

# Vote Buying in the UN General Assembly

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## Abstract

We examine the strategic relationship between U.S. foreign aid disbursements and voting in the United Nations General Assembly (UNGA). Since 1985, U.S. law has stipulated that the State Department identify important votes and that USAID take the voting behavior of recipients in the UNGA into account in its disbursement decisions. We examine the implementation of this policy and the effects of linking aid to important votes in the UNGA on aid recipients' voting decisions. We find that the strategic use of aid disbursements indeed induces strategic voting. In addition, recipient preferences, the credibility of U.S. aid linkages, and consequent voting and aid disbursement strategies vary significantly as a function of recipient regime type, level of development, and alliance relationships.

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# 1 Introduction

Vote buying is widely believed to affect UN General Assembly voting, and UN voting patterns are associated with development assistance and with flows from multilateral institutions such as the World Bank and the IMF. Nevertheless, UN voting is often used as a measure of countries' sincere preferences. Correlations do not establish whether vote buying in fact occurs: if donors prefer to give assistance to like-minded regimes, such associations can arise without affecting voting. Indeed, the donors may not be concerned about how aid recipients vote in the UN. If, as is more likely, rich countries both buy votes and give support to friendly regimes, it may be difficult to disentangle the two effects to determine how significant the effect of vote buying is in practice. The literature has struggled with these problems without providing a satisfactory solution.

When vote buying occurs, strategic interaction creates additional inference problems. Strategic votes reflect three influences: government preferences, the susceptibility of particular regimes to international influence, and the credibility of threats or promises that are used to influence votes. If we find, for example, that democratic regimes are more likely to vote similarly to the United States than autocratic regimes, this might be because they have similar preferences. Alternatively, democracies might be more vulnerable to U.S. influence and therefore more willing to comply with U.S. influence attempts. A third possibility is that U.S. threats or promises are more credible when addressed to democracies. It is essential to explicitly account for the strategic incentives of voters and vote buyers if we want to understand if and when vote buying that occurs.

We introduce a statistical technique that allows us simultaneously to estimate voting preferences, susceptibility to influence by the United States, and the credibility of U.S. influence attempts. We take advantage of the fact that since the mid-1980s, U.S. law has required the State Department to report how countries vote on issues that are regarded as important to U.S. interests, and has required USAID to use countries' voting records on these important issues as a criterion for disbursing aid. We estimate a strategic statistical model in which countries decide how to vote on an issue that has been designated by the United States State Department as important to U.S. interests, and then the United States decides whether to withhold a portion of committed aid, if the country votes against the U.S. position, or reward the aid recipient with additional aid, if the country votes in favor of the U.S. position. Because this model captures the strategic element of voting, we are able

to evaluate the effect of anticipated punishments or rewards on the voting decision. Furthermore, we are able to differentiate which regimes are most susceptible to influence and which influence attempts are most credible.

We find that the United States punishes and rewards recipients very differently depending on their regime type, political orientation (i.e., left-right orientation of executive), level of development, and alliance relationships. These differences in credibility, furthermore, are key to explaining the effectiveness of U.S. influence attempts. Furthermore, we find the effects of the Cold War on behavior to be strong for the U.S. but relatively insignificant for recipient countries. Specifically, the importance of the ideological orientation of the recipient country's government and the nature of its economic and security relationship with the U.S. change markedly after the Cold War. Our findings suggest that factors relevant to U.S. competition with the Soviet Union dominate the U.S.'s propensity to punish aid recipients during the Cold War but not after. Interestingly, the relationship between regime type, the credibility of U.S. influence attempts, and recipient voting behavior are found to be remarkably consistent during and after the Cold War. Finally, we provide a direct test of whether it is indeed necessary to model recipient voting behavior using strategic choice model. The test provides strong support for the idea that recipient voting behavior is significantly influenced by U.S. aid disbursements on the set of important votes.

## **2 Vote Buying in the United Nations**

An extensive quantitative literature beginning in the 1960s examines voting patterns in the UN General Assembly (Alker, 1964; Russett, 1966; Newcombe, Ross and Newcombe, 1970; Hagan, 1989; Kim and Russett, 1996; Voeten, 2000), and a smaller, related literature has sought to identify when and how major powers use foreign aid to buy votes (Wittkopf, 1973; Rai, 1980; Kegley and Hook, 1991; Wang, 1999). However, despite the fact that it has been official U.S. policy to link aid to designated important votes since the mid-1980s, convincing quantitative evidence of this linkage remains elusive. Wittkopf (1973), for example, finds limited support for a connection between U.S. foreign aid flows and UNGA votes, but his conclusions suffer from serious measurement and

inference problems.<sup>1</sup> Consensus has been hampered by mixed findings and the absence of a clearly specified model of vote buying (Rai, 1980; Kegley and Hook, 1991; Wang, 1999).

