

MODELING SELECTION BIAS IN STUDIES OF SANCTIONS EFFICACY

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Sanctions rarely work but they continue to be used frequently by policymakers. I argue that previous studies of sanctions ignore the problem of strategic censoring by focusing only on cases of observed sanctions. In this paper, I develop a unified model of sanction imposition and success and test it using a simultaneous equation censored probit model. This selection-corrected sanction model finds that the process by which sanctions are imposed is linked to the process by which some succeed while others fail, and that the unmeasured factors that lead to sanction imposition are negatively related to their success.

KEY WORDS: censored probit, democracy, sanctions, selection bias, strategic censoring.

INTRODUCTION

Over the past decade, economic sanctions imposed by the United States on the Saddam Hussein regime in Iraq have wreaked immense damage on that country's economy. All reports suggest that the sanctions are "working," i.e., that they are

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taking a devastating toll on the economy. Yet despite the stranglehold over the economy that the sanctions exert, Saddam Hussein remains defiant and, ten years after his misadventure in Kuwait, Hussein shows no signs of weakness. Essentially, this example suggests an empirical puzzle: Why would sanctions that destroy an economy not achieve the foreign policy goal they were imposed to achieve?

Between 1950 and 1970, sanctions were used 35 times, while in the next twenty years (1970 to 1990) 61 sanctions episodes were recorded (Hufbauer, Schott, and Elliott, 1990). Yet of the 115 sanctions that Hufbauer, Schott, and Elliott (hereafter HSE) collect data on, they code only a third as successes, i.e., as having achieved the goals for which they were imposed. That only a third of sanctions would succeed is disturbing enough given the immense human and economic costs these policy tools engender, but the situation might be even more bleak. Pape (1997), for instance, argues that a mere five of the HSE cases can be truly considered a success.

The question of why sanctions succeed has received considerable attention in political science research (Galtung, 1967; Doxey, 1972; Dashti-Gibson et al., 1997; Drezner, 1998, 1999, 2000; Hart, 2000; Bolks and Al-Sowayel, 2000; Morgan and Schwebach, 1995, 1997; Morgan and Miers, 1999). However, while numerous articles have been written attempting to test various hypotheses empirically about sanctions success, they are all guilty of the same mistake: selection bias. Since the data sets used in these articles contain only instances of sanctions, we have no information on cases in which sanctions might have been considered but not used for strategic reasons. Such selection bias of our data leads to biased inferences unless we account for the process which leads to the use of sanctions in some instances but not others (Heckman, 1979).

In this paper, I present a unified model of sanction imposition and success. I begin with a brief survey of the literature on economic sanctions. I argue that previous models have been incorrectly specified because they omit a key explanatory variable, viz., the target's regime type, for sanction success. Next, I specify and test a selection-corrected model of sanction success and find that doing so changes our results significantly. The paper thus makes two critical contributions to the study of economic statecraft in international relations, one methodological and the other theoretical. I conclude with a brief consideration of the implications of this paper for future studies of sanctions success.

CONVENTIONAL WISDOM

I focus on sanctions that punish an already performed action by the target. In defining sanctions in this manner, I deliberately do not diverge from the sanctions literature, which defines sanctions as "the deliberate, government-inspired withdrawal, or threat of withdrawal, of customary trade or financial relations" (HSE, 1990, p. 2). Further, in keeping with conventional usage, the *sender* imposes sanctions on the *target*.

What Is Success?

Since policymakers often use economic statecraft to cajole and coerce other states

to change their behavior (for the seminal statement on this topic, see Baldwin, 1985), the attention of scholars and policymakers understandably turns to their effectiveness. What constitutes success? Simply put, “an effective sanction in any political system is one which succeeds in producing the desired behavioral response from the individual or group to which it is communicated” (Doxey, 1972, p. 529). And are sanctions successful? The unambiguous answer is no. HSE code a mere 35% of all sanctions as successes, where success means that some or all of the stated foreign policy goals were achieved. Robert Pape (1997) reanalyzes the HSE dataset and concludes that HSE’s coding of success is flawed. Of the 40 cases HSE consider successful, Pape argues that 18 were settled by direct or indirect use of force, 8 show no evidence that the target made the demanded concessions, 6 do not even qualify as instances of economic sanctions, and 3 are indeterminate (1997, p. 93). If one accepts his coding rules, therefore, only 5 cases are unequivocal successes.

The central project for IR scholars has been to identify the determinants of sanctions success. This enterprise has been the subject of numerous historical case studies (Galtung, 1967, and Doxey, 1972, are the classic studies) of highly celebrated sanctions episodes, typically the UN-backed sanctions against Rhodesia and South Africa. With the publication of *Economic Sanctions Reconsidered* in 1985, the academic community for the first time had access to a large-n dataset of sanction cases. In particular, the large-n nature of the data set allowed for systematic testing of the various hypotheses generated by the case studies that preceded it. Indeed, the HSE data set has been the subject of *all* published empirical studies of sanction success and is the source of the extant knowledge in the discipline on this topic.

Why Do Sanctions Succeed?

