

Assembly (UNGA) as a good indicator of any general drift in recipient states' foreign policy towards the preferences of donor states. If aid-receiving states vote more with their benefactors than would be expected by chance alone, development assistance may be an effective tool of strategic influence. Early investigation of this proposition found that only the United States could compel its beneficiaries to shift their voting pattern in the UNGA (Wittkopf 1973). Wang (1999) found that on important issues, voting patterns are sensitive to *changes* in dependence on U.S. aid, but not to standing allocations. Lai and Morey (2006) demonstrate more directly that American military and economic aid produces voting patterns more consistent with American preferences. This effect, however, functions only when the recipient country is nondemocratic.

Donor nations' strategic intent may spread beyond the UNGA, however. Ensuring stability or propping up the status quo inside key states constitutes another likely instrumental goal of development assistance. Little work has investigated the potential link between regime stability and foreign aid. Morrison (2009) reports that the likelihood of regime change shrinks with aid allocation. Lai and Licht (2007), however, suggest this insulation effect may be conditional on initial regime type of the recipient state.

Linking Effectiveness to Target Incentives

The conditioning effect of target characteristics on the success rates across potential donor goals suggests the need for a new line of thinking. If the problem were simple variance in outcome across goals, Baldwin's caution to consider the role of baseline difficulty might suffice. However, the pattern surfaces in instrumental as well as humanitarian attempts at influence. The idea that donors lose their leverage on targeted countries when they cannot credibly commit to punishment presupposes an astute and equally strategic actor on the receiving side, but many investigations advance without considering the consequences of his/her existence. Is it, as Bearce and Tirone (2008)

suggest, reasonable to assume that aid monies provide all leaders an equally effective salve to smooth over the external imposition of costly policy concessions? And is knowledge of strategic importance the only reason to defy powerful benefactors? Or, is it – as Schelling insinuated –that target countries have thrown the steering wheel out the window, intentionally remaining incapable of meeting donor goals?

Targeted elites do not exist in a strategic vacuum where honoring agreements to external actors depends only on probability of punishment. All political elites depend on some core constituency of support within their population (Bueno de Mesquita, Smith, Siverson and Morrow 2005). Often, these dominant groups directly benefit from or were instrumental in creating the macroeconomic inefficiencies conditionality means to correct (Gordon 1992, 38). Changes in policy promise painful short-term adjustments to this core of elites for the sake of diffuse long-term benefits. Powerful groups will mobilize against this prospect (Olson 1971) and seek, instead, a means of converting aid money to further private benefits (Svensson 2000). "Given these realities," Gordon concludes that, "[foreign aid] has taken on many attributes of a 'game', in which donors attempt to 'buy' as much reform as they can… while recipient governments try to get as much money … as they can for as little reform as possible" (1992, 40).

The literature on aid effectiveness could benefit from a change in perspective. We must evaluate aid with respect to its full potential as a tool of influence for achieving both humanitarian and strategic aims. We must also consider more seriously the role of targeted leaders as they are the actors which aid aims to influence. Aid-receiving elites face pressures from below as well as above, and understanding their willingness and ability to deliver the changes requested by donors should constitute the core of a new model of aid effectiveness. Constructing such a model requires us to ask, how do the interests of targeted leaders' political base, their opposition and the external demand interact?

In the next section I review the literature on economic sanctions to demonstrate a similar disconnect between external intent and target priorities in studies of hostile foreign policy success. With both types of foreign policy tools thoroughly discussed, I introduce a body of theory which will be of assistance in determining when targeted leaders are actually vulnerable to external influence attempts.

Economic Sanctions: Short of War and Short on Results

By all accounts the use of economic sanctions by international organizations and powerful states was increasing rapidly into the 1990s despite intensely poor evaluations of the strategy's efficacy (e.g. Hufbauer, Schott and Elliott 1990). Optimistic interpretations of sanctions imposed between 1914 and 1990 give them a 35% success rate (Hufbauer et al. 1990); pessimists assign the same episodes a score of 5% (Pape 1997). While initial debate in political science focused on *whether* this form of hostile foreign policy works, recent research demonstrates a clear trend towards more sophisticated questions of *when* sanctions have a better chance of success and *why* we observe so dismal a pattern of outcomes.

Sanction Onset and Outcomes

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 (e.g. Hufbauer et al. 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 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 and Kim Jong Il's North Korea).

The most basic interpretation of the sanctions process holds that the restriction of economic relations (whether trade or foreign aid) 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 2008; Nooruddin 2002; Drezner 2000; Hufbauer et al. 1990). Cost, however, falls far short of a perfect predictor for either aspect of donor victory in the sanctions "game". Baldwin's (1971b,1985) theory of multiple power bases garners support from this. As he argued against Galtung (1967), sanctions are not this simple.

Formal theorists focus on another complication in the logic of sanctioning as a tool of influence. As they view the situation, the existence of any successes following implementation of sanctions is puzzling. 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 (e.g. Hovi, Huseby and Sprinz 2005). The formal approach to sanctions leads to the expectation that the more successful sanctions are those which never have to occur (Lacy and Niou 2004; Drezner 1999, 2003; Morgan and Meiers 1999; Morgan and Schwebach 1997; Smith 1996). This selection effect may account substantially for the poor observed performance of sanctions. While untested on the traditional (security-driven) population of sanctions due to an inability to observe the threat stage, there does appear to be a higher likelihood of concession prior to punishment when donor demands concern regulatory and trade issues; the concessions granted in

these situations also tend to be larger if they are offered up prior to punishment (Drezner 2003).

Obviously, sanctions happen despite the predictions of perfect information games. There are several explanations for this fact. The most obvious is simply misperception: targeted states believe that sanctions will not be very painful and/or that the sending states will not have the "guts" to follow through (Hovi et al. 2005). Alternatively, Schwebach (2000) finds, under imperfect information and low sender costs, sending sanctions can be a "pooling equilibrium", because irresolute potential senders have an incentive to "bluff". Drezner (1999) also locates strategic behavior as the explanation. In *The Sanctions Paradox* he argues that reputation costs prevent both senders and targets from backing down when their history has been conflictual.

Others locate the source of "irrational" sanctions in international norms or domestic politics. Those who label sanctions as "expressive" allege that their purpose is to mollify outraged domestic groups by demonstrating official disapproval for some behavior in the target state (e.g. Eland 1995; Haass 1998; Daoudi and Dajani 1983). Sanctions may be "punitive" in the sociological sense, a punishment for punishment's sake (Nossal 1989). For this school of thought, "… sanctions are symbols; their effectiveness is of secondary concern" (Drezner 1999, 12). We should not, therefore, be surprised if they continue to be employed despite rarely resulting in concessions.

The public choice approach, on the other side of the domestic coin, acknowledges that the sender's costs from imposing sanctions will be diffuse and thus unlikely to produce domestic opposition. At the same time, the economic distortions created by cutting off normal trade can produce profitable opportunities for narrow groups within the target state. "Sanctions rents", the ability to make money providing goods (imports) or services (exports) which have become suddenly scarce provide some governments with incentive to "endure" economic sanctions with a smile; 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). The public choice perspective thus points research in the direction of targets' political incentives.

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 a powerful conditioning effect of target institutions on the ability of sanctions to accomplish this basic victory: only democratic leaders are significantly destabilized by sanctions. Spurred on by this finding, Allen (2008b) 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 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 (Allen 2008; 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. Scholars in this vein have also argued that both monadic and dyadic predictions of the democratic piece transfer to the process of economic sanctions. While democratic leaders are more likely to provide concessions, they are more likely to utilize sanctions and less likely to target other democracies (Lektzian and Souza 2003; Cox and Drury 2006).

Autocratic leaders, according to Bolks and Al-Sowayel (2000), enjoy a broader set of possible strategies. Utilizing the language of selectorate theory (Bueno de Mesquita et al. 2005) but the logic of public choice (Kaempfer and Lowenberg 1999, 2000), Lektzian and Souza (2007) also condition their expectations about target behavior by regime type. The big winning coalitions of democratic leaders mean that those they care about will suffer the costs of sanctions, while nondemocratic leaders' elite constituency can easily be sheltered.

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) objects to the subtlety of the Kaempfer, Lowenberg and Mertens (2004) theory; he argues instead that dictators will always increase repression in the face of sanctions, and finds sanctioned nondemocratic leaders significantly likely to more severely repress their people. Allen's (2008) finding of significantly increasing variance in sanction outcome as the target's regime becomes less democratic, however, supports the more nuanced theory.

Linking Sanction Outcomes to Target Incentives

While findings appear to be compiling smoothly in this area, a couple snags persist. First, current studies do not connect the domestic conditions which make leaders vulnerable to the actual outcome of influence attempts. Instead, they address the domestic consequences of sanctions (e.g. domestic unrest, increased repression). Or, they investigate the effect of leadership turnover on sanction termination.[14] The simple story

[14] Jentleson and Whytock (2005) mark an exception to this criticism. Their study of the use of negative sanctions against Libya over time emphasizes the dependence of

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, while autocratic turnover hastens sanction termination (McGillivray and Stam 2004). If robust, this pattern throws serious doubt on the simple domestic sensitivity argument. We need a more nuanced understanding of the role of domestic political incentives on external actor's influence attempts, and rigorous tests which directly incorporate target incentives on influence outcomes.

Second, we should consider the role of internal factors in the success of threats as well as actually imposed sanctions. Both threats and punishments constitute attempts to influence. A few studies have utilized data and statistical fixes to account for the selection issue (Drezner 2003; Nooruddin 2002; Morgan and Schwebach 1997), but the theoretical importance of domestic incentives remains underdeveloped. In both stages, the targeted leaders' motivations tend to be measured only through regime dummies and the projected costs of the punishment. At the onset stage, studies either fail to locate a relationship between democracy and onset, or find that democratic leaders are less likely to be targeted (Cox and Drury 2006; Nooruddin 2002). Of course, this may be a consequence of the inability to account for the threat stage if democratic leaders are systematically more likely to give in right away due to their sensitivity. If this is so, however, the democratic leaders we observe being sanctioned should be particularly hard cases resolved to hold out against all odds, yet they are systematically more likely to give in and to give more than other sanctioned leaders. Existing models are not sufficiently accounting for the political incentives of these leaders. The fact that unexplained variance persists in the outcome of sanctions against autocratic leaders suggests a parallel

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leaders on key constituencies. This is, however, a single-case study subject to the usual shortcomings thereof.

need for theory regarding their motivations. The release of the Threats and Imposition of Economic Sanctions dataset of 888 episodes from 1971- 2000 provides a new opportunity to consider the effectiveness of sanctions as tools of influence (Mogan, Bapat and Krustev 2009).

Overall, these objections echo those voiced earlier with regard to the literature on foreign aid. The most recent developments in both areas highlight a need for more nuanced theory about the impact of targeted leaders' incentives on the ability of sending countries to achieve their goals. This need corresponds to the developing prominence of leader-centric theories of international politics. Researchers over the last ten years or so have placed increasing emphasis on the role of leaders as decision makers with domestically-driven, political motivations. This body of theory provides a jumping-off point for the development of a theory of foreign policy success which incorporates the interaction between targeted leaders' domestic political position and external demands. The next section draws together insights from the most pertinent leader-centric frameworks currently employed in the literature in order to chart a path towards the theory which will be forwarded in the following section.

Leader Incentives and International Relations

Realist analyses of foreign policy and indeed of international relations in general, tend to treat the state as a more or less unitary actor. With the rise of liberal theory and formal modeling, however, increasing attention has been paid to the micro-foundations of state behavior. Specifically, the incentives of elites in power now motivate a number of theoretical frameworks. Probably the most influential of these is Bueno de Mesquita and colleagues' selectorate theory, most thoroughly stated in *The Logic of Political Survival* (Bueno de Mesquita et al. 2005). Selectorate theory sprawls, covering topics in domestic and international politics. A more specialized theory has been developed over the last decade by McGillivray and Smith (2000). Their leader-specific punishment (LSP) model

speaks directly to matters of foreign policy. Below, I review the logic and contributions of both these models, and locate space for further development.

Selectorate Theory

Bueno de Mesquite et al.'s (2005) popular expected utility model of leaders' political incentives begins with two essential assumptions. First, decision-making within a state is carried out by a rational actor (or a group which behaves as one) whose primary goal is to stay in power. Second, that actor relies upon a key portion of the population – a winning coalition – the size of which is determined by institutional rules.[15] Whether this constituency includes a handful of key military-political/commercial figures or a plurality of adult citizens affects both the type of policies leaders provide and the freedom leaders enjoy in determining policies to maximize personal benefit (Bueno de Mesquita et al. 2005). Survival-driven leaders work to please their winning coalition, because their probability of staying in power depends on the satisfaction of these people. The current leader always faces challenges. The key to competing is to find the optimal division of state resources amongst public goods, private goods, and personal stockpiles ("slack").

For a large coalition, like that in a democracy, spending considerable portions on private goods is inefficient. Funds are limited and the larger the number across which private goods must be dispersed, the smaller the share for each individual. Public goods constitute the only means to provide respectable benefits to a broad swath of the population. For a small coalition, on the other hand, private goods are highly efficient. Since only a small number of individuals need to be attended to, significant personal gain

[15] More specifically, Bueno de Mesquita et al. (2005, 42-50) define the selectorate as the politically enfranchised portion of the population. These individuals possess the characteristics necessary to participate in the state's executive selection procedure. In the United States, for example, the selectorate includes all citizens of 18 years or older who have not been convicted of a felony. The winning coalition is a subset of the selectorate of the smallest number necessary to ensure the maintenance of executive office in the polity (Bueno de Mesquita et al. 2005, 51).

can be made available to each. Winning coalition members' loyalty to the incumbent derives from their benefit relative to the rest of the population, and the risk of being excluded in the future.

Since public goods are non-rival (i.e. they are enjoyed by all and cannot be restricted) and private goods are rival (i.e. they diminish with consumption and can be restricted), democratic winning coalition members do not benefit as much as autocratic winning coalition members. Also, given the large portion of the population which must be included in a democratic winning coalition, probability of being excluded in the future is much lower than in an autocracy. This disproportionate private benefit combined with the high likelihood of being excluded from future winners' circles in small winning coalition systems engenders a high level of loyalty. A strong "loyalty norm" allows leaders freedom to pursue sub-optimal public spending policies in pursuit of private benefit (Bueno de Mesquita et al. 2005, 93).

Bueno de Mesquita and colleagues have applied the predictions of selectorate theory to a dizzying array of questions in international and comparative politics with considerable success (e.g. Bueno de Mesquita and Smith 2007; Bueno de Mesquita et al. 2005; Bueno de Mesquita, Downs, and Smith 2005; Bueno de Mesquita 1999). In terms of influence attempts, the theory has been applied to the problem of aid-allocation, and to crisis bargaining. Bueno de Mesquita and Smith (2007) argue that foreign aid will be of more ready assistance to leaders with small winning coalitions, leading to a higher rate of allocation to leaders with relatively nondemocratic systems. The implication of this argument are first that nondemocratic leaders benefit more from aid money and second that they are more willing to provide concessions, but empirical tests to date have assessed only whether they receive aid more frequently. Dovetailing with Fearon's (1994) audience costs approach to credible threats, selectorate theory also contends that democratic leaders – responsible to provide public goods – will take firmer stances in conflict and will fight harder to achieve victory (Bueno de Mesquita 1999). Selectorate

theory's empirical record is commendable, especially given its elegance. Tests of the leader specific punishment model discussed next, not surprisingly, have drawn substantially from the insights and operationalizations of the selectorate theory approach (e.g. Bueno de Mesquita and Smith 2007; McGillivray and Smith 2004; McGillivray and Stam 2004).

Leader Specific Punishments

The leader-specific punishment model has been applied to both cooperative and hostile interactions (McGillivray and Smith 2004; McGillivray and Stam 2004; Bueno de Mesquita and Smith 2007). For this reason, it provides a useful framework for thinking about a theory of the effects of political incentives on the outcomes of influence attempts. At the same time, the LSP model exemplifies rather than resolves the extant issues.

The LSP model developed initially as a conceptual refinement of the prisoners' dilemma "grim trigger" strategy in international relations. Usually considered to result in permanent sub-optimal outcomes following a single defection, it can actually become a facilitator of international cooperation. Leader-specific punishments limit the threat of permanent punishment to the leader who defected first rather than the entire state. This facilitates international cooperation in two ways. First, it places a limit on the duration of conflict. Cooperation can be restored without losing face following turnover in administration. If nothing else, the punishment will die with the individual offender. Second, by placing a cap on costs, this strategy allows actors to more credibly commit to punishment, thus eliciting more cooperation from partners (McGillivray and Smith 2006).

LSP strategies operate by fomenting disapproval within the targeted leader's society. Cutting off trade or foreign assistance (or the threat thereof) imposes costs (or the threat thereof) on the population to whom the leader reports.

> The principals can end the punishment and restore cooperation simply by replacing the responsible agent. Hence, against leader-specific policies, citizens have an incentive to remove any wrongdoing agent (McGillivray and Smith 2000, 811).

Provided the costs of ousting leaders does not outweigh the costs of enduring continuous punishment, therefore, the LSP model predicts that failing to comply with external conditions will result in overthrow.

The prior discount is not a small one, though. The effectiveness of LSPs rests on the efficacy of the people. When institutions present an insurmountable hurdle, the people must endure punishment until their leader "exits" for natural rather than political reasons (McGillivray and Smith 2000). A leader who knows this to be the case will not be deterred by the threat of permanent punishment, because he/she knows that the LSP cannot tap into its domestic mechanism of fulfillment (McGillivray and Smith 2000, 2006).

The ability of LSP to trigger domestic replacement mechanisms leads McGillivray and Smith (2000) to posit less frequent and shorter punishments for democratic states. Democratic leaders, fearful of the people's punishment, will avoid incurring international punishment; those who do not will be quickly replaced. Empirical evaluations relating to this proposition return mixed findings. In trade, democratic turnover does not disturb relationships (ostensibly due to the selection process wherein democratic leaders take care not to trigger punishment), while democratic turnover also fails to alter sour relations (McGillivray and Smith 2004; McGillivray and Stam 2004).

Also consistent with LSP, democratic leaders appear more susceptible to the harms of sanctions and more likely to concede. Recent empirical studies demonstrate a strong conditioning effect for regime type on the depression of tenure faced by sanctioned leaders (Marinov 2005) and on the ultimate effectiveness of sanctions (Allen 2008; Cox and Drury 2006; Nooruddin 2002; Bolks and Al-Sawayel 2000). But further results in this literature suggest the need for additional theorizing. Allen (2008) for example demonstrates that considerable variation exists in the behavior of sanctioned autocratic leaders, upon whom the threat of agent-specific punishment is generally considered to be weakest. In further work, Allen (2008b) finds that sanctions ignite the

domestic punishment mechanism of civil unrest most effectively in mixed rather than democratic regimes. More importantly, even the "in-house" evaluation of LSP's predictions regarding sanctions raises serious questions. Abandoning the mechanism posited in theoretical treatments of LSP, McGillivray and Stam (2004) treat turnover as exogenous to international punishment, and lean heavily upon selectorate theory to explain why democratic leadership turnover does not increase the probability of sanctions terminating.[16]

Empirical results relating the agent-specific model to foreign aid also suggest that additional factors beyond electoral accountability must be considered. In the case of foreign aid, the agent-specific punishment model provides the implicit threat necessary to compel concessions following the delivery of funds (McGillivray and Smith 2000, 2006; Bueno de Mesquita and Smith 2007). Should a leader accept resources and fail to follow-through with the requested changes in behavior, the external actor can respond by cutting off funds until the untrustworthy leader has been ousted. The threat of the resources being rescinded makes policy concessions rational (Bueno de Mesquita and Smith 2007). The empirical findings relating strategic concessions in the U.N., however, find it is autocratic leaders – the most insulated from agent-specific punishments, and the most likely to benefit from sanctions rents (Kaempfer and Loewenberg 2000) – who most readily concede to the preferences of powerful donors (Lai and Morey 2006). In regards to the humanitarian aim of economic growth, Wright (2008, 2008b) has located trends in political survival and binding legislatures as further conditioning factors on the effectiveness of aid money sent to autocrats.

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16 Selectorate theory is used to argue that democratic leaders must assemble a large portion of the selectorate in their winning coalition. Since the proportion needed is so high, they expect that there will be considerable overlap between incumbent and challenger coalitions. Therefore, little will change in policy when leadership turns over. Autocrats, on the other hand, can start from scratch because they need only a small portion of the selectorate to stay in power.

Dynamic Processes and Variation in Leader Incentives

With regard to determining the outcomes of foreign policy whether combined or independent, selectorate theory and LSP lead to two problematic abstractions. Treating leaders' incentives as static (equivalent on the first day in office to the last day in office) and dichotomous (democratic or not) makes for blunt predictions of when leaders are actually vulnerable to international pressure. I will discuss each of these in general before moving on to consider them in light of foreign aid and economic sanctions.

First, in practice both selectorate theory and LSP models tend to treat leaders' incentives as stagnant over time. Certainly, intuition and evidence suggest that politics rarely stand still. Leaders' risk of losing power changes over time (Wright 2008; Chiozza and Goemans 2004; Bueno de Mesquita and Siverson 1995; Bienen and Van de Walle 1991), and leaders face different types of pressure at different points in their careers. Even Bueno de Mesquita et al. (2005, 100) acknowledge it should take some time for winning coalitions to solidify and thus for the "logic of political survival" to manifest. Recent empirical investigations have incorporated length of time in office as a theoretical explanation. The literature, for example, argues alternatively that new leaders are more likely to be the target of international hostility due to their higher likelihood of concession (Gelpi and Grieco 2001), or that newer leaders are more likely to respond aggressively because of their more frequent targeting (Wolford 2007).[17] Chiozza and Choi (2003) also posit that length of tenure may affect the incentives of leaders; experience, legitimacy and reputation provide a buffer allowing more difficult undertakings from which newer leaders would shy away. They consider conciliatory behavior in territorial disputes an apt example of such difficult undertakings and do find support for the idea that better established leaders more frequently initiate peaceful

[17] On the general war proneness of mixed regime and transitioning states, see Mansfield and Snyder (1995, 2002) and Snyder (2005).

resolution.[18] Most pertinent to the project at hand and perhaps the most sophisticated approach to this problem, Wright (2008) considers the impact of survival probability on the likelihood of autocratic leaders expropriating aid monies to private purposes vs. funneling them into growth-producing endeavors.

Survival-driven leaders will be responsive to changes in their prospects of survival in office. If being the target of foreign policy poses different risks/benefits to leaders over the course of their careers, we should see commensurate changes in their willingness to provide policy concessions to outside actors. We must acknowledge that dynamic processes of learning and institutionalization will produce variation in targeted leaders' sensitivity to external threat. New leaders may be less able to protect their winning coalitions from external harm or to grease the wheels of bureaucracy necessary to produce concessions. Established leaders may have accumulated enough political capital and skill to flout both international and domestic approval, withstanding external threats by fomenting nationalist response or providing costly concessions over the objections of a less competent opposition. The costliness of each type of behavior, however, depends on more than just how long a leader has been in charge.

The second major distortion in both LSP and selectorate theories is the tendency to categorize leaders' incentives dichotomously. Selectorate theory technically presents an ordinal measure of winning coalition size, *W*, but in practice it is usually interpreted dichotomously. The construction of the scale does not produce a true interval measure, making analysis of small changes in score conceptually difficult. In application, then, expectations tend to boil down to a dichotomy: big winning coalition (democratic

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[18] Horowitz, McDermott and Stam (2005) find escalatory behavior to increase with age, but age does not correlate perfectly with time in office. Colaresi (2004, 564), however, finds that punishment for over-cooperating in the context of rivalry, follows a parabolic relationship to leader experience, initially increasing to a peak around seven or eight years and then quickly decreasing.

accountability) equals high provision of public benefit (trustworthy partner); small winning coalition (no accountability), high provision of private benefit (untrustworthy partner). This is certainly part of the story, as empirical results demonstrate. Yet, if pleasing a winning coalition were this simple, we would observe two uniform blocks of behavior, with all large-coalition democracies and all small coalition autocracies behaving equivalently. In overly simple terms, we should see democracies providing concessions and autocracies resisting them. However, we see democracies resisting strategic concessions in response to foreign aid, but giving concessions in response to economic sanctions; autocratic leaders offering up U.N. votes in response to aid, but displaying wide variation in response to sanctions.

Measurement issues aside, political reality consists of considerably more than a loyal winning coalition. The very existence of such a group implies an analog, a "losing coalition" of disaffected individuals. Within selectorate theory, the ever present "challenger" threatens to poach members of the incumbent coalition to cobble together an alternative winning coalition composed largely of previous "losers". This challenger places a limit on the behavior of incumbents, preventing them in low loyalty systems from pursuing sub-optimal policies. The power of the opposition, however, is not entirely described by the size of the winning coalition. They often exert influence far beyond what would be expected. The threat of their mobilizing capacity gives leaders of all stripes pause. The actual viability of governing from a tiny winning coalition does not mesh with the existence of authoritarian institutions and power-sharing agreements (e.g. Gandhi and Przeworksi 2006, 2007).

Which benefit should a democratic leader pursue in order to please his/her winning coalition: friendly trade relations or a reputation for toughness? Which strategy keeps challengers to the autocratic regime at bay: rogue state status or close ties to outside powers? These questions can be answered neither by theories which treat democracy and autocracy as two homogenous groups, nor by those which treat external

demands as homogenous. We must pursue, then, a framework which places targeted leaders in their proper context, as decision makers facing demands from "above" and "below". The independent and joint impact of variation below the surface of regime type as well as in the demands of external actors must be considered. In the following section, I present such a framework. The success of influence attempts depends on their ability both to directly impact a leader's survival motive and on the pattern of costs which the demands stand to inflict upon the targeted society. Leaders weigh the immediate and future costs of influence attempts on their ability to maintain power in light of competing political groups, temporal political dynamics and the two-fold impact of external intervention upon these domestic factors.

CHAPTER 3

PRIVATE INTERESTS, PUBLIC OUTCOMES: DETERMINANTS AND EFFECTS OF POLITICAL INCENTIVES

Cold War theories of power focus on the properties of the sender state and the type of influence attempt; new theories of leader incentives emphasize mechanisms of domestic accountability within targets. Further understanding of the success and failure of foreign policy requires integration. External influence attempts – whether in the form of positive or negative sanctions – aim to alter targets' domestic policy. These external demands upon targeted leaders must be weighed against domestic situations, which make policy change more or less difficult. Under some situations, internal and external pressure will coincide; often, they will tug in opposite directions. The theory developed below pursues an understanding of the domestic conditions under which targeted leaders are vulnerable to different types of influence attempts.

This project requires a more nuanced approach to the role of domestic political considerations, and consideration of the interplay between these and the outside power's interference. Domestic situations interact with external intervention to shape not only the ability of punishments (rewards) to hurt (help) leaders, but also difficulty of policy change. Though we usually focus on the ability of external powers to inflict costs or benefits through intervention, the demands they make present additional costs. Changing policy, if nothing else, presents a bureaucratic hassle. It can also involve loss of face, social upheaval, and focused costs for important groups. The kind of foreign policy tool and the type of concession matter. But we must also consider how the incentives of leaders change with the proportion of the population to which they are held accountable, the strength of the political opposition to mobilize and contest policy, and the leader's own level of experience and security. Considering the interaction of these aspects of domestic politics with external interventions should enable the development of expectations regarding when and how targeted leaders' incentives will affect the success

of foreign policy strategies. Once enumerated, hypotheses regarding the conditions under which targeted leaders' incentives pull them into line with external powers should be matched with empirical tests of those *leaders' incentives* rather than *regime types*.[19]

In order to elaborate on the particularities of domestic conditions and international relations without losing track of generalizability, we should begin with a clear set of assumptions and track our expectations from there.

A Theory of Leader Incentives from the Inside Out

This project shares two foundational assumptions with the leader-specific punishment (LSP) and selectorate theories. To begin, leaders' decision-making process is rational and instrumental, aimed at keeping their jobs. Leaders are, in other words, survival-driven, acting so as to hang onto power as long as possible. While some find this an excessively pessimistic point of view, we must keep two things in mind. First, because leaders operate within the confines of institutions of government and mores of society rather than a vacuum, this assumption need not lead us to expect *every* leader to rob the people blind. True, there is a subset for which this is the case. But, an increasing portion of elites in the world work under the constraints of institutions designed to "pit ambition against ambition" (Madison [1787] 2001). Second, even adherents to a policy-driven model of decision-making must acknowledge that the leader who willingly self-destructs must be a rare beast. In most cases, one's political agenda profits more from continued control of policy rather than a fall from grace.

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[19] Wright (2008), for example, comes close to performing a true test of leader incentives rather than proxying by regime type. He generates a measure of predicted survival probability for autocratic leaders to test the hypothesis that such elites use aid differently depending on their security in office. I propose that the idea could be modeled even more directly by generating predicted values for leaders' expected costs (from hostile foreign policy) and benefits (from friendly foreign policy) based on the more nuanced theory of political incentives' role in the efficacy of policy strategies.

Also, I assume leaders rely on some portion of the population for their continued tenure in office. This assumption ranks among the central tenets of a liberal theory of international relations as codified by Moravcsik (2003, 518), and of course undergirds selectorate theory's primary theoretical mechanisms. For most leaders, survival-motivated behavior involves pleasing someone other than themselves. The size of this group relative to the rest of the population affects leaders' general policy orientation, structures acceptable political behaviors and changes the freedom leaders enjoy in setting (publically) sub-optimal policies. Winning coalitions of theoretically equal size are not necessarily equal in practice, however. The existence of a winning coalition suggests also the existence of a "losing coalition". In open societies this may be a coherent and organized opposition with institutional standing; in closed, a guerilla insurrection or a co-opted political movement. Under some conditions, losing coalitions are in a position to make demands upon the survival-driven leader. If they hold real institutional power and they are strong, leaders would do well not to ignore those who may make up the next challenger's winning coalition.[20]

Departing now from prior rational decision making models, I assume dynamic processes alter leader's security in office over time. In general, dynamics such as socialization, institutionalization, and learning have been presented as generators of change (e.g. Finnemore and Sikkink 1998; Box-Steffensmeier and Zorn 2001; Box-Steffensmeier, Reiter and Zorn 2003). For leaders, these processes generate changing risks over time. The same choice, emergent early in a leader's career may present a hurdle greater than if it were to surface later. Institutionalization and learning processes

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[20] This assertion holds even inside autocratic legislatures (Wright 2008b). Weeks (2008) has found that the opposition in many autocratic states can function to produce audience costs equal to those of democratic leaders.

generate this difference, affecting leaders' incentives in two ways. With time, both leaders' need and ability to effectively serve the interests of their winning coalitions change. An institutionalization process drives the dynamics of the "need to please", while learning accounts for ability to produce returns. These processes may produce the empirically noted tendency for leaders' declining baseline risk of losing office over time (Chiozza and Goemans 2004; Bueno de Mesquita and Siverson 1995; Bienen and Van de Walle 1991). Of course, every leader follows a different path through time: some may be remarkably successful at first and encounter an exogenous shock which decreases their productivity; others may be hopeless from the start and remain that way. Here, I focus on general processes which should underlie the story of any particular leader. Let us consider the "need to please" and learning effects in further detail.

Institutionalization can be thought of as producing changes in the "need to please" one's winning coalition over time. Political loyalty changes over time and its dynamic process differs across regimes. For autocratic leaders, it takes time for confidence and loyalty to build due to irregular means of turnover. Newly instated nondemocratic leaders *are* the challenger described by Bueno de Mesquita et al. (2005, 100) as unable to firmly demonstrate sufficient "affinity" for initial winning coalition members. With this shaky foundation, nondemocratic leaders face an initially very high "need to please". New autocratic leaders must spend more and more broadly in order to prevent coups and insurrection. If successful in maintaining support through the initial instability, however, the job security benefits of the small winning coalition system kick in. The high relative benefits earned by winning coalition members coupled with high likelihood of being rejected from membership in challengers' winning circle produce increasing loyalty over time. As loyalty firms up the "need to please" wanes and nondemocratic leaders acquire what Bueno de Mesquita et al. (2005) refer to as "slack", room to pursue individual benefit at the margins.

The pattern of winning coalition institutionalization differs in democratic systems. The process of political campaign and election, essentially an exercise in the mobilization and cementing of political coalitions, fuels this dynamic. Victory provides an intense burst of energy and cohesion. The popularity of elected officials is at its highest while the public remains under the influence of this feeling. During this "honeymoon" the competition finds it socially distasteful to comment negatively and mainstream media, consequently, present almost entirely positive evaluations of the new leader/government (Brody 1991, 27-44; Lockerbie, Borreli and Hedger 1998). The "need to please" for new democratic leaders, then, is relatively low. Higher levels of support and lower levels of criticism leave considerable space for political maneuver. This favorable climate does not last. As time progresses, public approval lags, opening the door for legal political competition to further wear away at that support; political actors, finding their share of policy goods or power unsatisfying, criticize incumbent policy choices and performance (Altman 2000; Brace and Hinckley 1992, 21-44 and 60-74; Light 1999, 36). In both presidential and coalition systems, these dynamics contribute to an increasing hazard of failure for democratically elected leaders (Warwick 1992; Altman 2000). Maintaining enough support to continue business after the honeymoon requires democratic leaders to work harder for their constituents and for the opposition. Thus, the "need to please" will be increasing over time for democratic leaders.

Aside from the temporal dynamics of loyalty, we should also expect leaders' simple ability to accomplish tasks to change over time. As they become familiar with the process of bargaining and delegating, with the nature of bureaucracy and the demands of appearances, leaders become more capable and efficient. Early in their careers, leaders may fail to serve their coalitions' interests out of incompetence rather than lack of desire. They may not be able to intuit the appropriate course of action when faced with a difficult choice, or they may lack the political capital to follow-through with what they know to be right. Grossly incompetent leaders, in the aggregate, should be removed or otherwise

interfered with fairly early in the game. This tendency will further exaggerate the increasing trend of competence over time.

Fourth, I assume ruling elites face demands from external powers. These outside actors make their demands known in the form of threats or promises and are capable of following such messages with punishments or rewards. Through interventions, external actors seek concessions from targeted states. Their demands may be strategic or humanitarian, involving either a change of existing policy (compellence) or the foregoing of an action which might otherwise be in their interest (deterrence). Their influence attempts can impose inequitably distributed costs/benefits upon the target populations at two stages. First, the threatened, promised, or actualized foreign policy intervention holds independent potential to impact the target. Second, the demanded concessions entail costs of their own.

Some costs tend to focus on particular sub-groups (e.g. a privileged minority, a business class, etc.); others inflict public bads, felt more by the poor (e.g. food shortages, decrease in security). In parallel, some benefits accrue to a minority and others to the public in general. An offer to increase trade levels, for example, proffers significant public benefit over time as overall economic growth increases. This benefit typifies the first means by which the external power may impact the target (i.e. through the actual foreign policy tool). If that offer were made contingent upon the elimination of domestic barriers to trade, the required changes in policy will most likely involve short-term, focused costs for those industries which have been sheltered by the status quo terms of trade. This cost, engendered by the concession rather than the actual external action, exemplifies the second type of impact.

The non-random impact of the actual or promised punishment/reward contributes significantly to the ability of the sanction to produce concessions. Sanctions which can successfully target influential portions of the population hold the potential to ignite the leader-specific punishment mechanism of domestic outrage. For a survival-driven leader,

the mechanism may also hold even if the sanction is a positive one. Powerful interests who benefit from the external action may tend to agitate in favor of the external actor's preferences. Concession agreements may hold legal clout or create a focal point around which the opposition could organize. A leader whose base of support is significantly impacted by the actions of an external power should therefore be pre-disposed to deliver concessions. But the costs of the externally requested changes also disperse inequitably amongst domestic groups. Some concessions cost more than others. Even leaders predisposed to give the external power what it wants may find certain concessions politically untenable.

To summarize, the discussion above enumerated four assumptions:

1. Leaders are survival-driven;

2. Leaders depend upon some sub-group of the population for their survival and face domestic competition;

3. Dynamic processes affect leaders' security and capability;

4. External actors make demands upon leaders, enforceable by inflicting costs or bestowing benefits on the targeted population.

Under this rubric, leaders targeted by a foreign policy action must consider two sets of costs: the likely personal costs/benefits (in survival terms) of the punishment/reward, and the potentially resistance-mobilizing costs of concessionary policy change. Foreign policy interventions operate by altering the domestic political arena. The incentive to give in or hold out will be conditioned both by the ability of the influence attempt to successfully penetrate the leadership and by the additional pain forthcoming should the policy changes be made. Each of these aspects of the influence process will be developed independently below, leading to hypotheses regarding the general effectiveness of influence attempts and the role of domestic conditions in securing concessions.

Tapping into the Survival-Motive: Foreign Policy Effectiveness

The survival-driven leader thinks first and foremost about survival. Foreign policy strategies with potential to successfully tap into this primary motive, either by helping or hurting, provide the sending state with increasing leverage over that leader. The ability of a policy tool to significantly impact the targeted leader's survival motive will be referred to here as "effectiveness". An effective influence attempt is one which would alter, in the intended direction, the probability of a targeted leader retaining office. Negative sanctions communicate disapproval, hostility and conflict; positive ones, approval, warmth and cooperation (Baldwin 1985, 135-6). In terms of leader survival, then, punishments and threats thereof intend to destabilize and endanger, while rewards and the promise thereof offer to make leaders' lives easier.

Specific influence attempts' effectiveness will vary by the dispersion of benefits (costs) within the target. In order to tap the survival motive, the costs (benefits) of the punishment (reward) must hit the targeted society where it counts. Changing the behavior of survival-motivated leaders requires significant impact upon the winning coalition. Some influence attempts accomplish this, and others do not. Effectiveness depends in part on the nature of the foreign policy tool itself, but the role of target institutions and leader experience must be considered as well.

It is important to remember that influence attempts operate through the ignition of a domestic process within the targeted state (McGillivray and Smith 2000). Their success depends upon the domestic institutions through which they must filter. Determining the effectiveness of a foreign policy tool requires analysis of how the raw nature of the action translates into an effect on targeted elite incentives. 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 sender.

As highlighted by the LSP model described in the literature review above, tactics which twist a target's arm through manipulation of domestic accountability are subject to conditioning by the efficacy of domestic accountability (McGillivray and Smith 2000). Foreign policy tools may exert a real effect upon the targeted population, but still be off-target if the winning coalition is sheltered from the impact. We must ask, therefore, 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.[21]

Beneath the overall conditioning effects of institutions, temporal dynamics factor into the sensitivity of leaders to external intervention. Learning and institutionalization may both contribute. Because institutionalization patterns differ across regime type, however, these two dynamic social processes will not necessarily contribute equivalently to effectiveness. I will begin by discussing the general contribution of learning effects, which are more straightforward. The likely role played by the "need to please", with its shifts across regime type, will be examined second.

Over time, leaders learn how to balance the demands of external powers and internal groups. They find means of sheltering vital supporters from the pain of outside interference, but also how to leverage outside demands against internal resistance. In short, they become better at their jobs. When targeted by hostile interference, an experienced leader should be better able to direct domestic outrage towards the external power. One less experienced may be able neither to dampen the harm imposed by negative sanctions nor to convince key supporters that the outside power should be

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[21] A recognition of this statement has emerged in recent empirical studies of both foreign aid and economic sanctions (e.g. Marinov 2005; Allen 2008,2008b; Lai and Morey 2006; Bueno de Mesquita and Smith 2007). I contribute to this new vein by presenting an integrated argument for effectiveness across positive and negative sanctions and by considering the additional impact of dynamic processes.

blamed for the pain. Negative sanctions, then, may be more effective when aimed at new leaders.

Positive sanctions, on the other hand, may be better targeted at experienced elites. The same increasing ability to "work" the external intervention over time applies to friendly gestures. A better knowledge of the political landscape and bureaucratic institutions increases the likelihood that a leader can funnel foreign assistance (whether it be money, goods, or good will) into benefits for key supporters. The effectiveness of positive sanctions, then, should increase when targeted at more experienced leaders.

The temporal dynamics of winning coalition loyalty may also condition the effectiveness of external influence attempts. Democratic winning coalitions cohere best early on, when even the opposition is unwilling to find fault with new leaders' actions. Autocratic winning coalitions, on the other hand, start out shaky and become more secure over time. Consequently, when considering the solidity of winning coalitions rather than the competence of leaders, the overtime conditioning of foreign policy targeting will differ by regime as well as sanction type.

Holding other important variables constant, a cohesive group should stand up better to negative sanctions and require fewer positive sanctions to maintain support. With the same qualifier, a shaky winning coalition should react more to positive and negative external cues which inform them of their leaders' competency. Rather than directly determining effectiveness, however, the solidity of a winning coalition will emphasize the pattern of the institutional conditioning of punishments and rewards. Members of the winning coalition, in other words, will place heavier weight on how their leaders manage the external intervention than on the intervention itself. International support and rewards, for example, will mean little to a disaffected coalition if they do not translate into real benefits. Likewise, international punishments will not further deplete support if they do not hurt the winning coalition.

For the time being, the most important aspect of temporal dynamics is that learning and institutionalization processes produce competing expectations. Learning should create a uniform pattern of resistance to negative sanctions and increasing benefit from positive sanctions over time. Institutionalization patterns for winning coalitions should generate more finicky temporal dynamics, with differences across regime type and specific policy tool.

Influence attempts, importantly, include not only actual actions – rewards and punishments – but also the threat and promise thereof. Their success depends in part on whether they impose contemporary effects or allude to future ones. External actors prefer to threaten and promise before punishing and rewarding. The alluring prospect of achieving influence on the cheap, however, creates incentives for even states unwilling to actually follow through to level threats to that effect (Schwebach 2000). Influence attempts which allude to future action rather than taking it, therefore, may inspire skepticism in the target. The effectiveness of threats and promises, then, must be discounted by the probability of follow-through from the sending state.[22]

This discussion has emphasized the importance of three concepts for determining the effectiveness of a foreign policy intervention, in terms of affecting leader survival: actual vs. threatened action, the conditioning impact of institutions of representation, and temporal political dynamics. The posited relationship between these factors and effectiveness may be more clearly delineated if represented mathematically, as in the equations below.[23]

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[22] An extensive literature, much of it formal, centers on the credibility of threats and promises in international politics. Democratic senders, with their ability to generate audience costs, endure embarrassment when they back down and are thus more able to send credible signals (e.g. Fearon 1995). Acting through an international organization also helps by providing a means of relinquishing the initiative (e.g. Schelling [1966] 1980).

[23] The mathematical expressions presented below function to clarify the theoretical argument, *not* to preview the form of the estimating function which will be used to test the hypotheses. Effectiveness itself cannot be directly observed; it must be modeled. In the

$$(E|s_n) = c_n(-I_n) - f(t_i) +/- f(t_i)(-I_n) \quad \text{(Eq. 3.1)}$$

$$(E|s_p) = c_p(I_p) + f(t_i) +/- f(t_i)(I_p) \quad \text{(Eq. 3.1.2)}$$

In equations 3.1 and 3.1.2, effectiveness, *E*, is the extent to which a sanction imposes the intended change in probability of survival. The subscript "n" denotes a negative sanction; "p" a positive one. The effectiveness of a negative sanction, $(E|s_n)$, increases as the probability of a targeted leader staying in office decreases. For positive sanctions, however, effectiveness, $(E|s_p)$, increases as the probability of a targeted leader staying in office increases. Effectiveness, as represented here, is a continuous variable ranging, theoretically, from positive to negative infinity. Figure 3.1 displays this possible range. When effectiveness equals 0, the sanction has no effect on the targeted leader's survival. A high positive effectiveness score indicates that a negative sanction contributes to a large decrease in the leader's probability of survival and that a positive sanction contributes to a large increase in the leader's probability of survival. Sanctions which receive a negative effectiveness score have "backfired", producing a real but undesired change in the targeted leader's security. Here a positive sanction would accidentally destabilize a leader, while a negative sanction provides unintended succor. In these situations it is appropriate to call the sanctions ineffective; though they exert an impact (effect in statistical language) they are likely to be useless or even counterproductive as tools of influence.

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equations below, effectiveness itself is the dependent variable. Experience contributes directly to effectiveness when presented this way. When estimating effectiveness, the dependent variable will be the probability of a leader failing at a given point of time. This probability will be a function of the manner of sanction, its institution-conditioned impact, and the leader's experience. In the necessarily indirect statistical evaluations of this argument, experience becomes an additional conditioning factor on the impact of sanctions on leader survival and must be modeled as such. Temporal dynamics of institutionalization of the winning coalition, also modeled as an interaction between sanctions impact and time, thus cannot be directly disentangled from experience. The relative power of the dynamics can be assessed by comparing patterns across regime type. If time has the same effect regardless of regime, then experience exerts a stronger influence; if time has different effects across regime, then loyalty dynamics exert a stronger influence. This issue will be discussed more thoroughly in the research design.

Returning now to Equations 3.1 and 3.1.2, the *c* terms, bounded by 0 and 1, discount the magnitude of costs/benefits accruing from the foreign policy action according to the credibility of the threats or promises made. Under a completely incredible threat, for example, c_n equals zero. As the sincerity of purpose on the part of the sender increases, they take on a value of 1.

The term *I* represents the impact of a sanction on leader i's survival, as conditioned by the institutions of representation in the target country. A positive value of *I* indicates increasing survival probabilities; a negative value, decreasing probabilities. For a negative sanction attempt to be effective, this quantity must be negative (see Figure 1). Thus when assessing the effectiveness of a negative sanction, the institution conditioned effect of the sanction attempt must be multiplied by negative one. This ensures that a positive value of *I* detracts from effectiveness, while a negative value contributes to effectiveness.

Additionally, the effectiveness of the intervention is affected by the amount of time the targeted leader has been in office, t_i. Time in office contributes in two ways: independently as a measure of experience, and interactively as a measure of institutionalization of loyalty. Each additional year need not contribute equally to these effects. If institutionalization and learning matter, the initial years in office should be especially formative. Time does not contribute linearly to the effectiveness of sanctions. Rather, the raw number of years/days a leader has served in office will be mediated by some function $f(t_i)$.[24] For negative sanction attempts, greater experience detracts from effectiveness, as indicated by the negative sign preceding $f(t_i)$ in Equation 3.1. Experience in office increases the effectiveness of positive sanctions as leaders learn how

[24] The most intuitive form of this function would be log-linear to account for the plausible expectation that there will be diminishing returns for experience. The relationship could, however, be more complicated. Before running final models, I will assess the relative fit of models utilizing log-linear, quadratic and cubic functions of experience.

best to exploit international approval for domestic benefit. In Equation 3.1.2, then, $f(t_i)$ is allowed to positively impact effectiveness. The interaction between I and $f(t_i)$ indicates the role of winning coalition loyalty on the effectiveness of sanctions. As noted earlier, the pattern of winning coalition cohesion differs across democratic and nondemocratic systems and its conditioning impact will depend on whether the sanctions are well-targeted at the winning coalition in the first place. The signage for this term, then, may be positive or negative in different situations.

Take, as an illustrative case, the use of threatened economic sanctions in an effort to influence a dictator. The effectiveness of a threat is assessed by predicting the costs of the punishment, (I_n), and discounting by the likelihood of follow through, c_n. The targeted dictator, then, considers the likely impact of sanctions on his winning coalition, which constitutes a narrow, elite portion of the population. Economic sanctions tend to produce public costs (e.g. 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). Responsible only to a small winning coalition made up of powerful elites, nondemocratic leaders generally manage to avoid punishment for these diffuse costs. Public choice, another theoretical framework which disaggregates actors within the state, also notes the possibility of nondemocratic governments directly benefiting from the economic distortions created by sanctions events. Nondemocratic leaders may find themselves better off due to a general weakening of the population through hunger or deprivation, the cooptation of "rents" by monopolizing the supply of suddenly scarce goods (Kaempfer and Lowenberg 1999, 2000), or the evisceration of formerly powerful groups whose usual business is disrupted (Rowe 2000). Because economic sanctions create public costs, against which dictators are insulated and from which they may actually benefit, the impact of this foreign policy tool on the targeted leader's survival prospects, (I_n), will likely either wash out or take on a positive sign. Discount this effect by the probability that the threat may not be followed up, c_n, and the

prospective effectiveness of the influence attempt falls even further. Finally, the temporal dynamics in this case will exert effects in the same direction. The experience of the targeted leader in office (t_i), further detracts from the effectiveness of threatened sanctions. The increasing stability of autocratic winning coalitions over time, coupled with the low baseline effectiveness (I_n), will also drag down effectiveness over time.

Let us examine, also, what happens when we shift key elements in the equation. First, alter the influence attempt from threatened to enacted economic sanctions. This pushes the value of c_n, eliminating a portion of the drag on effectiveness. Beyond this, however, the prospects for economic sanctions to effectively destabilize the targeted dictator remain low, given the small negative sign of $-(I_n)$.

In order to affect a serious change in the effectiveness of the sanction, we must find conditions under which (I_n) takes on a large negative value. When the impact, (I_n), is destabilizing for the targeted leader, the sanction's effectiveness increases. As noted in the theoretical discussion and the review of previous work above, target regime characteristics present a likely means of shifting the value of (I_n) (e.g. Lai and Morey 2006; Allen 2008, 2008b). Consider, then, the effect of targeting a democratic leader with the threat of sanctions rather than the dictator. Responsible to a much larger swathe of the population, it is unlikely that a democratic leader will consider their winning coalition immune to the public costs of economic sanctions. If the threatened punishment were to be enacted, then, the democratic target would expect to pay some costs in terms of likely survival as the population endures pain due to his/her perceived misbehavior in the international arena. The value of $-(I_n)$ thus would be large and positive due to better targeting of the winning coalition. This high base-level of effectiveness must be discounted first by the probability that the sending state is bluffing (c_n). Then the experience of the leader in office (t_i) may provide a means of avoiding these domestic costs, perhaps through a nationalistic framing or clever macroeconomic response. Longer time in office may not be a shield for the democratic leader, however. Given the

breakdown of political loyalty over time in such systems, a long-serving democratic leader may actually face sharper criticism from the population for inflicting public costs. The interaction between $-(I_n)$ and $f(t_i)$ then should take a positive value, increasing the effectiveness of the threatened economic sanctions. Overall, then, the expected effectiveness of threatened sanctions should be much higher when targeted against a democratic as opposed to a nondemocratic leader.

We should also consider an example of positive sanctions, the effectiveness of which is modeled by Equation 3.1.2. To begin, an external power sends foreign aid to a nondemocratic leader. This leader must consider the potential impact of this policy action on his/her ability to hold onto office (I_p). In principle, aid money is often designated by donors for specific uses. In practice, however, research and theory suggest that these external resources are at least partially fungible, opening up room in the budget for leaders to further pursue their pre-existing strategies (Olson 1971; Pack and Pack 1993; Boone 1995; Feyzioglu et al. 1998; Goldsmith 2001; Morrison 2007, 2009). To the extent that aid can be directed towards leaders' strategic interests, it should have a positive effect on their ability to maintain office. The magnitude of (I_p), however, depends in large part on the domestic institutions. With a small winning coalition and limited oversight, the autocratic leader is likely able to turn aid dollars into significant private perks for winning coalition members (e.g. Lai and Morey 2006; Bueno de Mesquita and Smith 2007; Smith 2008). This translates to a large positive value for (I_p) contributing to effectiveness. Because the aid money has been sent, c_p equals one, allowing (I_p) to act unmitigated by uncertainty. The effectiveness of the foreign aid also increases with the experience of the nondemocratic leader, $f(t_i)$. The more capable the leader, the more likely they are to efficiently convert the official assistance into private perks for supporters. Nondemocratic winning coalitions become more loyal over time. This raises a question as to the sign of the interaction between I_p and $f(t_i)$ in this case. If loyalty increases over time of what use will additional aid dollars be? The answer, I

believe, is that the trend of increasing loyalty is intimately linked to the expectation of substantial perks in the future. As each member becomes more certain of a continuing place in the winners' circle, the future earnings expected from diverted aid monies further entrenches loyalty. During the initial transition, however, these extra dollars the higher stakes provided by additional available funds may encourage higher competition and coax particularly uncertain members into the camp of a challenger. Thus, the sign for the interaction term here should be positive; the greater the individual benefits of coalition membership, the more loyal members will become over time.

Under different conditions, however, the value of (I_p) for foreign aid decreases. Aid money provides stabilization for autocratic leaders only because it smoothly augments their existing strategies of bribery, patronism and stockpiling. With a broad winning coalition and high levels of transparency, democratic leaders cannot convert aid into private perks for supporters. There would not be enough to go around, and the scandal of appropriating public funds for private use makes the attempt a dangerous prospect. They must instead hope that foreign aid manages to produce some public goods, but we know empirically that its ability to deliver such gains is highly conditional (e.g. Chenery and Strout 1966; Burnside and Dollar 2000, 2004; Islam 2002; Chauvet and Guillaumont 2002; Daalgard et al. 2004; Kosack and Tobin 2005). Even if the macroeconomic, social and geographic structures conform to ideal conditions for aid productivity, the individual benefits produced by an overall gain in public goods provision are generally smaller than those available from private goods (e.g. Bueno de Mesquita et. al 2005). Aid money does not convert directly into the currency of power within democratic regimes. The magnitude of (I_p) for the democratic leader, then, should be considerably smaller than that of an autocratic recipient. The condition could be improved by experience, as leaders learn how to work the system over time. A sufficiently large amount of experience could recover some of this deficit through the positive function $f(t_i)$. The pattern of loyalty deterioration will likely trump the positive

impact of experience, however. The population may be willing to interpret the winning of foreign aid as a sign of their new leader's competence, but the likelihood of observing any tangible effects over time is low. As the cycle of political competition ramps up over the course of time, democratic leaders who cannot provide substantial returns from aid could become targets for the media and the opposition. The symbolic support embodied in development assistance fades in the voters' minds while the opposition promises to provide tangible benefits. The interaction term in Equation 3.1.2 for a democratic leader takes a negative sign, further decreasing the effectiveness of foreign aid.

We may pin these terms to real world examples, as well. Consider the perhaps odd juxtaposition of Pervez Musharraf (president of Pakistan 2001-2007) and Mohammad Reza Pahlavi (Shah of Iran 1953-1979). Both leaders governed populations with strong radical Islamic tendencies while attempting to maintain tight ties with the United States; both leaders received large grants of military and economic U.S. aid to foster friendly, secular regimes in key strategic locations.[25] The career trajectories of the leaders, however, suggest that the impact of that aid money (I_p) differed significantly. The ability of the Shah to turn U.S. aid money and materials into direct benefit for a small number of influential supporters has been well-documented (Gasiorowski 1991). This stream of external support allowed the Shah to perch atop a fundamentally unstable situation for nearly 30 years. This longevity places the Shah in the top 10% of even the most autocratic leaders in history.[26] Given historical accounts of his utilization of foreign aid and this fact, it seems safe to assume that the magnitude of (I_p) for the Shah

[25] Of course, the Shah came to power by the direct machinations of the United States and the United Kingdom, a fact acknowledged by the U.S. government (U.S. State Department 2008).

[26] This figure obtained from the density of failure times of autocratic leaders, using the database Archigos 2.9, which contains data on leader tenure from 1800-2004, and a cut point of -5 on the Polity IV democracy-autocracy index (Goemans, Gleditsch and Chiozza 2009; Marshall and Jaggers 2004).

was very large. President Musharraf's rule deteriorated much more quickly, despite heavy grants of military and economic assistance (U.S. State Department 2008b). Though Musharraf's rise to power can hardly be called exemplary in terms of democratic values, he remained throughout his tenure far more accountable to the opposition and the people at large. Political controversy plagued his re-election campaign, and lingered even after he secured victory (e.g. Masood 2007; Gall 2007; Perlez 2008). Externally provided public goods may have benefited the military to which Musharraf originally owed his office, but it did not help him solve the political issues which would have satisfied the broader audience to which the constitution held him accountable. Aid thus could not silence the rising general dissatisfaction which preceded his replacement in November of 2007, suggesting a low value of (I_p) consistent with the abstract argument made above.

These illustrations highlight some general hypotheses regarding the effectiveness of influence attempts against leaders.

H1a: Leaders' experience in office decreases the effectiveness of negative sanctions and increases the effectiveness of positive sanctions.

H1b: Patterns of loyalty condition effectiveness differently across regime type and sanction type.

H2: Sanctions aimed at the winning coalition of targeted leaders will be more effective.

H3: Enacted sanctions will be more effective than threatened/promised sanctions.[27]

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[27] Superficially, this hypothesis appears to contradict the prevailing wisdom stemming from formal models of economic sanctions, namely that those likely to work will work in the threat stage. Effectiveness here refers only to the ability of an influence attempt to significantly affect the targeted leader's survival prospects, not to its ability to produce concessions. In this sense, the hypothesis should not be controversial. Moreover, a formal argument by Hovi, Huseby and Sprinz (2005) suggests that enacted sanctions can be successful if they disconfirm the target's

Generally, positive sanctions will increase in effectiveness over a leader's time in office, while negative ones decrease it. But this basic temporal dynamic must compete with the more complicated implications of winning coalition loyalty. Also, sanctions aimed at the portion of the population to which a leader is most accountable likely tap the survival motive more effectively. Influence attempts which would not be effectively leveled as threats may be more effective if actually enacted. These hypotheses unify expectations about the impact of foreign policy actions on leader survival across positive and negative sanctions, while incorporating the political reality of dynamic processes. This aspect of influence attempts, however, constitutes only one piece of the puzzle. Most leaders do not enjoy the freedom to react unilaterally to an external intervention, effective or not. Concessions cost. They require politicking, alteration of the status quo. If we wish to account for remaining variance in the success of sanctions in achieving concessions, though, we must consider more than simple effectiveness. Targeted leaders must balance the expected costs/benefits of sanctioning in terms of survival against those which would be incurred by providing concessions.

Surmounting Domestic Hurdles: The Political Costs of Concession

The non-randomly dispersing effects of concessionary policy change generate the second set of costs vital to the targeted leader's choice. External powers embark on influence attempts with many different goals in mind. Sometimes the aim is deterrent – "respect sovereign borders, *or else*…" – sometimes compellent – "withdraw your troops, *or else*…". The costliness of providing the concessions demanded through deterrence is generally considered to be lower than giving in to a compellent threat (Schelling [1966]

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belief that a) the sender will not follow through on the threat, or b) sanctions will not actually "hurt". In such cases, enactment will also make actual concession more likely.

1980). Indeed, in terms of losing face and in terms of sheer bureaucratic effort, keeping troops inside the border appears much less costly than bringing them back after a transgression. Deterrence concessions produce opportunity costs, as profitable actions are foresworn in order to avoid international disapproval; compellence concessions generate immediate costs, as policy must actually be changed in order to please external actors. For a survival-driven leader the most important question is not necessarily whether the demanded concessions involve opportunity vs. actual costs, but how those costs disperse through society.

When considering concession, targeted leaders essentially confront two simple questions: "Who stands to lose?" and "What can they do about it?". Answers to these questions depend on the type of demand made and how its costs promise to disperse throughout society, but also upon the strength of the targeted leader's position vis-à-vis the losing coalition. A concession may promise to impose costs upon the public, but a leader who enjoys broad-based support and is well-entrenched in power can take action without fear of serious opposition. The head of a popular reform government whose party holds an overwhelming majority in the parliament, for example, holds a position much more conducive to providing the macroeconomic reforms demanded by aid donors than one who controls only a razor thin margin.[28] Before, delving into such particulars, though, we must consider the potential types of costs imposed by concessionary policies and the potential sources of political mobilization in response to those costs.

Concession involves a change to the status quo policy arrangements within a targeted state. These policy arrangements determine the division of resources and services within the society. Changing this framework will prompt redistribution of costs

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[28] This argument may be compared to that of Ostrom and Job (1986), whose cybernetic decision-making model predicted that American presidents for whom public approval was higher would be more likely to engage in war.

and benefits; sooner or later, some members of the targeted population will benefit and some will pay for these changes. Variation in the difficulty of offering concessions stems from the underlying differences in likely costs of changing the status quo.

Economists and political scientists frequently categorize goods and resources in terms of two properties: exclusion and subtractability (e.g. Olson 1971; Hardin 1968; Ostrom 1990). Exclusion is the ability to legally restrict access to a resource, while subtractability means that use depletes the quality or quantity of the resource (Ostrom, Gardner and Walker 1994). Private goods include those which are both depletable and exclusive; public goods, those which are neither. Business profits, or money in general, could illustrate private goods, while the idea of national security is frequently used to exemplify public goods. We can extend this framework to the type of costs or benefits likely to result from concessionary policy change.

Changes in the status quo policy arena may alter the provision of public resources by the government, threaten the division of private goods within society, or adjust current levels of both types of goods. Concessions which would alter the division of private wealth, opportunities, or level of influence could be called private-costs producing concessions. Those which would deplete the supply or quality of a public good, such as sovereignty, security or prestige, could be termed public-costs producing. Let us consider briefly, a set of examples to demonstrate both the concept of the concessionary cost types and their relevance for both foreign aid and economic sanctions as classic cases of positive and negative policy tools.

The U.S. Agency for International Development (USAID) Democracy and Governance office provides resources and assistance for the development of stronger, more legitimate institutions in the developing world. A principle subset of this mission focuses specifically on the rule of law by pushing for separation between government branches and securing the judicial branch from manipulation, corruption and other threats to the equitable application of justice. The U.S. government has allocated between $6

and $7 million annually since 2004 to such efforts in Liberia, with limited success (Henderson, Jakosa and Gibson 2009). Eliminating bribery and increasing the necessary qualifications to practice law or sit on the bench in the Liberian legal system constitute a series of private costs to be suffered by an influential class of society as lawyers and judges may find their pockets lighter, their qualifications and decisions questioned.

For two months prior to the start of the second Iraq war, the United States pursued a different type of policy concession from Turkey, its strongest regional ally. The U.S. proffered an aid package of up to $32 billion in exchange for the right to launch the ground invasion of Iraq from Turkish soil (Filkins 200, 2003b; DeFrank and Bazinet 2003). This promise, along with the potential to influence decisions about Iraq in the future, won over the Turkish leadership, but neither the Turkish people nor the opposition were willing to trade sovereignty rights for hard cash (Blanchfield 2003; Raghavan 2003b). Though the leadership pushed a second vote to reconsider the issue, the parliament refused to grant the U.S. access beyond limited use of its airspace (Filkins 2003b; Birch 2003; Saunders 2003). Turkey's unwillingness to fulfill the aid-for-policy deal surprised some American planners (DeFrank and Bazinet 2003; Raghavan 2003; Filkins 2003b); the public concession-costs produced by admitting foreign troops for the purpose of war-making, namely diminished sovereignty and security, should be among the easiest to weather as will be discussed below.

Perhaps part of this difficulty can be explained by fleshing out the details of the overall aid package to Turkey. At the time, the Turkish economy was in a deep slump and was attempting to implement tough conditionality agreements stipulated by the International Monetary Fund. The conditionality terms called for "… privatization, budget cuts, and mass public-sector layoffs" (Pan 2003). During negotiations over troop placement, the United States insisted the IMF conditions be upheld and furthered. These economic reforms inflict intense, focused, and immediate private costs on the groups accustomed to benefiting from the inefficiencies of government-operated corporations,

from government employment, and from entitlement programs. By linking the IMF conditions to future aid monies, the United States essentially transformed the troop deal into a mixture of public and private costs. Concessions likely to produce such mixed costs should be considerably more difficult to provide than pure public-costs concessions.

Within the realm of economic sanctions, some of the most well-known cases historically have involved a tough mixed-costs issue: nuclear proliferation and disarmament. Long-standing sanctions against North Korea and Iran, as well as shorter ones against India and Pakistan, have focused on their refusal to cease development of nuclear weapons. As a concession, shutting down nuclear weapons programs would cause for any of these states, a perceived relative loss of security and sovereignty. These are public costs, but revoking the jobs and profits of military-industrial actors involved in the weapons programs would produce private costs.

The sanctions which India leveled against Nepal in 1989-1990 focused on a more strictly public-costs issue. In an attempt to distance the smaller, mountainous nation from the growing regional power of India, Nepal's King Birenda began purchasing weapons from China. India responded to the shift in allegiance by cutting off nearly all military and economic trade with Nepal. The sanctions ended as soon as a democratic government came to power and rescinded the relationship with China (Coll 1990). Reversing an alliance choice such as this one will usually involve public-costs: prestige, symbolic sovereignty, etc.

Finally, two very different economic sanctions events can demonstrate the potential of private concession-costs. In 1997, President Clinton issued an executive order preventing any new investments in the Burmese economy, citing gross violations of human rights and repression of the political opposition. Over the next ten years these sanctions were extended and increased in scope by George W. Bush as the government in Burma (Myanmar) continued to rule through repressive tactics (Niksch and Weiss 2009; Baker 2007). The use of sanctions to end human rights abuses requires the most difficult

of concessions types as it asks the regime to forgo the methods by which it maintains power. Removing torture and corruption from the arsenal of political strategies would certainly result in the loss of office, possibly even in a bloody manner.[29]

Another private-costs producing concession was demanded by the United States of Canada in 1999. A Canadian firm, Bow Valley Energy Ltd., made a contract to expand off-shore drilling in Iran's Balal oil field. The deal, worth $200 million, triggered the United States' ire under the Iran-Libya Sanctions Act (ILSA) which calls for sanctions against all companies investing more than $20 million in the oil and natural gas industries of Iran or Libya. The U.S. government publically stated that the deal would fall under the purview of the ILSA and require sanctions. In response, a Canadian foreign affairs department spokesperson stated simply that "[The Canadian government] will oppose any sanctions being applied against Canadian companies doing business in Iran" (quoted in Doggett 1999). Stopping the Bow Valley deal would have cost the firm its expected profits, a private cost. Canada's clear signal that it would defend its private business interests against American punishment led the U.S. to drop the matter (Doggett 1999).

The examples above demonstrate the existence of private, mixed and public costs to concession in positive and negative sanctioning events. Concessions also hold the potential to produce benefits in both private and public terms. Changing policy to end human rights abuses in response to economic sanctions, for example, would clearly produce a public benefit in terms of personal integrity rights and higher functioning rule of law. But, as Gordon pointed out in a summary of foreign aid conditionality in Africa (1992, 38), such concessions exhibit a "time-inconsistency problem". The benefits accrue only in the long-run, whereas the costs of change – like weakening a regime's

[29] Goemans (2008) found that mixed and transitioning regime leaders face higher likelihood of losing office irregularly and of being exiled, imprisoned or killed.

hold on power by removing its tools of repression – sting immediately. The more immediate costs of concession matter because they hold the potential to prompt domestic resistance. When assessing the wisdom of providing concessions, leaders must determine whether shifting policy will produce public costs, private costs, or a mixture of the two.

We should note here a vital difference between this logic and that promoted by selectorate theory. The theoretical importance of isolating public and private costs does not hinge on whether the leader's winning coalition stands to lose more than the losing coalition would. A democratic leader's winning coalition includes portions of both the elite and the public, but this does not mean that they are *always* more sensitive to public costs than to private costs. Canada's staunch defense of Bow Valley Energy aptly demonstrates the democratic government's willingness to stick up for business interests. It is the second question, "What can they do about it?", which really determines whether a leader will pay political costs for imposing a costly change. It is the relative ability of public and private costs to produce a response from the cost-bearing group which demands attention – not whether those costs center on the winning coalition. We need, therefore, to focus not on regime type, but on the likely shifts in political reaction given different dispersions of concessions' cost.

Mobilization to protest the actions of government happens neither automatically nor cheaply. Rational cost-benefit optimizing individuals weigh the benefits of changing policies against the costs of joining an effort to influence the government. They will contribute to the effort only if the personal benefits outweigh the personal costs. Shortfalls in public goods often result, because the personal share of an increase in public goods is likely to be very small compared to the personal dedication necessary to affect that increase. People tend to mobilize and protest when faced with focused costs, but not often at the prospect of foregone public goods (Olson 1971). Mobilization will occur in the face of policy change when the potential costs are focused on a particular group such that the benefits of success outweigh the effort of organization, or when motivated

entrepreneurs exist and willingly shoulder most organization costs. This general social dynamic has been blamed for many cases of under-provision of public goods, including what Gordon (1992) describes as the "foreign aid game" wherein changes to the macroeconomic framework cannot be negotiated even though they are in the long-term public good. Or as elegantly put by Morgenthau (1962, 305):

> In the measure that they succeed, [reforms which threaten the status quo] are bound to affect drastically the distribution of economic and political power alike. Yet the beneficiaries of both the economic and political status quo are the typical recipients of foreign aid given for the purpose of changing the status quo. To ask them to use foreign aid for this purpose is to require a readiness for self-sacrifice and a sense of social responsibility which few ruling groups have shown throughout history.

Both the entrenched interests of politicians and the principal of collective action, then, suggest that private-costs producing changes to policy will be difficult to achieve. Policy concessions leading to private costs focused on the elite promise to produce resistance from those likely to bear the costs. From the perspective of a survival-driven leader – *no matter the political system* – assailing the well-being of the establishment is more difficult than allowing the status quo to continue. Concordantly, providing concessions which generate public costs is easier for all types of leaders. My theory, in other words, does not expect democratic leaders to be inherently faithful stewards of the public good. Their survival motive may not be well-served by an attempt to improve public welfare at the expense of private interests. Without motivated leadership, the public is unlikely to mount an effective protest, leaving the leader's job prospects unchanged.

Effective leadership for the public, however, will not always be lacking. Entrepreneurs – individuals willing to take up the public mantle – emerge in at least two ways. First, under some circumstances, concessions may inflict heavy costs both on private groups and on the public. If external powers demand a reduction in arms spending, or the development of new military technology, for example, private contractors who benefit from government deals stand to experience focused costs in the form of lost business. The rest of the population, in turn, faces decreased security relative

to neighboring countries. In such situations, the prospect of personal loss provides incentives for the elite to organize the masses. Absent other domestic political considerations, then, we can conclude that concessions which impose mixed or private costs are less likely to be delivered than are those which impose only public costs.

Second, the "losing coalition" provides a natural source of motivated entrepreneurs. All leaders face some level of domestic political competition. With the prospects of seizing power as a motivating factor, the opposition seeks opportunities to destabilize the leadership. External intervention and the demand for concessions provide such opportunity. Intervention publicizes and focuses attention on the leaders' decisions. Cognizant of both the likely costs of concession and the effects of intervention, an effective opposition can advertize itself to those likely to bear the burden of a leader's decision. They may agitate in order to peel away enough supporters to overthrow the administration, or they may set up institutional roadblocks to prevent the enactment of concessionary policy. When the opposition is strong, it becomes harder for leaders to provide concessions which burden the public.

A strong opposition is not necessarily the exclusive purview of democratic systems. Nor is it necessarily present in all democratic systems at all times. Within autocratic systems, considerable variation in the power relationship between the ruler and the population exists (e.g. Peceny, Beer and Sanchez-Terry 2004; Lai and Slater 2005; Gandhi and Przeworksi 2006, 2007; Wright 2008b). Often, some level of cooperation must be sought in order to create the necessary private benefits needed to please autocratic winning coalitions, and compromises must be struck in order to dissuade violent uprisings (Gandhi and Przeworski 2006, 2007; Wright 2008b). Within democratic states, significant variation in the efficacy of the opposition also exists. Leaders cannot always secure majorities in support of their policies, but under some conditions they enjoy surprising freedom of action. The Prime Minister of England whose party enjoys a majority position in the parliament, for example, can force through

even controversial new policy. The strength of the opposition constitutes a source of variation within as well as across the broad typology of democracy vs. nondemocracy.

The opposition will vary in raw power across and within regime types, but its numbers are not the sole determinant of its efficacy. The temporal dynamics of institutionalization and socialization of a leader's administration alter the ease of destabilizing the incumbent leader, and thus the power of the opposition. Newer autocratic leaders face a great "need to please" their nascent winning coalition and to dampen the appeal of opposition claims. The offers of challengers are more credible earlier in the game, because members of the winning coalition, less certain of their current place, are likely to put less stock in the current leader's promises and to apply less skepticism to evaluation of the potential successor. The importance of the opposition's entrepreneurial role should be exaggerated in these early, more uncertain times. In order to foster the loyalty necessary to enhance his/her security in office, the new autocrat must be especially careful to avoid costs to his/her winning coalition and to deny the opposition any cause to mobilize political opposition. Given that any concessionary policy change involves costs to some portion of the population, new leaders are likely to resist externally-suggested changes. Over time, with the solidification of their winning coalition, autocrats gain the ability to tackle costly undertakings (e.g. Ostrom and Job 1986; Gelpi and Grieco 2001; Chiozza and Goemans 2003; Chiozza and Choi 2003). This may include providing concessions to external powers even if the opposition would agitate in response to the domestic costs.

The "need to please" for a democratic leader, however, increases over time. In the early days, popularity runs high and criticism low (Brody 1991, 27-44; Lockerbie et al. 1998). Even fairly controversial changes can slip by with little comment from the opposition; leaders hold the ability to incur domestic costs with lower likelihood of punishment. This immunity will not last, however. Popularity wanes over time, simultaneously increasing the likelihood of criticism (Brody 1991; Brace and Hinckley

1992) and decreasing the likelihood of cooperation from other branches or parties (Lockerbie et al. 1998; Light 1999; Altman 2000). The power of the opposition, O, to increase the costs of public-costs concessions should be initially low and increasing over time.

In the equation below the magnitude of the domestic hurdle to policy concession, D, is modeled by cost type, opposition strength and targeted leader's experience.[30]

$$D = \gamma_1 C_{private} + \gamma_2 C_{mixed} + \gamma_3 C_{public} + (\gamma_4(O \times C_{mixed}) + \gamma_5(O \times C_{public}))f(t_i) \quad \text{(Eq. 3.2)}$$

Theoretically, the three types of costs, C_{mixed}, $C_{private}$, and C_{public}, are mutually exclusive, continuous measures of the projected expense stemming from policy change. The constants γ_1- γ_5 weight the impact of the type of costs according to their mobilization capacity. Thus, $\gamma_1 \geq \gamma_2 > \gamma_3 > 0$, because all concessions carry some cost but private and mixed costs both promise to mobilize the elite, while public costs do not.

The mixed and public cost types are interacted with the strength of the opposition, *O*. As the strength of the opposition increases, the likelihood of mobilization in the face of public costs increases; mixed costs include public costs so the opposition is likely also to contribute to further mobilization under these situations. Therefore, $\gamma_4 > 0$ and $\gamma_5 > 0$, indicating the ability of the opposition to increase the difficulty of public-cost elements. Private costs are not interacted with the strength of the opposition, because it is assumed that the elite have an intrinsic motive to mobilize; they do not need external entrepreneurs to step in.

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[30] The same caveat discussed in an earlier footnote with regard to the role of time in the mathematical representation of effectiveness applies here. Were the domestic hurdle to policy concession the dependent variable – as imagined in Equation 2 – time in office would contribute additively for democratic and nondemocratic leaders. In the final test, however, probability of concession will be the dependent variable, and time will function as a conditioning factor on the cost terms – dragging down their intensity for nondemocratic leaders and increasing them for democratic leaders. Again, this issue is discussed more thoroughly in the research design.

Finally, the temporal dynamics of a leader's "need to please" and of accumulated experience, represented by a function of experience in office $f(t_i)$, further condition the ability of the opposition to champion the rights of the public. Over time nondemocratic leaders build up political capital which provides them a buffer of loyalty against policy backlash. The increased loyalty of winning coalitions and diminishing of competition over time makes these leaders more capable of bearing the costs of concession even if the opposition objects. Thus, if winning coalition cohesion drives the dynamic, $f(t_i)$ will be negatively signed for autocrats as their experience decreases the perceived difficulty of domestic hurdles. For democratic leaders, however, $f(t_i)$ will be positively signed. Competition, criticism, and disapproval increase over time for these leaders. Costs loom larger as their store of good political will lags. If, however, on-the-job learning probes the more powerful dynamic, $f(t_i)$ will be negative for both types, as their competence increases over time.

Again, walking through a hypothetical example will solidify the argument. Suppose the international community targets a leader with positive sanctions, the provision of military support and technical advisors, with the demand that a military build-up be terminated. The costs of such a concession are mixed – private groups lose military contracts and the public faces a potential relative decline in security and sovereignty. In this case, then, the terms for private and public costs drop out of analysis. Only the constituent and interaction terms involving mixed costs and the time function remain:

$$D_{mixed} = \gamma_1 C_{mixed} + \gamma_4 (O \times C_{mixed})(+/- f(t_i)) \quad \text{(Eq. 3.2.1)}$$

Mixed costs hold relatively high potential for mobilization, and thus contribute significantly to the domestic hurdles. The first term in Equation 3.2.1, then, would be large and positive. The effect of mixed costs could be further augmented by the presence of a strong opposition. Under conditions where political competition is high, political

entrepreneurs likely unite with the private cost-bearing groups to incite a greater public response. For a nondemocratic leader, this process decreases in likelihood and effectiveness over time; for the democratic leader, it increases.

Now consider if the demanded concession shifted towards a public cost, say the establishment of a foreign military base within the target's territory. Now the mixed and private costs terms drop out of the equation.

$$D_{public} = \gamma_3 C_{public} + \gamma_5(O \times C_{public})(+/- f(t_i)) \quad \text{(Eq. 3.2.3)}$$

Public costs, due to their lower capacity to trigger mobilization ($\gamma_3 < \gamma_{1,2}$), produce a lower value of D than would the mixed costs scenario discussed above. Providing concessions which produce primarily public costs is easier than those which produce mixed costs.

It is possible that the gap between D_{public} and D_{mixed} can be depleted if the opposition is stronger in the case with public costs. Mobilization in the face of public costs becomes more likely as the opposition increases in efficacy, as represented by $\gamma_5(O \times C_{public})$. If the value of o in Equation 3.2.3 is larger than that in Equation 3.2.2 and $\gamma_5 \geq \gamma_4$, than the domestic costs of providing the public concession could be at least as high as for the mixed cost case. An opposition willing and able to incite public response can occur in a number of different situations. Within a democratic system, an opposition which holds a significant portion of the legislature has much to gain from the exploitation of the government's difficulty in making mixed-costs concessions. Even outside democracy, leaders frequently inherit or create institutions which allow voice for opposition groups in order to maintain the peace (e.g. Przeworski, Alvarez, Chiebub and Limongi 2000; Gandhi and Przeworski 2006, 2007); the ready-made access makes it much easier for elites to work with the opposition to throw up hurdles to concession.[31]

[31] These institutions are not rare. According to coding by Przeworski et al (2000), updated by Wright (2008b), about 74% of non-democratic systems have some kind of legislature.

In terms of domestic hurdles to concession, the comparison between the Shah of Iran and Pakistan's President Musharraf may again be pertinent. While both were expected to provide strategic concessions, amounting to public costs, differing domestic hurdles influenced compliance. The Shah's opposition in the early years, while angered by his pro-U.S. policies, existed outside of government and faced high costs for mobilization activities (e.g. Randal 1978; Gasiorowski 1991; U.S. State Department 2008). He filled the legislature with loyalists, easily bought off and supportive of his pro-U.S. agenda (Abrahamian 1980, 419-25). President Musharraf, however, strapped by an increasingly strong and varied domestic opposition – ranging from the extremists in the North and the former ruling party in exile and the capital – failed to mount the expensive undertaking of pacifying the tribal regions (Masood 2007, 2007b; Perlez 2007; BBC… 2006).

The nature of domestic politics and external demands interact to produce greater or smaller domestic hurdles in the path of concession. The discussion above produces the following general expectations:

H4a: The domestic political difficulty of providing demanded concessions differs by the type of cost that concessions would produce.

H4b: Concessions which would impose public costs pose less domestic political difficulty than would those imposing private or mixed costs.

H5: A strong opposition increases the likely domestic political difficulty posed by public- or mixed-cost concessions.

H6a: The difficulty of concessions increases over time for democratic leaders and decreases over time for nondemocratic leaders.

H6b: The difficulty of concessions decreases over time for all leaders.

The expectation that providing concessions will produce domestic turmoil gives survival-driven leaders reason to resist external pressure. This expectation is affected by the type of costs likely to be generated, by the strength of the opposition and by the

temporal dynamics of learning and winning coalition cohesion. As discussed in the prior section, however, leaders also consider the direct effect of sanctions on their ability to maintain power when considering whether or not to give concessions. The next step requires incorporating the two means by which internal intervention affects targeted leaders' incentives – effectiveness and domestic concession costs – into a single model of the probability of concession.

Targeted Leader Incentives and the Success of Influence Attempts

Piecing together the arguments above produces a model of concessions determined by the potential political costliness both of sanctions themselves and of the changes in policy which they demand. Targeted leaders juggle internal and external demands with an eye to keeping their jobs for as long as possible. Domestic conditions may facilitate or foil external influence attempts at two stages. In terms of effectiveness, institutions of representation and the experience of leaders condition the ability of sanctions to exert any real effect on targeted leaders. Effective sanctions, whether positive or negative, are more likely to rouse a response from leaders, because they manage to tap into their primary motivation (survival). When the sanction is negative, its effectiveness depends upon ability to destabilize, and leaders will react to prevent or stop this external threat to their viability. When the sanction is positive, its effectiveness depends upon ability to instrumentally improve leaders' positions, and leaders will react to earn or secure this external facilitator of their viability.

These concessions, however, are not cost-neutral. Influence attempts would not be necessary if the desired actions would occur naturally. When it comes to actual delivery of concessions, domestic costs stem from an interaction between the type of cost and the presence of entrepreneurs to mobilize response. As the intensity of disruption in domestic politics caused by the concession increases, it becomes less likely that leaders

will deliver. The more difficult the task, the lower the probability of success (Baldwin 1985).

