Institutions and International Cooperation

AN EVENT HISTORY ANALYSIS OF THE EFFECTS OF ECONOMIC SANCTIONS

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This paper addresses a new issue in sanctions research: the determinants of the time it takes for nations to return to presanctions levels of trade after a sanctions episode ends. The authors argue that democratic institutions reduce transaction costs and promote trust between economic agents. Their primary hypothesis is that jointly democratic dyads return to their presanctions level of trade faster than nonjointly democratic dyads. To evaluate this argument, the authors have constructed an event history data set of 59 sanctions cases beginning between 1954 and 1992. The empirical analysis finds strong support for the theoretical hypothesis that democratic political institutions facilitate a return to trade. These results hold even in the presence of competing explanations, and are robust to alternative model specifications.

When a nation decides to impose sanctions on another country, there is a clear political choice by one nation to alter the trading relationship between nations. When this decision is made, it is understood that sanctions will end some day and the trading relationship will be allowed to return to normal.[1] What is not clear at all, however, is how long it will take for actual levels of trade to return to normal after the restrictions on trade are lifted. This question is of great importance for political as well as economic reasons. Nations implement sanctions to achieve a political goal, but when the dispute is over the use of economic leverage by one nation against the other, there may be long-term economic repercussions. Therefore, whereas most research on sanctions focuses on the expected decreasing level of dyadic trade during sanctions in an attempt to determine what leads to successful sanctions (Baldwin 1985; Kaempfer and

1. Although we recognize that the term *normal trade* may have certain normative connotations or may have a variety of technical meanings, for purely stylistic reasons we occasionally substitute its use for the more cumbersome phrase "presanctions level of trade."

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Lowenberg 1992, 1999; Martin 1992; Dashti-Gibson, Davis, and Radcliff 1997; Drury 1992; Hufbauer, Schott, and Elliott 1990; Lindsay 1986; Newnham 2000; Nossal 1989; Tsebelis 1990), this study focuses on what happens after sanctions end.

The primary goal of this study is to develop a theory with regard to the importance of democratic institutions in promoting trade. We draw on the economic and social capital literatures to argue that democratic institutions reduce transaction costs and promote trust and stability between economic actors. In turn, these forces promote trade. The second goal of this study is to empirically evaluate the theory. To make this assessment, we use cases of economic sanctions to see if the presence of democratic institutions is systematically associated with shorter times until trade returns to normal after sanctions, even when controlling for economic and political factors associated with sanctions dyads. Whereas others have evaluated the effect of democratic institutions on trade, the approach taken here is novel in that it addresses the role of institutions in reestablishing relationships after a dispute has occurred.

One final contribution made as a result of the empirical analysis is to provide valuable information to policy makers with regard to the potential lasting effects of economic sanctions on existing patterns of trade. The only work done on this issue found "that sanctions can have a lingering impact under some circumstances but that this is not a general effect" (Hufbauer et al. 1997, 6). Our analysis improves on the existing research in two ways. First, a superior methodological approach that explicitly incorporates the effect of time on the probability of returning to previous levels of trade is used. Second, a theoretical structure is provided that can help explain why trade returns quickly in some instances and takes longer at other times.

The study proceeds as follows. In the following section, we develop the theoretical argument connecting institutions to trade and derive several testable hypotheses. The second section describes our sanctions data set, which consists of cases beginning in or after 1954 and ending prior to 1992. Next, we use event history analysis to analyze the return to trade in our sample. Because the events are in discrete time, we employ a logit specification and include a cubic spline to account for temporal dependence. Empirically, we find strong support for the institutional variables. The article concludes with a discussion of the results and their relationship to international relations.

THEORY

Theoretically, the issue under investigation is a commitment problem. Businesses do not want to resume trade if they believe political problems will interfere with their commercial activity and perhaps harm them financially. The goal of a firm is to make a profit, yet making a profit is difficult when the revenue source—trade—may quickly be shut off by the government or the trading partner's government imposing economic sanctions. Thus, the use of economic sanctions increases the risk involved with foreign trade for businesses in both the sanctioning and sanctioned nation. In turn, increased risk increases the cost of conducting business, which affects the ability of a firm to thrive, especially in a competitive environment. This argument finds support in the literature from Morrow, Siverson, and Tabares (1998). They propose that when conflict

is more likely, potential trade must be more profitable to compensate for potential disruptions imposed by government actors.

> In peacetime, the anticipation of conflict determines the risk premium that trade enterprises face. If agents believe that conflict is likely, then only the most profitable businesses engaged in trade can justify the added political risk. (Morrow, Siverson, and Tabares, 1998, 650)

Thus, from the perspective of a business that has had its foreign trade interrupted by government-imposed sanctions, it seems clear that the postsanctions trading environment is riskier, and it is questionable whether it should ever renew its trust with any businesses operating under that government's regulations.

An alternative way of understanding the role of trust in economic transactions is in expected utility terms. Expected utility theory argues that an actor chooses the option with the highest expected pay-off, which is simply a function of various costs and benefits. Sanctions alter the expected utility calculations of actors by increasing the costs of trading. Given that sanctions have previously been imposed, firms must believe that the amount of risk now involved in their transactions has increased. Sanctions, then, increase the transaction costs of commercial relationships. Trust depends not so much on your own interests, but more on the belief that the interests of the trustee "encapsulate your interests" (Hardin 1993). If you believe that it is in the trustee's interest to be trustworthy, then you will trust her. Thus, the issue of trust is intimately connected with the issue of resuming trade after sanctions.