The initial statements of why sanctions succeed or, more accurately, fail so often were made by HSE themselves. They relied on simple bivariate correlations but their analysis generated so many testable hypotheses that those that followed could test using multiple regression techniques. In this section, I summarize briefly the collective wisdom based on these studies (HSE, 1990; Dashti-Gibson, et al. 1997; Drury, 1998; Drezner, 2000; Hart, 2000).

First, HSE suggest that the sanction adopted might not succeed because “the means [used were] too gentle” and might not have been adequate to engender change in the target state (1990, p. 12). The *cost to the target* is a principal independent variable used in all sanction studies since there is a clear reasoning behind why one might expect more costly sanctions to be more effective. Simply put, the higher the cost the more pressure the target government feels it must accede to the sender’s demands (Dashti-Gibson et al., 1997; Drury, 1998; Hart, 2000). Contrary to conventional wisdom, however, Eaton and Engers find that “sanctions that impose less harm on the target can sometimes be more effective than those that impose great harm” (1992, p. 899). This counterintuitive result is explained by the other factor that they consider important for a sanction’s success: credibility of the sender’s commitment. They argue that the “threat of a sanction or the promise of a reward can be effective only to the extent that the target believes that the sender will stick to its stated policy” (Eaton and Engers, 1992, p. 901). Since sanctions are not costless to the sender, a credible

sanction is one that imposes few costs on the sender while causing significant harm to the target's economy.

Just as with any other policy tool used by one state to cause another to change its policies, economic sanctions are thought to be more successful the greater the leverage the sender has over the target. In this regard, two concerns are the degree of presanction *trade linkages* by which one means "the degree to which the target relies on the sender for imports and exports" (Dashti-Gibson et al., 1997, p. 609) and the *relative capabilities* of the sender vis-à-vis the target (Morgan and Schwebach, 1997; Drury, 1998; Hart, 2000). Of course, these two hypotheses are closely related to the cost hypotheses since the larger the bargaining leverage the sender has over the target the more capable it is of inflicting debilitating costs on the target. This simple observation probably explains why the United States, with the world's largest economy, is the most frequent user of sanctions.

Just how much the costs matter depends on who suffers greatest under the sanctions. All too often, the costs of sanctions are felt disproportionately by disempowered masses in dictatorships while the ruling elites enjoy insulation from the hardships and austerity measures necessitated by the sanctions. Some *types of sanctions*, however, are more likely to affect leaders than masses and therefore should be expected to be more effective. For instance, Dashti-Gibson et al. argue that "financial sanctions may be more effective [than trade restrictions], in that while they are surely capable of inflicting damage on the public, they may also have a more direct and immediate impact on ruling elites by limiting their access to foreign currency" (1997, p. 610).

Sanction success is also affected by the reaction of the target. Galtung (1967) was among the first to write of a "rally around the flag" effect whereby sanctions fuel a patriotic response to outside pressure. Sanctions might serve to unite the domestic forces in the target state in support of their government against the interventionist policy of the sender (HSE, 1990, p. 12). This is especially probable since most recent sanctions episodes have involved developed Western states (mostly the United States) imposing sanctions against less developed Third World nations. The battle cry of anti-imperialism often resonates in those states as the people suffer under the weight of the sanction. When would such nationalistic backlash be more likely? Since support for one's own government is presumably correlated with the legitimacy of that government, the *domestic stability of the target* serves as an indicator of the potential for backlash. The greater the present stability, the less likely that domestic forces in the target state will rise up against their government.

HSE also discuss the potential emergence of a "black knight" that provides *assistance to the target*, thereby undermining the force of the sanctions. The target country might have a powerful ally that can absorb some of the economic pressure caused by the sanctions by providing an alternative market for boycotted goods and by giving aid to ameliorate the economic hardships caused by sanctions (HSE, 1990, p. 12). This point underscores the importance of *cooperation with the sender*. Lisa Martin argues that "although the goals of sanctions are highly political, states' ability to use sanctions is subject to the rules of economic exchange. This means that unilateral sanctions—those undertaken by just one government—usually fail because the target can find alternative markets or suppliers of the sanctioned goods" (1992, p. 3).

For Martin, then, the success of sanctions is dependent on the ability of the principal sender to ensure the cooperation of the other states in the international system. The collective action problem this represents is not insignificant, and the difficulty of overcoming it has been thought to be a large reason so few sanctions succeed.

Recent work by Drezner (2000) suggests that the conventional wisdom on the utility of cooperation with the sender is incorrect. Drezner argues that multilateral sanctions are often counterproductive for three reasons. First, senders are more likely to seek cooperation in cases where the target is tough and less likely to accede. Second, successful bargaining on the part of the senders makes compromise with the target less likely. And, finally, the primary sender is unable to maintain the cooperation due to defections amongst its allies (Drezner, 2000, p. 74).

Other work by Drezner (1998, 1999) suggests that sanctions are more likely to succeed if the sender and target enjoyed cordial previous relations. "Facing an adversarial sender, the target will be worried about the long-run implications of acquiescing. Because it expects frequent conflicts, the target will be concerned about concessions in the present undercutting its bargaining position in future interactions . . . Ceteris paribus, targets will concede more to allies than adversaries" (Drezner, 1998, p. 711).