Leaders do not always shirk the difficult decisions, however. Some have the political experience and capital to accomplish inherently risky foreign policy moves (e.g. Gelpi and Grieco 2001; Chiozza and Goemans 2003). Experience, in other words, can detract from the face-value of concessions' costs. Leaders who have built up a strong buffer through years of on-the-job training are better equipped to deal with the process of policy change.

In sum, sanction effectiveness, the costs of concessions and temporal dynamics all contribute to the probability that leaders will take the actions demanded by external powers. The two principle means by which the external actor affects leaders' incentives, effectiveness and concession costs, push in opposite directions. The argument, thus, leads to three final hypotheses:

H7: Concession becomes more likely as sanctions' effectiveness increases.

H8: Concession becomes less likely as the domestic difficulty posed by concession increases.

H9: The negative effect of likely domestic difficulty on likelihood of concession decreases as leaders become more experienced.

These predictions link the arguments about leader's incentives directly to the success of external influence attempts. Testing the propositions requires modeling the effect of both effectiveness of sanctions and magnitude of domestic hurdles on concessions. The most appropriate approach for doing so depends in large part on the type of data available for measuring the dependent variable, concession to external demands. Discussion turns to the more practical matters of research design and the modeling challenges particular to evaluation of foreign aid and economic sanctions in the following chapter. Before departing the realm of theoretical development, however, we must double back briefly to consider the strategic role of the external power.

Leader Incentives and Targeting

The relatively low success rate of many foreign influence attempts informs us of the imperfection of the targeting process. It may be fair to contend that sometimes sanctions have been employed in cases where they had little chance of success. But claiming that sanctions rain down randomly upon unsuspecting targets pushes, too far. The allocation of foreign aid and the imposition of economic sanctions, as key examples of external influence attempts, do not occur randomly. Sending states are not exempt from the rationality assumptions made at the start of this exercise. They will employ the tools which they deem appropriate to a situation, based not only on past experience and habit, but on some assessment of likely success. Aid-recipients are different from non-recipients; sanctioned states are different from those in good standing. And, in most cases, sanctioned states are different from aid-receiving states (although these tools are sometimes employed concurrently). In order to fairly evaluate the effectiveness of foreign policy on targets, this non-random sampling must be dealt with directly.

As the theory presented above emphasizes strategic leader incentives, we should account for the possibility that sending states consider these when making policy decisions. Senders should target leaders when they are most likely to be pliable. Leaders who value their survival in office above all else likely respond best to assistance at times when they feel themselves to be facing elevated risks. An external means of support, in other words, appears most valuable when internal support lags. Ironically, this is likely the condition which will produce the highest effectiveness for negative sanctions as well. While a leader's popularity is on the rise domestically, external intervention's power to inspire a change in opinion seems doubtful. Further destabilizing a leader whom the people have begun to question, however, is less of a long shot.

Regarding the allocation stage of external influence, I proceed with the following expectation:

H10: A leader facing relatively elevated risks of losing office is more likely to become the target of an influence attempt.

This additional hypothesis complicates the evaluation of all previous expectations. A non-random targeting process requires estimation procedures capable of accounting for selection bias, as well as the recursive nature of an investigation into leader survival where the probability of survival partially determines selection. Luckily, the censored probit – a two-stage model for limited dependent variables – provides an easily implementable means of addressing this issue which is also amenable to the problems of duration dependence and clustered data. The following chapter covers these as well as measurement and detailed specification issues.

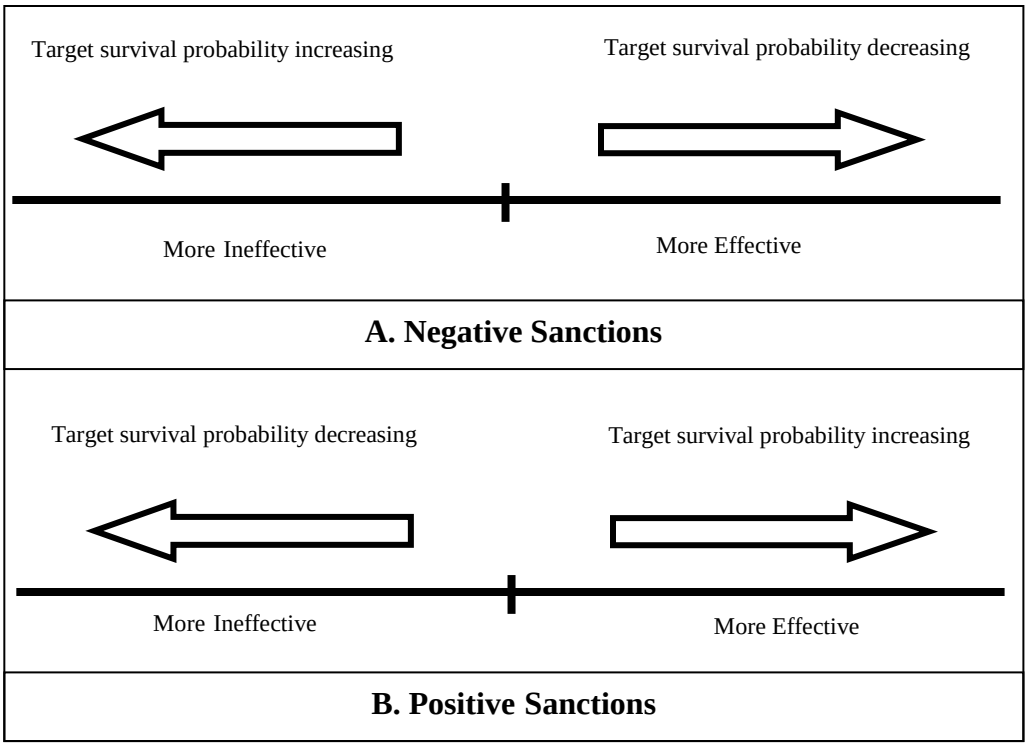

Figure 3.1 Theoretical Range of Sanction Effectiveness

CHAPTER 4

RESEARCH DESIGN: MODELING THE TWO STAGES OF INFLUENCE

The argument forwarded above suggests a very general model of foreign policy success. It applies to situations in which one international actor utilizes threats and punishments or promises and rewards to extract concessions from another. I propose to test its implications with respect to two common foreign policy tools of influence, foreign aid and economic sanctions. These cases provide the opportunity to evaluate the strength of the theory across positive and negative sanctions on a wide sample of targets. Individually and as a pair, they also offer significant variation in the type and magnitude of concessions demanded, which is necessary to test the model's predictions regarding the relative difficulty of providing changes which stand better chance of mobilizing the cost-burdened portion of the population.

Empirical examination of the claims made in Hypotheses 1-9 requires modeling of the effectiveness of aid and economic sanctions in significantly affecting the tenure prospects of targeted leaders. Measures of effectiveness appropriate for inclusion in further statistical analyses must be developed from the results. These measures will then be incorporated with indicators of the magnitude and nature of domestic hurdles into a model of aid and economic sanctions concessions. Tackling each of these steps in order, the research plan presented here will describe model specification and methods, and discuss data availability and measurement.

Modeling Effectiveness

Effectiveness, within the framework advanced here, refers to the ability of negative sanctions to destabilize and positive sanctions to insulate the leaders which they target. This is not an all-or-nothing quality: effectiveness exists theoretically along a continuum from the absolute reverse impact to the absolute expected impact, with null

effect in the middle (see Figure 3.1). The procedure outlined below allows sanctions' effectiveness to run the gamut from back-firing to perfectly performing

Effectiveness itself cannot be directly observed; it must be modeled indirectly by observing the impact of sanctions on leaders' probability of survival.[32] In modeling this probability, my tests will incorporate the factors emphasized in Hypotheses 1-4 as important determinants of effectiveness. Those sanctions which best target winning coalitions, tapping into the survival motive, will be more likely to produce desired effects. Actually enacted rather than threatened/promised actions should also create more tangible results in terms of "effectiveness" as discussed here. Finally, the experience of the targeted leader matters. Patterns of learning and winning coalition cohesion will condition both the ability of targets to benefit from rewards and the vulnerability of targets to punishments.

I will model leader failure, therefore, as a function of the manner of sanction, its institution-conditioned impact, and the leader's experience. If we could model effectiveness directly, our estimating function would take the form of Equations 3.1 and 3.1.2. When probability of failure – rather than effectiveness – serves as the dependent variable, time in office becomes a conditioning factor on the observed impact of sanctions on leaders. Early in a leader's tenure a negative sanction may exert powerful effects, but the magnitude of destabilization would diminish if aimed at the same leader on a later date. Within the jargon of event history analysis, a relationship of this type poses a problem of non-proportional hazards, because a single parameter estimate cannot properly describe the contribution of the variable to the leader's risks.

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[32]Myriad factors contribute to the success or failure of leaders in power. We cannot simply infer effectiveness of sanctions based on whether a leader loses office or not, because we cannot control for these alternative determinants.

The solution to this issue has been frequently discussed by political methodologists in recent years (e.g. Box-Steffensmeier and Jones 2004; Box-Steffensmeier, Reiter and Zorn 2003). Creating interaction terms, between the variable suspected of non-proportional effects and some function of time, allows explicit modeling of the dynamic process. These terms are interpreted just as any multiplicative relationship (e.g. Friedrich 1982; Brambor, Clark and Golder 2006; Kam and Franzese 2007). A log-linear functional form of time, $f(t_i) = \ln(t_i)$, meshes most closely with the posited processes of learning and institutionalization.

Because this key theoretical concept will be calculated from regression estimates, we must take care to address possible sources of bias and inefficiency in these parameters. The first threat to parameter validity comes from the structure of the data. Event history data likely exhibits duration dependence and right-censoring. Duration dependence is a direct conditioning effect of time on the likelihood of observing events of interest. In analyses of leader survival, a tendency for the likelihood of losing office to decrease over time has been frequently noted (Chiozza and Goemans 2004; Bueno de Mesquita and Siverson 1995; Bienen and Van de Walle 1991). Right censoring, non-observance of the event within the observation period, will also occur frequently in leader tenure data. Failure to account for duration dependence will produce biased estimates due to the relegation of time's independent impact to the error term. An inability to differentiate between leaders who have served for the same period of time, one of whom is right-censored and the other whose failure we eventually observe, introduces inefficiency and possibly bias.

An event history model can account for right-censoring and temporal trends in failure. Perhaps the simplest such model was suggested by Beck, Katz and Tucker (1998). Including transformations of time as regressors in a logistic regression effectively models duration dependence. As my theoretical variables already include a transformation of time, $f(t_i)$, I may not need to include any additional time variables. I

assessed the fit of the natural log of time as a control for duration dependence relative to the more flexible functional form of the cubic polynomial, and found in most circumstances that Baeysian Information Criteria supported the simpler formulations.[33]

A second anticipated source of bias stems from the potentially endogenous relationship between leader survival and foreign policy targeting. The strategic behavior of sending states ensures that foreign aid and economic sanctions fall non-randomly upon "eligible" leaders. The problem becomes even more serious if, as stated in H10, the targets' domestic risks factors directly into senders' calculations of expected effectiveness. Considering the problem through a latent variable framework may be of assistance here. We are interested in two continuous latent variables. The first $Y^*_{1,it}$ is the probability of a sanctioned leader *i* losing office in the time period *t*. The second, $Y^*_{2,it}$, is the probability of a leader *i* being the target of a foreign policy intervention in the time period *t*. These latent variables may be modeled as the following:

$$Y^*_{1,it} = \alpha_1 + \beta_1 \boldsymbol{X} + \varepsilon_1 \text{ , if } Y^*_{2,it} \geq Y^{\tau}_2 \qquad \text{(Eq. 4.1)}$$

$$Y^*_{2,it} = \alpha_2 + \gamma Y^*_{1,it} + \beta_2 \boldsymbol{W} + \varepsilon_2 \qquad \text{(Eq. 4.1.2)}$$

Where α gives the slope intercept, β contains slope coefficients, ε are random disturbance terms, **X** is a vector of covariates determining $Y^*_{1,it}$, **W** is a vector of covariates determining $Y^*_{2,it}$, and γ is the slope coefficient for the endogenous impact of $Y^*_{1,it}$ on $Y^*_{2,it}$. Y^{τ}_2 is a threshold value of the probability of targeting after which foreign policy targeting occurs and sanctioned leader survival is observed. The model used to estimate this

[33] The most prominent advice from political methodologists recommends the use of Cox proportional hazards models when non-proportional hazards are a possibility, because statistical tests for assessing the relationship between variable residuals and time can be easily performed (e.g. Box-Steffensmeier and Jones 2004; Box-Steffensmeier, Reiter and Zorn 2003). The actual "fix" for NPH, however, is simply the inclusion of interaction terms, which is not an option unique to the Cox model. The purpose of this stage of analysis is the production of a predicted valuable suitable for use in further regressions, justifying the use of a probit model, with its superior fit-statistics and interpretability.

relationship must account both for the "selection" process implied by Y_2^τ and for the reciprocal relationship represented by γ.

The nonrandom nature of the population of targeted leaders can be answered by a censored probit model (e.g. Dubin and Rivers 1989). This model uses a procedure analogous to Heckman's (1979) selection model, but can be estimated simultaneously and with two dichotomous dependent variables. The two processes (selection and outcome) are assumed to follow a joint normal distribution. The residuals of the two equations are thus allowed to travel together. This aspect of the relationship is estimated by the correlation coefficient ρ. The second problem can be addressed using an instrumental variables approach.

As recommended by Maddala (1986), an instrument $Y_{1,it}^{**}$ can be constructed for use in estimating $Y_{2,it}^{*}$ using parameters from a regression on the observed, dichotomous Y_1. First, a probit regression of Y_1 using the full population is performed. Second, predicted values are calculated from the regression $\hat{\beta}$. The predicted values are then adjusted by the standard error of prediction to create $Y_{1,it}^{**}$ an instrument for leaders' probability of failure, which can then be included in the probit regression of Y_2.

The estimation of sanction effectiveness, then, will require simultaneous estimation of two equations: one for targeting, one for survival. The survival equation must be estimated twice, once using the full population, and again using the targeted population. With these modeling decisions settled, the following section moves on to matters of specification and measurement of the two equations. Discussion will be divided by equation. First, the specification of the selection equation will be presented and explained. Second, the measurement choices for variables at the selection stage will be detailed. The process of specification and measurement will then be repeated for the survival outcome.

Specification: Selection Stage

The targeting of foreign policy, as discussed above, will be partially determined by the domestic risks faced by potential targets. As far as the theory at hand is concerned, any other contributing factors will be included as statistical controls. The selection equation then can be summarized as:

$$P\left(Y_{2,it} = 1 \middle| W_{it}, Y_{1,it}^{**}\right) = \Phi\left(\alpha + \boldsymbol{\beta V_{it}} + \gamma Y_{1,it}^{**} + \varepsilon_2\right) \quad \text{(Eq 4.2)}$$

Where $Y_{2,it}$ is a dichotomous indicator of policy targeting, Φ is the cumulative normal distribution function, $\boldsymbol{V}_{it}$ is a vector of controls which will vary by the type of policy, and $Y_{1,it}^{**}$ is the instrument for probability of failure which is estimated following the procedure described above. I will describe the included variables below, but descriptive statistics can be viewed in Tables 4.3 and 4.4 at the end of the chapter.

Dependent Variables

In the selection equation of the foreign aid process, $Y_{2,it}$ is an indicator of whether the leader in question received a non-zero amount of net bilateral development assistance. Data on aid amounts comes from the Organization for Economic Cooperation and Development (OECD) online database. In the full dataset of aid-eligible leaders from 1960-1999, 85.6% of leader years are marked by aid allocation.

For analysis of economic sanction effectiveness, $Y_{2,it}$ tags leaders experiencing the threat or imposition of sanctions in the given year. Targeting is determined using the Threat and Imposition of Economic Sanctions (Morgan et al. 2009) database, which covers 1971-1999. About 38% of cases in the full dataset achieve a positive value of economic sanction targeting.

Independent Variables

To account for more traditional explanations of aid allocation, I include a number of control variables. As an indicator of humanitarian need, I include lagged population

growth taken from the World Development Indicators. This variable is highly correlated with other measures of need, notably birthrate and infant mortality, but has substantially better coverage. Donors' commercial interests are captured by a logged measure of the value of imports from OECD donors received by the target in the prior year (Gleditsch 2002). Prior work on aid allocation in the selectorate theory school has found that donors tend to send aid to smaller coalition systems (Bueno de Mesquita and Smith 2007, 2009). I control for this using the ordinal measure of Bueno de Mesquita et al's (2005) winning coalition concept, *W*. Other indicators of strategic interest include: an indicator of internationalized civil conflict from the Uppsala Conflict Data Project (Gleditsch, Wallensteen, Eriksson, Sollenberg and Strand 2002), an indicator of former colonial status, an indicator for oil production capacity (Energy Information Administration 2009), and one for a defensive or offensive alliance tie to one of the OECD donors (Leeds, Ritter, Mitchell, and Long 2002). I account for the power or capacity of the potential target state with a measure of logged population size and lagged, logged economic growth rate (Gleditsch 2002). Aid allocation patterns are sticky, so I also utilize a lagged dependent variable to account for inertia.

Extant explanations of economic sanctions targeting frequently discuss target regime type (e.g. Nooruddin 2002). To control for a possible independent impact of level of democracy in the target, I include the ordinal *W* measure of winning coalition size (Bueno de Mesquita et al. 2005). I account for target power with the Correlates of War CINC score and logged population size. The CINC score is a composite indicator of each state's share of economic and military strength in the world system (Singer, Bremer and Stuckey 1972). As many economic sanctions stem from trade disputes, higher levels of trade may be related to onset. This possibility is modeled using a measure of logged total trade from Gleditsch's (2002) expanded trade and GDP dataset. Civil instability, measured using the Uppsala Conflict Data Project's level of civil conflict variable (Gleditsch et al. 2002), could constitute another reason for intervention. International

conflict, in the form of militarized interstate disputes (MID), could also induce economic sanctions. I include both the existence of a MID and an indicator for whether the potential target played the revisionist role in a MID to account for this potential source of targeting (Ghosn, Palmer and Bremer 2004). I also include a measure of whether the target received foreign aid in the current year. Finally, the prior occurrence of targeting is a clear predictor of future targeting, so I include a lagged dependent variable.

Specification: Outcome Stage

The theory forwarded in Chapter 3 alleges that sanction effectiveness will be conditioned by the credibility of the threat/promise, by the target institutions of representation and by the experience of the leader in office. Each of these elements must be worked into the leader failure equation. Of the cases under scrutiny, credibility plays a meaningful role only in the threat of sanctions. Promises of aid are so highly correlated with actual allocations that a measure of credibility would be static across the sample. It will, therefore, be left out of the assessment of received aid's impact on leader tenure. The models of leader survival given sanctions will thus be specified as:

$$\mathrm{P}\left(\mathrm{Y}_{1,\mathrm{it}}=1\middle|\mathbf{X},\mathrm{S}_{\mathrm{it}}^{\mathrm{p}}=1\right)= \Phi\left(\begin{array}{c}\alpha-\beta_{11}\mathrm{A}_{\mathrm{it}}-\beta_{12}\mathrm{W}_{\mathrm{it}}\mathrm{A}_{\mathrm{it}}-\beta_{13}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{A}_{\mathrm{it}}-\beta_{14}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{W}_{\mathrm{it}}\mathrm{A}_{\mathrm{it}}+\boldsymbol{\beta}\mathbf{Z}+\varepsilon_1,\\ \alpha+\boldsymbol{\beta}\boldsymbol{V}+\gamma Y_{1,it}^{**}+\varepsilon_2,\ \rho\end{array}\right) \qquad \text{(Eq. 4.3.1)}$$

$$\mathrm{P}\left(\mathrm{Y}_{1,\mathrm{it}}=1\middle|\mathbf{X},\mathrm{S}_{\mathrm{it}}^{\mathrm{n}}=1\right)= \Phi\left(\begin{array}{c}\alpha-\boldsymbol{\beta}\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathrm{W}_{\mathrm{it}}\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{W}_{\mathrm{it}}\mathbf{S}_{\mathrm{it}}+\\ \boldsymbol{\beta}\mathbf{c}\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathbf{c}\mathbf{W}_{\mathrm{it}}\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathbf{c}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathbf{S}_{\mathrm{it}}-\boldsymbol{\beta}\mathbf{c}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{W}_{\mathrm{it}}\mathbf{S}_{\mathrm{it}}+\boldsymbol{\beta}\mathbf{Z}+\varepsilon_1,\\ \alpha+\boldsymbol{\beta}\boldsymbol{V}+\gamma Y_{1,it}^{**}+\varepsilon_2,\ \rho\end{array}\right) \qquad \text{(Eq. 4.3.2)}$$

Equation 4.3.1 refers to aid ($\mathrm{S}_{\mathrm{it}}^{\mathrm{p}}$); 4.3.2, economic sanctions ($\mathrm{S}_{\mathrm{it}}^{\mathrm{n}}$). Bolded terms represent vectors rather than single values. Φ is the cumulative bivariate normal distribution function. Terms are indexed both by individual leaders *i*, and by observation years, *t*. The parameters α and $\boldsymbol{\beta}$ are constants to be estimated using maximum

likelihood. *W* is an indicator for institutions of representation which engender large winning coalitions. *A* measures aid received while ***S*** is a vector containing indicators for threatened or enacted sanctions. The credibility of the threats/sanctions is indicated by *c*. The log of leaders' experience in office is represented by $f(t_i)$. **V** is the vector of controls for sanction onset. **βZ** gives vectors of control variables and slope coefficients in the outcome equations. Finally, ***ε*** is a stochastic error term with normal distribution and mean zero.

Dependent Variables

Initial data on leaders' tenure in office comes from Archigos 2.9 (Goemans, Gleditsch and Chiozza 2009), which includes detailed information about entry and exit of 3,000 leaders from 1875-2004. Most studies of leader survival utilize the Archigos indicator of leader failure as a dependent variable, excluding deaths from natural causes. For the theory at hand, this variable contains noise. The effectiveness of a sanction is measured by its impact on leaders' ability to hold onto office. When leaders surrender power of their own accord or butt up against constitutionally mandated time-limits, their exit does not inform. A more accurate dependent variable would tap only those exits indicative of the dissolution of a leader's winning coalition. Accounting for this problem required investigation of the circumstances of exit for the leaders within my dataset which Archigos coded as "regular". These regular exits include term limits, impeachments, coalition dissolutions, elections and resignations.

For aid-eligible leaders from 1960-2000, I have coded both the situation of exit and the relationship between the entering and the exiting leaders. Means of exit can be coded as one of six types: election, term limit, constitutional limit (including impeachment, no-confidence votes, and dissolution of coalitions), resignation, end of caretaker regime, or unclear. Entering leaders can relate to the exiting leader in three ways. "Heir/Successors" come from the same party, administration, or the family of the

exiting leader. "Challengers" affiliate with opposing parties, or rival factions within the dominant party/military leadership. The "Neutral parties" coding is used when party affiliation cannot be determined or when an interim ruling coalition is put in place. Unless an heir/successor takes over following term-limits or resignation I code winning coalition failure as 1. Using this additional information I code an indicator for winning coalition failure. It obtains under two conditions: irregular replacement, or replacement by an actor who is not an heir/successor. [34]

Independent Variables

The principle theoretical variables in Equations 4.3.1 and 4.3.2 are measures of the use of either foreign aid or economic sanctions. Foreign aid is represented by *A* in Equation 4.3.1; sanctions by ***S*** in Equation 4.3.2. The sources and distributions of all variables discussed below can be found in Tables 3 and 4 at the close of this chapter.

Foreign Aid

The Organization for Economic Cooperation and Development (OECD) provides the most comprehensive source of information on aid flows from advanced countries to less developed states. This data is available for 1960-2004. The measure used here will be the total net bilateral official development assistance disbursed to the targeted state. In keeping with practices common in the literature, this measure is corrected for the size of the economy, lagged one year and logged in order both to correct for high skewness and to account for likely diminishing returns.[35]

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[34] For the economic sanctions model, coding on this new dependent variable remains incomplete. The Archigos fail variable, excluding natural deaths, is used instead.

[35] These transformations are vital to correct inference about aid's impact. Ten million dollars in aid may sound like a large amount, and would be for a country with annual GDP of $30 million. But, for a country more like Israel (GDP of $190 billion according to the CIA World Factbook), this amount is less impressive. Further, the diminishing returns effect and excess heteroskedasticity created by highly skewed, raw aid measures appear to have muffled the statistical significance of aid in earlier analyses (Lai 2003).

Economic Sanctions

The newly released Threat and Imposition of Economic Sanctions (TIES) data contains detailed information on 888 sanctions cases from 1971-2000 (Morgan et al. 2009). The most attractive aspect of TIES is that it accounts for threats as well as actually imposed sanctions. Two indicator variables based on information from TIES will tag leaders who have been targeted by either threat or imposition of economic sanctions in the current year. This strategy will allow testing for different effects across threatened vs. actual action.

Hypothesis 2 makes the claim that sanctions targeted at the leader's winning coalition will more effectively tap into the survival motive. The simplest interpretation of this hypothesis – that democratic leaders with a broad winning coalition will be more sensitive to the public costs of economic sanctions – can be evaluated with respect to the relative significance and magnitude of the slope coefficients for the $\mathbf{S}_{it}$ terms versus those for the interaction $W_{it}\mathbf{S}_{it}$ terms. Because democracy is measured with the indicator W, the first slope coefficient will indicate the impact of sanctions on nondemocratic leaders; the second the shift in that effect for democratic leaders. We would expect, given democratic leaders' supposed concern for public costs, that the slope coefficient for the interaction would be positive and significant, and that for the nondemocratic leaders to be negative or nonsignificant.

The TIES data appear to allow for a more sophisticated test of Hypothesis 2, however. Their variable entitled *threatened target interest* differentiates between sanctions aimed at the general public, regime leadership, military, political or business interests. This information might be used to create a measure of sanctioning sensitive to the targeting of costs on the leader's winning coalition, bluntly construed.

However tempting, conceptual problems plague the process of separating sanctions by their relation to different types of winning. First, the small vs. large winning coalition argument does not produce mutually exclusive pictures of coalition

membership. Specifically, no reasonable framework could claim that either arrangement excludes business elites. This creates a practical concern for any analysis of sanction targeting which includes the full sample of known sanction episodes. Of the sanctioned leader years for which TIES made coding decisions, 47% qualify as targeting business interests. Missing data constitutes the second largest category of threatened target interests, at over 28% of the sanctioned leader year sample from 1971-1999. This leaves only one quarter of observations available for potentially differentiating between the accuracy of sanction targeting against certain types of leaders.

The paucity of data itself is further complicated by the inability to separate the selection of a specifically targeted interest from the issue at hand. An overwhelming 92% of sanctions threatening business interests, not surprisingly, arise from trade disputes. The conflicts over which senders issue threats against the leadership or military are much more likely to involve traditional security and ideological concerns. For example, 25% of these sanctioned-leader years feature demands regarding curtailment of military actions and 64% of them focus on human rights violations. Sanctions targeted so broadly as to threaten the overall population tend to be of this more strategic nature as well. In practice, the relative similarity of purpose behind sanctions against the public and the military and leadership versus those against business interests clouds inference. Attempts to differentiate between sanctions which target the democratic winning coalition as opposed to the nondemocratic winning coalition tend instead to tap into the difference between trade and strategic sanctions.

Finally, attempting to separate sanction episodes on the basis of alignment with targets' winning coalitions requires two rather heroic assumptions. Specifically, such a coding scheme would imply (a) that the threatened portion of the population would actually bear the brunt of international punishment, and (b) that they would do so unwillingly as a punishment for actions not their own. While senders attempt to demarcate the segment of society which will bear the costs of external punishments

through choice of specific tools, in practical terms elites appear able to pass the costs of sanction off onto the public (Andreas 2005; Hufbauer and Oegg 2003; Joyner 2003; Cortright and Lopez 2002; Garfield 2002; Heine-Ellison 2001; Garfield et al. 1995; Hufbauer et al. 1990). Business elites do not necessarily suffer the costs of sanctions, as they can pass extra expenses on to their customers. Moreover, the policies or actions which prompt international trade disagreements may be highly favorable to the targeted domestic businesses. Rather than lobbying their leader to provide concessions, the threatened interest in these cases is likely to do the opposite, encouraging the leader to withstand international rancor for the sake of domestic support.

In sum, it appears on the surface that we may be able to test hypotheses about sanction targeting with increased precision, but this apparent clarity belies the underlying conceptual morass. I find it less troublesome to work with the assumption that sanction events tend to harm the public rather than private interests than to assume that current data provides more refined information. The principle analyses in Chapter 7 and 8, then, treat sanctions and threats uniformly as more dangerous for democratic leaders than nondemocratic leaders.

Credibility

The credibility of sanctions, *c*, is a nebulous concept, which may be measured in a number of ways. Domestic audience cost arguments hold that the more democratic the sanctioning state, the more credible their threats (e.g. Fearon 1994). The democracy score of the sending state, then, provides one possible scale of credibility. The TIES data includes other information which could be expected to affect targets' evaluation of the likelihood of action. One codes the source of the threat from within the sending state/institution. A statement of intent made by the executive or the primary decision-making body may hold more weight than one issued by lower functionaries. Alternatively, TIES provides a measure of "sender commitment" which is based upon the immediacy and specificity of the language used to threaten. By this standard, a weakly

committed sender may make it known that sanctions are among one of many possible actions they may be willing to take. A strongly committed sender will make a straightforward "if… then" statement about the conditions under which sanctions will be utilized, and what actions would suffice to end them. Finally, TIES also codes a qualitative assessment of the costs a sender is likely to suffer from imposing sanctions. This variable includes three values: minor indicates little evidence of any impact; major, significant macroeconomic difficulties; severe, potential to halt economic progress.

Each of these potential measures has its own shortcomings. The preference exhibited by democratic states for using sanctions rather than force may make democracy an undesirable measure of credibility. If most sanctioners are democratic, there will be little variation in credibility. This measure also complicates cases wherein sending states coordinate through international institutions.

The variety of reasons for which sanctions may be considered limits the value of the source of threat as a measure of credibility. Bureaucracies or other manner of lower functionaries may be quite credible sources of information regarding threat of sanctions when the issues at hand are technical in nature. The last options – sender commitment and sender cost – are both qualitative and rough. With only three possible codings each, they likely fail to capture a considerable amount of variation. The cost to sender variable also produces multiple expectations: on the one hand, threatening a high-cost response may be a way to generate costly signals and thus be highly credible; on the other hand, threatening to shoot oneself in the foot usually does not inspire confidence.

Of these imperfect potential measures, one does provide a plausible approximation of the audience costs argument across sender regime type and issue. The specificity with which a state or organization is willing to state its intent provides an intuitive measure of willingness to follow through. Thus, I will adopt a transformation of the TIES sender commitment to measure c from Equation 4.3.2. To make a scale

bounded by 0 and 1, I use the following simple strategy. Let *k* be the number of values possible within a measure, *m* be the measure itself, and *c* be the credibility scale:

$$c = m/k \quad \text{(Eq. 4.4)}$$

The measure of credibility employed here, then, ranges from zero in cases where no threat is issued to 1 in cases with the most precisely voiced conditions of threat or punishment by increments of one third.

Regime Type

My theory posits a qualitative difference in the behavior of democratic and nondemocratic leaders. Variation will occur within the regime types, but the rules of behavior will differ across. Therefore, the measure of regime type employed must allow for separation between the types. This can be accomplished through dichotomization of democracy scales. As I have incorporated some of the expectations of winning coalition theory, it makes sense to begin with Bueno de Mesquita et al's (2005) *W* scale. The scale ranges from 0-1, taking on values at each quarter point. Smaller winning coalitions receive smaller values. I break the scale at 0.75, as this point achieves the highest correlation with standard dichotomization points of the PolityIV democracy-autocracy scale and has been utilized in the literature (Quiroz-Flores 2009). This indicator, *W*, will be interacted with the measures of foreign policy targeting and also with time functions. The need for complex interactions makes the binary measurement scheme desirable for ease of interpretation as well as for consistency with expectations.

Leader Experience

Archigos 2.9 provides the measure of leaders' tenure in office (Goemans et al. 2009). The cumulative days which a leader has been in office will be logged to reflect the impact of institutionalization and learning effects. If these processes hold, than the first few years of experience should contribute most to changes in the effectiveness of

external influence attempts. Additional years of experience will be less vital, causing a diminishing returns pattern.[36]

Control Variables

In order to isolate the impact of the theoretical variables there are a few other domestic factors which should be included in the analysis. The regime type indicator will be included as constituent terms to control for its independent impact on leader survival. Economic conditions are also likely to affect leaders' ability to hold onto power. Economic growth from the prior year, calculated using Gleditsch's (2002) expanded trade and GDP data, is included for this reason. I also include a measure of domestic unrest, level of civil conflict, from the Uppsala Data Project (Gledtisch et al. 2007).

Effectiveness Datasets

Compiling the data for Equation 4.3.1, regarding the effectiveness of positive sanctions, produces a dataset of 4,752 leader years covering 791 subjects from 1960-1999. For Equation 4.3.2, full information is available on 3,372 leader years containing

[36] Time in office, of course, could contain information on processes beyond learning and institutionalization. In order to entertain alternative hypotheses, I will test the log-linear specification against two alternative forms. First, a quadratic relationship will be tested, allowing for the possibility that leaders are more insecure in their positions both very early and very late in their careers. The quadratic term is specified as, $f(t_i) = t_i + t_i^2$, by including a constituent and a squared term. Second, a very flexible alternative is the cubic polynomial method recommended by Carter and Signorino (2007), which allows the function of experience to move in a wide range of ways. If this specification proves superior, it would be consistent with an argument that the effect of experience is largely unpredictable. The cubic polynomial is $f(t_i) = t_i + t_i^2 + t_i^3$, which introduces three additional terms into the model for every variable which is expected to vary over time. Because of issues of numerical stability (the values become extremely large), these two transformations will be performed on a rescaled experience term which moves in three-year units (Carter and Signroino 2007). I will arbiter between the three plausible forms of experience's impact using the Bayesian Information Criterion (BIC) to determine which measure best contributes to model fit.

649 leaders between 1971 and 2000. The foreign aid dataset excludes donor states. Including the donors muddies the reference category for analysis making it less likely that an effect for democratic leaders would be uncovered. Descriptive statistics, sources and coding rules are detailed in Tables 4.1 through 4.4.

Constructing the Effectiveness Measure

The analysis of sanction effectiveness provides a means of assessing the validity of hypotheses 1-3. Effectiveness, however, also plays a direct role in the likelihood of concessions according to Hypothesis 7. Testing this aspect of the argument requires production of a measure of effectiveness suitable for inclusion in the analysis of concessions. The censored probit regression of leader failure provides coefficients relating the effect of sanctioning (positive and negative) on the probability of targeted leaders losing office, after factoring out selection and endogeneity. These parameters will be utilized to construct a measure of effectiveness which will be sensitive to the observed context of leaders.

Because the parameter estimates incorporate estimation uncertainty into the measure of effectiveness, I will bootstrap the estimates for both the regressions of equations 4.3.1 and 4.3.2 and the subsequent tests of concessionary behavior. In the initial models, the bootstrap will: 1) ensure that estimates are not overly dependent on the particular configuration of observations, and 2) provide a mean estimate of coefficient and variance parameters . Bootstrapping performs repeated draws of random samples of size N with replacement from the observed data. This provides us, essentially with a large number of hypothetical samples upon which the regression can be performed. This process removes the risk of influential observations or outliers biasing our estimates of the true parameter values.

Bootstrapping also takes advantage of the desirable properties of sample means. Despite variations in the underlying distribution, the sample mean will provide an

unbiased estimate of the population mean. By averaging across the bootstrap samples, then, we can uncover our best possible estimates of the true parameter values. The bootstrap, then, should increase our confidence in the estimates of equations 4.3.1 and 4.3.2 and thus of the measure of effectiveness constructed from them.

Selecting a Measure of Effectiveness

With a reliable vector of $\widehat{\boldsymbol{\beta}}$ obtained from the bootstrapped censored probits described above, several options remain for the actual construction of a measure of effectiveness. I defined effectiveness as the ability of a sanction to appropriately affect a target's ability to stay in power, ranging theoretically from positive to negative infinity. Effectiveness varies given the institutionally conditioned impact of sanctions and leaders' experience. An acceptable measure of effectiveness must reflect these theoretical expectations. It must: 1) take on both positive and negative values; 2) reflect a change in the leader's ability to stay in power; 3) incorporate the conditioning impact of institutions and leader experience.

Beyond these theoretical requirements, however, we must also be sure that any measure of effectiveness is tapping only into those factors which we assume to be important. Many contextual elements can contribute to an increase or decrease in the probability of a leader's failure at any point in time. In order to provide a fair test of the hypotheses regarding effectiveness' role in influencing concessionary behavior, we must isolate it from these other contextual factors. Below I will discuss two potential measures, a first difference and a combined coefficient. While, the first difference measure holds substantial intuitive appeal, it does not isolate the impact of institutions and leader experience on leader survival. The combined coefficient measure, however, explicitly incorporates all three theoretical requirements while excluding the impact of all other variables.

We may observe effectiveness as the difference between a leader's ability to maintain office after being sanctioned versus the ability to maintain office sans sanctioning. An intuitive representation of this value could be obtained by differencing the probability of failure at a high and low level of sanctioning. The formula for this calculation would be:

$$\widehat{E_{it}} = P\left(Y_{1,it} = 1 \middle| \boldsymbol{X}^{s}, Y_{2,it} = 1\right) - P\left(Y_{1,it} = 1 \middle| \boldsymbol{X}^{s\sim}, Y_{2,it} = 1\right) \quad \text{(Eq. 4.4)}$$

$\widehat{E_{it}}$ gives the change in probability of failure for leader *i* in time *t* which would occur were a sanction to be initiated or changed. For each year of observation a calculation of the leader's contemporary survival prospects can be calculated using the estimates $\widehat{\boldsymbol{\beta}}$ and **X**, the observed values of covariates from Equation 4.3.1 or 4.3.2. Within leader-years these covariate values remain constant, allowing the hypothetical calculation of their probability of failure were an external power to initiate a new influence attempt or change an existing one. This "potential" probability would be calculated by shifting the value of the sanctioning terms in $\boldsymbol{X}\widehat{\boldsymbol{\beta}_{it}}$.

For an aid-receiving leader, for example, the quantity $\boldsymbol{X}\widehat{\boldsymbol{\beta}}_{it}{}^{S}$ would be calculated using his/her actual allocated aid for the given year, and $\boldsymbol{X}\widehat{\boldsymbol{\beta}}_{it}{}^{\sim S}$ would be calculated with that aid amount set to the minimum non-zero amount of aid which could be received. The difference between the probabilities at these two points would simulate the extent to which the aid level actually allocated to that leader insulates him/her from loss of office. Note that this change can be positive, negative or null, reflecting the full range of possible effectiveness depicted in Figure 3.1.

A similar calculation could be made for leaders targeted with economic sanctions. $\boldsymbol{X}\widehat{\boldsymbol{\beta}}_{it}{}^{S}$ would be calculated with their actual information regarding threat or imposition of sanctions. For $\boldsymbol{X}\widehat{\boldsymbol{\beta}}_{it}{}^{\sim S}$ the sanctioning would be switched off. To maintain intuitive scaling, for economic sanctions, the probability of survival given no sanctions could be subtracted from the probability of survival given sanctions. Because an effective

sanction would cause $P\left(Y_{1,it} = 1 \middle| \boldsymbol{X}^{s\sim}, Y_{2,it} = 1\right) > P\left(Y_{1,it} = 1 \middle| \boldsymbol{X}^{s}, Y_{2,it} = 1\right)$, positive values of this difference will indicate effectiveness while negative values indicate ineffectiveness.

While this first-differences measure obtains high intuitive value, it has some statistical draw-backs. Utilizing predicted probabilities introduces the functional form of the normal distribution into the measure. The majority of action in the normal distribution function occurs at mean values; the form restricts covariates' impact at the tails. When the probability of an event occurring is already relatively high (low), the value of any covariate must be extreme in order to produce any noticeable shift. Thus the first-differences measure of $\hat{E}_{it}$ would produce very small values for any observation where the ex ante probability of leader failure is either very low or very high. A second problem stems from the measure's use of the full $\boldsymbol{X\hat{\beta}_{it}}$. Because all covariates – not just those regarding sanctioning – contribute to the probability of failure, leaders with the same level of sanctioning may obtain very different $\hat{E}_{it}$ scores, based on the values of variables of little interest to the analysis.

In some ways both these problems are also virtues. The first differences approach is sensitive to the context of each leader, assigning a value of effectiveness reflective of their entire situation. And, it may be accurate that for leaders whose situation predisposes them to failure or survival, sanctioning can exert itself only at the margins. Nonetheless, these features may inhibit statistical inference. A measure which may avoid this issue can be constructed using a variation of a popular alternative interpretation strategy: the marginal impact.

The marginal impact of any covariate, x, is the derivative of the likelihood function with respect to x. Intuitively, the first derivative tells us the change in the dependent variable resultant from an instantaneous change in x. In the simplest of regression models, the marginal impact of x would simply be its slope coefficient. For multiplicative relationship, the impact of x is, instead, a composite of its own impact and

that of the other with which it is interacted. Nonlinear models further complicate matters, as their estimating functions implicitly assume that the impact of any covariate depends on the values of all other variables (see Kam and Franzese 2007).

For our purposes, the complications raised by the multiplicative relationship are good. Theoretically, the effectiveness of aid depends both on domestic institutions and the personal experiences of recipient leaders. The marginal impact can encompass these variations comfortably, allowing construction of a measure sensitive to the conditional nature of effectiveness.

The complications associated with the nonlinear estimation procedures, however, raise problems for the construction of a valid $\hat{E}_i$ using the marginal impact. First, a true marginal impact from a nonlinear model cannot be separated from the assumptions of functional form. The censored probit, for example, assumes errors share a joint Normal distribution. The cumulative distribution of the Normal distribution follows an S-shaped curve. At the tails of this S, the predicted probabilities of events are extreme, and even covariates with a very strong impact on the dependent variable can do little to shift the prediction in either direction. The functional form restriction would lead to the underestimation of effectiveness for leaders who happen to fall in this tails of the distribution.

Second, the marginal impact in a nonlinear model includes information regarding the value of *all other covariates*. Rather than conditioning evaluation of $\hat{E}_i$ only on the values of those variables which we know to directly impact effectiveness, a true marginal impact measure would include information from every variable used in the estimation. In the outcome data, then, such a procedure could potentially assign very different values of $\hat{E}_i$ to leaders with the same regime type and the same experience in office. The discrepancy would stem from variation in values of covariates included as statistical controls, the impact of which lie outside our main field of inquiry.

To overcome these problems, I take the first derivative of the linear index of the outcome equation of my censored probit rather than of the full likelihood function. The linear index is simply the product of all covariates and slope coefficients, prior to any nonlinear transformation. It can be thought of as the predicted value of the underlying, unobserved, continuous, latent variable Y* which we attempt to tap with the binary dependent variable of leader failure. Because the linear index is (obviously) linear, taking its first derivative does not require information from the functional form or from any non-pertinent variables.

The first derivative of the linear index of leader failure with respect to foreign aid dependence, which I will refer to from now on as the combined coefficient measure of effectiveness, is simply:

$$\partial \boldsymbol{X_{it}\beta_1}/\partial A_{it} = \beta_{11} + \beta_{12}\mathrm{W} + \beta_{13}\mathrm{f(t_i)} + \beta_{14}\mathrm{f(t_i)W} \quad \text{(Eq. 4.5)}$$

Where A_{it} is the aid dependence of leader *i* at time *t*;β_{11} is the slope coefficient for aid dependence; β_{12} is the slope coefficient for the interaction between aid dependence and big winning coalitions; W is an indicator for big winning coalition systems; β_{13} is the slope coefficient for the interaction between aid dependence and the natural log of *i*'s experience in office; $\mathrm{f(t_i)}$ is the natural log of *i*'s experience in office; and β_{14} is the slope coefficient for the interaction between aid dependence, big winning coalitions and the natural log of *i*'s experience in office. For nondemocratic leaders, W equals zero, thus the second and fourth term will drop out of the equation.

The combined coefficient measure for effectiveness of economic sanctions, then will be the derivative of the outcome stage of Equation 4.3.2 with respect to the sanction indicator. This will be:

$$\partial \mathbf{X_i\beta}/_{\delta S_{it}} = \beta_1 + \beta_6 c + \beta_7 \mathrm{f(t_i)}c + \beta_8 cW + \beta_9 \mathrm{f(t_i)}cW \quad \text{(Eq. 4.6)}$$

Where β_1 is the coefficient for the sanction indicator; β_6, for the interaction between credibility and sanctions; β_7,the interaction between credibility, time and sanctions; β_8,

for the interaction between credibility, big winning coalition systems and sanctions; β_9,for the interaction between time, credibility, big winning coalition systems and sanctions. For nondemocratic leaders, the terms including W will be multiplied by zero and drop out. The effectiveness of threats is obtained using an equivalent formula. The only difference is that the coefficients for interactions with threats rather than imposed sanctions would be used.

Basically, the combined coefficient measure of effectiveness uses the $\widehat{\boldsymbol{\beta}}$ as weights to adjust the factors which affect effectiveness to create an additive, linear measure. Simply throwing aid receipt, regime type and leader experience into the outcome equations would not give us as informative a measure of effectiveness. Partially, this is because the measure would be spread across multiple parameters. But, it would also be less efficient, because in themselves each of these variables contains information which is both pertinent and impertinent to the relationship between effectiveness and outcomes. Democracy may relate to improvements in human rights or economic growth or to voting records in the United Nations independent of its impact on how effectively foreign aid rewards recipient leaders. The combined coefficient measure of $\widehat{E}_i$ utilizes what we have learned about the relationship between regime type and experience and effectiveness to create a variable which strips out the extraneous aspects of their relationship to outcomes.

The combined coefficient measure indicates the magnitude and direction of aid's effect on leader's latent probability of failure across regime type and over time. Positive values indicate increases in the likelihood of losing office; negative, decreases. For economic sanctions, then, positive values of the combined coefficient indicate increasing effectiveness; negative values indicate ineffectiveness (see Figure 3.1). For foreign aid, effectiveness relates inversely to the trend in risk of losing office: negative values of the combined coefficient will indicate effectiveness, and positive values will relate ineffectiveness.

Modeling Concessions

The outcome of ultimate interest to this theory is whether outside powers manage to wring the desired policy concessions from targeted leaders. Precise evaluation of this dependent variable requires two things. First, we need record of the demanded changes. Second, we need to know whether/which changes occurred. While very good data now exist on the use of both aid and economic sanctions, reporting on these two pieces of information differs across the policy tools. Before an appropriate estimation technique can be identified, we must determine the level at which the dependent variable will be measured. The data situation does not allow the models of concession given positive vs. negative sanctions to parallel smoothly. Discussion in this section, consequently, is divided between the tests of the argument on economic sanctions and foreign aid.

Specification: Concession to Economic Sanctions

After the targeting of a sanction, each passing unit of time may result in either further sanctioning or some manner of resolution. I categorize sanction terminations in terms of how successful the sender state appears to have been in exerting influence. Sanctions can end with successful influence, with the target providing at least some of the demanded concessions. They can also end with sender's cutting their losses, lifting sanctions without the desired changes in target behavior. A less one-sided arrangement can be accomplished through negotiations, possibly involving third parties or additional institutions. From the beginning of a sanction episode, then, some probability exists that the sender will either win, lose, draw or continue playing. In event history parlance, such processes are referred to as "competing risks" (e.g. Box-Steffensmeier and Jones 2004).

Competing risks can be fruitfully modeled with a series of specially coded indicator variables. Each of these indicators takes on a value of one when the event of interest occurs, and of zero when the episode endures. When an alternative event occurs, the competing risks indicator is set to a missing value. Analysis carried out on variables

coded for competing risks allows each observation to factor into the estimation of the probability that no event will occur, censoring those with an alternative event only in terms of estimating the hazard of a specific outcome.

To evaluate the competing risks problem of economic sanctions termination, then, I will specify three separate probit models. [37] These models will calculate the probability of each of the three possible outcomes – win, lose, or draw – relative to continued duration. My theory states that the probability of a sender winning concessions (i.e. successfully influencing a target) depends both on the effectiveness of the sanction employed and the domestic political difficulty of providing the demanded concessions. Effectiveness, itself a function of institutional and temporal conditioning, can be measured with the combined coefficient described in the prior section. Domestic political difficulty depends on three factors: the type of concession, the strength of the opposition, and the experience of the targeted leader.

Incorporating these expectations, then, we can define each of the competing risks equations to follow the form:

$$P(Y = y_i|\boldsymbol{X}) = \Phi\big(\alpha + \beta_1 \widehat{E^t}_{it} - \beta_2 \widehat{\mathrm{E^s}}_{\mathrm{it}} - \beta_3 \mathrm{T} - \beta_4 \mathrm{S} - \beta_5 \mathrm{C}_{\mathrm{it}} - \beta_6 (O_{it} \times C_{it}) + \beta 7\, tiOit \times Cit + \boldsymbol{\beta D} + \boldsymbol{\beta Z} + \varepsilon i \quad \text{(Eq. 4.7)}$$

Where Φ is the cumulative normal distribution; Y is a vector containing the competing risks indicators, which can take on a value y_i for win, lose or draw, and 0 for years in which the episode endures; $\hat{E}_i$ indicates the combined coefficient measure of effectiveness, with superscripts dividing imposed sanctions, *s*, from threatened sanctions,

[37] It may seem desirable to run ordinal probit analysis of win-lose-draw by dropping the ongoing cases. However, this would require sacrifice of the 40% of cases which do not feature a termination event and all the information contained within them. We would also be introducing an inference problem; excluding cases which have potentially been the most difficult to resolve could amount to systematically throwing out high values of private costs/mobilizing propensity to concessions or low values of effectiveness. Moreover, it would not be unproblematic to perform ordinal analysis on the win-lose-draw trichotomy. If covariates contribute in different ways or magnitudes to the process of losing versus drawing versus winning, then ordinal analysis is inappropriate.

t ; *T* is an indicator variable for cases which terminate at the threat stage; *S* is an indicator which switches on the year that a threat becomes an imposed sanction and remains on until termination; *C* is a nominal, trichotomous variable taking values 1-3 for private, mixed, and public costs-inducing concessions respectively; *O* is the strength of the opposition, measured as the opposition share of the most recent popular elections in democratic systems and as the existence of a legislative branch in nondemocratic systems; *t* is the natural log of a leaders cumulative time in office; and ***D*** is the cubic polynomial of sanction episode duration, and the vector **Z** contains all remaining control variables. This approach is similar to using a multinomial probit, but requires far less computing time. The simpler estimation strategy also allows me to account for another complicated aspect of the sanction process. [38]

The formal literature on economic sanctions has argued that a selection problem plagues many extant studies of sanction efficacy (Schwebach 2000; Lacy and Niou 2004; Drezner 1999, 2006). The standard claim holds that strategic, rational actors facing costly punishment will surrender at the threat stage, producing a sample of imposed sanctions systematically less likely to be successful. The TIES dataset, which identifies the threat stage of the sanction process as well as the date at which threats become imposed punishments, allows modeling of what was once an unobservable, confounding factor for statistical analysis.

I will take advantage of this advancement by estimating a separate probit model of the imposition of economic sanctions. This model will incorporate the measures of effectiveness and domestic difficulty. If the selection problem is as powerful as formal

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[38] Authorities in the field officially recommend multinomial logit for discrete-time competing risks analysis (e.g. Box-Steffensmeier and Jones 2004). Multinomial logit makes an assumption called independence of irrelevant alternatives which is untenable in this case. We cannot say that were "draw" to be removed from the possible outcomes all formerly inconclusive cases would split equally between "lose", "win" and "ongoing". Probit avoids this assumption without losing the parallel logic between the Cox model given discrete time observations.

theorists suggest, then imposition of sanctions may be a more appropriate measure of successful influence than the actual outcome variables. I will allow the two processes, imposition and concession, to relate to each other by estimating them simultaneously, allowing the errors to share a joint normal distribution, such that the correlation between errors can be estimated.

Dependent Variables

Political scientists and economists interested in economic sanctions have long recognized the centrality of concessions to the question of success/failure, leading to explicit attempts to measure this aspect of the process (e.g. Hufbauer et. al 1990; Morgan et. al 2009). The TIES authors provide at least three means of assessing the "success" of threatened or imposed economic sanctions (Morgan et. al 2009, 98-102 and fn12). For each case, scales from 0-10 measure the outcome vis-à-vis each party, from the worst possible to best possible result. A second option might utilize these scales to create an outcome indicator which tags cases where the end-situation favors one party over the other. The third option is a categorical variable called *final outcome* which characterizes the outcome as one of the following: partial acquiescence by target to threat, complete acquiescence by target to threat, capitulation by sender at threat stage, stalemate at threat stage, negotiated settlement at threat stage, partial acquiescence by target following sanctions, complete acquiescence by target following sanctions, capitulation by sender following sanctions, stalemate following sanctions, negotiated settlement following sanctions, or unknown.

The three different options provide different views of "concession". The scale variables isolate each party's end satisfaction level. An indicator based on these variables could isolate cases where the sender ends up with higher preference than the target. These are valuable approaches, but they do not offer the highest fit to my theoretical focus on the ability of sending states to incite concessionary behavior.

This central aspect of the situation is best captured by the categorical variable *final outcome* which explicitly states whether concessions were provided. This variable also acknowledges that sanctions sometimes terminate as a result of sender decisions or the intervention of third-parties rather than a target's decision to concede. I will utilize the information in this variable to construct the competing risks dependent variables: *win*, *lose* and *draw*. Win will obtain when the target provides partial or complete concessions; lose when the sender capitulates; draw, when negotiated settlement takes place. Each variable will equal zero when no termination event occurs, and missing when an alternative event occurs.

For the imposition equation, I have created an indicator variable which switches on the year that TIES reports a threat to switch to enacted sanction. There is no need to code imposition as missing for competing risks purposes. The same imposition model will be estimated jointly with each of the possible termination types. This strategy will allow the calculation of the probability of each outcome type as well as the probability of imposition.

Independent Variables

Two possible means of differentiating type of costs are easily constructed using the TIES data (Morgan et al. 2009). First, sanctions have been differentiated by the group within the target which is most likely to suffer: the general public, the regime leadership, business interests, political interests, the military or other. This provides an easy differentiation between public (all coded as general public) and private costs (all others), but not mixed. A second strategy would characterize costs based on the issues over which sanctions were threatened/implemented. TIES provides a 15-point categorization. Among these, five correspond roughly to private costs;[39] three, to public

[39] The private costs issues are: destabilize regime, improve human rights, improve environmental policies, trade practices, and implement economic reform.

costs;[40] seven, to mixed costs.[41] Because this second strategy allows for the three-point scale, I will use that framework to create a diffusion of concession-costs scale. It will equal one under private costs, two under mixed and three under public costs. Table 4.6 at the close of the chapter provides more detailed information on this coding scheme.

Measurement of the strength of the opposition within democratic regimes is provided by the Vanhanen (2000) Polyarchy dataset, from 1810-2000. The competitiveness variable reports the percentage of legislative seats won by the largest party. All else equal, the smaller this figure, the greater the ability of the opposing parties to restrict the action of the government. In order to preserve intuitive interpretation, I subtract the competitiveness score from 1, such that a larger score corresponds to a larger opposition.

Such detailed information regarding the power of the opposition is frequently either not available or meaningless in the context of autocratic institutions. In lieu of an interval measure, then, I will utilize an indicator for the presence of legislative institutions in authoritarian systems. Several studies in comparative politics have begun using similar indicators to analyze variation in the behavior of autocratic states (e.g. Wright 2008b; Gandhi and Przeworski 2006, 2007). The best coverage in terms of states and temporal range for such an indicator comes from the new version of the Institutions and Elections Project (Regan, Frank and Clark 2009).

The final theoretical variable of consequence is the experience of the targeted leader in office. Experience will be measured using the log of experience as described earlier. As described in Equation 4.7, the cost scale variable, opposition strength and

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[40] The public costs issues are: release citizens, property or material (sovereignty issue); deter/punish drug trafficking practices; retaliate for alignment choice.

[41] The mixed costs issues are: contain political influence; contain military behavior; solve territorial dispute; deny strategic materials; end weapons/materials proliferation; terminate support for non-state actors.

time will be interacted to capture changing efficacy of the opposition over the course of leader's careers.

Control Variables

Control variables for the sanctions concessions and imposition models will include factors which have been found important in previous research. Others have concluded that the sheer size of the expected cost – rather than who it befalls – matters. Therefore the TIES ordinal coding of expected target and sender costs will be incorporated (Morgan et al. 2009). These variables are trichotomous, ordinal scales ranging from minor to severe. The economic and military power of states will be tapped by economic growth (Gleditsch 2002) and the CINC-score (Singer et al. 1972). Trade has also been a principle variable in tests of sanction outcome and imposition (e.g. Nooruddin 2002), so I include the natural log of the sum of imports and exports to all states. Regime type has also been a central factor in previous studies. Cox and Drury (2006), specifically, argue that the strategic democratic peace dynamics located by Bueno de Mesquita (1999) should transfer to economic sanctions. To account for this possibility, I include winning coalition size and an indicator to tag cases where both the sender and target state have big winning coalitions (Bueno de Mesquita et al. 2005). Lastly, I account for duration dependence with the cubic polynomial of sanction episode duration. The constituent terms for opposition strength and leader experience will also be included as controls.

Economic Sanctions Concession Dataset

Table 4.5 at the end of this chapter, provides descriptive statistics, sources, and coding rules for each of the variables which will be utilized in Chapter 8's analysis of concession to economic sanctions. The full dataset contains 4,693 sanctioned-leader years, the majority of which feature sanctions aimed at democratic leaders. The competing risks framework and the inability to measure opposition strength and effectiveness equivalently across regime types will produce a series of empirical tests in

Chapter 8, rather than just one unified model. The details of this process will be further described in that chapter.

Specification: Concession to Foreign Aid

Finding a dependent variable for concession given economic sanctions required little more than opening up a ready-made dataset. With foreign aid, however, the concept of "concession" has not been explicitly measured. This data problem prevents perfectly parallel evaluation of positive and negative sanction outcomes. Large portions of foreign aid arrive with very explicit conditions attached, but there has been no standardized effort to collect the particularities of these agreements. Empirical studies of aid concession, then, must employ proxies for compliance. These proxies require one of two assumptions: either that "results" stem from concession (e.g. Bearce and Tirone 2008; Lai and Morey 2006; Finkel et. al 2006; Knack 2004; Wang 1999), or that lack of punishment implies good faith efforts to comply (e.g. Dreher 2006; Stone 2004). Failure to punish, however, constitutes a better measure of donor credibility than of target action; there are good reasons to expect aid will not be reliably revoked upon bad behavior (Stone 2004; Killick 1997; Mosley 1993).

Assuming concession from results requires more than a leap of faith. Given the variation in donor goals, assessing success with respect to only one type of outcome risks the error of judging strategic aid by humanitarian standards or vice versa. The OECD Creditor Reporting System (CRS) provides donor states a forum to declare the purpose of their assistance, from infrastructure improvements, to general budget support, to gender equality/sensitivity. A more accurate connection between results and concessions could be drawn using such information. The system, however, is relatively new. Having begun in 2001, reporting becomes consistent only in 2005 (see Bermeo 2008). Observations currently extend only to 2007. With only two years of reliable observation on data so

recent that most controls would be unavailable, at this stage the CRS data do not present an ideal front for testing a complicated theory.

The remaining option, therefore, is to make the assumption that results imply concession without forgetting that there is more than one type of concession. The domestic costs of providing concession vary given the likelihood that the cost-bearing group will mobilize in response. Adopting only one dependent variable as an indicator of concession would prevent full evaluation of this argument. While variation may remain in the strength of opposition, all the observations included would feature the same cost-bearing group – either the public or a private interest. In order to test whether different types of costs correspond to different probabilities of follow through, at least two results must be analyzed: one which implies private costs were suffered and one which implies public costs were suffered.

Most humanitarian goals of foreign aid – economic growth/poverty reduction, human rights improvements, democratization – imply a loss of private perks for some powerful group. Democratization and human rights changes assail the status quo mechanisms of government directly, while economic reforms promise to tear down protectionist measures from which business lobbies profit. Economic reform concessions are most often proxied using economic growth, data which is widely available (Gleditsch 2002). Indices of democratic governance also cover broad temporal and spatial domains (Marshall and Jaggers 2009; Freedom House 2008; Vanhanen 2000). Human rights measures, slightly more limited, are available beginning in the 1980s (Cingranelli and Richards 2009; Gibney, Cornett and Wood 2008).

Strategic goals usually produce public or mixed costs. General realignment of foreign policy requires a subtle disservice, a failure to pursue the national interest. Reducing arms-buildups and weapons programs hurt the private firms which lose government contracts, but they also hold potential to decrease relative security vis-à-vis other states. Allowing external military presence through grants of air-space or basing

rights erodes sovereignty – a fundamentally public good. Arguably, the propping up of unstable regimes can also be considered a public cost. If instability stems from public dissatisfaction and poor performance, external intervention in favor of the regime runs counter to the public will. Scholars have investigated general realignment through S-scores of United Nations General Assembly voting patterns (e.g. Lai and Morey 2006; Wang 1999). Changes in arms-spending can be inferred by national military expenditures, available from the Correlates of War project (Singer et al. 1972). Regime stability can also be easily assessed with existing data on regime type (Marshall and Jaggers 2009).

For each type of cost, then, three plausible measures of concession exist. For this project, I will adopt those which allow the greatest contribution to extant debates within the field. Among indicators for private cost-producing concessions, economic growth has received the most attention and is the most frequently touted goal of aid agencies. This ability to speak to a wide body of research makes growth the most desirable dependent variable in the private costs category. Here it will be measured using Gleditsch's expanded trade and GDP data (Gleditsch 2002). In the realm of public costs-generating concessions, UNGA voting patterns are the most frequently studied concession. Affinity scores between donor and recipient states will be utilized as the dependent variable for public costs (Gartzke 2006). Below I will discuss these variables in more depth. Following the explications of prior tests, I will first present the model specifications.

Generally, both UNGA affinity and economic growth are modeled using OLS regression, and my models will follow suit. For the private costs model, a dataset of aid-receiving leaders will suffice. The public costs measure of affinity, however, requires a set of donor-recipient dyads. This will be constructed using OECD reporting information

of donor allocations.[42] Among the sample of aid-receiving leaders and donor-recipient dyads we can estimate the following models:

$$C_{public} = \beta_1 A_{ijt} + \beta_2 \widehat{E_{it}} + \beta_3 A_{ijt} \hat{E}_{it} + \beta_4 O + \beta_5 f(t_i) + \beta_6 O \times f(t_i) + \boldsymbol{\beta Z} + \varepsilon \quad \text{(Eq. 4.8)}$$

$$C_{private} = \beta_1 A_{it} + \beta_2 \widehat{E_{it}} + \beta_3 A_{it} \hat{E}_{it} + \beta_4 O + \beta_5 f(t_i) + \beta_6 O \times f(t_i) + \boldsymbol{\beta Z} + \varepsilon \quad \text{(Eq. 4.9)}$$

Where C_{public} is change in affinity between targeted state *i* and donor *j* in the current year, and $C_{private}$ is the current year's growth rate in the target country. The constants **α** give slope coefficients, and **β** are OLS slope coefficients. The effectiveness of the aid money in stabilizing the targeted leader is represented by $\widehat{E_{it}}$, the combined coefficient measure of effectiveness. A_{ijt} represents the aid flow between state *i* and donor *j*, while A_{it} is the total flow of aid monies from all donors into state *i*. O is a measure of the strength of the opposition. The leader's experience in office will be modeled by $f(t_i)$, the natural log of cumulative days in office. Interacting this variable with opposition strength allows tests of whether the opposition's efficacy depends on leader's expertise or security in office. **Z** contains control variables appropriate to each dependent variable. Finally, the stochastic error terms, **ε**, are independently distributed normal with mean zero and variance one.

Dependent Variables

Equations 4.8 and 4.9 must be estimated separately. The units of observation across models do not mesh as the public-costs concessions will be assessed via donor-recipient dyads and the private-costs concessions with leader-years. Each dependent variable, however, must reflect the potential of concessionary behavior as a result of positive sanctioning. These variables, in other words, should emphasize *change*, rather

[42] Due to the use of these proxies, population selection also becomes controversial. All states experience some level of growth, and all states cast votes in the UNGA. These actions are not unique to aid-receiving leaders. Thus we must consider whether to include leaders who do not receive aid in the analysis. Because none of the theoretical variables should be expected to function similarly in cases of aid-receivers and non-aid-receivers, I feel justified in excluding the non-sanctioned leaders.

than extant levels of economic stability or foreign-policy affinity. To preserve this fundamental theoretical requirement, each of the dependent variables will be annual growth rates.[43]

Previous studies of UNGA affinity have utilized either the raw affinity score for important votes (Lai and Morey 2006) or the change in affinity (Wang 1999). Using a percent change from year to year, then, departs from tradition. I prefer to use the growth rate, principally, because a raw score reflects only a static agreement between the recipient and donor. This type of measure could be adversely affected by selection, if donors tend to allocate aid to leaders who agree with them more or less. More centrally, however, the one-shot measure does not tell us about concessionary behavior. If recipients are providing concessions in the UNGA, we should see movement in their voting behavior. The annual growth rate in UNGA affinity scores, is constructed using Gartzke's (2006) S-scores.[44] I subtract the prior year's affinity score from the current year's and divide by the prior year's score. To prevent missing information when affinity scores equal zero, I add one to both the numerator and denominator. The formula for growth in affinity, then, is:

$$S_{ijt} = (1 + s_{ijt} - s_{ijt-1})/(1 + s_{ijt-1}) \quad \text{(Eq. 4.10)}$$

Where S_{ijt} is the growth rate and s_{ijt} is the observed S-score between state *i* and donor *j* in time *t*. Adopting this transformation could introduce some problems, though, which should be examined before proceeding.

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[43] This strategy also has the added appeal of placing growth and affinity on similar scales, which will ease assessment of hypotheses regarding the relative difficulty of private- and public-costs concessions.

[44] For information on the calculation of S-scores, see Signorino and Ritter (1999). Basically, an S-score is a spatial measure of similarity between the voting pattern of two states which is bounded by -1 (complete dissimilarity of voting patterns) and +1 (identical patterns).

An immediate concern may be that aggregate voting patterns feature inertia, causing the growth rate to be highly static. While a legitimate objection, this does not appear to be empirically true. The growth in affinity scores averages .71 in the full sample of dyads provided by Gartzke (2006) with a standard deviation of .20. Its range includes both shifts towards and away from donors.

Some may also object that donor-recipient dyads which feature perfect agreement and continue to do so over the years will not be tapped as successful concessions when using a growth rate. It is true that the measure would consider a shift from zero agreement to, say, fifty-percent agreement, to be more evidence of concession than a perfect score which remains so. Rather than a problem, I consider this to be a virtue of the growth rate approach. Shifting closer is a bigger concession than staying the same and should be treated as such.[45]

The use of annual growth in GDP also presents some challenges. Most studies of aid's impact on growth utilize panels of data averaged over four to ten years in order to capture long-run changes in the economy (e.g. Wright 2008; Burnside and Dollar 2000, 2004; Daalgard et al. 2004). Because the principle theoretical variables of aid effectiveness, opposition strength, and leader experience move in the short term, I cannot adopt that approach. Averaging over four years would eliminate all of the variation in leaders' incentives in which I am interested. Recently, Thies and Sobek (2010) analyzed the relationship between growth and war without averaged panels. They preserved similarity with earlier approaches by including independent variables to account for the neoclassical growth model's key factors of physical capital, human capital, government

[45] Incidentally, this issue does not surface in the data. Of the full set of dyads, only 1,562 feature S-scores of 1, and none of them exhibited prior or subsequent perfect scores. They were assigned growth rates of between .5 and 1.17 with a mean of .56.

efficiency and baseline capacity for growth (e.g. Barro and Lee 1994; Koubi 2005; Feng 2003). I will follow this example.

Independent Variables

The key theoretical variables of opposition strength will be measured as described above for the economic sanctions model. For democratic leaders I transform Vanhanen's (2000) competitiveness variable by subtracting the seat share of the largest party from one to give a measure of how many seats the remaining parties control. In nondemocratic states, the existence of a legislative branch must suffice (Regan et al. 2009). Leader experience, again, will be captured by the natural log of the target's cumulative days in office taken from Archigos 2.9 (Goemans et al. 2009).

The remaining important theoretical variable for both regressions is the positive sanction itself, foreign aid. In the dyadic sample, I developed a measure to tap into the specific attempt at influence within each dyad. Dyadic aid dependence is the proportion of total net aid dependence attributable to donor *j*. More precisely, it is calculated as:

$$A_{ijt} = (a_{ijt}\ per\ capita/GDP\ per\ capita) \times 1/(a_{it}\ per\ capita/GDP\ per\ capita) \quad \text{(Eq. 4.11).}$$

Where a_{ijt} is the aid flow between recipient *i* and donor *j* in dollars; GDP is gross domestic product of recipient *i*'s state in year t; and a_{it} is total aid flow from all donors *J* to recipient *i* in year *t*. This measure holds two advantages over a simple measure of aid flow. First, it helps to incorporate the concept of credibility/commitment which could not otherwise be measured in the aid process. Senders who commit to provide the bulk of a recipient's aid portfolio should be considered more invested in the process of influence than those who contribute only a small portion. Second, dyadic dependence also tracks more closely to the recipient's specific obligations to the donor at hand rather than to donors in general.

Unfortunately, these advantages are not available for the monadic dataset of economic growth. Instead, this regression uses simple bilateral net aid dependence, the measurement of which was discussed earlier.

Control Variables

As these models tap into fundamentally different processes, the appropriate controls differ across models. I will begin by discussing those in the public-concesisons model. First of all, the ability to move one's affinity score depends upon the starting place. I include a variable which captures the dyad's first recorded affinity score to account for this potential stickiness.

I also incorporate a couple alternative measures of aid to ensure that the dyadic dependence measure really taps into obligations to the specific donor rather than just to the broad receipt of aid. The first of these is a measure of total aid received and the second is an indicator for non-zero net aid received as it is not uncommon for the overall combination of aid received and interest paid to cancel out (OECD 2009).

Further controls in this model attempt to account for other aspects of the dyadic relationship. I include measures to tap any violent interactions between the recipient and donor with joint MID involvement, and level of hostility in any militarized within the dyad (Ghosn et al. 2004). As UNGA votes are strongly related to geographical region, I include an indicator which determines whether the two states hail from the same region using Hensel and Diehl's (1994) regional indicators. I also construct indicators U.S.-Latin American dyads and U.S.-Middle Eastern dyads. I further account for non-concessionary changes in UNGA affinity by including a measure of regime similarity. This measure differences the Polity2 democracy scale of donors and recipients (Marshall and Jaggers 2009). I also include the W-scale based measure of similarity, joint democracy, which switches on if both states have winning coalition scores of .75 or greater. Other means of financial dependence on the donor state are captured by the

lagged and logged value of goods imported and exported from the donor to the recipient (Gleditsch 2002). The tendency of states in the "global south" to separate from the United States and its allies following the end of the Cold War is accounted for with an indicator which switches on in 1989. Finally I include economic growth as measures of the power of the recipient state (Gleditsch 2002).

In the economic growth model, as mentioned above, I incorporate measures consistent with the neoclassical model of economic growth (e.g. Koubi 2005; Thies and Sobek 2010). I capture human capital with population growth taken from World Development Indicators and infant mortality rate, the most updated version of which is offered by Abouharb and Kimball (2007). To account for physical capital, I include the government investment share, which is the ratio of government investment to GDP (Heston, Summers and Aten 2009). Government efficiency is captured by consumption over GDP excluding "productive" spending on defense, education, etc. (Heston et al. 2009). Finally, I include measures of initial GDP/capita and initial growth rate to account for income convergence. As additional controls, I include population size, lagged trade (Gleditsch 2002) and oil reserves (Energy Information Administration 2009).

Foreign Aid Concessions Datasets

The full datasets for public- and private-costs concessions to foreign aid are summarized in Tables 4.7 and 4.8 at the close of this chapter. The data cover the same temporal range, 1960-1999, dictated by the availability of foreign aid and W-scale data. Obviously, the sample size for the dyadic sample outstrips that for the monadic data. In the UNGA voting model, the full sample includes 99,578 donor-recipient-dyad years, about two-thirds of which feature nondemocratic targets. The growth models include 3,818 recipient-leader years. While most of the difference in size stems from the difference in unit of analysis, some cases are also lost in the growth model due to the

heightened difficulty of getting economic data versus the strategic data used as controls in the affinity models.

Table 4.1 Variables for Chapter 5's Model of Aid Allocation

Variable	Coding	Mean	Std. Dev.	Min	Max
Aid Allocation[1]	0 if zero net aid allocation from OECD donors in current year, (14.4%) 1 1 if nonzero net aid allocation from OECD donors in current year (85.6%)	.8560	.3511	0	1
Lagged Aid[1] Allocation	0 if zero net aid allocation from OECD donors in prior year (16.5%) 1 if non-zero net aid allocation from OECD donors in prior year (83.5%)	.8352	.3711	0	1
W, Big Winning[2] Coalition	0 if W-scale of wining coalition size is <.75, (64%) 1 if W-scale of winning coalition size is ≥.75, (36%)	.3566	.4791	0	1
Internationalized[3] Civil Conflict	0 if no recorded deadly civil conflict involving international actor, (98%) 1 if minor civil conflict involving international actor, no more than 25 deaths, (0.6%) 2 if civil war involving international actors, at least 1,000 deaths, (1.2%)	.0310	.2340	0	2
Oil Producing State[4]	0 if country has no known oil reserves, (44.5%) 1 if country has known oil reserves, (55.5%)	.5551	.4970	0	1
Former Colony	0 if never colonized, (32.8%) 1 if formerly colonized (67.2%)	.6723	.4694	0	1
Strong OECD Ally[5]	0 if no defensive or offensive alliance with any OECD donor state, (67%) 1 if defensive or offensive alliance with one or more OECD donor states, (33%)	.3303	.4704	0	1

Table 4.1 Continued

Variable	Coding	Mean	Std Dev.	Min	Max
Instrument for Probability of Failure, $Y_{1,it}^{**}$	Linear prediction (Xβ) of auxiliary regression of leader failure including all aid-eligible leaders divided by the standard error of the prediction.	-17.56	6.429	-32.36	2.337
Lagged Population[6] Growth	Last year's population in thousands, minus prior year's population in thousands, divided by prior year's population	2.3543	1.689	0	5.196
Population Size[7]	Natural log of population size in thousands	8.879	1.564	4.806	14.04
Lagged Imports from OECD Donor States[7]	Natural log of dollar value of goods imported from OECD donor states in prior year	.7311	.8692	0	5.196
Lagged Economic[7] Growth	Last year gross domestic product minus prior year's gross domestic product, divided by prior year's gross domestic product	.0083	.0858	-2.219	.6549
W-Scale[2]	Scale of winning coalition size	.4891	.2802	0	1

Table 4.1 Continued

Dataset Characteristics	
Unit of Observation	Aid-eligible leader years, includes all known leaders of states recognized by OECD as donor nations
Temporal Range	1960-1999, lower bound determined by availability of aid allocation data; upper by availability of W-measure
N	4,752

SOURCES:

1 Organization for Economic Cooperation and Development. (2009) "OECDstat Extracts". Available online at <oecd.stat.org>.

2 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival*.

3 Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. (2002) "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research*, 395.

4 Energy Information Administration. (2009) "International Energy Statistics". Available online at < http://www.eia.doe.gov/>. Accessed: April 2009.

5 Leeds, Brett Ashley, Jeffrey M. Ritter, Sara McLaughlin Mitchell, and Andrew G. Long. (2002) "Alliance Treaty Obligations and Provisions, 1815-1944." *International Interactions* 28: 237-260.

6 World Bank, World Development Indicators Online.

Table 4.2 Variable for Chapter 7's Test of Onset of Economic Sanctions

Variable	Coding	Mean	Std. Dev.	Min	Max
Target [1]	0 if no threats or enacted sanctions leveled in current year, (64.2%) 1 if threats or enacted sanctions leveled in current year, (35.8%)	.3578	.4794	0	1
Lagged Target[1]	0 if no threats or enacted sanctions in prior year, (65.8%) 1 if threats or enacted sanctions in prior year, (34.2%)	.3421	.4744	0	1
Former Colony	0 if state was not formerly colonized, (43.1%) 1 if state was formerly a colony, (56.9%)	.5690	.4953	0	1
Militarized Interstate Dispute[2]	0 if state not involved in a MID in current year, (66%) 1 if state involved in a MID in current year, (34%)	.3394	.4736	0	1
Revisionist State[2]	0 if state was not the revisionist actor in a MID in current year, (82.7%) 1 if state was revisionist actor in a MID in current year, (17.3%)	.1735	.3787	0	1
Intensity of Civil Conflict[3]	0 if no fatal civil conflict in current year, (84.6%) 1 if minor civil conflict of no more than 25 deaths in current year, (11.3%) 2 if civil war causing at least 1,000 deaths in current year, (4%)	.1948	.4887	0	2
Winning Coalition Size[4]	Scale of winning coalition size with 0 being smallest and 1 being largest	.5591	.3190	0	1

Table 4.2 Continued

Variable	Coding	Mean	Dev.	Min	Max
Instrument for Probability of Leader Failure	Linear index (Xβ) from probit regression of leader failure including all leaders	-1.297	.4501	-2.354	1.107
Net Aid Dependence[5]	Natural log of net one plus aid allocations per capita divided by GDP per capita	1.716	1.713	-1.589	5.805
Lagged Total Trade[6]	Natural log of dollar value of prior year's trade with all states	1.431	2.144	-6.230	7.391
Population Size[6]	Natural log of population size in thousands	9.102	1.543	5.327	14.039
CINC Score[7]	Share of global military and economic power, including military size and spending, iron and steel production, and population size	.0073	.0207	0	.1755
Data Characteristics					
Unit of Observation	Leader years				
Temporal Range	1971-1999, lower bound determined by TIES data, upper by W data				
N	3,777 with 649 separate leaders				

SOURCES:

1 Morgan, T. Clifton, Navin Bapat, and Valentin Krustev (2009) "The Threat and Imposition of Economic Sanctions, 1971-2000". *Conflict Management and Peace Science*, 26(1): 92-110.

2 Ghosn, Faten, Glenn Palmer, and Stuart Bremer. (2004) "The MID3 Data Set, 1993–2001: Procedures, Coding Rules, and Description." *Conflict Management and Peace Science* 21:133-154.

Table 4.2 Continued

3 Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research,* 395.

4 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival*.

5 Organization for Economic Cooperation and Development. (2009) "OECDstat Extracts". Available online at <oecd.stat.org>.

6 Gleditch, Kristian Skrede (2002) "Expanded Trade and GDP Data". *Journal of Conflict Resolution*, 46: 712-24.

7 Singer, J.David, Stuart Bremer and John Stuckey. (1972). "Capability Distribution, Uncertainty, and Major Power War, 1820-1965." in Bruce Russett (ed) *Peace, War, and Numbers*, Beverly Hills: Sage, 19-48.

Table 4.3 Variable for Chapter 5's Test of Foreign Aid Effectiveness

Variable	Coding	Mean	Std. Dev.	Min	Max
Winning Coalition Failure[1]	0 if Leader maintains office in current year, or if loss of office is regular with entry of an "heir/successor", (91%) 1 if Leaders loss of office in current year is irregular or followed by entry of "challenger" or "neutral party", (9%)	.0901	.2864	0	1
Big Winning Coalition System, W[2]	0 if W-scale <.75, (64.3%) 1 if W-scale ≥ .75, (35.7%)	.3566	.4791	0	1
Intensity of Civil Conflict[3]	0 if no fatal civil conflict in current year, (84.6%) 1 if minor civil conflict of no more than 25 deaths in current year, (11.3%) 2 if civil war causing at least 1,000 deaths in current year, (4%)	.2386	.5331	0	2
Aid Dependence, A_{it}[4]	Natural log of one plus net aid per capita divided by GDP per capita	2.310	1.862	-8.03	6.123
Aid Dependence×Time, $f(t_i)A_{it}$[4,5]	Aid dependence (see above) multiplied by the natural log of leader's cumulative days in office	16.82	14.33	-57.65	54.48
Aid Dependence×Big Winning Coalition System, WA_{it}[2,4]	Aid dependence (see above) multiplied by indicator for big winning coalition system	.7772	1.432	-4.496	5.802

Table 4.3 Continued

Variable	Coding	Mean	Std Dev.	Min	Max
Aid Dependence×Big Winning Coalition×Log of Time, $f(t_i)WA_{it}$[2,4,5]	Aid dependence (see above) multiplied by indicator for big winning coalitions and the natural log of leader's cumulative days in office	5.267	9.990	-29.55	50.35
Log of Leader's Cumulative Time in Office, $f(t_i)$[5]	Natural log of leader's cumulative days in office	7.234	1.424	0	9.740
Lagged Economic Growth[6]	Last year's GDP/capita minus prior year's GDP/capita, divided by prior year's GDP/capita	.0083	.0860	-2.219	.6550
Lagged Total Trade[6]	Natural log of dollar value of prior year's total trade with all states	.4254	1.913	-7.435	6.261
Data Characteristics					
Unit of Observation	Aid-eligible leader years				
Temporal Range	1960-1999, lower bound determined by aid data, upper by W-scale				

Table 4.3 Continued

Data Characteristics	
N	4,752 leader years, 543 of which will be censored in the second stage of the censored bprobit as they do not receive nonzero net aid flows

SOURCES:

1 Original coding by author, building on data from Archiogs 2.9.

2 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival*.