The solution to this problem of whether to resume trade has to do with institutions. The economic historian Douglass North notes that "when it is costly to transact, institutions matter" (North 1990, 12). The costs involved in transactions, according to North, consist of "the costs of measuring the valuable attributes of what is being exchanged and the costs of protecting rights and policing and enforcing agreements" (North 1990, 27). Institutions reduce transaction costs by legitimizing contracts, protecting private property and other economic concerns. Institutions achieve these ends by signaling to economic agents that their contracts will be enforced and their property rights protected. In short, institutions facilitate trade by transferring trust in existing relationships to newly formed, or re-formed, relationships embedded in the same institutions (Uzzi 1997). In this way, the idea of embedded trust provides an important link between the literature on institutions and on trust. Following this idea, the trust (or lack of trust) that firms have for trading with other firms after sanctions is not really trust in the firm as much as it is trust in the government institutions of the country.

More specifically, the key to understanding the resumption of trade after sanctions depends on the type of regimes in the two countries. Leeds (1999, 997) notes that "domestic political institutions affect international behavior because they affect the costs and benefits that leaders expect from different foreign policy actions." We build on Leeds's theory by noting that domestic political institutions affect the decisions of firms as well. After sanctions, firms in a democratic state are more likely to resume trade with another democratic state because they feel that they can measure the costs and risk up front.

Following Leeds, we argue that accountability characterizes institutions, both political and economic, in a democratic state. In nondemocratic states, governments have more flexibility to nationalize industries or interfere in trade; hence, firms in a democracy are less likely to trade with them. Put differently, when comparing a sanctioned democracy and a sanctioned nondemocracy, trade is more likely to return faster for the democracy because democratic institutions minimize economic risk, even while controlling for a previous sanction. Similarly, Bueno de Mesquita et al. (1999, 6) argue that "institutional arrangements produce different levels of constraint in different political systems." Broadly speaking, democratic leaders are more constrained than nondemocratic leaders. The large, diffuse winning coalitions that must be satisfied to gain reelection are made up, in part, of groups with interests in international trade. This prevents democratic leaders from acting capriciously in matters relating to international trade and provides additional security for firms in the democratic country and any country trading with it.

Following Olson (1993), Bliss and Russett (1998) argue that

> Entrepreneurs are also likely to be more confident in the continuity of business practices and the rule of law in another democracy than in an autocracy, where such capricious acts as expropriation may threaten their interests. (P. 1129)

In short, firms understand the institutional differences between democracies and autocracies and, for this reason, are more reluctant to resume trade with a nondemocratic state.

Figure 1 graphically shows the concepts involved in the theory and how they are related. First, when democratic institutions are present (top left corner) there is an increased amount of accountability compared with when the institutions are nondemocratic, which is associated with increased flexibility (Leeds 1999). The effect of the increased accountability in democratic institutions is a decrease in uncertainty and risk, which leads to a decrease in transaction costs when democratic institutions are present. These factors all contribute to promote trade between countries with democratic institutions. The increased amount of flexibility found in nondemocratic institutions produces the opposite effect. The increased flexibility leads to increases in uncertainty and risk and increased transaction costs. This is expected to lead to lower levels of trade between countries with nondemocratic institutions (North 1990).

Although the factors mentioned above can lead to differing levels of trade between countries with and without democratic institutions, there is an additional element that is intimately connected to promoting trade, in general, and specifically to the return to normal trade after sanctions end. When individual firms are considering trading with businesses in the sanctioned/sanctioning country after sanctions end, category-based trust or trust in embedded relationships can play a critical role (Uzzi 1997; Hardin 1993).

By assigning a higher level of trust in the category of businesses in countries with democratic institutions, firms can shortcut the cost of gaining so much specific information about each potential foreign trading partner. This can play a role in helping to reduce transaction costs. Therefore, if the category "democratic institutions" is associated

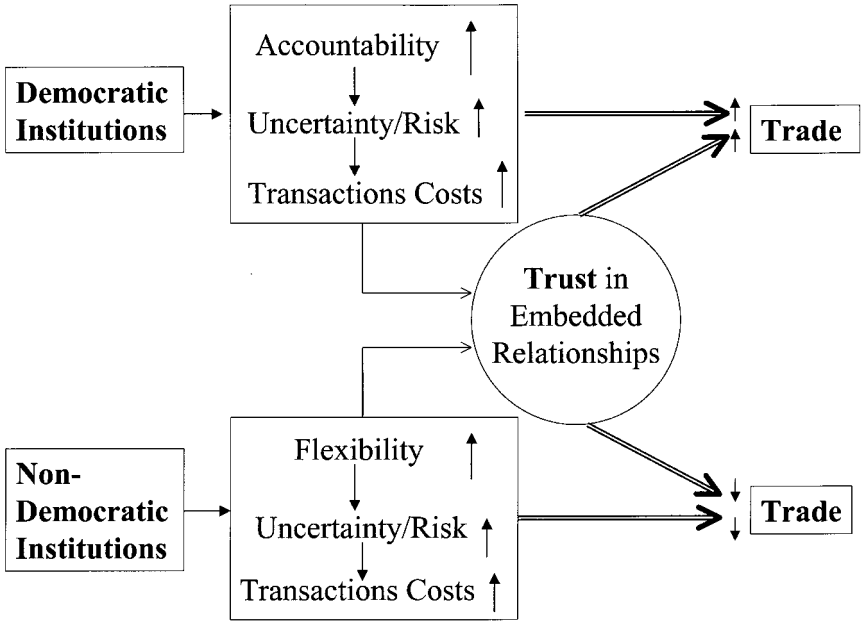

Figure 1: An Institutional Explanation of Trade

with accountability and "nondemocratic institutions" are generally associated with flexibility, this shortcut will lead to even greater trade with countries having democratic institutions. Hardin argues that you trust someone only when their interests are best served by being trustworthy. We feel that the accountability in democratic institutions makes them more trustworthy as a group. Thus, embedded trust also functions as a means to a quicker resumption of trade. Given the importance of institutions in facilitating trade, we derive three testable hypotheses.

