

The Threat and Imposition of Economic Sanctions, 1971–2000*

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Economic sanctions, increasingly used as instruments of foreign policy in recent decades, have been the focus of numerous academic studies. Recent theoretical advances in our understanding of sanctions cannot be tested adequately with existing data. This article presents a newly developed dataset that contains information on 888 cases in which sanctions were threatened and/or implemented in the 1971–2000 period. We describe the dataset, present descriptive statistics for some of the key variables included, and offer comparisons with the Hufbauer, Schott, and Elliot dataset on sanctions that has been frequently used in previous research. We also present simple statistical relationships between sanctions outcomes and some of the variables commonly believed to affect sanctions success.

KEYWORDS: dataset; economic coercion; economic sanctions; threats

A great deal of recent research has been devoted to the subject of economic sanctions. In many respects, this body of work represents science at its best. In particular, there have been strong connections between theory and evidence and the work has been cumulative. Early work was based on case studies focusing on prominent examples, such as the League of Nations sanctions on Italy over its invasion of Ethiopia or the long-lasting US sanctions on Cuba, and led to a

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consensus that sanctions are ineffective at inducing target states to change their policies (see e.g. Galtung, 1967; Hoffman, 1967; Baer, 1973; Schreiber, 1973; Olson, 1979; von Amerongen, 1980; Wallenstein, 1983). Several scholars then provided theoretical arguments explaining why sanctions would not “work” to bring about changes in target state policies (e.g. Barber, 1979; Baldwin, 1985; Lindsay, 1986; Wagner, 1988; Nossal, 1989; Tsebelis, 1990). Scholars were uneasy generalizing from case studies, however, and Hufbauer, Schott, and Elliot (1990) developed a large-N dataset (hereafter HSE) on which hypotheses regarding sanctions could be tested. A wave of quantitative empirical work followed, most of which demonstrated that while sanctions seldom induce targets to change their behavior, some identifiable factors do contribute to sanctions success (e.g. Lam, 1990; Dashti-Gibson, Davis, and Radcliff, 1997; Morgan and Schwebach, 1997; Bonetti, 1998; Drezner, 1998). Clearly, empirical studies have tested the best theories available and new theoretical arguments have been developed to explain novel empirical findings. Each new set of studies has built upon the last and we have seen our understanding improve with every step.

We have recently reached a point in this progression where available data are no longer suitable for testing our most recent theoretical advances. Specifically, many recent theoretical arguments suggest that threats are an important component in how sanctions episodes unfold. Targets can often anticipate that they would succumb to the pressures of sanctions and, to avoid the costs, they will alter their behavior at the mere threat of sanctions (see Smith, 1996; Morgan and Miers, 1999; Drezner, 2003). Since we would not observe sanctions in those cases in which they would be most effective, empirical work based on the HSE dataset, which includes only cases in which sanctions were actually imposed, might thereby seriously underestimate the political effectiveness of sanctions.

Moreover, many of the newer theories identify interaction effects among key independent variables. Miers and Morgan (2002), for example, suggest that the effectiveness of sanctions is a product of the interaction among the number of senders, the number of issues under dispute, and whether international organizations are involved. Similarly, McGillivray and Stam (2004) suggest that leadership changes, polity type, and the interaction between them are among the most important determinants of the duration of economic sanctions. To be sure, statistical analyses incorporating many independent variables and interactions among them have been conducted using the HSE data; but there are too few cases in the HSE data to afford much confidence in the results. Finally, some recent studies have addressed the question of which dyads are most prone to experiencing economic sanctions (Lektzian and Souva, 2003; Cox and Drury, 2007; Goenner, 2007) as well as whether sanctions increase the risk of militarized conflict (Lektzian and Sprecher, 2007). The reliance on the HSE dataset limits the conclusions of such studies because, as we show below, the dataset records only a rather modest part of all historically occurring sanctions cases and there are clear biases in the cases omitted.

In order to continue to advance our understanding of economic sanctions by testing hypotheses derived from recent theory, new data are needed. A new dataset on

the Threat and Imposition of Economic Sanctions (TIES) has been developed for precisely this reason. This article presents this dataset. In the next section, we describe the data and we briefly discuss some of the key decisions made in the process of collecting them. We then provide some descriptive statistics for several of the key variables included in the dataset and we offer comparisons between TIES and HSE. We conclude by presenting some simple associations between some variables that many believe contribute to sanctions success and our variables capturing sanctions outcomes. Our aim is to introduce the TIES data and to convince the reader that these data are quite useful for testing many hypotheses regarding sanctions.