A few studies offer associations between U.S. aid flows and agreement on votes with the United States as evidence of vote buying (e.g., (Wang, 1999)). These correlations, however, can be explained in two distinct ways. First, popular wisdom may be correct, so UN voting is associated with foreign aid because foreign aid is used to reward or punish countries for voting in particular ways. Alternatively, it may be the case that UN voting is not intrinsically important to aid donors, but rather reflects the sincere foreign policy preferences of UN members. In that case, any relationship between UN voting and aid flows can be interpreted as evidence that aid donors prefer to contribute resources to like-minded regimes that have similar foreign policy objectives. The political implication of this view is that UN voting is not corrupted by foreign aid flows, although perhaps only because the votes themselves are not sufficiently important to motivate aid donors.

A similar ambiguity arises in the literature on campaign contributions in the United States Congress. One set of studies assumes that votes are sincere, which is necessary, for example, if we want to use them to identify legislators' ideal points (Poole and Rosenthal, 1991). Another literature argues that campaign contributions are made as an explicit effort to buy votes (Grossman and Helpman, 1994). A third strand of literature criticizes the vote-buying view by arguing that contributions are made to support legislators who are known to share the donors' policy preferences—again, implying that votes are sincere rather than strategic (Snyder and Groseclose, 2000; Ansolabehere, de Figueiredo and Snyder, 2003). Similarly, some international relations scholars have treated UNGA voting as an index of states' sincere preferences (Gartzke, 2005; Russett and Oneal, 2001; Stone, 2004), while others have treated correlations between UNGA voting and foreign aid as unproblematic evidence of vote buying. One promising approach to dealing with these problems in American politics, the difference-in-differences approach that investigates the effects of changes in contributions on changes of votes, is less attractive in the UN context because

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<sup>1</sup>Wittkopf (1973) calculates the difference between expected aid flows and actual aid flows using a very suspect measure of expected aid flows. Specifically, Wittkopf assumes that “[t]he expected value,  $E_{i,j}$ , is calculated on the basis of the allocation of each donor's total aid volume as distributed proportionally among all of the recipients included in the “system” analyzed. The model assumes that donor  $i$  will send to recipient  $j$  approximately the same percentage of its total foreign aid as the percentage of the total aid which  $j$  receives from all donor countries combined (875).”

of data scarcity (Stratmann, 2002; Broz, 2005).<sup>2</sup>

The hypothesis that UN voting affects foreign aid is plausible—at least in key votes that attract substantial attention from donors—given what we know about the political biases and determinants of aid flows. Need-based criteria play an important role in determining aid flows, as do broad political objectives such as promoting democracy and human rights, but it is well established that the political agendas of the donors are critical and shift aid away from need-based allocations (Boone, 1996; Alesina and Dollar, 2000; Collier and Dollar, 2002). Studies specifically focused on the distribution of aid have shown that aid is strongly related to the geopolitical interests and foreign policy preferences of the donors (e.g. Maizels and Nissanke (1984); Boone (1996); Cashel-Cordo and Craig (1997); Schraeder, Hook and Taylor (1998); Alesina and Dollar (2000); Alesina and Weder (2002)). Studies that compare the aid allocations of multiple donors find that the reasons for giving aid vary enormously and are heavily influenced, for example, by the donors’ colonial ties (Svensson, 1999; Alesina and Dollar, 2000; Alesina and Weder, 2002; Neumayer, 2003). If these relationships are in fact strategic, they should hold most strongly for aid from the United States, which has the most far-flung foreign policy commitments, and they should apply particularly for the set of votes that the United States State Department designates as “important votes.”

A number of studies have found associations between UN voting and U.S. foreign aid, but the findings are mixed. For example, Rai (1980) finds an association between aid flows and UNGA voting, but cannot isolate the causal mechanism. Kegley and Hook (1991) do not find much evidence that the explicit linkage between UNGA voting on important issues and aid disbursements established in the 1980s has any effect on voting behavior. However, as Wang (1999) points out, most previous work had not distinguished between votes identified as important by the U.S. State department and ordinary votes.<sup>3</sup> Axel Dreher and Thiele (2008) disaggregate aid into categories and use an instrumental variable approach that addresses some of the ambiguities in the previous literature, and find evidence in favor of a vote-buying hypothesis. However, the notion that vote-buying induces strategic voting behavior that can bias statistical results if unaccounted for is not considered.

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<sup>2</sup>The difference-in-differences approach generates a conservative estimate of vote buying, because it identifies a relationship only when behavior changes.