Hypothesis 1: Dyads with a democratic initiator of sanctions will return to the presanctions level of trade sooner than dyads without a democratic sanctioner.
Hypothesis 2: Dyads with a democratic target of sanctions will return to the presanctions level of trade sooner than dyads without a democratic sanctionee.
Hypothesis 3: Jointly democratic dyads will return to the presanctions level of trade sooner than nondemocratic dyads.

In summary, the benefits of trade are influenced primarily by democratic institutions. Institutions facilitate trade because they establish a commitment to cooperation, which in turn reduces the costs of transactions and changes factors of behavior (Weingast 1998). As a result, if sanctions occur and trust is damaged, democratic institutions limit the fallout and facilitate healing.

RESEARCH DESIGN

Because our research question and hypotheses focus on the time until an event occurs, the most appropriate way to investigate it is with an event history model. Event history models are especially useful for investigating a change in states (Box-Steffensmeier and Jones 1997). For example, this research analyzes the factors that lead to a change from depressed levels of trade to normal levels of trade in a dyad. In event history terms, when this change occurs there is a failure. The statistical results allow us to specify the factors and values of the covariates that lead to event failure, that is, to a return to normal levels of trade.

Event history analysis revolves around two concepts: the risk set and the hazard rate (Allison 1984). The risk set comprises the number of dyads subject to event occurrence at a particular point in time. Among other reasons, the risk set is useful for understanding the unit of analysis. The unit of analysis is not the dyad or the sanctions case. Rather, it is the dyad year. Put differently, the statistical analysis is based on the number of years for which the dyads are at risk of failing. This value, the number of observations, is determined ex post.

The second important concept in event history analysis is the hazard rate. "The hazard rate is the probability that an event will occur at a particular time to a particular individual, given that the individual is at risk at that time" (Allison 1984, 16). This hazard rate is essentially the likelihood that trade will return to normal in the next interval given that it has not already returned to normal. If the risk of returning to normal trade is time invariant or does not depend on how long the two countries had previously gone without trade, the hazard would be duration independent. If the likelihood of returning to normal trade is dependent on the number of years that have passed without trade, we would say that the hazard for returning to normal trade is duration dependent. It is essential that any estimation of the probability of returning to normal trade over time be able to account for the possibility of duration dependence. In addition to accounting for the existence of duration dependence, event history models take into account censoring. Instead of having to discard a case because the event change has not occurred, event history analysis treats the case as censored yet still calculates the effects of the covariates on the time until censoring.

Because there are strong reasons to expect that the probability of trade returning to normal in any given year may be dependent on how long it has been since sanctions ended, we take steps to account for potential temporal dependence. The incorrect assumption of temporal independence, as Beck, Katz, and Tucker (1998) point out, leads to both inaccurate statistical tests and the loss of valuable information in the data. Temporal dependence can be accounted for in an event history logit specification by adding a series of dummy variables for each year, or more efficiently with a spline that includes temporal dependence in the model specification.

Equation 1 shows how time is included in the Cox proportional hazards model without time dependent covariates.

$$h_i(t) = \lambda_0(t) \exp\{\beta_1 x_{i1} + \ldots \beta_k x_{ik}\}. \quad (1)$$

In this model, the hazard for individual *I* at time *t* is the product of the baseline hazard $\lambda_0(t)$, which incorporates the length of time the unit has been at risk, and the *k* covariates, which are exponentiated. $\lambda_0(t)$ is regarded as the baseline hazard for any individual with all the covariates set to 0 (Allison 1995, 113). Logging equation 1 produces

$$\log h_i(t) = \alpha(t) + \beta_1 x_{i1} + \ldots \beta_k x_{ik}, \tag{2}$$

where $\alpha(t) = \log \lambda_0(t)$. $\alpha(t)$ can further be specified to estimate an exponential model, $\alpha(t) = \alpha t$ for the Gompertz model, and $\alpha(t) = \alpha \log t$ for the Weibull model (Allison 1995, 113). Specification of the $\alpha_t s$ in this manner allows survival analysts to test hypotheses with regard to the dependence of the hazard on time, but also forces a choice to be made with regard to the distribution. If strong priors exist with regard to the distribution of the hazard, it may be desirable to specify the hazard and test these assumptions. The downside of specifying a distribution is that it places restrictions on the form that the hazard can take. Without prior expectations with regard to the shape of the hazard, it may be more desirable to directly estimate the effect of time on the hazard, thus allowing the hazard to take on whatever form the data give it.

There are generally two ways to avoid having to specify the distribution associated with duration dependence. One is to use a Cox proportional hazards model that uses partial likelihood to estimate the β coefficients without having to specify the baseline hazard function $\lambda_0(t)$, and thus discarding any information about the $\alpha_t s$ from equation 2 (Allison 1995, 212). Second, the same model can be estimated with maximum likelihood techniques using a logit or cloglog link with the inclusion of variables in the model to account for time to get explicit estimates of the $\alpha_t s$. Because we are interested in observing the duration dependence in our model but do not wish to make any distributional assumptions, we choose the latter method.