3 Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research*, 395.

4 Organization for Economic Cooperation and Development. (2009) "OECDstat Extracts". Available online at <oecd.stat.org>.

5 Goemans, Hein, Kristian Skrede Gleditsch and Giacomo Chiozza. (2009) "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research*, 46(2), (March) 2009: 269-183.

6 Gleditch, Kristian Skrede (2002) "Expanded Trade and GDP Data". *Journal of Conflict Resolution*, 46: 712-24.

Table 4.4 Variable for Chapter 7's Test of Economic Sanction Effectiveness

Variable	Coding	Mean	Std. Dev.	Min	Max
Leader Failure[1]	0 if Leader maintains office in current year, (85.2%) 1 if Leader loses office in current year for reason other than natural death, (14.8%)	.1480	.3552	0	1
Sanction[2]	0 if no sanction enacted in current year, 73.7%) 1 if sanction enacted in current year, (26.3%)	.2628	.4402	0	1
Threat[2]	0 if no sanction threatened in current year, (77.9%) 1 if sanction threatened in current year, (22.1%)	.2213	.4151	0	1
Credibility[2]	0 if no threat or sanction issued in current year, (70.4%) .333 if threat/sanction issued with weak sender commitment, (2.5%) .667 if threat/sanction issued with moderate sender commitment, (8%) 1 if threat/sanction issued with high sender commitment, (19%)	.2527	.4070	0	1
Big Winning Coalition System, W[3]	0 if W-scale <.75, (64.3%) 1 if W-scale ≥ .75, (35.7%)	.4544	.4980	0	1
Credibility × Sanction, cS_{it}^2	Sanction indicator multiplied by credibility scale	.1914	.3747	0	1

Table 4.4 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Credibility × Big Winning Coalition System × Sanction, $cW_{it}S_{it}^{2,3}$	Sanction indicator multiplied by indicator for big winning coalition system and credibility scale	.1165	.3046	0	1
Credibility × Sanction × Time, $cf(t_i)S_{it}^{1,2}$	Sanction indicator multiplied by credibility scale and natural log of leader's cumulative days in office	1.302	2.623	0	9.725
Credibility × Big Winning Coalition System × Sanction × Time, $cf(t_i)W_{it}S_{it}^{1,2,3}$	Sanction indicator multiplied by indicator for big winning coalition system, credibility scale and natural log of leader's cumulative days in office	.7634	2.043	0	8.795
Credibility × Threat, cS_{it}^{2}	Threat indicator multiplied by credibility scale	.1692	.3537	0	1
Credibility × Big Winning Coalition System × Threat, $cW_{it}S_{it}^{1,2,3}$	Threat indicator multiplied by indicator for big winning coalition system and credibility scale	.1306	.3195	0	1
Credibility × × Threat × Time, $cf(t_i)S_{it}^{1,2}$	Threat indicator multiplied by credibility scale and natural log of leader's cumulative days in office	1.140	2.440	0	9.597
Credibility × Big Winning Coalition System × Threat × Time, $cf(t_i)W_{it}S_{it}^{1,2,3}$	Threat indicator multiplied by indicator for big winning coalition system, credibility scale and natural log of leader's cumulative days in office	.8550	2.138	0	9.061

Table 4.4 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Lagged Economic Growth[4]	Last year's GDP/capita minus prior year's GDP/capita, divided by prior year's GDP/capita	.0066	.0919	-2.219	.6016
Lagged Total Trade[4]	Natural log of dollar value of last year's total trade with all countries	1.431	2.144	-6.230	7.391
Intensity of Civil Conflict[5]	0 if no fatal civil conflict in current year, (84.6%) 1 if minor civil conflict of no more than 25 deaths in current year, (11.3%) 2 if civil war causing at least 1,000 deaths in current year, (4%)	.1948	.4887	0	2
Aid Dependence, A_{it}[6]	Natural log of one plus net aid per capita divided by GDP per capita	1.716	1.713	-1.589	5.805
Aid Dependence×Time, $f(t_i)A_{it}$[1,6][4,5]	Aid dependence (see above) multiplied by the natural log of leader's cumulative days in office	12.66	13.23	-12.54	53.21
Aid Dependence × Big Winning Coalition System, WA_{it}[3,6]	Aid dependence (see above) multiplied by indicator for big winning coalition system	.5771	1.218	-.4931	5.805
Aid Dependence × Big Winning Coalition × Log of Time, $f(t_i)WA_{it}$[1,3,6]	Aid dependence (see above) multiplied by indicator for big winning coalitions and the natural log of leader's cumulative days in office	3.941	8.570	-3.137	50.38
Log of Leader's Cumulative Time in Office, $f(t_i)$[1]	Natural log of leader's cumulative days in office	7.141	1.445	0	9.739

Table 4.4 Continued

Data Characteristics	
Unit of Observation	Leader years
Temporal Range	1971-1999, lower bound determined by TIES data, upper by W data
N	3,783 leader years, including 651 separate leaders

SOURCES:

1 Goemans, Hein, Kristian Skrede Gleditsch and Giacomo Chiozza. (2009) "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research*, 46(2), (March) 2009: 269-183.

2 Morgan, T. Clifton, Navin Bapat, and Valentin Krustev (2009) "The Threat and Imposition of Economic Sanctions, 1971-2000". *Conflict Management and Peace Science*, 26(1): 92-110.

3 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival*.

4 Gleditch, Kristian Skrede (2002) "Expanded Trade and GDP Data". *Journal of Conflict Resolution*, 46: 712-24.

5 Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research,* 395.

6 Organization for Economic Cooperation and Development. (2009) "OECDstat Extracts". Available online at <oecd.stat.org>.

Table 4.5 Variable for Chapter 8's Test of Concessions to Economic Sanctions

Variable	Coding	Mean	Std. Dev.	Min	Max
Win[1]	0 if sanction episode endures in current year, (91.7%) 1 if target offers partial or complete concessions in current year, (2.9%) Missing if sender capitulates or negotiated settlement takes place in current year (5.3%)	.0312	.1735	0	1
Lose[1]	0 if sanction episode endures in current year, (91.7%) 1 if sender capitulates in current year, (3%) Missing if target concedes or negotiated settlement takes place in current year, (5.2%)	.0324	.1770	0	1
Draw[1]	0 if sanction episode endures in current year, (91.7%) 1 if negotiated settlement takes place in current year, (2.3%) Missing if target concedes or sender capitulates in current year, (6%)	.0240	.1532	0	1
Imposed[1]	0 if sanction remains at threat stage in current year, (24.7%) 1 if sanction episode is imposed in current year, (75.3%)	.5613	.4963	0	1
Diffusion of Concession-Cost Scale[1]	1 if issue suggests private costs, (77.8%) 2 if issue suggests mixed costs, (11.6%) 3 if issue suggests public costs, (10.5%)	1.327	.6559	1	3

Table 4.5 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Legislative Branch[2]	0 if system has no legislative branch, (2.3%) 1 if system has legislative branch, (97.7%)	.9770	.1506	0	1
Winning Coalition Size[3]	Scale of winning coalition size, 0 being smallest, 1 largest	.7639	.2603	0	1
Joint Democracy[3]	0 if sender and target have nondemocratic or mixed institutions, (40%) 1 if both sender and target state have big winning coalition systems, (60%)	.5966	.4906	0	1
Anticipated Sender Costs[1]	0 if no costs coded for sender state, (43.2%) 1 if modest costs expected for sender state, (53.4%) 2 if moderate costs expected for sender state, (3%) 3 if severe costs expected for sender state, (.5%)	.6070	.5710	0	3
Anticipated Target Costs[1]	0 if no costs coded for sender state, (43.2%) 1 if modest costs expected for target state, (53.4%) 2 if moderate costs expected for target state, (3%) 3 if severe costs expected for target state, (.5%)	.7464	.6904	0	3
Sanction[1]	0 if threat never leads to an enacted sanction, (37.8%) 1 if threat eventually becomes enacted sanction, (63.2%)	.6322	.4823	0	1
Threat[1]	0 if no threat ever issued, (25%) 1 if threat issued. (75%)	.7526	.4315	0	1

Table 4.5 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Opposition Strength[4]	Measure of electoral success of opposition parties, subtracts the seat share of the largest party from one	55.39	23.42	30	100
Leader Experience[5]	Natural log of leader's cumulative days in office	6.990	1.218	.6931	9.725
Opposition Strength × Cost Scale, $O_{it} \times C_{it}$[1,4]	Opposition strength multiplied by diffusion of concession-costs scale (see above)	75.28	56.25	30	300
Opposition Strength × Cost Scale × Time, $f(t_i)(O_{it} \times C_{it})$[1,4,5]	Opposition strength multiplied by diffusion of concession-costs scale and time (see above)	536.4	445.0	59.87	2778
Legislative Branch × Cost Scale, $O_{it} \times C_{it}$[2,4]	Legislative branch indicator multiplied by diffusion of concession-costs scale (see above)	1.287	.6677	0	3
Legislative Branch × Cost Scale × Time, $f(t_i)(O_{it} \times C_{it})$[1,2,4]	Legislative branch indicator multiplied by diffusion of concession-costs scale and time (see above)	9.005	5.045	0	28.94

Table 4.5 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Lagged Economic Growth[6]	Last year's GDP/capita minus prior year's GDP/capita, divided by prior year's GDP/capita	.0487	.0627	-.6257	.6036
Lagged Total Trade[6]	Natural log of dollar value of last year's trade with all states	4.228	2.064	-4.556	7.391
CINC-score[7]	Targeted state's share of global economic and military power, by military size and spending, coal and steel production, and population	.0331	.0458	0	.1755
Duration of Sanction Episode[1]	Cumulative years since start of sanction episode, in three year units	1.950	1.706	.3333	9
Squared Duration of Sanction Episode[1]	Duration of sanction episode (see above) squared	6.714	11.63	.1111	81
Cubed Duration of Sanction Episode[1]	Duration of sanction episode cubed	31.76	83.07	.0370	729
Leader's Time in Office[5]	Leader's cumulative time in office, three year units	1.756	1.933	.0018	15.29
Squared Leader's Time in Office[5]	Leader's Time in Office (see above) squared	6.820	17.62	.0000	233.7
Cubed Leader's Time in Office[5]	Leader's Time in Office (see above) cubed	42.80	192.6	.0000	3572

Table 4.5 Continued

Data Characteristics	
Unit of Observation	Sanctioned-leader year
Temporal Range	1971-1999, lower bound determined by TIES data, upper by W data
N, democratic sample	3,540 sanctioned-leader years
N, nondemocratic sample	1,147 sanctioned-leader years

SOURCES:

1 Morgan, T. Clifton, Navin Bapat, and Valentin Krustev (2009) "The Threat and Imposition of Economic Sanctions, 1971-2000". *Conflict Management and Peace Science*, 26(1): 92-110.

2 Regan, Patrick M. Richard W. Frank, David H. Clark. (2009) "New Datasets on Political Institutions and Elections, 1972-2005". *Conflict Management and Peace Science*, 26(3): 296-304.

3 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival*.

4 Vanhanen, Tatu. (2000) "A New Dataset for Measuring Democracy, 1810-1998". *Journal of Peace Research*, 37(2): 251-65.

5 Goemans, Hein, Kristian Skrede Gleditsch and Giacomo Chiozza. (2009) "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research*, 46(2), (March) 2009: 269-183.

6 Gleditch, Kristian Skrede (2002) "Expanded Trade and GDP Data". *Journal of Conflict Resolution*, 46: 712-24.

7 Singer, J.David, Stuart Bremer and John Stuckey. (1972). "Capability Distribution, Uncertainty, and Major Power War, 1820-1965." in Bruce Russett (ed) *Peace, War, and Numbers*, Beverly Hills: Sage, 19-48.

Table 4.6 Coding of Diffusion of Concession-Cost Scale for Economic Sanctions

Cost Type	Logic	Issues Included
Private Costs Issues	Costs to correct issue under dispute will focus primarily on a specific group, either the regime leadership, the military or business interests	destabilize regime improve human rights improve environmental policies trade practices implement economic reform
Mixed Costs Issues	Costs to correct issue will likely produce loss in public-goods such as security or prestige as well as private costs to the leadership, business interests, or the military	contain political influence contain military behavior solve territorial dispute deny strategic materials end weapons/materials proliferation terminate support for non-state actors.
Public Costs Issues	Costs to correct issue will produce costs in public-goods such as security, prestige, or international reputation	release citizens, property, or material (sovereignty issue) deter/punish drug trafficking practices retaliate for alignment choice

NOTE: Issue categories taken from the variable *issue1* in the TIES dataset.

SOURCE: Morgan, T. Clifton, Navin Bapat, and Valentin Krustev (2009) "The Threat and Imposition of Economic Sanctions, 1971-2000". *Conflict Management and Peace Science*, 26(1): 92-110.

Table 4.7 Variable for Chapter 6's Test of Public-Costs Concessions to Foreign Aid

Variable	Coding	Mean	Std. Dev.	Min	Max
Growth in Affinity[1]	Current year's affinity score plus one, minus the prior year's affinity score, divided by one plus the prior year's affinity score	.7117	.1961	-.7001	2.520
Dyadic Aid Dependence[2]	Aid/capita divided by GDP/capita as a proportion of total aid/capita divided by GDP/capita	.0196	.0961	-4.513	4.395
Opposition Strength, O^3	One minus the seat share of the largest party	80.03	23.72	0	100
Opposition Strength × Time, $f(t_i)O^{3,4}$	Opposition strength multiplied by the natural log of leader's cumulative days in office	602.9	220.9	0	972.5
Leader Experience, $f(t_i)^4$	Natural log of leader's cumulative days in office	7.422	1.222	0	9.740
Legislative Branch[5]	0 if system has no legislative branch, (6.1%) 1 if system has legislative branch, (93.9%)	.9388	.2397	0	1
Legislative Branch × Time, $f(t_i)O^{3,5}$	Legislative branch indicator multiplied by leader experience in office (see above)	6.9759	2.149	0	9.740

Table 4.7 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Baseline Affinity in Dyad[1]	Affinity score in first year for which data on the dyad is available	.4378	.3043	-.5890	1
Regime Similarity[6]	Target Polity2 score transformed to positive values minus donor state's Polity2 score transformed to positive values	-10.27	8.116	-20	19
Joint Democracy[7]	0 if donor and recipient do not share big winning coalition, (70.9%) 1 if donor and recipient share big winning coalition, (29.1%)	.2910	.4542	0	1
Lagged Exports to Donor to Target[8]	Natural log of dollar value of last year's exports to donor minus prior year's exports to donor, divided by prior year's exports to donor	.7284	.8820	0	5.161
Lagged Imports from Donor to Target[8]	Natural log of dollar value of last year's imports from donor minus prior year's imports from donor, divided by prior year's imports from donor	.7791	.9120	0	5.544
Total Net Aid Received[2]	Natural log of one plus total net aid/capita over GDP/capita	1.0641	1.574	-1.589	6.126
Non-zero Net Aid Allocation[2]	0 if net aid allocated from all sources is zero, (58.7%) 1 if net aid allocated from all sources is not zero, (41.3%)	.4126	.4923	0	1
Militarized Interstate Dispute in Dyad[9]	0 if no MID in dyad,(99.8%) 1 if MID in dyad, (.2%)	.0019	.0446	0	1

Table 4.7 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Target Hostility Level in MID[9]	O if none, (99.8%) 1 if MID but no militarized response, (0%) 2 if threat to use force, (.01%) 3 if display of force, (.09%) 4 if use of force, (.13%) 5 if war, (0%)	.0082	.1731	0	5
Donor Hostility Level in MID[9]	O if none, (99.8%) 1 if MID but no militarized response, (.15%) 2 if threat to use force, (.01%) 3 if display of force, (.04%) 4 if use of force, (.04%) 5 if war, (0%)	.0044	.1087	0	5
Post-Cold War	0 if year prior to 1989, (61.5%) 1 if 1989 or more recent, (38.5%)	.3849	.4865	0	1
Same Region[10]	0 if donor and recipient come from different geographical region, () 1 if donor and recipient come from same geographical region, ()	.0000	.0030	0	1
US-Middle East Dyad[10]	1 if donor is U.S. and recipient is in Middle East, (.7%) 0 otherwise, (99.3%)	.0071	.0840	0	1
US-Latin American Dyad[10]	1 if donor is U.S. and recipient is in Latin America, (.8%) 0 otherwise, (99.2%)	.0084	.0913	0	1

Table 4.7 Continued

Data Characteristics	
Unit of Observation	OECD donor-recipient-dyad years
Temporal Range	1960-1999, lower bound determined by aid data, upper by W data
N, democratic sample	34,469
N, nondemocratic sample	65,109

SOURCES:

1 Gartzke, Erik (2006) "Affinity of Nations Index, 1946-2002". Available at <http://dss.ucsd.edu/~egartzke/>.

2 Organization for Economic Cooperation and Development. (2009) "OECDstat Extracts". Available online at <oecd.stat.org>.

3 Vanhanen, Tatu. (2000) "A New Dataset for Measuring Democracy, 1810-1998". *Journal of Peace Research,* 37(2): 251-65.

4 Goemans, Hein, Kristian Skrede Gleditsch and Giacomo Chiozza. (2009) "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research,* 46(2), (March) 2009: 269-183.

5 Regan, Patrick M. Richard W. Frank, David H. Clark. (2009) "New Datasets on Political Institutions and Elections, 1972-2005". *Conflict Management and Peace Science*, 26(3): 296-304.

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7 Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson and James Morrow. (2004) *The Logic of Political Survival.*

Table 4.7 Continued

8 Gleditch, Kristian Skrede (2002) “Expanded Trade and GDP Data”. *Journal of Conflict Resolution,* 46: 712-24.

9 Ghosn, Faten, Glenn Palmer, and Stuart Bremer. (2004) "The MID3 Data Set, 1993–2001: Procedures, Coding Rules, and Description." *Conflict Management and Peace Science* 21:133-154.

10 Hensel, Paul R. and Paul F. Diehl (1994). "Testing Empirical Propositions about Shatterbelts." *Political Geography* 13, 1 (January): 33-51.

Table 4.8 Variable for Chapter 6's Test of Private-Costs Concessions to Foreign Aid

Variable	Coding	Mean	Std. Dev.	Min	Max
Economic Growth[1]	Current year's GDP/capita minus last year's GDP/capita, divided by last year's GDP/capita	.0139	.0671	-.5904	.7769
Net Aid Dependence[2]	Natural log of one plus net bilateral OECD aid dependence per capita over GDP/capita	2.207	1.594	-1.589	6.126
Opposition Strength, O[3]	One minus the seat share of the largest political party	80.49	23.70	30	100
Opposition Strength × Time, $f(t_i)O$[3,4]	Opposition strength multiplied by natural log of leader's cumulative days in office	591.16	226.9	0	972.5
Leader Experience, $f(t_i)$[4]	Natural log of leader's cumulative days in office	6.111	2.999	0	9.738
Legislative Branch[5]	0 if system has no legislative branch, (6.2%) 1 if system has a legislative branch, (94%)	.9379	.2413	0	1
Legislative Branch × Time, $f(t_i)O$[3,5]	Legislative branch indicator multiplied by time (see above)	6.793	2.237	0	9.738

Table 4.8 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Lagged Population Growth[6]	Last year's population minus prior year's population, divided by last year's population, then multiplied by 100	2.357	1.688	-44.40	17.74
Lagged Government Consumption Share[7]	Last year's ratio of government consumption, less spending on infrastructure, military and education, to overall GDP	68.46	23.75	5.059	306.4
Lagged Government Investment Share[7]	Last year's ratio of government investment to GDP	17.86	11.99	-3.830	80.91
Initial Gross Domestic Product/capita[1]	GDP/capita for first year of observation, usually 1960, in constant U.S. dollars	2473	3335	.0002	33910
Initial Growth Rate[1]	Economic growth for first year of observation, usually 1960	.0140	.0360	-.0763	.1704
Lagged Economic Growth[1]	Last year's GDP/capita minus prior year's GDP/capita, divided by prior year's GDP/capita	.0077	.0921	-2.219	.6549
Oil Producing State[8]	0 if state has no known oil reserves, (44.4%) 1 if state has known oil reserves, (55.6%)	.5562	.4969	0	1
Lagged Total Trade[1]	Natural log of dollar value of last year's total trade with all states	.4263	1.914	-7.435	6.261

Table 4.8 Continued

Variable	Coding	Mean	Std. Dev.	Min	Max
Population size[1]	Natural log of population size in thousands	8.879	1.549	5.327	14.04
Data Characteristics					
Unit of Observation	Aid-receiving leader year				
Temporal Range	1960-1999, lower bound determined by aid data, upper by W data				
N, democratic Sample	1,433				
N, nondemocratic sample	2,385				

SOURCES:

1 Gleditch, Kristian Skrede (2002) “Expanded Trade and GDP Data”. *Journal of Conflict Resolution*, 46: 712-24.

2 Organization for Economic Cooperation and Development. (2009) “OECDstat Extracts”. Available online at <oecd.stat.org>.

3 Vanhanen, Tatu. (2000) “A New Dataset for Measuring Democracy, 1810-1998”. *Journal of Peace Research,* 37(2): 251-65.

4 Goemans, Hein, Kristian Skrede Gleditsch and Giacomo Chiozza. (2009) "Introducing Archigos: A Data Set of Political Leaders," *Journal of Peace Research*, 46(2), (March) 2009: 269-183.

5 Regan, Patrick M. Richard W. Frank, David H. Clark. (2009) “New Datasets on Political Institutions and Elections, 1972-2005”. *Conflict Management and Peace Science*, 26(3): 296-304.

Table 4.8 Continued

6 World Bank, World Development Indicators Online.

7 Heston, Alan, Robert Summers and Bettina Aten. (2009) Penn World Table Version 6.3, Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania.

8 Energy Information Administration. (2009) “International Energy Statistics”. Available online at < http://www.eia.doe.gov/>. Accessed: April 2009.

CHAPTER 5

COMING INTO MONEY: THE EFFECTIVENESS OF FOREIGN AID

This chapter marks the beginning of a series of empirical tests aimed at a better understanding of the process of influence in international politics. As I argued in Chapter 3, accomplishing influence through foreign policy tools requires both that the reward or punishment be effective and that the demanded concessions prove politically feasible. Effectiveness, then, is the first and most direct step in the progression from foreign policy enactment to foreign policy success. When the preferred tool, like foreign aid, is a reward, effectiveness involves bolstering the targeted leaders' hold on domestic office. The ability of any positive sanction to accomplish this first step varies both with the institutions of representation across targets and within the career of each individual target. The reader may recall that in Equation 3.1.2 I expressed these ideas as I_p, the institutionally conditioned impact of aid, and $f(t_i)$ the function of leaders' experience in office.

Hypotheses 1-2 expressed my general expectations regarding the ability of sanctions to appropriately sway leaders' survival prospects, but specifics with respect to foreign aid were not addressed. Before diving into the empirical evaluation, I will elaborate on the intervening factors in the ability of foreign aid to successfully insulate targeted leaders. Selectorate theory, if updated to consider the intervention of dynamic political processes, can assist in this process. The static formulation of selectorate theory leads to predictions of greater effectiveness for nondemocratic aid recipients, as external resources should be more fungible for leaders with smaller winning coalitions. The tests below disconfirm this expectation. Democratic leaders actually stand to benefit more from aid than do nondemocratic leaders. This effect changes dramatically over time, in keeping with the general pattern of winning coalition institutionalization across regime types. Static measures of aid do not achieve statistical significance while over-time dynamics generate statistically and substantively significant results. The temporal trends

differ across regime type, suggesting that patterns in winning coalition institutionalization exert stronger influence than do learning effects.

Following the presentation of auxiliary arguments, which clarify expectations regarding I_p and $f(t_i)$, I conduct the empirical test described in Equation 4.3.1. The predicted relationship between I_p, time in office and leader failure is estimated using a censored probit regression. The first stage of the model predicts the allocation of aid money; the second stage, leader failure. This approach allows unbiased estimation of aid's impact on the probability of losing office despite the connection between elevated leader risk and aid receipt. A dataset of 4,692 leader years covering 791 separate leaders from 1960-1999 provides material for analysis. For descriptive information and coding decisions for the included variables, please see the tables following Chapter 4.

Coming Into Money

Inside the framework of influence, foreign aid increases in effectiveness as it contributes to the continued tenure of targeted leaders. Though it may arrive with many strings attached, voicing the public intentions of donor states, the political power of aid lies in its ability not to achieve these goals directly but to goad targeted leaders into fulfilling them. The means of doing so lies, as always, in the survival motive. Despite conditionality agreements, aid may be a powerful tool in this sense. Empirical research suggests aid often operates to the benefit of local elites rather than to that of their populations or donors (Pack and Pack 1993; Boone 1995; Feyzioglu et. al 1998; Kosack and Tobin 2005). Foreign aid becomes a fungible resource for elites on the receiving side, facilitating further pursuit of existing goals. As Kosack and Tobin (2005) note, "… it ends up largely substituting for government spending that would have occurred anyway, thereby freeing up government monies to be spent as the government wants" (210). Fungibility can be understood quite literally. The reporting requirements enforced by donors may not be so strict as to prevent some of the money from disappearing;

institutions in the developing world are infamously opaque, making the path of money very hard to trace. It is not necessary, however, that the conversion be so explicit. Even if monies are distributed to civil society organizations, the fungibility principle holds. An influx of extra money for infrastructure development or welfare programs frees up domestic funds which might previously have been allocated to these sectors. If an outside power is feeding the people, the government need not.

If fungible, aid could provide targeted elites with the opportunity to convert some portion of the windfall into increased time in office through reinforcement of their supporters' loyalty. Institutions of representation will condition their success in two ways. First, institutions of leader selection determine the value of raw funds for purchasing continued tenure. Autocratic leaders responsible to the military and business elite could expect a much bigger bang for their buck than democratic leaders responsible to a plurality of their electorate. Second, even if democratic leaders wanted to turn aid into private benefits for key political actors, the overlapping authority structures of democracy should make it considerably more difficult. Congressional oversight, independent judiciaries, freedom of the press and electoral accountability impose costs on elite behavior which falls outside the rule of law. Without such obstacles, leaders need consider only whether their behavior delivers significant benefits. On both counts –value and efficiency – democratic institutions mitigate the fungibility of aid, its most direct route to effectiveness.

The expectation that democratic institutions decrease the value of raw funds stems from one of the first assumptions made in this model: that all leaders rely on a core constituency the size of which is determined by institutional rules. Though democracies' institutional arrangements vary substantially, electoral mechanisms guarantee that elected officials are responsible to a considerable portion of the population (e.g. a plurality of all registered voters). Autocratic leaders, on the other hand, are rarely responsible to more than a handful of powerful military, economic or party elites. This variation in relative

size shapes the incentives of both leaders and constituents through three key mechanisms: the balance between public and private goods provision, the strength of the "loyalty norm", and the overall rate of government spending (Bueno de Mesquita et. al 2005, 82-96).

Leaders in control of spending attempt to optimize the winning coalition's benefit through a mix of private and public goods while maximizing the proportion of government revenues available for their own personal use (Bueno de Mesquita et al. 2005, 79-80). When the winning coalition is small, leaders' ambitions are best served by providing private goods to their cadre of supporters; when large, by providing public goods to the entire population (Bueno de Mesquita et. al 2005, 91). Under circumstances of a small winning coalition, each individual member of the coalition receives a higher payoff, and the gap between members and non-members increases. Coupled with a higher likelihood of being excluded from future winning coalitions should their leader be defeated, this discrepancy engenders a high level of loyalty (Bueno de Mesquita et al. 2005, 92-93). Public goods produce non-excludable benefits, meaning the perks of winning coalition membership decrease with the size of the group. The strength of the loyalty norm decreases in turn, producing a much higher baseline risk of losing office for democratic leaders (Bueno de Mesquita et al. 2005, 93). By affecting the loyalty norm, relative winning coalition size also determines the amount of overall spending required. When loyalty is high (winning coalition small), leaders may safely reserve more of the state budget for personal use "…if they find themselves at risk of being deposed" (Bueno de Mesquita et al. 2005, 93).

Given these dynamics, foreign aid will factor differently into leaders' attempts to extend tenure. Autocrats oversee small winning coalitions and seek, therefore, to provide private benefits. Under these circumstances aid presents a potentially valuable tool. Due to higher coalition loyalty, autocrats need to spend relatively less of their available resources in order to outbid possible challengers. Though the actual amount of resources

expended is less, the difference made is greater. Autocratic leaders extend their tenure through private benefits and stockpiles, both of which can be directly expanded by external revenue (Bueno de Mesquita and Smith 2007, 259). Aid provides autocratic leaders with a greater capacity to make pay-offs and speeds the accumulation of "slack" resources to set aside for a rainy day.

The democratic leader's obligation to a large proportion of the population drives down the instrumental value of an additional dollar in the budget. They must commit to spend nearly all government resources, yet the public goods provided disperse equally amongst the entire population, diminishing private perks for supporters and deadening loyalty (Bueno de Mesquita et al. 2005, 92-98). Any newly arriving resources must be turned towards the overarching goal of shoring up support in the interest of tenure, and this means providing further public benefits. In terms of providing increases in leader survival through instrumental improvements in winning coalition conditions, however, aid may be an inefficient tool for two reasons.

First, foreign aid boasts only a patchy record of public goods provision. Economic growth, as mentioned earlier, can only be triggered by aid in already thriving economies (Kosack and Tobin 2005). Democratic leaders who are providing high levels of public goods, and thus enjoy a relatively low risk of losing office, may manage to coax some additional public benefit out of aid flows. Leaders presiding over weak economies, and thus facing higher risks, cannot make external funds work to their advantage. Given that a weak economy is consistently a key predictor of aid allocation (e.g. Lai 2003), aid is likely to have little or no discernable impact on existing levels of public goods.

Second, even if aid does produce some public goods improvement, democratic leaders may find it difficult to claim credit. Many public benefits attributable to aid monies, such as infrastructure or education improvements, trickle down through government agencies or are provided directly by foreign nongovernmental organizations. The line of attribution in these cases does not point unambiguously back to the head of

government, but branches out to external powers, bureaucrats and other government figures.

Since aid has little direct, instrumental value within the accepted frame of democratic behavior, recipient leaders may prefer to take a card from the authoritarian playbook. Consider, for example, the behavior of President Chiluba (1991-2001) in Zambia. Ex-post investigations by Transparency International have revealed that the President maintained a "… slush fund … to appropriate public funds and 'dole' them out to favoured or politically useful persons or groups without accounting for them," and that "Parliament allowed criminal funds to be operated throughout President Chiluba's tenure of office" (Yambayamba 2007, 5). The temptation and the actuality of such corruption exist in democracies. At the beginning of Chiluba's tenure, Zambia fell within the realm of democracy by the most commonly utilized measure in international relations scholarship, scoring a 6 on the Polity2 composite scale (Marshall and Jaggers 2003). Over the course of his term, however, the President's corruption assisted in the deterioration of governance in Zambia to a mixed score. Democratic leaders operating in better institutionalized systems, however, face significant institutional barriers which make corruption an inefficient strategy.

In consolidated democracies the oversight rights of other branches, freedom of the press and the rule of law constrain the executive (e.g. Linz and Stepan 1996, 7-14). When functioning properly and in conjunction, these characteristics should make it very difficult for leaders to divert aid monies and very politically costly to be caught in the attempt. In democracies, executives do not enjoy exclusive knowledge of the budget. Opposition parties in the legislature maintain the right to review and approve the allocation of funds and to track performance; the free press holds the right to request and publish internal documents regarding public finance. The transparency of oversight and the risk of exposure by the press would require elites to devote a considerable portion of diverted public resources to covering the trail by which they were diverted. A

functioning rule of law – manifest in an independent judiciary and the widespread norm of legality – ensures that politicians caught with their hands in the state's piggy bank face serious consequences. In short, savvy democratic elites know that their institutional environment encourages continued democratic behavior (Gates, Hegre, Jones and Strand 2006); diverting foreign aid directly to personal aggrandizement is simply not the most efficient way for them to utilize their resources.

Lacking consistently restrained executives, informed and involved publics, or effective oversight, autocratic elites remain relatively free to expropriate development assistance. The line between state funds and private funds is blurry at best. Little domestic cost exists to prevent the efficient conversion of aid to private goods and benefits. A leader like the Congo's Mobutu Sese Seku simply delivers envelopes full of money to key elites in order to purchase their loyalty (Wrong 2007, 22).

As stated in Hypothesis 2, sanctions which tap into the survival motive will be more effective. Aid cannot accomplish this equally in all target states. The institutionally conditioned impact of aid for democratic and nondemocratic leaders will differ. Democracy will correspond to lower values of I_p; autocracy to higher values of I_p. In sum, both the value of aid dollars in instrumental terms and the ease of utilizing them instrumentally decline under democratic institutions. If aid dollars cannot deliver a noticeable benefit to the members of the winning coalition, it cannot produce the desired insulation of targeted leaders and thus will be ineffective.

Having covered the role of institutional conditioning of aid effectiveness, we can now turn attention to temporal dynamics. Learning and the institutionalization of winning coalitions produce differential expectations. If experience (learning) matters most, the role of time should be simple: for all types of leaders, more experience should translate to more effective aid. Ability increases over time as on-the-job experience makes the problem of utilizing aid to advantage less daunting. Trial and error demonstrates which means of persuasion and framing perform best, which relationships

can be counted on, where oversight is most stringent and where it is lax. Leaders come into power with varying levels of expertise, but all will learn something from the process of governing and will take action to improve (Light 1999, 37). As experience accumulates, all leaders should learn how best to "work" external assistance to the advantage of their supporters (and thus themselves).

If institutionalization of political support contributes the greater impact, however, time will factor in differently for democratic and non-democratic leaders. For nondemocratic leaders, the early days in office feature the lowest levels of loyalty within the winning coalition. The award of additional resources at this point in time may seem ideal, but the externally-derived aid resources may exacerbate the number and intensity of initial challenges. Aid money may appear to potential challengers as similar to other non-tax revenue, such as natural resources. Often referred to as "lootable", this type of resource makes political power more valuable and encourages more frequent and serious challenges (e.g. Le Billon 2001; Smith 2008). While the winning coalition remains poorly institutionalized, challengers can poach members through promises to more favorably utilize the aid dollars. Thus aid may actually be less than helpful for new autocratic leaders. Those who manage to weather the uncertain days, however, will find aid a tool which meshes with normal tenure-extension activities. Aid will then, become increasingly beneficial for nondemocratic leaders over time.

Democratic leaders, of course, face a different dynamic. Their winning coalition stands behind them most firmly immediately following their rise to power. Elections invest new leaders with a legitimacy which tends to squelch criticism. Under the influence of this "honeymoon" effect, the democratic winning coalition would be disposed to credit the leader with having obtained foreign aid or with earning the good will of the international community. As the natural processes of political competition gear up, however, this charitable perception gives way to demands for results. Leaders who cannot make aid "work" for the people – likely to be a large proportion of aid-

receiving leaders – will find themselves criticized for it by an opposition which believes itself more competent. If conditionality agreements were made and not fulfilled, the competition has a tangible failure on which to focus their political attacks. Development assistance as a resource is not unique to the leader in power at the time of receipt. The extreme rarity of donor nations reducing aid amounts following democratic turnover bolsters domestic competition's ability to credibly promise to perform at least as well as the incumbent.[46] The effect of foreign aid on democratic leaders, then, may be expected to change over time: initially it will be beneficial, but in the long run aid allocations may hurt.

Finally, we must consider aid allocation before proceeding to analyze its effectiveness. As discussed in the literature review (Chapter 2), donors entertain multiple goals when deciding where to send aid dollars. Strategic, humanitarian and commercial interests mingle in the decision matrix. Variables to account for these possible motives will be included in the selection model. The element of most theoretical interest to the current project, however, is the targeted leader's risk of losing office. Sending states should perceive the most willingness to comply with policy change from those leaders who feel themselves at greater risk today than yesterday. Gordon (1992, 48-9), a high-ranking official in USAID, mused that complete economic breakdown coupled with threat of political regime change may be necessary to satisfaction of aid conditionality agreements in Africa. Regarding selection, then, the principle hypothesis for foreign aid is consistent with Hypothesis 10 from Chapter 3.

This additional discussion allows reframing of the general hypotheses from Chapter 3 in terms of foreign aid:

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[46] Only one democratically elected leader in my dataset immediately received less aid than his predecessor: Cardoso of Brazil (1995-2000) received 10 million US dollars less than Franco (1992-1994).

H1a: The effectiveness of foreign aid will increase with targeted leaders' experience.

H1b: The effectiveness of foreign aid will increase over time for autocratic leaders; and decrease over time for democratic leaders.

H2a: Foreign aid will impact the winning coalition of more autocratic leaders, increasing its effectiveness.

H2b: Foreign aid will have limited impact the winning coalition of more democratic leaders, decreasing its effectiveness.

H10: Aid allocation is more likely for leaders facing elevated risk of losing office.

With these more detailed expectations, we can proceed to tests of Equation 4.3.1. As the modeling decisions and specification have been discussed at length in Chapter 4, I will not review them here.[47] The empirical evaluation proceeds in two parts. First, I will discuss the tenability of the restated hypotheses based on the results of the censored probit regression of aid allocation and effectiveness. The analysis provides impressively clear support for the expectations that aid's effectiveness varies across institutions and time. The institutionalization hypotheses receive confirmation from the opposing pattern of change over time. Interestingly, autocratic leaders do not appear to benefit more than democratic leaders, as the destabilizing effect of external resources requires a considerable amount of time to reverse.

Second, I utilize the parameter estimates to produce $\hat{E}_{it}$, the measure of aid's expected effectiveness for each leader. This process begins with the simple bootstrapping process described in Chapter 4 which will provide a means of increasing the reliability of estimates while accurately representing uncertainty. From the 500

[47] Further information regarding estimation procedures can be found in Appendix 1.

bootstrap samples, ten estimates of $\widehat{\boldsymbol{\beta}}$ were randomly selected. I calculated the combined coefficient measures of $\hat{E}_i$ in each dataset (see Equations 4.4, 4.5.1, and 4.5.2). These calculations will be utilized in Chapter 6, where Hypotheses 4-9 will be evaluated.

Empirical Results

Though my hypotheses posit a connection between the process of aid allocation and leader failure, making the censored probit an appropriate strategy, we should evaluate the tenability of my expectations. Table 5.1 contains an independently run probit regression for survival using the modified aid variables which do not drop non-recipients. The parameters from this model allow production of the endogenous instrument in the allocation equation. The model achieves a desirable level of fit. Even using the more conservative expected percent correctly predicted (ePCP) (Herron 1995), this auxiliary regression correctly identifies 84.5% of cases. The instrument produced from this model, then, should be informative. Figure 5.1 charts the relationship between the instrument and the more intuitive concept of failure probability. As the probability of failure increases, so does the value of the instrument. Because the instrument is linear and the probability of failure is not, however, the relationship between them cannot be represented by a single slope coefficient. Table 5.2 allows the reader to peruse an independent probit model for aid allocation, to demonstrate its high fit. By ePCP, this measure correctly scores just less than 97% of cases. Models performing as well as these should provide a firm base for the censored probit.

Table 5.3 contains the results of the full model, a censored probit with endogenous regressor in the selection stage. To provide more robust results, a bootstrap process drew 500 samples of size N with replacement, clustering on leaders, and estimated equation 4.3.1 on each sample. I report the mean of these 500 estimated coefficients. The standard errors of each coefficient were also bootstrapped; the reported errors, then, are the mean estimates of standard error rather than the square root of the

variance in $\hat{\beta}$ across the 500 samples. The statistically significant χ^2 test indicates a significant relationship between the residuals of the allocation and failure equations. It is appropriate, then, to model the processes simultaneously rather than as separate events. With a reasonably well fitting model established, we may move on to discuss the substantive results and their implications for the hypotheses. This will begin with discussion of the allocation process.

The coefficients for the selection equation are located in the lower half of Table 5.3. First of all, the explanatory power of the lagged dependent variable must be noted. The level of inertia in the bureaucratic process of aid allocation appears to be quite high. Leaders who received aid in the previous time period have a 99.9% chance of receiving aid again. This is an increase in probability of .867 from lagged aid at zero, which is significant at 95% confidence. No other covariate's effect comes close to this one. Despite the power of the inertial relationship, however, several other important effects can be uncovered.

Figure 5.2 displays some effects which manage to achieve significance despite the influence of inertia. Panel A graphs the effect of the variable most central to the theoretical inquiry at hand. Though calculations of predicted probability were made using the instrument, I have displayed the estimates against their corresponding probabilities of failure for a more intuitive interpretation. This was accomplished by multiplying the value of the instrument by the standard error of the prediction and evaluating the normal distribution at the product value. Hypothesis 10 predicts that donor states will strategically target leaders they believe to be more pliable, namely those facing higher domestic risks of losing office. The effect displayed in Panel A supports this expectation. Leaders facing little risk stand almost no chance of receiving aid. Holding all else constant, that probability increases to about .138 after a modest increase in risks to its mean value. A further increase of one standard deviation moves the probability to .242, a 43% increase from the prediction at the mean. As leaders' likelihood of failure

increases to extreme values, the probability of aid allocation increases dramatically. Only about 10% of leaders face risks in excess of .48, but at this point probability of receiving aid reaches nearly .80. Sending states do, in short, appear to factor the target's political need for external assistance into their allocation calculus. Hypothesis 10, then, finds considerable support. This highly significant result affirms the need of the endogenous variable and censored probit to account for selection: a variable undoubtedly related to the dependent variable in our outcome stage exerts a significant impact on the selection mechanism.

Panel B presents additional evidence for the strategic behavior of donors: the impact of winning coalition size on allocation. As found by Bueno de Mesquita and Smith (2007, 2009), donors appear to steer aid towards leaders with smaller winning coalitions. The overall effect here, though it appears shallow in graphical form, is quite significant. An increase from minimum winning coalition size to a mean value of .5 produces an 80% drop in the probability of aid allocation, from .2445 to .1357.

These indicators of aid's strategic implementation, however, do not preclude humanitarian motives. Panels C and D indicate that humanitarian interests do drive aid allocation to some extent. The coefficients for donor imports and population growth demonstrate that needier populations enjoy higher likelihood of assistance. While some may assume donor imports would take on a positive sign, as senders allocate money to trade partners, it appears instead to represent the general function of the economy. Leaders of countries which import nothing from the OECD are 87% more likely to receive aid than are those which import at the mean level.[48] Countries with higher rates of population growth tend also to be poor. And as population growth increases from its

[48] This figure obtained by the following calculation: [P(aid|donor imports=min) – P(aid| donor imports =mean)]/P(aid| donor imports =mean) = (.2437 - .1303)/.1303=.8703. Each probability utilized has been determined to be significant at the 95% confidence level, as has the difference between them.

minimum to its maximum, the probability of receiving aid jumps from around 0 to nearly .5.

Before moving on to the outcome equation, I will briefly summarize the interesting results regarding other measures of strategic goals – oil production capacity, former colonial status, alliance with OECD members, and internationalized civil conflict. Except for oil, these variables achieve significance in the expected direction. The negative coefficient of oil reserves on allocation may be explained by wealth. For oil holding states, mean GDP/capita reaches 59.70 US dollars; the remaining states earn a mean GDP/capita of only 28.72 US dollars. Though significant, the remaining strategic reasons for investing exert an effect paltry in comparison to those mentioned earlier. Although former colonial status, has been found to important in several previous studies of allocation (e.g. Berthelemy and Tichit 2004), switching the indicator on in this model results in an increase of only .002. This very small effect may be due to variation in donor behavior which Stone (2006) has hypothesized. Internationalized civil conflict and alliance to OECD donors produce analogously small increases in allocation prospects.

With these loose ends accounted for, discussion moves on to the failure equation, the results of which are presented in the top rows of Table 5.3. While only limited information can be derived from the raw probit coefficients, the initial inspection supports H1b. Opposite signs for the time trend of aid to democratic and nondemocratic leaders suggests that the loyalty dynamic, rather than learning, drives the overtime changes in aid's impact on survival. The coefficient for aid's dynamic impact on nondemocratic leaders is negative, indicating that assistance decreases risks of failure over time; for democratic leaders, aid appears to increase risks over time. This pattern is consistent with both the hypothesis that democratic winning coalitions will interpret aid less favorably over time. The results in Table 5.3 bode less well for H2a and H2b. The constitutive term for aid which, given the two level interaction, represents the impact of aid to small winning coalition system leaders on their first day in office, achieves

significance with a positive sign. Aid, according to this coefficient, increases nondemocratic leaders' risks. The negative sign attached to aid to democratic leaders, similarly refutes expectations based on the static institutional argument.

The direction of effects alone, however, is not enough evidence to dismiss H2a and H2b. The time trend for aid to nondemocratic leaders indicates a shift in the initial (destabilizing) effect of aid on leaders' latent propensity to fail. Drawing conclusions regarding the hypotheses, then, requires evaluation of the full effect of aid on types of leaders across time. This involves transformation of the coefficients into either marginal effects or differences in predicted probabilities (e.g. Brambor, Clark and Golder 2006; Kam and Franzese 2007). The graphics in Figure 5.3 display the result of a Monte Carlo sampling from the distribution of Table 5.3's parameters. With a draw of 2,000 $\hat{\beta}$s, $\hat{\rho}$s, and $\widehat{\sigma^2}$s for 100 values of time in office, the probability of leader failure was calculated at the mean and then at the minimum value of aid. The difference between these two predicted probabilities indicates the magnitude of insulation or destabilization leaders experience as a result of aid. When the difference is positive, leaders receiving greater magnitudes of aid face higher risks of losing office than do those at the minimum level. A negative difference tells us leaders face lower risks at the high value of aid than at the minimum level. To give additional information, I report the percentage change in predicted probabilities rather than the simple change. This provides greater sense of how important the shift is. The large sample allowed for simple construction of 95% confidence intervals around the mean difference in predicted probabilities.

Panel A of Figure 5.3 illustrates the percentage change in probability of failure for democratic leaders given a standard deviation drop in aid from the mean value. Ninety-nine percent of democratic leaders fail prior to the 18th year in power, so the x-axis ends at that point. This visual representation provides considerable additional evidence in favor of H1b. For democratic leaders, whose winning coalition is most charitable early in the term, higher levels of foreign aid produce lower risks of losing office only in the early

years. The insulation effect is substantial, with risks in the first days of office reduced by 60%. The decay in benefits proceeds steeply, however. Within two years and nine months, the confidence intervals include zero. Another six months brings a significant risk-increasing effect, which tops out at a 26% increase for the longest-enduring democratic leaders. The dotted, gray, vertical line in the figure represents the 50th percentile in big winning coalition leader tenure. It almost perfectly matches the point at which aid's effect becomes destabilizing.

Panel B of Figure 5.3 also provides support for H1b, demonstrating a significant destabilizing effect which diminishes over time. Receiving aid during the first days of tenure increases a nondemocratic leader's likelihood of coalition failure by 27%. This dangerous impact reverses, but at a leisurely pace. Leaders must cling to power for 17 ½ years before the destabilizing effect loses significance. At this point 83% of autocratic leaders would already have lost office. The insulating effect reaches 11.7% at its highest point, corresponding to the very rare occasion of 46 years in office.

While this analysis provides significant support for the dynamic hypotheses four and five, it contradicts the basic winning coalition arguments voiced in H2a and H2b. Democratic leaders stand to benefit much more from aid than do their small winning coalition counterparts, at least for a couple of years. The insulation effect in the first two years of a big winning coalition system outstrips that available to nondemocratic leaders both in magnitude – 60% compared to 11.7% – and in the proportion of leaders likely to experience it – 50% versus 17%.

The support for H1b over the static institutional arguments suggests that institutional differences exert a greater impact through the conditioning of political processes than through their concrete existence. The results also demonstrate that a large portion of allocated aid actually destabilizes recipient elites. This means much of aid could be considered *ineffective*, according to the discussion in Chapter 3. This finding

places additional interest on the tests of concessionary behavior, to be performed in Chapter 6.

Summary of Results

This analysis has provided extremely strong support for my model of aid effectiveness. Aid can significantly shore up the career of new democratic leaders, insulating them from the threat of winning coalition failure by up to 60%. But, if arriving late, aid can also cause problems for those responsible to a large coalition of supporters. Ironically, foreign assistance causes serious problems for the small winning coalition leaders who accept it. A destabilization of 30% may follow from receipt of a generous amount of aid. The role of $f(t_i)$ and I_p, are thus essential to understanding the relationship between aid receipt and leader survival.

The findings reported here contribute a new dimension to our understanding of foreign policy, and of foreign aid in particular. Extant studies have considered a static picture of politics within target states (e.g. Kono and Montinola 2009; Bueno de Mesquita and Smith 2007). But the abilities and resources of politicians change over time in systematic ways about which we can theorize. More generally, the realm of friendly foreign policy effects deserves more scholarly attention, not least of all because it constitutes a significant public expenditure for many western nations. Studies of alternative strategies, such as military assistance and diplomatic support, should be conducted to determine whether a similar pattern holds.

More immediately, however, this analysis raises intriguing questions about the policy outcomes of aid allocation. When leaders receive aid that helps them hold onto power, does this translate to higher compliance with donor requests? When it hurts, do targets become more recalcitrant? Why take money which spawns dangerous levels of instability and competition? And, while on the surface it seems positive that aid tends to make democratic leaders more secure, this need not be the case. The insulation of status

quo administrations may also explain findings that donor goals of democratization (Knack 2004) and human rights improvements fail to be achieved (Regan 1995). Alternatively, the swift decay of the helpful effect into a political liability may be undergirded by increases in corruption and scandal as democrats utilize fungible aid monies to enrich themselves and their closest supporters. A parallel argument may be made for the seeming positive of aid's destabilizing effect on more autocratic leaders. The fall of such leaders does not necessarily imply the instatement of democracy. If these leaders are replaced by more of the same, aid may prove different than the traditional "resource curse" only in that it could have been prevented.

Making more informed allocation decisions requires not only an awareness of the effectiveness of foreign aid, but also an understanding of this second step. How does the impact of aid on leader survival relate to its ultimate success as a tool of influence? To pursue this aspect of the problem, discussion will now move on to consider the construction of measures of effectiveness. These measures will allow me to model the process of concession as a function both of aid's direct impact and the equally important role of domestic political difficulty.

Constructing Measures of Aid Effectiveness

The question pursued above – how foreign aid impacts the survival of recipient leaders – is interesting in its own right. For the purposes of the overall project, however, it represents only the first necessary step in a process of assessing the success of external influence attempts. The parameters estimated in the censored probit regression of aid allocation and leader failure will be used to construct measures of the effectiveness of foreign aid for different types of leaders.

These measures, which I will sometimes refer to as $\hat{E}_i$, utilize a popular means of interpreting multiplicative terms, the marginal impact. The marginal impact of any covariate, x, is the derivative of the likelihood function with respect to the x. Intuitively,

the first derivative tells us the change in the dependent variable resultant from an instantaneous change in x. To avoid the functional form problem, and to better isolate the impact of factors pertinent to effectiveness, I devised the combined coefficient measure as the derivative of the linear index of the outcome equation (see Chapter 4 for more detailed discussion).

The first derivative of the linear index of leader failure with respect to foreign aid dependence, which I will refer to from now on as the combined coefficient measure of effectiveness, is simply:

$$\partial \boldsymbol{X_{it}\beta_1}/\partial A_{it} = \beta_{11} + \beta_{12}\mathrm{W} + \beta_{13}\mathrm{f(t_i)} + \beta_{14}\mathrm{f(t_i)W} \qquad \text{(Eq. 5.1)}$$

Where A_{it} is the aid dependence of leader *i* at time *t*;β_{11} is the slope coefficient for aid dependence; β_{12} is the slope coefficient for the interaction between aid dependence and big winning coalitions; W is an indicator for big winning coalition systems; β_{13} is the slope coefficient for the interaction between aid dependence and the natural log of *i*'s experience in office; $f(t_i)$ is the natural log of *i*'s experience in office; and β_{14} is the slope coefficient for the interaction between aid dependence, big winning coalitions and the natural log of *i*'s experience in office. For nondemocratic leaders, W equals zero, thus the second and fourth term will drop out of the equation.

The combined coefficient measure indicates the magnitude and direction of aid's effect on leader's latent probability of failure across regime type and over time. Positive values indicate increases in the likelihood of losing office; negative, decreases. For positive sanctions, effectiveness relates inversely to the trend in risk of losing office: negative values indicate effectiveness; positive, ineffectiveness.

The evaluation of foreign aid outcomes will utilize two different sets of data. The first dataset allows tests of strategic, public-goods concessions through analysis of UNGA voting patterns. Each observation in this data constitutes an OECD donor-recipient dyad. The second dataset focuses on private-costs concessions in terms of

economic growth. This data contains aid-recipient leader years. I constructed the combined coefficient measure of effectiveness in each of these datasets.

Figure 5.4 charts the range of values of $\hat{E}_i$ in the dyadic dataset. The black line gives the value of the measure across time for democratic leaders, while the blue line charts its value for nondemocratic leaders. The dashed curves of corresponding colors provide the density of survival for each type of leader across the range of time.

As should be expected given the differences in probability which were examined in Figure 5.3, the combined coefficient for democratic leaders slopes positively over time. Aid initially insulates these leaders, making it an effective reward. At the deepest values of effectiveness, the combined coefficient is -0.271. Over time the destabilization of winning coalitions changes the impact of external resource flows, leading eventually to a combined coefficient of .107. Also consistent with the measure of substantive significance in Figure 5.4, the combined coefficient for democrats crosses the zero line very near to the 50% mark in the distribution of democratic survival times.

For nondemocratic leaders, in contrast, the combined coefficient slopes negatively, indicating increasing effectiveness over time. The combined coefficient begins at very high levels of ineffectiveness, with maximum value of .235. For a very small percentage of nondemocratic leaders surviving beyond the 18^{th} year in power, the combined coefficient indicates modest insulating potential with a low value of -0.022.

Across types of leaders, the combined coefficients' most common values are small in absolute value. Figure 5.5 provides the distribution of values of the combined coefficient in the dyadic sample across type of leader. For both types, the density curves peak just to the right of zero. For democratic leaders, however, the distribution is more symmetrical. The vast majority of nondemocratic leaders were assigned ineffective values of the combined coefficient measure.

Figures 5.6 and 5.7 provide the parallel examinations of the combined coefficient in the second dataset. The range, trend and distribution of the effectiveness measure in

the monadic sample exactly replicate that already discussed. The measure then, should represent the same dynamic regardless of the unit of analysis in the tests which will be carried out in the next chapter.

The direct estimation of aid's impact on leader failure tells us that aid functions effectively for about 50% of its democratic recipients but only 17% of its nondemocratic "beneficiaries". As attention turns to modeling economic growth and UNGA voting affinity as proxies for concessionary behavior, we will be able to assess the remaining principle claims regarding the ability of positive sanctions to operate as tools of influence. The argument that effectiveness produces increased will to cooperate with sending states must be tested against the difficulty of domestic political hurdles to concession.

Table 5.1 Probit Regression of Winning Coalition Failure, Including Non-Aid Recipients

Logged Aid/GDP to Small Winning Coalition Systems	0.2895*** (0.0837)
Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.0284** (0.0113)
Logged Aid/GDP to Big Winning Coalition Systems	-0.6358*** (0.1226)
Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.0740*** (0.0176)
Big Winning Coalition	0.5697*** (0.1095)
Ln(time)	-0.1514*** (0.0309)
Lagged Economic Growth	-0.6774*** (0.2484)
Lagged Log of Total Trade	-0.0126 (0.0186)
Intensity of Civil Conflict	0.1475*** (0.0508)
SOUTHAM	0.2267*** (0.0698)
SUBAFRICA	-0.1794** (0.0777)
SOUTHASIA	0.1759** (0.0892)
Constant	-0.6488*** (0.2314)
Pseudo R^2	.0822
-2Pseudologlikelihood	-1330.84
Wald $\chi 2$	246.89
Percent Correctly Classified	90.85%

NOTE: N=4,752 leader years from 1960-1999, including 791 separate leaders. Robust standard errors, in parentheses, are clustered on leaders. In this specification one unit was added to aid/GDP before taking the natural log to avoid dropping zeros.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5.2 Probit Regression of Aid Allocation

Probability of Leader's Winning Coalition Failing (Instrument)	0.0631*** (0.0208)
Lagged Aid Receipt	4.5245*** (0.3559)
Lagged Population Growth	0.0627*** (0.0216)
Lagged Imports from OECD Donor States	-0.5806*** (0.1413)
Natural log of Population	0.0702 (0.0769)
Lagged Economic Growth	-0.4455 (0.7901)
Winning Coalition Size	-0.7961* (0.4556)
Former Colony	0.4915** (0.2162)
Defensive or Offensive alliance with an OECD donor state	0.4602** (0.2020)
Potential for Oil Production	-0.6985** (0.3299)
Internationalized Civil Conflict	0.9786** (0.4846)
Constant	0.2036 (0.6221)
Pseudo R2	.8413
-2 Pseudologlikelihood	-267.945
Wald $\chi 2$	339.30
Percent Correctly Classified	98.21%

NOTE: N=4,752 leader years from 1960-1999 including 791 individual leaders. Robust standard errors, in parentheses, are clustered on leaders.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5.3 Bootstrap Estimates of Censored Probit of Aid Allocation and Leader Failure

		Coef.	Std.Err.	95% Confidence Interval	
Winning Coalition Failure	Logged Aid/GDP to Small Winning Coalition Systems	0.2348	0.0095	0.2162	0.2534
	Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.0264	0.0013	-0.0289	-0.0239
	Logged Aid/GDP to Big Winning Coalition Systems	-0.5356	0.0116	-0.5582	-0.5129
	Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.0695	0.0020	0.0657	0.0734
	Big Winning Coalition	0.3353	0.0088	0.3182	0.3525
	Ln(time)	-0.1611	0.0034	-0.1679	-0.1544
	Lagged Economic Growth	-0.7271	0.0757	-0.8755	-0.5788
	Lagged Log of Total Trade	0.0037	0.0015	0.0009	0.0066
	Intensity of Civil Conflict	0.1262	0.0042	0.1179	0.1344
	Constant	-0.4428	0.0266	-0.4950	-0.3906
Aid Allocation	Probability of Leader's Winning Coalition Failing (Instrument)	0.0834	0.0028	0.0780	0.0888
	Lagged Aid Receipt	5.0486	0.2570	4.5448	5.5524
	Lagged Population Growth	0.0696	0.0281	0.0146	0.1246
	Logged Population	0.1185	0.0142	0.0906	0.1464
	Lagged Imports from OECD Donor States	-0.6947	0.0642	-0.8205	-0.5689
	Winning Coalition Size	-1.0016	0.1153	-1.2275	-0.7757
	Former Colony	0.6075	0.0366	0.5358	0.6792

Table 5.3 Continued

Aid Allocation	Defensive or Offensive alliance with an OECD donor state	0.6836	0.0402	0.6047	0.7624
	Potential for Oil Production	-0.0164	0.3745	-0.7504	0.7176
	Internationalized Civil Conflict	-0.8062	0.0497	-0.9036	-0.7087
	Constant	0.1225	0.1366	-0.1452	0.3902
	ρ	-.4811	.1434	-.7114	-.1574
	Wald χ2 Test of Independent Equations	7.90*, p=.0049			

NOTE: Estimates of coefficients and standard errors based on 500 bootstrapped samples from the population of N=4,752 leader years from 1960-1999, including 791 separate leaders. Resampling was clustered on individual leaders.

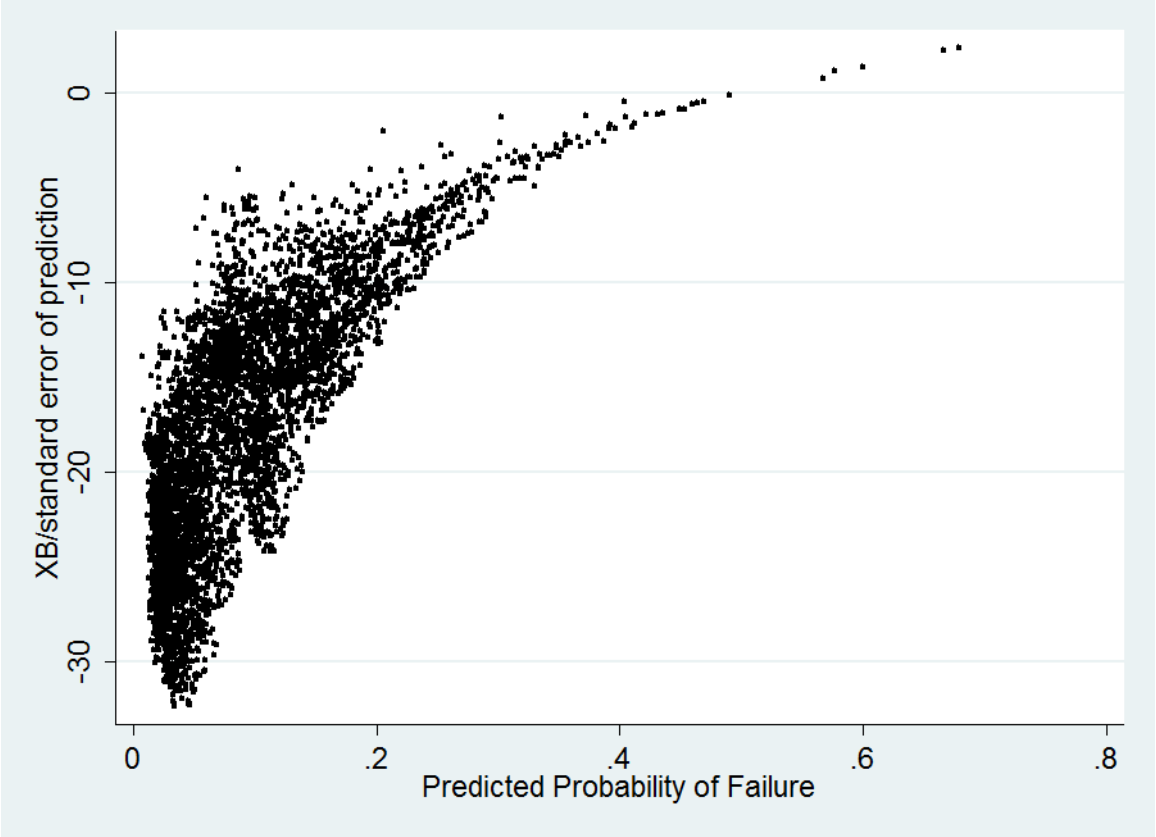

Figure 5.1 Relationship between Instrument and Predicted Probability of Failure

NOTE: Instrument and predicted probabilities of failure generated using estimates in Table 5.1.

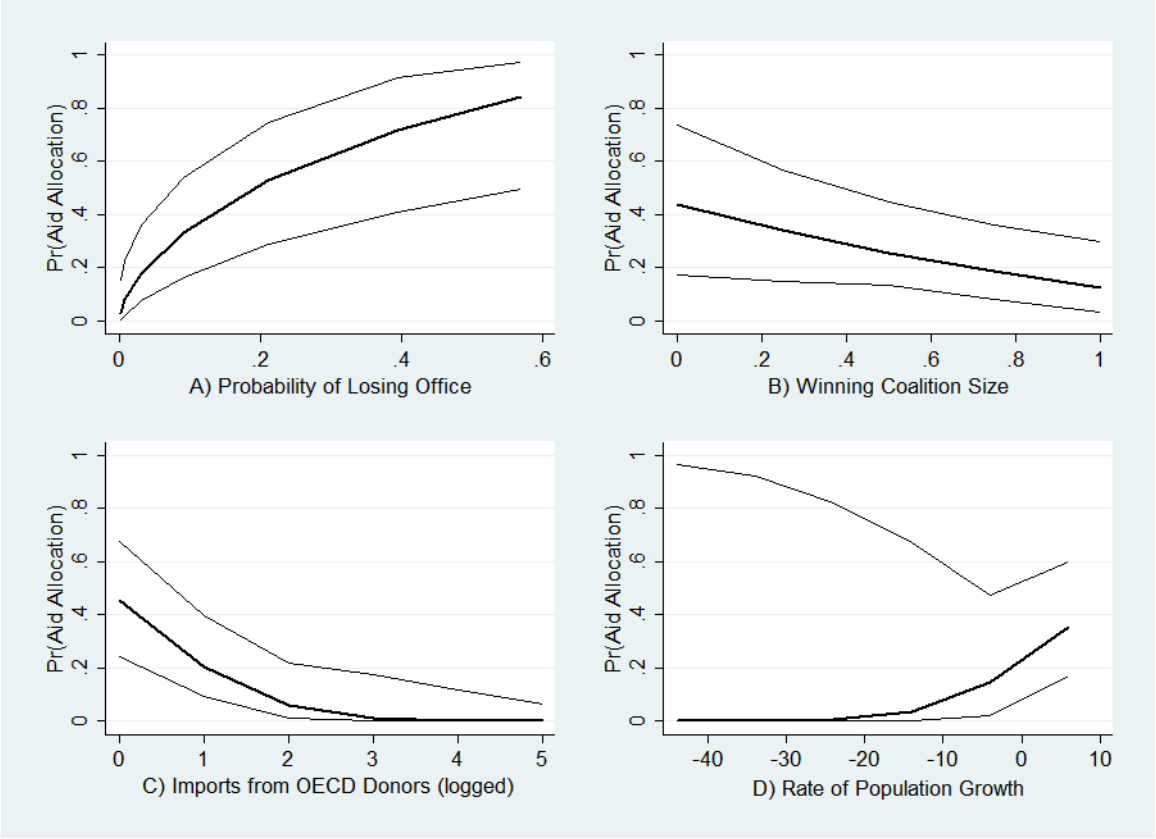

Figure 5.2 Predicted Probability of Aid Allocation Across Values of Covariates

NOTE: Reported statistic is the mean prediction from 1,000 draws from the variance-covariance matrix of the bootstrapped censored probit reported in Table 5.3. Thin lines give the 95% confidence interval based on that simulated distribution. All other variables held at means or modes as appropriate during the calculation. Calculations were made at seven values of each covariate ranging from the minimum to the maximum.

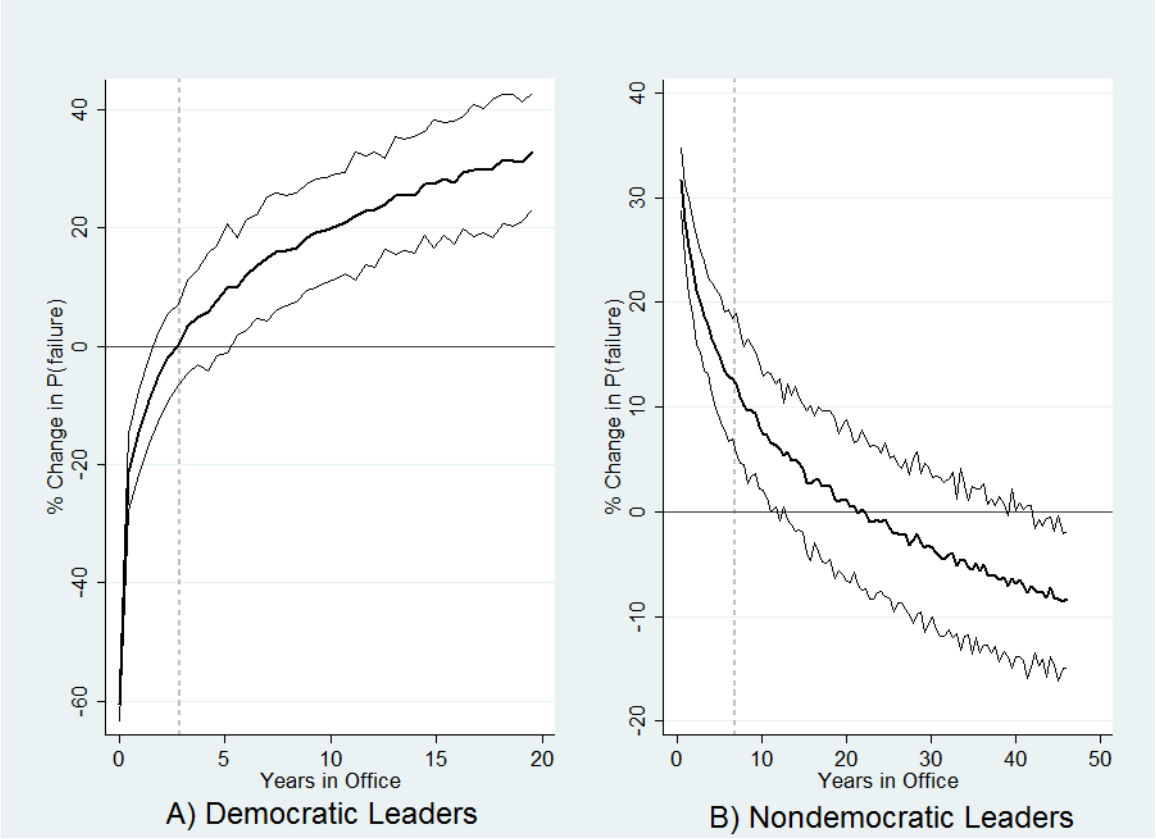

Figure 5.3 Difference in Probability of Leader Failure by Foreign Aid Dependence

NOTE: Reported statistic is mean difference in probability of failure between one-standard deviation above average aid dependence and mean aid dependence based on simulated distribution of 1,000 draws from the variance-covariance matrix of the censored probit reported in Table 5.3. Thin lines mark 95% confidence intervals based on that distribution. Gray horizontal line highlights the zero line. Dashed vertical line marks the 50% mark in the distribution of failure times for each type of leader. Democratic leaders are all those with W≥.75; nondemocratic all those with W<.75. Y-scales differ across panels.

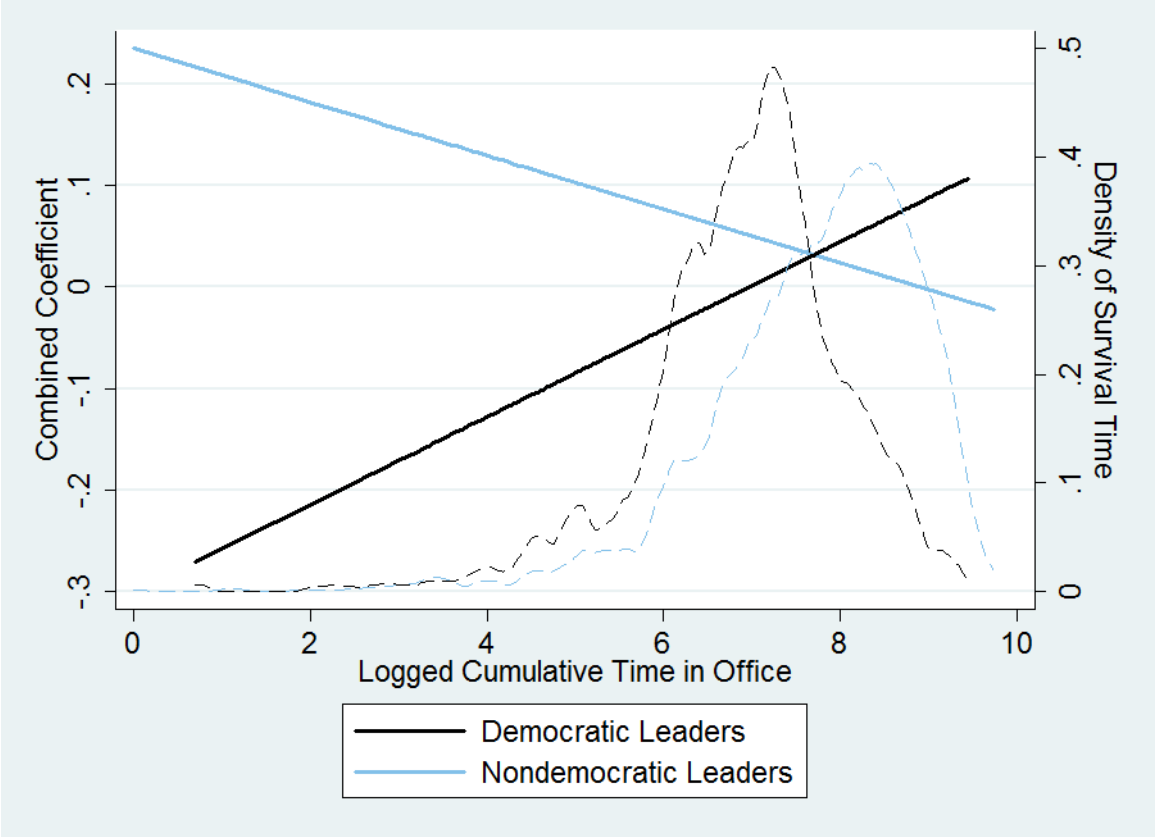

Figure 5.4 Range of Combined Coefficient Measure in Dyadic Sample of UNGA Affinity

NOTE: Combined coefficient measure is calculated using Equation 5.1. The dashed lines of corresponding color provide the distribution of survival times for the type of leader. Aid is considered effective when the combined coefficient obtains negative values; ineffective when it is positive.

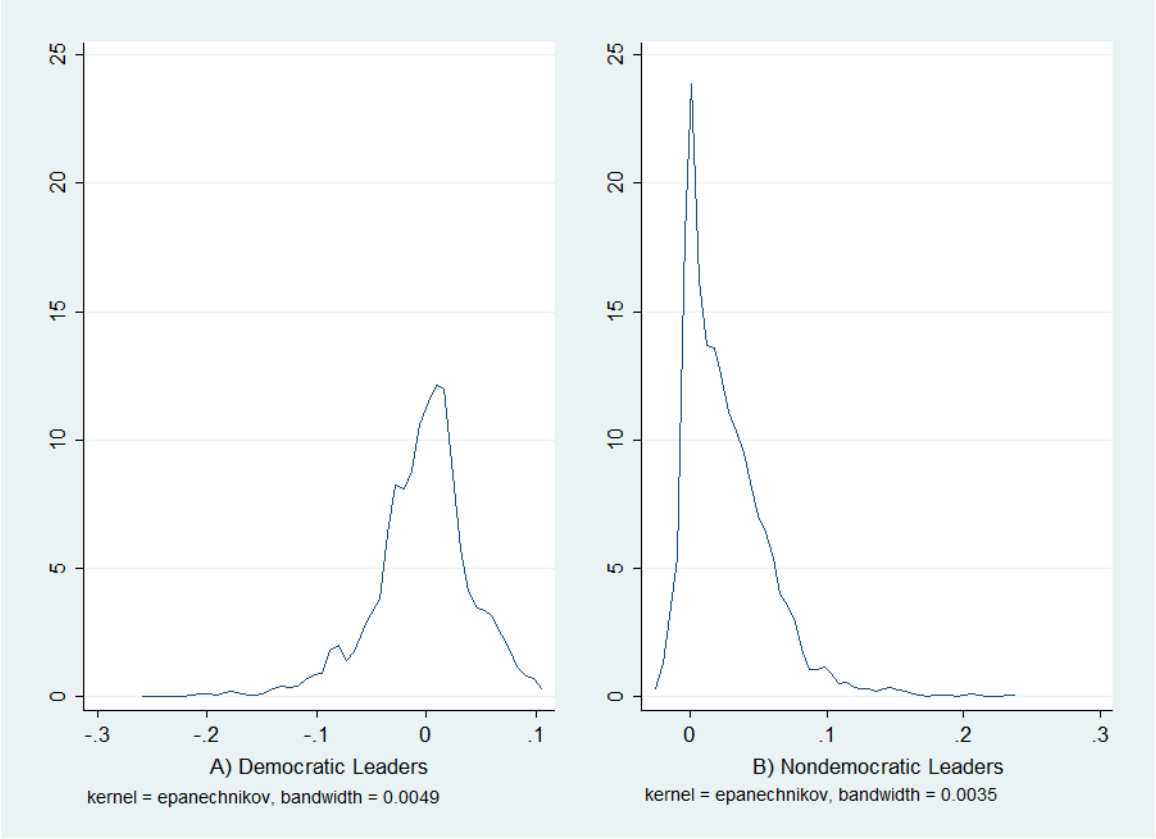

Figure 5.5 Density of Combined Coefficient Measure in Dyadic Sample of UNGA Voting Affinity

NOTE: Combined coefficient calculated using Equation 5.1. Foreign aid considered effective when combined coefficient is negative; ineffective when it is positive.

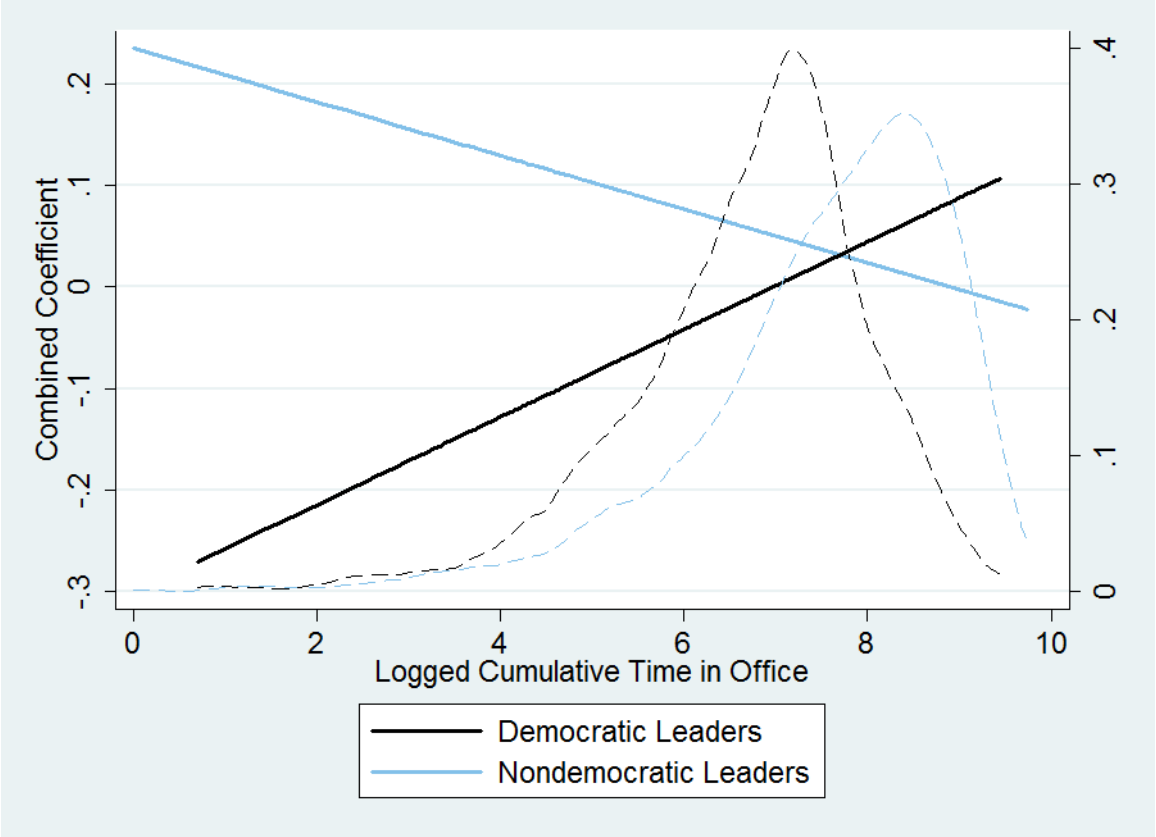

Figure 5.6 Range of Combined Coefficient Measure of Effectiveness in Monadic Sample of Aid-Recipient Leader Years

NOTE: Combined coefficient calculated using Equation 5.1. Dashed curves of corresponding color provide density of survival times for each leader type. Foreign aid considered effective when combined coefficient is negative; ineffective when it is positive.