A final reason that some sanctions succeed where others fail might have to do with the goals of the sanction. Dashti-Gibson et al. distinguish between two types of sanctions: "those that are designed to compel the target to make some concrete change in its policies (e.g., South Africa to end apartheid), and those that are entirely punitive (e.g., United States sanctions against Cuba to destabilize the Castro regime)" (1997, p. 610). Since the former goal requires a conscious decision on the part of the target to alter its policies, one expects sanctions with such policy goals to be less successful than their destabilization counterparts. Along similar lines, sanctions that have major goals (e.g., impair the military potential of the target or change the target's policies in a major way) are less likely to succeed because the price of concession is too high for leaders of the target state.

Replication of Conventional Wisdom

The primary data set used in all the empirical analyses considered above is based on work by HSE (1990). HSE collect data on 115 sanction episodes since 1914 and, as such, is the only large-n data set available to political scientists studying sanctions. One problem with HSE's effort is that they only use English language sources to generate the data, which means they probably omit sanctions between second and third rank powers since these were less likely to be reported in Western or U.S. newspapers (HSE, 1990, p. 4). This simple data collection rule introduces sample selection bias into our results since sanctions by smaller powers are systematically excluded from the data set. To avoid such selection bias, I limit the data to only sanctions with the United States as sender since 1945 (Pahre, 1998 makes the same decision for the same reason). Doing so causes the loss of some data but ensures us that our data represents the full universe of possible cases since all U.S. sanctions since 1945 are highly likely to have been reported in English language newspapers. Also, since the U.S. is increasingly the principal user of sanctions (HSE, 1990),

Table 1
Selection Bias in Studies of Sanctions Efficacy

| HSE Dataset | | Disaggregated HSE Dataset | | |
|-------------------------------|-----------|---------------------------|----------|-------------------|
| <u>Y1: Sanction?</u> | | | | |
| Target is in the | | 0.958*** | | 1.327*** |
| Western Hemisphere | | (0.349) | | (0.382) |
| % of trade that is | | -0.019*** | | -0.027*** |
| with the U.S. | | (0.007) | | (0.007) |
| Target is a major power | | -0.512 | | -0.671* |
| | | (0.404) | | (0.373) |
| Dyadic MID during year | | 1.234*** | | 1.134*** |
| | | (0.236) | | (0.231) |
| Target was member of | | 0.527** | | 0.334 |
| Soviet Bloc | | (0.236) | | (0.274) |
| Target's regime type | | -0.010 | | -0.041 |
| | | (0.029) | | (0.027) |
| Constant | | -0.503 | | -0.233 |
| | | (0.803) | | (0.727) |
| <u>Y2: Sanction Succeeds?</u> | | | | |
| Target's regime type | | | 0.125** | 0.126** |
| | | | (0.059) | (0.057) |
| Log of total cost as | 0.122 | | 0.439** | 0.232* |
| % of GNP | (0.096) | | (0.181) | (0.125) |
| Relative power | 0.118 | | -0.269** | -0.096 |
| | (0.331) | | (0.129) | (0.077) |
| % of trade that is | 0.247 | | -0.010 | 0.012 |
| with the U.S. | (0.479) | | (0.012) | (0.009) |
| Target's domestic stability | 0.003 | | 0.477 | 0.227 |
| | (0.012) | | (0.303) | (0.171) |
| Target has alliance | -0.919*** | | 0.255 | -0.604* |
| with the U.S. | (0.355) | | (0.292) | (0.306) |
| Prior cordial relations | -0.033 | | -0.541 | -0.014 |
| | (0.091) | | (0.566) | (0.317) |
| Dyadic MID during year | -0.242 | | -0.832 | -1.454*** |
| | (0.427) | | (0.544) | (0.402) |
| Black Knight? | 0.562 | | 0.579 | 0.250 |
| | (0.382) | | (0.424) | (0.226) |
| U.S. had cooperation | | | -0.945 | -0.514* |
| | | | (0.378) | (0.279) |
| Major goal? | | | -0.564 | -0.166 |
| | | | (0.427) | (0.257) |
| Constant | 0.102 | | 5.765 | 4.240*** |
| | (0.542) | | (3.128) | (2.016) |
| Sample size: | 71 | 4080 | 205 | Y1: 3783, Y2: 204 |
| Model Chi-squared: | | 50.36*** | 50.80*** | 69.40*** |
| Rho: | | | | -0.849*** |
| Standard error of Rho: | | | | (0.127) |

Note: Standard errors (in parentheses) are robust and corrected for clustering on target.

All estimations conducted in Stata 6.0

* = $p < 0.10$ ** = $p < 0.05$ *** = $p < 0.01$

limiting the data to just U.S. sanctions neither eliminates as many cases as one might fear nor compromises the generalizability of the results too greatly.

Using this data set of post-World War II U.S. sanctions, I replicate the standard results of the literature. The dependent variable is a dichotomous measure of success as coded by HSE (1990). The literature review above presents a coherent set of testable hypotheses that have been proffered by various scholars over the past 40 years. I combine these into an "industry standard" model of the determinants of sanctions success and conform to standard coding rules of the various variables (All variables are from HSE, 1990; for coding rules, see HSE, 1990).