<sup>3</sup>For an early treatment that defines “important” votes as those in which the U.S. and Soviet Union disagree, see Wittkopf (1973).

In addition, a number of studies have found associations between UN voting and aid from various donors and international institutions (Barro and Lee, 2005; Oatley and Yackee, 2004; Thacker, 1999; Stone, 2004). Indeed, one of the most robust findings about participation in IMF programs is that IMF lending is significantly shaped by the geopolitical preferences of the countries that contribute the most resources, particularly the United States. UN voting is rapidly becoming recognized as an important control variable in studies that seek to explain participation in IMF programs, and as a useful instrument for selection-controlled studies of their effects, because UN voting is presumably exogenous with respect to outcome variables such as economic growth (Steinwand and Stone, 2008). Several studies, following Thacker (1999), have used the similarity of a country's profile of votes in the United Nations General Assembly to those of the United States to measure political affinity to the United States. Thacker finds that increasing this congruence over time is associated with a higher probability of IMF lending. Barro and Lee (2005) find that IMF loans are associated with similarity to U.S. voting patterns in the UN and economic ties with the United States. This quantitative evidence therefore supports the anecdotal evidence that numerous countries that had not met the technical criteria to qualify for IMF support received it nevertheless because they played important roles in U.S. foreign policy. Prominent examples include Zaire and the Philippines during the Cold War, and Russia, Ukraine, Egypt, Pakistan and Turkey during the 1990s.

Several recent studies have focused on votes designated as important by the United States as a way to build a case for vote buying. Andersen, Harr and Tarp (2006) assume that aligning with the United States on important UNGA votes is a concession, and use these votes to construct a measure of the political concessions a country makes to the United States, which they use to estimate the probability that a country obtains an IMF loan. Kilby (2010) uses a similar measure in a recent study of World Bank loans. Using alternative strategy, Kuziemko and Werker (2006) narrow the interpretation of their empirical results by focusing on temporary membership in the UN Security Council, which creates a temporary opportunity to offer valuable concessions. UNSC voting is more significant than UNGA voting, so incentives to buy votes during crises are stronger. In addition, since temporary UNSC membership rotates and can only be held for two-year terms, it is possible to isolate the treatment effect from country fixed effects by studying changes in aid flows. The authors find that U.S. foreign aid increases significantly when a country becomes a temporary

UNSC member, and drops off again after membership lapses. Dreher, Sturm and Vreeland (2009) find a similar effect of temporary membership in the UN Security Council on World Bank loans. in which the United States made extensive offers of aid in order to line up support in the Security Council for its two wars with Iraq were not idiosyncratic, and in fact tell us something important about how the institution functions.

In sum, there is substantial reason to believe that vote buying occurs in the UN General Assembly, but this has not been definitively established. Furthermore, none of the existing work addresses the problem of strategic interdependence. The U.S. strategy of linking aid disbursement to voting on important issues should, if effective, induce strategic voting behavior on the part of recipients, which should make patterns more difficult for an analyst to observe. To fill this gap, we employ a strategic estimator that allows us to explicitly model the effects of U.S. linkage strategies on UN voting. An attractive feature of our model is that it allows us to model the effects of recipient characteristics such as regime type and political orientation on voting preferences, on the vulnerability of target governments to influence attempts, and on the credibility of those attempts.

forum. It also has implications that reach far beyond the United Nations. Thus, for example, our interpretation of the association between UN voting and IMF programs takes on different interpretations depending on the degree of strategic voting that takes place in the UN. Similarly, if UN voting patterns account for variations in international conflict, it is important to know whether these patterns reflect the countries' policy preferences and world views or ties of U.S.patronage (Russett and Oneal, 2001).

high-stakes maneuvering in the Security Council lies a middle ground, where major powers count votes on important issues and weaker countries have real choices to make. In these cases, individual countries vote in ways that tell us important things both about their preferences and about the ways in which the distribution of power in the international system skews majoritarian decision-making processes. In order to assess countries' true preferences, however, we have to estimate the degree to which their voting behavior is a strategic response to monetary incentives.

### 3 A Strategic Estimator for UN Voting

Previous studies of UN voting have not been unable to disentangle strategic and sincere voting because they have not estimated a strategic model. There are two important methodological issues here. First is the familiar problem of endogeneity, and the substantive concern is that UN voting may be associated with U.S. aid either because countries comply with U.S. preferences in order to obtain aid, or because countries that sympathize with U.S. positions in the UN are likely to receive aid irrespective of how they vote. Our approach deals with endogeneity by estimating equations for U.S. aid allocations and for UN voting decisions and by making identifying assumptions, as in an instrumental variables approach, but it takes advantage of the strategic structure of the model as part of the identification strategy. The second issue is strategic misspecification bias, and the substantive concern is that the relationships among preferences, voting and aid allocations may depend upon strategic calculations. In particular, we argue that the credibility of U.S. influence strategies varies systematically across countries, which affects the relationship between aid and UN voting. Estimators that fail to account for how the U.S. influence strategy induces strategic recipient behavior will be biased and inconsistent; the effect is equivalent to omitted variable bias (Signorino and Yilmaz, 2003).