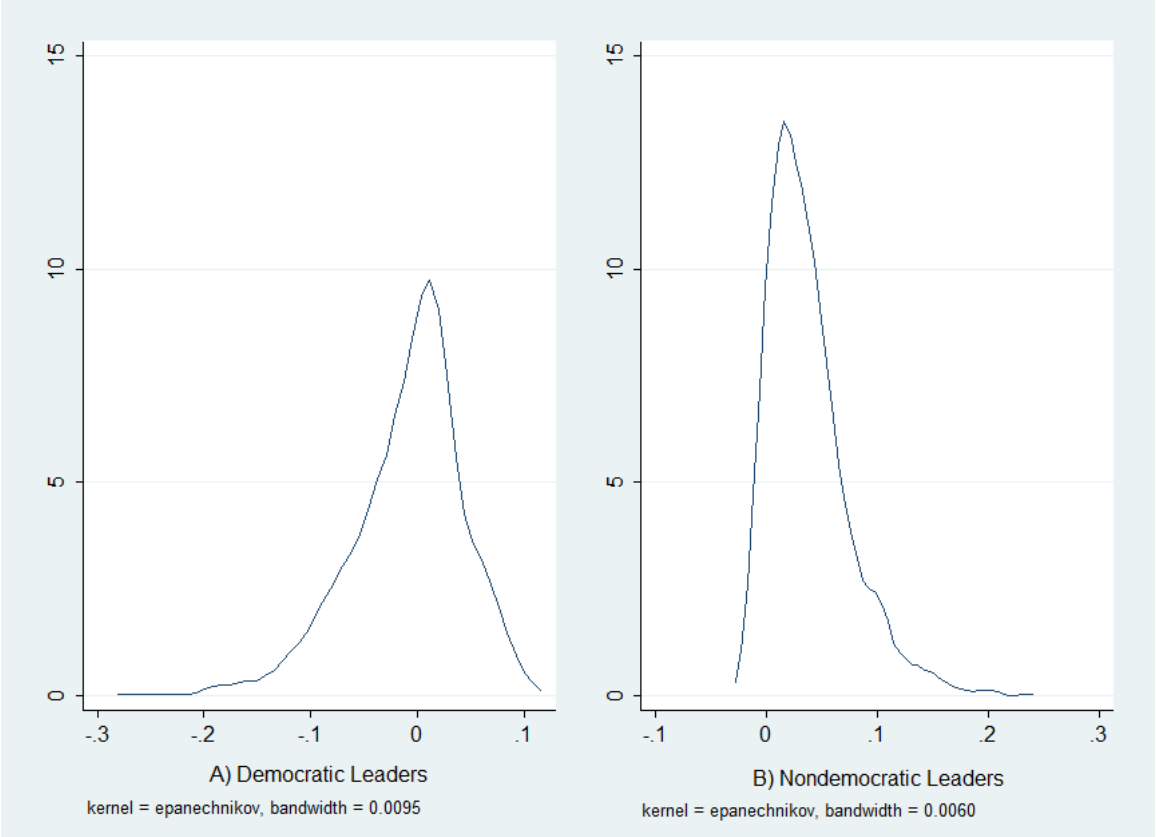

Figure 5.7 Density of Combined Coefficient Measure of Effectiveness in Monadic Sample of Aid-Recipient Leader Years

NOTE: Combined coefficient calculated using Equation 5.1. Foreign aid considered effective when combined coefficient is negative; ineffective when it is positive.

CHAPTER 6

PERSONAL BENEFITS, PUBLIC CONCESSIONS?: THE ROLE OF EFFECTIVENESS AND CONCESSION COSTS IN AID OUTCOMES

Morgenthau described foreign aid as a less-efficient version of the old, diplomatic strategy of bribery: "The transfer of money and services from one government to another performs here the function of a price paid for political services rendered or to be rendered" (1962, 302). Many may find this blunt profession offensive: foreign aid allocations represent a beneficent aspect of state behavior, an attempt to alleviate poverty and suffering. Either view, in isolation, will mischaracterize some portion of aid allocations and effectiveness. As scholars several decades ago acknowledged, the goals of donors vary from case to case; sometimes aid allocations represent the strategic/instrumental goals described by Morgenthau and sometimes they reflect the humanitarian concerns by which aid effectiveness is more traditionally evaluated.[49]

Rather than focusing strictly on variation in goals which may not be clearly stated or which may overlap, I chose to consider the relative value of foreign aid to recipients and the political difficulty of rendering the services to which Morgenthau alluded. The "price paid" by donors can be measured in the magnitude of aid flows. But on the receiving end, both the value of the "payment" tendered and the cost of making good on an aid-for-policy transfer defy easy observation. The previous chapter pursued the first quantity, estimating the value of foreign aid receipt in terms of the most vital political currency: job security. Though positive sanctions, such as foreign aid, should function as payment for targeted leaders, most recipients of aid do not benefit from the international reward. For nondemocratic leaders, receiving aid creates higher risks of

[49] For frameworks which divide aid allocations by the goals of donors see Dudley and Montmarquette (1976) and McKinley and Little (1977).

losing office for the duration of even very long careers. Democratic leaders can benefit, but only if the aid arrives while the halo of electoral legitimacy still shines. Experienced democratic leaders face destabilization upon receipt of foreign aid.

The payment tendered, in other words, often functions as a price rather than a perk. This ineffectiveness should factor into the likelihood that we will observe services being rendered. The analyses below evaluate this central contention while giving further attention to the second ambiguous quantity: the costs of providing concession. As noted above, donor goals differ, and the relative difficulty of providing concessions in line with donor goals do as well. Even recipients who benefit from aid and (therefore) prefer to please donor states may find that the domestic political difficulty of doing so outweighs their international reward. Difficulty, in my framework, depends on the capacity of concessions to spawn political mobilization. Small, powerful interests are more likely to rally in the face of private costs than is the general public (Olson 1971). Hurting the public, however, may be more difficult when a political opposition exists to champion its interests.

Below, I will further elaborate my expectations regarding the probability of concession given effectiveness and domestic difficulty, and then I will review the research design. In the empirical tests, I utilize proxy variables to tap into the granting of both private- and public-costs concessions following aid receipt. Economic growth will be treated as an indicator for politically difficult changes to status quo macroeconomic policy which favor powerful interest groups. Shifts in UNGA voting portfolios towards donor states, on the other hand, suggests relatively cheap changes in low-profile foreign policy issues. Both dependent variables will be modeled using OLS regression. For the broad strokes of my argument, the results below provide considerable support. Aid works better when it insulates its targets from domestic political difficulties. For democratic leaders, this means early in their tenure; for nondemocratic leaders, only very

late in the game. Opposition presence and strength affects concessionary behavior, but not in the expected direction.

Domestic Political Difficulty and Concessions

I conceptualize the difference in concession costs as the likelihood of organized domestic political reaction. Keeping with my general assumption that political actors behave rationally, we should expect that some changes to the status quo will better motivate groups to respond. I summarized the primary determinants of difficulty as:

$$D = \gamma_1 C_{private} + \gamma_2 C_{mixed} + \gamma_3 C_{public} + (\gamma_4(O \times C_{mixed}) + \gamma_5(O \times C_{public})) \times f(t_i) \quad \text{(Eq. 6.1).}$$

As described in Chapter 3, the C variables in this function represent three levels of diffusion of costs across segments of the target population. Because political action is costly, only those with something big to lose will devote time and energy to punish leaders for aid-based policy concessions. Concession costs which will likely focus on a specific, identifiable, and active portion of the population, thus will trigger political backlash with higher probability. These private costs hit people who have the information and the incentive to protect themselves. When costs spread onto the public, the incentive to react diminishes (Olson 1971). Delivering concessions which inflict public costs, then, should be easier for all types of leaders. In the mathematical notation, this means that $\gamma_3 < \gamma_2 \leq \gamma_1$.

Politics, however, is never simple. The reality of the political context mediates the difficulty of concessions in two ways. First, rallying in the face of public costs becomes more likely under the presence of "entrepreneurs" for whom fighting the incumbent always makes sense. The political opposition, or "losing coalition", plays this role. In Equation 6.1, the opposition is represented by the term O. If the opposition plays this entrepreneurial role within the field of political competition, then a strong opposition presence may increase the difficulty of providing concessions with public-costs elements.

Thus O is interacted with mixed and public costs in the function above. The slope terms for these interactions, γ_4 and γ_5, should be greater than zero.

Second, leaders' experience, competence and political security in office all vary over the course of their careers. We observed the impact of time and winning coalition security on the effectiveness of aid in Chapter 5. Now, we will likely observe it in the ability and willingness of leaders to provide concessions. The opposition's ability to rile the public and leverage the actions of incumbents to their advantage depends not only on their strength and legitimacy, but on that of the leader who they wish to overturn. Thus in Equation 6.1, the interactions between opposition strength and public-costs inducing concessions are further conditioned by leader experience, $f(t_i)$.

In terms of hypotheses, these expectations translate to the following:

H1: Concessions which would impose public costs pose less domestic political difficulty than would those imposing private or mixed costs.

H2: A strong opposition, conditioned by leader experience, increases the likely domestic political difficulty posed by concessions which would create public or mixed costs.

H3a: The difficulty of concessions matters more over time for democratic leaders and decreases over time for nondemocratic leaders.

H3b: The difficulty of concessions matters less as leaders gain experience.

Unfortunately, differentiating between the provision of public- versus private-costs producing concessions requires adoption of some strong assumptions. No systematic collection of data assesses either the goals and demands of donor states or the compliance of recipient leaders. The tests below utilize rough proxies for concession-type. Reform of mercantilist or protectionist macroeconomic policies provides the exemplary private-costs-producing concession. In order to change the economic status quo, target elites must revoke the private benefits of the businesses which benefit from protection. The goal of such reforms, ostensibly, is to produce public benefits in the form

of economic growth. To proxy such reforms, then, I employ economic growth as the dependent variable of models assessing likelihood of private-costs concessions. The ability of aid to produce long-term economic growth has been hotly contested in the empirical literature for many years.[50] Strong restrictions in the conditions under which aid provides this benefit constitute the common thread in the debate. These restrictions, however, do not tap directly into the incentives of recipient elites.

For public-costs concessions, I employ another measure which has been classically treated as a means of evaluating the impact of development assistance. Voting in the United Nations General Assembly (UNGA) produces non-binding resolutions of largely symbolic import. These non-binding votes provide a nice test of public-costs, because (1) no particular domestic constituency is likely to feel focused pain and (2) information regarding each state's voting record is obscure. Recent work has demonstrated that the ability of USAID monies to produce noticeable shifts in UNGA voting patterns differs by recipient regime type (Lai and Morey 2006).[51] I provide an analysis of UNGA voting which is broader in terms of scope, including all OECD donors. I also incorporate a direct test of aid effectiveness on the willingness to provide these concessions.

To convert the broader hypotheses above to the terms being tested, then:

H4: Concession becomes more likely as aid effectiveness increases.

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[50] See, for example: Chenery and Strout (1966), Burnside and Dollar (2000, 2004), Islam (2002), Chauvet and Guillaumont (2002), Daalgard, Hansen and Tarp (2004).

[51] Lai and Morey (2006) find that only nondemocratic leaders become more likely to vote with the U.S. following aid receipt. The assumed mechanism for this effect was greater ability of nondemocratic leaders to benefit from aid receipt. We now know, however, that this is not generally the case, based on the analyses reported in Chapter 5. Aid hurts the ability of nondemocratic leaders to maintain power.

H5: Aid's impact on economic growth (as a proxy for private-costs concessions) will be smaller than its impact on UNGA voting patterns (as a proxy for public-costs concessions).

H6: Opposition strength will decrease increases in UNGA voting affinity (as a proxy for public-costs concessions) more than it will decrease chances of economic growth (as a proxy for private-costs concessions).

H6b: The difference in aid's impact on economic growth (as a proxy for private-costs concessions) versus UNGA voting patterns (as a proxy for public-costs concessions) will be decreasing over time for nondemocratic leaders and increasing for democratic leaders in accordance with winning coalition institutionalization.

H6c: The difference in aid's impact on economic growth (as a proxy for private-costs concessions) versus UNGA voting patterns (as a proxy for public-costs concessions) will be decreasing over time for all leaders as they gain experience.

Before diving into the details, I will provide a very brief summary of the empirical research design.

Research Design Review

Though the dependent variables of economic growth and UNGA voting affinity require the undesirable assumption that the result flows directly from a concessionary act, they allow relatively easy statistical analyses using Ordinary Least Squares regression. The differing trends in aid effectiveness and ability to measure opposition strength across regime types prompts separate analysis of leaders in each category. In the democratic leader models, opposition strength can be measured as a continuous variable by the vote/seat share of opposition parties (Vanhanen 2000). For autocratic systems, however, this data may be unavailable or inapplicable. The existence and legitimacy of opposition movements cannot be assumed in nondemocratic systems, but this does not mean it

should be treated as universally nil. Most nondemocratic leaders share power, symbolically if not practically, with a legislative branch of government. Though participation in legislative politics may be sharply curtailed, or co-opted by the regime, the existence of the legislature suggests that leaders see some need to negotiate with the losing coalition in order to maintain the safety of their position (e.g. Gandhi and Przeworski 2006; Wright 2008). In some cases, legislative actors may be powerful enough to act as representatives of the people, obstructing concessions which would be damaging to them. For the small winning coalition sample, then, I will use an indicator for existence of a legislative branch as the measure of opposition strength.

Though the democratic and nondemocratic leaders' concessionary behavior will be analyzed separately, the models estimated in each sample will be identical save for the opposition strength variables. Recall from Chapter 4, that the models of aid concessions will take the following form:

$$C_{public} = \beta_1 A_{ijt} + \beta_2 \widehat{E_{it}} + \beta_3 A_{ijt} \times \hat{E}_{it} + \beta_4 O + \beta_5 f(t_i) + \beta_6 O \times f(t_i) + \boldsymbol{\beta Z} + \varepsilon \quad \text{(Eq. 6.1)}$$

$$C_{private} = \beta_1 A_{it} + \beta_2 \widehat{E_{it}} + \beta_3 A_{it} \times \hat{E}_{it} + \beta_4 O + \beta_5 f(t_i) + \beta_6 O \times f(t_i) + \boldsymbol{\beta Z} + \varepsilon \quad \text{(Eq. 6.2)}$$

Where C_{public} indicates a public-costs concession, in this case shifts in UNGA voting affinity towards donors; $C_{private}$, a private-costs concession, in this case macroeconomic policy change favorable to economic growth rather than private interests. In the public concessions model, individual leaders are indexed by the term *i*, with years in their careers differentiated by *t*, and specific pairings with OECD donors *j*. A_{ijt}, then indicates the aid flows to leader *i* from donor *j* at time *t*. I will briefly discuss this finer grained measure of attempted influence within dyads below. Besides this variation in measurement of aid flows, the public and private concessions models contain identical theoretical variables. For both equations, then, $\hat{E}_{it}$ is the combined coefficient measure of aid effectiveness constructed at the close of Chapter 5. The strength of the opposition, measured as vote/seat share in the democratic model and presence of legislature in the

nondemocratic model, is indicated by O.[52] The natural log of leader's experience in office is $f(t_i)$. For each model a battery of appropriate control variables **Z** will also be included. For information on the control variables, please see the more detailed discussion in Chapter 4. The vector **β** gives OLS slope coefficient estimates, and ε indicates the error term assumed to be distributed Normal with mean of zero and variance of one.

The UNGA voting affinity model for democratic leaders utilizes a set of 34,469 donor-recipient-leader dyad-years from 1960-1999, including 9,398 dyads. The data covers only current aid recipients, though the amount of dyadic aid flows may be zero in any given year (or for the duration) if allocations come exclusively from other OECD states. The dyads include 93 democratic aid-receiving states and 484 individual leaders. For nondemocratic leaders, the aid-receiving sample includes 65,109 observations across 8,816 donor-recipient-leader years, from 452 leaders of 109 countries.[53] The annual growth rate in UNGA voting affinity serves as the dependent variable for the first set of analyses.[54] Averaging 69% for democratic and 73% for nondemocratic leader-donor dyads, this variable suggests that recipients tend to move their countries' voting patterns towards donors.

To separate out general aid receipt from obligations to a specific donor, I created a measure of dyadic aid dependence. Based on the proportion of net bilateral assistance

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[52] The opposition strength, of course, is also indexed by the current leader and the current year. I omit the subscripts for space and ease of presentation.

[53] The very large N in these analyses stems from the pairing of each individual leader (484 in the democratic sample and 452 in the nondemocratic sample) with each of the 27 OECD donor states for each year from 1960-1999 in which information from all variables is available.

[54] The annual growth rate is the current year affinity score minus the past year's affinity score, divided by the past year's affinity score with one added to both the numerator and denominator to prevent losing information when the S-score equals zero (see Equation 4.10). Rather than simply telling us whether states tend to agree with their donors, this variable captures *movement* in voting patterns, which is more consistent with the idea of concessionary behavior.

allocated by donor *j* to recipient *i*'s total net development assistance, this measure gives a clearer picture of both how invested the donor is in the influence attempt and how obligated the leader is likely to feel to the specific donor.[55] The average net dependence on dyadic donors is the same across regime types, at about 8%. The variance and maximums, however, are quite different. The maximum dyadic dependence on a specific donor for nondemocratic leaders is 80%, compared to 42% for democratic leaders.[56]

The dataset of economic growth in democratic systems contains 1,433 leader years from 1960-1999 covering 76 aid-receiving countries. For the autocrats, the data cover 2,385 leader years in 95 separate states.[57] The measure of aid here does not differentiate by donor, but aggregates the net flows from all OECD donors. For democratic leaders, net OECD official development assistance per capita averages 1.53% of GDP/capita; for nondemocratic leaders, 2% of GDP/capita.

Similar equations will be estimated across the UNGA and growth models, with key variables including the impact of aid on leader survival, aid levels, the interaction between aid effectiveness and aid levels, the strength of the opposition, leader experience, and the interaction between opposition strength and leader experience. Control variables will differ across models to account for the different processes pertinent

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[55] In keeping with the literature, all aid quantities were adjusted by recipient GDP and population size, so the dyadic aid dependence measure is:

$$A_{ijt} = (a_{ijt}\ per\ capita/GDP\ per\ capita) \times 1/(a_{it}\ per\ capita/GDP\ per\ capita);$$

Where a_{ijt} is the aid flow between recipient *i* and donor *j* in dollars; GDP is gross domestic product of recipient *i*'s state in year t; and a_{it} is total aid flow from all donors *J* to recipient *i* in year *t*.

[56] For nondemocratic leaders this dyad is Mexico-Canada in 1965. For democratic, it is Costa Rica-Netherlands in 1998.

[57] These datasets are strikingly smaller due to the monadic leader-year unit of analysis. Aside from this, there is more missing data in the economic control variables than in the strategic data used in the UNGA voting models, which leads to the loss of 17 states in the democratic model and 14 in the nondemocratic model of economic growth.

to UNGA voting and economic growth (see Chapter 4). To account for the inclusion of the estimated impact of aid on leader survival, I bootstrap the estimates, drawing 1,000 samples of size N with replacement and performing the regression on each. The reported estimates are the observed mean coefficients and normal-based confidence intervals across these 1,000 runs. With these general elements of the research design fresh in mind, I will proceed now to discussion of results, beginning with the models of big winning coalition leaders.

Empirical Results: Foreign Aid Outcomes for Democratic Targets

Tables 6.1 and 6.2 contain the bootstrapped OLS estimates for shifts in UNGA voting behavior and economic growth in democratic aid-receiving states. Evaluating hypotheses 4-6 requires interpretation of two interaction effects across the two models. The first interaction is between the effectiveness of aid and the level of aid dependence (either on a dyadic donor or on total bilateral assistance). The constitutive and interaction terms for this piece of the influence story can be found in the first three rows of Tables 6.1 and 6.2. The second interactive effect is the conditioning of the opposition's ability to influence concessionary behavior by leader experience. The terms pertinent to this relationship lay in the fourth to sixth rows of each table. In each case, the constitutive terms represent the marginal influence of the current variable when its partner takes a value of zero. Standard error estimates for these terms are equally conditional upon the value of the other variable. The coefficient for the product term relates the shift in the effect of a constituent term as the other increases by one unit. Let us begin by examining the role of effectiveness.

Hypothesis 4: The Impact of Aid Effectiveness in Public- and Private-Cost Concessions

It is impressive, given the conditional nature of these effects, that the constitutive effects for both aid effectiveness and dyadic dependence (see rows one and two of Table 6.1) reach high levels of statistical significance. If no aid flows between the donor and target, the effectiveness of received aid will drive down the likelihood of voting records drifting closer.[58] Likewise, the independent impact of dependence on the donor, when effectiveness holds at zero, is negative. If aid has no impact on survival then dependence will decrease the likelihood of strategic movement towards donors' preferences. The interaction between these two terms (row 3), however, is positive, indicating that increases in aid magnitude magnify the impact of effectiveness while decreases in effectiveness attenuate the impact of aid dependence. The non-significance of the product-term does not necessarily indicate that the conditioning effect does not exist. As both of the variables included in this interaction are continuous, assessing the conditional impact requires calculating the conditional impact across the full range of values (see Brambor, Clark and Golder 2006). [59]

I accomplish this task in Figure 6.1, which charts the combined coefficient or marginal effect, of aid dependence across values of effectiveness at two levels of aid dependence, the minimum and two standard deviations above the mean.[60] This conditional effect of aid dependence speaks directly to Hypothesis 4. Dyadic aid

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[58] Recall that high effectiveness scores have negative values indicating reduction in likelihood of failure. The positive coefficient thus tells us that negative (positive) values of effectiveness produce lower (higher) levels of affinity growth.

[59] This point is especially well-taken given that aid dependence is measured in this regression on a logarithmic scale, creating a variable with both negative and positive values.

[60] The combined coefficient is simply the derivative of the linear index with respect to the covariate. In this case: $\partial Y / \partial AidDep = \beta_1 + \beta_2 \hat{E}$. This quantity was evaluated across the full range of $\hat{E}_i$ and at key levels of aid dependence.

dependence measures the donor state's commitment to aid allocation as a tool of influence vis-à-vis the recipient leader. Charting its effect across the values of $\widehat{E}_{it}$, tells us how the success of aid as a policy tool changes given both the level of commitment of the sender and the ability of aid to tap into the survival motive.

I chose to chart this effect at the minimum and two standard deviations above average dyadic dependence to demonstrate the majority of the range of this impact while maintaining a clean scale of reference. At the maximum level of aid dependence of 80%, a value which did not exist in the democratic sample, the effect is so large as to obscure the still significant but more modest impact at smaller values.[61] Recall that effectiveness here means a reduction in the likelihood of failure, so negative numbers on the x-axis correspond to high levels of effectiveness. The x-axis is restricted here to show effectiveness scores only within the first five years of office, as the impacts fail to achieve significance after this turning point. The shaded bars include all values of the combined coefficient within the 95% confidence bounds. Lighter shading indicates heavier dependence upon the dyadic donor. The black bar, then, represents the impact of aid dependence when less than one-hundredth of 1% of the targeted leader's overall aid dependence can be attributed to the donor; the light gray bar marks the impact of aid dependence when the donor contributes 39% of the observed aid dependence. A thin, bright-blue, dashed line highlights the zero line. When the combined coefficient lies above this line, aid dependence produces a positive shift in the voting similarity between target and donor; when the combined coefficient falls below the zero line, it produces a negative shift in voting affinity.

To begin with the broad trend, democratic leaders award effective aid allocations with agreement in the UNGA only for donors who contribute less than average amounts

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[61] The impact charted at mean levels of aid dependence more closely resembles that of elevated dependence.

to overall dependence. As the donor's prominence in the aid profile increases, the impact on voting behavior reverses. Democratic leaders heavily indebted to a single donor tend to have voting patterns less similar to that donor when the aid effectively reduces their risks of losing office. When aid becomes ineffective, increasing the risks of democratic failure, aid dependence ceases to significantly impact shifts in UNGA voting patterns.

For brand new democratic leaders, minor contributions to aid dependence can produce a nearly .25 standard deviations larger shift in UNGA affinity.[62] To move a case with an expected change at the mean level of .688 by this quantity would amount to a 6% increase in affinity. As donor dependence increases, however, this effect diminishes, creating movement from the mean of only 2% following a one standard deviation increase in the rate of donor dependence. At average levels of dependence the impact is very small, but negative and significant. Donors whose contributions constitute about 40% of democratic leaders' net aid inflows may see their recipients drift farther away from them. When aid is most effective, producing serious insulation benefits, lop-sided obligations may cause targets to distance themselves in order to avoid charges of selling votes. A leader who might previously have been expected to shift policy modestly in the direction of a donor may reduce this move by 20% if the donor takes up an elevated proportion of the overall aid flows.[63] As aid becomes less effective, the impact of aid flows – whether positive or negative – diminishes, dropping out of significance, as noted above.

For democratic leaders, even the relatively cheap concession of providing strategic changes in UNGA voting patterns appears to be likely only under very strict

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[62] This quantity based on the median conditional coefficient estimate divided by standard deviation of changes in UNGA voting affinity for democratic leaders: .04212/.1779= .2380

[63] This quantity uses a UNGA shift of .3342, which is two standard deviations below the mean, as the baseline of comparison.

situations. If aid is effective in insulating these leaders, they proceed cautiously. A donor who may be considered an obvious influence on strategic behavior will be shunned, even if that donor's assistance enhances the target's domestic security. More subtle interventions, however, may fly under the radar, and successfully produce public-costs concessions if the positive sanction can deliver helpful reductions in the likelihood of losing office.

The difficulty of aid in achieving this public-costs concession does not bode well for its performance in the economic growth equation. The inefficient estimates for both constitutive coefficients and the product-term in Table 6.2 contribute to this pessimistic evaluation. The constitutive term for aid effectiveness, however, is significant at the lower confidence level of 90%. To give a full evaluation of the effect, I performed the same procedure as used above to produce Figure 6.2. As expected, the combined effect of aid dependence and effectiveness never achieves significance in the growth model. For all three levels of dependence, the bars in Figure 6.2 overlap zero at the 95% confidence level. This is a very difficult test: even if economic reforms are filed in the current year, real changes in economic growth most likely would not surface in the immediately following year. To ease this issue, I experimented with lagging the impact and flow of aid monies at two and three years, but results become only less significant as the immediacy of targeted leaders' personal benefit decreases.

In sum, the results discussed above provide mixed evidence for Hypothesis 4. Aid generally does not appear to function well as a tool of influence in these equations. Elevated levels of dependence on specific donors demonstrate commitment or investment in the influence attempt. Yet, the harder donors try to influence, the worse their outcome becomes. This tendency of democratic leaders to distance themselves from highly influential donors presents an interesting wrinkle. Lai and Morey (2006) speculated that such a dynamic may be at play when they found that democratic aid-recipients do not drift towards the U.S. on strategic votes in the UNGA. The results are not necessarily

resoundingly negative, though. Aid does sometimes move recipients closer to the voting profile of donors. This happens when donors use a very subtle hand, contributing only a small sum to the overall aid portfolio. These modest influence attempts only succeed, however, in the early years of a democratic recipient's tenor, when the flow of aid money is effective in lowering domestic risks. Moreover, the results here foreshadow promising conclusions for Hypothesis 5. The capacity of aid to influence strategic concessionary behavior far outstrips its ability to generate the private-cost concessions necessary to produce economic growth.

Hypotheses 5 and 6: Opposition Influence on Public- and Private-Costs Concessions over Time

The second interaction effect in the models of public- and private-costs concessions to foreign aid attempts to tap into the role of the opposition as mediated by leader experience/career trajectory. If the opposition operates as an entrepreneurial representative of the public, a stronger presence should decrease the likelihood of public-costs concessions relative to private-costs concessions. This does not appear to be the case.

The constitutive term for opposition strength in the UNGA voting affinity model (see row 4 of Table 6.1) is very small and insignificant, but represents only its impact on the leader's first day in office. A leader's experience, absent opposition presence, decreases the size of shifts in voting affinity. The positive coefficient on the interaction term, however, indicates that increases in opposition strength will decrease and likely reverse that trend. More specific and substantively interesting information about this relationship can be uncovered by calculating the combined effect across the range of both variables.

Doing so produces Figure 6.3, which charts the combined coefficient of opposition strength at three levels across leader experience. Again, the shaded bars in

this figure encompass the 95% confidence interval around the combined coefficient. Lighter color corresponds to higher levels of opposition strength. The x-axis is restricted to ten years to allow for easier viewing. After the ten year mark, the coefficient plateaus, so this restriction does not inhibit assessment of either statistical or substantive significance.

Overall, this graph illustrates the unexpected result that as the opposition becomes more powerful, incumbent leaders become more willing to sacrifice the public good for the private benefits of garnering favor with donor nations. For the first six months of a leader's term, the opposition exerts no influence on the likely shift of UGNA voting profiles towards aid donors. At the one year mark, the opposition contributes to slight increases in the magnitude of UNGA shifts towards donor states. Depending on vote/seat share, this effect ranges from .07 to .2 standard deviations. As the leader becomes more experienced, the average increases grow to between .11 and .34 standard deviations within four years.

A leader with ten years' experience facing an extremely powerful opposition may increase the magnitude of affinity shifts by 11% above the mean. Though this effect is not enormous, it is considerably stronger than the (non)result regarding opposition's role in changing level of economic growth. All three coefficients relating to this interaction effect in Table 6.2 fail to reach significance even at the 90% level.[64] Opposition strength, leader experience, and the interaction between the two, play no role in the growth of democratic aid-recipients' economies.

This might be interpreted as roundabout support for H5's expectation that the opposition's role would be emphasized in the decision to give public-costs concessions. Mostly, however, the result stems from the generally inefficient modeling of annual

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[64] Calculating the combined coefficient confirms that the effect of these variables remains insignificant across the full range of both.

economic growth. And the direction of the effect of opposition strength on UNGA voting refutes the expectation the "losing coalition" will take up arms for the public and prevent incumbents from making concessions which would cost the public. Instead, it appears that democratic leaders whose domestic support base falters may seek to please powerful external allies rather than fight a losing battle at home. It may also be that the UNGA matters so little that the opposition does not observe what happens in votes, leaving the incumbent fully free to do as he/she sees fit. In future it may be interesting to determine whether this result holds for all types of UNGA votes or if this finding is driven by votes over which the domestic population would have unclear preferences.

The time trend for relative difficulty of offering private- versus public-costs concessions may be tentatively aligned with the learning argument in H6c. If the effect of the opposition on private-costs concessions is always indistinguishable from zero, the increasing magnitude of public costs-concessions over time suggests that leaders with more experience may know better how to manage the tension between internal and external demands.

Summary of Results for Democratic Leaders

The overall results for democratic leaders lend support to the widespread skepticism regarding aid's efficiency as a tool of influence. Even when aid manages to insulate democratic leaders from the domestic political risks of losing office, recipients appear loathe to visibly return the favor. This is especially discouraging given that the UNGA floor provides a low-cost means of offering donor states strategic concessions. While the direct reward function of aid does not appear to be instrumental, democratic leaders may be spurred to side with donor states when their hold on power is shakier. High levels of opposition combine with savvy and experienced leaders to create larger shifts in voting affinity over time.

In the next section, I move on to consider the support for hypotheses 4-6 in the nondemocratic sample. For leaders with small winning coalitions, foreign aid constitutes a destabilizing influence for all but the most experienced autocrats. Increasing the value of holding office through an external windfall induces a "resource curse" effect of quickly cycling leadership. The driving expectation below, then, will be that the ineffectiveness of aid as a policy reward will produce blowback.

Empirical Results: Foreign Aid Outcomes for Nondemocratic Leaders

The first six terms in Tables 6.3 and 6.4 mirror those discussed above, modeling the two key interaction effects: aid effectiveness and dependence, and opposition strength and leader experience. The difference lies in the values of aid effectiveness and the measurement of opposition strength. $\hat{E}_i$ for this sample almost always holds a positive value, indicating a backfire in the reward. And opposition strength is proxied by the presence of a legislative branch rather than the relative electoral or parliamentary strength. The discussion below, however, will focus on the similarities in modeling rather than these differences. I will follow the same steps, evaluating Hypothesis 4 before proceeding to consider the role of the opposition and leader experience.

Hypothesis 4: The Role of Aid Effectiveness in Public- and Private-Costs Concessions

In the nondemocratic sample, the pattern of signs for the interaction between aid effectiveness and dependence is opposite that of democratic leaders. Aid effectiveness, for dyads where no aid flows, increases the likelihood of affinity. If aid would hurt, then non-contributing donors are discouraged by punishment in the UNGA, if it helps then they are rewarded. Aid dependence itself, if the transfer has no direct impact on leader survival, would increase cooperation. The negative sign on the interaction between these two terms indicates that as aid becomes more ineffective the positive effect of

dependence will reverse, creating punishments for aid that backfires. The negative interaction term also suggests that increases in dependence will deepen the negative impact of ineffectiveness on UNGA voting.

Following the same procedure as used to interpret the parallel regression above, Figure 6.4 charts the combined coefficient of aid dependence at three values of dependence across the range of effectiveness. The lighter the shading of the 95% confidence interval bars, the higher the dependence on the dyadic donor. The minimum is less than 1%, mean is 8% and two standard deviations above the mean is about 40% of total dependence stemming from dyadic aid flows. Aid effectiveness for nondemocratic leaders increases over time, such that temporally, the x-axis runs backwards, beginning with highly experienced leaders at the left and ending with brand new leaders on the right.

Figure 6.4 demonstrates the tendency of nondemocratic leaders to accurately reward and punish their donor states. New nondemocratic leaders who experience an overall destabilization as their aid dependence increases will distance themselves from dyadic donors who contribute large proportions of that hurting aid. Highly experienced leaders who benefit from the fungible aid resources, likewise, move toward those to whom they are most indebted while punishing those who do not spend much. In terms of magnitude, the shifts closely resemble those observed in the democratic sample, but the behavior corresponds much more closely to expectations.

Figure 6.5 plots the other side of this interactive relationship, the combined coefficient of effectiveness. As expected, here, the level of dependence inflates the existing pattern of effectiveness' relationship to UNGA voting shifts. Those who pay very little money do not receive punishment when overall aid receipt hurts nondemocratic leaders. But at notable levels of dyadic dependence, ineffectiveness produces large reductions in UNGA voting affinity. Dependence at one standard deviation above the mean level in the first year of office leads to a reduction of three-quarters of a standard

deviation in UNGA voting changes. This increases to a full standard deviation decrease when aid dependence increases to 40%. And at maximum dependence, the initial effect is an enormous eight standard deviation decrease in affinity. On the other side of the zero line, the reward for assistive aid to highly experienced nondemocratic leaders pales in comparison. Being the primary donor to a secure nondemocratic leader who benefits from aid may produce an increase in UNGA voting affinity of, at most, .22 standard deviations.

Within the realm of public-costs concessions, then, aid plays the expected role for nondemocratic leaders. When aid backfires it produces negative results for donors; when modestly effective it produces modest successes. Before concluding that nondemocratic leaders indeed make more sense than democratic leaders, however, we must evaluate the results for private-costs producing concessions.

Table 6.4 contains the bootstrap estimates for the regression of nondemocratic aid-recipients' annual economic growth. The constitutive effects for aid dependence and effectiveness both achieve significance with negative coefficients. When aid has no effect on leader survival, it tends to drive down economic growth; absent aid flows, likely ineffectiveness (i.e. newness of leader) also drives down growth. The interaction of these two, however, is positive and significant. Increasing ineffectiveness boosts the economic productivity of aid dependence; growing aid flows reverse the impact of ineffective aid, turning it into a boon for economic progress.

Figure 6.6 confirms that the relationship between aid dependence and economic growth operates inversely to its relationship with UNGA voting. When aid hurts nondemocratic leaders, high levels of it correlate with more economic growth, while lower levels drive down growth. Lower levels of dependence on ineffective aid shift economic growth downward, while higher levels of ineffective aid boost growth. At the other end of the x-axis, high levels of aid sent to very experienced leaders who stand to benefit from the windfall are associated with lower levels of economic growth. In other

words, aid that "works" in terms of leaders' survival-driven incentives does not appear to produce private-costs producing concessions as well as aid that does not "work".

For a nondemocratic state with a brand new leader, aid flows of 114% of the overall economy, which is just two standard deviations above the mean of aid-receiving dependence, relate to a .09 boost in economic growth rate. This is an increase of 1.3 standard deviations and a 690% increase from the mean level of growth.[65] The impressive public benefit, however, wears off after about three months. Over these three months, the ineffectiveness of aid in instrumental terms declines by half. It would be very difficult to attribute an increase in annual economic growth to reforms passed by a leader who held power for little more than one quarter of the year. The importance of aid dependence for determining economic growth remains statistically insignificant until the impact of aid on leader survival declines to one-tenth of its original strength. This takes about nine years. At this point, the aid flows begin substantially dampening economic growth, driving it down by 160% compared to the mean.[66] Leaders for whom aid creates an insulating effect, those in power for around 25 years, oversee an impressive 270% decline in GDP/capita growth following elevated aid flows.[67]

This temporal trend corresponds loosely to an argument made by Wright (2008). He claims that autocratic leaders utilize aid differently depending on how safe they feel in office. When the prospects for survival are long, they devote aid to public goods; when they begin to fear deposal, they switch to stockpiling and bribes. This coincidence, in my opinion, is just that. As I noted above, the benefits for economic growth occur only when a leader has held power for less than three months. The odds that growth results from

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[65] Quantity obtained through the following calculation: .0968692/.0140247=6.9070426.

[66] Quantity obtained through the following calculation: -.0225442/.0140247=-1.607464.

[67] Quantity obtained through the following calculation: -.0376547/.0140247=-2.6848845.

actions taken by that leader are slim to none. Linking macroeconomic change even to a one-year lag in policy change already makes an extremely difficult case; most economists utilize panels of four to ten year averages in growth (see for example Burnside and Dollar 2000, 2004).

A second reason to question the similarity between these findings and those in Wright (2008) can be found in the negative slope of the black bar in Figure 6.4. This bar demonstrates the reverse in the pattern for leaders receiving the minimum level of aid relative to economy size. For these leaders, aid constitutes only a very small fraction of state revenue, only .002% of overall funds.[68] When allocated to brand new leaders who face destabilization as a result, these small flows produce a decrease in per capita GDP growth of .028, which is a 200% decline from average levels. Again, this effect lasts for only the first three months of tenure. After nine years, the effect resurfaces in significance on the other side of the zero line. Highly experienced nondemocratic leaders receiving minimal amounts of aid relative to their overall economy size produce increases of 46% to 98% in economic growth as effectiveness increases. This positive result diminishes dramatically as the rate of aid dependence increases.

In Figure 6.7, the combined effect of effectiveness itself is charted at two levels of aid dependence, the minimum and two standard deviations above the mean. I omit the mean level in this graph as its 95% confidence bounds always include zero and tend to obscure the lower and upper bounds of the other elevated and low levels respectively. We can see here again, that as aid dependence increases, the role of ineffectiveness flips to the opposite of that predicted. Ineffective but plentiful aid increases economic growth, while plentiful effective aid decreases it. Only at minimal levels of dependence does aid

[68] Raw aid flows in this ballpark range from $2-107 million. These amounts were allocated to Chile, the United Arab Emirates, Kuwait, and Iran primarily in the 1980s.

effectiveness appear to lead to successful influence over the private-costs issues which must change in order to produce economic growth.

While puzzling at first, a bit of digging has uncovered a result similar to that for public-costs concessions in the democratic sample. Effectiveness operates as expected only for subtle interventions. "Strong-arming" the aid-for-policy transfer creates massive distortions in the domestic political economy making growth less likely than it was before. This result corresponds to arguments in the literature regarding the likely negative effects of dependence on external funds (see for example Burnside and Dollar 2004; Daalgard, Hansen and Tarp 2004; Svennson 2000; Brautigam and Knack 2004).

The principle claim of Hypothesis 4 gains qualified support from both models. More effective aid does produce evidence of both public- and private-costs concessions, but its ability to do so depends heavily on the amount of aid committed. To obtain strategic concessions in the UNGA, donors must commit to supply a heavy proportion of a leader's aid portfolio; for the private-costs concessions of economic reform, however, the donor must show restraint, supplying only the necessary amount of monies to prevent severely distorting the economy.

The findings suggest a conflict with Hypothesis 5, that public-costs concessions should be more easily obtained than private-costs concessions. While the effective portion of aid in terms of economic growth can produce increases of between 46% and 98%, the largest estimated increase in UNGA affinity following massive aid flows is 55%. At the lower end of its positive impact, the change drops down to 1.5%. The magnitude of the shift in private-costs concessions far outstrips that seen in the more strategic, public-costs arena. Donors, also, must expend more in relative funds in order to obtain this smaller change in strategic behavior. This inefficiency may be mediated by the more direct nature of the pay-off for donors in the UNGA vs. the economic growth example.

Hypotheses 5 and 6: The Role of Opposition Strength in Public- and Private-Costs Concessions over Time

The interaction between opposition strength and leader experience for nondemocratic leaders can be found in rows four through six of Tables 6.3 and 6.4. Hypothesis 5 states that the oversight and competition functions of the "losing coalition" should invite them to protect the population from concessions which decrease the public good. In democratic systems, we found the opposite to be the case (increasingly so over time). The same holds for nondemocratic leaders.

The independent impact of legislative "power-sharing" in nondemocratic aid-recipients, holding leader experience at zero, does not reach significance in the UNGA voting model. The constitutive term for leader experience in row five of Table 6.3, however, is negative and significant. As in the democratic leaders' equation, the independent impact of leader experience is conservative, decreasing the average change in dyadic affinity score. When paired with a legislature, however, this pattern changes.

Because the interaction here includes a binary variable, the first difference provides an excellent summary of both statistical and substantive significance. To construct such a measure I calculated the predicted change in UNGA affinity with the legislature indicator switched on and switched off at 200 values of leader experience.[69] Each of these calculations was repeated 1,000 times using random draws of $\widehat{\boldsymbol{\beta}}$ from the parameter matrices of the bootstrap estimates in Table 6.3. Figure 6.8 displays the result of this procedure. The thick line gives the median estimate from the simulated distribution, while thin lines mark the upper and lower bounds of the 95% confidence interval.

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[69] The predicted value in OLS is simply the product of all covariates and their coefficients. For these calculations all other variables were set at average or modal values as appropriate. The only factor besides the legislature indicator which varies is the effectiveness measure, because it is a function of time. Within each comparison, however, effectiveness takes on the same value, and its effect cannot influence the result.

From the earliest days in office, hypothetical aid-receiving leaders identical except for this institutional characteristic significantly diverge in their UNGA voting behavior. With one year in power, nondemocratic leaders with legislative institutions move their votes 5% closer to their donors than those without this check. The difference increases gradually over time, tapping out at UNGA affinity shifts about 11% larger.

Also mimicking the findings in the democratic sample, legislative presence and leader experience make neither independent nor conditional difference in the economic growth equation. Opposition presence and oversight appears to drive leaders into the arms of their external benefactors rather than keep leaders honest. The consistency of this pattern in nondemocratic systems is interesting both for those who claim authoritarian institutions increase public goods (Gandhi 2008; Gandhi and Przeworski 2007) and those who are more skeptical (Wright 2008b). While the institutions do not appear to create a positive outcome, the outcome they create is more similar to that observed in aid-dependent democratic states.

The similarity of the patterns across democratic and nondemocratic systems also provides evidence in support of H6c. Since the level of public-costs concession increase with experience for both types of leaders, the underlying mechanism is likely on-the-job learning. If patterns of coalition support drove the finding, the effect should have been reversed in the democratic sample.

Summary of Results for Nondemocratic Leaders

Just as the results for democratic leaders painted a gloomy picture for the efficacy of foreign aid as a tool of influence, so do those for nondemocratic leaders discussed immediately above. When aid is effective in reducing the risks of autocratic targets, it can increase the likelihood of both public- and private-costs concessions. The majority of the time, however, aid falls in the laps of leaders who will likely suffer as a result. When this is the case, the ineffectiveness of aid to dictators diminishes their willingness to

fulfill the type of aid-for-policy deals which much of our theoretical literature assumes to take place (see Bueno de Mesquita and Smith 2007; McGillivray and Smith 2000, 2004). Though perhaps pessimistic, this outlook corresponds well with the expectations of Hypothesis 4.

Unexpectedly, however, it appears that for nondemocratic leaders the right level of aid dependence at the right time creates very respectable gains in economic growth, a proxy for private-costs producing concessions. For UNGA vote shifts, the impact of effective aid is smaller and comes at a higher price. This contradicts the logic expressed in H5, that public-cost concessions should be easier to provide and thus more noticeable following effective aid. Similarly, Hypothesis 6's argument that the opposition will protect public interests falls flat in this analysis. Leaders who face some kind of domestic check at home become more likely to sacrifice public goods in favor of international approval. Finally, the results lend strong support to the learning hypothesis of temporal dynamics in H6c.

Conclusion: Aid as a Tool of Influence

The broad sweep of my argument describes two pieces in the process of successful influence: effectiveness and domestic difficulty. If a foreign influence attempt is to succeed it must both tap into the leader's survival motives and request feasible concessions. The series of tests above evaluated the groundwork of my theory with respect to foreign aid as an exemplar of foreign policy rewards. I hoped that this approach would shed light on the disappointing performance of foreign aid which has been so frequently lamented in both the literature on humanitarian goal fulfillment and strategic concessions to aid. Before moving on to apply my framework to other types of policy tools, I would like to briefly examine the links between the findings above and the extant literature.

The study of concessionary behavior in the UNGA has waxed and waned over the past decades. The earliest investigations found that only the United States could compel its beneficiaries to shift their voting pattern in the UNGA (Wittkopf 1973). Wang (1999) found that on important issues, voting patterns are sensitive to *changes* in dependence on U.S. aid, but not to standing allocations. And, most recently, Lai and Morey (2006) demonstrated more directly that American military and economic aid to nondemocratic leaders produces voting patterns more consistent with American preferences. The Lai and Morey (2006) piece works with assumptions similar to mine, namely that the impact of aid on leaders' ability to survive in office is what will determine concessionary behavior. Their tests, however, utilized regime-dummy interactions rather than estimates of aid effectiveness. While this method did not produce completely faulty inferences, it painted over significant variation within the concessionary behavior of both types of leaders.

My results from Chapter 5, first of all, demonstrated that effectiveness varies. It is not enough to argue simply that regime type determines whether aid is fungible and therefore whether leaders benefit from its allocation. Any fungibility of aid resources, in fact, appears to do just the opposite of what the static selectorate theory model would predict (e.g. Bueno de Mesquita and Smith 2007). Aid destabilizes all but the most experienced of nondemocratic leaders. The finding in Lai and Morey (2006), that only nondemocratic leaders provide concessions in the UNGA, therefore is likely powered by two unusual sets of autocrats: extremely long-lived leaders receiving extraordinary portions of their aid from a single donor, and very new nondemocratic leaders receiving almost no funds from the specific donor.

The Lai and Morey (2006) conclusion that democratic leaders do not provide strategic concessions to aid donors holds up fairly well to my more direct analysis of effectiveness. Rather than simply not moving closer to their donors, however, I find that heavily indebted democrats actually shift their votes farther away from their biggest

donors. Moreover, this recalcitrance cannot be attributed to a failure to benefit from fungible aid dollars. The same democratic leaders who punish their donors can expect to benefit more than any other aid-recipients. While my theory did just as little to predict this counterintuitive behavior as previous studies, my research design was able to locate the unexpected relationship.

The tests here also uncovered the impact of opposition presence on the likelihood of strategic concession, a variable overlooked in previous studies. Leaders facing greater political challenges at home, in defiance of my particular hypothesis, become even more willing to sacrifice the public interest and side with donor-states in the UNGA. This finding holds true across both types of institutions, bolstering the growing argument in the literature to treat authoritarian institutions seriously (e.g. Wright 2008b; Gandhi 2008; Gandhi and Przeworksi 2007).

I also tested the impact of foreign aid on the more traditional proving ground for aid success: economic growth. The literature on this topic contains a long history of skepticism. The Burnside and Dollar (2004) finding that aid's capacity to produce economic growth in more democratically governed states has proven both controversial and influential. My results do not support their claim. The patterns in economic growth observed across the democratic and autocratic samples suggest that the most propitious circumstances for success are these: nondemocratic system, experienced leader, small aid flows relative to economy size. These results connect to some strong arguments for the moderation of aid magnitude (Daalgard et al. 2004; Svennson 2000; Brautigam and Knack 2004). The importance of leader experience and nondemocratic regimes stems from the effectiveness of aid, fitting with my expectations.

Returning now to the bigger picture, the analysis which I have reviewed here does not produce a glowing recommendation for the potential to change the world through positive sanctions. In Chapter 5 we found that aid often fails to function as a reward for recipient leaders, and now we have seen that this shortcoming matters for influence

outcomes. "Hurting aid" to nondemocratic leaders suppresses shifts in favor of donors' preferences in the UNGA and fails to shift democratic leaders. Even when aid "works", however, its ability to produce results is conditioned by the magnitude of allocation, the type of concession under consideration, and the regime type of the target state. Donors seeking public-costs concessions should target experienced nondemocratic leaders with massive flows, and inexperienced democratic leaders with modest transfers. Getting private-costs concessions out of democratic leaders through positive sanctions may not be feasible. But for nondemocratic systems, aid seems able to prompt the type of reforms which lead to economic growth if sent to very experienced executives in very small amounts.

These tests, however, it must be noted, are necessarily indirect and also quite difficult. Before giving up on the utility of foreign aid, we should evaluate its performance in a variety of other circumstances. A viable starting-point for this investigation might be events data tagging overall cooperative vs. hostile actions taken by a target towards its donors. While the effectiveness of aid may put recipients in the mood to please their donors, it might not be an incentive powerful enough to produce the larger concessions studied here. Instead, it may contribute to an increase in overall cooperative language and actions. The recent addition of more fine-grained data regarding the intention of donors when allocating aid will also allow construction of tests which more fairly assess aid for its ability to accomplish its stated goals. These data, of course, are limited by the obvious political difficulty of clearly stating the less-humanitarian interests of donors.

For the time being, our discussion will now turn to the viability of my theory of influence in a different class of policy tool. For positive sanctions, domestic institutions, effectiveness and the relative difficulty of offering concessions have all played important roles in the provision of concessions. The exact relationships may have defied more specific expectations, as in H6, but the results still support the general thrust of my

argument. Influence happens in two parts: effectiveness in tapping into the survival motive and viability of demanded concessions. In Chapter 7, I will assess the effectiveness of economic sanctions, as an exemplar of negative foreign policy tools, while Chapter 8 will model the concessionary behavior of targets.

Table 6.1 Bootstrap Estimates of OLS Regression on Change in UNGA Voting Affinity for Democratic Leaders

	Coefficient	Std. Error	95% Confidence Interval	
Effectiveness of Aid	0.2495	0.0554	0.1409	0.3581
Logged Dependence on Dyadic Donor	-0.0257	0.0112	-0.0478	-0.0037
Effectiveness×Dependence on Donor	0.5663	0.3076	-0.0367	1.1692
Opposition Strength	-0.0004	0.0004	-0.0013	0.0005
Logged Leader's Time in Office	-0.0136	0.0038	-0.0210	-0.0061
Opposition Strength×Time in Office	0.0002	0.0001	0.00001	0.0003
Baseline Dyadic Affinity at Entry Year	-0.4394	0.0100	-0.4591	-0.4198
Regime Similarity	0.0014	0.0004	0.0007	0.0021
Jointly Democratic Dyad	0.0162	0.0055	0.0054	0.0269
Strong Alliance in Dyad	0.0274	0.0135	0.0010	0.0538
Same Region	0.2003	0.0668	0.0694	0.3312
Militarized Interstate Dispute	-0.0004	0.1408	-0.2765	0.2756
Hostility Level of Target State in Dispute	0.0584	0.0410	-0.0219	0.1388
Hostility Level of Donor in Dispute	-0.0433	0.0255	-0.0933	0.0066
Non-zero Net Aid	-0.0057	0.0028	-0.0113	-0.0002
Post-Cold War	-0.0125	0.0024	-0.0173	-0.0078
Logged Exports of Target Goods to OECD Donors	-0.0205	0.0053	-0.0310	-0.0101

Table 6.1 Continued

Logged Import of OECD Donor Goods to Target	0.0196	0.0050	0.0099	0.0294
Logged Total Aid Dependence	0.0072	0.0013	0.0045	0.0098
US-Middle East Dyad	0.1593	0.0702	0.0216	0.2969
US-Latin American Dyad	0.2102	0.0260	0.1593	0.2610
Constant	0.9523	0.0248	0.9037	1.0008
Wald $\chi 2$	4375.64, $p<.0001$			
R^2	0.5472			
RMSE	0.1197			

NOTE: N= 34,469 donor-recipient-leader dyad years from 1960-1999 including all OECD donor states and aid-receiving leaders. Estimates reported are bootstrap coefficients and standard errors based on 1,000 samples of size N drawn with replacement. Sampling clustered on donor-recipient-leader dyads.

Table 6.2 Bootstrap Estimates of OLS Regression on Economic Growth of Democratic Aid Recipients

	Coefficient	Std. Error	95% Confidence Interval	
Aid Effectiveness	0.1181	0.0670	-0.0132	0.2495
Logged Net Aid Dependence	-0.0005	0.0025	-0.0054	0.0045
Effectiveness×Aid Dependence	-0.0407	0.0308	-0.1011	0.0197
Opposition Strength	0.0004	0.0004	-0.0004	0.0012
Leader Experience	0.0017	0.0027	-0.0035	0.0069
Opposition Strength×Time	-0.00003	0.00005	-0.0001	0.0001
Infant Mortality Rate	-0.00004	0.00009	-0.0002	0.0001
Population Growth	-0.0003	0.0028	-0.0057	0.0052
Government Consumption Share	0.0002	0.0002	-0.0002	0.0005
Government Investment Share	0.0008	0.0003	0.0003	0.0014
Lagged Growth	0.1170	0.0679	-0.0162	0.2501
Initial GDP/capita	0.0013	0.0013	-0.0012	0.0037
Initial Growth Rate	0.0478	0.0645	-0.0787	0.1743
Oil Producing State	0.0027	0.0062	-0.0095	0.0148
Lagged Imports from Donor State	-0.0055	0.0036	-0.0125	0.0016
Logged Population	0.0038	0.0034	-0.0029	0.0105
Constant	-0.0630	0.0365	-0.1345	0.0085
Wald $\chi 2$	36.06, $p<.01$			
R^2	0.0763			
RMSE	0.0545			

NOTE: N=1,433 aid-receiving leader years from 1960-1999. Estimates reported are bootstrap coefficients and standard errors based on 1,000 samples of size N drawn with replacement. Sampling clustered on country. All variables lagged one year except for oil producing indicator, population size and controls for economy strength at first observation.

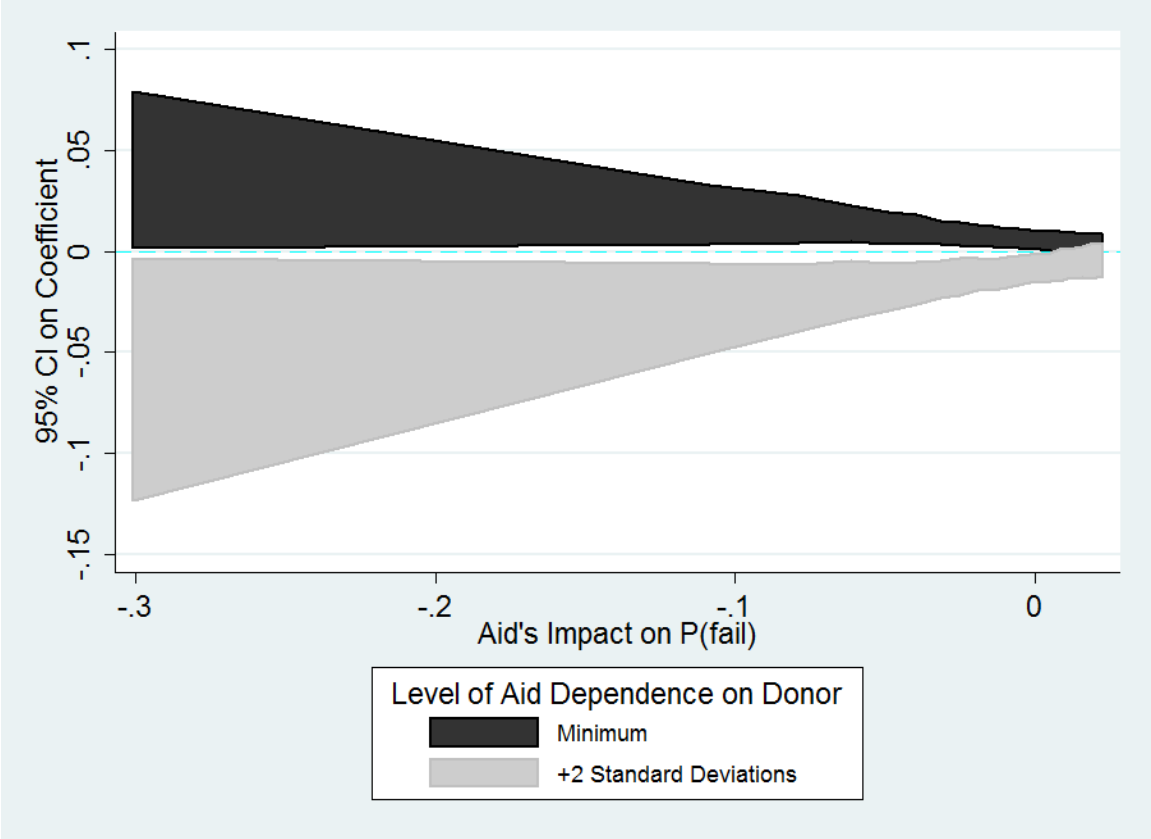

Figure 6.1 Conditional Effect of Aid Dependence on Democratic Aid-Recipients' Changes in UNGA Affinity by Level of Aid Dependence on Donor and Effectiveness

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.1. Minimum non-zero aid dependence on donor is .01% of total net aid dependence, mean is 8%, and two standard deviations above the mean is 39%. Thin, bright blue, dashed line highlights the zero line.

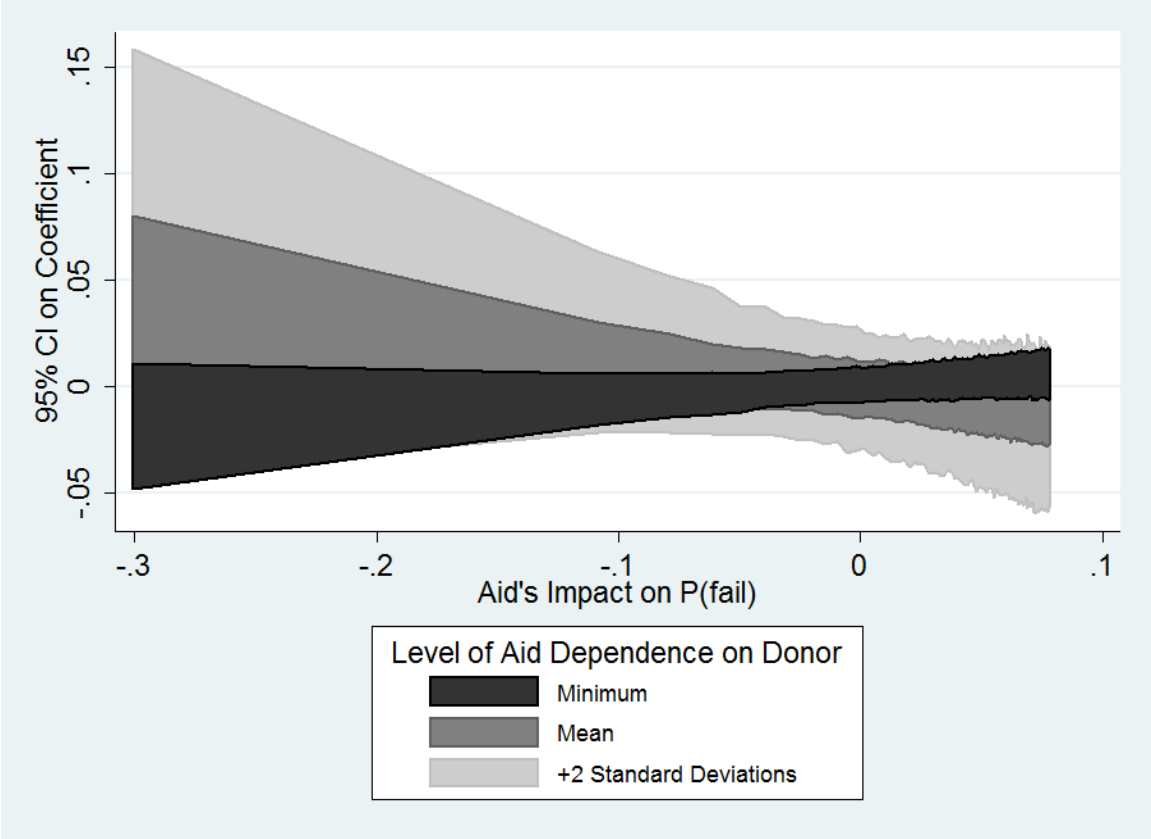

Figure 6.2 Conditional Effect of Aid Effectiveness on Economic Growth in Democratic Recipient States by its Values and Level of Aid Dependence

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.2.

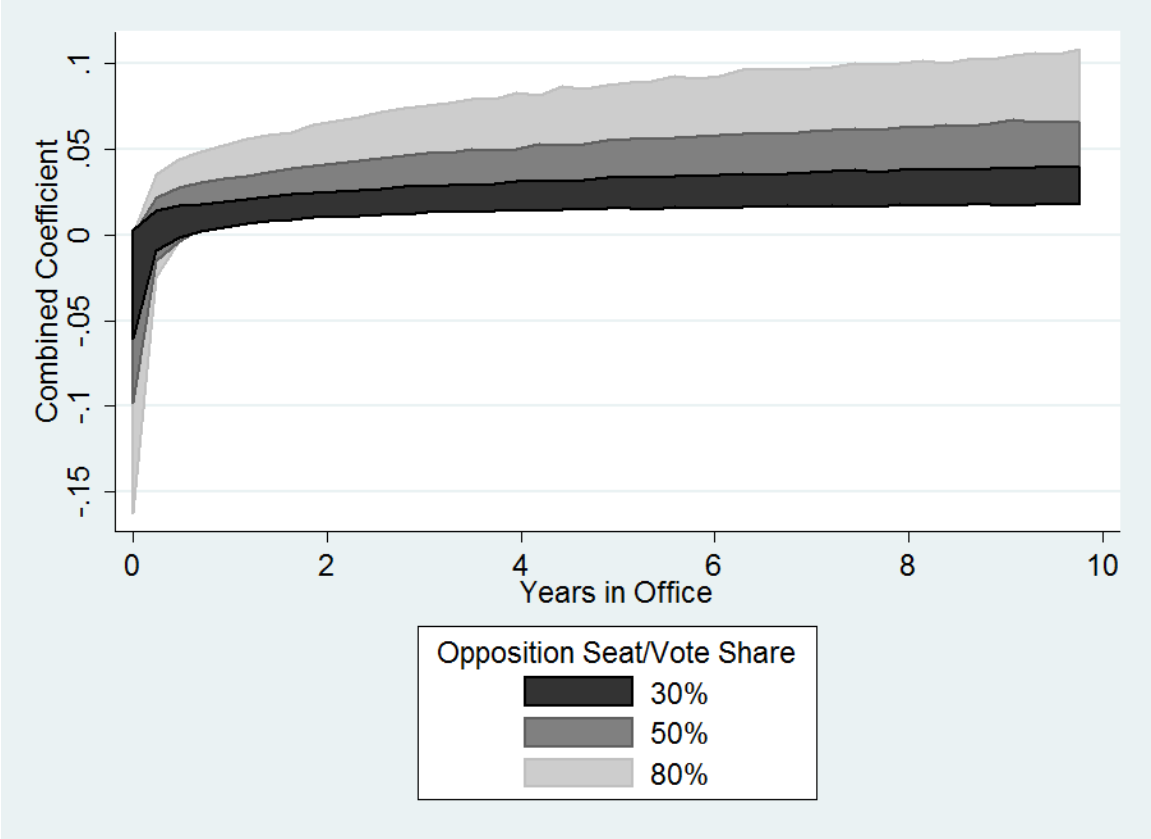

Figure 6.3 Combined Effect of Opposition on Democratic Aid Recipients' Shifts in UNGA Voting Affinity by Strength and Leader Experience

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.1.

Table 6.3 Bootstrap Estimates of OLS Regression on Change in UNGA Voting Affinity for Nondemocratic Leaders

	Coefficient	Std. Error	95% Confidence Interval	
Effectiveness of Aid	-0.2988	0.0864	-0.4682	-0.1295
Logged Dependence on Dyadic Donor	0.0640	0.0192	0.0264	0.1016
Effectiveness×Dependence on Donor	-1.3849	0.4414	-2.2502	-0.5197
Legislative Branch	0.0102	0.0209	-0.0308	0.0511
Logged Leader's Time in Office	-0.0161	0.0030	-0.0220	-0.0102
Legislative Branch×Leader Experience	0.0019	0.0030	-0.0040	0.0079
Baseline Affinity in Dyad at Entry Year	-0.3755	0.0076	-0.3904	-0.3607
Regime Similarity	-0.0004	0.0002	-0.0008	0.0000
Jointly Democratic Dyad	-0.0172	0.0291	-0.0743	0.0399
Strong Alliance in Dyad	0.2157	0.0943	0.0308	0.4006
Same Region	0.0271	0.0525	-0.0759	0.1301
Militarized Interstate Dispute	-0.0252	0.0138	-0.0523	0.0019
Hostility Level of Target State in Dispute	0.0680	0.0269	0.0154	0.1207
Hostility Level of Donor in Dispute	0.0072	0.0057	-0.0039	0.0184
Non-zero Net Aid	-0.0084	0.0027	-0.0137	-0.0032
Post-Cold War	0.0094	0.0066	-0.0036	0.0224
Logged Exports of Target Goods to OECD Donors	-0.0004	0.0002	-0.0008	0.0000

Table 6.3 Continued

Logged Imports of OECD Donor Goods in Target	-0.0063	0.0064	-0.0188	0.0061
Logged Total Aid Dependence	0.0084	0.0013	0.0057	0.0110
US-Middle East Dyad	0.4436	0.0382	0.3687	0.5186
US-Latin American Dyad	0.2071	0.0685	0.0729	0.3413
Constant	0.9596	0.0216	0.9173	1.0019
Wald $\chi 2$	4062.39, p<.00001			
R^2	0.4127			
RMSE	0.1587			

NOTE: N=65,109 donor-recipient-leader dyad years from 1960-1999 including all OECD donor states and aid-receiving leaders. Estimates reported are bootstrap coefficients and standard errors based on 1,000 samples of size N drawn with replacement. Sampling clustered on donor-recipient-leader dyads.

Table 6.4 Bootstrap Estimates for OLS Regression of Economic Growth in Nondemocratic Aid-Receiving States

	Coef.	Std. Err.	95% Conf. Interval	
Aid Effectiveness	-0.2308	0.1066	-0.4398	-0.0219
Logged Net Aid Dependence	-0.0063	0.0024	-0.0111	-0.0015
Effectiveness×Aid Dependence	0.1033	0.0352	0.0344	0.1723
Legislative Branch	0.0239	0.0160	-0.0074	0.0553
Leader Experience	0.0038	0.0018	0.0003	0.0073
Legislative Branch×Leader Experience	-0.0029	0.0020	-0.0068	0.0010
Infant Mortality Rate	-0.0001	0.0000	-0.0002	0.0000
Population Growth	0.0015	0.0019	-0.0023	0.0053
Government Consumption Share	0.0000	0.0001	-0.0002	0.0002
Government Investment Share	0.0008	0.0002	0.0003	0.0013
Growth	0.0780	0.0342	0.0111	0.1450
Initial GDP/capita	-0.0048	0.0011	-0.0070	-0.0026
Initial Growth Rate	0.0214	0.0432	-0.0632	0.1060
Oil Producing State	0.0177	0.0044	0.0091	0.0262
Logged Trade	-0.0009	0.0019	-0.0046	0.0027
Logged Population	-0.0006	0.0024	-0.0053	0.0041
Constant	-0.0082	0.0295	-0.0650	0.0496
Wald $\chi 2$	113.02, $p<.0001$			
R^2	0.0724			
RMSE	0.0724			

NOTE: N=2,385 aid-receiving leader years from 1960-1999 across 94 countries. Estimates reported are bootstrap coefficients and standard errors based on 1,000 samples of size N drawn with replacement. Sampling clustered on donor-recipient-leader dyads.

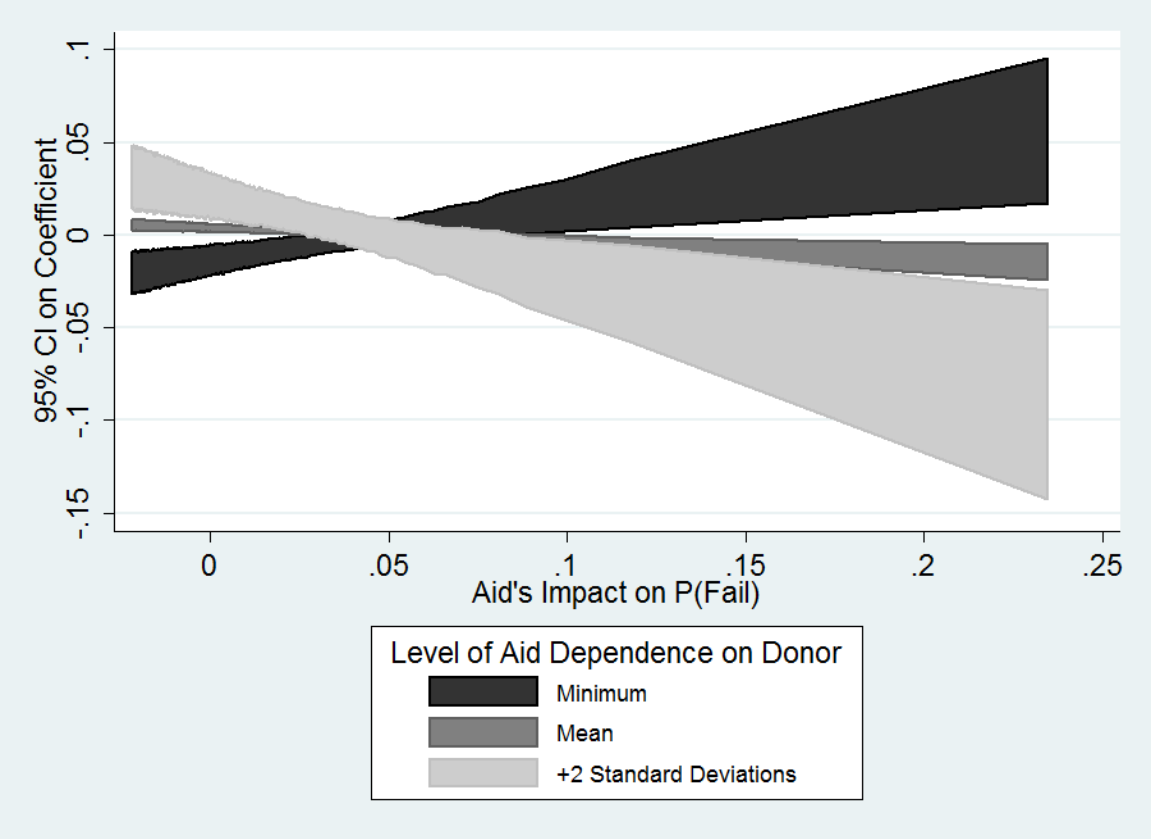

Figure 6.4 Combined Effect of Aid on Nondemocratic Aid Recipients' Shifts in UNGA Voting Affinity by Dyadic Dependence and Effectiveness of Aid

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.3.

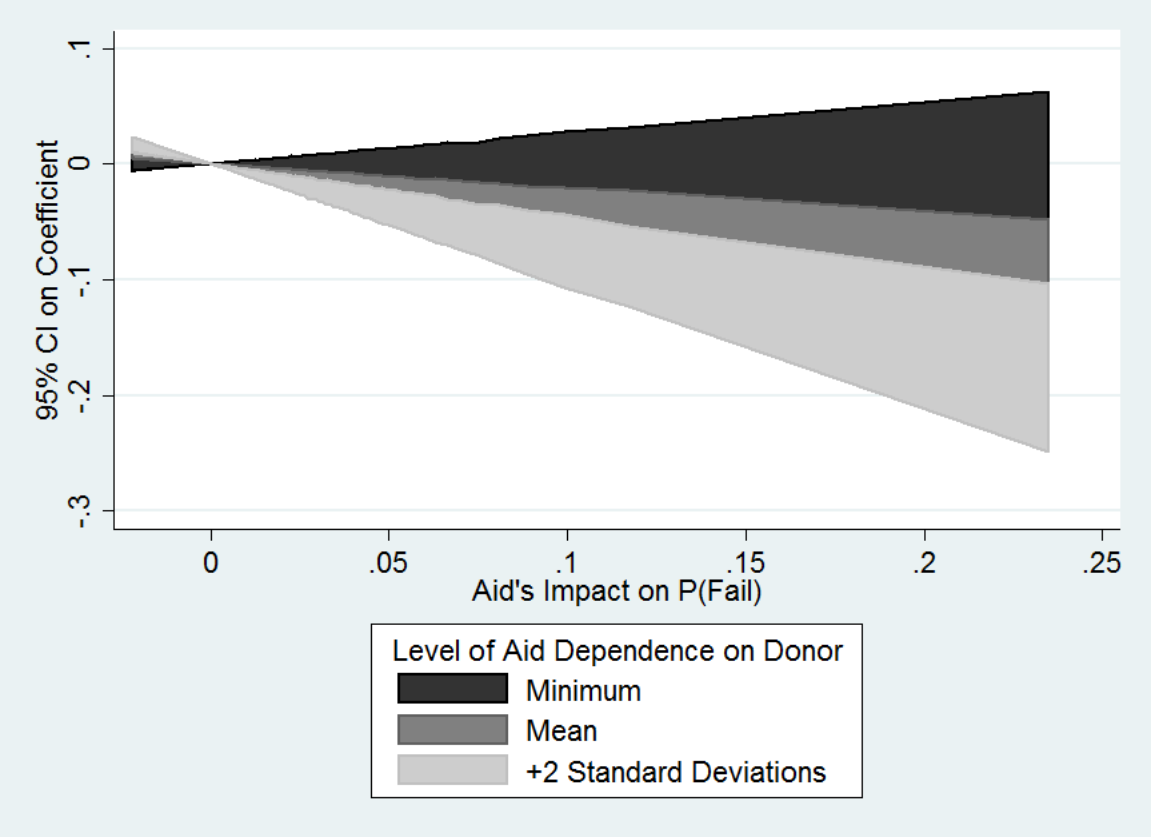

Figure 6.5 Combined Impact of Aid Effectiveness on Nondemocratic Aid Recipients' UNGA Voting Affinity by Dyadic Dependence

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.3.

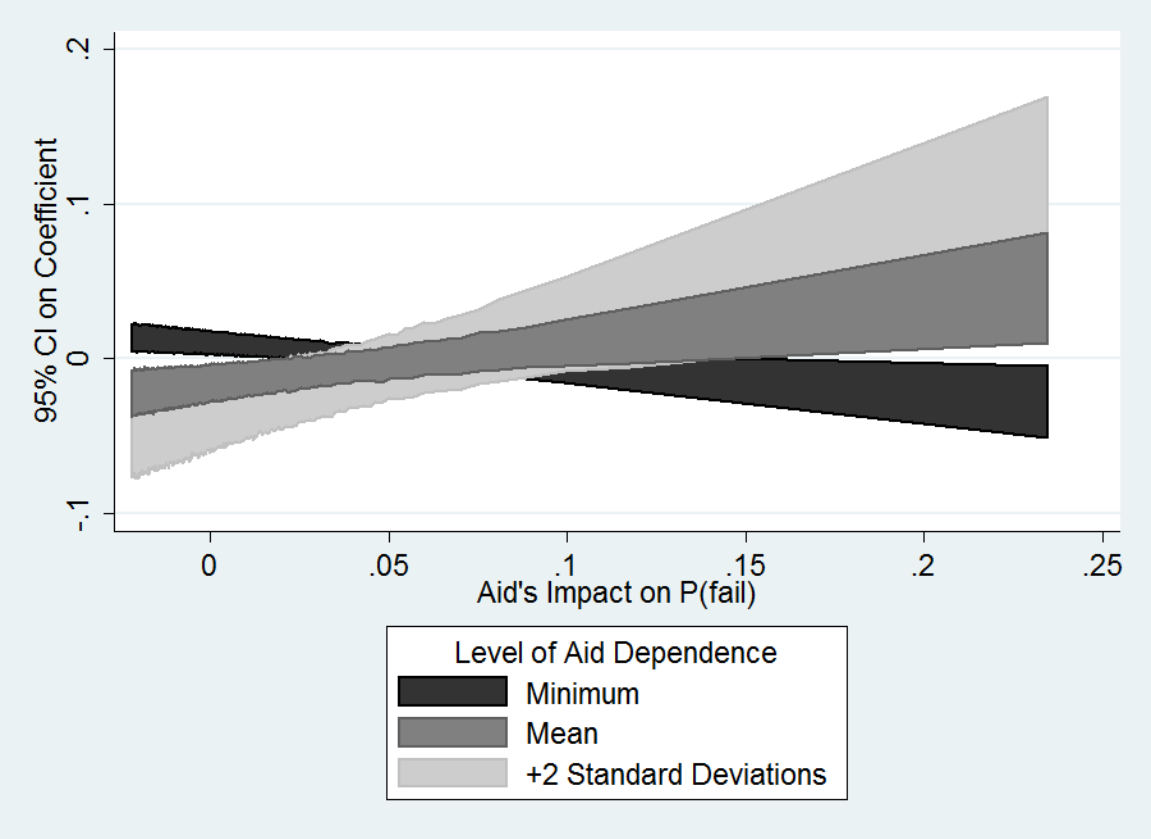

Figure 6.6 Combined Effect of Aid on Economic Growth in Nondemocratic Recipient States by Dependence and Aid Effectiveness

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.4.

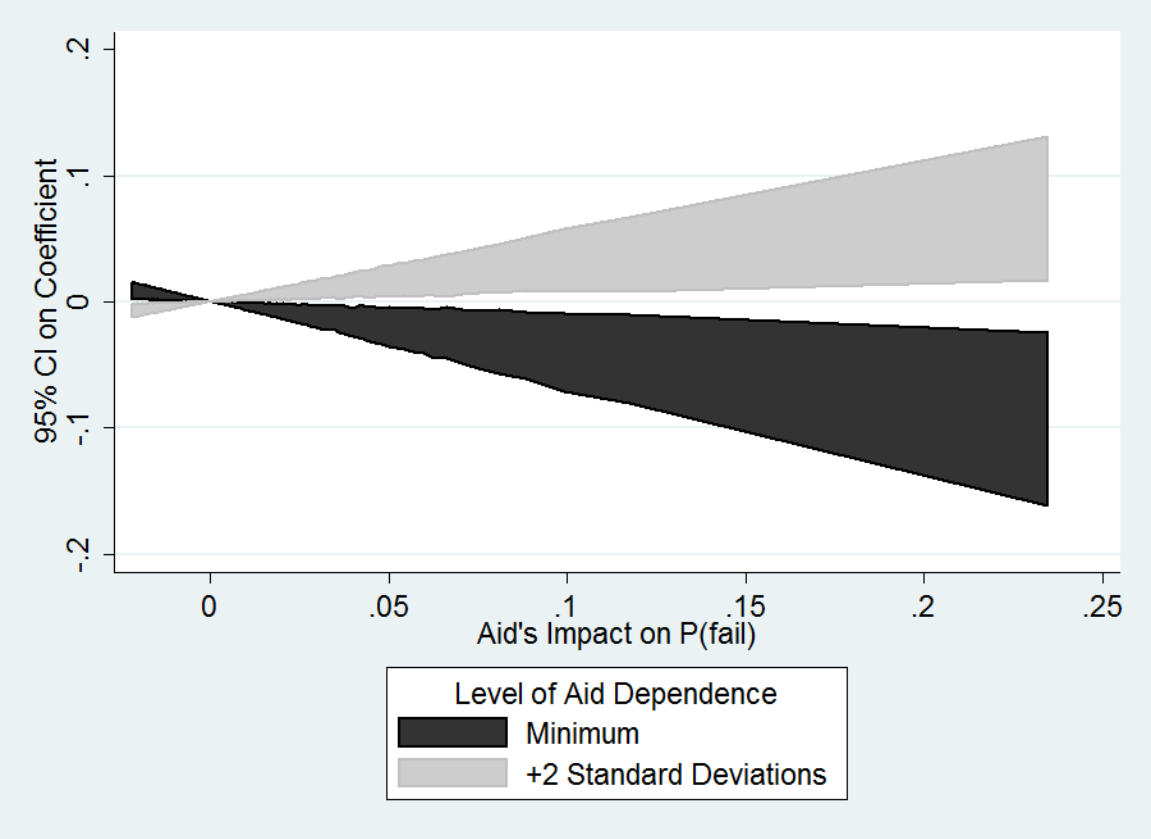

Figure 6.7 Combined Impact of Aid Effectiveness on Economic Growth in Nondemocratic Recipient States by Level of Dependence

NOTE: Bars represent 95% confidence interval around median estimated conditional coefficient based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression reported in Table 6.5.