The first model presented in Table 1 is disappointing with only cooperation with sender being statistically significant. The signs of the various coefficients are in the theoretically expected direction and, interestingly, Drezner's paradox of economic cooperation is confirmed by the data. In fact, the only variable that reaches conventional levels of statistical significance is the dichotomous indicator for cooperation with the sender. Moreover, this result is robust (in the Leamer sense of the term) since including a control for the type of sanction does not alter the result. The disappointing feature of these results is that in both models *no other* variables come near significance statistically. More troubling is that in all but two cases the 95% confidence interval includes 0, so we can not even be certain about the sign of the coefficients. Other than the cooperation variable discussed above, the only other variables worth discussing are total cost to the target (as a proportion of GNP) and a dummy variable for previous relations between sender and target. Conventional wisdom holds that increasing costs should increase the probability a sanction succeeds, and Drezner's (1998) conflict expectation model hypothesizes that targets that have cordial prior relations with the sender should be more likely to concede. While neither variable is significant at conventional levels, in both cases the confidence interval lies in the correct directions (positive for both).

A UNIFIED MODEL OF SANCTION IMPOSITION AND SUCCESS

The results thus far do not inspire much confidence in our general theoretical model of sanctions success. I argue that these nonfindings are a result of the selection bias inherent in their design.

Consider Figure 1, which represents an extremely simple sanctions game. In this stylized game, State A, or the target, takes some policy. State B, or the sender, objects to the policy and must decide whether or not to do something about it. If the sender chooses to impose sanctions, then the target must decide whether or not change its behavior or to resist the sanctions.

The key point here is that the occurrence of sanctions is intrinsically linked to their success (Smith, 1995, p. 229). This is not a point that has been lost on students of economic sanctions. Morgan and Miers write, for instance, that, "empirical investigations focusing only on cases in which sanctions have been applied would suffer from a severe selection bias" (1999, p. 12). Hart states that there exists an "interesting link between domestic politics, strategic selection, signaling and foreign policy" (2000, p. 281), and Bolks and Al-Sowayel suggest that "discrete events, like economic sanctions, highlight issues of selection and sampling effects associated with

policy outcomes" (2000: 242 fn3).

Why is it a problem to look only at cases of sanctions when evaluating the determinants of sanction success? Well, as Figure 1 suggests, senders make a strategic choice as to whether or not they impose a sanction. Therefore, the set of observed sanctions represents a nonrandom sample of cases, and therefore we are unable to infer the causes of success with much certainty (see Achen, 1986, for the seminal discussion of the statistical problems caused by nonrandom assignment in social science quasi-experiments). In effect, we do not see a set of cases (i.e., our data are censored) because the sender chooses not to impose a sanction due to some internal

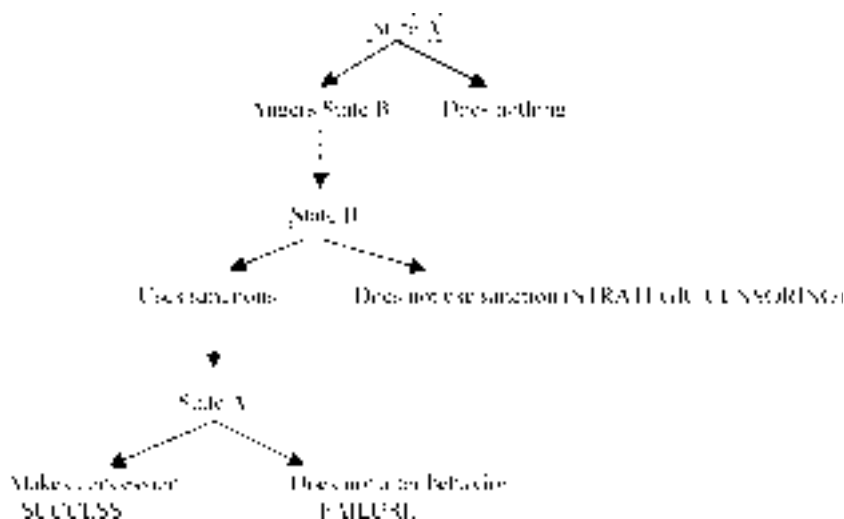


Figure 1. An extremely simple sanctions game.

reasoning, which is a problem if the decision to impose a sanction is related to the probability of success. Such strategic censoring is rife in international relations (see Lemke and Reed, 2001, for an application of strategic censoring to rivalry studies), although we have only recently begun to account for it.

Yet despite this recognition of the substantive importance of accounting for such selection bias, *not a single* published empirical study of sanctions success models the selection process. The statistical consequence of ignoring such selection bias is inconsistency and inefficiency in our estimates. The substantive consequence is that our conventional wisdom about sanctions must be cast in some doubt since it is based largely on these flawed studies.