Strategic effects are important because the effectiveness of U.S. influence attempts depends upon their credibility. Suppose that the United States threatens to reduce aid to a developing country if it votes against the U.S. position on an important vote, and we observe that the country defies the U.S. demand. Two inferences are possible. It may be the case that the country's leadership is highly motivated to resist U.S. policy preferences. Alternatively, the government might not be strongly opposed to the U.S. position, but the leadership might calculate that the U.S. threat is unlikely to be carried out. It is impossible to accurately estimate either government preferences or the effectiveness of influence attempts without considering the effect of variations in credibility. Consequently, we use a strategic statistical estimator designed to capture this effect.

The structure of the statistical model we estimate is depicted in Figure 1. First, the recipient country decides whether or not to vote with or against the U.S. position. If the recipient votes against the United States on an important vote, the United States decides whether to punish it with significant aid reductions. If the recipient's vote coincides with the U.S. position, the United



States chooses whether to reward it with a significant increase in aid flows.<sup>4</sup> The model imposes the simplest possible structure that allows for strategic voting and for threats and promises to be linked to aid flows.

In order to convert the formal model into a statistical model, we add a stochastic component to the utilities of the actors, which gives us a distribution over the four possible outcomes of the model. We characterize this disturbance as agent error, which seems appropriate to our context (Signorino, 1999, 2003). Agent error might occur in the voting stage, for example, because the UN ambassador is not informed, or not informed in a timely manner, of the preferences of the leader, or because disagreements within the government give the ambassador discretion to vote his or her own preferences. Agent error might occur at the disbursement stage because of a disagreement between the executive and legislative branches of government, because of an interagency dispute, or because of some other intervening variable that is orthogonal to UN voting, such as the recipient country’s policies regarding human rights, trade or the environment.

[Figure 1 about here.]

The recipient and the United States make decisions in the game by weighing their expected utilities for each possible action. The model explicitly allows the recipient’s voting decision to be affected by the anticipated alteration of aid flows by the United States. We start from the last move in the game, the United States’ decision to punish, reward or do nothing in response to the recipient’s vote, and move up the game tree to show the players’ expected utility calculations. For each vote, or observation,  $i = 1 \dots n$ , the recipient decides whether or not to vote with the U.S. position. If the recipient does not vote with the United States, the United States makes the

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<sup>4</sup>We specify punishments and rewards as dichotomous to insure that we are isolating unexpected and significant fluctuations in aid flows. Additionally, given that aid disbursements can fluctuate for a variety of reasons that are not related to UN Voting, we want to account for this in our measure of rewards and punishments. Consequently, an approach that simply measures the difference between commitments and disbursements would not be appropriate. Thus, we are careful to specify a model that is very accurate in predicting aid disbursements so we can isolate cases in which there are significant and unexpected deviations. See the section that outlines the dependent variables for details.

following comparison<sup>5</sup>

$$p_{i,4} = U_{US}^*(Pun|Disagree) > U_{US}^*(\neg Pun|Disagree) \quad (1)$$

$$= U_{US}(Pun|Disagree) + \epsilon_4 > U_{US}(\neg Pun|Disagree) + \epsilon_3. \quad (2)$$

Assume the  $\epsilon$  terms are independent and identically distributed (i.i.d.) Type 1 Extreme Value, which yields

$$p_{i,4} = \frac{\exp^{U_{US}(Pun|Disagree)}}{\exp^{U_{US}(Pun|Disagree)} + \exp^{U_{US}(\neg Pun|Disagree)}} \quad (3)$$

$$p_{i,3} = 1 - p_{i,4}. \quad (4)$$

In deciding whether or not to reward the recipient when the recipient votes in agreement, the U.S. makes a similar comparison which leads to expressions almost identical to those in equations 1–4. The recipient makes its decision to vote with the U.S. position or not by calculating, with some error, its utility for voting in agreement and disagreement with the U.S. The recipient's utility for comparison for its voting decision is a function of its preferences over outcomes and the probability the United States will subsequently reward or punish it with aid. The comparison of the expected utilities for voting with or against the U.S. position take the following form