The Data

The Threat and Imposition of Economic Sanctions (TIES) dataset includes 888 cases in which one or more states threatened and/or imposed economic sanctions on a single target. The current version of the dataset covers the 1971–2000 time period.¹ For our purposes, economic sanctions are defined as: *actions that one or more countries take to limit or end their economic relations with a target country in an effort to persuade that country to change its policies. By definition, a sanction must (1) involve one or more sender states and a target state, and (2) be implemented by the sender in order to change the behavior of the target state.* By this, we mean that for actions taken to limit an economic relationship to be considered sanctions, they must be accompanied by a demand that the target change some aspect of its behavior. These demands can be for changes in economic policies (e.g. tariffs or other trade behavior) as well as for changes in political, military, or any other type of behavior.² Actions taken by states that restrict economic relations with other countries for solely domestic economic policy reasons (e.g. restrictions based on consumer safety or environmental standards) therefore *do not* qualify as sanctions. Sanctions may take many forms, including actions such as tariffs, export controls, embargoes, import bans, travel bans, asset freezes, aid cuts, and blockades. For the purposes of this dataset, all sanctions cases may include only one target state. If a sender state(s) makes threats against multiple targets, a new case is created for each individual target.

The data for these decades were primarily gathered using Lexis-Nexis, as well as the Facts on File and Keesing's Record of World Events online databases. In the first stage of the process, we produced a list of candidate cases through a

¹Our original goal was to extend the temporal domain of the dataset back to 1945. We had estimated that we would find around 400 cases occurring between 1945 and 2000. Since there were many more cases than we anticipated, resources permitted gathering data only for the shorter time span.

²Some have adopted the position that “sanctions” used over economic issues (such as retaliatory tariffs) should not be included with sanctions over political issues in an evaluation of sanctions effectiveness. Thus, for example, the HSE dataset does not contain trade sanctions cases. We see no theoretical reason suggesting these cases should be excluded. Any user of our data who disagrees can, of course, simply remove the trade cases.

keyword search.³ We then researched extensively each of these candidate cases and prepared written case summaries of those cases believed to meet our requirements for inclusion in the dataset.⁴ Coders then used an MS Access template to code sanctions episodes from these written summaries.⁵

A sanctions episode is assumed to begin when the sender(s) makes a threat about the possibility of sanctions or imposes sanctions with no previous threat. A threat need not be specific; it must only declare that sanctions are a possibility against the target state. Threats may be initiated in several ways, such as verbal statements by government officials, drafting of legislation against a target state, or the passage of a conditional law against a target state stipulating that sanctions will be imposed if certain target behaviors are not changed. Moreover, we code threats made by any individual who holds an official foreign policy position that is sufficiently senior for their statements to be reflected in official documents or reflected in the public record. Since some sanctions are initiated by legislation, this includes even individual legislators. The dataset includes a variable identifying the individual issuing the sanctions threat, so any users believing that we should consider only threats made by very senior officials can exclude other cases.⁶

In several instances, sanctions are imposed as a result of legislation designed to monitor the behavior of potential target states. Examples include yearly determinations of whether a country is engaged in nuclear proliferation, drug trafficking, or the sponsorship of terrorism. The sender conducts routine investigations of the behaviors of potential targets on an annual basis. If this is the case, the initial passage of legislation is considered to be the start of an episode for countries that are violating the conditions of the law needed to avoid sanctions. Alternatively, if a country is in compliance with the law when it is passed but later violates the law, the episode is considered to have begun after the potential target chooses to violate the sender's law. This would allow an episode to begin with actions taken by the target state.

There is a cost to this coding decision in the sense that we miss some number of very real threats. We believe this number to be quite small, however, and we are certain that this decision is the most preferable among the alternatives. In a sense, the USA (and other countries) make multiple, annual threats against every other state in the world over issues such as human rights, nuclear proliferation, and the sponsorship of terrorism. For example, if the Canadian government were to adopt a policy of genocide toward the Quebecois, many countries, probably including the USA, would impose sanctions. If the Canadian government were to

³The keywords used to detect potential cases included sanctions, tariffs, export controls, embargoes, import bans, travel bans, asset freezes, aid cuts, and blockades.

⁴Naturally, many of the candidate cases proved to not meet our requirements and many others were merged into single cases. At this and subsequent stages of the winnowing process, we always tried to err on the side of inclusiveness.

⁵These summaries are currently not publicly available. We hope to make them so sometime in the future.

⁶By our definition, we are considering sanctions to be a nation-state-level phenomenon. We do not include threats made by subnational-level officials, nor do we include "sanctions" imposed by subnational-level units.

consider such a policy, they would probably realize that these sanctions would be imposed. Since Canada is not considering such a policy, we do not believe it appropriate to consider them as actually being under such a threat, however. The coding rule outlined above was adopted expressly to avoid including a large number of such cases in the dataset as instances of threatened sanctions.

A sanctions episode consists of a threat to impose sanctions, a threat and an imposition of sanctions, or the imposition of sanctions without a threat. Each case can consist of several individual episodes. For example, if a sender threatened to impose sanctions if the target tested a nuclear device and, at a later date, imposed sanctions because the threat was not heeded, these two episodes would comprise a single case. A sanctions episode is considered to start on the day when the sender makes a threat or implements sanctions. An episode is considered to end on the day when one or more of the following takes place: (a) the target acquiesces to the demands, (b) the sender changes the demands made on the target, (c) the composition of the group of sender states changes, (d) the type of sanctions threatened or imposed changes, or (e) the target state changes (which changes the case as well). Cases are identified as related sets of episodes. Cases start when their first episode starts and end when the last associated episode ends.