The crux of the critique presented above is that the decision to intervene through the use of a sanction is biased (Smith, 1996) and that this bias corrupts our results regarding sanction efficacy unless we account for it. Substantively, the critique should be thought of as about process rather than outcomes. Ignoring the *process* by which a sender nation decides to impose a sanction biases our results about the outcomes of

these decisions. If the factors affecting the decision to impose a sanction are correlated with their probability of success, our estimates of the impact various independent variables have on success will be systematically biased and therefore untrustworthy. To account for this, one has to model explicitly the process by which a sender nation chooses to impose sanctions. It is to this task that I now turn.

A Model of Sanction Imposition

Sanctions may be imposed for a variety of reasons. As discussed earlier, sanctions are just one of a set of policy options that could be used by a sender to accomplish some end. So the question of why states use sanctions must logically be answered on two separate levels. The first must tackle the question of why a politician might choose to use sanctions as opposed to any of the other policy options to which they have access. A simple rational-choice perspective would argue that the answer should lie in some expected utility maximization process carried out by leaders. Accordingly, the reasoning might be that given some set of goals, economic sanctions represent the most cost efficient way of getting one's way. "Because economic sanctions can impose costs (both on the states that employ them and on their targets) without carrying the degree of risk attached to military actions, governments use them to signal resolve and exert pressure for policy changes" (Martin, 1992, p. 3). Along similar lines, HSE posit that "world leaders often find the most obvious alternatives to economic sanctions unsatisfactory—military action would be too massive, and diplomatic protest too meager" (HSE, 1990, p. 13).

So, given that leaders decide that economic sanctions are an effective means to achieving some set of ends, the next question concerns the nature of these ends. The traditional view is that states use sanctions for foreign policy purposes or, more precisely, to effect "changes expressly and purportedly sought by the sender state in the political behavior of the target state" (HSE, 1990, p. 2). This foreign policy approach is not without its critics. David Baldwin uses a social-power framework to posit multiple objectives to the sender government. His argument is that the audience to which the sender is performing in enacting the sanction might not be the target state at all, but rather the domestic interest group lobbies that demand something be done against the target state (Baldwin, 1985, pp. 130–134). HSE imply just as much when they write that "sanctions can provide a satisfying *theatrical display*, yet avoid the high costs of war" (HSE, 1990, p. 13; emphasis mine).

Kaempfer and Lowenberg (1988) summarize these two views well. They term the traditional approach "instrumental." According to the instrumental theory, the purpose of sanctions is "to bring about policy change in the target nation through imposing the severest possible economic harm" (1988, p. 786). The alternative approach, which they label the "expressive" theory of sanctions, suggests that "sanctions might have an altogether different goal—namely, to serve the interests of pressure groups within the sender country" (1986, p. 786).

The expressive theory of sanctions has gained ground in recent years. Perhaps it reflects the increasing cynicism of our times, but the notion that politicians manipulate foreign policy for domestic policy concerns is hardly incredible. Pahre offers a provocative statement to this effect (1998). "Policy makers," he writes, "must be

maximizing some utility other than change in the target country's behavior" (1998, p. 3). Pahre utilizes the concepts of *subversion*, *deterrence*, and *symbolism* to describe these alternative ends. Accordingly, Pahre writes that when "sanctions seek 'compliance,' they rarely succeed but their symbolic goals may be important both domestically and internationally. Sanctions may also deter future "misbehavior" . . . [and] . . . might also serve political ends in the sender country" (Pahre, 1998, p. 3).

For policy analysis, the question of *why* sanctions are used is vital, for it is meaningless to concern oneself with the success of sanctions unless one has a proper understanding of the meaning of "success." Consider the case of Cuba (HSE Case # 60-3). Since 1960, the United States has maintained sanctions against Castro's communist regime. From an instrumental theory perspective, the foreign policy goal of the sanction was to destabilize the Castro government¹ (HSE, 1990, p. 19). Given that Castro is still in power forty years later, it is doubtful that many would argue that sanctions against Cuba have succeeded. But, approaching this particular case from an expressive theory point of view yields a different interpretation. Florida contains a large number of politically vocal Cuban immigrants who resent the Castro dictatorship and lobby extensively for the maintenance of sanctions against Cuba. Any administration that repealed the sanctions would run the risk of having its action being interpreted as being soft on communism and as being anti-Cuban-American. As such, the sanctions serve a symbolic purpose, signaling to the world that America will not condone communism in its own backyard. From an expressive point of view then, one could argue plausibly that the sanctions against Cuba have succeeded.

The question of success is clouded by the ambiguity of original purpose. Leaders justify sanctions in foreign policy terms, and it is unlikely that any leader has openly defended a sanction as a means to retaining office. Absent such data, though, despite the reasoning underlying the sanction, the outcome is observationally equivalent. That is, whether sanctions are imposed to achieve foreign policy goals or to send signals to domestic audiences, the outcome is a sanction against some target. Resolving this issue requires researchers to focus on the politics of sanctions at the domestic level (Morgan and Schweback, 1995) and to develop more thoroughly a political model for why sanctions are used. This, however, is not the goal of this paper and therefore I make two simple assumptions about the sender's leaders.