$$p_{i,2} = U_R^*(Agree) > U_R^*(Disagree) \quad (5)$$

$$= U_R(Agree) + \epsilon_2 > U_R(Disagree) + \epsilon_1. \quad (6)$$

If we again assume that the  $\epsilon$  terms are i.i.d. Type 1 Extreme Value, we obtain

$$p_{i,2} = \frac{\exp^{(p_{i,6}U_R(Agree,Rew)+p_{i,5}U_R(Agree,\neg Rew))}}{\exp^{(p_{i,6}U_R(Agree,Rew)+p_{i,5}U_R(Agree,\neg Rew))} + \exp^{(p_{i,4}U_R(Disagree,Pun)+p_{i,3}U_R(Disagree,\neg Pun))}} \quad (7)$$

$$p_{i,1} = 1 - p_{i,2}. \quad (8)$$

We utilize the statistical backwards induction technique (SBI) developed by Bas, Signorino and Walker (2007). The SBI technique is employed by separately estimating the logit equation for each possible decision in the game rather than simultaneously estimating the full system of equations. Thus, we first estimate the United States' utility for punishing disagreement (i.e.,  $X_{24}\beta_{24}$ ) and the United States' utility for rewarding agreement (i.e.,  $X_{26}\beta_{26}$ ) with two logit regressions. Second, we estimate the recipient's utilities over all possible outcomes (i.e., equation 8). The probabilities over

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<sup>5</sup>Note that *Pun* stands for punish, *Rew*, for reward, *Agree* for agreement with the U.S. position, and *Disagree* for disagreement with the U.S. position. The numbers on the probabilities and  $\epsilon$  terms correspond to the numbers assigned to the players' actions in figure 1.

the U.S. actions in equation 8 (i.e.,  $p_{i,3}-p_{i,6}$ ) are obtained from the two regressions that estimated the U.S. utilities. SBI is attractive in our context because it ensures that the likelihood is concave, so our results reflect the true maximum likelihood estimate. Additionally, computational time is decreased significantly relative to simultaneous estimation of the full system of equations. The disadvantage of SBI is that the standard errors in the recipient’s estimates are biased downwards because the probabilities are treated as fixed. Following the recommendations of Bas, Signorino and Walker (2007), we correct the standard errors using the bootstrap. Although the standard errors for the U.S. utilities are not affected by this issue, we also bootstrap them to be conservative.<sup>6</sup> To account for the fact that there are multiple important votes in each year, we sample by vote to ensure that this does not deflate the standard errors.<sup>7</sup>

Below, we specify the utilities of the recipient and the United States with some of the same variables. Consequently, to identify the model both the recipient and the United States must have the utility for at least one outcome that is possible at their initial information set and affects their utilities normalized to zero.<sup>8</sup> Also, no regressor can be estimated in every utility. We normalize both the recipient’s utility for not being punished after voting in disagreement (a sincere opposition vote with no consequences) and the U.S. utility for not rewarding the recipient following a vote in agreement (harmony) to zero. Thus, all estimated coefficients for each player in each of the remaining utilities are interpreted relative to these outcomes. Normalization of a player’s utility for one possible outcome in the strategic logit model is analogous to the standard method of identifying a multinomial logit model. This model effectively captures strategic voting and allows threats and promises about important UNGA votes to be linked to aid flows.

## 4 Data

We utilize data on aid flows from the United States, voting by the United States and U.S. aid recipients in the UNGA, and data on other variables of interest. The data on aid flows from the United States to potential recipients are published by the OECD Development Assistance

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<sup>6</sup>500 bootstrap iterations are run in each model.

<sup>7</sup>This is analogous to estimating clustered “robust” standard errors, although it often yields more conservative estimates.

<sup>8</sup>In this game, an initial information set for each player is the node at which it makes its first move in the game (Lewis and Schultz, 2003).

Committee and cover 1960–2001. The data include both Official Development Assistance (ODA) and Official Assistance (OA) disbursements in millions of U.S. dollars.

We utilize the *Documenting Votes in the UN General Assembly, v2.0* data set compiled by Voeten (2005), and we focus on votes defined as important by the U.S. State Department in its annually published *Report to Congress on Voting Practices in the United Nations*.<sup>9</sup> The temporal domain starts in 1985, the year in which U.S. law first required the State Department to report how countries vote on issues that are regarded as important to U.S. interests, and ends in 2001. The United States is never absent for important votes. Votes in which the recipient country is absent are excluded. We exclude absences for recipient countries because it is unlikely that the United States rewards or punishes states for not showing up. Given that we posit a strategic relationship between important votes and aid we want to be extremely careful not to include unimportant or aberrant votes in our analysis.