The basic idea behind using a logit specification to estimate our model is simple. Each dyad's survival history is broken into one set of observations per discrete time unit, which is the dyad year. Then "after pooling the observations, the next step is to estimate a binary regression model predicting whether an event did or did not occur in each time unit" (Allison 1995, 212). An additional advantage to this specification is that including time-varying covariates becomes a simple matter of varying observations from one time unit to the next. All of the main theoretical variables regarding democracy were entered into the model in this way, as time-varying covariates.

In the standard logit model, only the $\exp\{\beta_1 x_{i1} + \ldots \beta_k x_{ik}\}$ from equation 1 is estimated. Following the notation of Beck, Katz, and Tucker (1998), the $e^{x_{i,s}\beta}$ of equation 3 below is equivalent to $\exp\{\beta_1 x_{i1} + \ldots \beta_k x_{ik}\}$ of equation 1.

$$h\left(s|x_{i,s}\right) = h_0\left(s\right)e^{x_{i,s}\beta}. \tag{3}$$

In the logit model, the discrete hazard of returning to normal trade for any dyad in any period is the probability that that dyad will return to normal trade $P(y_{i,t} = 1)$. However, to correctly estimate the hazard allowing for time dependence, Beck, Katz and Tucker (1998) show that the logit specification must change from the simplified logit model

$$P\left(y_{i,t}=1\middle|x_{i,t}\right)=\frac{1}{1+e^{-x_{i,t}\beta}} \tag{4}$$

to one that includes a dummy variable that counts the number of 0s, or nonevents, preceding the current observation.

$$P\left(y_{i,t}=1\middle|x_{i,t}\right)=h\left(t\middle|x_{i,t}\right)=\frac{1}{1+e^{-\left(x_{i,t}\beta+k_t\right)}}. \tag{5}$$

If the k_t dummies are not included in the logit specification, it would be the equivalent of assuming a constant baseline hazard. This would force the model to reflect a situation of duration independence that, if incorrect, would produce inaccurate statistical tests through incorrect standard errors, the loss of valuable information in the data leading to inefficiency, and potentially incorrect parameter estimates in some cases (Beck, Katz, and Tucker 1998, 1269).

The k_t can also be interpreted as baseline hazards for each period when all of the independent variables are held at 0. Equation 6 below shows the basic logit with dummy variables for years included and all covariates set to 0. Equation 6 can also be used to produce a graph of the baseline hazard.

$$P\left(y_{i,t}=1\middle|x_{i,t}=0\right)=\frac{1}{1+e^{-k_t}}. \tag{6}$$

Although using the series of dummy variables κ_t accounts for the duration dependence in the model, a more efficient way of doing this is to include splines that usually require smaller degrees of freedom and are interpretable as a smoother baseline hazard. In our model, we included a variable "postsanction years" measuring the time since sanctions ended and chose three knots, placed at times 2, 3, and 13. Thus, we included three splines in our final model. Two of the three spline variables in our final model were significant at the .05 level, indicating that the probability of trade returning to normal does exhibit duration dependence.

CASE SELECTION AND OPERATIONALIZATIONS

Like most research on sanctions, our primary source of data comes from Hufbauer, Schott, and Elliott (1990). This data source has received some criticism (Pape 1997) relating mostly to its coding of outcomes. As a source of cases, it is much better, although it is somewhat biased toward the United States. Due to data limitations, we limit our temporal domain to all sanctions cases beginning after 1954 and ending prior to 1992. Next, we eliminated cases in which the United Nations were involved. Because this study focuses on dyadic relationships, the inclusion of United Nations cases would overwhelm the data set with multiple cases derived from a single episode and call into question the generalizability of the findings. In addition, for any given sanctions episode, many members of the United Nations are likely to have minimal trade with the

sanctioned country, therefore, they are only formally sanctioning another country. We include cases in which there are a definite number of multiple senders or targets. For example, in 1956 the United States, United Kingdom, and France all sanctioned Egypt over their moves to nationalize the Suez Canal. Because it is clear who the actors are and they are arguably independent agents, we code this as three separate dyadic cases. Missing trade data caused us to discard a few other cases. Our final data set consists of 58 sanctions episodes. The appendix describes the cases.

DEPENDENT VARIABLE

In event history analysis, the dependent variable is event occurrence, which for this analysis is when trade returns to the presanctions level of trade. This variable was coded using dyadic trade data from Barbieri (1998).[1] In constructing the variable, we first recorded the amount of trade in the year prior to the imposition of sanctions. We then observed whether trade had returned to the presanctions level in each year after sanctions ended. If it met or exceeded the presanctions trade, the case was coded as having failed. If it did not meet presanctions trade, it was coded as 0. Cases in which trade did not decline despite the imposition of sanctions or in which trade declined but surpassed the presanction amount of trade before the end of sanctions were coded as returning in the first year. This variable ranges from 1 to 21 years, with a mean of about 4 years.