First, I assume that leaders seek to remain in office. Second, I assume leaders would rather a sanction work than fail. The first assumption is commonplace, the second intuitive. If the instrumental theory of sanctions is correct, then the second assumption is obvious. And if the expressive theory of sanctions is correct, then the assumption simply suggests that a successful sanction would please domestic interest groups more than one that failed. Regardless of why the sanction was imposed, a successful sanction makes the sender's leader look good. If Castro's government were to fall in Cuba, U.S. leaders would undoubtedly attribute his demise to half a century of sanctions even if the only reason they have been imposed and maintained over that period was purely domestic politics.

Intuitively, the interaction between the sender and target is a bargaining game: Both states have a preferred outcome from their interaction and the sender seeks concessions from the target by using the sanction as leverage. Therefore, a simple hypothesis is that a sender is more likely to impose a sanction on another state if it

holds a bargaining advantage over the potential target. That is, a sender might be more inclined to pick a fight it thinks it can win. But this intuitive conclusion illustrates well the problems created by sample selection effects. Morgan and Miers (1999) write that the opposite is probably what we would actually see in the empirical record. "It is distinctly possible that in those cases where sanctions would bring about a change in the target state's behavior, the threat of sanctions alone might be sufficient to produce the desired effect. That is, we might see sanctions actually imposed only in those cases in which they are unlikely to succeed so that empirical analyses based only on cases in which sanctions were applied might face a serious selection effects problem" (Morgan and Miers, 1999, p. 1).

A second set of hypotheses about the decision to get involved stem from the strategic nature of coercion decisions. State-centric IR theorists, whether of the realist or liberal school, suggest that states act to maximize utility even though they might disagree about what ought to be included in the utility function. In the case of sanctions, I argue that a sender is more likely to choose to get involved in a situation when it senses a strategic objective might be at stake. In the case of the United States during the time period in question (i.e., the Cold War), strategic interests were heavily influenced by geopolitical considerations. Therefore, I hypothesize that the relationship of the potential target with the Soviet Union influenced the United States's sanction decision. Also, for the same reasons, the geographical location of the potential target is relevant. States closer to the sender will receive more attention since they are likely to be both more strategically important and more vulnerable to economic sanctions (states close to each other are far more likely to trade with each other than states separated by immense distances).

The final hypothesis that will be tested concerns the regime type of the target. While the statistical results have not typically cooperated, there exists a consensus among theorists of economic sanctions that costs to the target are a key factor in predicting success of a sanction. Sanctions that are extremely damaging to the target's economy are expected to be far more likely to succeed. The causal mechanism implicit in this hypothesis is that leaders of the target state engage in a rational cost-benefit analysis and find it harder to justify resisting the sender's pressure as the costs of the sanction go up. Recent studies, however, point out that "costs often affect ordinary citizens far more than the entrenched elites who actually make policy decisions" (Dashti-Gibson et al., 1997, p. 610; for similar sentiments, see Galtung, 1967, and Simon, 1995, p. 205). A related critique of the cost hypothesis is that sanctions that affect more politically influential sectors of the target are more likely to succeed (Kaempfer and Lowenberg, 1988, p. 720–721; Morgan and Schwebach, 1995, p. 252).

Scholars argue that sanctions that impose costs only on the masses but leave elites unharmed are less likely to succeed since elites make the policy decisions. What they miss, of course, is that the type of regime in place mediates the relationship between masses and elites. In a democracy, political leaders are assumed to be motivated primarily by the prospect of reelection and therefore of pleasing their domestic constituencies. Therefore, elites are not insulated from high sanction costs even if these are disproportionately borne by the masses. Presumably the suffering electorate would signal to their leaders that continued hardship is unacceptable and therefore a demo-

cratic target's leaders ought to be more willing to concede to the sanction. The implications of this logic extend also to the decision of the sender to impose the sanction. Assuming as I do that senders would rather the sanction succeed than fail, it therefore follows that senders should be more likely to sanction democracies than nondemocracies.

To test these hypotheses about sanction imposition, I use six independent variables to model the process by which some states are selected to receive a sanction. These are: 1) whether the target is in the Western Hemisphere: since the sender is the United States in all cases considered here, I argue that the United States is more likely to use sanctions against states that are geographically proximate both for strategic reasons (a modern Monroe Doctrine) and for practical reasons (these states are more likely to be dependent on the United States for trade); 2) the dyadic trade levels between the United States and target: the higher the presanction trade level the greater the leverage for the sender; 3) whether the target is a major power that could retaliate against the United States; 4) whether there was a militarized interstate dispute (MID) between the target and the United States in that given year; 5) whether the target was a member of the Soviet bloc; and 6) the regime type of the target.

Other Methodological Issues

Before we can get to the results of this selection-corrected model, a brief consideration of the methodological issues entailed must occur. The notion of sample selection implies that there exists a set of potential targets but that sanctions are only imposed on some subset of them. To model the selection process then we have to collect data on these potential targets. Doing so requires us to specify the set of targets from which the sender can choose.