Each case contains a single target, but there may be several senders. For example, when the USA and Canada imposed sanctions on Pakistan and India over nuclear proliferation, we consider that to be two cases, one for India and one for Pakistan, with two senders each. The episodes in a case must also be related in terms of the issues at stake. There can be multi-issue episodes, provided that the threats and demands made over the issues are tied together. In other words, if there is a demand to improve human rights *and* stop supporting terrorists, that would comprise a single case. If, on the other hand, sanctions are imposed over human rights issues, and some time later additional sanctions are threatened over support for terrorism, those would be separate cases.

We had initially hoped to release the episode-level data to allow the systematic study of the evolution and unfolding of sanctions cases. We were not satisfied with the level of intercoder reliability, however, and elected to release only the version of the data in which the episodes are aggregated into cases. We tested several versions of the codebook and coder instructions in an effort to achieve a high level of intercoder reliability. In the episode-level data, coders too often disagreed over the precise start and end dates of episodes, as well as what activities were to be included in each episode. These disagreements cancelled out as the data were aggregated to the case level and we were able to achieve high levels of reliability.⁷

⁷Throughout the data collection process, we took great pains to continuously evaluate the reliability of our work. We had two or more people generate the candidate list for several years to insure that our processes were not missing possible cases; we have two, or more, written case summaries for many of our cases to insure consistency in the preparation of these documents; and we had two or more coders code many cases. In most of the reliability tests we conducted, we had exact agreement between coders over 90% of the time. We dropped a few variables from the dataset because we could not achieve exact agreement at least 75% of the time. Thus, for example, we had hoped to include variables that would characterize the costs of sanctions much more precisely. We found that coders were able to achieve acceptable reliability only for cruder measures of costs.

For each case, data are provided for several variables characterizing the nature of the threats and/or sanctions that occurred. An abridged list of variables, with some brief descriptions, are presented in the Appendix.⁸ The first set of variables identifies the target, up to five senders, the primary sender, and whether an international organization was involved in the threat or imposition of sanctions. If sanctions are issued under the auspices of an international organization, we also identify the organization by its abbreviation and specify whether or not the target was also a member of that organization. We use standard Correlates of War country codes to identify targets and senders with a couple of modifications. Most importantly, since a number of more recent sanctions have been imposed against the EU as a single entity, we use code 1000 to represent it.⁹ In addition, we provide the start and end dates of the sanctions case. The start date is the date of the first threat, or when sanctions are imposed if there was no preceding threat. A sanctions case can end in any of several ways. If the target makes sufficient concessions to satisfy the sender, the case ends when those concessions are made. A case can also end when the sender ends sanctions or publicly lifts the threat, regardless of the target's behavior. Furthermore, since our sanctions cases are target specific, we consider sanctions to end if the target state ceases to exist or changes identity; if sanctions continue against the new political entity, we would consider that as the start of a new case. Finally, as one might expect, some cases just fade away. Threats are made, but never carried out even though the target made no concessions. Sometimes, such threats are issued only once, other times they may be reiterated several times. In the absence of any evidence that a case was resolved, we code the end date as missing.

The next set of variables identifies characteristics of the threats. We specify the issue in dispute with a variable that allows for 14 categories (and a miscellaneous category). We also identify which political actor within the sender country issued the threat, the specificity of the demands, and the commitment of the sender defined as the relative ambiguity of the threat. We identify the type of sanctions threatened, as well as the type, if any, of carrots offered and the type, if any, of diplomatic sanctions also threatened. Other variables characterize the anticipated costs of the sanctions to the target and sender. These variables are presented as trichotomies (minor costs, major costs, severe costs). As noted above, we had hoped to be able to provide much more refined indicators of costs, but were unable to achieve a satisfactory level of intercoder reliability for finer characterizations. We also provide a variable that identifies the interests within the target state that are the focus of the threatened sanctions; that is, we identify whether the sanctions would principally harm the general population, the military, the regime, business interests, etc.

⁸A complete description of the variables included in the dataset, the coding rules for these variables, and the data can be found at <http://www.unc.edu/~bapat/TIES.htm>.

⁹When the EU is the sanctions sender, we code the presence of an international organization and list "EU" as the organization's abbreviation. In addition, we code Hong Kong as 1001 and Macau as 1002, as these entities have been targeted by sanctions over trade disputes. In several instances, sanctions were imposed on countries that COW starts recording in its state list at a later date.