INDEPENDENT VARIABLES

The theoretical argument discussed earlier says that democratic institutions are central to explaining the variation in the return to presanctions levels of trade. To test this argument, we employ three variables that capture the influence of democratic institutions. The extent of democratic institutions in both the sender and target nations is operationalized with the democracy score from the Polity III index (Gurr, Jaggers, and Moore 1989); this variable ranges from 0 to 10.[2] To measure joint democracy, we first convert the sender and target democracy scores to dichotomous variables. Each of these takes on a value of 1 if a nation registers 6 or higher on the Polity Democracy scale. Then, we multiply the two dummy variables to attain the interaction term—joint democracy. This variable takes on a value of 1 only if both nations are democratic; it takes on a 0 value for mixed regime dyads and dyads in which both nations are not democracies.

1. We thank Katherine Barbieri for making her trade data available.

2. Because our theory is about the influence of democratic institutions on trade, we focus on the democratic component of the Polity III index. The strong negative relationship between democracy and autocracy scores in the Polity data allows the democracy variable alone to measure democracy and, indirectly, autocracy. It would unnecessarily complicate matters to use an index of both the democracy and autocracy scores because this makes the determination of a democratic state more ambiguous. By using only the democracy scale, we can apply the conventional method of calling a state a democracy if it registers a democracy score of 6 or greater.

CONTROL VARIABLES

Other factors besides political institutions must be controlled for that could influence the amount of trade between countries. For this reason, we incorporate a number of control variables into our empirical analysis. First, to control for economic factors not associated with sanctions, we include an array of variables associated with a "gravity" model of trade. The gravity model of dyadic international trade says that trade is a function of the size of each nation's economy and the distance between the two states, where the first two concepts are positively related to trade and the third is negatively related to trade (Bergstrand 1985, 1989).

Gravity models typically employ gross domestic product (GDP) to measure the size of a nation's economy. Unfortunately, GDP data are unavailable for many poor or communist countries, especially during the cold war. The energy index from the Correlates of War Composite Index of National Capabilities, however, is a useful proxy for the size of a nation's economy; nations that consume more energy have a larger GDP. Because economic size varies widely among and between countries, we take the natural log of these variables.[3] Our distance data come from EUGENE (Bennett and Stam 1999), which for noncontiguous states measures the distance between capital cities; for contiguous states, this variable assumes a value of 0.

We also include several control variables that relate specifically to the sanctions episode. In particular, it is likely that the intensity of the sanctions is inversely related to the resumption of trade between the countries. If sanctions are more intense, there is not only more ground to make up to return to normal levels of trade, but also the trust between the countries will have been more extensively damaged. Plus, sanctions that are more intense are more likely to be multilateral sanctions, which is likely to produce more acrimony between the sender and target (Martin 1992). Intensity is measured as the difference between dyadic trade at 1 year prior to sanctions and the lowest level of trade during the sanctions episode. This measure allows us to directly assess the impact of sanctions on dyadic trade.[4]

Next, it is important to control for the length of the sanctions episode. According to trade diversion theory, the longer sanctions are in place the more likely a nation is to find alternative suppliers. We use Hufbauer, Schott, and Elliott's (1990) data to construct the measure of sanctions length.

Another control variable concerns the policy success of the sanctions. If the target changes its policy, then this may be a sign that the target values trade with the sender, so one should expect a quick return to trade. We use the Hufbauer, Schott, and Elliott

3. Note that we are not employing a gravity model. Rather, we include concepts suggested by the gravity model because of their expected influence on trade. For this reason we do not log the dependent variable and the other independent variables.

4. In focusing on the decline in trade, this measure assesses the "depth" of the sanctions. We considered an alternative measure to capture the breadth of the sanctions by coding the type of sanctions imposed: import, export, or financial. If all three segments of the economy were targeted, then the sanctions are "total." Not surprisingly, the correlation between "total" and "intensity" is very high, so both cannot be in the model at the same time. Individually, however, each produces similar results.

TABLE 1
Event History Logistic Estimates

Variable	β	(SE)
Sender democracy	.799	(.255)***
Target democracy	.416	(.165)***
Joint democracy	–3.011	(1.359)**
Size of sender's economy	–1.313	(.771)*
Size of target's economy	.124	(.219)
Log of distance	–1.018	(.780)
Intensity	–8.193	(2.056)***
Length of sanctions	–.074	(.088)
MID during return to trade phase	–1.334	(1.595)
Target regime change	–.800	(.919)
Policy success	–.292	(.376)
United States dummy	1.848	(2.890)
Postsanction years	1.187	(1.140)
Spline 1	1.092	(.684)
Spline 2	–.625	(.374)*
Spline 3	.023	(.011)**
Constant	19.365	(13.542)
Log likelihood	–35.204	
N	139	
Percentage correctly predicted	92.09	
Reduction of error (ROE)	73%	

NOTE: ROE = 100 • (% correct – % modal category)/(100 – % modal category). Reported significance levels are two-tailed.
*$p < .10$. **$p < .05$. ***$p < .01$.

(1990) result variable to measure this concept. This variable takes on values between 1 and 4, with 4 indicating complete policy success.[5]

Regime changes in the sanctioned nation may also encourage a quick return to normalized relations. If the target changes its regime to become more democratic during the sanctions episode, then it is at least likely that the political differences are now minimized because most initiators of sanctions are democracies. To measure this variable, we look at a nation's democracy score on the Polity III index (Gurr, Jaggers, and Moore 1989) in the year sanctions were implemented and again at the end of the sanctions episode. If the democracy score transitions from a value less than 6 to a value greater than 6, it is coded as a regime change.[6] Nonregime changes are coded 0.