The question of the full universe of cases from which the sender must choose its targets is worthy of some discussion. Assume for instance that the sender in question is the United States. One might plausibly argue that in any given year not all dyads of which the United States is a part are equally likely to witness a sanction imposed by the United States. After all, if the other state in the dyad never does anything to offend the sensibilities of the United States, then there is no reason that the United States would arbitrarily impose a sanction. Ideally, therefore, one would want to establish some criterion under which a sanction is more likely to be used than not and then use this subsample to model the probability of sanction imposition.² But as the HSE data show the stated reasons for sanctions being imposed are rather disparate. Arguing that the relevant population, from which the sender chooses its targets, is limited by a particular policy goal is liable to impose structure where there is none. Clearly this decision of what the appropriate sample is depends on the goals of one's research project. In my case, since I am interested in diagnosing whether selection bias is a problem in our analyses of the general effectiveness of all sanctions, I believe that my decision to use all U.S. dyads is justified.

The decision to use sender-target dyads (all U.S. dyads in this paper) as the potential set of targets in the first stage problematizes the unit of analysis decision in the second stage. HSE's approach is to collect data on a "sanction episode." A sanction episode refers to the interaction between sender and target from the moment a sanc-

tion is imposed to when it is removed. Why should we believe that a sanction episode as defined by HSE is the correct unit of analysis? The question one must ask is whether one believes that each sanction episode was produced by the *same* data generating process. In fact, one might argue that a sanction that succeeds after ten years is qualitatively different from one that succeeds after two. Put another way, does it make sense to conceive of both as random draws from the *same* underlying distribution of sanction episodes?

Sanction episodes are clearly the incorrect unit of analysis. Episodes are not created by some common underlying data-generating process but rather by the aggregation of the true unit of analysis, i.e., annual observations on a dyad experiencing a sanction. Each year the sender must make a decision to impose a sanction, or, having already done so, to retain it. Similarly, once a sanction is imposed, the target must decide to resist or concede each year.

Note that this implies that a sanction might succeed or fail each year it is in place rather than only in its final year as is currently the case. The current method of collecting data by sanctions episode implies that sanctions end either 1) when they succeed, or 2) when the sender no longer wishes to continue them despite not having achieved its goals yet. As such, in the case of successful sanctions, it is implied that the success occurs in the final year of the sanction. But this need not be so, especially for sanctions that have more intangible goals such as destabilizing another government. It is conceivable, even likely, that the sanction will create some destabilization any year it is in effect, thereby possibly constituting a partial success in years other than the final one. If the sender believes that there are more concessions to be gained, the sanction might be continued but continuation should not be interpreted as a lack of success up till that point. Collecting success data at this annual level of analysis would allow us therefore to study the dynamics of sanction processes. In the absence of such data, I code success conservatively and code a sanction as successful only in its last year of existence.

Using HSE's sanction episodes causes one other methodological problem. A standard assumption in statistics is that our disturbances are purely random draws from the population. If this is not true, then our data violates the Gauss-Markov assumption that $E[\varepsilon_i | \mathbf{X}] = 0$. Put another way, the standard assumption is that the "Mean of each ε_i conditioned on all observations x_i is zero. This conditional mean assumption states that no observation on \mathbf{x} convey information about the expected value of the disturbance. It is conceivable . . . that although x_i might provide no information about $E[\varepsilon_i | \cdot]$, x_j at some other observation . . . might" (Greene, 1997, p. 231).

During the 1970s the United States imposed two separate sanctions on Brazil (HSE Cases 77-7 and 78-2) which ran concurrently. Surely it is plausible to suggest that the first sanction affected the probability of success for the second sanction. To see the problem more starkly, consider what considering these as separate and independent observations implies. Stated differently, this suggests that in 1979, when there were two separate sanctions in place against Brazil, the cost of each sanction is independent of the other and that the probability that either succeeds can somehow be linked only to the cost imposed by that particular sanction. Surely it is more plausible to argue that if higher costs do lead to sanction success then in years where there are multiple sanctions in place the cost of sanctions to the target is the sum of

each and that this cumulative cost affects the joint probability that either or both sanction might succeed. Disaggregating the data set into annual observations accounts for this problem. In years that more than one sanction existed on the same target, the cost to the target is the sum total of the various estimated annual costs of each sanction.

RESULTS

Using this disaggregated dyad-year version of the HSE database, I model the selection bias using a censored probit model. This model has the advantage of estimating an additional parameter ρ , which indicates the statistical linkage between the two dependent variables, sanction imposition, and sanction success, by indicating the covariance between the disturbances (Achen, 1986; Reed, 2000; Lemke and Reed, 2001). A statistically significant ρ would support the claim that the two processes of imposition and success are linked and should not be considered separately.

Having constructed a basic empirical model of sanction imposition, the remainder of the exercise is fairly straightforward. First, I reestimate the probability of success of a sanction using the disaggregated sanctions database, which I created using HSE's original data. These results are directly comparable to those reported in Table 1 and allow us to see how construing a different data-generating process and therefore a different unit of analysis (i.e., dyad-year as opposed to sanction episode) affects our findings. Second, and most importantly, I estimate a selection-corrected censored probit model of sanction success in which the first stage equation is a model of sanction imposition and the second stage is a model of sanction success.