## 4.1 Regressors

We utilize several regressors to estimate the utilities of the recipient countries and the United States over the outcomes in the model, including variables specific to the recipient country and variables that characterize the relationship the recipient has with the United States. The variables specific to the recipient are Polity IV scores, GDP per capita, and the political orientation of the executive (Keefer, 2007). Both GDP per capita and bilateral trade flows are measured in 1996 U.S. dollars to ensure comparability across the two measures and over time.<sup>10</sup> To measure the political orientation of the executive, we create two binary variables that indicate whether a recipient country’s executive was left of center and whether it was right of center, respectively. The excluded category includes governments that are centrist and those whose orientations are not clear.

[Table 1 about here.]

To model bilateral relationships we include bilateral trade flows and a variable that indicates whether the recipient is in an alliance with the United States (Oneal and Russett, 2005). In addition, to model specific characteristics of particular votes, we include a variable that indicates whether

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<sup>9</sup>We thank Xun Jiang, who extended the Voeten data to include all important votes, and shared his data with us. We compared these data to that of Kilby (2010) and found few differences in coverage.

<sup>10</sup>Bilateral trade is measured in millions of 1996 U.S. dollars.

the United States voted “No.” “Yes” and “No” votes are qualitatively different, because UNGA proposals almost always pass, so “No” votes find the United States in the minority, and usually badly isolated. In the late 1970s, after the United States lost control of the UNGA agenda to the Group of 77, the United States began to vote “No” on most roll calls, where it had previously cast a majority of “Yes” votes. Roll calls on which the United States votes “No” tend to be embarrassing to U.S. diplomacy, so the State Department is particularly interested in identifying a few stalwart supporters to provide cover. The United States votes “No” on almost 75% of State Department-identified important votes in the sample.

## 4.2 Dependent Variables

Our dependent variables measure whether countries voted with or against the U.S. position on important votes in the UNGA and whether there were significant deviations of U.S. aid disbursements from the trend. First, we utilize voting records on important votes and create a binary variable that indicates whether the votes of the United States and recipient countries coincide on each vote of interest. Thus, if the United States and the recipient both vote “Yes” or both vote “No,” this variable equals 1, while it takes a value of 0 otherwise.<sup>11</sup>

Creation of the aid disbursement variables requires care to ensure that we do not treat aid fluctuations that result from temporal trends such as inflation or exogenous factors unrelated to particular UN votes as punishments or rewards. Note that we need two dependent variables, one to indicate whether the United States punished the recipient with a significant aid reduction following conflict on an important vote, and a second to indicate whether the United States rewarded the recipient with a significant increase in disbursement following agreement. For this purpose we could have used a naive punishment variable, such as one that takes a value of 1 if aid disbursements in a given year are lower than aid commitments. However, such a measure would include numerous false positives, because aid disbursements generally lag behind commitments for a variety of reasons that are unrelated to UN voting.

We choose a conservative coding strategy to avoid imputing political motivations to random

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<sup>11</sup>We treat “Abstain” as agreement with the United States position, as the United States works hard in many cases to get countries to abstain on particularly sensitive issues. Although we think this is the right choice substantively, we also tried treating abstentions as disagreements and did not find markedly different results.

fluctuations, coding "punishments" and "rewards" only when aid disbursements fall outside the 95% confidence interval of the expected level. We estimate the predicted aid disbursement for each country in each year with a lagged dependent variable, fixed effects model. This approach has several important advantages. First, we use the information about projected disbursements contained in aid commitments, so our variables can be interpreted as discretionary deviations by the executive branch from appropriated aid levels. Second, our estimation procedure explicitly controls for temporal trends in disbursement. Finally, the fixed effects account for the fact that some recipient countries receive more aid for idiosyncratic reasons. For example, we know that Egypt is a major recipient of U.S. aid for geopolitical reasons, and our approach accounts for this. For each country  $i$ , aid at time  $t$  is estimated using the following specification

$$AID_{i,t} = \beta_0 + COM_{i,t}\beta_1 + AID_{i,t-1}\beta_2 + \epsilon_{i,t}. \quad (9)$$

The inclusion of more than one lag has no effect on the fit or predictions of the model, so we include only the first lag. The model explains the variation in yearly aid disbursements across recipients very well (R-squared  $> 0.99$ ), but more variance in aid disbursements remains to be explained for each recipient over time (R-squared  $\approx 0.53$ ). The bivariate correlation between actual aid disbursements and predicted aid disbursements is 0.85, which indicates that the model's predictions are quite accurate.

[Figure 2 about here.]