Our next set of variables provides information on any sanctions that were actually imposed. These variables mirror those for threats, in that they seek to identify the type of sanctions imposed, whether and what type of carrots and diplomatic sanctions accompanied economic sanctions, what interests within the target were mainly affected by the sanctions, and the actual costs of the sanctions to the target and sender. We also provide a set of variables that characterize the outcome of the case. The first of these presents a tenfold categorical description of the outcome that conveys whether the target or sender partially or completely acquiesced at either the threat or imposition stage, whether there was a stalemate at either stage, or whether there was a negotiated settlement at either stage. Two additional variables specify the nature of the settlement for the sender and target on a 0–10 scale, with zero representing the worst possible outcome from that actor's perspective and 10 representing the best possible outcome.¹⁰

Descriptive Statistics and Comparisons with HSE

In this section, we present basic descriptive statistics for several of our variables and offer comparisons of these with those for the frequently used Hufbauer, Schott, and Elliott (HSE) dataset.¹¹ HSE, beginning in 1914 and ending in 2006, covers a longer time span than does TIES, yet TIES contains far more cases—888 compared with 204 in HSE, of which 139 occur in the 1971–2000 period covered by TIES. Table 1 presents the number of cases in each dataset by decade. We also provide the number of TIES cases in which sanctions were actually imposed and not just threatened. The advantage of the HSE data in terms of the longer temporal domain is clear, as is the advantage of TIES in terms of the number of cases. It also becomes apparent, however, that the difference in the number of cases between HSE and TIES steadily increases over each of the decades for which we have TIES data. The TIES data show that the increase in the use of sanctions as an instrument of policy from 1971–2000 is stark—the number of cases more than doubles every decade. This trend is not at all apparent from the HSE data. This has significant implications for any conclusions we reach regarding changes in sanctions behavior over time, which substantially lessens the advantage of a longer temporal domain.

Obviously, some of the additional cases contained in TIES can be attributed to the fact that we explicitly sought to identify cases in which sanctions were only threatened. Threat-only cases account for 361 of TIES cases, which leaves 527 in which sanctions were actually imposed. Many of the other additional cases are included because we include many more cases of trade sanctions than do HSE. As can be seen from Table 1, however, even if we also remove those cases, we are

¹⁰Although we took great pains to insure that the dataset is as complete and accurate as possible, there is probably no question that errors of omission and commission remain. Anyone who believes they have discovered an error in coding or a missing case should feel free, and even encouraged, to inform us. We will investigate all suggestions and incorporate changes into a future version of the dataset.

¹¹For the purpose of comparisons, we use the updated list of HSE cases available at <http://www.petersoninstitute.org/research/topics/sanctions/sanctions-timeline.cfm>.

Table 1. HSE and TIES Cases Distributed by Decade

| Decade | HSE | TIES | | |
|-----------------|-----|------|----------------|--------------------|
| | | All | Sanctions only | Non-trade disputes |
| 1914–1920 | 3 | – | – | – |
| 1921–1930 | 2 | – | – | – |
| 1931–1940 | 8 | – | – | – |
| 1941–1950 | 15 | – | – | – |
| 1951–1960 | 16 | – | – | – |
| 1961–1970 | 20 | – | – | – |
| 1971–1980 | 45 | 114 | 66 | 99 |
| 1981–1990 | 38 | 249 | 118 | 92 |
| 1991–2000 | 56 | 525 | 343 | 139 |
| 2001–2006 | 1 | – | – | – |
| Total 1971–2000 | 139 | 888 | 527 | 330 |
| Grand total | 204 | 888 | 527 | 330 |

still left with 330, which is over 50% more than HSE, and 237% more than HSE for the 1971–2000 period. We believe the biggest difference in the number of cases is simply the result of TIES being more thorough. Rather than focusing only on the well-known, high profile cases, we made a concerted effort to find the less obvious, less severe cases. The fact that HSE contains only a single case for the 2000–2006 period suggests quite strongly that they have excluded many cases, particularly those that are not “high profile.” On the other hand, TIES cases were selected after the systematic search procedures described above. Although there is still the possibility that a sanctions event was not recorded in the archives we used to compile the data, we believe our approach results in a much more representative sample of these events.

We believe the greater inclusiveness of TIES is the result of the considerable measures we took to insure that as many cases as possible were identified. We took pains to hire assistants with a wide variety of language abilities in order to make use of non-English sources. We also spent a great deal of time seeking out cases that were not widely publicized or well known. An important result of this is that TIES cases are, on average, different in key respects from HSE cases. We offer two examples. First, only 46% of our cases involve the USA as the primary sender, whereas 60% of HSE cases do so. HSE can be criticized for being too focused on US cases, so even though the recent version of their data is an improvement in this regard, TIES has even less bias toward the USA.

Second, the mean duration for HSE cases with known ending dates is 6.6 years, whereas the mean duration for TIES cases with known ending dates is 2.7 years. Figure 1, which overlays the duration histograms of the two datasets, shows that HSE severely underestimates the number of relatively short cases that are resolved within a couple of years. While it might seem that TIES underestimates the number of longer cases, the better coverage of HSE on this score is explained by the fact that TIES includes only post-1971 cases.