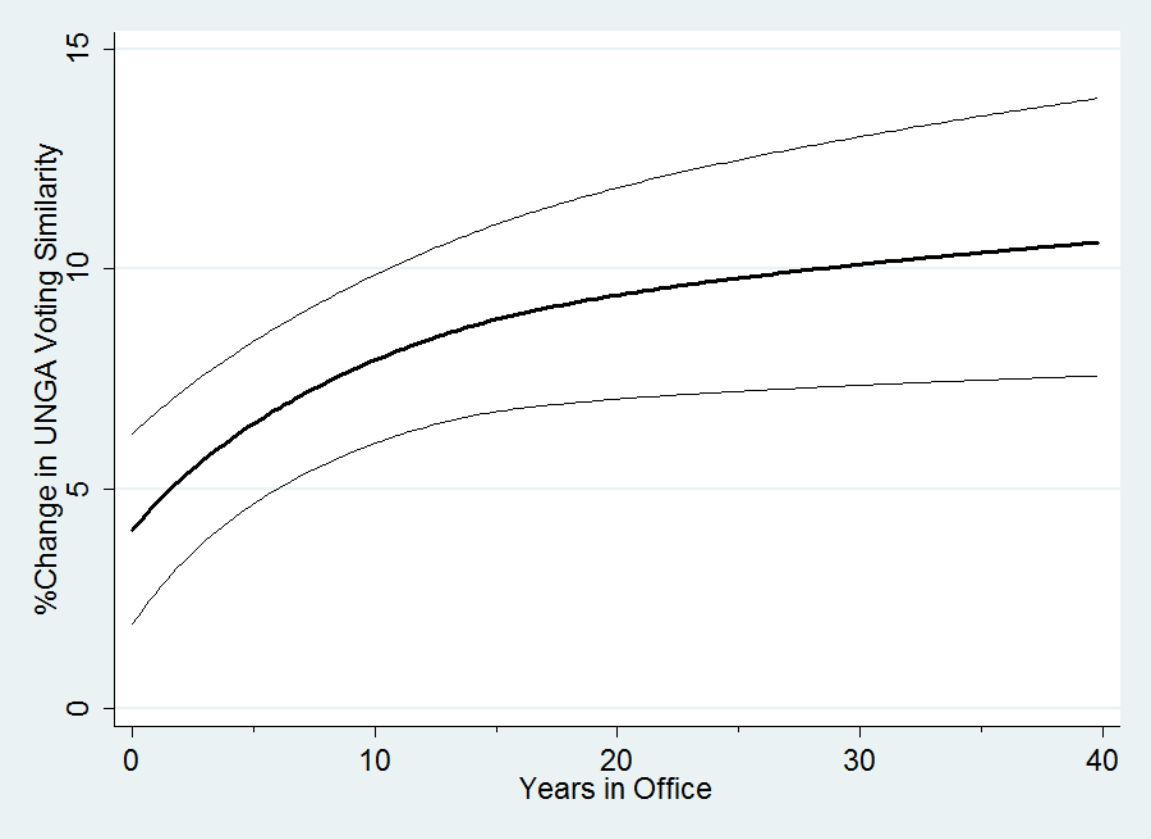

Figure 6.8 Change in Predicted UNGA Affinity Change for Nondemocratic Aid-Recipients with Legislative Branches

NOTE: Reported statistic is median percentage change in linear prediction based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped OLS regression in Table 6.3. Thin lines mark 95% confidence interval based on that distribution. All continuous variables were held at mean values for the calculation, including dyadic and total aid dependence. The exception to this rule is aid effectiveness, which because it is a function of time, varies with experience in the calculations. Indicator variables were set to modal values.

CHAPTER 7

FALING OUT OF FAVOR: THE EFFECTIVENESS OF ECONOMIC SANCTIONS

While any given exchange in the international system has a high probability of being cooperative, states often employ tools of punishment. Continuing the focus on economic strategies, this chapter turns our attention to the negative side of states' toolkits with an examination of economic sanctions. Scholars have noted a sharp increase in the use of this policy tool over the past decades, yet its ability to produce favorable results for sending states remains highly controversial (e.g. Pape 1997; Hufbauer et al. 1990; Morgan et al. 2009). I contend that much of this controversy may be accounted for if we consider the political impact of sanctions on targeted leaders.

In Chapter 3 I argued that this political impact, the "effectiveness" of a sanction, is conditioned by the facts of politics within the target state as well as the appropriateness of the tool itself. I represented these expectations with the following function:

$$(E|s_n) = c_n(-I_n) - f(t_i) +/- f(t_i)(-I_n) \quad \text{(Eq. 7.1).}$$

The term c_n, bounded by 0 and 1, indicates the credibility of the sending state's threat or punishment; $(-I_n)$ represents the institutionally conditioned impact of sanctions on leader survival, and $f(t_i)$ is the logarithmic function of the targeted leader's experience in office. The effectiveness of economic sanctions on targeted leaders, consistent with the general hypotheses H1 and H2 derived from this function, varies across regime type and over the course of leaders' careers. The credibility of threatened and enacted sanctions also conditions the magnitude of political consequences. Before presenting specific results, however, I will elaborate on the concepts of $(-I_n)$ and $f(t_i)$ under the specific situation of economic sanctions. The endeavor parallels that carried out in Chapter 5, drawing on extant theories as well as empirical findings to inform expectations for the application of the general theory to this specific case.

The following empirical tests will evaluate each of the hypotheses regarding variation in effectiveness. To account for the theorized endogeneity between leaders' likelihood of losing office and the targeting of economic sanctions, Equation 4.3.2 proposes a censored probit. The first stage estimates the probability of being targeted, using an instrument derived from an auxiliary regression on leader failure. Leaders facing elevated risks of losing office are significantly more likely to be targeted than those who are very safe in their positions. An increase of two standard deviations from the median level of risk creates a 170% increase in targeting likelihood. Despite this significant finding, however, the Wald test of correlation between the equations for failure and sanction targeting suggests that the processes need not be modeled simultaneously. Consequently, the results below will be presented as separate probit regressions. In the targeting equation, Murphy-Topel corrections were applied to the variance-covariance matrix in order to account for the estimation error in the instrument for failure (Murphy and Topel1985).[70]

The leader failure equation provides tests of hypotheses regarding the institutional and temporal conditioning of sanction effectiveness. Lending support to the learning argument the tendency for effectiveness to decline over time surfaces as the strongest and most consistent finding from this stage. Enacted sanctions can destabilize targeted leaders, but only very early in their careers. Interestingly, this period of effectiveness wreaks similar havoc on democratic and nondemocratic leaders. Threatened sanctions, however, perform differently across regime type. Democratic leaders enjoy large decreases in their likelihood of losing office when threatened by external punishment, with the potential benefits increasing steadily over time. For nondemocratic leaders,

[70] I utilize the procedure suggested by Hole (2006), which produces a variance-covariance matrix consistent with the recommendations in Murphy and Topel (1985).

threats also significantly decrease risks, but only when aimed at established leaders. Below I will present these findings in more detail.

Falling out of Favor

As a tool of influence, the first test of an economic sanctions' mettle lies in its ability to destabilize targeted leaders. The display of disapproval from a trading partner or the broader international community remains simply a display if it cannot penetrate the logic of survival-driven politics. In order to achieve this central goal, the economic punishment must impact the winning coalition. Generally, economic sanctions generate heavier costs for the mass public than for the elite (e.g. 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). The value of I_n will vary with the consistency of the impacted segment of society and the targeted leader's winning coalition.[71] Economic sanctions, which hurt the general public, will not hurt a leader who rules through the subjugation of the people. Rather, an external punishment which misses the mark may backfire, insulating targeted leaders from domestic processes of turnover. Below I provide more detailed discussion both of the type of sanctions and of the relationship between sanction consequences and leader survival.

[71] Economic sanctions, of course, are not monolithic. Sending states choose from a range of possible economic punishments, ranging from freezing financial assets, to banning travel, to complete economic embargo (Morgan et. al 2009; Hufbauer et al. 2007). Unfortunately, as is discussed more fully below, it is very difficult to separate the domestic interest group which sending states *intend* to target from those who actually end up bearing the brunt of costs and, further, from the issue over which the dispute first emerged. While I have made some attempts to parse out these differences and will briefly discuss some of the results from more nuanced analyses, the principle models employ measures more blunt, but, I believe, more reliable and consistent.

Institutionally Conditioned Impact of Sanctions on Survival

($-I_n$)

When policymakers and scholars use the term economic sanction, they refer to a broad array of policy instruments. Hufbauer, Schott and Elliot (1990), in their pioneering coding project, defined two basic types of economic sanction: trade based and financial. Sanctions on trade involved limitations on imports or exports, such as product bans, tariffs or embargos. Their concept of financial sanctions encompassed restrictions on food or development aid as well as the freezing of foreign assets and currency manipulations (Hufbauer et al. 1990, 36-8). The more recent TIES data project (Morgan et al. 2009), identifies nine more specific means of imposing economic punishments.[72]

The idea that the type of economic sanction employed might play a role in effectiveness is not a new one. Dashti-Gibson, Davis and Radcliffe (1997) hypothesized that financial sanctions should produce more favorable results for sending states, given their more targeted impact on decision-makers. Similar arguments have been forwarded by the public choice perspective (e.g. Kaempfer and Lowenberg 1999). The TIES project takes the intuition even further, attempting to determine the intent of sending states by identifying the interest group specifically mentioned in threats, categorizing threats as centering on the public, business and political interests, the military or the regime leadership.

When institutional rules empower a broad swath of the population, public costs producing sanctions stand greater chance of affecting targeted leaders. Democratic leaders, in general, cannot expect to keep their positions if they cannot maintain the general welfare of the population. Small winning coalition leaders rely on a coterie of

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[72] This coding scheme includes: total embargo, partial embargo, import restriction, export restriction, blockade, asset freeze, termination of foreign aid, travel ban, and suspension of economic agreement. A prominent competing frame focuses on the difference between unilateral and multilateral sanctions (e.g. Martin 1993).

business, political or military elites. Responsible only to this handful of powerful people, nondemocratic leaders generally manage to avoid punishment for diffuse public costs. Economic sanctions which create food shortages, general economic downturns and infrastructure deterioration will prove ineffective against small winning coalition leaders. Public choice theorists push this expectation further, postulating that autocratic leaders will directly benefit from the economic distortions created by costly sanctions. Nondemocratic leaders may find themselves better off due to a weakening of the population through hunger or deprivation, the cooptation of "rents" by monopolizing the supply of suddenly scarce goods (Kaempfer and Lowenberg 1999, 2000), or the evisceration of formerly powerful groups whose usual business is disrupted (Rowe 2000).

Credibility of Threats and Impact of Sanctions on Survival

(c_n)

Within the framework of effectiveness forwarded in Chapter 3, the potential for negative sanctions to impact targeted leaders depends upon the commitment level of sending states. The formal literature on economic sanctions places heavy weight on a selection process whereby the most successful events terminate at the threat stage (see for example Drezner 2000). In terms of effectiveness, however, threats entail potential costs whereas enacted sanctions inflict contemporary costs. The latter will be more effective in terms of actually changing leaders' probability of survival. The credibility of senders, in practice, however, is not so discrete. Even after enacting sanctions, the sender may exude varying levels of commitment. The credibility of an ongoing sanction may be relatively low if the sending state fails to voice its terms in an open and verifiable way, or if signals regarding willingness to maintain sanctions until concession are mixed.

Target Experience in Office ($f(t_i)$)

The effectiveness of economic sanctions is not solely a function of $c(I_n)$, however. The ease of tapping a leader's survival motive – even with a well-tailored, highly credible

threat – varies with that leader's experience and the cohesion of his/her winning coalition. The dynamic processes of learning and coalition institutionalization may both intervene in the ability of sanctions to negatively impact targeted leaders, but the effects of the two processes would differ. As was the case with the investigation of foreign aid, these processes produce competing hypotheses when applied to economic sanctions.

The learning process proposes leaders will learn better, faster ways to pursue their goals over time. Learning takes place regardless of institutional set-up. If competence increases over time, we should see economic sanctions decreasing in effectiveness with leaders' experience. An experienced leader has discovered means of sheltering vital supporters from the pain of external punishments, as well as strategies for leveraging external hostility against internal opposition. Skilled use of national images and symbols can transform perceptions of a sanction event, from a personal failure to a national crisis. Without the weight of experience, however, a leader may find it difficult both to deflect blame and to shield key groups. Economic sanctions should be more effective when timed to hit inexperienced leaders.

If the dynamic processes of coalition institutionalization take precedence, however, the expectations for sanction effectiveness will differ across regime types as well as over time. Democratic leaders enjoy the highest level of coalition loyalty at the beginning of their careers, before they begin making compromises (and enemies) in the policy making arena (Altman 2000; Brody 1991, 27-44; Lockerbie et al. 1998; Warwick 1992). The overarching empirical phenomenon of declining support for democratically elected leaders may overwhelm the impact of increased expertise. Elections lend a high level of legitimacy and prestige to democratic leaders, making even domestic criticism unpopular. If the negative input of legal domestic political competition is likely to fall on deaf or hostile ears during this stage of a democratic leader's career, the chances for external punishment to reap the intended effect are slim. Under such conditions, the target population is unlikely to perceive the imposition of economic sanctions as a failing

of their new leader. The opposite opinion may even be likely to form, producing a rallying effect similar to that hypothesized by Galtung (1967), which he expected even in the face of high public costs. The triggering of such a rally would seriously detract from the effectiveness of economic sanctions against a democratic leader.

The nature of democratic politics does not allow the permanent investment of legitimacy, however. Over time, regardless of term limits, the loyalty of democratic winning coalitions declines, creating an increasing risk in the probability of losing office (e.g. Warwick 1992; Altman 2000). As domestic competition's criticisms become better received, and skepticism regarding the intentions and competence of the incumbent, the effectiveness of economic sanctions should also rise. A population already willing to think badly of their government will be more willing to consider external punishments as evidence of bad behavior, facilitating the leader-specific punishment mechanism posited by McGillivray and Smith (2000).

For nondemocratic leaders, as the reader will likely recall, the pattern of coalition cohesion reverses. Legitimacy in most small winning coalition systems derives from staying power, rather than a ritual of leadership turnover. The early days in office for a nondemocratic leader feature higher risks, as their likelihood of being replaced declines over time (Chiozza and Goemans 2004; Bueno de Mesquita and Siverson 1995; Bienen and Van de Walle 1991). Signals of international disapproval may weigh heavier on the decisions of winning coalition members and potential challengers during the early period of a new nondemocratic administration. Challengers may find it easy to promise an end to outside punishments, and possibly even to suggest a profitable reversal of international opinion, if given the opportunity to lead. Winning coalition members who are uncertain of their place at the table due to the uncertainty surrounding the early years of autocratic rule, will lend greater weight to the promises of challengers than to those of their embattled leader. Economic sanctions, then, will exert their greatest effect on these new nondemocratic leaders. Over time, small winning coalition systems engender high levels

of loyalty in their members. Once well established, a nondemocratic leader need not worry about the ability of economic sanctions to destabilize the political arena.

Strategic Selection

Finally, the role of strategic selection must be considered. As discussed in Chapter 2, the imposition of economic sanctions is costly for sender states. This policy tool is not randomly assigned; states weigh options and consider the likely pliancy of targets before making threats. The general expectation that sending states will employ tools of influence against leaders facing elevated risks will hold with respect to economic sanctions. To ensure a higher likelihood that the population will perceive economic sanctions as a sign of leader failure, rather than as undesirable international interference in domestic affairs, sender states should avoid threatening leaders whose support base is very strong. If a leader is already facing serious opposition at home, additional damage to winning coalition satisfaction will be multiplied in domestic perception.

Research Design Review

This additional discussion allows reframing of the general hypotheses from Chapter 3 in terms of economic sanctions:

H1a: The effectiveness of economic sanctions will decrease with targeted leaders' experience.

H1b: The effectiveness of economic sanctions will decrease over time for autocratic leaders; and increase over time for democratic leaders.

H2: Economic sanctions will be more effective against democratic leaders than nondemocratic leaders.

H3: Enacted economic sanctions will be more effective than threatened sanctions, conditional on credibility.

H4: Targeting is more likely for leaders facing elevated risk of losing office.

These hypotheses will be tested against a sample of 3,781 leader years from 1971-1999, including 651 separate leaders. The sample of included leaders differs from that in Chapter 5 in two respects. First, the temporal range in this analysis is ten years shorter, beginning in 1971 rather than 1960. The most detailed and up-to-date data on economic sanctions, the TIES dataset, requires this restriction. Second, the analysis of foreign aid did not include leaders from the OECD donor states – as they have zero probability of allocating aid to themselves. This is not an issue with respect to economic sanctions, facilitating the inclusion of American and Western European leaders in the sample. Given the in-depth treatment of research design issues in Chapter 4, I will provide minimal discussion of such issues here. This chapter will proceed through two additional sections. The first will present empirical results, discussing the tenability of H1-H4 in terms of economic sanctions. The second will describe the construction and behavior of the combined coefficient measures of economic sanction effectiveness (see Equation 4.6). These measures will be utilized in Chapter 8, for tests of economic sanction outcomes.

Empirical Results

H4 indicates an endogenous relationship between the targeting of economic sanctions and the probability of leader failure. When modeled simultaneously in a censored probit regression with endogenous regressor (similar to the strategy employed in Chapter 5), however, the equations for two dependent variables do not pass a test of independence. Rather than utilizing the more complex model, then, I disaggregate analysis into the independent equations. I will begin discussion with the probit regression of sanction targeting, whose results can be found in Table 7.1.

Strategic Selection of Sanction Targets

In order to evaluate H4, this model features an instrument for the probability of failure derived from the linear index of the model predicting leader failure (see Table

7.2). The inclusion of this instrument introduces additional sources of estimation uncertainty. Accordingly, I adjusted the standard error estimates to incorporate the variance from the prior regression using a Murphy-Topel procedure (see Hole 2006). Despite both this adjustment and the failure of the independent equations test, a highly significant, positive relationship links the probability of failure and the likelihood of being targeted with threatened or enacted economic sanctions. A difference in Bayesian Information Criteria between a model of targeting excluding the instrument and the model reported in Table 7.1 provides very strong support for the inclusion of the instrument.[73] Additionally, the model including probability of leader failure performs marginally better according to the expected proportional reduction in error and expected proportion correctly predicted.

The most powerful predictor for being a target of economic sanctions in the current year, not surprisingly, is having been targeted in the year prior. Figure 7.1 displays the difference in predicted probability produced by switching on the lagged dependent variable. Leaders whose countries were targeted before are almost guaranteed to remain targets – with a probability of targeting .82 higher than those who were not sanctioned in the prior year. Involvement in militarized disputes, in contrast, significantly increases the probability of sanctioning by about .05. Because of the strength of the inertia in economic sanction targeting, the remaining calculations were carried out with the lagged dependent variable set to zero.

Figure 7.2 displays the predicted probability of targeting across the range of four pertinent continuous variables. Panel A describes the evidence pertinent to evaluation of H4, the impact of ex ante leader risks. As the probability of losing office increases, so does that of being targeted with economic sanctions. Extremely secure leaders face

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[73] $BIC_u - BIC_c = 205.3$.

almost no chance of targeting. At the median level of risk, a .09 probability of losing office, a leader's probability of targeting if not targeted in the prior year is only .03. An increase of two standard deviations, to a risk of about .3 produces a 173% increase in probability of targeting. Continuing to the uppermost values of risk, sanctioning likelihood increases to one in three. This finding provides support for H4, that higher-risk leaders make more attractive targets.

The remaining effects, though significant, do not produce relative changes of this magnitude. Panel B, denotes the modestly increasing probability of economic sanctions targeting as level of official development assistance relative to economy size increases. Aid can sometimes be utilized as a companion policy to economic sanctions, or can be allocated by other states in order to offset the harm done by others' economic sanctions. At the highest possible value of aid flow, however, the probability of becoming a sanctions target remains below 10%. In general, this finding shows that foreign aid and economic sanctions are allocated differently and for different reasons.

The relationship between trade and sanctioning, displayed in panel C, is negative. Those states most intimately linked by economic lines to the rest of the international system are slightly less likely to be targeted than are those on the outskirts. Panel D shows a slight, shallow decrease in the probability of sanctioning as the proportion of the population included in the winning coalition grows. Clearly, the change in likelihood of sanctioning from the smallest to largest winning coalitions systems fails to reach significance. This is interesting, given the likely higher sensitivity of more democratic leaders to the type of costs produced by most economic sanctions.

The Effectiveness of Sanctions and Threats

For evidence regarding hypotheses 1-3 we turn now to the parameter estimates for leader failure found in Table 7.2. The sanction type indicators interact with both the credibility scale and the indicator for big winning coalition systems. In multiplicative

models such as this the coefficients for constitutive terms refer to the effect of each covariate when all of those with which it is interacted take on values of zero. The sanction and threat indicators' constitutive terms (in row one and two of Table 7.2) represent very little information, referring to the impact of a threat or sanction aimed at nondemocratic leaders on their first day in office with a credibility of zero. Because the credibility scale scores 0 only when no threat is made, this combination of values is nonsensical. The credibility scale coefficient, in turn, gives the effect of a unit increase in the credibility of threats or sanctions which were never made. These coefficients mark the baseline from which the effect will depart as additional variables' impacts accumulate. The significant negative coefficient for the threat term (row two) tells us that while holding all other factors at zero, threats appear to insulate leaders from the risks of losing office.

The interaction terms between credibility and sanction type indicate the shifts in probability of failure likely to be observed if sender states targeting nondemocratic leaders look more resolved in the initial statement of intention. The further interactions with the natural log of cumulative days in office ($f(t_i)$) pick up possible changes in this effect as leaders become more competent, or as winning coalition cohesion changes over time. Finally, those terms interacted with the indicator for large winning coalitions relay the additional shifts in effect of credibility and time for democratic leaders. In order to obtain all the information necessary we must determine how the likelihood of leader failure changes when all three layers of interaction overlap in substantively interesting ways.

This information is not accessible without performing further calculations, but the pattern of significance and direction of these coefficients can provide preliminary information regarding the tenability of my expectations. The trend of sanction effects is likely to be less efficient than that for threats due to the greater standard errors for each term. Because each of the standard errors is highly conditional the effect is not

necessarily insignificant when combined, but it will likely be noisier. Also, the difference in the impact of sanctions across regime types is likely to be in magnitude rather than direction, if it exists at all. For both types, positive coefficients for the credibility interaction indicate increasing effectiveness with sender resolve. This increase will be pulled down over time in both cases as well, as the coefficients for the interactions with time are negatively signed.

In the case of threats, however, the trends for democratic and nondemocratic leaders will likely differ. For nondemocratic leaders, credibility of threats increases their effectiveness (see coefficient for C*T). Analogous increases in credibility actually decrease the ability of threats to destabilize democratic leaders, as marked by the negative coefficient for the interaction between credibility, big winning coalitions and threats. The over time trends of credibility also pull in opposite directions.

Determining the statistical and substantive importance of the conditional impact of sanctions on democratic and nondemocratic leaders, however, requires additional calculations. With layers of interactive effects such as we see in this model, the first differences approach provides a useful measure of both statistical and substantive significance (e.g. Kam and Franzese 2007). In order to obtain such a measure, I performed Monte Carlo simulations, sampling from the distribution of Table 7.2's parameters. Draws of 1,000 $\widehat{\boldsymbol{\beta}}$s and $\widehat{\boldsymbol{\sigma}^2}$s for 200 values of time in office were repeated for each arrangement of the values of sanction, threat, credibility and regime type. The probability of leader failure was calculated with the sanction type indicator switched on for a high value, and off for the low value. This low value was then subtracted from the high. Dividing this difference by the lower probability and multiplying by 100 produces a percentage change scale.

The resulting measure compares the probability of failure given targeting with either imposed or threatened sanctions versus the probability of failure without international interference. The simulated distribution allowed for simple location of 90%

confidence intervals around the median difference in predicted probabilities. A positive difference occurs when sanctions produce an elevated risk of failure; negative when they reduce risks. Thus large positive values indicate highly effective sanctions (or the threat thereof), and large negative values indicate highly ineffective sanctions (or the threat thereof). Figures 7.3 and 7.4 display the relative differences in the probability of failure for nondemocratic and democratic leaders respectively. Black curves mark the effect for enacted sanctions, while threat impact is depicted in bright green. Thin lines indicate the 90% confidence bounds around the median prediction. The level of credibility increases from left to right.

The negative slope of both sets of curves across Figure 7.3 and 7.4 suggests declining effectiveness over time, which meshes with the learning argument voiced in H1a. The patterns of magnitude and significance of effects produce mixed results for H3, which states that threats should be less effective while higher credibility should increase effectiveness.

Beginning with nondemocratic leaders, (see Figure 7.3) the impact of both threatened and enacted sanctions increases with credibility. Threats with low specificity produce no significant change in the likelihood of failure for autocrats while sanctions at this stage exert a 34% increase in risks. Raising credibility to its highest level boosts these effects to an estimated 161% for threats and 76% for imposed sanctions. Credibility, then, performs as expected for these leaders. The more sincere and to the point a sender is in specifying the grounds for repealing sanctions, the more damaging the consequences for targets. The contradiction for H3 lies in the higher estimated effect for threats as opposed to sanctions. Outside of low credibility, threats appear more damaging both in terms of magnitude and breadth of time. While a leader with one and one-third years of experience no longer experiences a significant destabilization from enacted sanctions, high credibility threats carry real bite for nearly three years.

For democratic leaders (see Figure 7.4), the impact of both imposed and threatened sanctions also increases with credibility, but this does not imply increasing effectiveness in all cases. Across the duration of the reasonable range of democratic leader tenure, threats produce an insulating effect. The more experienced the leader and the more credible the sender, the more pronounced the effect becomes. At low levels of credibility, the insulation begins with an 11% reduction in risk of losing office and accumulates to an 83% drop for leaders enduring to the 90th percentile of survival times. With medium credibility the insulation tops off at 96% and at high credibility it reaches 99%. In democratic systems, it seems threats of punishment from the outside trigger the rally in support of executive power which Galtung (1967) predicted. This finding may also mesh with one located in a case-study of U.S.-China relations, in which threats against China on human rights issues tended to exacerbate the situation (Drury and Li 2006). The same does not appear to be true of imposed sanctions. Actual punishments can cause new democratic leaders between a 22 and 49% increase in the likelihood of losing power depending on sender credibility. The window for destabilization, however, is very short. Senders must hit democratic leaders before they begin their second year in power if they hope to inflict political damage.

In sum, then, the learning hypothesis receives strong support from the pattern of decreasing effectiveness over time in both democratic and nondemocratic systems. Only inexperienced leaders who have not learned how to deflect the costs of punishment or how to spin international disapproval into domestic rallies suffer significant pain as a consequence of sanctioning. Hypothesis 3's contention that threats will be less effective than imposed sanctions appears to hold only for democratic states, where threats actually backfire, improving the domestic standing of leaders. The role of credibility stated in H3, however, does appear to hold. More credible sanctions inflict more intense effects.

Finally, evaluating H2 requires comparison of the effects across regime types. On this score, expectations receive little confirmation. When sanctions are imposed, the rate

of destabilization is uniformly higher for nondemocratic leaders. And, as opposed to performance against democratic targets, threats work quite effectively against inexperienced nondemocratic leaders. Much of the discrepancy may stem from the uneven distribution of specific types of sanctions across democratic and nondemocratic targets.

Ninety-two percent of sanctions targeting business interests emerge over trade disputes of some kind. These sanctions constitute the single largest category of coded observations in the TIES dataset, but the equivalence in effectiveness and intent of trade and strategic sanctions is not guaranteed. It may well be that trade-based sanctions surface when leaders consciously decide to protect their domestic interests despite the wishes of external powers. Perhaps especially for democratic leaders, such decisions will likely be quite popular with both business leaders (who benefit from unfair terms of trade) and the populace who sees a leader standing up for national interests.

The posited mechanism of domestic punishment for incurring international disapproval, then, may not function as smoothly in these cases. Indeed, many authorities in the field argue that trade-disputes which results in economic punishment or threats should not be treated as "economic sanctions" at all (e.g. Pape 1997, 1998; Hufbauer et al. 1990). Pape (1998, 71), for example, declares that commercially charged issues ignite domestic interests and prompt distributional conflicts which should qualify them as a different set of tools.

While I find the logic behind these objections slightly dubious,[74] I do acknowledge that the potential difference between trade and strategic sanctions dynamic

[74] Specifically, I find it strange to argue that commercial disputes prompt nationalism and distributional conflict more intensely than would, for example, disputes regarding military development or territorial disputes. All external demands involve some kind of domestic costs and, as Drezner (2003) argues, artificially limiting the universe of sanctions episodes may bias our estimates of when and how frequently economic sanctions prove useful as tools of influence.

could muddy our ability to evaluate H2, because trade-dispute sanctions disproportionately target democratic leaders. Table 7.3 tabulates the proportion of targeted observations which feature threats or sanctions against business interests by regime type. Only about one quarter of targeted nondemocratic leader years fall into the category of strict business disputes, compared to nearly 70% of observed sanction-years against democratic leaders. This difference achieves a very high level of statistical significance and may inhibit attempts to evaluate the impact of sanctions on leaders of different type.

To investigate this potential problem, I specified a model of leader failure which differentiates between business-based and more strategic sanctions.[75] Figures 7.5 and 7.6 chart the predicted probability of failure for democratic leaders across three conditions: targeting with business-based sanctions or threats, targeting with other sanctions or threats, and no targeting. As might be suspected, democratic leaders targeted with strategically motivated sanctions face higher risks of losing office than they would if sanctions were not enacted. The business-targeting sanctions also boost the probability of failure but only marginally and not significantly. The difference between strategically motivated sanctions and not being targeted, however, is positive and significantly different from zero at the 90% confidence level for only the first 5 months of democratic leaders' stay in power.

Figure 7.6 demonstrates an interesting wrinkle. The blue and green curves marking the trend in probability of failure given business-minded threats of sanction and no targeting respectively look very similar to those from the previous graph, sloping negatively. The trend of risks for leaders threatened with strategic sanction, however, drops below both of the other curves. At high levels of credibility, the curve develops a

[75] The model here includes only strategic sanctions in the interactions between time and credibility, leaving business sanctions to be assessed via the constitutive term for credibility.

positive slope which pulls the risks of strategically threatened democrats back above those of non-targeted ones. These highly credible strategically-motivated threats achieve significant disruptions compared to the baseline of no targeting. For democratic leaders, then, it appears that non-trade-based sanctions and threats do perform effectively and that the relatively poor performance in the original model may be attributed at least partially to the divergence of effects between types of tools.

Given these findings, I perform a final regression analysis using threat and sanction indicators recoded to exclude these primarily trade-based episodes.[76] The results of this model can be found in Table 7.4. The change in coding does not adversely affect the fit of the model, which still correctly categorizes 80.56% of cases, according to the expected percent correctly predicted measure. The findings relative to H1-H3, however, differ somewhat. Figures 7.7 and 7.8 chart the percentage changes in probability of failure given targeting with strategically motivated sanctions and threats.

When comparing strategic sanctions to a reference category including trade sanctions and non-targets, the effectiveness of threats against nondemocratic leaders dissipates. As Figure 7.7 demonstrates, however, the impact of imposed strategic sanctions destabilizes nondemocratic leaders at a higher rate than was originally concluded. At medium credibility, nondemocratic leaders targeted with strategic sanctions face an initial upswing in risks of 80%. The increase jumps to 105% for highly credible sanctions. The impact of strategic sanctions also remains significant for longer, dropping out of significance after about three years.

For democratic leaders (see Figure 7.8), the imposition of strategic sanctions does not produce significant changes in the risk of losing office compared to being targeted

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[76] This model does not *drop* observations which feature trade-disputes. Doing so would result in exclusion of 46% of cases in the dataset. Rather, these cases have been relegated to the reference category, as they would be in any analysis which defines economic sanctions as non-commercially-based restrictions in economic activity (or threats thereof).

with trade sanctions or not being a target at all. At less than optimum levels of credibility, threats also do little to shift the likelihood of failure. Hinting at the de-institutionalization process of democratic winning coalitions, however, external threats of punishment emerging from strategic dispute produce significant increases in the likelihood of democratic leader failure after five years.

Even with this alteration of the analysis, then, the effectiveness of imposed sanctions against democratic leaders continues to be lower than that against nondemocratic leaders. Threats may be effective if strategically motivated and aimed at democratic leaders whose winning coalitions have begun to falter, but the overall picture remains non-supportive of H2.

The failure of this endeavor to change results regarding the hypotheses combined with the high volume of missing data on the criterion for defining business-based sanctions lead me to prefer the original model (see Table 7.2). In the next section, I will present the findings from each of the models above in more general terms. This discussion, hopefully, will clarify some of the finer points as well as my reasons for preferring the first formulation.

Summary of Findings

The investigation of sanctions' impact on leader survival above proceeded through three different empirical models. In the first (and preferred model), I define economic sanctions as any threatened or actual restriction in economic relations, regardless of the issue over which the dispute emerged. The impact of sanctions and threats, then, are estimated compared to the probability of failure of leaders who were not threatened or sanctioned.

In the second model, I allowed for the potential uniqueness of trade-based sanctions by excluding any such threats or imposed sanctions from the interactions with credibility and time. The trade-based sanctions remained in the analysis, however,

because I did not recode the credibility scale to exclude them. This allowed me to evaluate the probability of failure given business-motivated sanctions relative to that given both more traditional sanctions and no sanctions targeting.

The second analysis suggested that the trend in probability of leader failure under trade sanctions more closely resembled that of non-sanctioned leaders than those facing strategically-motivated international punishments (see Figures 7.7 and 7.8). I then specified a third model, which, following some prominent scholars in the field (e.g. Pape 1997; Hufbauer et al. 2000), defines economic sanctions more strictly as only non-trade based disputes. This definition relegated observations of sanctions intended to punish the practices of domestic business-interests to the reference category with non-sanctioned leader years.

I pursued these extra tests of effectiveness in the hope that a more nuanced treatment of what constitutes an economic sanction would generate results more consistent with my hypotheses. In Table 7.5, I have arrayed the results across these three steps to summarize their similarities and differences. Displayed in this way, we can more clearly see that restricting the scope of sanctions ultimately produced a model with less efficient estimates of effectiveness without actually altering the conclusions regarding Hypotheses 2 and 3.

In the original model, we found that imposed and threatened sanctions more effectively destabilize nondemocratic leaders than democratic leaders. The conclusion regarding Hypothesis 2, then, was an unequivocal rejection. Calling the definition of sanctions into question shakes the certainty of this conclusion. In both the comparison and the strategic sanctions only models, threats of economic sanction effectively destabilize democratic leaders while failing to significantly impact nondemocratic leaders. The imposed sanctions, in these models, however, still hurt nondemocratic leaders more, making it difficult to conclude that H2 receives more than mixed support.

Hypothesis 3, which contends that imposed sanctions should exert a greater impact on leader survival than threatened sanctions, received mixed support across the board. In the first model, sanctions proved more effective than threats for democratic leaders, but not for nondemocratic leaders. Under the restricted definition of economic sanctions, this pattern reversed, with threats working better against democrats and sanctions working better against autocrats. No consistent pattern of higher impact emerged for either threats or imposed sanctions.

Table 7.5 also demonstrates the decreased efficiency of the estimates in a model which fails to treat trade-based restrictions in economic activity as economic sanctions. In the third column, only two of the possible four combinations of sanction and leader type achieve statistical significance. Treating leaders targeted with trade sanctions as equivalent to non-sanctioned leaders muddied the reference category. With a smaller difference between sanctioned and "non-sanctioned" leaders it became difficult to distinguish between the two. This inefficiency, combined with the lack of significant changes in the conclusions regarding Hypotheses 2 and 3, bolsters my preference for the original formulation.

While not particularly favorable for these more specific hypotheses, the analysis above does support the broader thrust of my argument: the impact of economic sanctions does vary with domestic institutions of representation ($-I_n$) and with leader experience ($f(t_i)$). The most important claims of the general theory of leader incentives and foreign policy effectiveness, then, fare well. The failure of H2, does, however, present some interesting challenges to findings and assumptions in the extant literature from which it was derived.

Analysts frequently argue that democratic states give in to sanctions at a higher and possibly faster rate than nondemocracies (e.g. Allen 2008, 2008b; Cox and Drury 2006; Nooruddin 2002; Bolks and Al-Sowayel 2000). The theory undergirding this prediction and empirical findings parallels that developed at the start of this chapter:

democratic leaders respond to the needs of the public, because their continued stay in power requires provision of public goods. Leaders responsible for and vulnerable to the demands of the broader public, will be more be punished by that public for the incursion of international punishments (McGillivray and Smith 2000, 811). And, Allen (2008b) finds some evidence to support the idea that sanctions will prompt domestic punishment of more democratic leaders; following sanction imposition, the number of riots and protests in democratic states increases significantly.

This study, however, is one of the first to directly attempt to model what has been so frequently assumed: that democratic leaders stand to lose more as a result of economic punishment than nondemocratic leaders. While Allen (2008b) provides tests of two possible causal mechanisms, my analyses attempt to locate the aggregate, or average outcome. Riots and protests may increase in democratic states without actually leading to a higher likelihood of democratic turnover. Precisely because of the freedom in democratic systems, it is perfectly possible – even likely – for some portions of the population to become hopping mad in the face of international punishment while others remain complacent or supportive of the policies which prompted the sanctions. In other words, the capacity for voice does not guarantee that a population will align itself with external interests rather than its own leader. Some evidence in support of this argument surfaced in McGillivray and Stam's (2004) investigation of leader turnover and sanction duration.

Allen (2008b) also found that the rate of riot and protest does not increase in nondemocratic systems targeted with economic sanctions, which would seem to conflict with my finding that sanctioned nondemocratic leaders face higher spikes in domestic risk. Again, these two findings do not necessarily conflict. First, because many of the leaders categorized as nondemocratic by virtue of winning coalition size are not treated as nondemocratic by the Polity2 cut-point scheme employed in Allen's work. But, more fundamentally, nondemocratic leaders hold office on the basis of elite, rather than mass,

approval. Public outbursts and riots, then, may not provide the most immediate measure of their job security. The displeasure of those vital to nondemocratic tenure would likely manifest in a more "subtle" and immediate manner.

My findings also challenge the only prior study of economic sanctions' impact on leader survival. Marinov (2005) also argued that domestic political destabilization constitutes the first step in successful negative sanctions. The formal model upon which Marniov designed his study, however, does not focus upon the conditions under which sanctions would achieve this necessary destabilization effect. Instead, the model concludes that senders will only enact sanctions when they believe that they will have this destabilizing effect, therefore the destabilizing effect should exist. Using a fixed-effects panel logistic regression and the Hufbauer, Schott and Elliott (2000) data (HSE), he does find a significant increase in likelihood of losing office following the imposition of sanctions. And, layering on a series of interaction effects to test whether some sanctions are more destabilizing than others, he finds that sanctions against democrats seem to be more damaging.

Marinov's (2005) results are not robust in the TIES data. Even after restricting the TIES data to strategic sanctions more similar to those tagged in HSE, I could replicate neither the base finding that sanctions unconditionally destabilize leaders, nor the interactive model's finding that sanctions hurt democrats more.[77] This earlier finding proves fragile because of its unwillingness to consider the process of influence in terms of the competing pressures on and dynamic nature of targeted leaders. Rather than asking the more nuanced question, "When might sanctions effectively tap into leaders' survival motive?", the analysis sought out a blunt up or down vote on their potential effectiveness.

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[77] My findings, however, do hold up to the alternative specification of a fixed-effects logit.

If effectiveness constitutes a necessary condition for successful influence, then the observed proportion of sender victories in economic sanctions episodes should prime us to expect some serious restrictions at this first step in the process. My results demonstrate vividly the narrow window of vulnerability through which sender states must squeeze in order to effectively punish targeted leaders. For both democratic and nondemocratic targets, the effectiveness of threats and sanctions rapidly decreases with leader experience. Destabilization only occurs against targets who have had less than three years' time to learn how to protect themselves and their winning coalitions from external interference. For democratic leaders, the window of efficacy for threats leaves not even a crack through which the survival motive can be effectively tapped. Threats of external meddling in domestic business prompt a hardening of positions and possibly even a swell of support at home which actually helps targeted leaders.[78] At least one of the conditioning factors for effectiveness allows senders room to affect outcomes. The credibility of threats and punishments inflates their impact on targets, both in magnitude and in duration of significant impact.

In sum, then, my tests have supported the overall argument that sanction effectiveness will vary by threat/imposition, sender credibility, target regime, and target experience. In the next section, we construct the measures necessary to complete our tests of negative sanctions and influence. In order to follow through and test the second half of my argument, we must create a measure of effectiveness to be included in models of sanction concession. The estimates in Table 7.2 will be used below to construct the weighted measure of how the significant factors add up to effectiveness.

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[78] Of course, as we noted before, this finding changes when we limit the definition of economic sanctions to purely strategic issues. Democratic leaders threatened with sanctions for human rights abuses or supporting terrorist organizations do not receive a bump in support at home.

Constructing Measures of Sanction Effectiveness

I proceed in the current section to the creation of $\hat{E}_i$, mirroring the methods used in Chapter 5. To obtain estimates with heightened certainty, I performed a bootstrap procedure to take 500 samples of size N with replacement, estimating the probit reported in Table 7.2 on each sample. The mean estimates from these 500 runs will be used as the vector of coefficients for the measures of effectiveness. Below I will briefly review the intuition behind and the generation of the combined coefficient measure of effectiveness. Summaries of the behavior of the measures in the sanction-year dataset which will be used to model concessions will follow.

The combined coefficient strategy draws on the concept of marginal impact. To obtain a linear measure of $\hat{E}_i$ I take the derivative of Equation 4.3.2's linear index with respect to the type of sanction. As the level of interaction in the economic sanctions model exceeds that in the foreign aid model, this process is slightly more complicated than before. For imposed sanctions, the formula for the combined coefficient will be:

$$\partial \mathbf{X_i\beta} / \delta S = \beta_1 + \beta_6 c + \beta_7 f(t_i) c + \beta_8 cW + \beta_9 f(t_i) cW \quad \text{(Eq 7.2)}$$

Where β_1 is the coefficient for the sanction indicator; β_6, for the interaction between credibility and sanctions; β_7,the interaction between credibility, time and sanctions; β_8, for the interaction between credibility, big winning coalition systems and sanctions; β_9,for the interaction between time, credibility, big winning coalition systems and sanctions. For nondemocratic leaders, the terms including W will be multiplied by zero and drop out. The effectiveness of threats is obtained using an equivalent formula. The only difference is that the coefficients for interactions with threats rather than imposed sanctions are used.

Positive values of this measure indicate that the combined effect of the sanction or threat, given sender credibility, target institution type and leader experience, contributes to an increasing risk of the targeted leader losing office. This impact may not have

translated to a significant shift in the predicted probability of failure. In terms of negative sanctions, increases in risks correspond to higher levels of effectiveness. Negative values occur when the sanction backfires, making the leader safer than before. When this happens, the sanction can be called ineffective.

For nondemocratic leaders, the combined coefficient measure for effectiveness of imposed sanctions ranges from -.331 to .299 across the observations in the sanctioned-leader year dataset. The range of threatened sanctions for these leaders spans -.666 to 1.278. Figure 7.9 charts these measures across the range of time against the density of leader survival. The combined coefficient measures reflect the higher effectiveness of threats as opposed to sanctions, in the steepness of the slopes and magnitude of their values.

Figure 7.10 demonstrates the range and pattern of $\hat{E}_i$scores for democratic leaders across time and level of credibility. Again, the trends coincide with the differences in probability discussed above. The threat measure remains negative across the panels, though its slope and magnitude intensify with credibility. The values range from zero to -2.04, indicating the ineffectiveness of threats against these leaders. The imposed sanction measure begins with relatively effective scores, reaching .50 above the zero line. But the bulk of observations cluster in the period of time during which imposed sanctions receive ineffective scores, sinking to a contribution of -.494 to the probability of failure.

Table 7.1 Probit of Sanction Targeting

Probability of Failure Instrument	0.6209*** (0.1098)
Lagged Target	3.1396*** (0.1050)
Logged Net Aid Dependence	-0.0906*** (0.0343)
Logged Trade (lag)	0.1045** (0.0437)
Logged Population	0.1066*** (0.0378)
CINC Score	0.4842 (2.3290)
Former Colony	0.2556*** (0.0897)
Revisionist State in Militarized Dispute	0.0528 (0.1407)
Militarized Interstate Dispute	0.2488** (0.1023)
Intensity of Civil Conflict	0.0376 (0.0645)
Winning Coalition Size	-0.2510 (0.1687)
Constant	-1.9721 (0.4124)
Wald $\chi 2$	1314.78
Pseudo R^2	.762
Expected Percent Correctly Predicted	92.99%

NOTE: N= 3,777 leader years from 1971-1999, including 649 separate leaders. Murphy Topel Standard Errors, in parentheses, corrected for the use of the instrument for probability of failure.

* p<.10, ** p<.05, *** p<.01

Table 7.2 Probit of Leader Failure by Sanction Targeting

Sanction (S)	0.1193 (0.1157)
Threat (T)	-0.2910* (0.1677)
Credibility of Threat or Sanction (C)	0.3030 (0.1895)
Log of Leader's Cumulative Time in Office (days) $f(t_i)$	-0.1589*** (0.0347)
Democratic System (W)	0.5240*** (0.1159)
Credibility of Sanctions Against Nondemocratic Leaders (C*S)	0.3047 (0.5413)
Time Trend for Credibility of Sanctions Against Nondemocratic Leaders $f(t_i)$(C*S)	-0.0776 (0.0714)
Credibility of Sanctions Against Democratic Leaders (C*W*S)	0.4928 (0.7208)
Time Trend for Credibility of Sanctions Against Democratic Leaders $f(t_i)$(C*W*S)	-0.0837 (0.1081)
Credibility of Threats Against Nondemocratic Leaders (C*T)	1.9548*** (0.7083)
Time Trend for Credibility of Threats Against Nondemocratic Leaders $f(t_i)$(C*T)	-0.2396** (0.0974)
Credibility of Threats Against Democratic Leaders (C*W*T)	-1.8712*** (0.5520)
Time Trend for Credibility of Threats Against Democratic Leaders $f(t_i)$(C*W*T)	0.0374*** (0.0114)
Economic Growth (lag)	-0.5646** (0.2265)
Log of Trade (lag)	-0.0132 (0.0245)
Intensity of Civil Conflict	0.1157** (0.0527)
Logged Aid Dependence	0.1286 (0.0989)

Table 7.2 Continued

Time Trend for Logged Aid Dependence	-0.0148 (0.0129)
Logged Aid Dependence of Democratic States	-0.4303*** (0.1389)
Time Trend for Logged Aid Dependence of Democratic States	0.0568*** (0.0201)
Constant	-0.3687 (0.2652)
Wald $\chi 2$	294.55
Pseudo R^2	.0970
Expected Percent Correctly Predicted	80.64%

NOTE: N=3,783 leader years from 1971-1999. Robust standard errors, in parentheses, clustered on 651 individual leaders. Coefficients for regional dummies (South America, Sub-Africa and South Asia) not reported here.

* significant at 10%; ** significant at 5%; *** significant at 1%

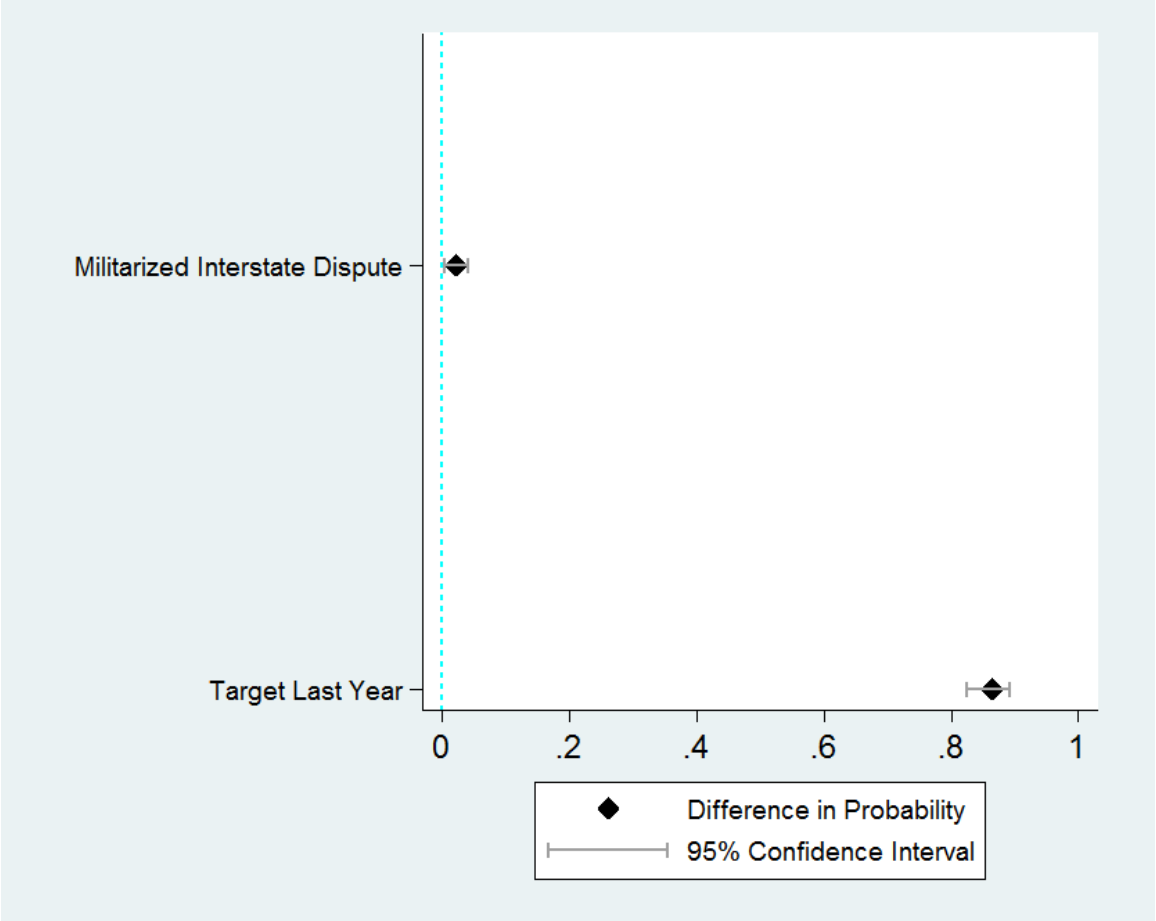

Figure 7.1 Difference in Probability of Sanction Targeting by Discrete Variables

NOTE: Reported statistic is the median predicted change in the probability of sanctioning given indicated variable obtains versus not from a simulated distribution of 1,000 draws from the variance-covariance matrix of the probit regression reported in Table 7.1. Confidence intervals were constructed on the basis of the same simulated distribution.

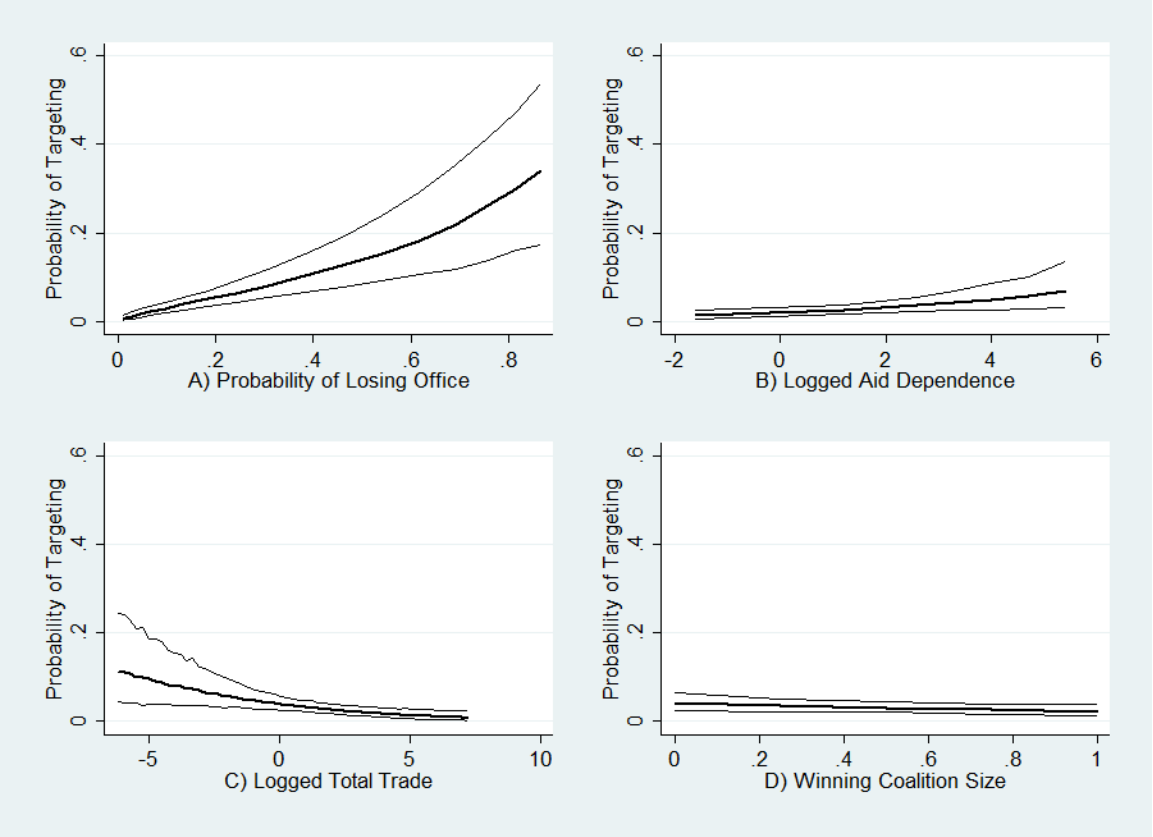

Figure 7.2 Probability of Sanction Targeting Over Range of Continuous Variables

NOTE: Probabilities calculated using 1,000 draws from the covariance matrix of the bootstrap results displayed in Table 7.1. Dashed lines indicate 95% confidence intervals around the median calculation from that sample. Probabilities calculated over full range of each variable of interest while all continuous controls were held at means and dummies at modal values. The lagged dependent variable is switched off during all calculations, as its power soaks up all of the impact. Its influence is charted in Figure 7.1.

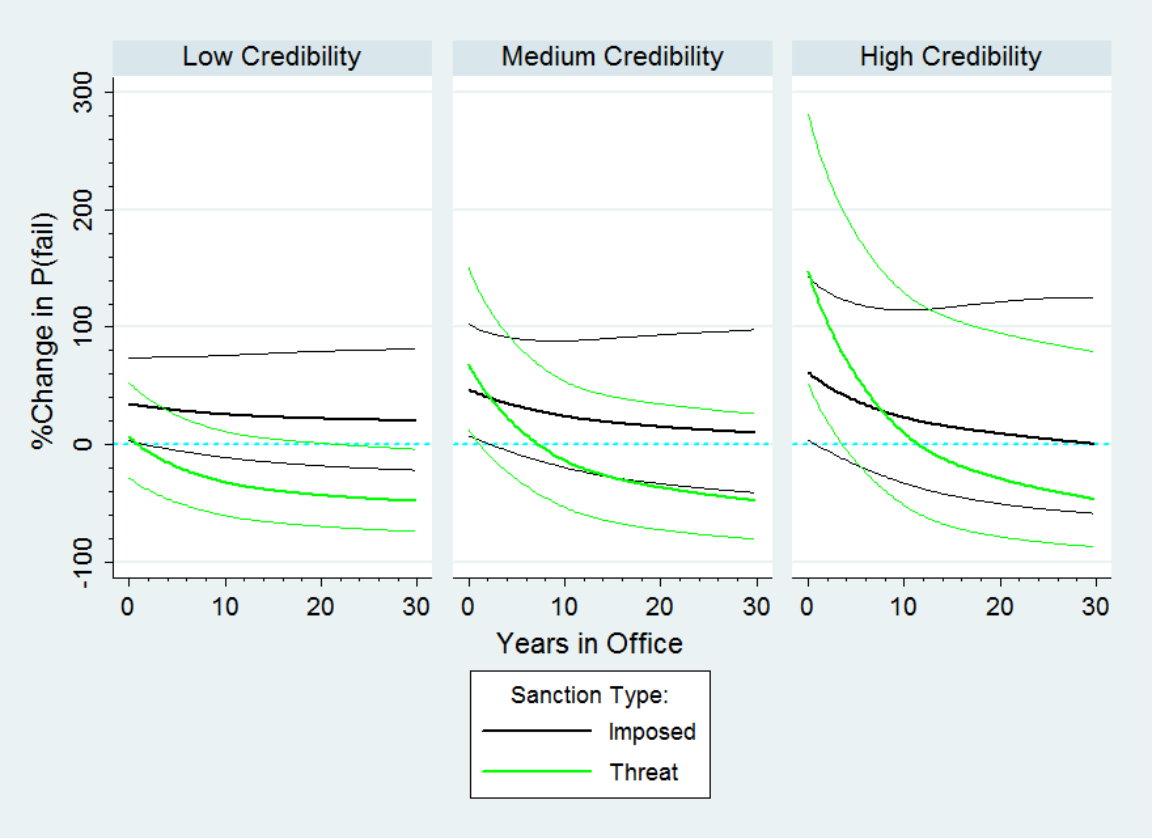

Figure 7.3 Change in Probability of Nondemocratic Leader Failure by Sanction Type

NOTE: Reported statistic is the median calculated change in probability from a simulated distribution of 1,000 draws from the variance-covariance matrix of the estimates reported in Table 7.2. Thin lines mark 90% confidence intervals based on that distribution. Effects are significant when confidence intervals do not include zero. The change is calculated the difference between the probability of failure given sanctions occur and the probability of failure were no sanctions to occur. This difference is then adjusted to create a percentage change. Bright blue dashed line indicates the zero line.

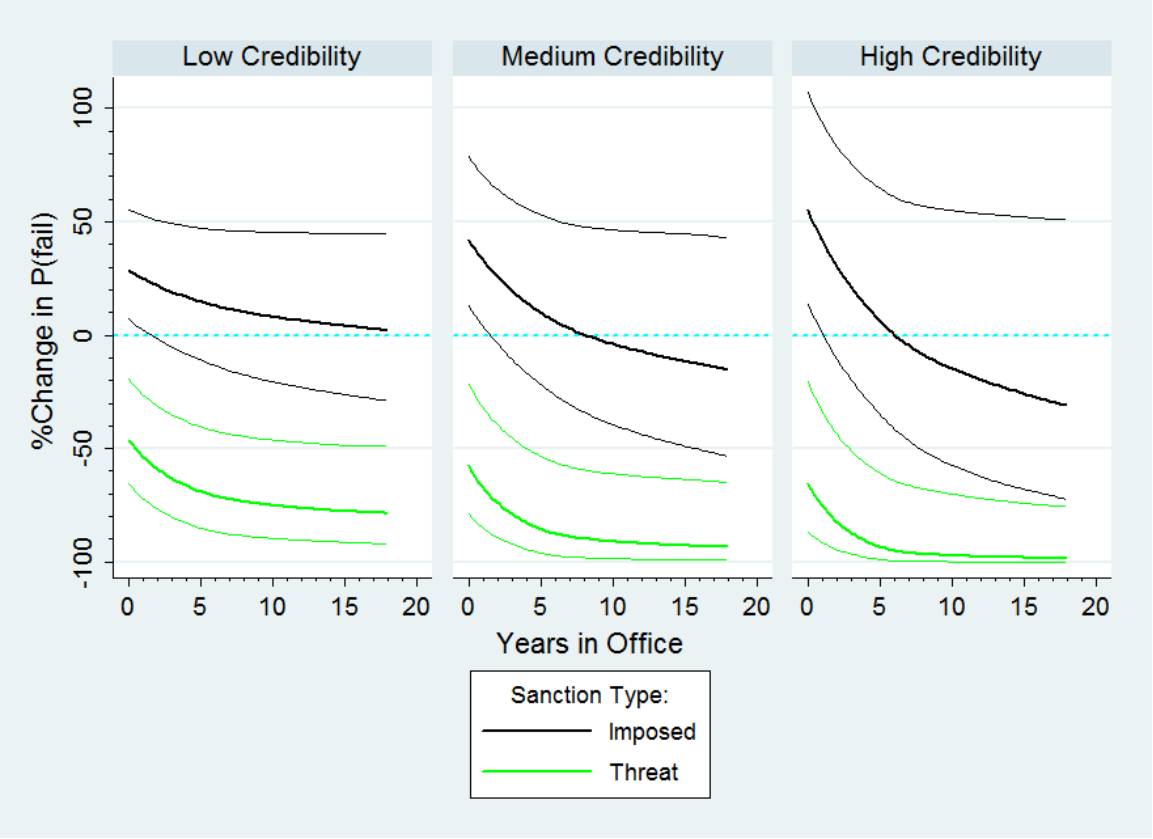

Figure 7.4 Difference in Probability of Democratic Leader Failure by Sanction Type

NOTE: Reported statistic is the median calculated change in probability from a simulated distribution of 1,000 draws from the variance-covariance matrix of the estimates reported in Table 7.2. Thin lines mark 90% confidence intervals based on that distribution. Effects are significant when confidence intervals do not include zero. The change is calculated the difference between the probability of failure given sanctions occur and the probability of failure were no sanctions to occur. This difference is then adjusted to create a percentage change.

Table 7.3 Difference of Proportions of Business-Based Sanctions by Regime Type

Democratic System		***Targeting Focuses on Business Interests*** *No*	*Yes*	*Total*
	No	74.41% (474)	25.59% (163)	100% (637)
	Yes	31.57% (359)	68.43% (778)	100% (1137)
	Total	46.96% (833)	53.04% (941)	100% (1774)

NOTE: Percentages calculated by row with raw frequencies in parentheses. Test refers to proportion of business-based threats or sanctions conditional on a sanction or threat being issued.

Difference of proportions test = -.4284, $p<.00001$

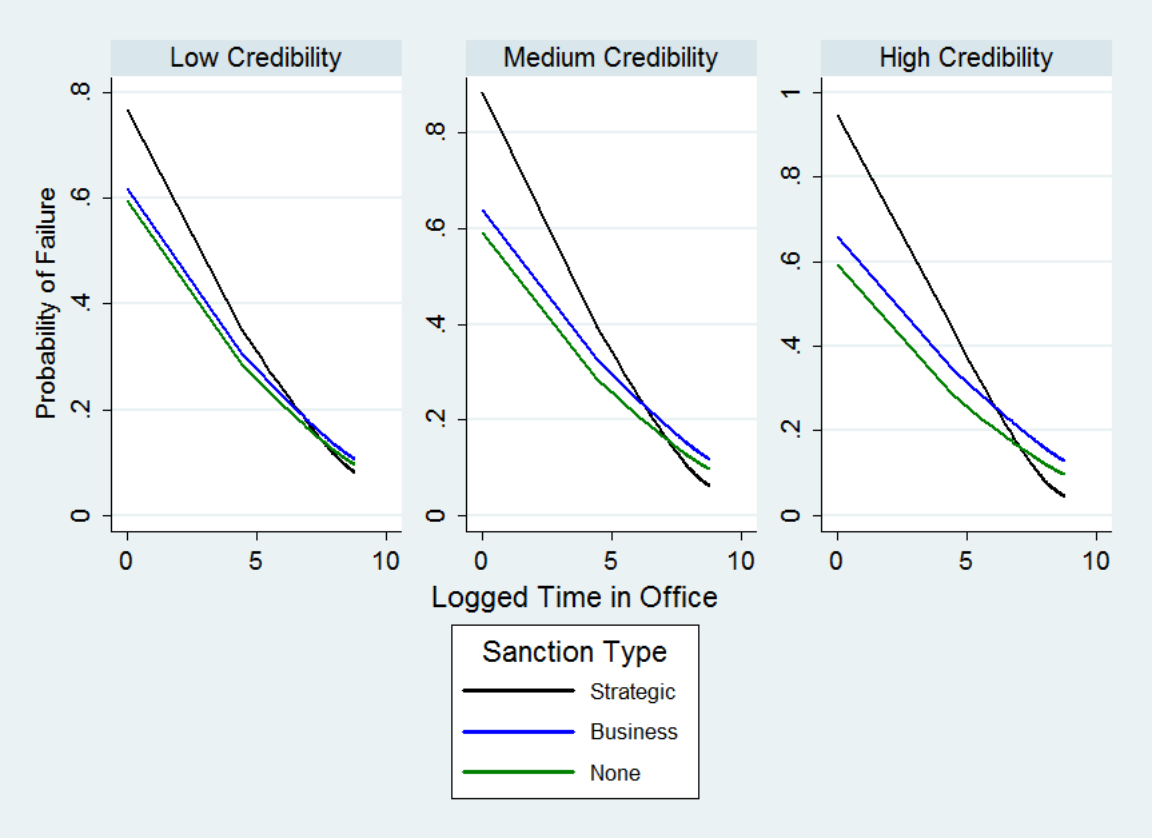

Figure 7.5 Probability of Democratic Leader Failure by Type of Imposed Sanction

NOTE: Charted curves are median predicted probabilities of failure based on simulated distribution of 1,000 draws from the variance-covariance matrix of a probit regression reported in Table A7.1. Confidence intervals are omitted in order to reduce clutter. At the 90% level, confidence bounds for business and none overlap for the majority of observation time. For a subset of time, the confidence interval for other also overlaps.

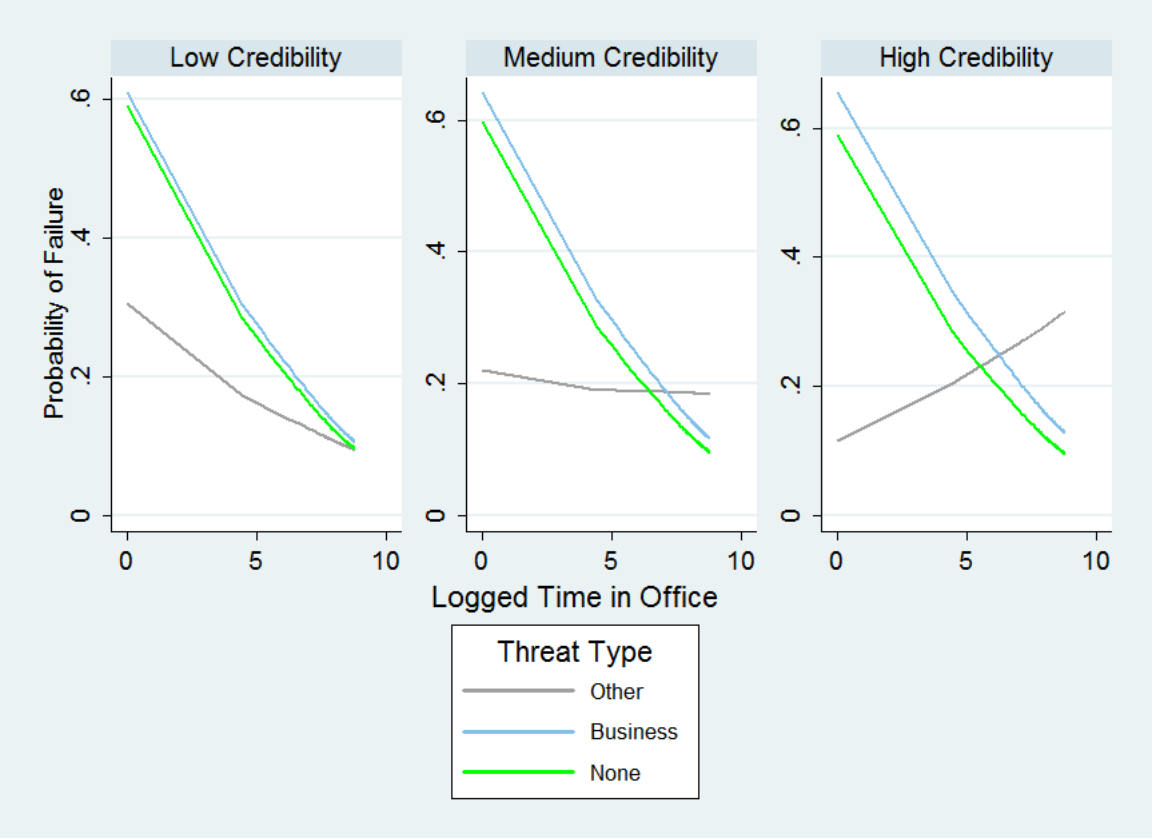

Figure 7.6 Probability of Democratic Leader Failure by Type of Threatened Sanction

NOTE: Charted curves are median predicted probabilities of failure based on simulated distribution of 1,000 draws from the variance-covariance matrix of a probit regression reported in Table A7.1. Confidence intervals are omitted in order to reduce clutter. At the 90% level, confidence bounds for business and none overlap for the majority of observation time. For a subset of time, the confidence interval for other also overlaps.

Table 7.4 Probit of Leader Failure by Strategic-Dispute Sanction Targeting

Sanction (S)	0.0823 (0.1418)
Threat (T)	-0.1302 (0.4779)
Credibility of Threat or Sanction (C)	0.2506* (0.1404)
Log of Leader's Cumulative Time in Office (days) $f(t_i)$	-0.1700*** (0.0329)
Democratic System (W)	0.5032*** (0.1105)
Credibility of Sanctions Against Nondemocratic Leaders (C*S)	0.8770 (0.5387)
Time Trend for Credibility of Sanctions Against Nondemocratic Leaders $f(t_i)$(C*S)	-0.1371* (0.0700)
Credibility of Sanctions Against Democratic Leaders (C*W*S)	-0.4250 (0.7506)
Time Trend for Credibility of Sanctions Against Democratic Leaders $f(t_i)$(C*W*S)	0.0279 (0.1140)
Credibility of Threats Against Nondemocratic Leaders (C*T)	-1.2016 (0.8446)
Time Trend for Credibility of Threats Against Nondemocratic Leaders $f(t_i)$(C*T)	0.1574* (0.0851)
Credibility of Threats Against Democratic Leaders (C*W*T)	0.3928 (0.3334)
Time Trend for Credibility of Threats Against Democratic Leaders $f(t_i)$(C*W*T)	-0.0019 (0.0036)
Economic Growth (lag)	-0.5847** (0.2280)
Log of Trade (lag)	-0.0206 (0.0233)
Intensity of Civil Conflict	0.1000* (0.0519)
Logged Aid Dependence	0.1182 (0.0963)

Table 7.4 Continued

Time Trend for Logged Aid Dependence	-0.0141 (0.0125)
Logged Aid Dependence of Democratic States	-0.4409*** (0.1378)
Time Trend for Logged Aid Dependence of Democratic States	0.0587*** (0.0200)
Constant	-0.2701 (0.2526)
Wald $\chi 2$	264.66
Pseudo R^2	.0935
Expected Percent Correctly Predicted	80.56%

NOTE: N=3,783 leader years from 1971-1999. Robust standard errors, in parentheses, clustered on 651 individual leaders. Coefficients for regional dummies (South America, Sub-Africa and South Asia) not reported here. Sanctions and threats which target only business interests are not coded as sanctions or threats in this model.

* significant at 10%; ** significant at 5%; *** significant at 1%

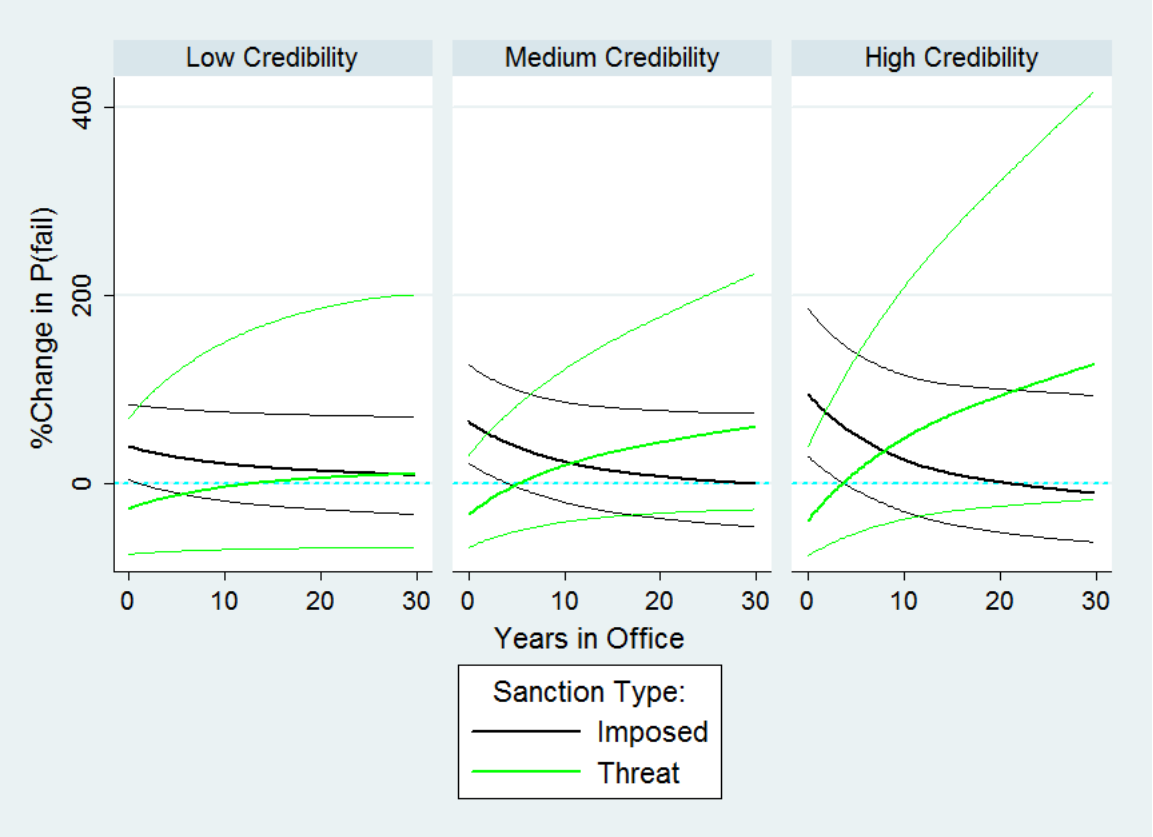

Figure 7.7 Difference in Probability of Nondemocratic Leader Failure by Strategic Sanctions

NOTE: Reported statistic is median predicted percentage change in probability of failure from simulated distribution of 1,000 draws from the variance-covariance matrix of the probit regression reported in Table 7.4. Thin lines demarcate 90% confidence intervals based on that distribution.

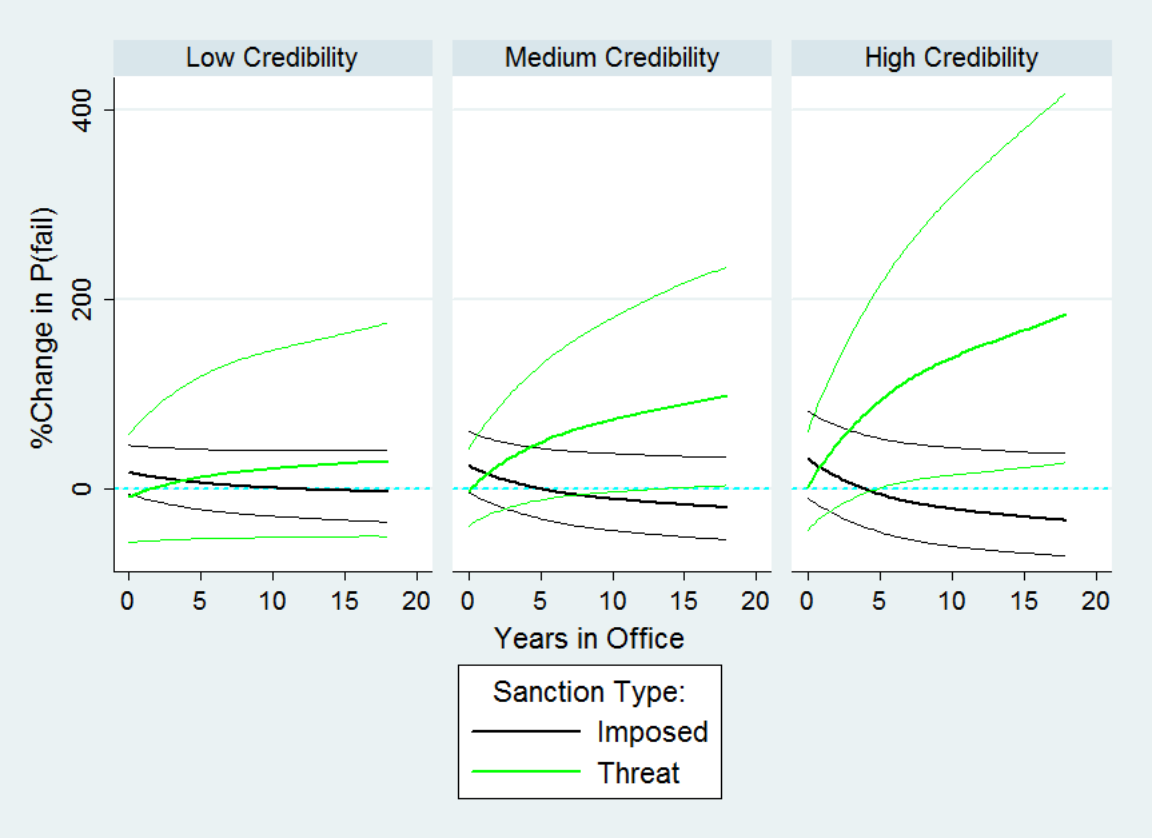

Figure 7.8 Difference in Probability of Democratic Leader Failure by Strategic Sanctions

NOTE: Reported statistic is median predicted percentage change in probability of failure from simulated distribution of 1,000 draws from the variance-covariance matrix of the probit regression reported in Table 7.4. Thin lines demarcate 90% confidence intervals based on that distribution.

Table 7.5 Summary of Results Regarding Effectiveness of Sanctions and Threats

	Preferred Model (Table 7.2)	Comparison Model	No Trade Model (Table 7.4)
Definition of Sanctions?	All	Non-trade based	Non-trade based
Comparison Category?	Non-sanctioned leader years	Non-sanctioned leader years	Trade-based and non-sanctioned leader years
Sanctions against Democratic Leaders			
Max Change in Chance of Losing Office?	49%	60%	None
Significant During what Years of Tenure?	< 2 years	< .2 years	None
Sanctions against Nondemocratic Leaders			
Max Change in Chance of Losing Office?	76%	76%	105%
Significant During what Years of Tenure?	< 1.33 years	< 2 years	≥ 3 years
Threats against Democratic Leaders			
Max Change in Chance of Losing Office?	-99%	224%	100%
Significant During what Years of Tenure?	≤ 50 years	≥ 2.3 years	≥ 5 years
Threats against Nondemocratic Leaders			
Max Change in Chance of Losing Office?	161%	None	None
Significant During what Years of Tenure?	< 3 years	None	None
Support for learning (H1a) or winning coalition cohesion (H1b)?	Learning	Both	Both
Support for H2, more effective against democrats?	No	Mixed	No
Support for H3, more effective when imposed?	Mixed	Mixed	Mixed

NOTE: Figures reported are maximum estimated median values which are significant at the 90% level.

* This magnitude of destabilization estimated at the 18th year in office, which is the 95th percentile of democratic leader survival.

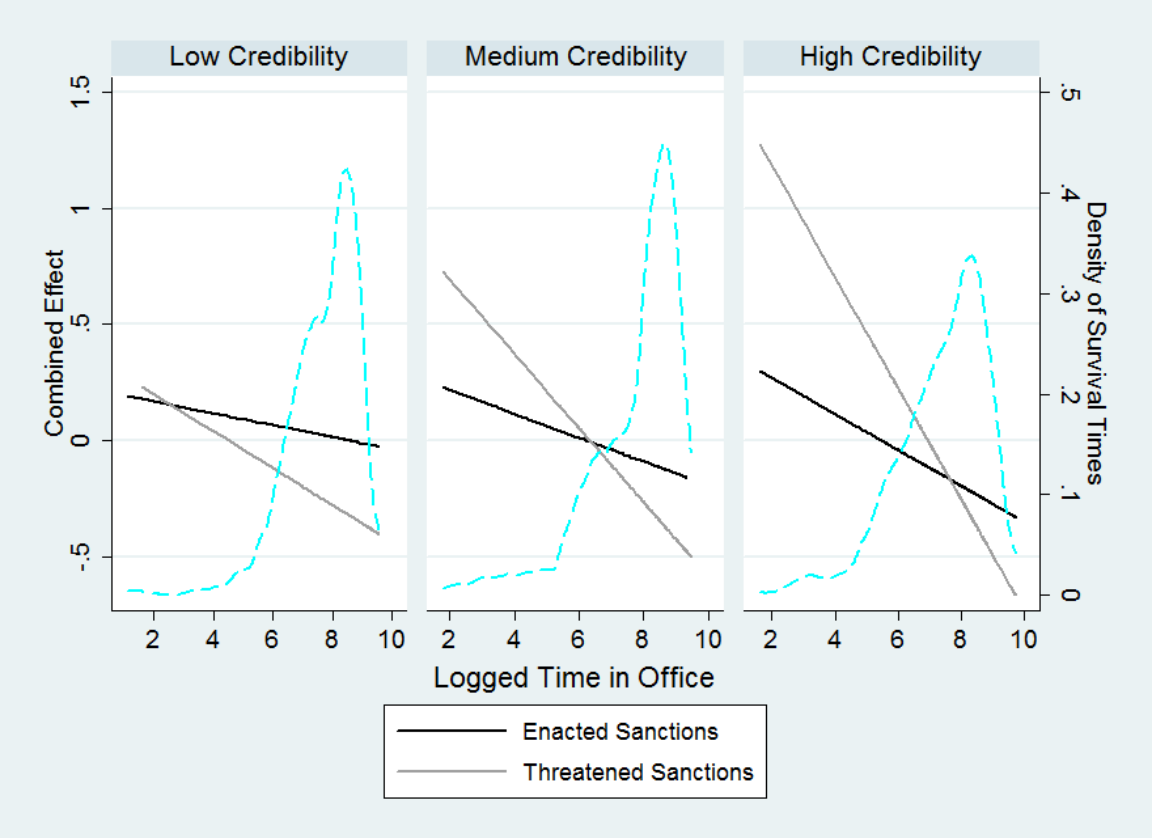

Figure 7.9 Range and Trend of Effectiveness Measures for Nondemocratic Leaders

NOTE: The figure charts range of across observed values for nondemocratic leaders targeted with observed type of sanction in the dataset of sanctioned-leader years which will be employed in Chapter 8's analysis of sanction outcomes.