We utilize the model shown in table 2 to produce predicted aid disbursements with 95% confidence intervals for each recipient in each year. The punishment variable takes a value of one if the actual aid disbursement is below the lower bound of the 95% confidence interval, and zero otherwise, and the reward variable takes a value of 1 if the actual disbursement is greater than the upper bound of the 95% confidence interval, and zero otherwise. Our approach is scale invariant, so the construction of the dependent variable does not lead to spurious inferences, for example, that countries that receive relatively high levels of aid (e.g., Israel) are more likely to receive punishments or rewards. The distribution of the data across all possible outcomes in the model is depicted in figure 2.

[Figure 2 about here.]

The distribution of data across the four possible outcomes in the model is as expected: punishments and rewards are rare, because we have defined them conservatively. Punishment by the United States following disagreement on an important vote happens only about 3% of the time, while a decision not to punish takes place around 63% of the time. This distribution is similar to that found in other dependent variables in international relations that measure punishments (e.g., economic sanctions). The United States uses punishments and rewards selectively, even when we limit our analysis to important votes.

## 5 Results

The results of the full strategic model are presented in Table 3. The model correctly predicts over 84% of the observations, which indicates that it fits the data quite well. This is an improvement of 34% over predicting the modal category. All of the columns of coefficients result from the same model, and each column in the table contains the estimates for either the recipient’s or the U.S. utility for a particular outcome. For example, the first column contains the estimates for the recipient’s utility for being punished after voting in disagreement with the United States. As noted above, all estimated coefficients for the recipient are interpreted relative to the utility for disagreement without consequences, so the coefficients in the first column represent the cost of aid withdrawn. Similarly, all coefficients for the United States are interpreted relative to the utility for agreement with no reward, so the coefficients in the last column represent the cost/benefit of providing rewards when recipients accommodate U.S. vote choices. We first discuss the U.S. utility for punishing and rewarding, and then discuss recipient behavior. In general, we base our discussion on the substantive effects reported in tables 4–5 and figures 4–5, because the estimated coefficients in table 3 are not straightforward to interpret.

### 5.1 Punishments and Rewards: U.S. Behavior

Table 4 contains the probability that the United States punishes following disagreement and rewards following agreement at various levels of the statistically significant variables. The first row of Table 4 shows the probability of punishment and reward when all variables are held at their median values, and each subsequent row alters the value of one variable to isolate its effect on the predicted

probabilities. Thus, the second row shows the probability of punishment and reward when the recipient is an autocratic country (Polity Score=-9) and all other variables are held at their median values. Specifically, the second column of table 4 shows the change in the probability of punishment relative to the median case (i.e., the first row), and the third column expresses this as a percentage change in probability. Columns 4-6 repeat this procedure for the probability that the United States rewards the recipient country when it votes in agreement. Thus, the second row indicates that a relatively autocratic recipient is 33% less likely to be punished if it opposes the United States and 63% less likely to be rewarded if it cooperates than in the median case (Polity Score=-1).

The baseline predicted probabilities in the first row of table 4 reflect the fact that the United States uses aid-based punishments and rewards sparingly (as demonstrated in Figure 2), and the fact that we have defined our reward and punishment variables conservatively. The choice of carrots or sticks depends on whether the United States votes “Yes” or “No.” In the baseline case, when the United States votes “No” because it is in the minority, the probability of punishing a country that deviates from the preferred U.S. position is 0.042 when all variables are held at their median or mean, while the probability that it rewards compliance is only 0.008. In contrast, when the United States votes in favor of a resolution, it is more likely to reward members of its coalition (.042) than chastise its opponents (.005). The different U.S. strategy reflects a basic difference in the kinds of issues on which the United States finds itself in the minority: the 26.5% of important resolutions that the United States supports are not as contentious as the 73.5% that it resists. Important votes on which the United States votes “Yes” pass by large margins; on the other hand, when the United States votes “No,” it is usually badly isolated, so the symbolic value of attracting some support is maximized. The United States is five times more likely to use carrots than sticks when it is trying to promote an important resolution, and eight times more likely to use sticks rather than carrots when it is isolated and trying to resist one. The distinction between “Yes” and “No” votes is one that the literature has not previously made, perhaps because it has not distinguished between punishments and rewards.

The U.S. decision to use aid as an inducement depends on the recipient’s level of development and its trade relationship with the United States. Poor countries are much more likely to be punished when they oppose the U.S. position, while more developed countries are more likely to be



rewarded when they offer support. This suggests that punishments are less costly to apply to weak countries, and the United States prefers to use positive incentives with more developed countries that are better able to resist. Trade exposure has a uniform effect of reducing the credibility of U.S. aid linkages: countries that trade substantially with the United States are less likely to be punished or rewarded for their voting behavior. This reflects the logic of vote buying: rational actors buy the cheapest set of votes to build their coalitions. Some of this effect is attributable to the effects of scale, since vote buying is least expensive when it is directed at small countries whose votes are most easily bought. Furthermore, trade creates interdependence, which lowers the salience of foreign aid in bilateral relations, and apparently makes the United States reluctant to tie aid to UN voting.