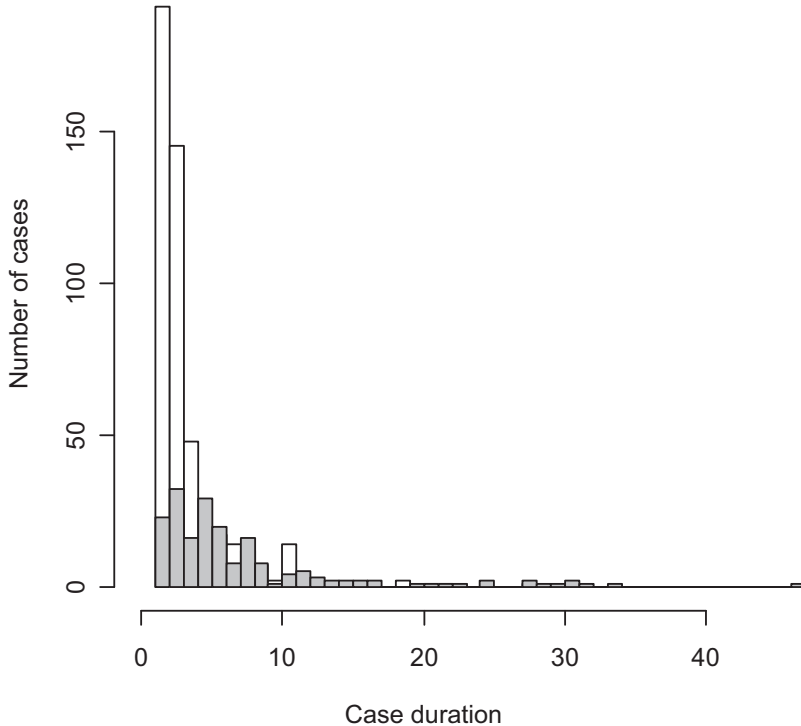


Figure 1. Histogram of HSE Case Durations (gray) Overlaid on Histogram of TIES Case Durations (white)

This discrepancy might have significant consequences for the conclusions we reach regarding the effectiveness or duration of sanctions. It is certainly possible that effective sanctions work quickly and without much publicity while those cases that receive a great deal of attention and last for years do so precisely because they are ineffective. A sanctions dataset that focused on only the well-known cases might be biased in favor of sanctions that do not work. Similarly, it might be the case that the most well-known cases are more likely to be those that are particularly costly, or involve certain high profile issues, or that occur in the context of especially hostile relationships. Under such circumstances, the data are likely to be biased.

One of the major contributions to have come from the work using HSE was to demonstrate that sanctions can, in fact, help the sender achieve its political objectives. Previous empirical work drew from studies of very high profile cases, which tended to be high profile because they were spectacular failures. A scholarly consensus had formed that sanctions never work. HSE were able to show that sanctions can sometimes work, and that we can identify some variables that contribute to their success. If their data are biased in favor of failures, the effectiveness of sanctions might be even greater than we now believe. If we, as do HSE, consider “success” to be a score of 9 or greater on their 16-point success

score, 34% of their cases are sanctions that “worked.” If we consider the two variables in TIES that depict the extent to which sender and target achieved their objectives and define “success” to occur when the sender achieves more of its objectives than does the target, sanctions “work” in 45% of TIES cases.¹² Alternatively, if we consider the categorical outcome variable from TIES, of the 60% of cases with known final outcomes, about 30% conclude with partial or total target concessions and another 25% end through some form of negotiated settlement. Only 33% of these cases end with sender capitulations and 12% end in stalemate.

Obviously, the TIES dataset differs from HSE in several important respects. It is not just that there are more cases. There does seem to be some systematic bias in the cases that HSE omit. At the very least, the additional cases included in TIES are more likely to be much shorter in duration and to end in success for the sender. The important question, however, involves whether or not we are led to the same conclusions regarding the hypotheses we test using the different datasets and whether TIES permits testing of hypotheses that cannot be tested using HSE. In the next section, we present some simple associations intended to show that analyses based on TIES will contribute to the advancement of new knowledge.

Simple Factors of Sanctions Success

We are using this section to present some simple bivariate associations that speak to a number of hypotheses found in the sanctions literature. We do not conduct extensive, detailed tests and the results presented here should be taken as illustrative and tentative. Our goal is to suggest that TIES can be helpful in testing hypotheses, not to provide definitive empirical answers to our questions. We have selected associations to present that have particular interest in the literature and that, as a set, demonstrate the value of TIES. We show that our data support some of the findings produced using HSE, but that some of our results suggest relationships exactly the opposite from what has been found using earlier data. We also try to demonstrate that TIES can be used to test important hypotheses that have not been tested before. It should be remembered that we are not experimenting with different operationalizations of key variables, we are not examining the robustness of our results, and we are not including control variables, conducting diagnostics, or considering alternative model specifications. We are not testing any specific theoretical hypotheses. We are presenting simple relationships that are meant to be suggestive.

A common argument in the literature suggests that the key determinant of sanctions success is the degree to which sanctions are costly to the target. Intuitively, higher costs should be more likely to lead a target to change its policies. Variations on this argument suggest that the costs to the sender should be inversely related to success. Previous results based on HSE data have provided support

¹²Recall that TIES also includes a categorical variable characterizing the outcome of each case. This variable could also be used as an indicator of sender success. Moreover, one could measure success using only the variable depicting the extent to which the sender achieved its objectives, with no comparison to the corresponding variable for the target.