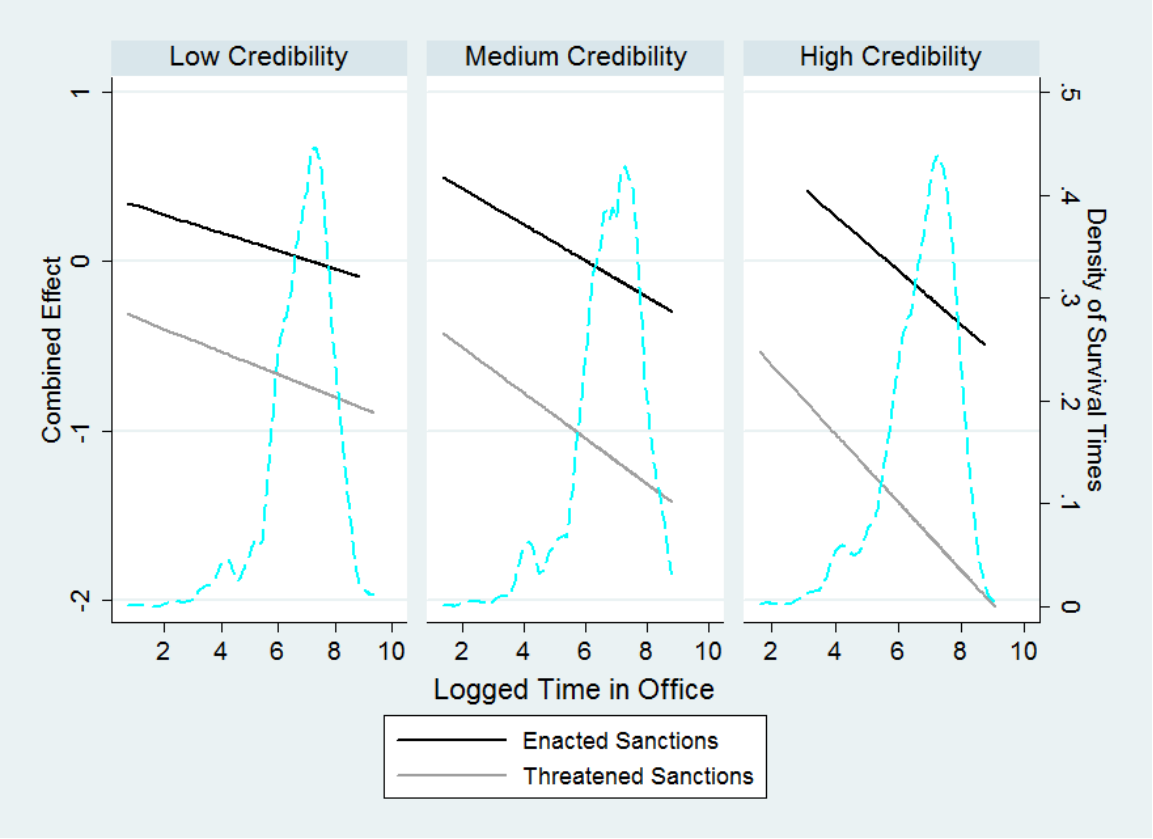

Figure 7.10 Trend and Range of Effectiveness Measures for Democratic Leaders

NOTE: The figure charts range of across observed values for democratic leaders targeted with observed type of sanction in the dataset of sanctioned-leader years which will be employed in Chapter 8's analysis of sanction outcomes.

CHAPTER 8

FEELING THE SQUEEZE: DOMESTIC AND EXTERNAL PRESSURES' ROLE IN THE OUTCOME OF ECONOMIC SANCTIONS

The results presented in Chapter 7 bear witness to the complex and dynamic nature of economic sanctions episodes. The effectiveness of this policy tool wavers given not just the type of targeted leader, but also the experience level of that leader; not just by the overlap of the cost-bearing population with the winning coalition, but by the credibility of the senders' demands and whether the negative sanction progresses from threat to punishment. This wide array of factors affecting the ability of sanctions to destabilize target leaders meshes well with extant findings regarding the low success rate of economic sanctions. If effectiveness marks the first step in achieving influence, then most economic sanctions open with a major disadvantage.

But even those sanctions which manage to destabilize leaders face another barrier to overall success. Giving in to concession itself can create additional political costs for targeted leaders. These potential costs represent the second intersection of foreign policy tools with target incentives. Below I will review the basic argument of the domestic hurdles to concession and specify hypotheses more fitted to the case of economic sanctions. Then, I will outline the competing-risks framework which will allow evaluation of concessionary behavior.

Across regime types, these tests reveal interesting but unexpected results. Domestic politics definitely matters. Likelihood of successful external influence declines when demanded concessions promise to excite powerful domestic groups. Denying the argument for entrepreneurial protection of public interests, however, the presence of a strong opposition actually exacerbates this inequitable treatment. Targeted leaders' experience in office also contributes to likelihood of concession. Even if the sanction (threat) backfires, leaders tend to offer concessions when they are most secure in office.

For democratic leaders, this means early; for autocrats, late. The willingness of these leaders to offer concessions for ineffective sanctions suggests that some kind of issue-linkage or tradeoff takes place between domestic costs and international gains when leaders have plenty of security to spare.

Domestic Hurdles and Sanction Outcomes

While sanctions may, like a slowly lowering ceiling, exert pressure on targeted leaders, we should not forget that the floor may also be rising. Leaders feel the squeeze from both directions. All else constant, an effective sanction should win more concessions than an ineffective sanction. But all else is not constant. Concessions to external forces produce costs for domestic groups. Sometimes these costs diffuse equally across the public, but often they will cluster to some extent upon a small group. In cases when the costs of concession will be born privately or by both focused groups and the general public, that rising floor may be studded with nails. Leaders could concede in the face of hurting sanctions, only to be ousted by outraged domestic forces. This risk generates the domestic political difficulty of concessions, which will vary by the type of concession demanded, the strength and presence of a political opposition, and the experience of the targeted leader.

In Chapter 3 I summarized the nature of difficulty (D) as:

$$D = \gamma_1 C_{private} + \gamma_2 C_{mixed} + \gamma_3 C_{public} + (\gamma_4(O \times C_{mixed}) + \gamma_5(O \times C_{public})) \times f(t_i) \qquad \text{(Eq. 8.1).}$$

The **C** variables in this function represent three possible types of cost to concession, private, mixed and public. The essential difference between these types is the extent to which costs would focus on a certain subset of the population. I focus on this element of the costs to concession, because it should relate most directly to the likelihood of political mobilization, and thus to leader's incentives. Collective action itself is expensive, so its occurrence depends on both the willingness and opportunity of the affected population to act. Overcoming the cost barrier is simplest when each member of a small group would

suffer a noticeable cost (Olson 1971). Even within democracies domestic groups possess varying efficacy. While anybody can feel outrage, doing something about that feeling is a luxury item. The likelihood of organized political backlash, then, should be increasing with the element of public costs, such that $\gamma_3 < \gamma_2 \leq \gamma_1$.

Even if a concession will cost every citizen a tiny slice of dignity or income, the likelihood of punishment is fairly low unless a core of individuals exists whose preferences include a high proclivity for punishing the powers that be. In Chapter 3 I posited that a strong, legalized political opposition represents just such a core of "entrepreneurs" willing to take up public goods problems in the interests of overthrowing the existing leadership. The interaction terms in Equation 8.1 represent the potential for the opposition to act in this capacity. If the "losing coalition" takes action on behalf of public interests, then the parameters for the interaction effects should be positive, indicating an increase in the difficulty of betraying public domestic interests. An extremely weak or nonexistent opposition cannot disrupt the incumbent, because they do not present a viable political alternative. Weak oppositions may exist if institutions of representation provide no legal precedent for their operation, or if the organized parties performed poorly in recent elections. To summarize: if $O>0$ then, $\gamma_5 \geq \gamma_4 > 0$.

The experience of leaders in office will further condition the ability of the opposition to affect difficulty either in accordance with the incumbents' popularity or their capacity. This caveat is represented in Equation 8.2 by the interaction with $f(t_i)$. Strength itself may not be able to combat the legitimacy of a newly elected democratic leader or firmly entrenched nondemocratic leader. Winning coalition cohesion would produce differential time dynamics across regime type, with opposition efficacy increasing over time in democratic systems and decreasing in nondemocratic systems. Over time, executives develop expertise, skills and connections which may allow them to offset the costs produced by conceding to external demands. These skilled leaders can offer opposition parties side payments or spin the situation to pin continued sanction

costs on obstructionist politicians. If this developing expertise, rather than popularity, drives temporal effects, then the efficacy of the opposition should be decreasing over time across regime types.

The difficulty of providing concessions in the face of external threats and punishments, then, will vary by: (1) the portion of the population likely to be hurt by the change in policy, (2) the strength of the opposition, and (3) the course of the targeted leader's career. In hypothesis form, these conclusions translate to three hypotheses (restated from Chapter 3):

H1: Concessions which would impose public costs pose less domestic political difficulty than would those imposing private or mixed costs.

H2: A strong opposition, conditioned by leader experience, increases the likely domestic political difficulty posed by concessions which would create public or mixed costs.

H3a: The difficulty of concessions increases over time for democratic leaders and decreases over time for nondemocratic leaders.

H3b: The difficulty of concessions decreases as leaders gain experience.

These expectations regarding the burdens of domestic politics are not directly investigated, as they would require substantial data collection. Rather, the extrapolated impact of these factors on the outcome of sanction events will be examined. The principle claims tested below are the following:

H4: Concession becomes more likely as sanctions' effectiveness increases.

H5: Concession becomes less likely as the costs of concession focus on specific groups.

H6: The strength of the opposition, conditioned by leader experience, decreases the likelihood of concessions with public-costs elements relative to those with private-costs elements.

H6b: The ability of the opposition to affect concession decreases over time for nondemocratic leaders and increases for democratic leaders.

H6c: The ability of the opposition to affect concession decreases as leaders become more experienced.

Obtaining a test of such complex dynamics requires highly conditional models, the specification of which will be briefly reviewed below. For a more detailed discussion, please see Chapter 4.

Research Design Review

These expectations derived from my general theory of sanctions as tools of influence will be tested below using a competing risks framework. This set-up acknowledges sanctions episodes as processes which unfold over time with uncertain ending points. After the targeting of a sanction, each passing unit of time may result in either further sanctioning or some manner of resolution. I categorize sanction terminations in terms of how successful the sender state appears to have been in exerting influence. Sanctions can end with the sender winning, the sender losing, or a "draw". The sender wins when the target state acquiesces at least partially to the demands, and loses when forced to capitulate by a resolved target. Sometimes, the parties agree to negotiate a less one-sided settlement, and I refer to these cases as "draws", although this may not accurately reflect the ultimate terms of the agreement.[79] Ostensibly, however, the TIES coding suggests that the difference between a "win" and a negotiated settlement is that both parties give a little rather than the target simply providing concessions.

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[79] Because the written case studies from which the TIES data were coded have not been made publically available, the details of these settlements remain unknown. For example, I do not know whether they are bilateral negotiations; involve third-party mediation; or employ some manner of binding adjudication. These negotiated settlements may produce equitable decisions, or decisions slanted in favor of either party. TIES offers a set of variables which code the outcome in terms of the degree to which it favors one party over another. But these variables are very subjective and contain more missing observations than the simpler variable used to construct my dependent variables.

Tables of statistical results will be divided by three equations, one for each of the potential outcomes (win, lose, draw). In each, the outcome at hand is modeled against continued duration. The coefficients relate the effect of the covariate on bringing about the current outcome as opposed to further duration of the sanction episode. For purposes of evaluating influence, the "sender wins" model carries the most weight. Recall from Chapter 4's discussion of model specification that all equations will take the following form:

$$P(W|\boldsymbol{X}) = \Phi\big(\alpha + \beta_1 \widehat{E^t}_{it} - \beta_2 \widehat{\mathrm{E^s}}_{\mathrm{it}} - \beta_3 \mathrm{T} - \beta_4 \mathrm{S} - \beta_5 \mathrm{C}_{\mathrm{it}} - \beta_6 (O_{it} \times C_{it}) + \beta_7 t_i (O_{it} \times C_{it}) + \boldsymbol{\beta D} + \mathbf{\beta Z} + \epsilon\big) \quad \text{(Eq 8.1)}$$

Where *W* is an indicator variable coded 1 in the year in which a target acquiesces (partially or completely), 0 for years in which the episode endures, and missing in the year in which an alternative termination type obtains; $\hat{E}_i$indicates the effectiveness measure created at the close of Chapter 7, with superscripts dividing imposed sanctions from threatened sanctions; *T* is an indicator variable for cases which terminate at the threat stage; *S* is an indicator which switches on the year that a threat becomes an imposed sanction and remains on until termination; *C* is a nominal, trichotomous variable taking values 1-3 for private, mixed, and public costs-inducing concessions respectively; *O* is the strength of the opposition, measured as the opposition share of the most recent popular elections in democratic systems and as the existence of a legislative branch in nondemocratic systems; *t* is the natural log of a leaders cumulative time in office; and ***D*** is the cubic polynomial of sanction episode duration, and the vector **Z** contains all remaining control variables.

I code the nature of concession costs by the issue over which the sanctions episode emerged. Broadly, if the issue under contention involves mostly symbolic issues or national security issues, it is considered that concessions would produce public costs. As the issue turns towards the behavior of private industries or interests, I code the likely concession costs as mixed and then private. More specific information about the coding

scheme can be found in Chapter 4. Across regime types, most sanction demands suggest private concessions costs. For democratic targets, this category constitutes nearly 82% of the sample, compared to 66% for nondemocratic targets. Table 8.1 charts the cost scale against the frequency of sender victory. Though smaller in raw number, mixed- and public-costs concession types feature considerably larger proportions of victories for sending states. Winning is more than triply likely when seeking public- rather than private-costs concessions.

The inability to measure domestic opposition equivalently across regime types and the separate measures of effectiveness for big and small winning coalitions leads me to estimate separate models for these groups. To account for the use of estimated instruments for the effectiveness of economic sanctions, I employ a bootstrapping procedure as in the prior chapters. Below, results will be displayed for big winning coalition systems prior to discussion of less democratic systems. This separate analysis raises no problems for the majority of the hypotheses. H6b, however, explicitly refers to patterns across rather than within regime types. While the individual analyses will speak to the relative strength of institutionalization versus learning, definite conclusions must wait until both have been completed.

The dataset of sanctioned democratic leaders contains 3,566 observations of sanctioned leader years by episode from 1971-1999. At the last observation point, 2,157 of these leader-episode observations continued without resolution. Of those cases which do reach conclusion, 32% are sender victories, 38% losses and 27% negotiated settlements. For nondemocratic leaders the sample is smaller, containing 1,147 sanctioned-leader years with 506 right-censored observations with no resolution. The

spread of termination types is fairly similar, with 39% victories, 28% losses and 23% draws.[80]

The formal literature on economic sanctions has argued that a selection problem plagues many extant studies of sanction efficacy (Schwebach 2000; Lacy and Niou 2004; Drezner 1999, 2006). The standard claim holds that strategic, rational actors facing costly punishment will surrender at the threat stage, producing a sample of imposed sanctions systematically less likely to be successful. The TIES dataset, which identifies the threat stage of the sanction process as well as the date at which threats become imposed punishments, allows modeling of what was once an unobservable, confounding factor for statistical analysis. Indeed, the proportion of sender victories declines by 40% when sanctions against democratic leaders transition from threat to punishment. A similarly significant pattern holds for sanctions against nondemocratic leaders, though the overall proportion of wins is higher, at 4.1% of observations (difference in proportions= .0264, p=.029). Table 8.2 displays the significant trend of fewer victories following imposition across the pooled sample of leaders (p=.016).

This dynamic suggests that much of the influence process can be observed in the run up to sanction imposition. To account for this potential, I also model the likelihood of sanction imposition, using the indicator variable *S* discussed above as the dependent variable. For democratic leaders, the imposition model includes 3,540 sanctioned=leader years from 1971-1999 with 1,360 leader-sanction episodes. Of these cases, 54% become imposed and 46% terminate at the threat stage. In the nondemocratic sample, 61% of 1,147 leader years escalate to enacted sanctions and 39% ending with threats. This set

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[80] An additional 10% of nondemocratic termination types are officially determined by the TIES investigators as "ongoing" despite the end of the episode. This occured when a new issue was introduced in the face of stalemate. For democratic termination types only 3% of cases fall in this category. For the competing risks framework, these cases are treated simply as ongoing.

contains 476 separate leader-sanction episodes. I began by including all of the above postulated effects in the imposition model and pared it down by removing the non-significant covariates to avoid including all of the same parameters as in the final outcome stage. This procedure creates some differences between the models for democratic and nondemocratic leaders. Specifically, in the democratic leader sample, opposition strength and its interactions with the likely type of concession costs fail to significantly affect the likelihood of imposition; this is not the case when examining the impact of a legislative branch in the nondemocratic-target models. Finally, I estimate the competing risks probit models and the imposition equations simultaneously using a bivariate normal distribution. With these key elements of the design and data fresh in mind, we now proceed to evaluate the results for the competing risks probit of sanction imposition and outcome across democratic and nondemocratic leaders.

Empirical Results: Sanctions against Democratic Leaders

Before embarking on the simultaneous models, consider the model of sanction imposition. Table 8.3 contains the estimates and fit-statistics for this probit regression, which includes effectiveness measures, cubic polynomials of leader's experience in office and episode duration to model potentially nonmonotonic temporal dynamics, and a host of control variables. The probit model fits quite well, correctly categorizing 79% of cases according to the expected percent correctly predicted. The model's predictions reduce expected error in categorization by 58%.

This model includes measures of effectiveness for both threats and sanctions. Prior to imposition, of course, the sanction effectiveness measure cannot refer to an actual impact on target leader survival. Rather, it provides an interesting measure of how the target would be likely to suffer/benefit should the issued threat be converted to actual punishment. The large, negative, significant coefficient for this "expected" effectiveness indicates that sanctions which would destabilize targets make imposition less likely,

while those which insulate leaders increase the chances of enactment. When sanctions may be effective in destabilizing democrats, they are less likely to progress from the threat stage to imposition. If they are not likely to hurt, however, they become more likely to be issued. This pattern coincides with the selection pattern suggested by game theoretic models.

The effectiveness of threats, however, does not appear to mesh with expectations. The positive coefficient for threat effectiveness suggests ineffective threats will *drive down* the likelihood of imposition. Though imposition does not bode well for the chances of sender victory (see Table 8.2), the success rate for episodes which terminate at the threat stage falls far from perfect. About 46% of cases ending prior to imposition do so as a result of sender capitulation. Ineffective threats may inform senders that their attempts to gain concessions through economic coercion hold little chance for success, leading them to abandon the effort before incurring any further costs. In the subsequent simultaneous models, we will be able to investigate this potential further.

Before moving on to the more complicated models, though, let us examine the importance of some additional dynamics for the imposition stage. Table 8.4 displays the relative size and significance of the change in probability of imposition given increases in a host of significant variables from the regression. For continuous variables, I estimated the predicted change in probability given an increase from the mean to the maximum value; for nominal variables, from the minimum to the maximum. The biggest shift in probability of imposition stems from the movement of threat effectiveness from its mean value of -.837 to its maximum of zero impact on leader survival. The increase in effectiveness produces a 250% jump in the likelihood of threats escalating to imposition.[81] A similar shift in expected sanction effectiveness produces a smaller raw

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[81] This figure derived by the calculation: (0.9406 - 0.2710)/0.2710=2.4712, where ..9406 is the probability of imposition at the maximum value of threat effectiveness and 2710 is the probability of imposition at the mean value of threat effectiveness.

change in probability, but the percentage decrease is huge. With all variables held at their means, an expectation that sanctions would prove mildly insulating produces a probability of imposition around .3278. The shift down to a probability of .0009 if instead the sanction were expected to increase the chances of failure by .502 constitutes a decrease of nearly 4,000% in the likelihood of imposition. The power of this influence, however, must be tempered by the knowledge that such an effective sanction would be aimed at a democratic leader in his/her first hours in power, with the highest possible credibility from the sender. Increasing tenure in office even by two days drops this prediction to a 210% decrease in likelihood of sanction imposition.

The anticipated economic, rather than political, costliness of sanctions also influences the sanctioning process. Boosting the likely costs of either party from mild to extreme produces an increase in the chance of imposition. These shifts in probability relate to 200% increases in imposition probability for both types of costs. This finding is indicative of some arguments in the literature which predict that higher saliency will lead to increased probability of escalation from the threat process and lower likelihood of concession (Drezner 1999; Drury and Li 2006). It may also coincide with Schwebach's (2000) claim that targeted states may not believe the sincerity of senders even when their threats constitute costly signals due to the "pooling" of resolved and unresolved actors.

The imposition model also presents evidence pertinent to a conflict in the extant literature. In one of the first attempts to model the imposition stage of sanctioning, Nooruddin (2002) found that democracy did not affect the probability of being targeted with imposed sanctions. Cox and Drury (2006) contend, however, that in a dynamic similar to that which keeps democracies from fighting each other, that more democratic targets should be significantly less likely. The TIES data demonstrates in raw numbers, that democratic targets are not only common, but more prevalent than nondemocratic

targets.[82] As targets become more representative and democratic, the probability of escalation from the threat stage declines by .1407, or 29%. These cases of early termination correspond to a much higher probability of concession, fitting better with Nooruddin's (2002) finding that democrats give in with higher probability. In cases where both the sending and targeted states have democratic institutions, however, the probability of imposition jumps by 40%. This meshes well with Cox and Drury's (2006) argument about jointly elevated audience costs producing harder cases, as those which escalate to imposed sanctions are less likely to be resolved in the sender's favor.

Increases in target power, in terms of both economic vitality and military might, drive down the likelihood of sanction imposition. The percentage drops in probability given higher economic growth or CINC-score are 85% and 39%, respectively. These findings also support Cox and Drury's (2006) model of imposition. They found that asymmetries in power make sanctioning more likely. As economic sanctions are generally tools of the wealthy, increasing power of the target indicates more symmetrical relationships. A final bit of evidence consistent with the Cox and Drury (2006) models comes from the increased probability of sanctions for targets with more extensive trading networks.

Estimating imposition and the outcome equations simultaneously will allow evaluation of more specific relationships between imposition, outcomes and effectiveness, including measures of substantive impact. The resulting bivariate probit regressions can be found in Tables 8.5-8.7.

For all three competing risks dependent variables (win, lose and draw), Wald tests of independent equations indicate positive, significant correlation between error terms. The imposition models, as should be expected, retain basically identical results across the

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[82] This fundamental difference in the distribution of sanctioned-regime types stems from TIES' inclusion of trade-based rather than strictly "high politics" sanctions.

three tables as were viewed in the independent test. Across the outcomes, however, the pattern of coefficient significance differs considerably.

In the outcome equations, only a small number of control variables ever achieved significance: anticipated sender costs, anticipated target costs, and economic growth. The effects of controls, when significant, seem intuitive enough. Higher costs of sanction for the target increase the likelihood of targets giving concessions, while higher costs of sanction to the sender increase the likelihood of senders giving up. Target economic growth, which is significant at the 90% level in the sender win equation, relates positively to victory. This makes sense in the context of Cox and Drury's (2006) argument that more symmetrical situations end more simply. To maintain the competing-risks frame, I included these three variables which achieved significant impact in at least one model in each of the three outcome equations.

While many of the key variables achieve statistical significance in the sender wins equation (Table 8.5), they consistently fail to affect the probability of sender loss or negotiated settlement. Clearly, draws feature more ambiguity regarding level of influence than do cases in which targets simply surrender. Negotiated settlements may constitute a face-saving mechanism for leaders of *either side* who are not resolved enough to endure the cost of sanctioning. The level of influence may still be quite high if the sender state manages to rope an unruly target into negotiations through an institution likely to favor the sender's position. This ambiguity makes predicting this type of outcome very difficult for my model. With the exception of sanction and threat effectiveness, the factors important for determining influence do not appear to play a role in the decision to compromise. Better information regarding the nature of the negotiation process may alleviate this issue in the future.

Similar, but more universal problems surface in the equations for sender capitulation. Factors inside the targeted state relate well to the decision of targets to provide concessions but tell us little about the tendency of sending states to withdraw

their claims. Explanation of this dynamic likely requires more detailed information regarding situations inside the sending state and possibly differentiation between uni-and multilateral sanctions. I consider the failure of my predictions to model the alternative outcomes interesting and fruitful ground for future endeavors, but I will not invest further space in their exposition here.

The highly conditional arguments in H1-3 dictate a model riddled with multiplicative terms. The role of different cost types is conditioned by opposition strength and by the trend of the targeted leaders' careers, necessitating an interaction between two continuous variables and one ordinal. The measures of $\hat{E}_i$, while not explicitly interacted with other covariates, are inherently conditioned by time and credibility due to their construction. The coefficients in Tables 8.5, therefore, can be of only limited assistance in terms of evaluating expectations. Rather than laboriously picking through this list of numbers, I will proceed by calculating measures of substantive significance which provide tests of the hypotheses. I will begin with Hypothesis 4, the role of sanction effectiveness on the ability of senders to influence targets.

Hypothesis 4: The Effectiveness of Threatened and Enacted Sanctions

The expectation that the effectiveness of foreign policy tools will play a central role in their ability to influence targets constitutes one of this dissertation's central claims. When sanctions hurt leaders more, the outcome of the episode should favor sending states, all else constant. The analysis in Chapter 7 provided estimates of sanction effectiveness over time and across levels of sender credibility. The dynamic trends in that analysis supported the learning hypothesis that all leaders increase in proficiency while on the job. Enacted economic sanctions destabilize leaders only for the first one to two years of office, while threats actually insulate most democratic leaders (see Figure

7.4). Higher levels of sender credibility amplify the impact of these negative foreign policy tools, whether effective or ineffective.

Those dynamics should resurface in the impact of effectiveness on imposition and sanction outcomes. Threats which effectively destabilize leaders should decrease the likelihood of imposition and increase the likelihood of sender victories; those which backfire should create stagnation or force sending states to retreat. In the face of hurting imposed sanctions, very inexperienced democratic leaders may prefer to offer concessions, but the window of effectiveness is narrow. The pattern should be most discernable at the highest level of credibility.

To assess the significance of effectiveness across time, credibility and sanction type, I begin by evaluating the substantive impact on imposition. Figures 8.1 displays the differences in probability of imposition produced by threat (in)effectiveness. To create these figures, I calculated a baseline probability of victory with the effectiveness measures restricted to zero then subtracted this from the predicted probability at "observed" levels of effectiveness. I located measures of confidence around this prediction by simulating a sampling distribution with 1,000 draws from the variance-covariance matrix of the bootstrapped estimates (Table 8.5). This endeavor reveals evidence contrary to my expectations.

The thin gray dashed line in Figure 8.1 demonstrates the value of the expected impact of sanctions on leader survival given target experience in office and sender credibility. Its negative slope marks the decrease in effectiveness as leaders accumulate experience. The dark gray lines chart the impact of this "expected" sanction effectiveness on the likelihood of imposition. The thick line, which indicates the median predicted change in probability of imposition, follows the zero-line across the bulk of time. It is significantly different from zero only when the targeted leader has held office for about one week. Against these extremely new targets, the expected impact of sanctions can reduce the likelihood of imposition by 200%. This extremely narrow

window of efficacy seems fitting given the largely insignificant impact of actualized sanctions on democratic leaders.

The bright green lines in Figure 8.2 show the change in probability attributable to the insulation effect of threats on democratic targets. The thick line gives the median predicted change, while the thin lines demarcate the 95% confidence interval around that quantity. The Y-axis gives the raw change in the probability of imposition. These small values correspond to very large relative changes as holding the value of effectiveness measures at zero produces extremely small probabilities of imposition. At low levels of credibility, the impact achieves significance across the full range of democratic experience, beginning with a 73% reduction and decreasing gently to an estimated 23% drop for extremely long-tenured democratic leaders. As the sender's commitment increases, the thin, green dashed curve marking the effectiveness of threats shifts downward, relating a decrease in effectiveness. As this occurs, the slope of the thick, green curve which indicates the difference in probability of imposition steepens. This steepening tells us that the impact of threat effectiveness approaches zero at a faster rate when senders signal higher commitment to their threats. At medium credibility, threat effectiveness wards off imposition for the first six years in office, reducing likelihood of sanctioning by 98% initially with a drop off to 14% just before losing significance. The highest credibility threats feature accelerated speeds, crossing into insignificance in just three years.

This ability of threat effectiveness to reduce the rate of imposition may be both good for sending states and bad for my theory. Threats of sanction against democratic leaders qualify as *ineffective* tools within my framework, as they help leaders who should be hurt. Yet, this ironic impact appears to increase the likelihood that the sanction process will terminate before the enactment of sanctions. Episodes concluded at the threat stage, tend to end in favor of the sending state (see Table 8.2).

Figure 8.2 confirms this counterintuitive result. Ineffective threats against democratic leaders increase the probability of the best type of victory for senders: one which does not even require imposing punishment. This graph charts the difference in probability of sender victory at the threat stage given observed levels of effectiveness rather than zero effectiveness. Again, these small shifts amount to extremely large relative increases due to the very small baseline probability of winning. At low credibility in the first year of office, the insulating impact of threats actually produces a 600% jump in the likelihood that senders will win concessions prior to imposing sanctions. Credibility inflates the magnitude of this increase to 1000% for moderately- and 5000% for highly-committed senders. These enormous effects are significant for leaders who have held office for 10 years or less, which includes the vast majority of democratic executives.

This puzzling finding may be driven by partial concessions and trade disputes. Democratic leaders who actually benefit from the external disapproval may be in a good position to offer some minor concessions in order to garner good will internationally. During their first years in office, the extant risk of losing office is very small and popularity is likely very high. At this time, targets may consider both the insulation potential of the standing threat and the costs of whatever concession is demanded relatively trivial. Over the course of their tenure democratic leaders' positions become more uneasy, and the risks-reducing potential of international threats increases in magnitude and instrumentality. When facing real risks, the insulation no longer appears trivial and leaders no longer offer concessions as a result of ineffective threats.

Though not shown here, I calculated similar measures of substantive and statistical significance for the impact of threat effectiveness on the alternative outcomes of sender loss and negotiated settlement at the threat stage. Effectiveness never significantly affects the likelihood that senders will capitulate prior to imposing sanctions. It does, however, trigger a massive uptick in the probability of negotiated

settlements during democrats' first year in office. Backfires in threat impact actually increase the likelihood of wins and draws without significantly altering the chances of losing vis-à-vis continued stagnation. This evidence presents a pattern far from that predicted in H4. When sanctions backfire, my theory predicts negative outcomes for sending states, but the predicted probabilities here suggest the opposite. After imposition, the results prove no more favorable. The (in)effectiveness of imposed sanctions produces no statistically significant changes in the probability of any of the three competing outcomes.

Hypotheses 5 and 6: Concession Costs and the Role of the Opposition over Time

Hypotheses 5 and 6 embody this project's second principle claim: the effectiveness of external pressure alone cannot explain target behavior. Domestic political barriers to concession seriously disrupt the causal line connecting punishment and acquiescence. These barriers, however, pose variable difficulty for the concession-minded target. When the cost-bearing group faces its own obstacles to action, such as poverty, apathy or lack of information – leaders may find it unnecessary to continue bearing the costs of external disapproval. The more centralized the likely pain of concession, however, the more powerful and likely the domestic backlash. Figures 8.3-8.5 examine this piece of the argument.

The thick lines in these figures track the median predicted difference in probability given a change from one type of concession cost to another. In these calculations, the probability of an event given the second listed concession type is subtracted from the probability given the first type. Then this difference is adjusted by the probability of the first concession type to create a percentage change.[83] Thin lines

[83] In the "Public vs. Private" panels, then, the charted percentage change is:

give the 90% confidence interval based on a simulated distribution of 1,000 draws from the variance-covariance matrix. For the calculation, all other variables, including the strength of the opposition, are held constant at means or modes as appropriate. The resulting graphs provide a test of the substantive and statistical importance of cost type independent of opposition strength. Since success rates increase in episodes settled at the threat stage, I will begin by considering the impact of cost type on the likelihood of imposition

The left-most panel of Figure 8.3 compares the probability of imposition across public-and private-costs inducing concessions. Providing concessions which damage private interests should always be more difficult, leading to a higher likelihood of threats going unanswered and senders resorting to actual sanctions. The picture here supports this story. The probability of imposition is 40% lower for public-costs issues than private at the beginning of a leader's tenure. The difference diminishes in magnitude over time, but not in significance. After five years, the public-costs-motivated threats are still 30% less likely to escalate to imposition.

Mixed-costs-generating concessions also tend towards settlement at the threat stage, though the difference, as plotted in the middle panel of Figure 8.3, is smaller. Disputes with new democratic leaders over mixed-costs-producing changes in policy will be 22% more likely to terminate with threats than were they likely to produce private costs. As hypothesized, this smaller difference is not random. Mixed costs are more similar to private costs than to public costs. A mixed-costs issue is up to 40% more likely to be imposed than a public-costs-motivated threat, as demonstrated by the strictly positive curve in the third panel of Figure 8.3.

$((\Pr(Y = 1|Cpublic) - \Pr(Y = 1|Cprivate))/\Pr(Y = 1|Cprivate)) \times 100.$

In the discussion of effectiveness above, the tendency for episodes which end at the threat stage to favor sending states was problematic for expectations. Here, such a finding, which is displayed in Figure 8.4, supports H5. Senders demanding public-costs concessions of new democratic leaders enjoy a 1000% greater likelihood of winning at the threat stage than those who demand private-costs-producing change. Within five years, this advantage reduces by half to a still impressive 400%. But an additional year of experience in the target and the effect no longer reaches significant levels. Mixed-costs concessions are also more likely to result in wins prior to imposition if aimed at relatively inexperienced leaders. As shown in the center panel of Figure 8.4, this difference begins at 220% and diminishes over time similarly to that for public versus private. Also consistent with my expectations, the curve for difference between probability of success in mixed- versus public-costs cases is negative. Within the first two years of a target's tenure, mixed-cost concessions are 63% less likely to be granted prior to imposition. Sanction episodes arising over public-costs-producing concessions, then, are both more likely to be settled prior to imposition and more likely to result in favorable outcomes for sending states.

Figure 8.5 presents the more difficult case, testing the relative likelihood of concession after the imposition of sanctions. Here, the same patterns hold, but with much briefer periods of significance. It seems that leaders who were willing to grant concessions of any kind tend to do so either immediately after enactment, or prior to the imposition of sanctions in the first place.

Figures 8.3 - 8.5 do not depict static relationships. Across all of these figures, the difference between types of concessions decreases in magnitude and significance over time. As leaders gain experience and their winning coalitions begin to crumble, the relative ease of sacrificing the public good shrinks and eventually disappears. At average levels of opposition strength, democratic leaders beyond their seventh year in power are

not significantly more likely to offer public-costs concessions than private-costs concessions.

As the opposition's strength increases, I predicted that the gap between costliness of public and private costs-producing concessions would shrink. Acting as entrepreneurs, the opposition should publicize and punish the decisions of incumbent leaders to provide concessions which alleviate their personal discomfort at the expense of the public. H6 contends that the viability of this opposition role will vary over time with either the institutionalization of the leader's winning coalition or the accumulation of the necessary expertise and experience to out-maneuver political opponents.

Figure 8.6 plots the percentage change in probability of sender victory given the change from public to private costs across three levels of opposition strength.[84] The right-hand panel holds opposition strength at one standard deviation below the mean (32%), the middle holds it at one standard deviation above the mean (57%), and the left-hand holds it at two standard deviations above the mean (69%).[85] The black curves in this graph depict the change in probability of sender victory following imposition; bright green, the change in probability of victory at the threat stage. Again, then lines indicate 90% confidence intervals and thick the median prediction based on a simulated distribution of 1,000 draws from the parameter matrices in Table 8.5.

The left-hand panel of Figure 8.6 charts the change in probability of concession by cost-type given a 32% opposition presence. At this low level of opposition, the difference cannot be distinguished from zero for either imposed or threatened sanctions; the confidence bands include zero for the duration of leader tenure. Unchallenged

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[84] The pattern described for public versus private costs in Figure 8.6 holds for mixed versus private, and mixed versus public costs as well.

[85] I do not plot the curves at the mean level of opposition strength here as the previous three figures held opposition strength at that level.

democratic leaders, this suggests, do not treat concessions differently. They appear equally willing to offer public- and private-costs producing changes.

Increasing the opposition presence to one standard deviation above average, as is displayed in the middle panel of Figure 8.6, shifts both curves upwards and tightens the confidence bands.[86] With a stronger than average opposition presence, then, the difference is very large and significant at about 1000% for threats and 220% for imposed sanctions. For the threat stage, this effect is double that at average levels of political competition. Continuing to an even more powerful opposition presence, in the final panel of Figure 8.6, the difference again increases in magnitude to nearly 1500% and 500% for threatened and imposed sanctions respectively.

As opposition strength increases, we see leaders treating public-costs concessions as *more* rather than *less* desirable compared to private-costs concessions. The difference decays over time, but perhaps more gradually than would be consistent with the institutionalization hypothesis. The significance of the difference between public- and private-costs concession likelihood in the face of imposed sanctions maintains significance for three years; at the threat stage, the difference endures for six to nine years depending on opposition strength. Were we to plot the predicted probabilities of concession by type of cost, rather than the differences examined above, we would observe that over time, the predicted probabilities of each type of concession converge to the same low value. More experienced leaders offer concessions with lower probability, irrespective of the type of costs they will produce. This functional form of the predicted probability curves may make it difficult to accurately observe how leader experience, cost-type and opposition strength intersect. To overcome this issue, I have plotted a

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[86] The difference in scale across y-axes may initially obscure this shift. But, forcing similar scales across panels creates an even more difficult figure to evaluate.

quantity of interest which is not influenced by the cumulative normal probability curve: the combined coefficient.

Figure 8.7 summarizes the opposing conditioning effects of time and opposition strength on the likelihood of senders winning concessions of different types. The black lines plot a smoothed curve of the median combined coefficient of opposition strength over time and cost types. [87] This curve utilizes the bottom x-axis and left-hand y-axis to plot the average effect of the opposition over time and cost-type. Moving from the left-most to the right-most panel, the type of concession cost becomes more privatized. As this happens, the median opposition strength curve shifts downward: each step away from public costs deflates the influence of the opposition. Within each panel, as we move from the origin to the right along the x-axis, the median curve exhibits negative slope. This indicates that the accumulation of leader experience also decreases the opposition's impact.

While time and focused costs diminish the opposition's impact on concessions, its strength pulls the effect in the opposite direction. I demonstrate this with the overlaid scatter plots across the three panels of Figure 8.7. Each dot in these plots marks the value of the median combined coefficient evaluated at a single combination of opposition strength and leader experience. Arrayed along the upper x-axis and right-hand y-axis, these dots display two interesting pieces of information. First, their positive slope moving from left to right within each panel illustrates that the impact of the opposition increases with strength. Second, the scatter plot shows the increased breadth of effect over time as the opposition gains power. In the public costs panel, for example, the

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[87] The combined coefficient was calculated using the following formula:

$$\beta_0 + \beta_{0\times C}\times C + \beta_{t\times 0\times C}\times C\times \ln(t)$$

To evaluate at particular values of opposition strength (in the scatter plots), this quantity was simply multiplied by the value of opposition strength.

combined effect of the opposition at its maximum strength ranges from nearly 2.5 on a leader's first day of office to just .52 during a leader's 30th year in office. This variance in impact over time decreases across the panels. The median effect of even a very powerful opposition on the probability of private-costs concessions ranges only from 0 - .53.

Overall, Figure 8.7 tells us that the role of the opposition matters most when public-costs concessions are on the table. The stronger the opposition, the bigger the push towards sacrificing public-interests in the early years of office and the wider the difference between experienced and inexperienced leaders. These patterns suggest that the opposition increases the likelihood that leaders will pawn their international punishments off on the public. This reading of the results refutes the hypothesis that opposition politicians will champion the interests of the public in order better to position themselves for future elections. Such a conclusion, however, may be too simplistic. The tests tell us that initially, when the opposition is strong, public costs concessions are massively more likely than private costs concessions. This aggregate finding does not necessarily imply that opposition parties collude with incumbents to provide external actors with concessions. Rather, the dynamics plotted here mesh with strategic behavior anticipating the tendency for democratic coalitions to deteriorate over time.

Without an effective opposition presence, sanctioned democratic leaders have relatively free reign to determine their level of concessionary behavior. They do not worry that a slight to an interest group will prompt defection to the opposing political camp, because that camp's weak foothold provides no viable alternative. A strong and active opposition, however, would happily welcome the support of disgruntled private interests, creating incentives to treat more powerful constituents differently. Democratic leaders facing both international punishment and domestic opposition enjoy this luxury of favoring private interests *only* in the early years of office, when criticism is muted by the overall legitimacy of the election process. As the inexorable decline of coalition

cohesion progresses, targets have less room to trade (even cheap) domestic costs for increased international good will. While the opposition presence does not appear to function consistently as a rallying force for public interests, it does impact the willingness and opportunity of leader's to offer concessions. The opposition constricts the range of options for democratic leaders, forcing them to protect elites when they have the political capital to do so.

Summary of Outcome of Sanctions against Democratic Leaders

Above, I presented a series of tests for Hypotheses 4-6. I expected the probability of concession to increase: 1) with sanction effectiveness, 2) with the public element of concession costs, 3) with the freedom from political opposition. Further, I expected that the influence of the opposition would be conditioned by cost-type and leader experience, driving down public-costs concessions and changing in efficacy over time. These hypotheses represent my attempt to more fully incorporate the domestic political landscape of targeted leaders. The evidence, as it turns out, provides strong confirmation of the need for such an endeavor while lending only limited support to my specific expectations.

The major claim of Hypothesis 4, that effectiveness will improve influence attempts, received little support from the analyses of the effectiveness measures and concession probability. Threats of economic sanction are ineffective tools, insulating leaders who should be destabilized. This insulating effect, however, both decreases the likelihood of threats escalating to imposition and increases the likelihood of concession by new democratic leaders. These leaders do not string the sanction episode out in order to reap the benefits of ineffective threats, but tend to settle the matter *on terms favorable to the sending state*. During these earliest moments, imposed sanctions would be damaging for democratic leaders, and this expectation further decreases the likelihood of

costly sanctions being imposed. I believe these two findings combine to imply that democratic leaders maneuver to provide concessions when they have the political capital to do so – early in their terms.

A "safety surplus" may exist for brand new democratic leaders. Regular election cycles and the legitimacy of the electoral process create an extremely low probability of losing office. At this time, threats provide "excess" insulation and punishments produce only moderate destabilization. Democratic leaders in this situation may be willing to trade some domestic costs for increased international good will. The findings, of course, may also be overly influenced by the non-strategic concessions which dominate the sanctioned-democrat pool. To determine the exact workings productive of this aggregate result, I need access to more detailed information regarding the nature of the sanctions in the sample. Unfortunately, TIES has yet to open their case summaries to the broader academic community.

The results regarding Hypothesis 5 provide resounding support. Public-costs concessions decrease the likelihood of imposition and increase the likelihood of sender victory at both the threat and enacted sanction stages. Giving in to demands which impact the population in ways unlikely to inspire political action is simply easier; leaders prefer to offer these concessions versus those indicative of private-costs.

Over the course of a democratic leaders' career, the gap between likelihood of concession by type declines. Holding opposition strength constant at its mean, the significance of the difference disappears after seven years. This eventual indifference stems from the overall decline in likelihood of concession over time. Irrespective of concession-cost type, the probability of giving in to sanctions converges to very low values as democratic leaders approach ten years in office. This decline suggests that the difficulty of any type of concession increases as democratic leaders' winning coalitions falter over the course of their careers, which meshes well with the institutionalization argument in H6b.

Further evidence of the importance of domestic politics surfaces when we allowed the difference in probability of concession by cost-type to vary with opposition strength. As the strength of the political opposition increases, the initial gap in willingness to sacrifice public rather than private interests widens. Indeed, decreasing the opposition's presence to a standard deviation below the mean eradicates the significance of the gap altogether. Leaders facing no significant challenge do not differentiate between sanctions which hurt the public and the elite. In order for private interests to garner favoritism from their incumbent leader, that leader must believe that there is a viable political alternative to which they may turn if displeased. The threat to abandon the winning coalition, in other words, rings tinny if the opposition is fragmented and poorly represented.

While this finding affirms the importance of the opposition to concessionary behavior, it directly contradicts Hypothesis 6's claim that the "losing coalition" would protect public interests. Instead, it appears that opposition parties compete with incumbents for the support of the powerful, preferring to construct overlapping challenger coalitions rather than rallying new participants. This dynamic echoes that located in Chapter 6, suggesting that the opposition generally plays within the existing framework of power rather than expanding the scope of conflict.

While democratic leaders constitute the bulk of sanction episode observations, the most high-profile and recognizable targets of international punishment oversee systems with less political freedom. If the findings here are driven primarily by the high proportion of trade-sanctions in the democratic sample, we may find very different results in the next section. Below I present the results of parallel analysis of small winning coalition systems.

Empirical Results: Sanctions against Nondemocratic Leaders

The hypotheses regarding nondemocratic leaders' willingness and opportunity to provide concessions mirror the conditional complexity of those for democratic leaders. Unlike in democratic systems, however, the data on political opposition lacks detail. The use of binary variables to indicate whether any competing legislative institutions exist necessarily obscures real variation in the viability of opposition in these small winning coalition states. While undesirable theoretically, this data shortcoming produces results which are considerably more approachable. Rather than interactions with two continuous variables and an ordinal scale, H5 and H6 in this sample can be evaluated using an interaction with only one continuous variable. Exposition of results here, then, requires fewer, and less complicated graphics. Evidence will be discussed in the same order as above, beginning with the imposition model.

Table 8.8 contains the independent model of sanction imposition against nondemocratic leaders. With a sample of roughly one-third the size of that for democratic leaders, the model still fits well. Over 71% of cases are correctly classified according to the ePCP measure. The model reduces expected proportion of error by 39%. Unlike the model for democratic leaders, significant combined coefficients supported the inclusion of the opposition strength and time interactions. The more complicated temporal dynamics modeled by a cubic polynomial of leader experience in the democratic sample were not supported here. The simple log of time model received strong support by a difference of Bayesian Information Criterion test.

The sign and significance of the measures for threat and expected sanction effectiveness parallel those from the model of democratic leaders. Recall from the analysis in Chapter 7, however, that the range and values of effectiveness in the nondemocratic sample differ (see Figures 7.9 and 7.10). Sanctions, and especially threats, more effectively destabilize nondemocratic leaders. The effectiveness measures

here take on large positive values for the first years in which nondemocratic leaders hold office. This is quite different from the consistently negatively signed effectiveness measure for threats in the democratic population. So, the positive coefficient in the first row of Table 8.8 tells us ineffective threats decrease the probability of imposition, while effective threats increase it. For the expected impact of imposed sanctions on leader survival, the coefficient holds a negative sign. Threats of sanction which would hurt nondemocratic leaders were they to be carried out drive down the probability of imposition. The expectation of sanctions backfiring, however, makes their actual imposition against nondemocratic leaders more likely.

The differences in probability of imposition in Table 8.9 support this initial reading. For these calculations, continuous measures were moved from their mean to maximum and nominal from their minimum to maximum values while holding all other covariates at their mean or modal values as appropriate. This gives us a snapshot view of the magnitude and direction of variable effects, though discerning the full contribution of the interactive relationships will require additional efforts. Shifting threat effectiveness from its mean value of -0.14 to its maximum of 1.28 increases the probability of imposition by .4725, an 89% jump. Given how troubling the findings from Chapter 7 were for my expectation that enacted sanctions should be more effective than threats, this inverse relationship of threat effectiveness with imposition, an early indicator of failure, is very interesting. A similar relative change in the expected effectiveness of imposed sanctions (from its mean of -0.04 to its maximum of .30) *decreases* the likelihood of imposition by 100%. This suggests that at the very least, sanctions which would be effective assist sending states in settling matters prior to incurring the actual costs of imposition.

The control variables function quite differently in the autocratic sample. Anticipated costs on both sides created increases in the likelihood of imposition against democrats. When targeting nondemocratic leaders, however, increasing costs for the

sender drives down the likelihood of sanction enactment. Raise the likely costs of sending states from mild to extreme and the probability of those sanctions ever happening drops by 73%. As might be predicted by those who argue autocrats do not personally experience the costs of economic sanctions, a similar increase in anticipated target costs does not change the likelihood of imposition.

The impact of economic growth on sanction imposition is also opposite that observed in the democratic sample. Here, stronger economies actually increase the likelihood of imposition, as do larger CINC-scores. More powerful nondemocratic targets, both in economic and military terms, then, engage in more intractable conflicts with sending states. This finding not only reverses that found in the democratic sample, but also refutes the Cox and Drury (2006) argument that less asymmetrical relationships tend to settle sanction episodes easily. While the primarily democratic pool of sending states may treat more powerful democracies as partners, they likely view powerful autocracies as threats to be addressed with higher levels of hostility. There may be an exception to this rule for trade partners, as higher levels of trade drive down the likelihood of sanction imposition by about 16%.

While each of these individual findings may contain interesting connections to the literature, the overall variation in results itself suggests problems for the empirical evaluation of sanction outcomes. If not only significance but direction of effects differs across the democratic and nondemocratic samples, we must question whether unified models can detect legitimate relationships. Regime type may matter at every stage of the process, not just in determining whether and how directly foreign policy tools intersect with leaders' survival motives. Even the implicit message of punishment may differ. If threatening a powerful democratic target implies a competitive relationship, while threatening a powerful dictator implies an "enemy" relationship, then many aspects of the

processes may not be considered parallel.[88] This problem could also provide an alternative explanation for the wider variation in the concessionary behavior of autocratic leaders located by Allen (2008b) if some autocrats experience sanctions as a more punitive exercise due to a conception of the sender-target relationship as one of enemies.

This possibly fundamental divergence in sanction processes by types, however, does not appear to function in terms of how the effectiveness of threats and expected punishments work. The direction and significance levels of the effectiveness measures remain consistent across the samples. Threat effectiveness produces perverse results, while expected effectiveness appears to function "properly" driving down the likelihood of escalation to full-fledged sanctions.

The imposition equation also contains measures of the likely domestic difficulty of providing concessions. This may seem to be an odd modeling choice, as each of the constitutive pieces of the interactive relationship between cost scale, legislative branch and leader experience fail to achieve significance. None of these individual pieces provide evidence regarding the conditional relationship between the three factors, however. The combined impact in fact, achieves significance at the 95% level.

To demonstrate this significant impact, I calculated the combined coefficient of concession cost scale. This is the linear combination of the first derivative of the linear index $\mathbf{X\beta}$ with respect to the cost scale measure and the values of cost scale, legislature and time.[89] Figure 8.8 charts the combined coefficient of the cost scale variable across its three categories with respect to time and legislature presence. The thick black line

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[88] For theory on the importance of how two parties view their relationship in determining the outcome of interactions see Wendt (1999) or Bull (1977). Drezner's (1999) formal treatment also suggests that the likely progression of sanction episodes will differ starkly between states with strong trade-ties and friendly histories and states with histories of conflict and isolation.

[89] More precisely, I calculate the combined coefficient of cost scale as the following: $\beta_C + \beta_{O\times C}(\text{legislature}) + \beta_{t(O\times C)}(\text{legislature}\times\ln(t_i))$.

gives the combined coefficient for cost type's impact when the system has no legislative branch. The thick blue line charts the effect of cost-scale when there is a legislature. Thin lines of corresponding colors mark the 95% confidence interval, calculated using the formula for variance of a sum of random variables and an assumption of Normal distribution (e.g. Friedrich 1982).

The value of the concession-costs scale remains constant within each of the panels in Figure 8.7. Across these panels, the public element of the likely costs of concession increases from none on the left, to full on the right. I calculated the measure in-sample, using actual observations of the cost types, legislature presence and leader experience. This strategy means that the combined coefficient is available for graphing only at actually observed values of these variables, rather than all theoretically possible values. The black lines, marking the combined coefficient when there is no legislature present, thus exist only for a sub-set of values of leader experience in the mixed- and public-costs panels.

In these systems, diffusion of concession costs amongst groups in the target state never exerts a significant effect on imposition; the 95% confidence bounds always include zero. But when the legislature is present, public and mixed costs create significant, dynamic effects. When concessions will create private costs, the legislature's presence does not shift the combined effect significantly away from zero. In the middle panel, depicting the effect under mixed-costs concession, however, the slope of the effect steepens considerably. This shift in slope creates a significant effect during the early years of nondemocratic leaders' tenure. Increasing the cost scale by an additional unit to public costs further increases the positive slope of the effect and its significance. In the early stages of nondemocratic leaders' careers, the more diffused costs make imposition less likely. The strength of this repression deflates over time, becoming insignificant for mixed cost cases, and briefly reversing in effect for fully public costs. If settlement prior to imposition suggests concessions as strongly as was the case in the democratic sample,

this pattern may mimic that found among the democrats with respect to opposition presence. Below, we will estimate the process of imposition and outcome simultaneously to allow further exploration of these effects.

Tables 8.10 and 8.11 contain bootstrap parameter estimates from the competing risks models of wins and draws. Unfortunately, I could not estimate a parallel model of the "sender loses" outcome. Because the data contain no instances of a sender capitulating on an issue of mixed or public costs against a nondemocratic leader without domestic institutional checks, the model cannot be estimated including the interaction between these factors. The competing risks framework is not damaged by the inability to estimate the impact of covariates on the likelihood of sender capitulation. As noted above, most findings are not strong for this equation, and the coding of the other two outcome types is not affected. The data problem itself, however, actually supports the patterns which have emerged in the democratic sample regarding public costs. If these types of concessions are easy to win, then there should never be a reason for senders to back away from them.

In the sender wins model (Table 8.10), the imposition model mirrors the independent one. Most control variables in the outcome model fail to reach significance, with the exception of economic growth. In the bootstrapped model, the estimated confidence interval for the correlation of the errors across equations includes positive and negative values. Despite this anomaly, the Wald test firmly rejects the null hypothesis that the processes should be treated as independent. A model run simply using the observed data, rather than re-sampling through the bootstrap procedure, returns a more normal range of values for ρ and confirms the results of the test of independent equations. I feel, therefore, that it is appropriate to proceed with the simultaneously estimated models. The same is not true of the negotiated settlement models. Even without the bootstrap procedure, the probability of draws and the likelihood of imposed sanctions in the nondemocratic sample do not appear to be systematically related. This may be due to

the failure of my postulated factors to significantly relate to the negotiation process between sender states and their nondemocratic targets. Nothing significantly relates to the decision to settle these sanction episodes through a mutual agreement of some sort.

Again, the choice of utilizing a competing risks framework was driven primarily by the methodological appropriateness of treating the multiple types of events, not a theoretical interest in the factors producing the alternative outcomes. It is not clear how negotiated settlements relate to influence, and the factors driving sending states to capitulate have not been treated in my theory. For my purposes then, the success of my tests hinges on the sender win equations, which tell us about actual concessionary behavior, and hence about influence. The discussion below proceeds to evaluate the results of the bivariate probit with respect to Hypotheses 4-6.

Hypothesis 4: The Effectiveness of Threatened and Enacted Sanctions

Chapter 7 revealed the general patterns of sanction impact on leader survival. Provided medium or higher credibility, both threatened and imposed sanctions can destabilize nondemocratic leaders in their first three years of office. The impact of threats, however, was estimated to be considerably stronger and longer lasting in their negative impact on autocratic leaders (see Figure 7.3). Higher credibility on the sender's part emphasizes these dynamic trends in effectiveness. Hypothesis 4 predicts that leaders targeted with hurting threats should prefer to settle in order to end their own discomfort. We may look to the probability of imposition as the first place for the impact of effectiveness to surface.

The independent model of imposition presented above, suggested that the findings regarding threat effectiveness might defy Hypothesis 4. To investigate the effect more thoroughly, I calculated the probability of imposition at observed levels of effectiveness and a baseline probability with the impact of sanctions or threats on leader failure

restricted to zero. Subtracting the baseline level from the predicted probability provides a measure of both substantive and statistical significance for the impact of effectiveness on imposition across time and levels of credibility.

I chart this difference in probability in Figure 8.9. The dark gray lines depict the difference in probability of imposition due to the expected effectiveness of imposed sanctions; bright green, the difference due to the effect of threats on targets' survival prospects. The thin lines of corresponding color give the 90% confidence interval based on a simulated distribution of 1,000 draws from the parameter estimates in Table 8.8. Level of sender credibility varies from low to high across the panels. I also overlaid the value of the effectiveness measures in dashed lines of matching color. When the effectiveness measures hold positive values, sanctions are effective; when they cross below the zero line, the estimated impact of sanctions is to make leaders safer.

The value of the effectiveness measure for threats quickly crosses the zero line when senders make low credibility threats. Yet, the (in)effectiveness of these threats consistently, significantly decreases the likelihood of sanctions. This relationship changes as senders' become more credible in their threats. At medium credibility, the difference curve shifts upward, but its confidence bands always encompass zero. Under the highest credibility threats, in the right-hand panel of Figure 8.9, nondemocratic targets experience destabilization for about four years. During and beyond this period, however, the effectiveness of threats actually makes the probability of sanction imposition more likely. The bright green difference curves depict this significant impact, remaining above the zero line for nine years. This increase in probability of imposition begins at 200% on a leader's first day in office and declines over time to 11% just before dropping out of significance.

Charting the full range of threat effectiveness' impact on imposition, then, reveals a more complicated dynamic than the single-shot first-difference presented in Table 8.9. When senders issue ineffective and non-credible threats, the conflict is more likely to

terminate prior to sanction imposition. More resolved senders who issue effective, credible threats, however, may see their effective tools of influence contributing to a more costly sanction episode. Provided that the subsequent probability of concession also declines, this dynamic does not fit with the basic expectation of Hypothesis 4. The effectiveness of these threats should increase the level of successful influence for sender states, yet it appears instead to muddy the situation.

An interesting possible reason for this contradiction may be found by examining the dark gray dashed line in the left-hand panel of Figure 8.9. This curve charts the "expected" effectiveness of highly credible imposed sanctions. While the estimated impact of threats is highly destabilizing for the first five years of the autocratic target's tenure, the expected impact of actualized sanctions drops below the zero line within little over a year. Many nondemocratic leaders being hurt by threatened sanctions, in other words, would not only stop feeling the pain but possibly start feeling positive results were those sanctions to be realized. This discrepancy between the effectiveness of threats and punishments could provide targets with a clear incentive to escalate discord with resolved sending states. Interestingly, it would not encourage them to behave with similar recalcitrance against less credible threats. The insulatory impact of low-credibility threats almost always outstrips that expected for enacted sanctions at similar levels of resolve.

Overall, the relationship between expected effectiveness and imposition probability fits fairly well with expectations. The solid dark gray curves in Figure 8.9 provide this supportive evidence. The likelihood of an episode advancing to imposition increases when nondemocratic targets should expect to benefit from enacted sanctions; it drops or remains unaffected when they should expect to be hurt. At low levels of credibility, this impact registers a 93% drop in the likelihood of senders actualizing threats, though the overtime trend diminishes this impact until it drops out of significance around the twelve year mark. The expectation of costs at the medium credibility level

also decrease the likelihood of sanctions by 98%, though only against a very inexperienced leader. After one and one half years, nondemocratic leaders have nothing to fear from imposed sanctions, and the likelihood of imposition begins to increase as a result of this boldness. With four years' experience, the likelihood of a medium credibility threat being imposed increases by 11%. When the target has more than two years experience, a highly credible threat is 20% more likely to be imposed than if the leader expected sanctions to be non-effective. During the brief period of time during which highly credible imposed sanctions would be expected to hurt, their effectiveness does not significantly relate to the probability of imposition.

In general, episodes which terminate prior to imposition boast a higher proportion of wins for sending states. When enacted punishments are likely to be ineffective, even when the threat of them hurts, early termination becomes less likely. This finding in and of itself does not inform us of the outcome. To connect imposition to overall sanction outcome, Figure 8.10 charts first differences in the probability of victory at the threat stage. Again, the predicted probability of concession at the threat stage here is compared to a baseline which restricts the effectiveness of threats or expected effectiveness of sanctions to zero. I chart the median predicted difference from a simulated sampling distribution of 1,000 draws from the parameter estimates in Table 8.8. Thin lines mark the 90% confidence intervals from that distribution. Green lines relate the difference due to threat effectiveness; dark gray, that due to expected effectiveness of imposed sanctions. The sender's credibility varies from low to high across the panels.

At low credibility, those ineffective threats which quizzically increased the likelihood of early settlement also appear to increase the likelihood of victory. The impact is substantively small, hovering between one and five percent. At medium credibility, threat (in)effectiveness makes no significant contribution either to the likelihood of imposition or of early sender victory. When the sender exhibits high commitment, however, the effectiveness of threats produces a slight but significant

decrease in the likelihood of settlement prior to imposition. These effective, highly credible threats both increase the likelihood of imposition and decrease the likelihood of concession for those cases which do not progress to imposition. The pattern fits my earlier postulation that leaders may greet effective threats with recalcitrance if they expect that escalating the situation to imposed sanctions could decrease their domestic risks.

Across levels of sender credibility and time, the expected effectiveness of sanctions decreases the likelihood of autocratic leaders granting concessions at the threat stage. Even for the brief periods of time when sanctions might hurt, the expected impact drives down the potential for successful influence. If autocratic leaders know that only the most inexperienced among them stand to lose from imposed sanctions, then they may have the incentive to weather the initial pain in exchange for the long-term benefits. This expectation decreases the likelihood of senders winning concessions at the threat stage by over 100% over time and credibility.

Once sanctions become imposed, their effectiveness relates much less efficiently to the likelihood of concessions. Figure 8.11 gives the change in probability of concession to enacted sanctions resultant from the predicted level of effectiveness. At low and medium credibility, the impact fails to reach significance at the 90% level. When the sender is highly credible, the effect of sanctions on targeted leaders can produce increases in the likelihood of victory, but only when the leader has held office for more than ten years. During this portion of leader tenure, sanctions would not be destabilizing. As with democratic leaders threatened early in their tenures, these leaders are also likely to be very secure in power. Concessions at this advanced point in tenure, then, may represent a strategic decision to trade domestic costs for international favor.

Overall, those low and medium credibility threats under which nondemocratic leaders might expect to suffer are more likely to be terminated at the threat stage. The conditions of these early settlements, however, are less likely to be favorable for the

sender. The period of sanction effectiveness lasts only a couple of years, and perhaps because targets can credibly tell sending states that they do not mind being sanctioned in the future, these episodes tend to settle early and against the interests of senders. In the first two years, early wins for senders prove 1-5% less likely than they would be if the sanctions did not "work" at all. As the expected effectiveness of sanctions decreases, further declines in the likelihood of sender victory at the threat stage follow.

Ironically, in cases which move on to the imposition phase, the ineffectiveness of highly credible sanctions against experienced nondemocratic leaders increases their willingness to offer concessions. Combined, these findings suggest that nondemocratic leaders inclined to offer concessions may find it difficult to give in at the threat stage. In order to appear justified in giving up, they prefer to force senders' hands and hold out until the threats become imposed sanctions. If so, the type of concession may matter. I turn to the influence of cost-type and opposition presence in the next section.

Hypotheses 5 and 6: Concession Costs and the Role of the Opposition over Time

My argument contends that in terms of the difficulty of domestic political hurdles, regime type matters less than fans of democracy may tend to believe. For any leader, selling out the powerful involves more difficulty than selling out the weak. Institutions matter in that they may allow for the input of opposition groups, whose preferences lead them to stick up for the public. In democracies, the opposition enjoys a permanently legitimate role, though their strength and efficacy varies over time. In autocratic systems, the tolerance of opposition activities cannot be assumed, but it should not be ignored. Most nondemocratic leaders share power, symbolically if not practically, with a legislative branch of government. Though participation in legislative politics may be sharply curtailed, or co-opted by the regime, the existence of the legislature suggests that leaders see some need to negotiate with the losing coalition in order to maintain the

safety of their position (see for example, Gandhi and Przeworski 2006). In some cases, legislative actors may be powerful enough to act as representatives of the people, obstructing concessions which would be damaging to them. A pattern analogous to that uncovered regarding the impact of concession-cost type and opposition strength in democratic systems should surface here. The temporal dynamics, however, may differ, as hypothesized in H6a.

The simplest means of presenting evidence of the conditional relationships expressed in H5 and H6 for nondemocratic leaders is to chart the difference in probability of imposition and concession by cost type and presence of legislature. These differences were calculated using the same general procedure as described above. I calculated the probability of the event under both the first type of cost noted in the panel titles and the cost noted second. I then subtracted the probability under the second type of cost from that under the first to produce the relative change in probability given the change from the first type of cost to the second.

As noted above, the action for the cost and opposition variables is divided between the imposition and outcome stages. When considering imposition independent of outcomes, however, cost-type and opposition presence do not produce significant shifts in probability despite the significant combined coefficients charted in Figure 8.8. I will present figures, then, only for the impact on sender victory prior to and following imposition. One implication from Figure 8.8 holds true: nondemocratic leaders with no (even symbolic) institutional check on their decision power treat concessions equivalently regardless of whom they might hurt. Concession type matters only under the presence of a legislative branch. As was the case for democratic leaders facing less than the average level of opposition strength, nondemocratic leaders who have no check do not fear private interests more than the public.

The existence of a legislature, even if its power is limited in practice, changes the nature of the game. Figure 8.12 gives the relative probabilities for victory at the threat

stage by type of cost for leaders whose systems have a legislature. The difference here, reaches significance at the 90% level only against exceptionally new and exceptionally experienced targets. For the brand new autocrat, offering public-costs concessions at the threat stage is more likely. But the extremely rare autocrat who holds office for more than 40 years would prefer to offer private-costs concessions. At this stage, the subtler distinction between mixed and fully public costs is very difficult to distinguish from zero.

The findings following imposition provide much stronger support for Hypothesis 5. Figure 8.13 charts the difference in probability of sender victory after enacting sanctions by cost-type. When sanctions have been imposed, nondemocratic leaders who share power with a legislative branch generally prefer to hurt the public rather than a private interest. The difference in likelihood of offering public- versus private-costs concessions becomes significant against leaders with more than three years' experience. At this point, the relative preference for hurting the public makes public-costs concessions 150% more likely. The impact continues steadily until the target accumulates a much higher than normal tenure.

The same pattern holds, at slightly lower levels, for mixed-costs concessions versus private-costs concessions, displayed in the middle panel of Figure 8.13. When costs spread across the public and some private interests, nondemocratic leaders' willingness to provide concessions increases. A leader with more than four years experience provides a 58% greater likelihood of offering mixed-costs concessions versus those which would create purely private-costs. This difference increases gently over time to around a 100% high before turning back. The smaller magnitude of this effect is confirmed in the final panel of Figure 8.13, which shows the significant difference between mixed- and public-costs concessions. Nondemocratic leaders are between 20 and 60% less likely to make changes to the status quo which produce mixed costs compared to those which would generate purely public costs.

Private interests dominate nondemocratic winning coalitions. Targeted leaders should always shield these vital members of the power structure from the demands of outside powers. When a legislature exists to increase transparency and allow development of political alternatives, nondemocratic leaders always perform this basic function. Their willingness or capability to do so, however, increases over time. Perhaps precisely because of the increased transparency and existence of other known political figures, nondemocratic leaders take a more conservative approach to protecting their private interests in their first years in power. Until they are certain of the long-term winning coalition stability, they avoid the highest levels of favoritism. Alternatively, the temporal dynamic may stem from increasing competence. Leaders with more experience may know better how to sell out the public without spawning wide-spread discontent.

Summary of Outcomes of Sanctions against Nondemocratic Leaders

Above, I presented a second round of tests for Hypotheses 4-6. In the nondemocratic sample, my expectations mirrored those for the big winning coalitions. The probability of concession, according to my model of influence, should always increase with sanction effectiveness and decrease with domestic political difficulty. The opposition should fit into the difficulty of concessions, but its efficacy should also be conditional on the type of costs likely to result from changes to the status quo and on the experience of the incumbent leader. The evidence from the nondemocratic sample confirms that domestically-based incentives power concessionary behavior, and provides interestingly tight parallels to the patterns observed in democratic targets.

The major claim of Hypothesis 4, that effectiveness will improve influence attempts, received qualified support from the analyses of the effectiveness measures and concession probability amongst nondemocratic targets. Threats of economic sanction can be more effective tools than imposed sanctions, but this discrepancy actually produces

poor outcomes for sending states. Targeted leaders who know that escalating an episode to sanction enactment will make them safer have very little actual incentive to give in at the threat stage. This means that even effective threats increase the probability of sanction imposition while reducing the likelihood of concession at the threat stage. Because nondemocratic leaders with more than two or three years in power can always expect to weather the punishment of economic sanctions without suffering destabilization, the effectiveness measures always contribute to a lower probability of victory at the threat stage.

One apparent exception to this rule can be found in high credibility economic sanctions against experienced autocrats. Despite the ineffectiveness of these sanctions, which create an estimated insulation against domestic risks, targets in this situation offer concessions at a higher rate. This finding harkens back to the puzzling likelihood of ineffectively threatened new democratic leaders to provide higher probability of concession. New democratic leaders and experienced nondemocratic leaders share a similarly low ex-ante level of risk. Their disparate processes of winning coalition cohesion mean that these times coincide to their safest years in office. The security of their positions may allow for an easier trade-off between incurring domestic costs and currying international favor by offering concessions.

Tests of the relative likelihood of providing concessions based on diffusion of costs produced qualified support for Hypotheses 5. Public-costs concessions increase the likelihood of sender victory at both the threat and enacted sanction stages, but only if the leader shares power with a legislative branch. Leaders who face no checks do not need to treat concessions differently depending on domestic difficulty, because domestic difficulty is always low. When an opposition exists, however, giving in to demands which impact the public almost always proves preferable to demands which would hurt private interests. For imposed sanctions, this preference for public rather than private costs increases dramatically over time. This temporal pattern contrasts with that for

democratic leaders, whose willingness to provide concessions declines with experience. Taken together, these dynamics provide strong support for the primacy of winning coalition cohesion in driving temporal effects, as argued in H6b.

The nondemocratic target does not differentiate between the type of costs induced by concession unless a legislature exists to potentially question the decision to give in to external power. But, the opposition's role here does not correspond to the one predicted in Hypothesis 6. As in the democratic sample, a stronger losing coalition does not mean that leaders treat the public better. Rather, these leaders tend to favor private interests even more intensely when they have a viable political alternative available. At this point, the failure of the opposition to live up to the democratic expectations of Hypothesis 6 may constitute the single strongest finding in the dissertation. Across regime types and policy tools, the opposition makes it more likely that targeted leaders will favor private interests in their concessionary behavior.

Conclusion: Economic Sanctions as Tools of Influence

The evidence discussed above focuses directly on evidence regarding hypotheses, placing aside more general conclusions regarding sanction success and extant findings in the literature. Historically, the principal debate amongst scholars has focused on whether sanctions work at all (e.g. Marinov 2005; Pape 1997; Elliott 1998). Though plagued by conceptual and measurement problems, the analysis of sanction success has finally come to a point where we can ask, instead, "Why do sanctions so frequently fail, and when might they be more effective?" In answering these questions, the literature has overwhelmingly focused on two factors: selection problems and regime type. My focus on targeted leader's incentives furthers investigations on the role of both these factors in the more nuanced research agenda.

Formal models of the sanction process have blamed both measurement and conceptual problems for the paltry success rate observed in economic sanctions (see

Drezner 1999, 2003; Hovi et al. 2005). If our data and definitions systematically exclude those sanctions which are most likely to be successful, then our pessimism may be overblown. Some attempts have been made to deal with the selection problem on a small-scale (e.g. Drezner 2003; Nooruddin 2002). To my knowledge, however, the tests carried out in this chapter constitute the first systematic attempt to test the frequent claim that those sanctions most likely to be effective are precisely the sanctions which never escalate to imposition.

In raw numbers, the claim from the formal literature appears to hold up. The proportion of successes amongst sanctions which terminate prior to imposition is 58% greater.[90] But the tests which I performed above, demonstrated that this improved record does not stem primarily from the likely effectiveness of the imposed sanctions in disrupting their targets. For democratic leaders, the expected impact of imposed sanctions factors into neither the likelihood of imposition nor the probability of granting concessions at the threat stage. In fact, the probability of early termination in favor of sending states increases when threats are most *ineffective*, when they actually make democratic targets safer in power. The expected effectiveness of imposed sanctions against nondemocratic leaders also generally fails to boost the likelihood of early victory. While sanctions might be expected to hurt brand new autocrats, they do not work well against the experienced. And targets behave as though they understand this: they withstand pressure to provide concessions, escalating the likelihood of imposition and decreasing the likelihood of providing early concessions.

The second principle factor emphasized in the extant literature on economic sanction success is regime type. Democracies, most argue, feature leaders whose

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[90] The difference in proportion of successes, 4.36% of sanctioned-leader years to 1.8% of sanctioned-leader years, in threatened versus imposed sanctions respectively is significant at the 95% confidence level.

championing of the public interest determines their continued stay in power (e.g. Allen 2008, 2008b) . These leaders, then, must be more sensitive to the kinds of costs inflicted by imposed economic sanctions. This supposed sensitivity was assumed, also, to account for the improved success rate of economic coercion against democratic targets (Allen 2008, 2008b; Cox and Drury 2006; Nooruddin 2002; Bolks and Al-Sawayel 2000). First of all, the TIES data do not support the overall finding from earlier datasets that democratic leaders concede more often. When trade issues and threatened sanctions are included in the sample, autocrats give in to sender demands at higher rates than democrats.[91] Secondly, democratic leaders do not suffer higher domestic costs as a result of sanction targeting. They actually benefit from the threat of sanctions as their populations reject external influence attempts and rally around their leadership. Nondemocratic leaders experience bigger and longer periods of destabilization as a result of external threats and punishments than do democratic leaders.

In contradiction to both the literature's arguments and my own expectations, targeted leaders, regardless of regime type, actually appear particularly willing to offer concessions in the face of threats or punishments which backfire. Ineffective threats against new democratic leaders and ineffective sanctions against experienced nondemocratic leaders increase the probability of concessions substantially over what would be observed if the external interventions did not impact target survival prospects. These leaders give outside powers what they want, but not because failing to do so would be so domestically costly that they were left no alternative. Instead, these leaders enjoy so comfortable a position at home that they feel free to trade a little domestic stability for a little international favor.

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[91] Partial or complete acquiescence characterizes 4.10% of nondemocratic sanctioned-leader years compared to 2.57% of democratic sanctioned-leader years. This difference is highly statistically significant (p=0.008).

In terms of successful influence then, it seems that economic sanctions simply cannot get a break. The problem is not necessarily that those which would work better terminate at the threat stage; neither is it that leaders who benefit from sanctions universally refuse to grant concessions. Rather, targeted leaders give concessions when they have the political will and the political capital to do so. The coercive actions of external powers appear to do little to change their underlying preferences. The efficacy of economic sanctions as a tool of influence, even under the most favorable circumstances, deserves questioning. If sending states intend sanctions to produce results rather than to signal preferences, they should take steps to improve the process. Perhaps the first step should be taking care to demand only easy concessions. For the vast majority of sanctioned leaders, public-costs concessions are the most likely to be provided. Wise targeting should also aim for leaders facing opposition at home, but with a wealth of political capital to spare.

Table 8.1 Difference of Proportions of Sender Win by Cost of Concessions

Concession Costs	***Sender Wins Concessions***		
	No	*Yes*	*Total*
Private	97.70% (3569)	2.30% (84)	100% (3653)
Mixed	96.71% (529)	3.29% (18)	100% (547)
Public	92.70% (457)	7.30% (36)	100% (493)
Total	97.43% (4555)	2.94% (138)	100% (4693)

NOTE: Percentages calculated by row with raw frequencies in parentheses.

Kendall's Tau-b = .0755, ASE=.018

Table 8.2 Difference of Proportions of Sender Win by Imposition of Sanctions

Imposed Sanctions	***Sender Wins Concessions***		
	No	*Yes*	*Total*
No	96.21% (1981)	3.79% (78)	100% (2059)
Yes	97.72% (2574)	2.28% (60)	100% (2634)
Total	97.43% (4555)	2.94% (138)	100% (4693)

NOTE: Percentages calculated by row with raw frequencies in parentheses.

Difference of proportions = .0151, Z=3.04, $p<.01$

Table 8.3 Probit of Sanction Imposition against Democratic Leaders

Effectiveness of Threat	1.345*** (0.214)
Effectiveness of Sanction (expected)	-6.825*** (0.694)
Threat	-4.296*** (0.555)
Leader's Experience	-0.963*** (0.208)
Leader's Experience Squared	0.287*** (0.100)
Leader's Experience Cubed	-0.028** (0.013)
Anticipated Sender Costs	0.564*** (0.147)
Anticipated Target Costs	0.497*** (0.134)
Diffusion Scale of Concession Costs	-0.281*** (0.107)
Economic Growth (lag)	-3.562*** (0.707)
Logged Trade (lag)	0.124** (0.048)
CINC-score	-3.191** (1.346)
Winning Coalition Size	-1.453*** (0.498)
Joint Democracy	0.445*** (0.144)
Sanction Episode Duration	2.431*** (0.214)

Table 8.3 Continued

Sanction Episode Duration Squared	-0.797*** (0.077)
Sanction Episode Duration Cubed	0.063*** (0.006)
Constant	4.522*** (0.680)
Wald $\chi 2$	408.52***
Pseudo Log Likelihood	-1141.3363
Pseudo R^2	0.5323
Expected Percent Correctly Predicted	79.10%

NOTE: N=3,540 sanctioned-leader years from 1971-1999 covering 1,360 separate sanction-leader episodes. Robust standard errors, in parentheses, clustered on these episodes.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8.4 Substantive Impact of Variables on Probability of Sanction Imposition Against Democratic Leaders

	Change in P(Imposition)	95% Confidence Interval	
Threat Effectiveness	+0.6697	0.5808	0.7586
Sanction Effectiveness	-0.3269	-0.4021	-0.2518
Anticipated Sender Costs	+0.5846	0.3819	0.7873
Anticipated Target Costs	+0.5379	0.3158	0.7601
Target Economic Growth	-0.3446	-0.4147	-0.2745
Target Total Trade	+0.1340	0.0319	0.2360
Target CINC Score	-0.1591	-0.2734	-0.0449
Target Winning Coalition Size	-0.1407	-0.2341	-0.0472
Joint Democracy	+0.1582	-0.2510	-0.0653

NOTE: This table utilizes parameter estimates from independent model of sanction imposition reported in Table 8.3. Differences calculated are an increase from the mean to the maximum for continuous variables, and from minimum to maximum for nominal variables. For each calculation all other variables are set at mean or modal values as appropriate. As the effectiveness measures are a function of leader experience, the values of the cubic polynomial of time in office were set to values consistent with the in-sample mean and maximum values of the measures. Confidence intervals derived using Delta Method via Scott Long's estpost suite for Stata10.

Table 8.5 Bootstrap Parameter Estimates for Bivariate Probit of Sender Victory and Sanction Imposition against Democratic Leaders

		Coef.	Std. Error	95% Confidence Interval	
Sender Wins	Effectiveness of Threat	-0.5990	0.2384	-1.0663	-0.1317
	Effectiveness of Sanction (Imposed)	0.5922	0.8246	-1.0241	2.2084
	Threat	-0.7345	0.3323	-1.3858	-0.0831
	Imposed Sanction	-0.3625	0.2633	-0.8786	0.1535
	Log of Leaders Cumulative Time in Office (f(ti))	0.0597	0.1085	-0.1529	0.2723
	Diffusion of Concession Costs Scale (C)	-0.2246	0.2985	-0.8097	0.3604
	Opposition Strength (O)	-0.0026	0.0094	-0.0210	0.0157
	Opposition Influence on Cost Scale (O*C)	0.0185	0.0087	0.0013	0.0356
	Time Trend for Opposition Influence on Cost Scale (f(ti)*O*C)	-0.0016	0.0010	-0.0036	0.0004
	Anticipated Sender Costs	-0.2784	0.1530	-0.5782	0.0215
	Anticipated Target Costs	0.4891	0.1263	0.2416	0.7366
	Target Economic Growth (lag)	2.0519	1.1897	-0.2800	4.3837
	Constant	-1.8265	1.0066	-3.7994	0.1464

Table 8.5 Continued

		Coef.	Std. Error	95% Confidence Interval	
Imposition of Sanctions	Effectiveness of Threat	1.3793	0.2450	0.8991	1.8596
	Effectiveness of Sanction (Expected)	-6.9887	0.7602	-8.4786	-5.4987
	Threat	-4.2376	2.4645	-9.0680	0.5927
	Anticipated Sender Costs	0.5428	0.1612	0.2269	0.8587
	Anticipated Target Costs	0.5055	0.1393	0.2325	0.7784
	Diffusion Scale of Concession Costs	-0.2650	0.1268	-0.5135	-0.0165
	Target Economic Growth (lag)	-3.7820	0.7717	-5.2945	-2.2695
	Target's Total Logged Trade (lag)	0.1514	0.0502	0.0531	0.2497
	Target CINC-score	-3.5618	1.4654	-6.4340	-0.6896
	Target Winning Coalition Size	-1.6283	0.5111	-2.6300	-0.6265
	Joint Democracy	0.3945	0.1682	0.0649	0.7241
	Constant	4.5974	2.5434	-0.3876	9.5824
ρ		0.3305	0.1579	0.0210	0.6400
Wald $\chi 2$ Test of Independent Equations			4.9274, p=0.0264		

NOTE: Reported parameters are mean estimates from 500 bootstrap samples of 3,336 sanctioned-leader years from 1971-1999. Sender wins variable is coded using a competing risks framework, resulting in 188 censored observations at the outcome stage. Samples drawn with replacement, clustering on sanction-leader episodes. Though coefficients are not reported here, cubic polynomials of sanction episode duration were included in each equation, and cubic polynomial of leader experience was included in imposition equation to account for duration dependence.