Using this data set, I first estimate my models of sanction imposition and success separately. Then I combine them in a censored probit model to correct for selection bias. The second column of Table 1 gives the results of my model of sanction imposition. As expected, the United States is more likely to use sanctions on states located in the Western Hemisphere. However, presanction dyadic trade is negatively related with sanction imposition. That is, contrary to expectations, the higher the U.S. share of the potential target's trade, the less likely it is that the United States imposed a sanction on it. This result, which at first is counterintuitive from a bargaining viewpoint, makes sense when one considers the Morgan and Miers's (1999) point that trade dependent states are more likely to concede to the mere threat of sanctions, and therefore we do not observe sanctions in those cases. An alternative explanation for this result is that these states are too valuable to the U.S. economy and therefore less likely to be punished. The other results are in accordance with our expectations. Major powers are less likely to be sanctioned. States involved in a MID with the United States are more likely to be sanctioned, which suggests that senders might often use sanctions as a complement to military tactics as in the Gulf War. Soviet bloc members were more likely to be sanctioned by the United States as a result of Cold War containment doctrine. Finally, the target's regime type is not statistically significant and the 95 percent confidence interval on this variable includes zero so we can have little confidence in its coefficient.

The bottom half of Table 1 shows the results of three separate statistical models of

sanctions success. Column 1 is an independent probit using the original HSE data, column 3 is also a single probit regression but this time with the disaggregated annual HSE data, and column 4 is the censored probit model used to correct the selection bias in the previous models.³ The results of the censored probit are different both substantively and statistically than either of the independent probits, and it is to a comparison of these results that I now turn.

The estimates of the sanctions success model are considerably different when one accounts for the selection process. First, the target's regime type matters. Sanctions against democracies are more likely to succeed and this result is statistically significant. Recall that Hart (2000) also tested this hypothesis but found that it was not significant. Correcting for selection alters this finding in an important manner. Second, the cost of the sanction also matters and is positively related to sanction success but the size of the coefficient is half of what it is in the independent probit model. Third, Drezner's (1998, 1999) conflict expectations model predicts that targets should be more likely to concede to allies than to adversaries. However, targets that had an alliance with the United States were less likely to concede to the sanction, and this result is statistically significant. Fourth, states who were embroiled in a MID with the United States (clearly adversaries of the United States) are found to be far less likely to concede with the single largest coefficient in the model. And, finally, this paper finds that cooperation on sanctions can often be counterproductive.

None of the other conventional hypotheses receive any support once I correct for strategic censoring. In general, though, the size of the coefficients do change quite dramatically for the majority of the included variables, thereby altering our appraisal of their relative contribution to the success of sanctions. Clearly selection bias is a problem, and analyses that ignore it are likely to generate incorrect and inconsistent findings.

CONCLUSION

This paper makes an important contribution to the study of sanction efficacy. I argue that existing empirical analyses are biased because they use a problematic data set and ignore issues of selection bias. After disaggregating the data and controlling for selection bias, I find that the following factors affect the likelihood that a particular sanction will succeed: target regime type, total costs to the sender, whether the target is in an alliance with the United States, whether the target and the United States were involved in an MID with each other, and whether the United States had cooperation in imposing the sanction.

My findings signal a direction for future research on sanctions as well. Obviously simply disaggregating already aggregated data is inadequate in the long run, although it suffices well as a diagnostic exercise in the short run. Future efforts to study sanctions need to begin with a careful consideration of research design questions that inform efforts of primary data collection. Second, given the development of a vibrant literature in international relations regarding selection bias, ignoring such issues in studies of sanctions is no longer excusable. Instead, attempts must be made to create unified theories of sanctions that begin with explanations for why sanctions are selectively used and end with a consideration of how such selection processes

affect the probability of success. My model of sanction imposition is clearly a first cut at just such an effort, and I hope that this paper will spur future research to tackle the questions raised here more thoroughly. Finally, my finding that democracies are more likely to yield to sanctions places the target's regime type squarely on the research agenda, where it awaits more careful theories about why different regimes might be more susceptible to economic statecraft.

NOTES

1. This is one of the three goals of the sanction according to HSE. The other two goals were 1) to settle expropriation claims and 2) to discourage Cuba from foreign military adventures (HSE, 1990, p. 19).
2. For instance, one might think that countries that are considered human rights violators are more likely to have sanctions imposed upon them than other states (Weiss, 1999). But only 23.6% (28 out of 119 cases) of HSE's sanction episodes list improvement of human rights (I included better treatment of dissidents and ending support for terrorism in this category) as one of the stated policy goals of the sanctions. Indeed, it appears that human rights were only a motivation during the Carter presidency.
3. Column 4 of Table 1 presents the results of the censored probit sanctions model. The coefficient on ρ is highly significant both statistically and substantively. The point estimate for ρ is over six times larger than its standard error and the sign on the coefficient is negative. Thus, it appears that the process by which sanctions succeed is not independent of the process by which they are imposed, and, specifically, the negative sign indicates that the unobserved factors that lead to sanction imposition are negatively related to the probability of their success.

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