5. The Hufbauer, Schott, and Elliott (1990) coding of policy success is somewhat controversial. Pape (1997), in particular, takes issue with their codings. For this reason we also run the analysis using Pape's recoding of this variable. We report the results in the discussion section at the end of the article.

6. There are no cases in which a sanctioning democratic country transitioned to a democracy score below 6 on the Polity III index. It is interesting to note that the general pattern is for a sanctioned nation to become more democratic and less autocratic during a sanctions episode. This is an interesting phenomenon that bears on the success of sanctions and is an important avenue for future research.

Finally, we include two additional control variables. Because the Hufbauer, Schott, and Elliott (1990) data, and correspondingly, our data are heavy on the United States, we include a dummy variable for the United States. This variable takes a value of 1 when the United States is involved in the sanctions. In addition, we control for continued hostility after the sanctions end by including a variable that measures whether the dyad experienced a militarized interstate dispute (MID) in the return to presanctions trade period.[7] If there is a MID in the return phase, then it is likely that the trading environment is characterized by high risk so it should take longer for trade to return to normal. This variable assumes a value of 1 when a MID occurred, 0 otherwise.

RESULTS AND DISCUSSION

Table 1 presents the empirical results. Overall, the model performs very well, predicting about 92% of all cases correctly, which is a 73% reduction in error from the null model that predicts about 70% correctly. Several individual variables also perform well, yielding important substantive insights. Of the variables designed to control for basic market forces, that is, the gravity type control variables, the size of the sender's economy is negative and marginally significant at the .10 level, whereas the size of the target's economy and the distance between nations are not strongly related to the time it takes for trade to return to normal after sanctions.[8]

Next, of the four control variables related to the sanctions episode, the most important, from a substantive and statistical significance perspective, is intensity. As expected, this variable is significant and negative, indicating that the more intense the sanctions, the longer it takes for trade to return to the presanctions level. The other three control variables related to the sanctions episode—length, policy success, and target regime change—are all insignificant.[9] It is worth noting that the insignificance of the length variable and the marginal significance of the economic variables undermine trade diversion arguments, which suggests that longer sanctions and sanctions by states more equipped to find alternative suppliers lead to longer return times. We analyze this argument in greater depth in the next section of the article.

The minimum democracy score change for our cases of regime change is 7. In other words, this variable is not sensitive to the cutoff point chosen, that is, there are no cases of a nation moving from a 5 to a 6 and being coded as a regime change in our data. Each of the cases we code as a regime change involves substantial movement on the Polity III democracy scale.

In some cases, there is no Polity score in the year sanctions were imposed or the year sanctions ended. For these cases, we measure regime changes by using the closest year for which a score is available. We also run the analysis dropping these cases.

7. Militarized interstate disputes data were obtained from Bremer (1996).

8. We also ran a model using gross domestic product (data from the Penn World Tables [Summers and Heston 1991]) to ensure that our proxy for economic size was not influencing the results. The statistical results do not change in any substantive way, although we lose 15 cases.

9. To test for robustness, we recoded the policy success variable based on Pape's (1997) codings. None of the theoretical or control variables changes in sign or significance. Similarly, we recoded the target regime change variable to reflect missing data. Although we lose 23 cases with this coding, none of the theoretical variables changes in sign or significance.

The variables related to duration dependence also yield important insights. As Table 1 indicates, two of the three spline variables are statistically significant, whereas the other spline is significant at the .11 level. The significance of these variables indicates that trade in 1 year is related to trade in the previous year, therefore, one must control for duration dependence to make correct inferences. Substantively, the opposite signs on the spline variables are also important. The first spline variable is positive, indicating that the likelihood of failure is high in the first 2 years. The second spline variable, however, is negative, which suggests that during the period covered by that spline the effect of time on the probability of failure is negative. In substantive terms this means that if trade is still depressed after the first 3 years, it is likely to remain depressed in the subsequent year. Finally, the positive sign on the third spline indicates that after 13 years, dyads are increasingly likely to return to trade.

Importantly, the empirical analysis confirms all three of the theoretical hypotheses. The first hypothesis states that dyads with a democratic initiator of sanctions will return to the presanctions level of trade sooner than dyads without a democratic sanctioner. To confirm this hypothesis, the variable "sender democracy" must be significant and positive. It is. In substantive terms, this means that if the sanctioning nation is a democracy, dyadic trade is likely to resume to normal in a shorter period of time than if the sender was not democratic. The empirical model presents similar support for the second hypothesis that dyads with a democratic target of sanctions will return to the presanctions level of trade sooner than dyads without a democratic sanctionee. In short, democratic institutions in either the sender or target nation facilitate the resumption of trade.

The third hypothesis that jointly democratic dyads will return to the presanctions level of trade sooner than nondemocratic dyads also receives strong empirical support. Support for this hypothesis comes from the variable joint democracy. Based on a cursory observation, one may interpret the negative coefficient on joint democracy as increasing the amount of time it takes for trade to return to the presanctions level. Joint democracy, however, is an interaction variable so its interpretation is not straightforward. Joint democracy alone only tells us the differential impact of having a second member of a dyad be a democracy; it does not tell us the overall effect of joint democracy. The interpretation of this variable requires the summation of three coefficients: sender democracy, target democracy, and joint democracy (Hardy 1993).[10] Recall that joint democracy is the product of two dummy variables. To construct it, we converted both sender and target democracy into dummy variables, with a score of 6 or higher counting as a democracy. Therefore, to interpret joint democracy, one has to multiply the coefficients of the continuous measures of the sender and target democracy scores by 6 and then add this value to the coefficient on joint democracy.[11] After performing