Table 8.6 Bootstrap Estimates for Bivariate Probit of Sender Loses and Sanction Imposition against Democratic Leaders

		Coef.	Std. Error	95% Confidence Interval	
Sender Loses	Effectiveness of Threat	0.0140	0.2172	-0.4116	0.4396
	Effectiveness of Sanction (Imposed)	1.0236	1.0677	-1.0691	3.1162
	Threat	-0.0323	0.3021	-0.6243	0.5598
	Imposed Sanction	-0.2962	0.2844	-0.8535	0.2612
	Log of Leaders Cumulative Time in Office (f(ti))	0.2121	0.1171	-0.0174	0.4415
	Diffusion of Concession Costs Scale (C)	0.3061	0.3153	-0.3119	0.9241
	Opposition Strength (O)	-0.0033	0.0111	-0.0251	0.0185
	Opposition Influence on Cost Scale (O*C)	0.0106	0.0124	-0.0136	0.0348
	Time Trend for Opposition Influence on Cost Scale (f(ti)*O*C)	-0.0017	0.0014	-0.0045	0.0010
	Anticipated Sender Costs	0.4680	0.1454	0.1830	0.7529
	Anticipated Target Costs	-0.2487	0.1304	-0.5042	0.0069
	Target Economic Growth (lag)	0.1474	1.0289	-1.8692	2.1640
	Constant	-2.3240	1.1832	-4.6431	-0.0049

Table 8.6 Continued

		Coef.	Std. Error	95% Confidence Interval	
Imposition of Sanctions	Effectiveness of Threat	1.4204	0.2321	0.9655	1.8753
	Effectiveness of Sanction (Expected)	-7.1738	0.7824	-8.7072	-5.6404
	Threat	-4.2149	2.5113	-9.1369	0.7071
	Anticipated Sender Costs	0.5761	0.1505	0.2812	0.8710
	Anticipated Target Costs	0.4438	0.1544	0.1412	0.7464
	Diffusion Scale of Concession Costs	-0.3636	0.1380	-0.6340	-0.0931
	Target Economic Growth (lag)	-3.8868	0.7753	-5.4064	-2.3671
	Target's Logged Total Trade (lag)	0.1549	0.0566	0.0438	0.2659
	Target CINC-score	-3.8862	1.5736	-6.9704	-0.8020
	Target Winning Coalition Size	-1.5262	0.5419	-2.5884	-0.4641
	Joint Democracy	0.3808	0.1680	0.0516	0.7100
	Constant	4.6300	2.5734	-0.4137	9.6738
ρ		0.3835	0.1974	-0.0033	0.7703
Wald Test of Independent Equations		4.5970, p=.032			

NOTE: Reported parameters are mean estimates from 500 bootstrap samples of 3,349 sanctioned-leader years from 1971-1999. Sender loses variable is coded using a competing risks framework, resulting in 169 censored observations at the outcome stage. Samples drawn with replacement, clustering on sanction-leader episodes. Though coefficients are not reported here, cubic polynomials of sanction episode duration were included in each equation, and cubic polynomial of leader experience was included in imposition equation to account for duration dependence

Table 8.7 Bootstrap Estimates for Bivariate Probit of Negotiated Settlement and Sanction Imposition against Democratic Leaders

		Coef.	Std. Error	95% Confidence Interval	
Negotiated Settlement (Draw)	Effectiveness of Threat	-0.4885	0.2521	-0.9826	0.0057
	Effectiveness of Sanction (Imposed)	-0.4954	0.9853	-2.4265	1.4357
	Threat	-1.1572	0.2947	-1.7349	-0.5795
	Imposed Sanction	-0.7558	0.2623	-1.2700	-0.2416
	Log of Leaders Cumulative Time in Office (f(ti))	-0.1060	0.1390	-0.3785	0.1665
	Diffusion of Concession Costs Scale (C)	0.0660	0.4727	-0.8604	0.9924
	Opposition Strength (O)	0.0161	0.0130	-0.0095	0.0416
	Opposition Influence on Cost Scale (O*C)	-0.0096	0.0157	-0.0404	0.0212
	Time Trend for Opposition Influence on Cost Scale (f(ti)*O*C)	-0.0001	0.0018	-0.0035	0.0033
	Anticipated Sender Costs	0.3214	0.1749	-0.0214	0.6641
	Anticipated Target Costs	0.0842	0.1506	-0.2109	0.3793
	Target Economic Growth (lag)	-0.6322	1.1074	-2.8027	1.5383
	Constant	-0.3181	1.2086	-2.6870	2.0507

Table 8.7 Continued

		Coef.	Std. Error	95% Confidence Interval	
Imposition of Sanctions	Effectiveness of Threat	1.3434	0.2295	0.8935	1.7933
	Effectiveness of Sanction (Expected)	-6.8153	0.7427	-8.2711	-5.3596
	Threat	-4.2423	2.4149	-8.9753	0.4908
	Anticipated Sender Costs	0.5695	0.1612	0.2535	0.8855
	Anticipated Target Costs	0.4458	0.1542	0.1436	0.7480
	Diffusion Scale of Concession Costs	-0.3594	0.1365	-0.6270	-0.0918
	Target Economic Growth (lag)	-3.9019	0.7668	-5.4048	-2.3990
	Target's Logged Total Trade (lag)	0.1484	0.0560	0.0387	0.2580
	Target CINC-score	-4.0139	1.4509	-6.8576	-1.1702
	Target Winning Coalition Size	-1.6113	0.5486	-2.6866	-0.5361
	Joint Democracy	0.3731	0.1597	0.0600	0.6862
	Constant	4.6954	2.5077	-0.2197	9.6104
ρ		0.8971	0.2346	0.4374	1.3568
Wald Test of Independent Equations			17.4542, p<.000001		

NOTE: Reported parameters are mean estimates from 500 bootstrap samples of 3,323 sanctioned-leader years from 1971-1999. Settlement variable is coded using a competing risks framework, resulting in 201 censored observations at the outcome stage. Samples drawn with replacement, clustering on sanction-leader episodes. Though coefficients are not reported here, cubic polynomials of sanction episode duration were included in each equation, and cubic polynomial of leader experience was included in imposition equation to account for duration dependence

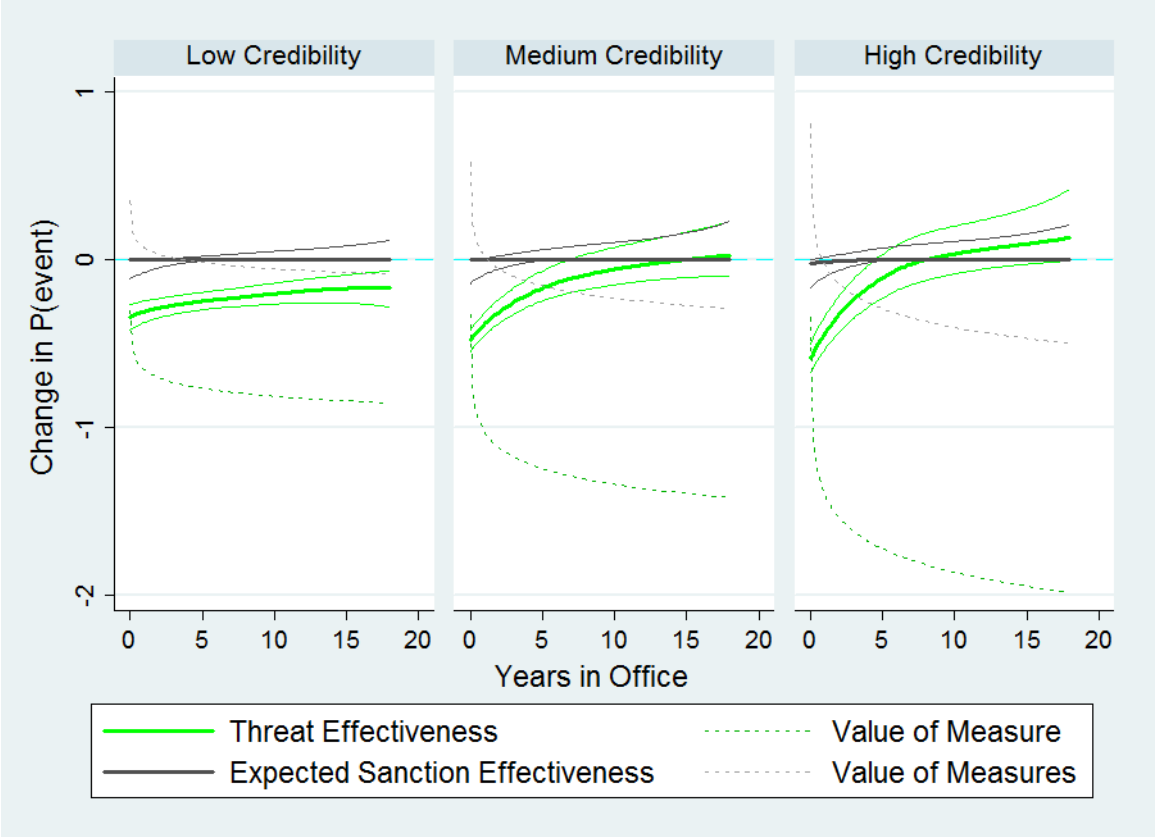

Figure 8.1 Difference in Marginal Probability of Imposition against Democratic Leaders by Effectiveness

NOTE: Figure charts predicted change in probability of imposition given shift of observed effectiveness value to zero impact on leader survival. Reported statistic is median predicted change based on simulated distribution of 1,000 draws from the variance-covariance matrix of bivariate probit reported in Table 8.5. Thin solid lines mark 90% confidence bounds based on that distribution. Dashed lines of corresponding color relate the value of the effectiveness measures given the value of time and credibility. Bright blue dashed line highlights the zero line.

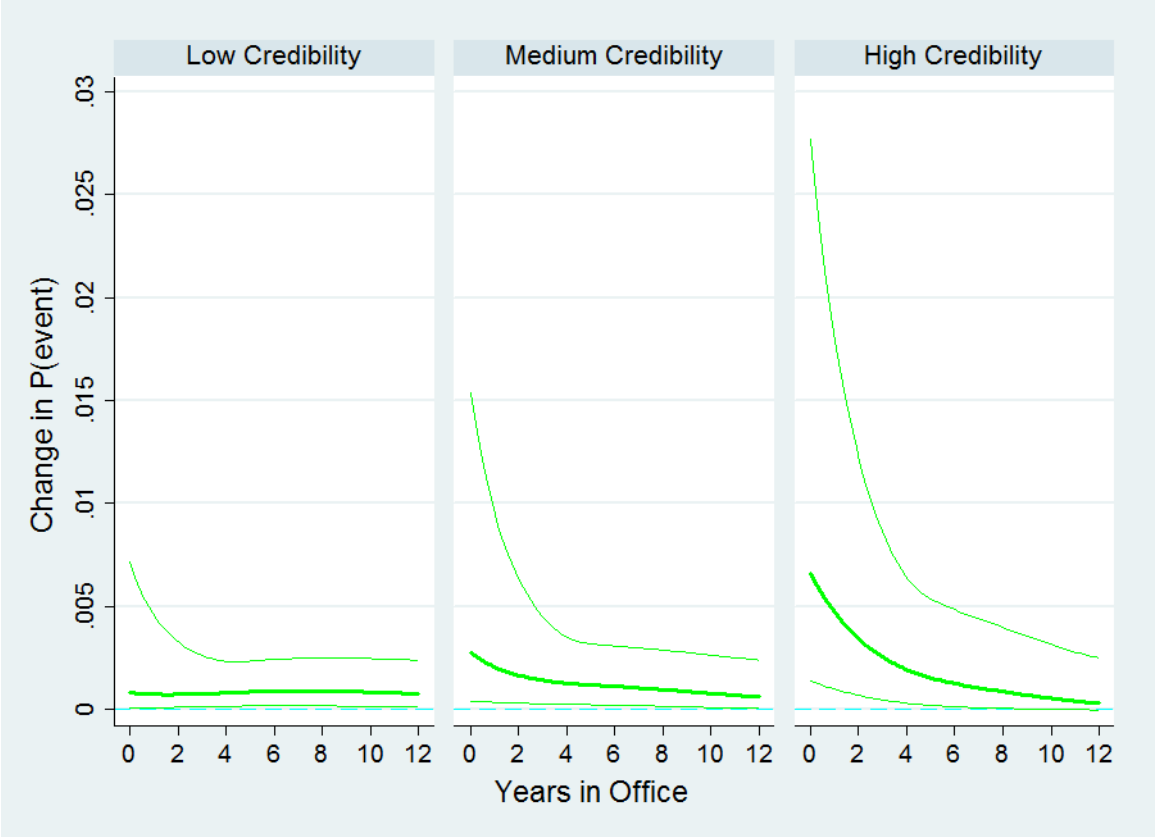

Figure 8.2 Difference in Probability of Sender Win at Threat Stage by Threat Effectiveness

NOTE: Figure charts predicted change in probability of imposition given shift of observed effectiveness value to zero impact on leader survival. Thick line marks median predicted change from simulated distribution of 1,000 draws from the variance-covariance matrix of the bootstrapped bivariate probit in Table 8.5. Thin lines give 90% confidence bands based on the same distribution. X-axis is restricted to twelve years to ease viewing of significant portion of effect. No excluded observation achieves significance at the 90% level.

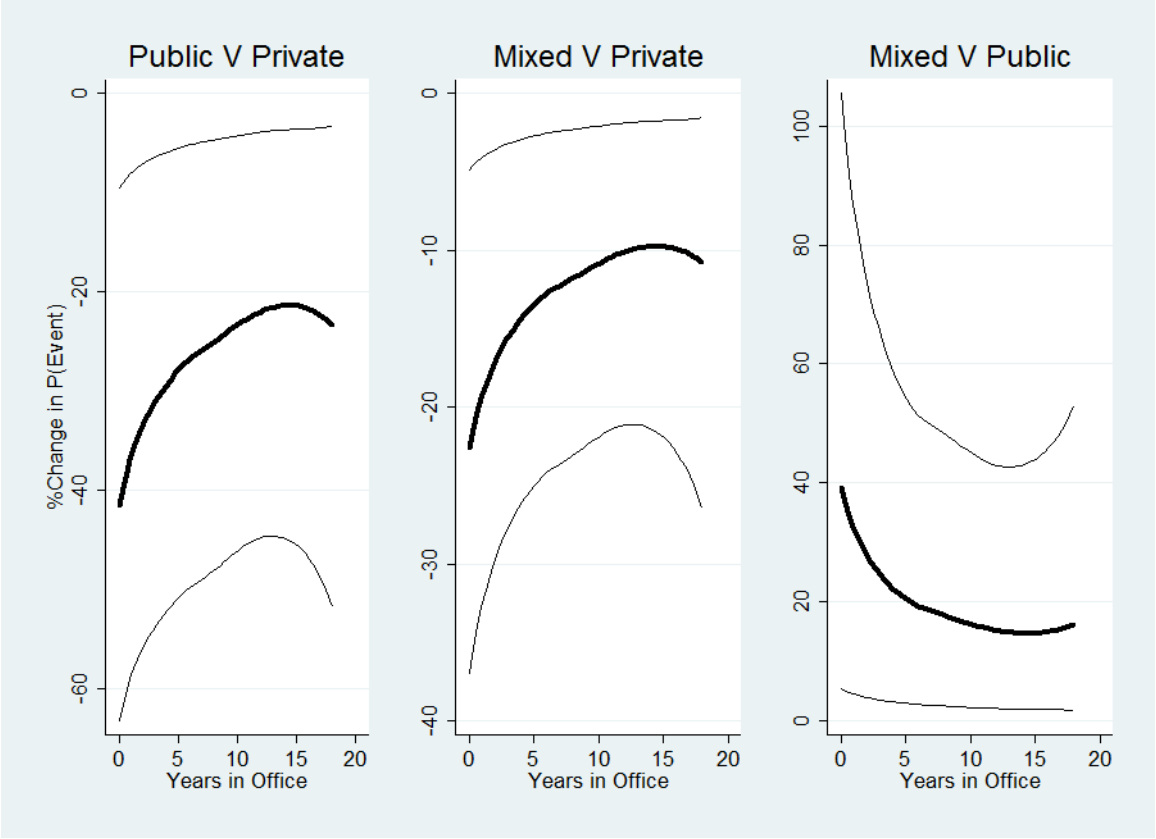

Figure 8.3 Change in Probability of Sanction Imposition against Democratic Leaders by Type of Cost

NOTE: Figure charts the percentage change in probability of sender win produced by switching concession cost-type from the first term, to the second term. Public Vs. Private costs, for example, is the probability from a public-costs concession minus the probability from a private-costs concession, divided by the probability from a private-costs concession. Reported statistic is the median calculated change based on a simulated distribution of 1,000 draws from the variance-covariance matrix of bivariate probit in Table 8.5. Thin lines mark the 90% confidence interval based on that distribution. Y-scales differ across panels.

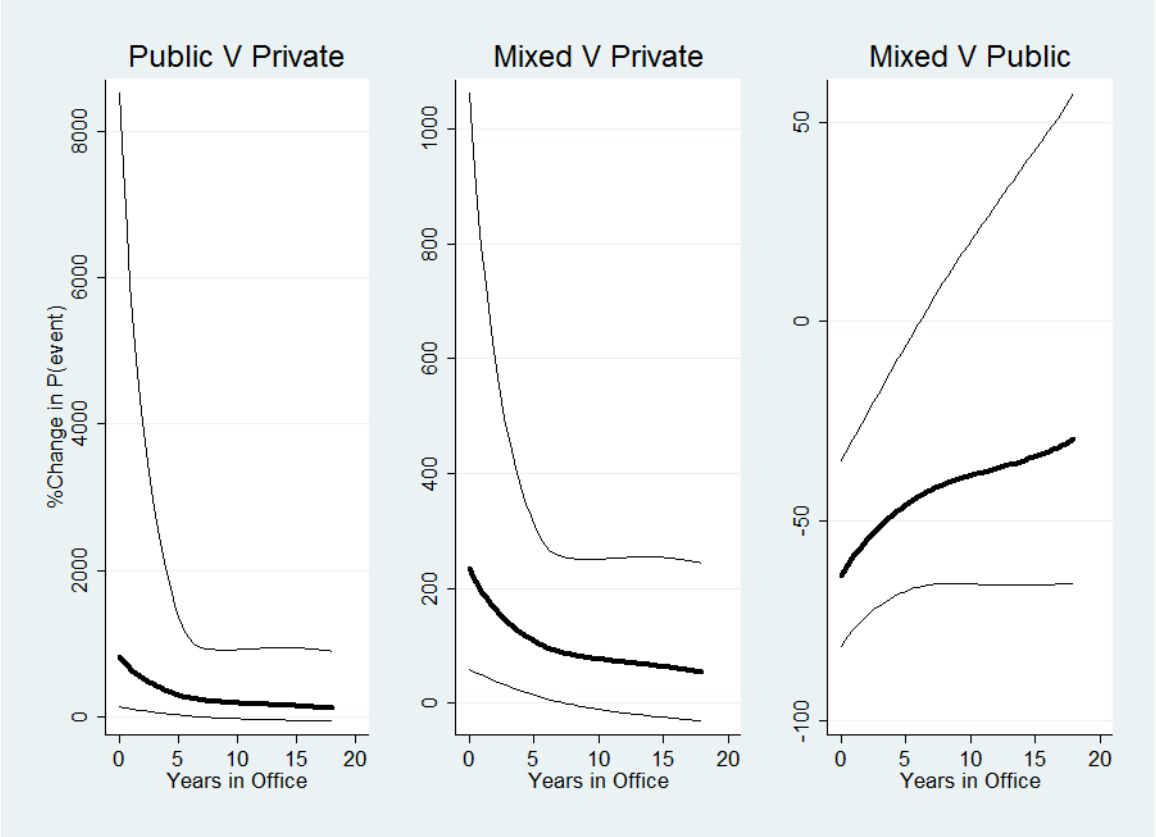

Figure 8.4 Difference in Probability of Sender Victory against Democratic Leader at Threat Stage by Type of Concession Cost

NOTE: Figure charts the percentage change in probability of sender win without imposition produced by switching concession cost-type from the first term, to the second term. Public Vs. Private costs, for example, is the probability from a public-costs concession minus the probability from a private-costs concession, divided by the probability from a private-costs concession. Reported statistic is the median calculated change based on a simulated distribution of 1,000 draws from the variance-covariance matrix of bivariate probit in Table 8.5. Thin lines mark the 90% confidence interval based on that distribution. Y-scales differ across panels.

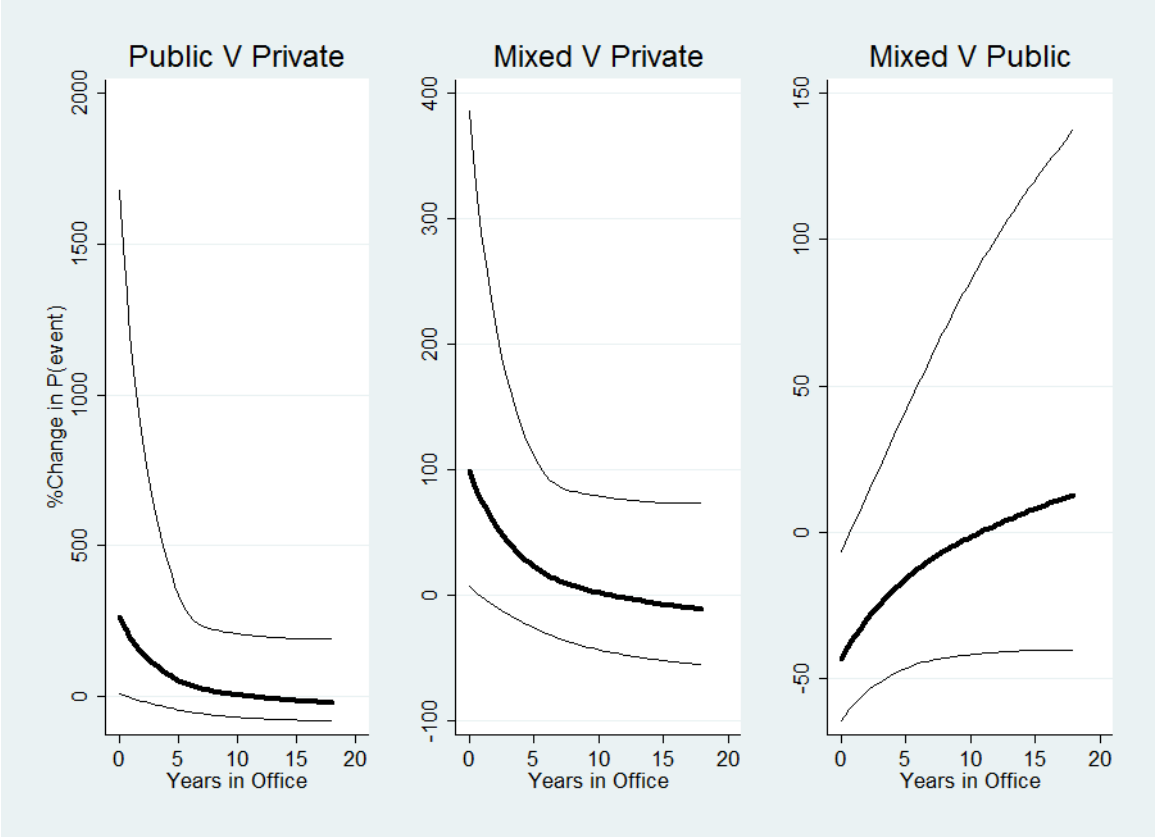

Figure 8.5 Change in Probability of Sender Victory against Democratic Target after Imposition by Concession-Cost Type

NOTE: Figure charts the percentage change in probability of sender win after imposition produced by switching concession cost-type from the first term, to the second term. Public Vs. Private costs, for example, is the probability from a public-costs concession minus the probability from a private-costs concession, divided by the probability from a private-costs concession. Reported statistic is the median calculated change based on a simulated distribution of 1,000 draws from the variance-covariance matrix of bivariate probit in Table 8.5. Thin lines mark the 90% confidence interval based on that distribution. Y-scales differ across panels.

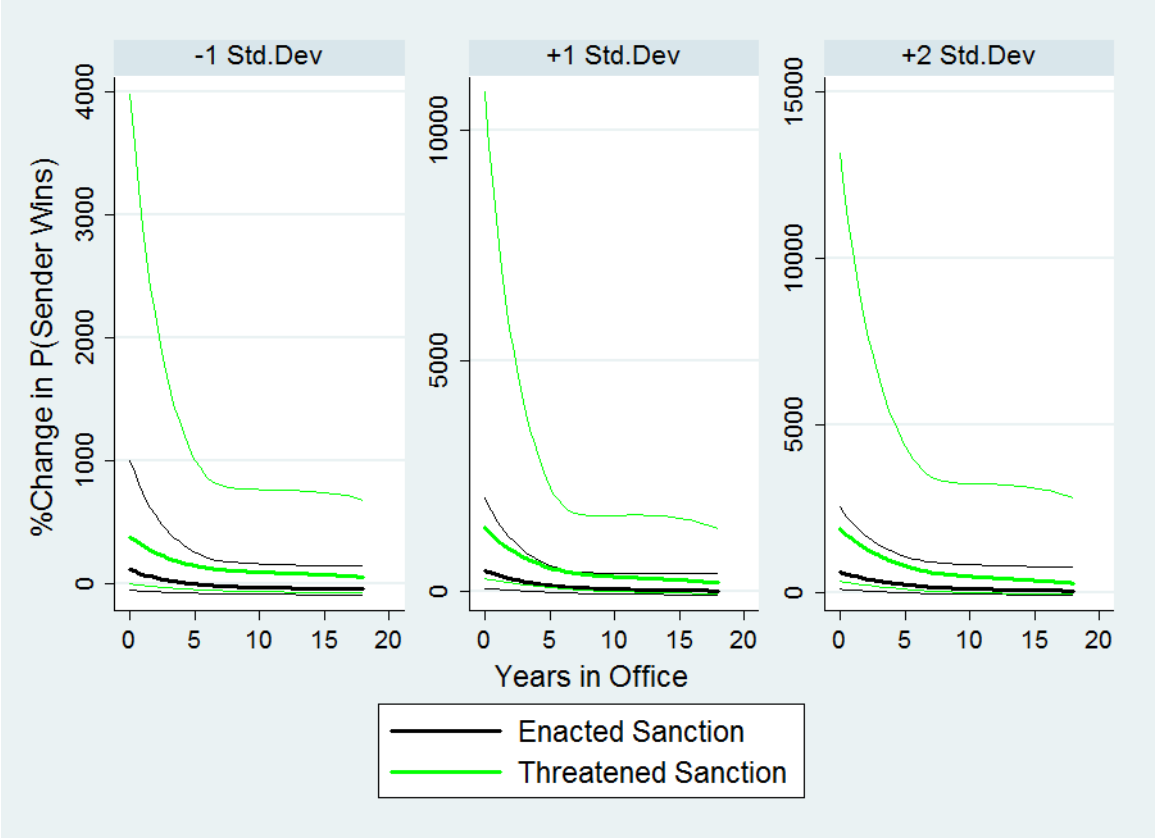

Figure 8.6 Change in Probability of Sender Winning Public- Versus Private-Costs Concessions against Democratic Leader by Opposition Strength and Time

NOTE: Threatened sanction line charts probability of concession prior to imposition, and enacted sanction line charts probability of concession after imposition. Thick line gives median predicted change from simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit in Table 8.4. Thin lines give 90% confidence interval based on that distribution. For enacted sanctions, the predicted probabilities are of sender victory after imposition; for threatened sanctions, of sender victory at the threat stage. Moving from left to right, the panels feature increases in the strength of the opposition. One standard deviation below the mean of opposition strength is a 32% share of seats/election results, one standard deviation above the mean is 57%, and two standard deviations above is 69%. Y-scales differ across panels.

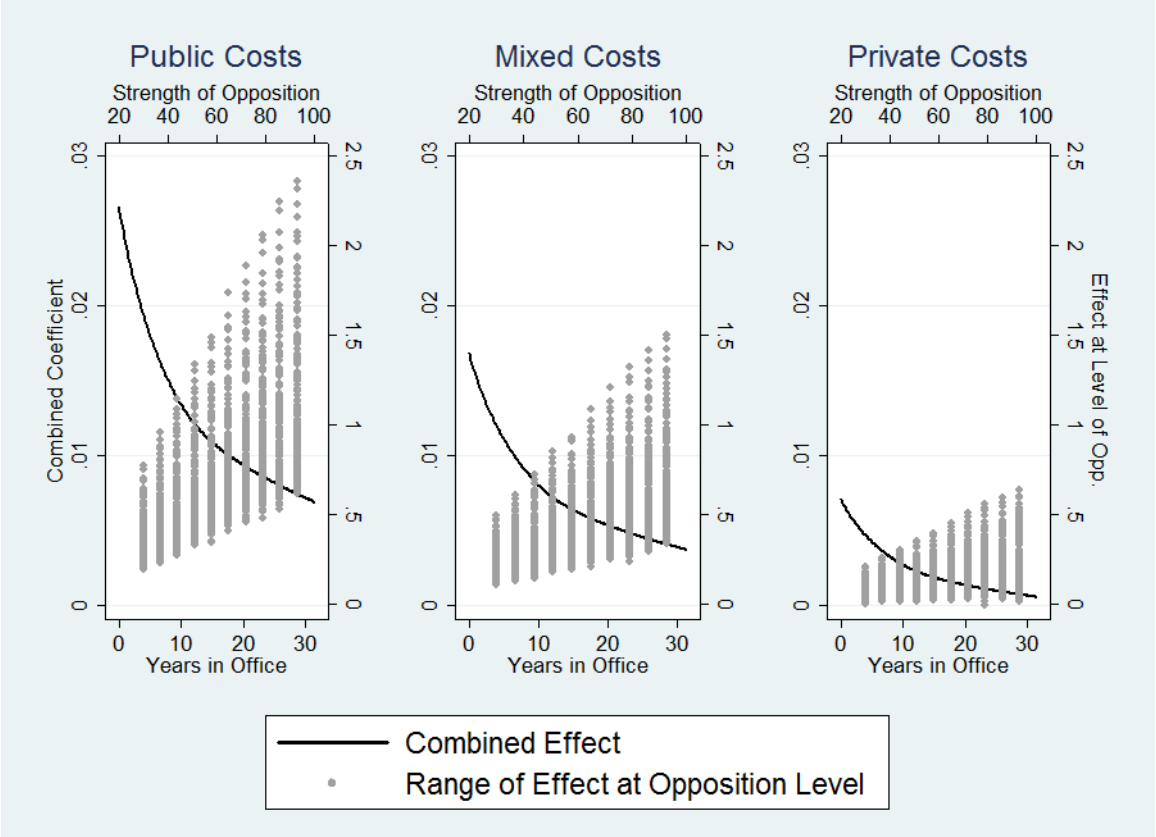

Figure 8.7 Summary of Opposition Strength Effect on Sender Win across its Range, Time, and Concession-Cost Types

NOTE: NOTE: Figure reports median values from simulated sampling distribution of 1,000 draws from the variance-covariance matrix. Combined coefficient is calculated as: $\beta_0 + \beta_{0\times C}\times C + \beta_{t\times 0\times C}\times C\times \ln(t)$. The right-hand y-axis charts the product of this quantity and the level of opposition strength. The coefficient line was smoothed with Lowess function. Variance in the effect evaluated across the range of opposition strength stems from the value of time; lower values of time correspond to higher values of the evaluated coefficient, consistent with the negative trend. Scatter plot utilizes upper x-axis and right-hand y-axis.

Table 8.8 Probit Regression of Sanction Imposition Against Nondemocratic Leaders

Effectiveness of Threat	3.554*** (0.578)
Effectiveness of Sanction (Expected)	-8.564*** (1.340)
Log of Leader's Cumulative Time in Office (t)	0.132 (0.154)
Diffusion Scale of Concession Costs (C)	0.102 (0.223)
Legislative Branch (O)	-0.396 (0.496)
Legislative Branch's Effect on Cost Scale (O*C)	-0.811 (0.572)
Time Trend for Legislative Branch's Effect on Cost Scale (t(O*C))	0.094 (0.069)
Anticipated Sender Costs	-0.518** (0.237)
Anticipated Target Costs	-0.191 (0.143)
Target Economic Growth (lag)	0.548 (0.495)
Target Logged Total Trade (lag)	-0.116*** (0.040)
Target CINC-score	10.274*** (2.247)
Sanction Episode Duration	1.766*** (0.237)
Sanction Episode Duration Squared	-0.454*** (0.085)
Sanction Episode Duration Cubed	0.034*** (0.008)
Constant	-0.911 (1.193)

Table 8.8 Continued

Wald $\chi 2$	223.83
Log PseudoLikelihood	-505.2072
Pseudo R^2	0.3368
Expected Percent Correctly Predicted	71.29%

NOTE: N=1147sanctioned-leader years from 1971-1999. Robust standard errors, in parentheses,clustered on 376 sanctioned-leader episodes.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8.9 Substantive Impact of Variables on Probability of Sanction Imposition Against Nondemocratic Leaders

	Change in P(Imposition)	95% Confidence Interval	
Threat Effectiveness	+0.4725	0.3037	0.6413
Sanction Effectiveness	-0.4521	-0.6260	-0.2782
Anticipated Sender Costs	-0.5628	-0.9671	-0.1585
Anticipated Target Costs	-0.2179	-0.5373	0.1016
Target Economic Growth	+0.1035	-0.0644	0.2713
Target Total Trade	-0.1821	-0.3082	-0.0560
Target CINC Score	+0.2770	0.1829	0.3711

NOTE: This table utilizes parameter estimates from independent model of sanction imposition reported in Table 8.8. Differences calculated are an increase from the mean to the maximum for continuous variables, and from minimum to maximum for nominal variables. For each calculation all other variables are set at mean or modal values as appropriate. As the effectiveness measures are a function of leader experience, the log of time in office was set to values consistent with the in-sample mean and maximum values of the measures. Confidence intervals derived using Delta Method via Scott Long's estpost suite for Stata10.

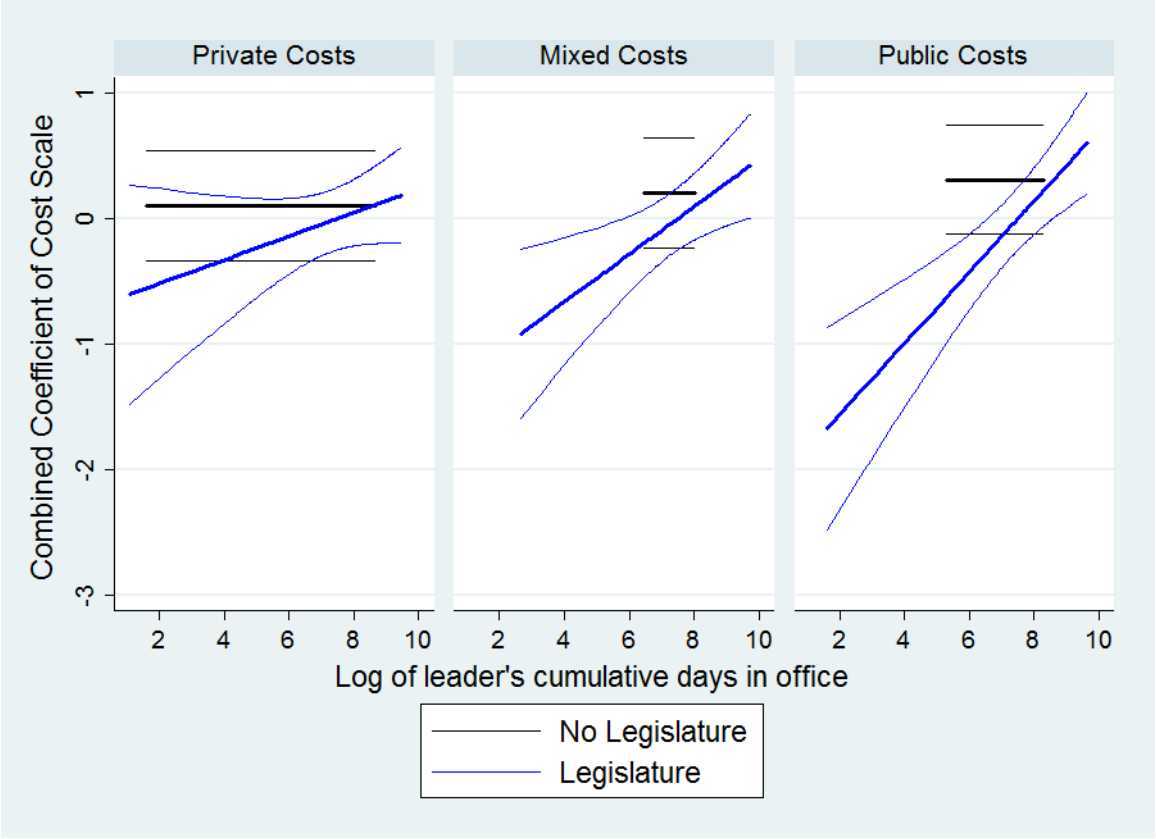

Figure 8.8 Contribution of Concession-Cost Scale to Probability of Sanction Imposition against Nondemocratic Leaders

NOTE: Lines track product of combined coefficient of cost scale measure in-sample using estimates from probit regression of imposition reported in Table 8.8. The calculation for the combined coefficient is: $\beta_C + \beta_{O\times C}(\text{legislature}) + \beta_{t(O\times C)}(\text{legislature}\times\ln(t_i))$. Thin lines mark the 95% confidence intervals derived using the formula for the variance of a sum of random variables and z-statistics.

Table 8.10 Bootstrap Estimates of Bivariate Probit Regression of Sender Victory and Sanction Imposition Against Nondemocratic Targets

		Coef.	Std.Error	95% Confidence Interval	
Sender Wins	Effectiveness of Threat	1.4545	0.8015	-0.1165	3.0255
	Effectiveness of Sanction (Imposed)	-1.0079	1.6178	-4.1787	2.1628
	Threat	0.4088	0.7623	-1.0852	1.9029
	Imposed	-1.3974	0.3580	-2.0991	-0.6958
	Log of Leader's Cumulative Time in Office (t)	0.1425	0.2006	-0.2508	0.5357
	Diffusion Scale of Concession Costs (C)	-0.1005	2.5881	-5.1730	4.9721
	Legislative Branch (O)	-0.5543	3.8079	-8.0177	6.9091
	Legislative Branch's Effect on Cost Scale (O*C)	-0.0285	2.6600	-5.2421	5.1851
	Time Trend for Legislative Branch's Effect on Cost Scale (t(O*C))	0.0364	0.0708	-0.1023	0.1751
	Anticipated Sender Costs	-0.2320	0.2209	-0.6649	0.2009
	Anticipated Target Costs	0.1498	0.1468	-0.1378	0.4375
	Target Economic Growth (Lag)	1.8027	0.8472	0.1422	3.4633
	Sanction Episode Duration	0.8348	0.7361	-0.6079	2.2775
	Sanction Episode Duration Squared	-0.3473	0.3687	-1.0700	0.3754
	Sanction Episode Duration Cubed	0.0280	0.0466	-0.0633	0.1192
	Constant	-2.0693	4.2372	-10.3740	6.2355

Table 8.10 Continued

		Coef.	Std.Error	95% Confidence Interval	
Sanction Imposition	Effectiveness of Threat	3.5423	0.6504	2.2677	4.8170
	Effectiveness of Sanction (Expected)	-8.1068	1.5068	-11.0600	-5.1535
	Log of Leader's Cumulative Time in Office (t)	0.1006	0.1731	-0.2386	0.4399
	Diffusion Scale of Concession Costs (C)	0.0473	0.2615	-0.4652	0.5599
	Legislative Branch (O)	-0.4648	0.5717	-1.5852	0.6557
	Legislative Branch's Effect on Cost Scale (O*C)	-1.0835	0.6503	-2.3580	0.1911
	Time Trend for Legislative Branch's Effect on Cost Scale (t(O*C))	0.1358	0.0796	-0.0202	0.2917
	Anticipated Sender Costs	-0.4891	0.2691	-1.0165	0.0384
	Anticipated Target Costs	-0.1608	0.1661	-0.4864	0.1649
	Target Economic Growth (lag)	0.4532	0.5620	-0.6483	1.5547
	Target Logged Total Trade (lag)	-0.1063	0.0423	-0.1893	-0.0233
	Target CINC-score	11.1533	2.4872	6.2785	16.0281
	Sanction Episode Duration	1.7254	0.7261	0.3024	3.1485
	Sanction Episode Duration Squared	-0.4410	0.3693	-1.1649	0.2829
	Sanction Episode Duration Cubed	0.0323	0.0481	-0.0620	0.1266
	Constant	-0.6439	1.3393	-3.2688	1.9810
ρ		0.8544	76.0764	-1.0000	1.0000
Wald Test of Independent Equations			$\chi 2$=13.6066, p = 0.0002		

NOTE: N=1085 sanctioned-leader years from 1971-1999 including 345 individual leader-sanction episodes. Reported parameters are bootstrap coefficients and standard errors from 500 samples of size N clustered on these episodes. Dependent variable is coded for competing risks, resulting in 250 censored observations in the outcome stage. The confidence intervals for ρ which include the full range from -1 to +1 are an anomaly which surfaces only in the bootstrapping process.

Table 8.11 Bootstrap Estimates of Bivariate Probit Regression of Negotiated Settlement and Sanction Imposition Against Nondemocratic Targets

		Coef.	Std.Error	95% Confidence Interval	
Draw (Negotiated Settlement)	Effectiveness of Threat	1.2533	1.0954	-0.8936	3.4002
	Effectiveness of Sanction (Imposed)	1.8892	2.1383	-2.3017	6.0801
	Log of Leader's Cumulative Time in Office (t)	0.7878	1.1428	-1.4521	3.0277
	Diffusion Scale of Concession Costs (C)	-0.0697	0.8773	-1.7892	1.6498
	Legislative Branch (O)	0.2430	0.2106	-0.1698	0.6558
	Legislative Branch's Effect on Cost Scale (O*C)	-0.0007	1.8997	-3.7241	3.7227
	Time Trend for Legislative Branch's Effect on Cost Scale (t(O*C))	0.2335	2.9674	-5.5826	6.0496
	Anticipated Sender Costs	-0.0935	1.8778	-3.7739	3.5870
	Anticipated Target Costs	-0.0130	0.0876	-0.1846	0.1587
	Target Economic Growth (lag)	0.3924	0.2713	-0.1394	0.9242
	Effectiveness of Threat	-0.1971	0.2175	-0.6235	0.2292
	Effectiveness of Sanction (Expected)	0.8807	0.9331	-0.9481	2.7094
	Sanction Episode Duration	-1.0460	0.5494	-2.1228	0.0308
	Sanction Episode Duration Squared	0.3067	0.1828	-0.0517	0.6651
	Sanction Episode Duration Cubed	-0.0224	0.0196	-0.0607	0.0159
	Constant	-3.6645	3.9004	-11.3093	3.9802

Table 8.11 Continued

		Coef.	Std.Error	95% Confidence Interval	
Sanction Imposition	Effectiveness of Threat	3.6535	0.6907	2.2998	5.0073
	Effectiveness of Sanction (Expected)	-8.4484	1.6270	-11.6373	-5.2595
	Log of Leader's Cumulative Time in Office (t)	0.1796	0.1817	-0.1765	0.5358
	Diffusion Scale of Concession Costs (C)	0.1450	0.4878	-0.8111	1.1010
	Legislative Branch (O)	-0.3528	0.7128	-1.7499	1.0444
	Legislative Branch's Effect on Cost Scale (O*C)	-0.6746	0.7485	-2.1416	0.7923
	Time Trend for Legislative Branch's Effect on Cost Scale (t(O*C))	0.0691	0.0782	-0.0842	0.2225
	Anticipated Sender Costs	-0.5273	0.2734	-1.0632	0.0085
	Anticipated Target Costs	-0.1533	0.1707	-0.4878	0.1811
	Target Economic Growth (lag)	0.5497	0.5526	-0.5334	1.6327
	Target Logged Total Trade (lag)	-0.1042	0.0495	-0.2012	-0.0071
	Target CINC-score	10.3317	2.9734	4.5039	16.1596
	Sanction Episode Duration	1.7567	0.8008	0.1871	3.3262
	Sanction Episode Duration Squared	-0.4501	0.4049	-1.2437	0.3435
	Sanction Episode Duration Cubed	0.0331	0.0526	-0.0700	0.1363
	Constant	-1.3444	1.4995	-4.2835	1.5946
ρ		0.3505	106.4067	-1.0000	1.0000
Wald Test of Independent Equations			$\chi 2$=.0836, p = 0.7725		

NOTE: N=1066 sanctioned-leader years from 1971-1999 including 339 individual leader-sanction episodes. Reported parameters are bootstrap coefficients and standard errors from 500 samples of size N clustered on these episodes. Dependent variable is coded for competing risks, resulting in 282 censored observations in the outcome stage. The confidence intervals for ρ which include the full range from -1 to +1 are an anomaly which surfaces only in the bootstrapping process.

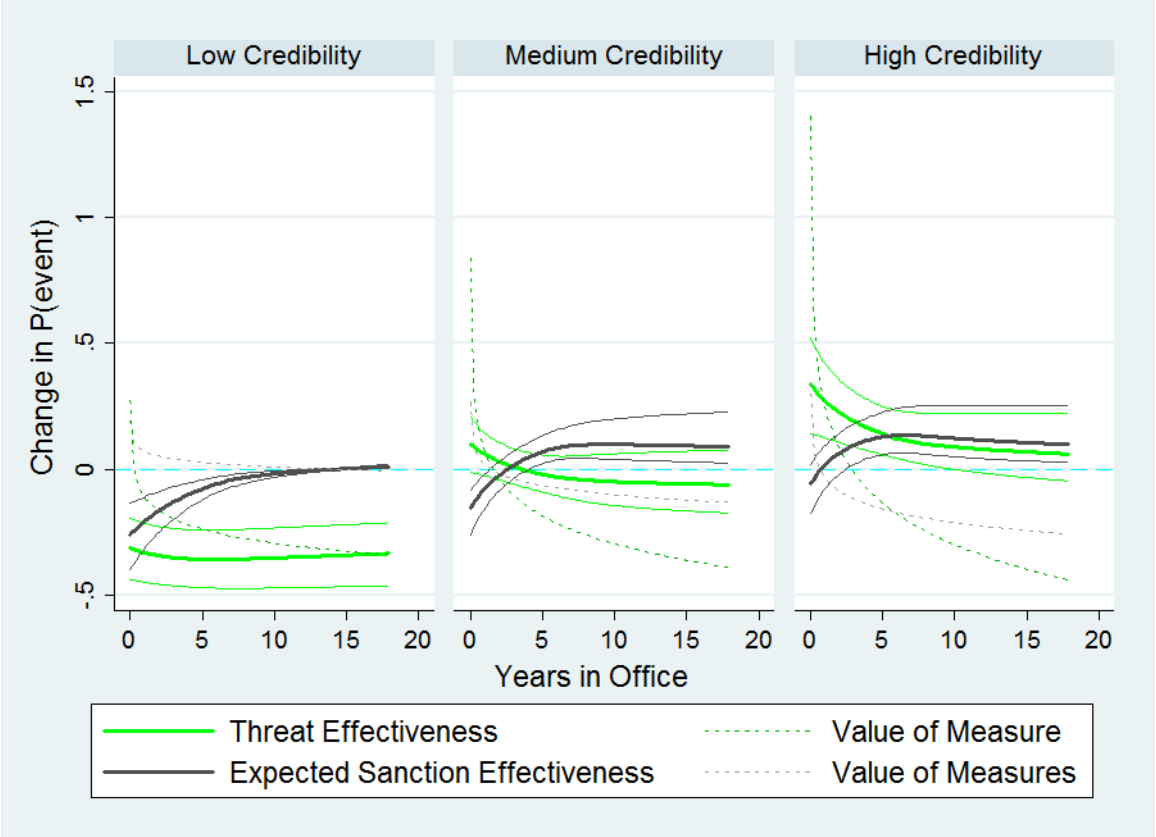

Figure 8.9 Difference in Probability of Sanction Imposition against Nondemocratic Leaders by Effectiveness

NOTE: Figure charts change in probability generated by observed level of sanction effectiveness compared to baseline of no impact on target survival. Reported statistic is median calculation from simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 8.8. Thin lines mark 90% confidence bounds based on that distribution. Dashed lines of corresponding color give the value of the effectiveness measures at given time and level of credibility. Bright blue dashed line highlights the zero line.

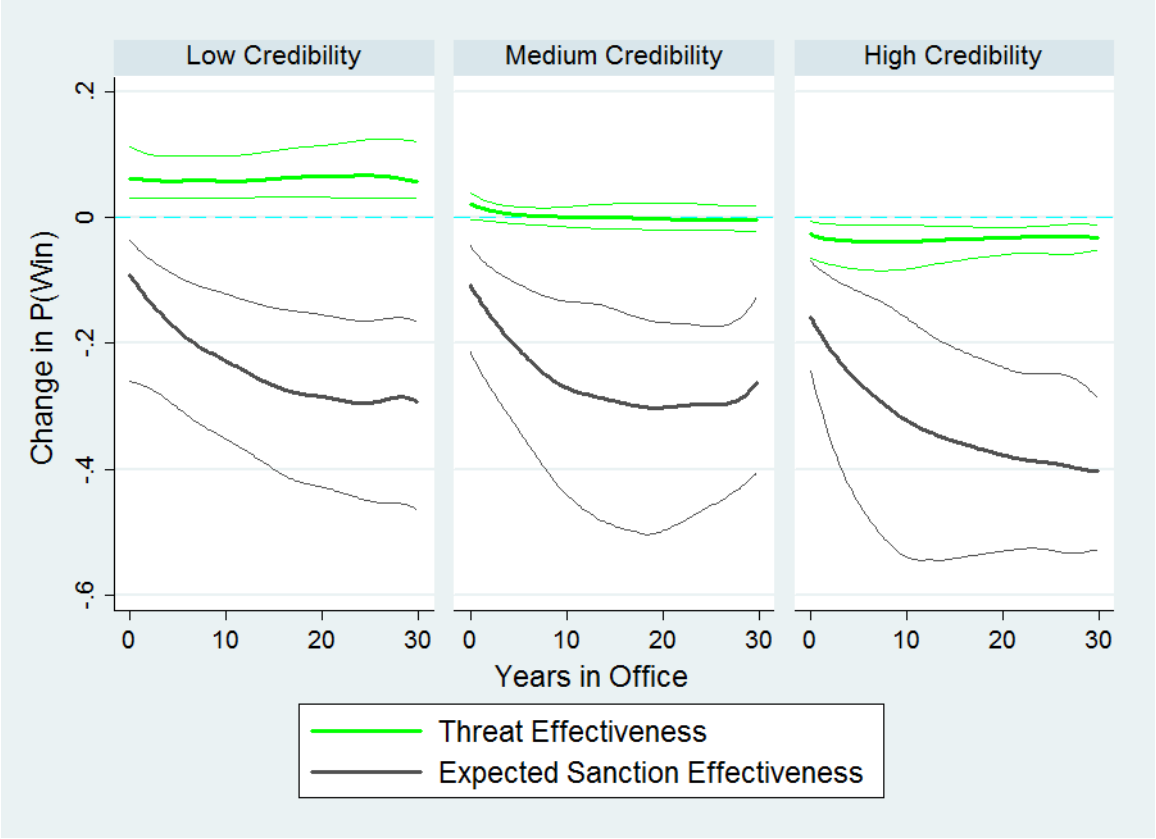

Figure 8.10 Difference in Probability of Sender Win at Threat Stage against Nondemocratic Leader by Effectiveness

NOTE: Figure charts change in probability generated by observed level of sanction effectiveness compared to baseline of no impact on target survival. Reported statistic is median calculation from simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 8.8. Thin lines mark 90% confidence bounds based on that distribution. Bright blue dashed line highlights the zero line.

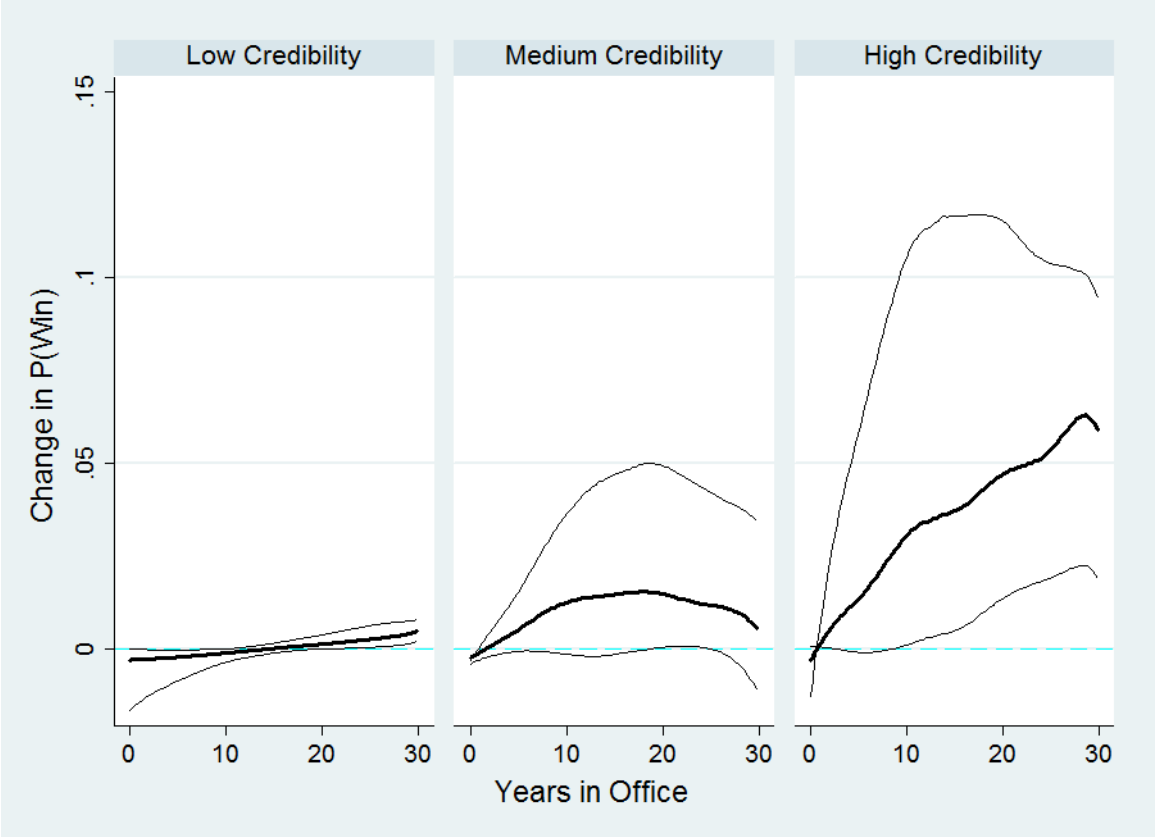

Figure 8.11 Difference in Probability of Sender Win after Sanctions against Nondemocratic Leader by Effectiveness

NOTE: Figure charts change in probability generated by observed level of sanction effectiveness compared to baseline of no impact on target survival. Reported statistic is median calculation from simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 8.8. Thin lines mark 90% confidence bounds based on that distribution. Dashed lines of corresponding color give the value of the effectiveness measures at given time and level of credibility. Bright blue dashed line highlights the zero line.

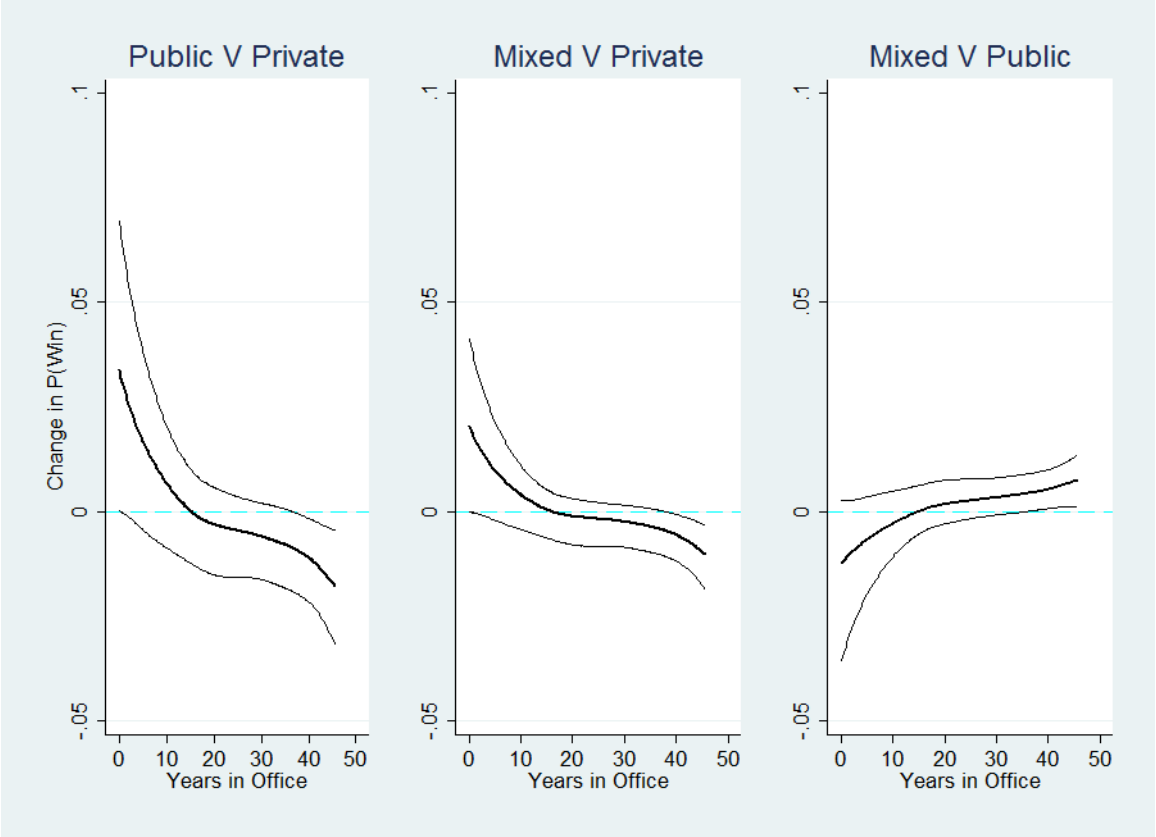

Figure 8.12 Difference in Probability of Sender Winning Concessions from Nondemocratic Leader at Threat Stage by Concession Type

NOTE: Figure charts median predicted change based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 8.8. Thin lines mark 90% confidence interval based on that distribution. Probabilities calculated with legislative branch indicator switched on.

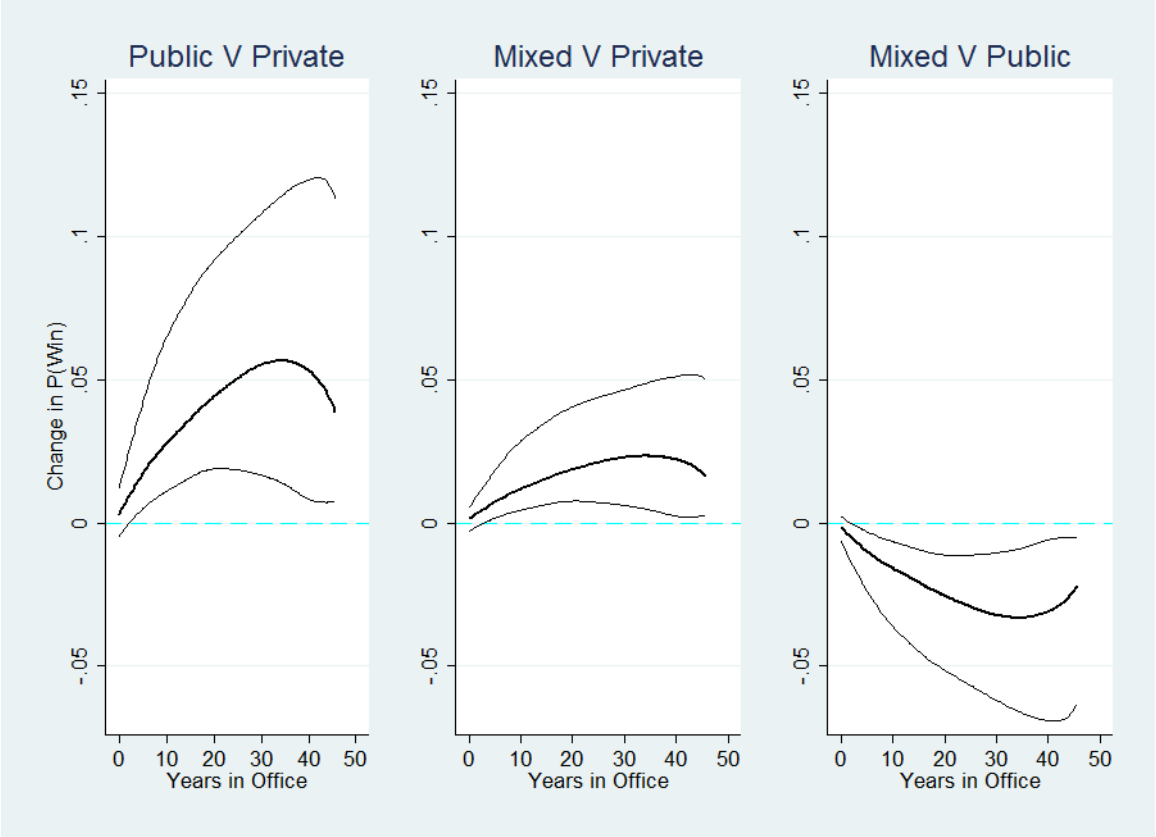

Figure 8.13 Difference in Probability of Sender Winning Concessions from Nondemocratic Leader after Imposition by Concession-Cost Type

NOTE: Figure charts median predicted change based on simulated distribution of 1,000 draws from the variance-covariance matrix of the bivariate probit reported in Table 8.8. Thin lines mark 90% confidence interval based on that distribution. Probabilities calculated with legislative branch indicator switched on.

CHAPTER 9

CONCLUDING REMARKS ON TARGET INCENTIVES AND FOREIGN POLICY SUCCESS

The last 344 pages have delved deeply into particulars. Testing the general argument made in Chapter 3 required adoption of auxiliary hypotheses, more nuanced expectations regarding the specific operation of foreign aid and economic sanctions. For some of these, statistical analyses uncovered strong support. For others, general patterns in the data provided partial confirmation. And for a few, evidence soundly contradicted prior expectations. Each of these findings hold interest in and of themselves, but too close a focus on the pieces may have obscured their contribution to the overall aim of the project. Discussion in this final chapter will review some of these key findings, but focus primarily on their relation to the broader model.

To evaluate the preceding analyses with respect to the general theory of influence via targeted leaders' incentives, I will summarize them in terms of the overall consistency with the proposed models of effectiveness, difficulty, and concession. In the introductory remarks, I made several bold claims, arguments for the theoretical and practical benefits of conceptualizing the success of foreign policy in terms of leader's incentives. Following the summarization of findings in terms of the general model, I will return to each of these claims. Have these innovations really provided the extra purchase which I expected them to? Given the findings here, do targeted leaders' incentives still deserve center stage in the development and refinement of international relations theory?

Tapping into the Survival Motive: Aggregate Findings

The assumption of survival-driven leaders defines the baseline incentives of every potential target of foreign policy. If staying in power is the primary motive, then accomplishing influence requires that foreign policy tools affect targets at this level.

Effectiveness of foreign policy tools, I argue, however, can be considered neither certain, nor constant across the array of potential and actual targets.

I made several fundamental claims about the impact of foreign policy tools on targets' most basic incentive, the probability of survival. This equation summarizes the majority of these claims:

$$E_s = c(I_s) + f(t_i) + f(t_i)(I_s) \quad \text{(Eq. 9.1).}$$

This function argues the effectiveness of a foreign policy tool (*s*) is mediated: 1) by the institutions of representation within the target (*I*), 2) by the credibility of the sending state (*c*), 3) by the experience of the targeted leader in office at the time of targeting ($f(t_i)$), and 4) by the institutionally determined meaning of that experience ($f(t_i)(I)$). To simplify further, the effectiveness of any specific tool should differ: by the regime type of targets, by the credibility of senders, with time against each target, and differently over time across regime type of targets. The arguments about leader experience represent the competing temporal dynamics of learning and winning coalition institutionalization. If leaders learn on the job, then experience will matter, but equivalently across regime types. If, instead, the more important dynamic is the cohesion of the leader's support base, then experience will factor differently into the effectiveness of policy tools across regime types.

In a second set of arguments regarding effectiveness, I also specified that the underlying nature of effectiveness was continuous, such that $-\infty \leq E \leq \infty$. Negative values of effectiveness obtain when a foreign policy reward hurts its target and when a foreign policy punishment helps its target. I expected that just as effectiveness would prime targets to grant concessions, cases of such ineffectiveness would increase recalcitrance. This line of thought produces an additional three claims: 5) sanctions may range from highly ineffective to highly effective; 6) effectiveness increases the probability of concession; 7) ineffectiveness decreases the probability of concession.

Table 9.1 summarizes the findings from Chapters 5 and 7's analyses of effectiveness with respect to the first four claims. These chapters uncovered intriguing dynamics in the impact of foreign aid and economic sanctions on targeted leaders' survival. Each row of Table 9.1 represents one of the claims made above. Shaded diamonds represent findings of a difference in direction of the policy tool's effect on leader survival; hollow diamonds, of a difference in the magnitude or significance. These findings, which I will walk through below, provide resounding support for the first four claims about effectiveness.

Extant theory has argued extensively that foreign aid should be of most instrumental use to nondemocratic leaders,[92] and that economic sanctions should be most damaging to democratic leaders.[93] My findings call both arguments into question, while providing support for my claims about effectiveness. As the shaded diamonds in the first row of Table 9.1 indicate, aid and sanctions do indeed operate differently across democratic and nondemocratic institutions. Foreign aid, however, actually provides its biggest benefits to democratic leaders; while economic sanctions prove most destabilizing against nondemocratic leaders.

If this were the full story, however, the diamonds in this first row would be consistently hollow, indicating only that the effects were larger or smaller across regime types. Rather, I uncovered fundamental differences in the impact of foreign aid and threats of economic sanction across different target institutions. The initial effect of foreign aid on democratic leaders is beneficial, driving down the risk of losing office by

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[92] For this argument, see Lai and Morey (2006) or Bueno de Mesquita and Smith (2007); an opposing view is expressed by Kono and Montinola (2009).

[93] See, for example, Letzkian and Souza (2003, 2007), Bolks and Al-Sawayel (2000) Kaempfer and Lowenberg (1999, 2000), Kaempfer, Lowenberg and Mertens (2004), Cox and Drury (2006), or Allen (2008b).

up to 60%; for nondemocratic leaders, the effect reverses, increasing the likelihood of removal by up to 30%.

Threatened economic sanctions, whose findings are summarized in the second column of Table 9.1, also operate differently by leader type. Democratic leaders, over the course of their careers, experience welcome reductions in domestic risk as a result of threats from external powers. This insulation ranges from 50-100%. Such threats against newly established autocrats, however, boost domestic risks by between 20 and 110%.

The third diamond relating to claim 1 is hollow. For enacted economic punishments, effectiveness differed across regime types in terms of magnitude rather than direction. Economic sanctions increase the probability of failure for new leaders of both types, but much more severely for nondemocratic leaders.

Findings relating to my second claim lay in the next row of Table 9.1. I argued that the credibility of sending states' threats and punishments would elevate effectiveness. This expectation could be tested only with respect to negative sanctions, given difficulty of distinguishing between promises of aid and actual allocations. The hollow diamonds in both columns indicate that this contention was largely supported across threats and sanctions. As Chapter 7's figures illustrated, the destabilizing impact of imposed sanctions became larger in magnitude and more statistically significant against all types of leaders as credibility increased.

In the case of threatened sanctions against democratic leaders, however, the inflationary impact of credibility meant a growing reduction of risks. Ironically, then, this result shows higher credibility threats to be more *ineffective* than those with less commitment from the sending state. This interesting finding suggests that democratic publics do not react favorably to interference from the outside. The more specific is a threat in terms of outlining the behaviors which the sending state disapproves of and the actions necessary to prevent punishment, the more will a democratic system reward its leader for withstanding external pressure.

The third and fourth claims refer to the competing hypotheses regarding temporal dynamics. If learning matters most, then claim three holds that the impact of foreign policy tools will matter over time within regime types but not differently across them. If instead, the loyalty of winning coalitions drives dynamics, then the pattern of temporal changes in effectiveness (i.e. increasing over time, or decreasing over time) will vary across regime types. The three shaded diamonds in row three of Table 9.1 tell us that the impact of all three policy tools varied in direction of effect for at least one type of leader.

The insulating impact of foreign aid on democratic leaders just reviewed above, for example, "wears off" within the time frame of most democratic executives' first term in office. After just about three years, aid begins to hurt its democratic recipients. This dynamic may be viewed in the career of many democratic aid recipients. John Kufour, president of Ghana from 2001-2009, for example, won his country increased aid flows by continuing the macroeconomic reforms suggested by outside powers. These increased aid grants may have contributed to his re-election in 2004. Rewards for his efforts culminated in a 2006 Millennium Challenge Corporation contract worth $547 million. Despite this international reward, Kufour's party faced a tough campaign two years later, losing both the presidency and their status as the biggest party in parliament. Kufour's long time political competitor, John Atta Mills achieved this victory and instated a government with an anti-corruption focus, according to U.S. State Department evaluations (U.S. State Department 2010).[94]

The initial impact of foreign aid on nondemocratic leader survival lasts considerably longer. Risks of autocratic leader failure while receiving elevated aid flows remains higher than average until the executive accumulates 17 years' experience, a

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[94] Other cases of democratic leaders who won large foreign aid awards only to be ousted or destabilized in the face of corruption scandals and opposition rallies may include: President Mwai Kibaki of Kenya (2002-present) and Prime Minister Chuan Leekpai of Thailand (1997-2001),

landmark obtained by only 13 percent of nondemocratic leaders between 1960 and 1999. Extremely long-lasting autocratic reigns, progressing beyond thirty years, may eventually allow leaders to benefit from aid. The rapid progression of executives in the Philippines from 1948-1965 may illustrate the higher domestic risks of aid-receiving nondemocratic leaders. During this time period, four leaders cycled in and out of office. Several of these turnovers featured breakdowns of internal administrations and jockeying for U.S. favor and foreign aid allocations (Root 2008, 86-102). The rapid turnovers ceased with the rise of Ferdinand Marcos, who dismantled the remaining pretenses of democratic institutions and consolidated power for 21 years.

For the positive sanctions examined in Chapter 5, then, the overall pattern of effectiveness differs in direction across regime types. Aid becomes more effective as autocratic recipients gain experience; less effective as democratic recipients endure in office. These opposing patterns point to the prominence of winning coalition cohesion. I indicate this support for the fourth claim regarding the nature of effectiveness with a shaded diamond in the first row of the fourth column of Table 9.1.

The findings from Chapter 7's analysis of negative sanction effectiveness, however, provide more support for the learning hypothesis in claim 3. Nondemocratic targets experienced both threats and imposed sanctions as declining in effectiveness over the course of their careers. The median estimated impact of these negative sanctions becomes negative, though statistically indistinguishable from zero, within a few years. For democratic targets, the same pattern holds under enacted sanctions, but not threats. I indicate this largely robust dynamic of sanctions' switch from positive to negative impacts on survival with shaded diamonds in the second and third columns of Table 9.3's third row. The relative consistency of this finding across democratic and nondemocratic targets also requires hollow diamonds in the fourth row of Table 9.1. While the impact of sanctions on autocrats is larger and lasts longer than that against democrats, it follows the same curve of decreasing effectiveness over time. The results for negative sanctions

then, meet the expectations of a learning process. Over time, leaders appear able to develop the skills necessary to shield themselves from the deleterious impact of international disapproval.

Compiling the evidence with respect to these four claims, then, demonstrates that expectations regarding the importance of institutions, credibility and leader experience received consistent support across the analyses of leader survival. Some of these findings presented challenges to specific auxiliary hypotheses and the extant claims of the literature, but these inconsistencies do not refute the general model of effectiveness reviewed in Equation 9.1.

Three claims about effectiveness remain to be evaluated. Claims five through seven state that each type of foreign policy tool possesses a continuous range of impact on leader survival from highly effective to highly ineffective, and that this range will relate to the likelihood of concession in the most straightforward way. Table 9.2 summarizes the results pertinent to these expectations. In this table, check marks indicate supportive evidence derived from the analysis. Dashed hollow circles mark cases where the pertinent findings failed to gain statistical significance. When the findings achieved significance, but contradicted the hypothesis, the cell will contain an ×. I allow the size of these markers to indicate variation in the strength of the results, but this extra nuance was utilized in only two cases which will be noted below.

Consider, first, the validity of claim 5, that each type of foreign policy held the potential to impact leaders' survival positively and negatively. The preceding chapters amply demonstrated the existence of ineffective sanctions. Foreign aid likely hurts a large proportion of recipients, while threats of economic sanction always insulate democratic leaders. Chapters 6 and 8, which tested the relationship between (in)effectiveness and the probability of concession provided considerably less support for the final claims about effectiveness.

For democratic leaders (see rows two and three of Table 9.2), the consistency of aid effectiveness with claim six depends on the type of concession demanded and the sending state's magnitude of commitment. Aid effectiveness did not relate significantly to increases in economic growth, my proxy for private-costs producing concessions. [95] In the model of UNGA voting behavior, which taps public-costs, insulating aid can sometimes produce concessionary behavior. A democratic recipient will move voting patterns towards those of a donor only if that donor's contributions represent only a small portion of the state's overall aid dependence. Donor states which indicate a high level of interest in the behavior of a specific recipient by financing the bulk of its overall aid portfolio will be stung. Perhaps acting to maintain at least the façade of independence and legitimacy, democratic leaders distance their countries' voting patterns from those of high volume donors. The smaller check mark in the first column of Table 9.2's second row reflects this very conditional kernel of support for claim six.

As reviewed extensively above, democratic leaders with more than three years' experience actually suffer destabilization as a result of high aid flows. Those who experience this ineffective aid, however, do not appear to punish donors for the backfire. During the period of experience when aid hurts, effectiveness exerts no significant impact on the change in UNGA voting patterns or economic growth. This is indicated by the empty circle in the first column of the fourth row in Table 9.2.

The (in)effectiveness of negative sanctions against democratic leaders also relates poorly to claims six and seven. I marked the cell for evaluating effectiveness of threats against democratic leaders with "NA", not applicable. Because threats never exert an "effective" impact on democratic leader survival, evaluating them in these terms would be unreasonable. Just below this cell, I assigned a small check mark to claim seven with

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[95] I will discuss the overall role of concession difficulty in the next section.

respect to threatened democratic leaders. Though threats always insulate, the magnitude of this ineffectiveness increases as leaders gain experience. During their earliest years in office, which correspond to the lowest levels of ineffectiveness, threatened democratic leaders provide more concessions than they would if threats had no impact on their probability of survival. This increase in probability of concessions is extremely large in relative terms, ranging from a 600% to 5,000% increase depending on sender credibility. This clearly contradicts the spirit of claim six – the probability of concession should be lowered by these ineffective threats.

We might interpret these findings as containing a glimmer of support for the claim afterall, if we consider the change in the probability of concession over time. Though the initial impact is enormous and counterintuitive, the elevated level of concessionary behavior given ineffective threats declines as leaders gain experience. As the level of insulation from threats increases, the difference in probability of concessions prior to sanction imposition eventually drops out of significance. The more ineffective the threat, then, the less likely is the relative likelihood of concession from a democratic leader. The final two cells relating claims six and seven to democratic leaders were assigned dashed circles; the effectiveness of imposed sanctions never achieved statistical significance in the concessions equations.

Less ambiguous support for claims six and seven can be found in the results for positive sanctions to nondemocratic leaders (see fourth and fifth rows of Table 9.2). For nondemocratic leaders, proclivity to provide concessions to foreign aid allocation also varied by the amount of aid dependence. Despite this, though, nondemocratic leaders proved more willing to provide concessions of both private- and public-cost types during the small portion of time when aid is most effective. I indicate this supportive evidence with a check mark in the first column of row four. Chapter 6 also illustrated a tendency of nondemocratic leaders receiving ineffective aid to punish their "benefactors" by moving their countries' UNGA voting patterns away, supporting claim seven.

The negative sanctions columns for nondemocratic leaders contain no direct support for claims six and seven. Nondemocratic leaders respond counter-intuitively to effective threats. Highly credible threats leveled against new autocrats should produce destabilization. Yet these effective threats actually drive down the probability of early settlement and of sender victory. Thus I include an × in the threats column for nondemocratic leaders. If we consider the relationship between ineffective threats and concession, results also look poor. Low credibility, ineffective threats increase the probability of early concessions by 1-5%.

I do not believe that the findings for threats against nondemocratic leaders are wholly nonsupportive, however. In the imposition models, I also included a measure for the "expected" effectiveness of imposed sanctions. This measure conveyed the expectation that after a short number of years, imposed sanctions will no longer significantly destabilize their autocratic targets. When we consider the impact of this knowledge, we see the probability of early concessions drop by up to 100%. While indirect, I believe this to be supportive evidence for claim seven, that ineffective threats should reduce likelihood of concessions. Thus I include an × for the direct impact of threat ineffectiveness and a check mark for the indirect impact of expected imposed sanction effectiveness in the cell for claim 7b.

The nonsignificant findings between imposed sanction effectiveness and concessionary behavior in the punishments column of Table 9.2 should not be surprising. Once imposed, economic sanctions exert a significant impact on democratic leader survival for only a very short period of time. Imposed sanctions against new nondemocratic leaders do create destabilization for several years. During this time, however, effectiveness does not relate significantly to concessionary behavior.

The final application of claim seven, that ineffective imposed sanctions should decrease the likelihood of concession from nondemocratic leaders, is directly contradicted by the evidence in Chapter 8. For highly experienced nondemocratic

leaders, the effectiveness measure of imposed sanctions takes on ineffective values. When the sender exhibits high levels of commitment, these ineffective scores actually increase the probability of offering concessions. Thus, I must assign an × to the final cell in Table 9.2.

Weaker results for claims about ineffectiveness, then, cluster in the negative sanctions columns. Here, the "backfire" makes targets safer, whereas misfires in positive sanctions create danger. The prediction that targets would sit back and relax when external actors accidentally insulate them, may have been naïve. While the incentive to punish outsiders for hurting when they mean to help seems straightforward, the analog does not need to be true. Benefiting from a policy action which was intended to harm may put new democratic leaders and experienced nondemocratic leaders in an interesting bargaining position. For both types of leaders, the time periods of unexpected increases in concessionary behavior correspond to times when internal security should be very high. This security buffer may allow them to trade domestic costs for future international favors.

In sum, then, the results across all four empirical chapters provide very strong support for claims about the factors which condition effectiveness of sanctions, but fairly weak support for the ultimate argument that effectiveness drives concessionary behavior. The impact of foreign influence attempts on leader survival varies across types of targets, and within targets over time. Effective rewards tend to increase the probability of concession, while ineffective ones either fail to return concessions or reverse their likelihood. Effective threats and punishments usually do not relate efficiently to the probability of concession. For nondemocratic leaders, the expected ineffectiveness of imposed sanctions may loom larger in decision making than the actual impacts of threats. In the next section, I will walk through a similar analysis of the results for the impact of domestic difficulty on likelihood of concession.

Domestic Hurdles to Concession: Aggregate Findings

The likely political difficulty of providing concessions marks the second major intersection of foreign policy tools with targeted leaders' incentives. Targets face internal constraints on their ability to change the status quo, because groups may organize to protect their interests and punish leaders who give in to external pressure. The likelihood of this domestic backlash, however, varies across targets and sender demands.

The bulk of this variance, I argued, stems from the likelihood that the demanded concession would create costs for a segment of the population which was likely to punish the leader. The experience of the targeted leaders and the strength of opposition parties should also play a role. These three elements combined to produce the following function:

$$D = \gamma_1 C_{private} + \gamma_2 C_{mixed} + \gamma_3 C_{public} + \gamma_4 (O \times C_{mixed}) + \gamma_5 (O \times C_{public}) + f(t_i) \quad \text{(Eq. 9.2)}$$

I ordered the weights, $\gamma_1 \geq \gamma_2 > \gamma_3 > 0$, to reflect that all concessions carry costs (***C***) but those which impact private groups result in more dissent than those which do not. Political organization proves easiest in the event of focused, private costs. So, concessions which would produce private costs should be more difficult for any leader to provide. I expected that the opposition would mediate the public's disadvantage, pulling up the domestic hurdle for mixed- and public-costs concessions. Finally, the accumulated experience of the leader should influence the barriers to concession as dynamics in expertise or winning coalition cohesion alter leaders' ability to withstand the protests of organized interests and opposition parties. To summarize then, the difficulty of offering concessions 1) decreases with proportion of public costs irrespective of target regime type, 2) increases with public costs when an opposition exists to fight for them, 3) decreases with leader experience, or 4) varies differently over time by regime type, costs and opposition strength.