Table 2. Economic Coercion Costs and Outcomes

| | Sender failure | Sender success |
|------------------------------------|--------------------------------------|----------------|
| A. Actual target costs | | |
| Minor | 97 (62.2%) | 59 (37.8%) |
| Major | 18 (52.9%) | 16 (47.1%) |
| Severe | 1 (6.2%) | 15 (93.8%) |
| | chi-square = 18.6394 p-value = 0.000 | |
| B. Actual sender costs | | |
| Minor | 108 (57.4%) | 80 (42.6%) |
| Major | 5 (35.7%) | 9 (64.3%) |
| Severe | 1 (100.0%) | 0 (0.0%) |
| | chi-square = 3.2841 p-value = 0.194 | |
| C. Anticipated target costs | | |
| Minor | 143 (62.4%) | 86 (37.6%) |
| Major | 41 (47.1%) | 46 (52.9%) |
| Severe | 5 (31.2%) | 11 (68.8%) |
| | chi-square = 10.5542 p-value = 0.005 | |
| D. Anticipated sender costs | | |
| Minor sender costs | 172 (55.6%) | 137 (44.4%) |
| Major sender costs | 12 (57.1%) | 9 (42.9%) |
| Severe sender costs | 2 (100.0%) | 0 (0.0%) |
| | chi-square = 1.5969 p-value = 0.450 | |

for these contentions. These hypotheses have generally found support from studies using the HSE data and, as can be seen from Table 2, the expected relationships hold when using TIES.

In Table 2, we again define sender success as occurring when the 0–10 scaled variable depicting the degree to which the sender achieved its objectives is greater than the 0–10 scaled variable depicting the degree to which the target achieved its objectives. In Table 2A, we see that, for those cases in which sanctions were imposed, as target costs increase the probability of success for the sender increases. When target costs are minor, senders succeed 38% of the time. The success rate climbs to 94% when the costs to the target are severe. In Table 2B, we see that sender costs do not have a statistically significant relationship with success; but, as one moves from minor sender costs to major sender costs, the probability of success actually increases. There are too few cases of major or severe sender costs to have confidence in any inferences drawn, however.

In Tables 2C and 2D, we show that TIES can provide some novel analysis. Here, we focus on those cases in which sanctions were threatened and our independent variables depict the costs that target and sender would *anticipate* were the threats to be carried out. Clearly, the greater the anticipated costs to the target, the greater the likelihood that the sender’s threat will be effective. In moving from minor anticipated target costs to severe anticipated target costs, the success rate increases from 38% to 69%. The anticipated costs to the sender do not have a statistically significant effect on the outcome, however. While the relationship is very slightly in the expected direction, there are too few cases in which the sender

Table 3. Success Rates of Unilateral and Multilateral Sanctions

| | Sender failure | Sender success |
|---------------------|----------------|----------------|
| Single sender | 173 (60.5%) | 75 (39.5%) |
| Second sender or IO | 113 (45.2%) | 91 (54.8%) |

Chi-square = 9.9406; *p*-value = 0.002.

expects to suffer major or severe costs for confidence about the results. These simple associations do suggest that costs, anticipated and actual, matter. They also provide evidence suggesting that senders are strategic in their threats. At the very least, senders try to threaten sanctions from which they will suffer relatively little. These results are certainly worthy of more in-depth investigation.

Another factor frequently asserted to have a strong effect on sanctions success is whether sanctions are imposed unilaterally by a single sender or multilaterally by a coalition of senders. This question has been at the center of one of the most puzzling paradoxes in research on economic sanctions. Intuitively, it seems obvious that multilateral sanctions would be more effective. The costs to the target should be greater, and the target should have fewer options for finding substitute trading partners. Some have argued that the key factor in sanctions success is whether the initial sender can build a coalition around sanctions (Martin, 1993). Curiously, analysis conducted on this question using the HSE data has consistently shown that unilateral sanctions are more likely to be successful than are multilateral sanctions (Hufbauer, Schott, and Elliott, 1990; Kaempfer and Lowenberg, 1998; Drury, 1998) and several theoretical arguments have been developed to show why this might be the case (see Miers and Morgan, 2002, for a review of these).

In Table 3, we show the simple relationship between unilateral vs. multilateral sanctions and sender success that one observes using the TIES data. Our measure of success is as above and our independent variable is simply whether sanctions were threatened or imposed by a single sender or by several senders. Interestingly, when using the TIES data, we find a strong relationship suggesting that multilateral sanctions are more likely to be successful than are unilateral sanctions. Multilateral sanctions were successful 55% of the time while unilateral sanctions were successful 40% of the time. Thus, the empirical association we find using TIES data is completely opposite to that found using HSE. Moreover, the finding using TIES is entirely in line with our simple intuition.¹³ This suggests that analyses using TIES may very well change at least some of what we think we know about sanctions. Certainly, further investigation is warranted.