10. We thank Bill Reed for pointing out the special interpretation of this variable.

11. We multiply the sender and target democracy coefficients by 6 because this is the minimum value needed to qualify as a democracy. Multiplying them by higher numbers will only improve our results. 6 • 0.7989912 (sender democracy) = 4.7939; 6 • 0.415805 (target democracy) = 2.4948; therefore, sender democracy plus target democracy equals 7.2887. Adding the coefficient on joint democracy (–3.011) produces the overall impact of joint democracy: 4.277. The *t* test for the interaction takes the following form: $t = 6 \bullet B_i + 6 \bullet B_j + B_{jk}/\text{sqrt}\ (V_i + V_j + V_{jk} + 2 \bullet COV_{i,\,jk} + 2 \bullet COV_{j,\,jk} + 2 \bullet COV_{i,\,j})$.

TABLE 2
Marginal Effect of Institutional Variables
on the Odds of Returning to Normal Trade

Variable	*Change in the Odds of Event Occurrence*
Sender democracy	+2.22
Target democracy	+1.52
Joint democracy	+72.02

these calculations, it is clear that joint democracy has a positive sign, which indicates that having both members of a dyad as democracies increases the likelihood of event failure. Substantively, this means two democracies will return to trade sooner than either mixed regime dyads or dyads composed of two nondemocracies. The overall inference to draw from these three hypotheses is that democratic institutions increase the return to trade, even in the presence of countervailing pressures from other relevant economic and sanctions-specific factors.[12]

To facilitate the interpretation of the logit coefficients, it is beneficial to discuss their substantive impact on the dependent variable. One way of doing this is to calculate marginal effects of the variables. For a logistic model, the marginal effects produce a change in the odds of an event occurring. This change, or odds ratio, is simply a ratio of the odds at two different values of X (Hamilton 1992, 230):

$$\hat{\Omega} = \frac{\hat{\varphi}_1}{\hat{\varphi}_0}.$$

If the X variable of interest is unrelated to the Y variable, then the coefficient on X will be 0, and the odds ratio, which can be calculated by taking the antilogarithm (e to the power) of the logit coefficient, $\hat{\Omega} = e^h$, will be 1. The stronger the relationship between the two variables, the farther the odds ratio will be from 1.

For a dummy variable, this ratio is the odds of the event with and without the dummy variable included in the full model with all other variables held constant at their mean. If X is a measurement variable, e^b shows the effect of a 1 increment change in the variable. This is important to keep in mind when substantively interpreting odds ratios because the increment of the variable will have an effect on the size of the odds ratio (Hamilton 1992, 231).

When the sender is a democracy, the odds of returning to normal trade are about two and one-quarter times greater than when the sender is not a democracy (see Table 2). Target democracy also has a large effect, although not as large as sender democracy. If the target is a democracy, then the odds of returning to normal trade increase by about one and one-half times. Finally, a jointly democratic dyad is approximately 70 times

12. Because the United States is involved in most of the cases, we ran a model including a dummy variable for the United States. The results were nearly identical (no variables changed sign or significance) to those reported in Table 1.

TABLE 3
Mean Return Time to Presanctions Level of Trade by Length of Sanctions and Dyad Type

Length of Sanctions	*Dyad Type*	*Mean Return Time*	n
Four years and less	Joint democracy	2	17
	Mixed	2.86	15
	All	2.41	32
Five years and greater	Joint democracy	0.43	7
	Mixed	4.36	11
	All	2.83	18
Mean length = 5.17	Joint democracy	1.54	24
Mean length = 4.50	Mixed dyads	3.50	26

more likely to experience event failure than a nonjointly democratic dyad, all else being equal. In other words, jointly democratic dyads return to trade much faster than other types of dyads.

DISCUSSION

Because we have a relatively small number of sanctions cases, we conducted a number of robustness tests to examine the sensitivity of our statistical results. First, the Hufbauer, Schott, and Elliott (1990) data are United States heavy, and correspondingly so is our data set. To see if the United States was driving the results, we included a dummy variable that takes a value of 1 when the United States was a participant and a value of 0 otherwise. As Table 1 indicates, this variable is not significant, and even while controlling for this potentially confounding influence, the democracy variables are significant.

Next, to see if the results on the democracy variables are overly sensitive to a few cases, we omitted the three Suez cases from our data set and reran the model. Substantively the results do not change, although the target democracy variable is only significant at the .05 level.

Finally, we conducted further analysis of the trade diversion hypothesis. This hypothesis asserts that longer sanctions force nations to find alternative suppliers, therefore, after sanctions end there is little incentive to resume trade with the sanctioning country, and the return to trade takes longer. If this argument is correct, then the mean time to return to normal trade should not vary across different types of dyads. However, if the democratic institutions theory is correct, then democracies should return to normal trade sooner even while controlling for the length of sanctions that last the same amount of time. Because the mean length of sanctions is about 4 years, we divide our cases into those that were shorter than the mean and those that were longer than the mean length (see Table 3).[13] For sanctions that last less than 4 years, two democracies

13. These results are robust in the presence of different cutoff points. For example, if we divide the data into cases that last 1 year, the modal length, and cases that last more than 1 year, the results are the same.

return to trade almost 1 year sooner. The difference in return time is especially large for sanctions that last 5 years or longer. In this set of cases, jointly democratic dyads return to trade almost immediately after the sanctions end. It appears that the reason for this result is connected to our theory of trust and democratic institutions. Of the seven democracy versus democracy cases that have sanctions lasting 5 years or longer, five of the cases involve a regime change—the target became a democracy—during the sanctions episode, thus, supporting the idea that if the policy differences have been clearly resolved, then the risk of additional sanctions is less risky, so trade should return quickly. Finally, the bottom two rows of Table 3 compare the mean length of sanctions and the mean return time for all cases in each type of dyad. The interesting insight from this comparison is that jointly democratic sanctions tend to be slightly longer than sanctions in mixed dyads, although the former return to trade much faster. In short, we find little support for a trade diversion hypothesis but strong support for the influence of trust and democratic institutions on the return to trade after sanctions end.