Working with the basic assumption that increasing difficulty will produce lower proportions of success, I summarize the relevant results from Chapters 6 and 8 in Table 9.3. Statements about decreasing difficulty receive a check mark when the probability of concession rises with the stated variables' values; statements about increasing difficulty, when the probability of concession decreases. ×'s mark the cases where evidence contradicts the statements about difficulty. The pattern of results in Table 9.3, overall, suggests that the model of difficulty works very well in all but one respect.

The first claim about domestic difficulty is evaluated in the first and second rows of Table 9.3. In every instance where analysis allowed direct comparison of costs (namely the negative sanctions models), public-costs producing concessions appear easier to obtain. The only exception to this finding stems from Chapter 6's analysis of nondemocratic leaders' economic growth and UNGA voting affinity. Very small flows of foreign aid to very experienced leaders correlated with relatively large spurts in economic growth, while very large levels of dependence on a particular donor were necessary to create relatively smaller shifts in voting behavior. Given the difficulty of the test in the positive sanction arena, the overall picture looks strong despite this anomaly.

The results for the role of the opposition, however, do not support expectations and do not appear to be anomalous. Across the columns of Table 9.3, this second claim receives ×'s indicating direct contradiction of expectations. The failure of the opposition to reduce the gap between likelihood of public- and private-costs concessions proves the most consistent finding in the difficulty model. Across regime types and foreign policy tools, targeted leaders become even more likely to offer public- rather than private-costs concessions when they face some opposition at home. Instead of rallying other members of the "losing coalition" in attempts to overthrow the incumbent leader, this finding suggests that opposition groups spend their energy attempting to appeal to members of the existing winner's circle. When credible alternatives exist, leaders work extra hard to protect private-interests from costly concessions desired by external powers.

Finally, evidence pertinent to claims three and four, regarding the relative power of the learning and institutionalization arguments, is summarized in the last two rows of Table 9.3. Support for learning versus institutionalization differs across the positive and negative policy tools. In the foreign aid models, the difference between probability of concession by cost-type and opposition strength decreases for both democratic and nondemocratic leaders as they become more experienced. In the economic sanctions chapter, however, we saw that the role of opposition strength and cost-type was generally decreasing for democratic leaders and increasing for nondemocratic leaders over time.

Altogether, the results provide strong confirmation for arguments regarding the relative difficulty of hurting private interests compared to public interests. My fault in this model lay in the overly optimistic predictions about the ability of political competition to correct this imbalance. Influence attempts always stand better chance of success when the demanded concessions spare the privileged few. For states hoping to exercise their power in the international system, this finding may be discouraging.

The Value of Incentives-Based Theory

At the start of this project, I outlined several ways in which my approach would further both the theoretical and methodological study of influence in international politics. Now that we have reviewed the performance of my argument across models of positive and negative sanctions, it seems fitting to return to these claims and assess them in light of the results. The focus on incentives of targeted leader incentives drove me to pay special attention to the dual pressures on leaders' decisions to provide concession, to attempt direct estimation of the impact of influence attempts on targets, and to consider the potential similarities between positive and negative policy tools. Let us examine each of these steps in turn.

When external powers attempt to influence the policy choices of others, they further complicate an already multifaceted process. Targeted leaders must balance the

dual pressure of external and internal demands. My model explicitly acknowledged this balancing act in a number of ways, each of which, I believe, contributed to the overall understanding of influence success. Most obviously, I incorporated this in the two-part model of influence. Similar to an opportunity and willingness frame, I argued that while policy tools may tap appropriately into leaders' survival motives, domestic constraints make some types of concession simply more difficult to supply. The results summarized above provide resounding support for this contention. Effective rewards and punishments face an uphill battle when they seek changes which will damage private interests in the target country.

Paying closer attention to the role of pressures from below also indicated the importance of variation across *and* within the often dichotomized blocks of democratic and autocratic. Without recognizing the possibility of differences between the impact of foreign policy tools across regime types, we would have concluded that foreign aid never significantly changes the likelihood of targets' survival. Similarly, we would never observe the conflicting pattern of threats against democratic and nondemocratic leaders. In short, we would often erroneously assume that external pressure cannot tap into leaders' survival motives.

Simply differentiating between types of leaders, however, does not assure against this error. A simple indicator for democratic institutions would still report that foreign aid does not "work". Leaders' stability and competence change significantly over time, changing their vulnerability to external influence. Allowing for these temporal dynamics reveals the window during which external powers can be effective in their attempts to alter targets' leader incentives, even if that window turns out to be very small.

In the introduction I also boasted that my efforts to directly model the impact of foreign policy tools on leader survival would improve upon extant methods. These existing methods include interacting foreign policy tools with regime type and utilizing results from formal models to simply assume the relative strength of the impact across

regime types. My results demonstrate that these prior techniques obscured the mechanism behind willingness to offer concessions. In the foreign aid literature, Bueno de Mesquita and Smith (2007) have adopted the assumption that fungibility allows autocratic leaders to benefit more than democratic leaders. This assumption does not stand up to direct assessments, and so the proposed logic behind the observed patterns in aid allocation must be questioned.

The practice of regime indicator interactions (e.g. Lai and Morey 2006) will also obscure the true patterns of foreign policy impact and influence. Effectiveness varies not only by regime type, but with leader experience and sender credibility. Leaving out these pieces of the puzzle makes the old method imprecise and prone both to sometimes falsely reject hypotheses regarding the viability of foreign policy instruments and to sometimes falsely attribute causal power to an impact which does not exist. While the Lai and Morey (2006) conclusion that nondemocratic leaders provide more concessions in the UN does hold up, my techniques have demonstrated that the finding is driven by a small subset of very experienced autocrats. This attenuation of the claim can change policy recommendations: not all autocrats are more likely to cooperate in exchange for aid.

The final, and perhaps the broadest, claim I made for the focus on targeted leaders incentives was identifying the parallel process of influence using positive and negative tools. In many respects, the analyses herein have confirmed these parallels. The same broad factors of regime type and leader experience mediate the effectiveness of both foreign aid and economic sanctions. Concessions become more likely when they produce public costs whether the external actor chooses to pursue the change through punishments or rewards.

While these factors emphasize a similarity across the processes of reward and punishment, an important exception emerged in the ultimate impact of ineffective sanctions on the probability of concession. In this case, theorizing about rewards and punishments simultaneously may have obscured a practical matter: being accidentally

destabilized may be quite different from being accidentally insulated. When an external power hurts those it means to help, the strategic exchange may stall with the recipient of the booby-trapped reward simply defecting. An external power which sets out to hurt and instead helps may sometimes find itself dealing with an unexpectedly gracious target. Confirming this difference requires analysis of additional foreign policy punishments to ensure that the finding is not particular to ineffective economic sanctions.

The failing here, if uncovering an interesting anomaly may be called such, does not stem from the focus on leader incentives. If anything, we might blame it on a lack of fully following through on the commitment to take targets' preferences seriously. When they have the leeway to do so, many leaders may prefer to gain some leverage over an external power by providing concessions even when the influence attempt did not "work". In broader terms, the research design and theoretical frame as pursued here did not allow for the investigation of issue-linkages between sending and targeted states. This seems to be a natural next step for a leader-incentive based approach to foreign policy.

Conclusion

The results above square in many ways with the dismal projections for the success of both foreign aid and economic sanctions. Foreign aid operates effectively for only 50% of democratic and 2% of nondemocratic recipients. Even when aid works, though, its efficient operation does not always promote concessionary behavior. We can say the same for economic sanctions, though they prove effective against an even smaller portion of targets. The process of exercising influence from the outside requires both timing and restraint. In these final remarks, I would like to outline the situations and strategies for the best probability of successful influence.

Across tools of influence, types of targets and the costs of concessionary demands, outside powers stand the best chance of getting their desired outcome when

they set their sights on leaders with strong domestic coalitions. This statement directly contradicts the logic of the selection argument which I tested in Chapters 5 and 7. Sending states target leaders with elevated risks of losing office, yet the impact of sanctions on leader survival produces the biggest gains in probability of concession against new democratic leaders and long-established autocratic leaders. These leaders benefit from the allocation of foreign aid and provide policy concessions in return. They also benefit –or at least suffer no harm – from targeting with the threat or imposition of economic sanctions, and they provide concessions in these cases as well. Fundamentally, these are the leaders whose winning coalitions feature the highest level of cohesion. For democrats this cohesion stems from the legitimizing process of electoral institutions; for autocrats from the accumulation of private benefits over time. With winning coalitions so strong and the likelihood of losing office so low, new democratic and established nondemocratic leaders can afford to undertake costly concessions when external actions provide them some extra safety.

Even a well timed influence attempt, however, can be disrupted by other factors. For effective aid to put democratic leaders in a concessionary mood, it must be discrete. Donors who maneuver to become the primary source of a state's aid dependence will find their democratic beneficiaries less grateful than might be expected. Elevated aid dependence forces these leaders to distance themselves from donor states, resulting in an influence backfire despite good timing. The opposite problem holds for nondemocratic aid-recipients. Because they do not need to appear legitimate in the same way as democrats, these leaders reward donors who jockey for the primary donor status.

States hoping to influence others through policy tools of this caliber would also do well to moderate their aims. Concessions which promise to produce only diffuse or

symbolic costs prove simpler to win, regardless of the type of target.[96] This recommendation places a serious limit on the scope of states' power to alter each other's behavior. Many may say that a tool which cannot illicit difficult concessions is not a tool of power at all. This problem harkens back to Baldwin's (1971) argument regarding the fair evaluation of foreign policy actions. Serious variation exists in the difficulty of tasks set forth in the international arena, and we must consider this range when we set out to determine whether specific tools are worth using.

Economic sanctions, for example, have been called upon most often to deal with the most difficult situations – private-costs concessions. Their extremely poor track record owes much to the height of this initial hurdle. The chances for success appear much higher for sending states who seek public-costs changes to the status quo. Economic sanctions can work. But as tools of influence, they have serious limitations. These limitations mean they should be fazed out of use in many areas where they have become prominent. Most notably the disruption of autocratic regimes, human rights violations, and nuclear weapons development all fall under the private and mixed costs categories in which sanctions are most likely too weak to affect change. Threats to pile extra sanctions atop the government of Iran in response to its continuing nuclear program, for example, will not prove efficacious. My results suggest that a more successful strategy may actually be to reverse existing sanctions and grant Iran some "ineffective" foreign aid.

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[96] The sole exception to this general rule surfaced in the economic growth model for nondemocratic leaders.

Table 9.1 Summary of Findings Regarding Theory of Foreign Policy Effectiveness

	Evidence by Sanction Type		
		Negative	
Claims about Effectiveness	*Positive*	*Threats*	*Punishments*
1) Differs by Regime Type of Target	♦	♦	◊
2) Differs by Credibility of Sender	--	◊	◊
3) Differs with Experience of Target within Regime Type	♦	♦	♦
4) Differs with Experience of Target across Regime Type	♦	◊	◊

♦ Indicates difference in direction of Effectiveness

◊ Indicates difference in magnitude or significance of Effectiveness

NOTE: Table summarizes results from Chapter 5 and Chapter 7 across big and small winning coalition systems. Positive sanctions are foreign aid and negative sanctions include threatened and imposed economic sanctions. Highest level of support across these regime types is reported in row three. In democratic systems, threats vary only in magnitude over time; in nondemocratic systems, punishments vary only in significance over time.

Table 9.2 Summary of Findings for Effectiveness and Probability of Influence Success

Claims about Effectiveness and Probability of Concession	*Evidence by Sanction Type*		
		Negative	
	Positive	*Threats*	*Punishments*
5) Effectiveness Obtains Positive and Negative Values	✓	✓	✓
6a) Probability of Concession by Democratic Leaders Increases with Effectiveness	✓	NA	◌
7a) Probability of Concession by Democratic Leaders Decreases with Ineffectiveness	◌	✓	◌
6b) Probability of Concession by Nondemocratic Leaders Increases with Effectiveness	✓	×	◌
7b) Probability of Concession by Nondemocratic Leaders Decreases with Ineffectiveness	✓	× ✓	×

✓ Indicates findings consistent with statement

× Indicates findings inconsistent with statement

◌ Indicates nonsignificant findings

NOTE: Point 5 summarizes findings from Chapters 5 and 7, which test the impact of positive sanctions, measured as foreign aid, and negative sanctions, measured as threatened and imposed economic sanctions, on targeted leaders' survival probability. Points 6a-7b summarize findings from Chapter 6 and 8 which test the probability of concession given the effectiveness of these positive and negative sanctions. Threats of economic sanction never effectively impact democratic leaders, hence the claim of increasing concession probability with effectiveness is not applicable (NA). Also, for democratic leaders positive sanction effectiveness achieved statistical significance only in the public-costs concessions model of UNGA affinity.

Table 9.3 Summary of Findings Regarding Domestic Difficulty and Probability of Concession

	Evidence by Sanction Type		
		Negative	
Claims about Difficulty of Concession	*Positive*	*Threats*	*Punishments*
1a) Decreases with Public Element of Costs in Democratic Systems	✓	✓	✓
1b) Decreases with Public Element of Costs in Nondemocratic Systems	×	✓	✓
2) Increasing with Opposition Strength if Public Costs	×	×	×
3) Role of Opposition Strength and Cost-Type Decreasing Over Time	✓	×	×
4) Role of Opposition Strength and Cost-Type Varying over Time by Regime Type	×	✓	✓

✓ Indicates findings consistent with statement

× Indicates findings inconsistent with statement or nonsignificant

NOTE: Table summarizes findings from Chapters 6 and 8, which analyze the likelihood of concession to foreign aid and economic sanctions. "Increasing" difficulty here means that the probability of concession declined; "decreasing", that it rose.

APPENDIX A

BREAKDOWN OF EQUATIONS AND ESTIMATION PROCEDURES

In this appendix, I will walk through the models estimated in the empirical chapters, presenting the equations and the order in which they were estimated. These equations are more complete than those listed in Chapter 4, as they explicitly state controls. Before jumping into the specific models, I will outline the general procedure for testing each piece of the argument.

My hypotheses require that I test:

1. The relationship between domestic institutions of representation and leader experience and the impact of sanctions on leader failure (i.e. "effectiveness");
2. The relationship between probability of leader failure and the targeting of sanctions/allocation of aid;
3. The impact of effectiveness on concessionary behavior;
4. The impact of concession difficulty on concessionary behavior;
5. The impact of political opposition and leader experience on concessionary behavior.

In order to evaluate each of these aspects empirically, I first perform event history analyses of leader failure which include interactions to determine the importance of institutions and leader experience, and which account for the possibly endogenous relationship between targeting and leader risks. These analyses allow me to produce measures of effectiveness, $\hat{E}_i$, which are then incorporated into models of concessionary behavior.

In the event history analyses, I must address the possibility of a nonrandom process, whereby the strategic planning of sending states leads to a population of targeted leaders whose prior risks of losing office are higher than those of the general population of leaders. I deal with this possibility by estimating an auxiliary regression of leader failure which includes all relevant leader years and generating an instrument, $\hat{Y}_{1it}^{**}$, which

is the estimated latent probability of failure for all leaders. This instrument is included in the models of sanction targeting and aid allocation. Maddala (1986) recommends that the predicted values from the auxiliary regression be adjusted by their standard errors when used in simultaneous estimation. For the foreign aid equations, when the simultaneous estimation of the targeting and survival equations proved warranted based on Wald tests of independent equations, I utilized this procedure. In the sanctions equations, the Wald test did not support joint modeling. In Chapter 7, therefore, I present two probit models, one of sanction targeting and one of leader failure. The sanction targeting model includes a simpler instrument for the probability of leader failure, just the linear prediction from the leader failure model. As advised by Murphy and Topel (1985) I adjust the variance matrix of that probit to reflect the additional estimation uncertainty introduced by use of this predicted value.

When estimating the models of leader failure, I perform a bootstrapping procedure to improve confidence in the parameter estimates. Bootstrapping repeatedly draws samples of size N with replacement from the observed dataset. The model is estimated on each of these bootstrap samples, and then the estimates are averaged across the samples. This produces estimates which are ensured against the influence of outliers and the particular observations include in the sample. Bootstrapping does not incorporate the additional estimation uncertainty from including the estimated instrument of leader failure in the "selection" stage of the jointly estimated model.

Now I will present the estimated equations for the tests of my theory in terms of positive and negative sanctions.

Estimating the Effectiveness of Positive Sanctions

In the tests of foreign aid effectiveness, I begin with an auxiliary regression of leader failure which includes all aid-eligible leaders from the period 1960-1999, Y_{0it}:

$$P(Y_{0,it} = 1|X) = \Phi(\alpha - \beta_{01}\mathrm{A_{it}} - \beta_{02}\mathrm{W_{it}A_{it}} - \beta_{03}\mathrm{f(t_I)A_{it}} - \beta_{04}\mathrm{f(t_i)W_{it}A_{it}} + \beta_{05}\mathrm{W_{it}} + \beta_{06}\mathrm{f(t_i)} + \beta_{07}\mathrm{Growth_{t-1}} + \beta_{08}\mathrm{Trade_{t-1}} + \beta_{09}\,\mathrm{CivCon} + \beta_{010}\mathrm{SOUTHAM} + \beta_{011}\mathrm{SUBAFR} + \beta_{012}\mathrm{SOUTHASIA} + \varepsilon_0) \quad \text{(Eq. A1)}$$

Where, A_{it} is the aid dependence of leader i at time t; W_{it} is an indicator for if winning coalition scale in leader i's state at time t is greater than or equal to .75; $f(t_i)$ is the natural log of leader i's cumulative days in office; Growth is natural log of lagged economic growth; Trade is natural log constant dollar value of lagged total trade with other states; CivCon is the intensity of civil conflict at time t; and SOUTHAM SUBAFR and SOUTHASIA are indicator variables for the regions South America, Sub-Saharan Africa and South Asia, each of which were significant. For more information on the coding of these variables see Table 4.1. This model achieves a high level of fit, with an expected percent correctly predicted (ePCP) of 84.5%, indicating that the instrument constructed from these estimates should perform well.

I construct the endogenous instrument for leader failure as:

$$Y_{1it}^{**} = (\boldsymbol{X_{it}}\widehat{\boldsymbol{\beta}}_{\boldsymbol{0}})/\sigma_0 \quad \text{(Eq. A2)}$$

Where X_{it} are the covariates listed above, $\widehat{\boldsymbol{\beta}}_{\boldsymbol{0}}$ are the beta estimates from the probit regression described in Equation A1, and σ_0 is the standard error of the prediction from that regression.

Next I estimate the model of aid allocation, Y_{2it}, including this instrument:

$$P(Y_{2it} = 1|X, Y_{1it}^{**}) = \Phi(\alpha_2 + \gamma_{21}Y_{1it}^{**} + \beta_{21}Y_{2it-1} + \beta_{22}Popgrw_{t-1} + \beta_{23}Imports_{t-1} + \beta_{24}Pop_t + \beta_{25}Growth_{t-1} + \beta_{26}w_{it} + \beta_{27}Colony + \beta_{28}Ally_t + \beta_{29}Oil + \beta_{210}IntCivCon_t + \varepsilon_2) \quad \text{(Eq.A3)}$$

Where Y_{2it-1} is a lagged dependent variable; Popgrw is lagged population growth; Imports is natural log of constant dollar value of last year's imports from OECD donor states; Pop is the natural log of population size in thousands; Growth is lagged economic growth; w_{it} is the winning coalitions size scale; Colony is an indicator for whether leader i's state was once a colony; Ally is an indicator for holding a defensive or offensive alliance with an

OECD donor at time t; Oil is an indicator for holding oil reserves; and IntCivCon is an indicator for the occurrence of internationalized civil conflict at time t. These covariates control for several explanations of aid allocation: inertia, need, power, strategic interest. My contribution to the model of allocation is the instrument for leader failure, which tests whether the perceived willingness to offer concessions in exchange for external support drives allocation patterns. I run this model with and without the instrument to determine whether its inclusion is important and statistically supportable. Bayesian information criterion tests provide strong support for including the instrument ($BIC_{inst} - BIC_{noinst} = 17.155$).[97] The model of aid allocation performs extremely well, achieving an expected percent correctly predicted of 96.83%.

The next step in the modeling process is to link the processes of aid allocation and leader failure. The risks of leaders do contribute significantly to the probability of allocation, creating a pool of aid recipients whose ex ante risks of failure are higher than those who do not receive aid. This relationship may make it difficult to uncover any insulating effect of aid: if aid is allocated primarily to leaders whose risks are higher than normal, a simple estimation of aid's impact on leader failure is likely to tell us that aid hurts leaders. Aid, however, may be helping some of these higher risk leaders to hang on to power for longer than they would be able to otherwise. Compared to leaders who do not face these higher initial risks, the aid-receivers look worse off. To fairly evaluate the extent to which aid helps or hurts then, we must take into account these initial disadvantages and then compare aid-receivers to each other.

I accomplish this through a censored probit with the endogenous instrument of leader failure in the "selection" stage. To produce meaningful results, a censored probit

[97] At this point, one may worry about the lack of a control for duration dependence in this model. In Appendix B I present alternative specifications accounting for duration dependence in terms of leaders' tenure and in terms of the years since aid was first received.

model requires an exclusion restriction: at least one covariate in the "selection" stage must be absent from the "outcome" stage, in this case, the leader failure model. This requirement is easily met in these models, as the factors used to model aid allocation are almost completely dissimilar from the factors utilized to model leader failure.

The censored probit estimated holds the following form:

$$\mathrm{P}\left(\mathrm{Y}_{1,\mathrm{it}}=1\middle|\mathbf{X},\mathrm{S}_{\mathrm{it}}^{\mathrm{p}}=1\right)=$$
$$\Phi_2\begin{pmatrix}\alpha_1-\beta_{11}\mathrm{A}_{\mathrm{it}}-\beta_{12}\mathrm{W}_{\mathrm{it}}\mathrm{A}_{\mathrm{it}}-\beta_{13}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{A}_{\mathrm{it}}-\beta_{14}\mathrm{f}(\mathrm{t}_{\mathrm{i}})\mathrm{W}_{\mathrm{it}}\mathrm{A}_{\mathrm{it}}+\boldsymbol{\beta}\mathbf{Z}+\varepsilon_1, \\ \alpha_2+\gamma Y_{1,it}^{**}+\boldsymbol{\beta}\boldsymbol{V}+\varepsilon_2,\ \rho\end{pmatrix}$$

(Eq. A4).[98]

Where, $Y_{1,it}$ is an indicator for failure of leader i at time t; and $Y_{2,it}$ is an indicator for nonzero, net aid allocation to leader i at time t. The $Y_{1,it}$ equation, therefore, is estimated only on the population of leaders for which a positive aid allocation decision was made.[99] Φ_2 represents the cumulative bivariate Normal distribution. All covariates, including controls summarized in vectors **Z** and **V**, are as described in the constituent equations above. The errors across the two equations are assumed to belong to a joint Normal distribution with mean of zero, variance of one and covariance ρ. I estimated these equations jointly using the censored probit routine in Stata10, clustering errors on the 793 leaders in my aid-eligible sample.[100]

The inclusion of the instrument for leader failure adds extra uncertainty to this estimation, which is not accounted for in the censored probit routine. When I applied Murphy-Topel corrections to the variance structure of the independent probit equation of

---

[98] I adopt notation similar to that used to describe the log likelihood function of the censored probit model in Reed (2000) and Timpone (2002).

[99] The model could also be estimated as an endogenous bivariate probit without dropping cases of non-recipients in the outcome stage. See Appendix B for these results. The same general patterns hold for all important theoretical variables when estimated in this fashion, but as I expected, the inclusion of non-recipients attenuates the combined effects of aid on leader survival over time.

[100] Stata command <`heckprob`>.

aid allocation, however, the standard errors did not increase (see estimates reported in Table A1 below). This is likely due to the lack of overlap in covariates across the equation utilized to estimate the instrument and the model of allocation, and assures me that the estimates in the censored probit should be trustworthy.

To guard against outlier bias and produce a generally more robust vector of parameter estimates for the censored probit model, I next applied a bootstrapping procedure. I drew 1,000 samples of size N, clustering on leaders, from the observed dataset. The system of equations A4 was estimated in each of these samples, and the bootstrap standard errors and average coefficient estimates across the samples were saved. This procedure produced the estimates utilized in the measure of effectiveness.

To generate the measure of effectiveness in the models of concessionary behavior, I merged the vector of $\widehat{\boldsymbol{\beta}}$ into the concessions datasets. I used the parameter estimates to calculate $\hat{E}_i$ as the first derivative of the linear index of the outcome equation of Eq. A4 with respect to aid dependence. This is:

$$\partial \boldsymbol{X_{it}\beta_1}/\partial A_{it} = \beta_{11} + \beta_{12}\mathrm{W_{it}} + \beta_{13}\mathrm{f(t_i)} + \beta_{14}\mathrm{f(t_i)W_{it}} \qquad \text{(Eq. A5)}$$

Where W_{it} is the big winning coalition indicator for leader i at time t in the concessions data, and $f(t_i)$ is the natural log of leader i's cumulative days in office in the concessions data.

Modeling Concessions to Foreign Aid

In the final steps of the analysis, I incorporate the estimated effectiveness of foreign aid into models of two types of potential concessionary behavior: economic growth as a proxy for economic concessions, and changes in UNGA voting affinity as a proxy for strategic concessions. The economic concessions fit my typology of private concessions, as reforming the macroeconomic structure assaults vested interests which have profited from distortions and inequities in the market. Trading votes in the UNGA typifies a public-cost concession as it is a largely symbolic move. Thus I have two

models of concessionary behavior, each with continuous dependent variables. I estimate them simply using OLS regression. The models include the effectiveness measure as well as variables tapping into domestic political dynamics. Opposition strength and leader experience will interact to condition the difficulty of offering concessions of differing type. Because the effectiveness takes on opposite slope and opposition strength must be measured differently in the democratic and nondemocratic samples, I perform the regressions in each subsample rather than pooling leader types.

The UNGA voting data is summarized in Table 4.7. It contains a set of OECD donor-recipient dyads for 1960-1999. The dataset contains only aid recipients, but there is not necessarily a transfer of aid between each specific donor and recipient pair. In 55.62% (19,626 dyad years) of democratic observations and 62.65% (40,848 dyad years) of nondemocratic observations no transfer of official development assistance takes place between the donor and potential recipient. The dataset thus contains both dyads which feature positive sanctions and dyads which feature no positive sanctions. The data does not contain leaders who were completely shut out by every one of the OECD donors, but it does contain leaders with whom specific donors decided not to deal.

The UNGA, or public-costs concessions, model takes the form:

$$C_{public} = \beta_1 A_{ijt} + \beta_2 \widehat{E_{it}} + \beta_3 A_{ijt} \hat{E}_{it} + \beta_4 O_{it} + \beta_5 f(t_i) + \beta_6 O_{it} \times f(t_i) + \mathbf{\beta Z} + \varepsilon$$

(Eq. A6)

Where A_{ijt} is the share of recipient i's total aid dependence which stems from donor j's aid allocations and $\hat{E}_{it}$ is the measure of aid effectiveness for leader i at time t. I interact these two variables to assess how the effectiveness of the sanction impacts its ability to produce concessions, and vice versa how the commitment level of the sender conditions the impact of effectiveness on the outcome. O is the measure of opposition strength in leader i's state at time t. For the democratic leaders sample, this is the seat share of

opposition parties; for nondemocratic leaders, it is an indicator for the existence of a legislative branch. The control variables noted in vector **Z** are summarized in Table 4.7.

For the growth model, I have a dataset of all aid-receiving leaders from 1960-1999 for which information on included variables is available. The model takes the form:

$$C_{private} = \beta_1 A_{it} + \beta_2 \widehat{E_{it}} + \beta_3 A_{it} \hat{E}_{it} + \beta_4 O + \beta_5 f(t_i) + \beta_6 O \times f(t_i) + \boldsymbol{\beta Z} + \varepsilon$$

(Eq. A7)

Mimicking, the public-costs model, this regression includes the effectiveness measure of aid, aid dependence, and the interaction of the two. To tap the domestic political difficulty of offering the concessions given opposition strength and leader experience, I include the same variables as discussed prior. The controls in vector ***Z*** are summarized in Table 4.8. I also bootstrapped the parameter estimates of these two regressions.

Estimating the Effectiveness of Economic Sanctions

To estimate the effectiveness of economic sanctions in Chapter 7, I begin with an auxiliary probit regression of the failure of leader survival estimated on the full sample of leader years from 1971-1999:

$$P(Y_{0,it} = 1|\mathbf{X}) = \Phi(\alpha - \beta_{01}\mathrm{S_{it}} - \beta_{02}\mathrm{W_{it}S_{it}} - \beta_{03}\mathrm{f(t_i)S_{it}} - \beta_{04}\mathrm{f(t_i)W_{it}S_{it}} + \beta_{05}\mathrm{cS_{it}} - \beta_{06}\mathrm{cW_{it}S_{it}} - \beta_{07}\mathrm{cf(t_i)S_{it}} - \beta_{08}\mathrm{cf(t_i)W_{it}S_{it}} + \beta_{09}\mathrm{T_{it}} - \beta_{010}\mathrm{W_{it}T_{it}} - \beta_{011}\mathrm{f(t_i)T_{it}} - \beta_{012}\mathrm{f(t_i)W_{it}T_{it}} + \beta_{013}\mathrm{cT_{it}} - \beta_{014}\mathrm{cW_{it}T_{it}} - \beta_{015}\mathrm{cf(t_i)T_{it}} - \beta_{016}\mathrm{cf(t_i)W_{it}T_{it}} + \beta_{017}\mathrm{W_{it}} + \beta_{018}\mathrm{c} + \beta_{019}\mathrm{f(t_i)} + \boldsymbol{\beta Z} + \varepsilon_0)$$

(Eq. A8).

Where $Y_{0,it}$ is an indicator for leader exit; S_{it} is an indicator for implemented sanctions against leader i at time t; Wit is an indicator for big winning coalitions; $f(t_i)$ is the natural log of leader i's cumulative days in office; c is the credibility scale ranging from zero if no threat or sanction was implemented against leader i at time t, to 1 if the sanction/threat was highly credible; T_{it} is an indicator for a threat of economic sanctions against leader i at time t. The vector of control variables ***Z*** can be viewed in Table 4.4. Notably, the

controls contain variables tracking the conditional impact of aid dependence tested in Chapter 5.

The auxiliary model correctly places 80.64% of leader exits, a fit slightly less impressive than that achieved by the auxiliary regression of aid-eligible leaders. The sample of all leader years contains more variation in types of leaders and types of failures. Also, the dependent variable utilized in these analyses is not as efficient as I have not yet coded the winning coalition failure variable for all leader years. Despite these issues, the model still performs fairly well. I use the regression parameters to generate an instrument for the probability of leader failure, as described above. I include this measure in the model of sanction targeting, for which the dependent variable is an indicator which equals one if either S_{it} or T_{it} is one. The probit model of targeting is:

$$P\left(Y_{2,it} = 1 \middle| \mathbf{X}, Y_{1,it}^{**}\right) = \Phi\left(\alpha + \gamma Y_{1,it}^{**} + \beta_{21} Y_{2it-1} + \beta_{22} FrmrColony + \beta_{23} MID_{it} + \beta_{24} Rev_{it} + \beta_{25} CivConfLev_{it} + \beta_{26} w_{it} + \beta_{27} A_{it} + \beta_{28} Trade_{it-1} + \beta_{29} Pop_t + \beta_{210} CINC_{it} + \varepsilon_2\right) \quad \text{(Eq. A9).}$$

Where $Y_{2,it}$ is the targeting indicator; $Y_{1,it}^{**}$ is the instrument of leader failure estimated as the linear prediction of Equation A8 divided by the standard error of the prediction. *FrmrColony* is an indicator for whether the state was once a colony; MID_{it} indicates the involvement of state i in a militarized interstate dispute; Rev_{it} switches on if the state was the revisionist actor in a MID. $CivconfLev_{it}$ is the intensity of civil conflict. The size of winning coalition in i is indicated by wit. A_{it} is the ratio of net aid allocation to state i in year t over GDP in that year. $Trade_{it-1}$ is the natural log of the dollar value of imports and exports to and from state i in the prior year. Pop_t is the natural log of the population of state i in year t. $CINC_{it}$ is the Correlates of War concentration of power measure for state i in year t. These covariates are described in more detail in Table 4.2.

To determine the appropriateness of including the instrument for leader failure, I ran Bayesian Information Criterion tests. These tests provided very strong support for accounting for leaders' risks of losing office in the model of targeting ($BIC_{inst} - BIC_{noinst}$

= 205.3). The significance of the coefficient for the leader failure instrument and the results of the BIC test suggest an endogenous relationship between targeting and leader failure which should be modeled. I attempted this with a process parallel to that in the models of foreign aid above. However, the correlation between the errors of the two equations did not achieve significance and the Wald test of independent equations returned a non-significant $\chi 2$ score.[101] Therefore, I proceed with separate models.

I adjust the variance-covariance matrix of Equation A9 using a Murphy-Topel procedure to account for the additional uncertainty introduced by the estimated instrument. This inflates some of the standard errors, but not enough to alter conclusions about the significance of variables' impacts. To produce stronger and more reliable estimates of the coefficients in Equations A8, I applied a bootstrapping procedure. I drew 1,000 samples of size N from the observed dataset of leader years. The model was estimated on each sample and the mean estimates from the 1,000 runs are utilized to interpret the results.

The findings in this model were unexpected, demonstrating a greater impact on nondemocratic leader survival than democratic. As this goes against findings in the literature, I investigated further. The primary reason for this difference, I assumed, is the new body of sanctions included in the TIES dataset. Older analyses have excluded sanctions emergent from trade or business disputes from the definition of "economic sanctions". TIES does not. These trade-based sanctions are more numerous than the traditional strategic type and are much more likely to be aimed against democratic than nondemocratic leaders. To see whether this explains the unexpected results, I recoded S_{it} and T_{it} to exclude these types. I did not alter the credibility scale, however, allowing this

[101] The insignificance of the ρ and failure of the Wald test holds whether I utilize a censored probit or an endogenous bivariate probit.

variable to hint at variations in the probability of leader survival from the trade sanctions. This second equation then was specified as:

$$P(Y_{1,it} = 1|\boldsymbol{X}) = \Phi(\alpha - \beta_{11}S_{it}^{\sim tr} - \beta_{12}W_{it}S_{it}^{\sim tr} - \beta_{13}f(t_i)S_{it}^{\sim tr} - \beta_{14}f(t_i)W_{it}S_{it}^{\sim tr} + \beta_{15}cS_{it}^{\sim \boldsymbol{tr}} - \beta_{16}cW_{it}S_{it}^{\sim tr} - \beta_{17}cf(t_i)S_{it}^{\sim tr} - \beta_{18}cf(t_i)S_{it}^{\sim tr} + \beta_{19}T_{it}^{\sim tr} - \beta_{110}W_{it}T_{it}^{\sim tr} - \beta_{111}f(t_i)T_{it}^{\sim tr} - \beta_{112}f(t_i)W_{it}T_{it}^{\sim tr} + \beta_{113}cT_{it}^{\sim tr} - \beta_{114}cW_{it}T_{it}^{\sim tr} - \beta_{115}cf(t_i)T_{it}^{\sim tr} - \beta_{116}cf(t_i)T_{it}^{\sim tr} + \beta_{117}W_{it} + \beta_{118}c + \beta_{119}f(t_i) + \boldsymbol{\beta Z} + \varepsilon_1) \quad \text{(Eq. A10).}$$

Where $S_{it}^{\sim tr}$ and $T_{it}^{\sim tr}$ are the sanction and threat indicators recoded to exclude the trade sanctions. The constitutive term for credibility, c, given the interactions with these indicators, contains information about leaders' probability of failure given the use of trade-based sanctions.

I estimated the probability of failure for leaders targeted with strategic and trade-based sanctions and found that the trends for those facing trade sanctions looked more similar to that of those facing none at all. On this basis, I estimated a third probit model which recoded even the credibility scale to exclude these types of sanctions. The overall findings, however, remained basically the same despite these changes. Rather than pretending then, that trade sanctions do not exist, I prefer to work with the original estimates of sanctions' impact on leader failure.

I generate the measure of sanction and threat effectiveness in the concessions dataset as the first derivative of the linear index of Equation A8 with respect to each:

$$\partial \boldsymbol{X_i\beta}/_{\delta S} = \beta_{01} + \beta_{02}W_{it} + \beta_{03}f(t_i) + \beta_{04}f(t_i)W_{it} + \beta_{05}c + \beta_{06}cW_{it} + \beta_{07}cf(t_i) + \beta_{08}cf(t_i)W_{it}$$

(Eq. A11)

$$\partial \boldsymbol{X_i\beta}/_{\delta T} = \beta_{09} - \beta_{010}W_{it} - \beta_{011}f(t_i) - \beta_{012}f(t_i)W_{it} + \beta_{013}cT_{it} - \beta_{014}cW_{it} - \beta_{015}cf(t_i) - \beta_{016}cf(t_i)W_{it} \quad \text{(Eq. A12)}$$

According to these measures the effectiveness of sanctions and threats will depend on winning coalition size, leader experience and sender credibility. I switch the

effectiveness of threats variable on only in cases where a threat was made prior to imposition.

Modeling Concessions to Economic Sanctions

To model leaders' decisions to concede or hold out in the face of threatened and imposed economic sanctions, I will estimate models on the set of all leaders who were targeted with threatened or enacted sanctions from 1971-2000. Because I have information on the threat stage, I am able to deal directly with the major selection problem discussed in the literature (e.g. Drezner 1999, 2003; Lacy and Niou 2002). Most formal arguments state that only those sanctions which are the least likely to succeed proceed beyond the threat stage to enacted sanctions, making fair evaluation of their success rate impossible. The TIES dataset ameliorates this problem by providing information on the issuance of threats, dates at which threats progress to imposed sanctions, and concessions information at both stages. There are a handful of cases in the data which progress immediately to imposition, but most proceed through the more traditional route.[102]

The process of sanction termination fits a competing risks framework. From the enactment of threats or imposed sanctions, the episode stands a chance of ending in multiple ways or of continuing. The probability of the sending state coming out on top, however, depends on more than just the effectiveness of the influence attempt and the difficulty of offering the demanded concessions. Sanctions which progress beyond the point of threats are systematically less likely to result in concessions. I must model more

[102] It is possible that an additional selection issue may surface if sending states are cautious to issue only threats which are likely to be met. This does not appear to be the case, as threats of sanction happen fairly frequently and can be issued by bureaucrats rather than the core leadership. The threat stage appears to be a matter of procedure rather than a truly strategic process as implementation is. I will incorporate non-threatened leaders in a test in Appendix B.

than just the competing risks and duration processes in this stage of the project then, but also the relationship between sanction imposition and the ultimate outcome.

I manage these multiple inference problems by constructing competing risks dependent variables for sender victory, loss and draws and estimating each jointly with a model of sanction imposition.

$$win = \begin{cases} 1 \; if \; the \; target \; provides \; concessions \; in \; current \; year \\ 0 \; if \; the \; sanction \; episode \; is \; ongoing \; in \; current \; year \\ missing \; if \; the \; sanction \; episode \; terminates \; in \; another \; fashion \end{cases}$$

$$lose = \begin{cases} 1 \; if \; the \; sender \; gives \; up \; without \; getting \; concessions \; in \; current \; year \\ 0 \; if \; the \; sanction \; episode \; is \; ongoing \; in \; current \; year \\ missing \; if \; the \; sanction \; episode \; terminates \; in \; another \; fashion \end{cases}$$

$$draw = \begin{cases} 1 \; if \; the \; episode \; reaches \; negotiated \; settlement \; in \; current \; year \\ 0 \; if \; the \; sanction \; episode \; is \; ongoing \; in \; current \; year \\ missing \; if \; the \; sanction \; episode \; terminates \; in \; another \; fashion \end{cases}$$

$$imposed = \begin{cases} 1 \; if \; the \; episode \; has \; progressed \; to \; imposed \; sanctions \\ 0 \; if \; episode \; has \; not \; progressed \; to \; imposed \; sanctions \end{cases}$$

The measurement issues regarding effectiveness and opposition strength still require separate modeling of nondemocratic and democratic targets of economic sanctions. For each type of leader, then, I estimate three bivariate probit models of the following form:

$$P(Y = y_i, \text{imposed} = 1|\boldsymbol{X}) = \Phi_2 \begin{pmatrix} \alpha + \beta_1 \widehat{E^t}_{it} - \beta_2 \widehat{\text{E}^s}_{\text{it}} \times \text{S} - \beta_3 \text{T} - \beta_4 \text{S} - \beta_5 \text{C}_{\text{it}} - \beta_6 (O_{it} \times C_{it}) \\ + \beta_7 t_i (O_{it} \times C_{it}) + \boldsymbol{\beta d} + \mathbf{\beta Z} + \varepsilon_i \, , \\ \alpha_2 + \beta_{21} \widehat{E^t}_{it} + \beta_{22} \widehat{\text{E}^s}_{\text{it}} + \beta_{23} \text{T} + \beta_{24} \text{C}_{\text{it}} + \mathbf{\beta t_i} + \; \mathbf{\beta d} + \mathbf{\beta V} + \varepsilon_2, \rho \end{pmatrix}$$

(Eq. A13).

Again, Φ_2 is the cumulative bivariate Normal function. Errors from the two equations are assumed to follow a joint Normal distribution with mean 0, variance 1 and covariance ρ. Rather than selecting into the imposed group, however, these equations are uncensored. All sanctioned-leader years factor into each equation. $\widehat{E^t}_{it}$ and $\widehat{\text{E}^s}_{\text{it}}$ are the effectiveness

measures for threats and sanctions respectively, generated using Equation 4 above; *T* is an indicator that a threat was issued; *S* is an indicator for imposed sanctions; C_{it} is the diffusion of concession-costs scale which ranges from 1 to 3; O_{it} is the strength of the opposition, measured as the share of seats in the legislature held by parties other than the largest in the democratic model and by an indicator for the existence of a legislature in the autocratic model; t_i is the leader's cumulative time in office, logged in $f(t_i)$; $\boldsymbol{t_i}$ is the cubic polynomial of leaders time in office (in three year intervals); ***d*** is the cubic polynomial of episode duration; and **Z** and **V** are vectors of control variables for each equation. The controls are summarized in Table 4.5.

In the model of imposition, I include both the effectiveness measure for threat and imposed sanctions. This strategy allows an interesting analysis of whether leaders focus more on the impacts of the current influence attempt (i.e. $\widehat{E^t}_{it}$) or the likely effects were the situation allowed to escalate (i.e. $\widehat{E^s}_{it}$). In the overall outcome model, I interact $\widehat{E^s}_{it}$ with the sanctions indicator *S* so that it will indicate only actual effectiveness rather than "expected" effectiveness.

Table A1 Probit of Aid Allocation with Murphy-Topel Standard Errors Correcting for Estimation Uncertainty around Instrument of Leader Failure

Probability of Leader's Winning Coalition Failing (Instrument)	0.0637*** (0.0193)
Lagged Aid Receipt	4.542*** (0.361)
Lagged Population Growth	0.0622*** (0.0217)
Lagged Imports from OECD Donor States	-0.575*** (0.142)
Natural log of Population	0.0741 (0.0770)
Lagged Economic Growth	-0.373 (0.784)
Winning Coalition Size	-0.909* (0.469)
Former Colony	0.478** (0.215)
Defensive or Offensive alliance with an OECD donor state	0.464** (0.203)
Potential for Oil Production	-0.685** (0.325)
Internationalized Civil Conflict	0.981** (0.474)
Constant	0.280 (0.621)

NOTE: N=4,752 aid-eligible leader years with 793 individual leaders from 1960-1999. Murphy-Topel corrected standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

APPENDIX B

ALTERNATIVE SPECIFCATIONS AND ROBUSTNESS CHECKS

In this appendix, I will present some variations on the models in Chapters 5-8 with respect to potential concerns. I separate the results by the chapter to which they pertain, beginning with Chapter 5's tests of aid effectiveness and closing with Chapter 8's bivariate probits of sanction imposition and concession.

Alternative Models of Aid Effectiveness

In this section I will address several potential problems with the model of aid effectiveness presented in Chapter 5: duration dependence in the aid allocation model, censored versus bivariate probits in the simultaneous model of allocation and leader failure, omitted variable bias in the leader failure equation due to exclusion of negative sanctions.

Duration Dependence in Aid Allocation Model

I was initially cautious about including both the instrument of leader failure and a function for duration dependence in the aid allocation model due to the (obviously) tight relationship between the probability of leader failure and leaders' time in office. If I account for duration dependence with a cubic polynomial for time, the instrument loses significance. When graphed, duration dependence appears to take a quadratic form. I test a quadratic form against the cubic polynomial of time, and the quadratic form is confirmed. In Table B1, I report coefficients and fit statistics for each of these forms of the aid allocation model. Figure B1 demonstrates the underlying reason for the instrument's loss of significance once we control for potential duration dependence. The thick black line in this figure charts the product of the coefficients for the quadratic form of duration dependence (reported in the third column of Table A1) and the values of the quadratic function of time. The thin lines give the 95% confidence intervals around the

estimated duration dependence given the estimated standard errors. I calculated the conditional standard errors as:

$$\sigma_{\beta_{12}t+\beta_{13}t^2} = \sqrt{\sigma_{\mathrm{t}}^2 \times t + (t^2)^2 \times \sigma_{\mathrm{t}^2}^2 + 2 \times t \times t^2 \times \sigma_{\mathrm{t,t}^2}^2} \quad \text{(Eq. B1).}$$

Below the estimated duration curve, I have arrayed the actual values of the instrument for leader failure, Y_{1it}^{**}, against time with small red dots. The thick red line is the best estimated quadratic fit for the instrument to time. Clearly, these trends are highly correlated. The instrument most likely loses significance (as opposed to the time trend failing to reach significance), because, though its relationship to time is very strong, it also includes information about other variables.

If we model duration dependence instead, as the cubic polynomial of years since receiving aid – more similar to the procedures used in the literature, such as Lai (2003) – however, the instrument does not drop out of significance. These results are reported in Table B2. This time trend is not highly significant, and its inclusion is only weakly supported by a difference of Bayesian Information Criteria ($BIC_{duration} - BIC_{noduration} = 2.07$). I chart the shape of duration dependence with 95% confidence intervals in Figure B2. The standard error of the cubic polynomial was derived analogously to above, except with extra terms represent the duration variable cubed. The dashed blue curve in this figure charts the density of observations by the duration variable. The vast majority of cases cluster early, where the duration dependence is not significant. The 75th percentile of duration begins at year 9 and the 90th at year 16.

Including the duration dependence parameters in this model creates some shifts in the size of coefficient estimates, but directions and levels of significance do not change. One of the biggest shifts in magnitude comes in the probability of leader failure instrument, which shrinks from .0637 to .0437. This change results likely from the still strong correlation ($r = -0.3519$, $p < .0001$) between aid duration and the probability of leader failure. This is by far the largest partial correlation amongst the other covariates

and the duration variable. The relationship between the two is depicted in Figure B3. Note that the leader failure instrument uses the right-hand y-axis in this figure. Its values are consistently smaller than those of the duration variable.

For purposes of the overall project, the most important question is whether excluding duration dependence introduced bias in the estimates of aid's impact on the probability of leader failure. Table B3 contains censored probit coefficients for the model using duration dependence parameters in the allocation stage and one not. The outcome equation results are found in the second half of the table. Here the changes in coefficient size are considerably smaller than they were in the allocation stage. For the terms relating the impact of aid to nondemocratic leaders, both the constitutive term and the interaction with time are slightly larger in the model which includes duration dependence in the aid allocation model. Those for big winning coalition leaders are both slightly smaller.

To further investigate these differences, I bootstrapped the coefficient estimates to obtain results comparable to those which I utilized in the original analyses. The bootstrapped coefficients and standard errors can be found in Table B4. The process of estimating these models over many draws from the observed data introduces some strange findings. First, note that the duration dependence terms in the allocation stage no longer reach significance. The standard errors on the civil conflict variable also explode to huge values. Finally, the ρ parameter does not achieve statistical significance when bootstrapped. The instability of the model including duration dependence makes it an undesirable choice for estimating parameters upon which I must rely in later chapters.

Censored Vs. Endogenous Probits

Another key choice in the modeling strategy was employing censored rather than endogenous probit models to estimate the impact of aid on leader failure. The censored probit technically assumes that a value of the outcome variable can only be observed

when the "selection" variable obtains. The relationship between leader failure and aid allocation, obviously is not this determinative. Leaders fail whether they receive aid or not. Some may argue then, that the more appropriate modeling decision would be to estimate a bivariate probit including the full sample of aid-eligible leaders in the outcome equation while allowing correlation between the errors of the aid allocation and leader failure models. To distinguish between aid recipients and non-recipients, such a model might include a variable to indicate aid allocation. Thus the aid allocation model's dependent variable would also be included in the leader failure model.

This strategy would be appropriate if the general risk pattern of leaders who receive aid and leaders who do not were basically similar. Including the indicator for aid receipt would allow the probability of leader failure for aid-recipients to shift up to indicate their higher ex-ante risks. But, empirically, it does not appear that aid-receivers and non-recipients resemble each other this closely. Aid recipients' baseline hazard of losing office over time differs substantially from those of the aid-eligible leaders who donors overlook. The difference is one of form, not simply slope intercept. To illustrate this, I estimated a stratified Non-proportional Hazards Cox model, allowing the baseline survivor and hazard functions of leaders to be estimated separately based on aid receipt. If there were no serious difference in the patterns of risks for these two types of leaders, the Cox model, which estimates the survival functions based on the data rather than fitting it to a specific functional form, would return to similar curves. Figure B5 plots these curves, demonstrating the fundamental difference between these types. The survival probabilities of non-recipients are higher than those of aid-recipients for the majority of survival time under analysis, but the two curves differ in more than just a shift in slope. While the survival prospects of aid-recipients steadily and sharply decline over time, those of non-recipients plateau.

I do not believe that this difference in risks between leaders to whom aid is allocated and those to whom it is not can be modeled by an indicator variable in the

leader fail equation. Perhaps estimating separate duration dependence functions across the two groups would be appropriate, but I hesitate to include another set of interactions with time and aid in a model which already includes five functions of leaders' tenure in office.

Omitted Variable Bias

A third concern in the leader failure equations of Chapter 5 is the failure to include measures of the effect of economic sanctions. My theory argues that leader survival should be affected by both positive and negative sanctions, yet my model of foreign aid does not control for the impact of punishments. I decided not to include sanctions, because the best sanctions dataset currently available, TIES, does not include information on episodes occurring prior to 1971. Including indicators for sanction targeting, then, would require me to drop a full decade of data on the object of most immediate interest here: leader survival and foreign aid. Below, Table B5 displays a model of aid allocation and leader survival which controls for negative sanctions in both the targeting and outcome stage.

Interestingly, it does not appear that sanctions and aid are utilized in a complementary fashion as has been argued by some in the literature. Sanctions and threats in the past are associated with slightly lower probability of aid allocation in the future. The difference in probability is .11. In the leader failure equation, the same general patterns hold for foreign aid, but the coefficients are generally smaller and less efficient – not particularly surprising as they have become more conditional as time is now also interacted several times with sanctions indicators, and 1,100 observations have been lost. Among the subset of aid-receiving leaders, the impact of sanctions and threats appears to be much less significant, with only the constitutive term for threats and the time trend for threats against democratic leaders reaching traditional levels of confidence.

Alternative Models of Economic Sanction Outcomes

I anticipate questions regarding selection bias in the bivariate probit regressions of sanction imposition and concessions. It is possible that a selection process operates prior to the issuance of threats which creates a body of cases which are either more/less likely to be imposed or more/less likely to achieve concession. The issuance of threats takes place often at low levels of government, through bureaucrats, individual politicians or staff members. Only about 20% of the threats issued in the TIES database stem directly from the executive office or the government. This leads me to believe that the bulk of selection bias stems from the decision to impose sanctions following threats, rather than from the decision to offer a threat in the first place. Those threats issued by individual legislators or bureaucrats are made for personal or procedural reasons rather than for the sake of strategic influence attempts.

To check for any possible bias in the results stemming from selection at the threat stage, I ran a model of threat targeting and calculated an inverse Mill's ratio using its parameters. Loosely following a Heckman procedure, I think included this inverse Mill's ratio in each stage of the bivariate probits of sanction imposition and concession. This parameter should control for any systematic bias in the sample of observed threats. Table B7 below displays the results for democratic targets. The Mill's ratio does not achieve significance in the imposition model, and the coefficients and significance levels consequently fall very close to those originally reported. In the victory equation, however, the selection parameter is highly significant and positive. This changes the sign and significance of the effectiveness measure for threats. It becomes negative and significant. The marginal effect of threats and threat effectiveness, however, remains insignificant if calculated and charted.

The parallel results for nondemocratic leaders can be found in Table B8. The selection parameter is significant in both equations for this subsample, but this does not alter the key findings.

Table B1 Comparing Duration Dependence in Aid Allocation Models using Cubic Polynomial of Leader Failure to Address Dependence

	No Duration Dependence	Cubic Polynomial of Time	Quadratic Function of Time
Probability of Leader's Winning Coalition Failing (Instrument)	0.0637***	0.0208	0.0214
	(0.0193)	(0.0203)	(0.0187)
Lagged Aid Receipt	4.542***	4.565***	4.567***
	(0.361)	(0.350)	(0.352)
Lagged Population Growth	0.0622***	0.0815***	0.0815***
	(0.0217)	(0.0248)	(0.0248)
Lagged Imports from OECD Donor States	-0.575***	-0.570***	-0.571***
	(0.142)	(0.142)	(0.143)
Natural log of Population	0.0741	0.123*	0.123*
	(0.0770)	(0.0715)	(0.0711)
Lagged Economic Growth	-0.373	-0.688	-0.679
	(0.784)	(0.829)	(0.829)
Winning Coalition Size	-0.909*	-1.185**	-1.188**
	(0.469)	(0.480)	(0.480)
Former Colony	0.478**	0.509***	0.509***
	(0.215)	(0.186)	(0.185)
Defensive or Offensive alliance with an OECD donor state	0.464**	0.434**	0.434**
	(0.203)	(0.191)	(0.191)
Potential for Oil Production	-0.685**	-0.874***	-0.875***
	(0.325)	(0.263)	(0.266)
Internationalized Civil Conflict	0.981**	0.896*	0.895*
	(0.475)	(0.528)	(0.528)
t	--	-0.336**	-0.320***
		(0.156)	(0.0736)
t2	--	0.0219	0.0180***
		(0.0319)	(0.00561)
t3	--	-0.000235	--
		(0.00172)	
Constant	0.280	-0.0574	-0.0535
	(0.621)	(0.573)	(0.566)
-2Pseudo Loglikelihood	-265.8	-246.52	-246.52

Table B1 Continued

Pseudo R^2	0.843	0.8540	0.8540
Bayesian Information Criterion	633.198	620.030	611.579
Expected Percent Correctly Predicted	96.83%	97.10%	97.10%

NOTE: N= 4,752 aid-eligible leader years from 1960-1999 including 793 individual leaders. Robust standard errors in parentheses. Smaller BIC indicates better fit, with penalization for including extra terms which do not contribute to explaining variance.

*** p<0.01, ** p<0.05, * p<0.1

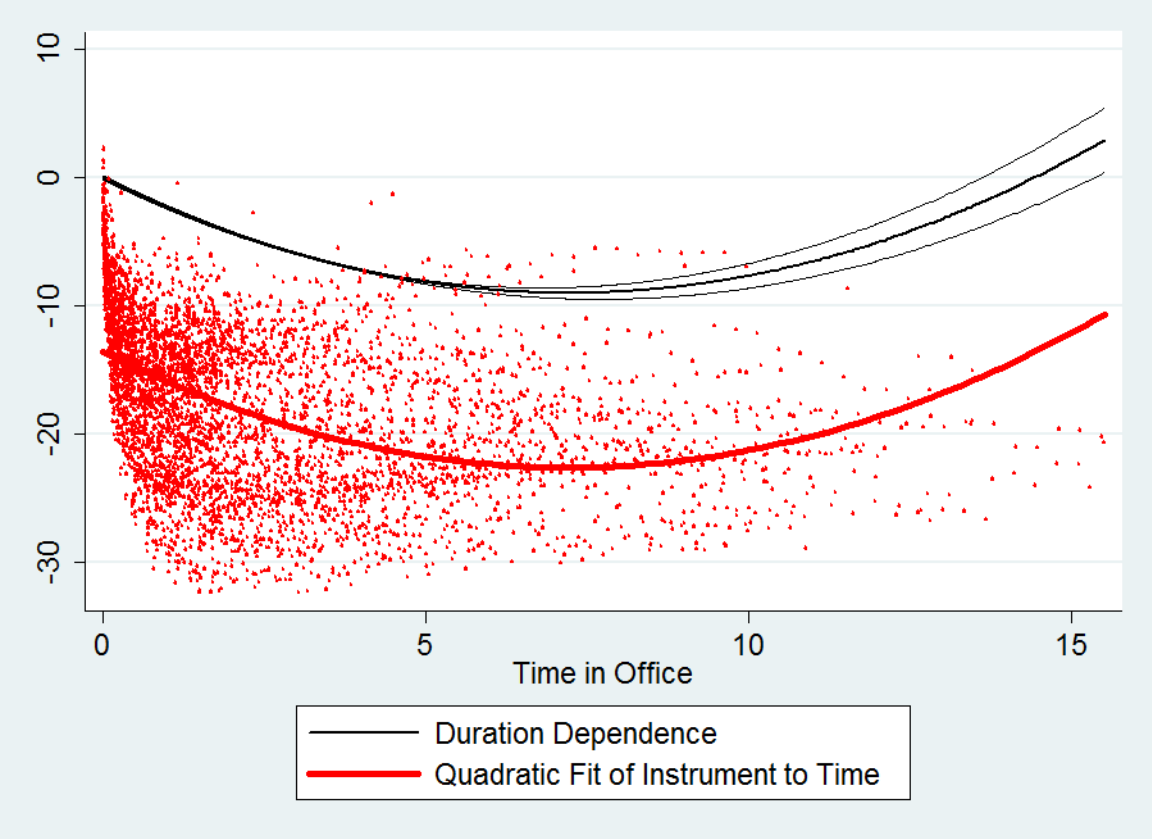

Figure B1 Similarity of Duration Dependence and Fit of Failure Instrument to Time

NOTE: Duration dependence is the linear combination of the coefficients from third column of Table A1 above and values of time. Scatter plot arrays value of failure instrument against time, and thick red line is the best quadratic fit of the relationship.

Table B2 Comparing Duration Dependence in Aid Allocation Models Using Cubic Polynomial of Years Since Aid Allocation to Adress Dependence

	No Duration Dependence	Years Since Aid Cubic Polynomial
Probability of Leader's Winning Coalition Failing (Instrument)	0.0637***	0.0467**
	(0.0193)	(0.0193)
Lagged Aid Receipt	4.542***	4.702***
	(0.361)	(0.405)
Lagged Population Growth	0.0622***	0.0708***
	(0.0217)	(0.0210)
Lagged Imports from OECD Donor States	-0.575***	-0.559***
	(0.142)	(0.149)
Natural log of Population	0.0741	0.0917
	(0.0770)	(0.0716)
Lagged Economic Growth	-0.373	-0.590
	(0.784)	(0.818)
Winning Coalition Size	-0.909*	-1.047**
	(0.469)	(0.478)
Former Colony	0.478**	0.541***
	(0.215)	(0.198)
Defensive or Offensive alliance with an OECD donor state	0.464**	0.391**
	(0.203)	(0.197)
Potential for Oil Production	-0.685**	-0.752***
	(0.325)	(0.287)
Internationalized Civil Conflict	0.981**	0.892*
	(0.475)	(0.521)
d†	--	0.404
		(0.566)
d2	--	-1.095**
		(0.480)
d3	--	0.302***
		(0.114)
Constant	0.280	0.0592
	(0.621)	(0.579)

Table B1 Continued

-2 Pseudo Loglikelihood	-265.8	-252.1
Pseudo R^2	0.843	0.851
Bayesian Information Criterion	633.198	631.135
Expected Percent Correctly Classified	96.83%	96.99%

NOTE: N=4,752 aid-eligible leader years clustered on 793 individual leaders. Robust standard errors in parentheses. Smaller BIC indicates better fit, with penalization for including extra terms which do not contribute to explaining variance

† Duration dependence is measured here with decades since first aid was received.

*** p<0.01, ** p<0.05, * p<0.1

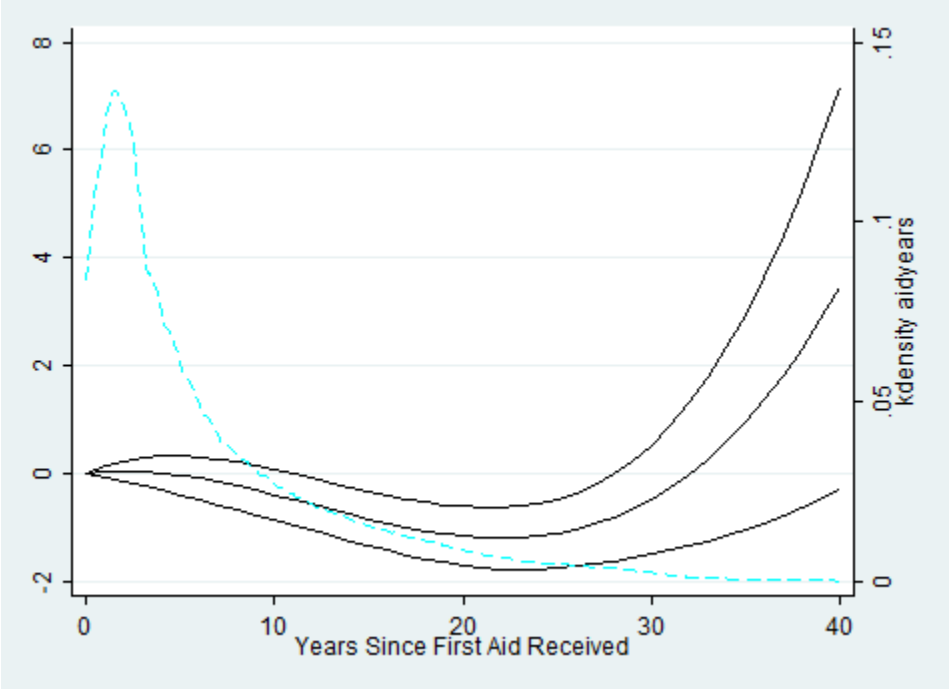

Figure B2 Duration Dependence in Aid Allocation Model using Years Since First Aid

NOTE: Figure charts duration dependence from second column of Table B2 with 95% confidence intervals. Dashed bright blue line gives density of observations at the values of the duration variable.

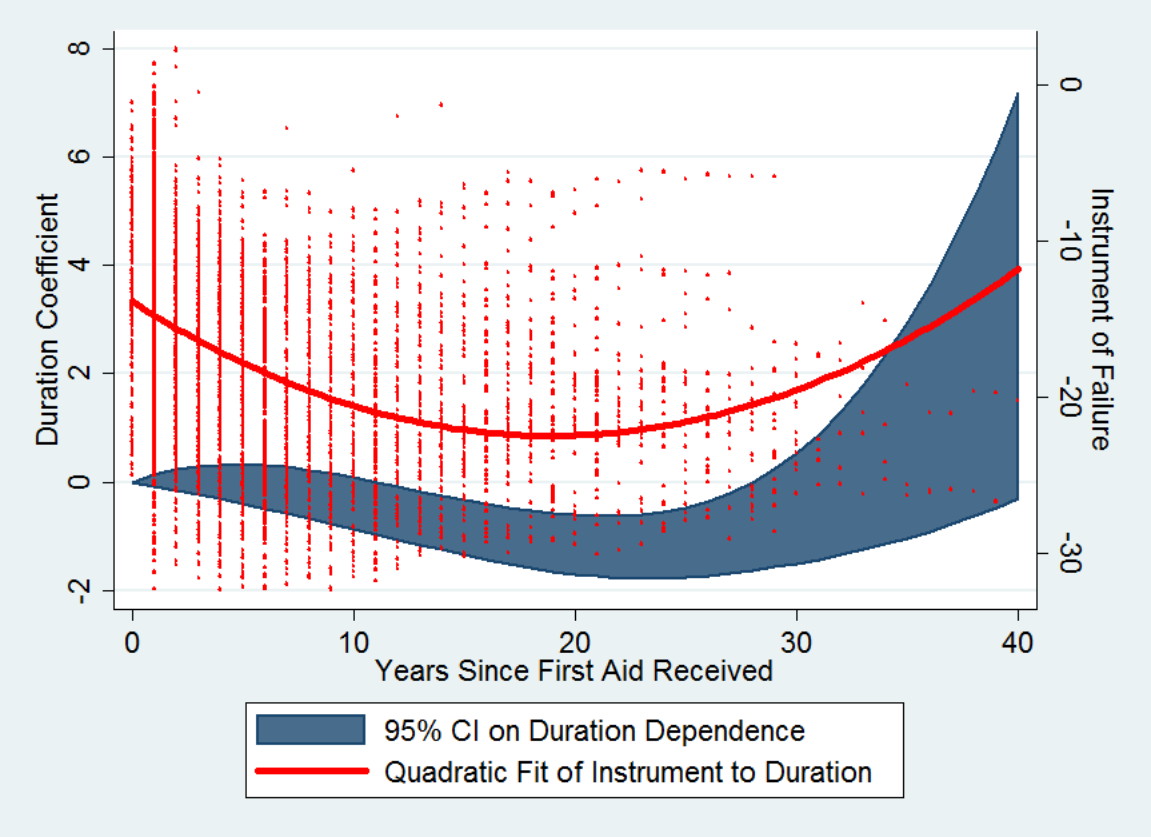

Figure B3 Relationship Between Duration Dependence and Leader Failure in Years Since First Aid Model

NOTE: Figure plots 95% confidence interval of duration dependence measured as cubic polynomial of years since aid was first received against the best quadratic fit of the instrument for probability of leader failure. The correlation between the first term in the duration polynomial and the instrument is -.3035, $p<.0001$.

Table B3 Effect of Modeling Duration Dependence in Allocation Stage on Censored Probit Estimates

		Duration Dependence	No Duration Dependence
Aid Allocation Models	Probability of Leader's Winning Coalition Failing (Instrument)	0.04772** (0.01906)	0.06512*** (0.01901)
	Lagged Aid Receipt	4.67581*** (0.40580)	4.52542*** (0.36169)
	Lagged Population Growth	0.07204*** (0.02001)	0.06212*** (0.02069)
	Population	0.09519 (0.06993)	0.07642 (0.07566)
	Lagged Imports from OECD Donor States	-0.55445*** (0.15114)	-0.56842*** (0.14373)
	Winning Coalition Size	-1.05774** (0.45789)	-0.92053** (0.45496)
	Former Colony	0.53035*** (0.19202)	0.47307** (0.20973)
	Defensive or Offensive Alliance with an OECD Donor State	0.39503** (0.19750)	0.46804** (0.20283)
	Internationalized Civil Conflict	0.86869* (0.50399)	0.95200** (0.46681)
	Oil Production	-0.74050*** (0.27530)	-0.67381** (0.31669)
	d†	0.36420 (0.55574)	--
	d2	-1.10432** (0.48152)	--
	d3	0.30912*** (0.11469)	--
	Constant	0.05870 (0.56936)	0.27987 (0.61278)

Table B3 Continued

Leader Failure Models	Logged Aid/GDP to Small Winning Coalition Systems	0.28566*** (0.10014)	0.27956*** (0.09963)
	Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.03124** (0.01365)	-0.03024** (0.01361)
	Logged Aid/GDP to Big Winning Coalition Systems	-0.57004*** (0.12653)	-0.57171*** (0.12627)
	Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.07154*** (0.01776)	0.07184*** (0.01773)
	Big Winning Coalition	0.40940*** (0.13512)	0.40876*** (0.13526)
	Ln(time)	-0.14540*** (0.04077)	-0.14886*** (0.04061)
	Lagged Economic Growth	-0.71452*** (0.25966)	-0.71539*** (0.25957)
	Lagged Log of Total Trade	0.00101 (0.02008)	0.00197 (0.02002)
	Intensity of Civil Conflict	0.13436*** (0.05163)	0.13536*** (0.05164)
	SOUTHAM	0.24152*** (0.07690)	0.24234*** (0.07684)
	SUBAFRICA	-0.13627* (0.08168)	-0.13424 (0.08176)
	SOUTHASIA	0.19304** (0.09171)	0.19412** (0.09171)
	Constant	-0.60813** (0.29942)	-0.59019** (0.29860)
ρ		-0.67635*** (0.21529)	-0.54842*** (0.17775)
Wald $\chi 2$ Test for Independent Equations		9.870, p=0.00168	9.520, p=0.00203

NOTE: N= 4,752 aid-eligible leader years 1960-1999. Robust standard errors, in parentheses, cluster on 793 leaders. 543 leader years when no aid was allocated.

*** p<0.01, ** p<0.05, * p<0.1

Table B4 Bootstrapped Censored Probit Coefficients with Duration Dependence in Allocation Model

		Bootstrapped Coefficients
Aid Allocation	Probability of Leader's Winning Coalition Failing (Instrument)	0.04772** (0.02103)
	Lagged Aid Receipt	4.67581*** (0.69483)
	Lagged Population Growth	0.07204 (0.05513)
	Population	0.09519 (0.09461)
	Lagged Imports from OECD Donor States	-0.55445** (0.22887)
	Winning Coalition Size	-1.05774* (0.57264)
	Former Colony	0.53035** (0.23875)
	Defensive or Offensive Alliance with an OECD Donor State	0.39503* (0.23364)
	Internationalized Civil Conflict	0.86869 (35,644.69249)
	Oil Production	-0.74050** (0.34531)
	d†	0.36420 (0.81425)
	d2	-1.10432 (0.93131)
	d3	0.30912 (0.29401)
	Constant	0.05870 (0.75746)

Table B4 Continued

Leader Failure	Logged Aid/GDP to Small Winning Coalition Systems	0.28566*** (0.10297)
	Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.03124** (0.01395)
	Logged Aid/GDP to Big Winning Coalition Systems	-0.57004*** (0.13086)
	Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.07154*** (0.01841)
	Big Winning Coalition	0.40940*** (0.13795)
	Ln(time)	-0.14540*** (0.04209)
	Lagged Economic Growth	-0.71452** (0.30221)
	Lagged Log of Total Trade	0.00101 (0.02000)
	Intensity of Civil Conflict	0.13436** (0.05391)
	SOUTHAM	0.24152*** (0.07703)
	SUBAFRICA	-0.13627 (0.08344)
	SOUTHASIA	0.19304** (0.09441)
	Constant	-0.60813* (0.31181)
ρ		-0.67635 (2.26025)

NOTE: Results are bootstrapped coefficients and standard errors across 1,000 draws of 4,752 aid-eligible leader years clustered on 793 specific leaders from 1960-1999.

*** p<0.01, ** p<0.05, * p<0.1

Table B5 Stratified Cox Model Estimates of Leader Failure

	Coefficient
Logged Aid/GDP to Small Winning Coalition Systems	2.329*** (0.159)
Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.306*** (0.0220)
Logged Aid/GDP to Big Winning Coalition Systems	-0.590*** (0.189)
Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.0451 (0.0289)
Big Winning Coalition	1.172*** (0.191)
Lagged Economic Growth	-0.789 (0.488)
Lagged Log of Total Trade	-0.0164 (0.0318)
Intensity of Civil Conflict	0.278*** (0.0771)
SOUTHAM	0.517*** (0.145)
SUBAFRICA	-0.314** (0.144)
SOUTHASIA	0.407** (0.164)
Observations	4,930
Events	443
Subjects	827
-2PseudoLogLikelihood	-2337
χ^2	266.5

NOTE: Standard errors, in parentheses, clustered on leader. Sample size is larger in this model as the selection equation's data limitations are not involved. Baseline hazard and survival functions stratified by aid allocation. Reported coefficients are best fit across aid-receiving and non-aid-receiving samples.

*** p<0.01, ** p<0.05, * p<0.1

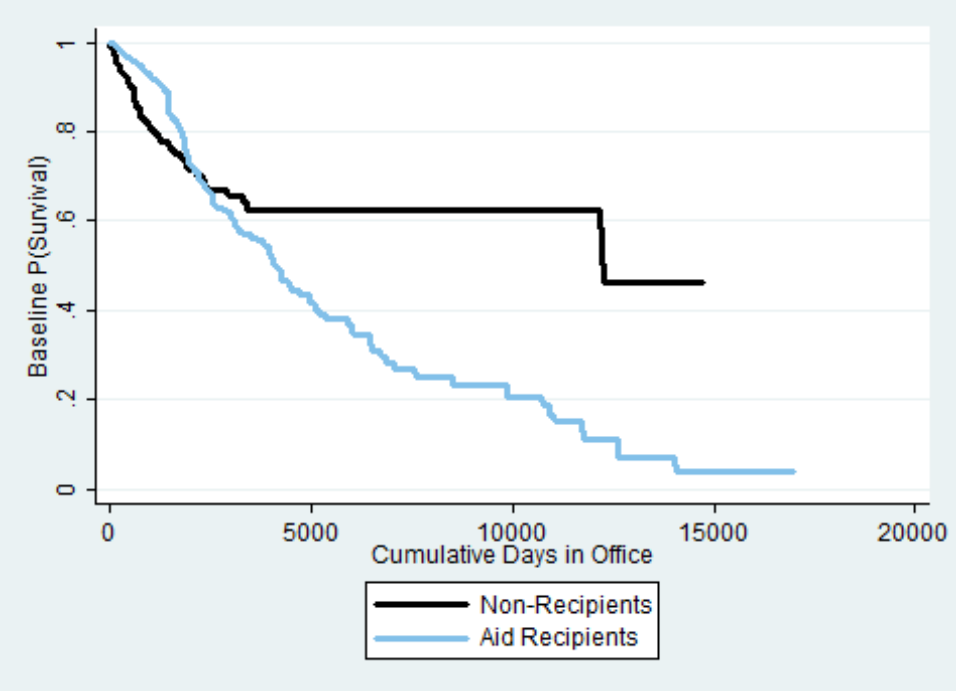

Figure B4 Baseline Survivor Function of Leaders by Aid Allocation

NOTE: Curves are baseline survival functions from stratified Cox model reported in Table B5. The Cox model estimates the baseline function non-parametrically using partial likelihood maximization and information in the data.

Table B6 Censored Probit of Aid Allocation and Leader Failure Including Sanctions' Effectiveness Terms

		Coefficient
Leader Failure	Logged Aid/GDP to Small Winning Coalition Systems	0.260**
		(0.124)
	Ln(time)*Logged Aid/GDP to Small Winning Coalition Systems	-0.0337**
		(0.0156)
	Logged Aid/GDP to Big Winning Coalition Systems	-0.372***
		(0.140)
	Ln(time)*Logged Aid/GDP to Big Winning Coalition Systems	0.0513***
		(0.0198)
	Big Winning Coalition	0.426***
		(0.152)
	Ln(time)	-0.0560
		(0.0494)
	Sanction	0.104
		(0.118)
	Threat	-0.320*
		(0.163)
	Credible	-0.164
		(0.233)
	Credibility of Sanction Against Nondemocratic Leader	0.882
		(0.626)
	Ln(time)* Credibility of Sanction Against Nondemocratic Leader	-0.0743
		(0.0753)
	Credibility of Sanction Against Democratic Leader	0.913
		(0.803)
	Ln(time)* Credibility of Sanction Against Democratic Leader	-0.193
		(0.117)
	Credibility of Threat Against Nondemocratic Leader	0.629
		(0.820)

Table B6 Continued

Leader Failure	Ln(time)* Credibility of Threat Against Nondemocratic Leader	-0.0538 (0.112)
	Credibility of Threat Against Democratic Leader	-1.036 (0.656)
	Ln(time)* Credibility of Threat Against Democratic Leader	0.0289** (0.0123)
	Lagged Economic Growth	-0.658** (0.274)
	Lagged Log of Total Trade	0.00815 (0.0265)
	Intensity of Civil Conflict	0.111** (0.0537)
	SOUTHAM	0.191** (0.0749)
	SUBAFRICA	-0.0899 (0.0838)
	SOUTHASIA	0.265** (0.104)
	Constant	-1.022*** (0.394)
Aid Allocation	Probability of Leader's Winning Coalition Failing (Instrument)	-0.00327 (0.0228)
	Lagged Aid Receipt	4.156*** (0.221)
	Lagged Sanction Targeting	-0.597*** (0.213)
	Population	0.0492** (0.0193)
	Lagged Imports from OECD Donor States	0.0842 (0.0738)
	CINC-Score	-2.071 (2.916)

Table B6 Continued

Aid Allocation	Winning Coalition Size	-0.953** (0.436)
	Economic Growth Lag	-0.834 (0.769)
	Former Colony	0.397** (0.178)
	Defensive or Offensive Alliance with an OECD Donor State	0.212 (0.147)
	Internationalized Civil Conflict	0.671 (0.780)
	Oil Production	-0.628*** (0.206)
	d†	-0.575 (0.658)
	d2	-0.218 (0.553)
	d3	0.103 (0.124)
	Constant	-1.022 (0.692)
Ath/Rho		-7.446 (61.35)
Wald Test of Independent Equations		0.0147, p=.903
Observations		3,647
Number of Subjects		622
Number of Censored Observations		407
-2 PseudoLoglikelihood		-1285
Model $\chi 2$		216.6

NOTE: Robust standard errors in parentheses are clustered on leaders. Temporal range is 1971-1999.

*** p<0.01, ** p<0.05, * p<0.1

Table B7 Bivariate Probit of Sanction Imposition and Sender Victory Against Democratic Leaders with Selection Parameters

	Sender Wins	Sanction Imposed
Effectiveness of Threats	-0.534**	1.474***
	(0.211)	(0.244)
Threat	-0.548*	-4.287***
	(0.296)	(0.538)
Expected Effectiveness of Sanctions		-7.682***
		(0.809)
Effectiveness of Imposed Sanctions	0.175	
	(0.884)	
Imposed	-0.446	
	(0.272)	
t		-0.839***
		(0.215)
t^2		0.215**
		(0.0990)
t^3		-0.0199
		(0.0129)
Anticipated Sender Costs	0.00360*	0.00577***
	(0.00186)	(0.00180)
Anticipated Target Costs	-0.00188	0.00886***
	(0.00189)	(0.00182)
Diffusion of Costs Scale	-0.260	-0.236**
	(0.266)	(0.100)
Lagged Economic Growth	1.174	-3.629***
	(1.122)	(0.809)
Lagged Total Trade		0.172**
		(0.0786)
Target CINC-Score		-3.235**
		(1.510)
Target Winning Coalition Size		-1.709***
		(0.606)

Table B7 Continued

Joint Democracy		0.114 (0.152)
Inverse Mill's Ratio	**0.512***** **(0.188)**	**0.253** **(0.311)**
d	-1.013* (0.592)	2.291*** (0.204)
d^2	0.433 (0.355)	-0.745*** (0.0727)
d^3	-0.0755 (0.0580)	0.0580*** (0.00584)
Ln(Time)	-0.0131 (0.0942)	
Opposition Strength	-0.00184 (0.00917)	
Opposition Strength*Cost Scale	0.0187** (0.00778)	
Ln(Time)*Opposition Strength*Cost Scale	-0.00176** (0.000863)	
Constant	-1.241 (0.868)	5.868*** (0.687)
ρ	0.486*** (0.157)	
Wald Test of Independent Equations	9.596, p=.002	
Observations	3,394	
Subjects	1,260	
Model $\chi 2$	684.3	
-2 PseudoLoglikelihood	-1405	

NOTE: Robust standard errors, in parentheses, clustered on sanctioned-leader episodes.

*** p<0.01, ** p<0.05, * p<0.1

Table B8 Bivariate Probit of Sanction Imposition and Sender Victory Against Nondemocratic Leaders with Selection Parameters

	Sender Win	Imposition
Effectiveness of Threats	1.020 (0.647)	3.279*** (0.562)
Threat	0.627 (0.383)	
Expected Effectiveness of Sanctions		-7.934*** (1.140)
Effectiveness of Imposed Sanctions	-0.642 (1.281)	
Imposed	-1.453*** (0.257)	
Ln(Time)	0.0186 (0.155)	0.0478 (0.138)
Diffusion of Costs Scale	-0.135 (0.218)	-0.0483 (0.179)
Legislature	-0.399 (0.454)	-0.612 (0.444)
Legislature *Cost Scale	-0.309 (0.421)	-0.949* (0.507)
Ln(Time)* Legislature *Cost Scale	0.0751 (0.0479)	0.126** (0.0619)
Anticipated Sender Costs	0.00417 (0.00281)	-0.00551* (0.00329)
Anticipated Target Costs	-0.00829*** (0.00310)	-0.00550 (0.00346)
Lagged Economic Growth	1.548** (0.682)	0.410 (0.478)
Inverse Mill's Ratio	**0.320**** **(0.133)**	**0.571**** **(0.277)**
d	0.935** (0.419)	1.990*** (0.279)
d^2	-0.376** (0.168)	-0.537*** (0.101)

Table B8 Continued

d^3	0.0304* (0.0160)	0.0395*** (0.00960)
Lagged Total Trade		-0.00694 (0.0570)
Target CINC-Score		10.37*** (2.425)
Constant	-1.913 (1.373)	-1.819* (1.075)
rho	1.266***	
	(0.295)	
chi2_c	18.42, p<.0001	
Observations	1,105	
N_clust	333	
Model $\chi 2$	658.7	
-2PseudoLoglikelihood	-651.9	

NOTE: Robust standard errors, in parentheses, clustered on sanctioned-leader episodes.

*** p<0.01, ** p<0.05, * p<0.1

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