We have thus far shown that TIES can be used to replicate previous empirical work using the HSE data and that in some cases, the results using TIES corroborate earlier findings. In other cases, they corroborate these findings but allow us to improve our existing understanding, and in still other cases, they contradict

¹³Elsewhere (Bapat and Morgan, 2007) we have explored this more fully. The relationship appears robust and cannot be attributed to the types of cases we include that HSE do not (i.e. threat-only cases or cases involving trade disputes).

Table 4. Behavior Specificity, Sender Commitment, and Economic Coercion Outcomes

| | Sender failure | Sender success |
|--------------------------------------|----------------|----------------|
| <i>A. Behavior specificity</i> | | |
| Ambiguous | 74 (82.2%) | 16 (17.8%) |
| Specific | 122 (46.9%) | 138 (53.1%) |
| chi-square = 33.8092 p-value = 0.000 | | |
| <i>B. Sender commitment</i> | | |
| Weak | 31 (81.5%) | 7 (18.5%) |
| Moderate | 92 (62.1%) | 56 (37.9%) |
| Strong | 74 (44.8%) | 91 (55.2%) |
| chi-square = 20.7092 p-value = 0.000 | | |

earlier results. We now want to demonstrate that TIES can be used to investigate relationships that have been ignored previously and that some of these might be particularly important to our understanding of sanctions—and might have particular policy relevance. Our examples of this focus on two characteristics of threats: the specificity of the demand made on the target and the level of commitment of the sender as indicated by the degree to which the threat was clear or ambiguous. We associate each of these variables with the likelihood of sender success, as measured above. Table 4 presents our results.

In Table 4a, we associate sender success with the specificity of the sender’s demands made upon the target. The independent variable is a dichotomy capturing whether the sender made very precise demands (“turn those responsible for placing a bomb on that airplane over to us”) or more ambiguous demands (“stop supporting international terrorism”). It appears that senders are much more likely to succeed when they make demands that are clear and precise. Senders succeeded in 53% of the cases in which their demands were specific and in only 18% of the cases in which their demands were not. In Table 4b, we associate sender success with the degree of commitment by the sender. Here, we measure commitment by the degree to which the sender’s threats were unambiguous. This is represented as a trichotomy. We associate ambiguous threats (“do as we say or we will re-evaluate our economic relationship”) with lower levels of sender commitment and very precise threats (“do as we say or we will increase tariffs on our imports of white wine from you to 100%”) with high levels of commitment. As sender commitment increases, the likelihood of sender success also increases, moving from 19% under weak commitment, to 38% under moderate commitment, and to 55% under strong commitment.

We have not yet investigated these relationships in detail and there are a number of possible explanations. It might be that behavior matters—you are more likely to get what you want when you are clear about what that is and when you are committed to getting it. It might also be that selection effects are at work—you are less specific in your demands and less committed when you believe your threat will fail. This is strong evidence, however, that threats matter and that we should devote time and effort to studying sanctions threats in more detail.

Conclusion

Our goal in this paper has been to introduce the new TIES dataset. We believe it will prove to be a valuable resource in the study of economic sanctions and that it will help advance our understanding of how sanctions can be used as an instrument of foreign policy. We have also tried to highlight the differences between TIES and the HSE data that have been predominantly used in empirical work on economic sanctions. We believe that HSE will continue to be a valuable tool in research and we recognize that it retains some distinct advantages over TIES, in particular because of its longer temporal domain. TIES will be more useful for many studies needed to move our understanding of sanctions forward, however. The fact that it contains more cases will make it more useful for testing theories that contain many variables and that specify complex interactions among independent variables. Most importantly, it will allow us to investigate the role of threats in sanctions episodes. Since many have argued that successful sanctions are likely to work quickly, perhaps without even being imposed, this is critical to our understanding. We have also demonstrated that analyses using TIES corroborate many of the findings drawn from HSE, but that we also find contradictions and we show that we can address empirical questions with TIES that must be ignored with HSE. Sorting out these contradictions and exploring these new avenues for research may advance our understanding a great deal. We invite those interested to use the data and to contribute to this next wave of research.

Appendix

In this Appendix, we provide a list of the key variables included in the TIES dataset. We also provide some information regarding the measurement of the variables and the category types for the main categorical variables. We do not provide complete definitions and coding rules; interested readers and users are encouraged to visit <http://www.unc.edu/bapat/TIES.htm> for the user guide, codebook, and dataset. The first set of variables categorizes the case and those involved in it. The next set (beginning with number 11 in this list) describes characteristics of the threats, if any, issued. The third set (beginning with number 22 in this list) describes the characteristics of sanctions imposed, if any. The final three variables characterize the outcome of the case. It should be kept in mind that the variable numbers here do not correspond with the variables in the dataset. For simplicity, this list is abridged. (For example, in the dataset, there are several variables associated with 'Issue' to capture cases in which sanctions were threatened or imposed over multiple issues.)