CONCLUSION AND FUTURE RESEARCH

This research project had two aims. The first was to develop a theory regarding the influence of democratic institutions on promoting trade. The arguments advanced in this study parallel other research that shows that democracies tend to trade more with other democracies than with nondemocracies. We advance this argument by evaluating it under a new set of circumstances. Complementing earlier research that finds that democratic institutions promote trade, we find that democratic institutions promote a return to trade after sanctions have ended.

In addition, we more firmly anchor the argument on economic principles. Economic agents desire to trade with each other and they will do so when the benefits outweigh the costs. When trade does not occur, it is typically because some factor has increased the risk of conducting trade, thus making the cost prohibitively high. This risk is especially high in dyads that have experienced sanctions because firms will have less confidence that their transactions will be permitted to continue. In essence, trust is a necessary precondition for economic activity, and democratic institutions are central to creating trust (Jackman and Miller 1998). Following North (1990), we argue that institutions reduce transaction costs and uncertainty, which allows the benefits of commercial activity to outweigh the costs, thereby encouraging economic activity. In particular, democratic institutions reduce transaction costs and build trust through their accountability to the public. The second goal of this research was to empirically examine this

argument. The statistical results support the theoretical argument, namely trade returns to the presanctions level faster in dyads with democratic institutions.

APPENDIX
Sanctions Cases in Data Set

Case Number	*Sender*	*Target*	*Years of Sanctions*
5420	750 India	235 Portugal	1954-1961
5430	230 Spain	200 United Kingdom	1954-1984
5610	2 United States	666 Israel	1956-1960
5621	2 United States	651 Egypt	1956-1957
5622	200 United Kingdom	651 Egypt	1956-1957
5623	220 France	651 Egypt	1956-1957
5631	2 United States	200 United Kingdom	1956-1957
5632	2 United States	220 France	1956-1957
5710	850 Indonesia	210 the Netherlands	1957-1962
5720	220 France	616 Tunisia	1957-1966
5810	365 USSR	375 Finland	1958-1959
6010	2 United States	42 Dominican Republic	1960-1962
6020	365 USSR	710 China	1960-1969
6110	2 United States	780 Ceylon/Sri Lanka	1961-1965
6210	2 United States	140 Brazil	1962-1964
6320	850 Indonesia	820 Malaysia	1963-1966
6330	2 United States	850 Indonesia	1963-1966
6820	2 United States	135 Peru	1968-1974
7010	2 United States	155 Chile	1970-1973
7111	2 United States	750 India	1971-1972
7112	2 United States	770 Pakistan	1971-1972
7211	200 United Kingdom	500 Uganda	1972-1979
7212	2 United States	500 Uganda	1972-1979
7320	2 United States	732 South Korea	1973-1977
7410	2 United States	640 Turkey	1974-1978
7420	20 Canada	750 India	1974-1976
7430	20 Canada	770 Pakistan	1974-1976
7512	20 Canada	732 South Korea	1975-1976
7540	2 United States	560 South Africa	1975-1982
7550	2 United States	811 Cambodia	1975-1980
7610	2 United States	165 Uruguay	1976-1981
7710	2 United States	150 Paraguay	1977-1981
7720	2 United States	90 Guatemala	1977-1986
7730	2 United States	160 Argentina	1977-1983
7740	20 Canada	740 Japan	1977-1978
7750	2 United States	93 Nicaragua	1977-1979
7760	2 United States	92 El Salvador	1977-1981

7770	2 United States	140 Brazil	1977-1984
7840	2 United States	750 India	1978-1982
7880	2 United States	620 Libya	1978-1990
7920	2 United States	770 Pakistan	1979-1990
7940	2 United States	145 Bolivia	1979-1982
Case Number	*Sender*	*Target*	*Years of Sanctions*
8120	2 United States	290 Poland	1981-1987
8210	200 United Kingdom	160 Argentina	1982-1983
8221	210 the Netherlands	115 Surinam	1982-1988
8222	2 United States	115 Surinam	1982-1988
8310	900 Australia	220 France	1983-1986
8330	2 United States	552 Zimbabwe	1983-1988
8410	2 United States	630 Iran	1979-1981, 1984-1990
8510	2 United States	560 South Africa	1985-1990
8610	2 United States	652 Syria	1986-1990
8710	2 United States	95 Panama	1987-1990
8730	2 United States	92 El Salvador	1987-1988
8812	2 United States	775 Burma/Myanmar	1988-1990
8821	2 United States	520 Somalia	1988-1990
8822	200 United Kingdom	520 Somalia	1988-1990
8910	750 India	790 Nepal	1989-1990
8930	2 United States	625 Sudan	1989-1990

SOURCE: Hufbauer, Schott, and Elliott, 1990.
NOTE: $N = 58$.

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