1. Case ID
2. Start Date
3. End Date
4. Senders—lists the COW country codes for up to five key senders
5. Primary Sender—lists the COW country code for the primary sender
6. Institution—indicates whether an international organization was involved as sender
7. Institution ID—identifies the organization involved, if any
8. Target State—the COW country code for the target

9. Target State Institution—indicates whether the target was a member of the sanctioning institution, if any
10. Issue—a set of variables identifying the issues over which sanctions were threatened and/or imposed. The following categories are available:
 1. contain political influence
 2. contain military behavior
 3. destabilize regime
 4. release citizens, property or material
 5. solve territorial dispute
 6. deny strategic materials
 7. retaliate for alliance or alignment choice
 8. improve human rights
 9. end weapons/materials proliferation
 10. terminate support for non-state actors
 11. deter or punish drug trafficking practices
 12. improve environmental policies
 13. trade practices
 14. implement economic reform
 15. other
11. Threat ID—identifies the person(s) in the sender government issuing the threat. Available categories are:
 1. bureaucracy
 2. individual legislator
 3. legislature
 4. executive staff member
 5. executive
 6. government
 7. head of international institution
 8. international institution
 - 99. unknown
12. Sender Commitment
 1. weak
 2. moderate
 3. strong
13. Offending Behavior Specificity
 1. ambiguous
 2. clear
14. Sanction Type Threatened—identifies the type of sanctions. Available categories are:
 1. unspecific
 2. total economic embargo
 3. partial economic embargo

4. import restriction
 5. export restriction
 6. blockade
 7. asset freeze
 8. termination of foreign aid
 9. travel ban
 10. suspension of economic agreement/protocol
 - 99. not applicable (no threat made)
15. Carrots—identifies if sender also offered carrots and, if so, the type. Categories available:
1. economic payments or aid
 2. trade concessions
 3. removal of previous sanctions
 4. military aid
 5. political concessions
 - 99. missing—no carrot identified
16. Diplomatic Sanctions—identifies if sender also threatened diplomatic sanctions and, if so, the type. Categories available:
1. expulsion of ambassador
 2. recall of ambassador
 3. temporary closing of embassies
 4. ending diplomatic contact
 - 99. missing—no diplomatic sanctions identified
17. Anticipated Target Economic Costs—trichotomous categorization of anticipated costs of threatened sanctions
18. Anticipated Target Economic Costs Figure—dollar amount of anticipated costs of threatened sanctions
19. Anticipated Sender Economic Costs—trichotomous categorization of anticipated costs of threatened sanctions
20. Anticipated Sender Economic Costs Figure—dollar amount of anticipated costs of threatened sanctions
21. Threatened Targeted Interest—identifies the interest within the target state affected by the threatened sanctions. Categories available:
1. general
 2. regime leadership
 3. business interest
 4. political interest
 5. military
 6. other
22. Sanction Identity—identifies the actors within the sender responsible for implementing sanctions. Categories are:
1. bureaucratic
 2. legislative

3. judicial
 4. executive
 5. government
 6. international institution
23. Sanction Type—specifies the types of sanctions actually imposed. Categories are:
1. unspecific
 2. total economic embargo
 3. partial economic embargo
 4. import restriction
 5. export restriction
 6. blockade
 7. asset freeze
 8. termination of foreign aid
 9. travel ban
 10. suspension of economic agreement/protocol
 11. other
 12. not applicable
24. Carrots—identifies carrots provided, if any, along with sanctions. Categories are:
1. economic payments or aid
 2. trade concessions
 3. removal of previous sanctions
 4. military aid
 5. political concessions
25. Carrot Value—an estimate of the economic value of carrots
26. Diplomatic Sanctions—identifies diplomatic sanctions that were also imposed. Categories are:
1. expulsion of ambassador
 2. recall of ambassador
 3. temporary closing of embassy
 4. ending diplomatic contact
27. Target Economic Costs—trichotomous indicator of the cost of sanctions to target
28. Monetary Cost to Target Figure—dollar amount of costs of sanctions to target
29. Sender Economic Costs—trichotomous indicator of the cost of sanctions to sender
30. Monetary Cost to Sender Figure—dollar amount of costs of sanctions to sender
31. Final Outcome—a categorical variable specifying the nature of the final outcome. Categories are:
1. partial acquiescence by target to threat
 2. complete acquiescence by target to threat
 3. capitulation by sender in threat stage
 4. stalemate in threat stage
 5. negotiated settlement in threat stage
 6. partial acquiescence by the target following sanctions
 7. complete acquiescence by the target following sanctions

8. capitulation by sender after imposition of sanctions
 9. stalemate after imposition of sanctions
 10. negotiated settlement after imposition of sanctions
 11. unknown
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32. Settlement Nature for Sender—a 0–10 scale representing the nature of the settlement for the sender, with 0 being the worst possible outcome and 10 the best
 33. Settlement Nature for Target—a 0–10 scale representing the nature of the settlement for the target, with 0 being the worst possible outcome and 10 the best

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