The results in table 3 indicate that the left-right orientation of the executive also significantly affects the choice between positive incentives and sanctions. Relative to regimes with centrist executives, the United States is less likely to punish regimes with right-wing executives when they vote in opposition and less likely to reward regimes with left-wing executives when they vote in support. Table 4 indicates that countries with right-wing executives are 50% less likely to be punished when they oppose the United States. The United States appears to be averse to punishing right-wing governments for their votes in the United Nations because right-leaning governments support policies that the United States finds beneficial on a wide range of other issues, such as economic reform. On the other hand, aid recipients with left-wing executives are 38% less likely to be rewarded by the United States when they support its position. This suggests that the United States has been reluctant to use aid policy to support left-wing governments, even when they cooperate with U.S. policy. The effect of U.S. reluctance to reward left-wing governments and to punish right-wing governments is that both face weaker incentives to comply with U.S. preferences than centrist governments. We probe this relationship further below, where we analyze subsamples of the data produced during and after the Cold War.

[Table 3 about here.]

The results reveal that the United States conditions its behavior on regime type in significant ways, but not in the way the literature typically supposes. The results in Table 4 indicate that U.S. promises and threats to condition aid on UN voting are most credible for the set of democracies.

The United States is reluctant to punish autocracies, perhaps because they are more dependent on aid flows to maintain power (Bueno de Mesquita et al., 2003), and only receive aid in the first place if they are important to U.S. foreign policy (Schraeder, Hook and Taylor, 1998). The United States is also reluctant to reward autocracies with increased aid, perhaps because giving aid to dictators is unpopular in Congress. These findings stand in contrast to the argument of (Bueno de Mesquita and Smith, 2007) that aid is directed disproportionately to authoritarian countries precisely because their narrow bases of support make it less expensive to purchase policy concessions from them. If this were the case, autocracies should be most likely to be punished when they oppose the United States and rewarded when they comply; but we find that democracies are more likely to be punished for non-cooperation and rewarded for cooperation. It is a striking finding that the United States is less nimble in its use of aid to reward and punish autocracies, and as we discuss below, this makes them less supportive of U.S. positions. The present analysis cannot offer a direct test of alternative mechanisms to explain this finding, but our conjecture is that aid to autocracies is tied to particular, long-term policy goals such as regional stability or military basing rights, and is provided primarily to prevent regime change. If this is the case, it could be excessively costly to use this aid to influence UN voting.

Although we cannot directly test this conjecture, the nonlinear effects of the variables allow us to probe a bit further. Figures 4(a) and 4(b) plot the effects of varying development and regime type simultaneously.<sup>12</sup> As noted above, a low level of development makes punishments more likely and rewards less likely. Figure 4(a) shows that the United States is substantially more likely to withhold aid from a relatively poor recipient than from a more highly developed recipient, and from a democracy than from an autocracy. The interaction between these two effects is significant, and the effect of regime type is strengthened in the set of poor recipients. The slope of the curve is much steeper when countries are poor, indicating that poverty is a reason for the United States to be more reluctant to punish authoritarian countries rather than democratic ones. Relatively poor autocratic regimes have high aid dependency ratios (aid to GDP) and may be vulnerable to political instability if aid is cut off. Consequently, this finding suggests that the U.S. disinclination to punish authoritarian countries may be largely attributable to concern about political stability.

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<sup>12</sup>Note that GDP per capita and trade were divided by 1000 and 10000 respectively to make estimation computationally easier. Thus, interpretation of the axis in all of the figures should be adjusted accordingly.

Rewards have a similar interpretation, which reflects the fact that rewards and punishments are strategic substitutes. Figure 4(b) demonstrates that democracies are more likely than autocracies to receive a reward if they cooperate by voting with the United States, and the probability that they receive a reward increases as their income increases. Again, the interaction between these effects is important. Poor countries and autocracies are unlikely to be rewarded under any circumstances, but the effect of development on the probability of being rewarded increases rapidly as countries become more democratic, and the effect of being democratic increases rapidly as countries become more developed. This suggests a political interpretation. Democratic leaders often have electoral incentives to oppose U.S. policy, and as the level of development of their countries increases, they become increasingly resistant to U.S. pressure. As a result, using negative incentives becomes less attractive, and rewards increase because they represent a substitute for sanctions.

The case of Nicaragua in the early 1990's illustrates the way in which the United States uses aid disbursements to punish and reward relatively poor democracies. In 1990, Nicaragua conducted multiparty elections that were won by the conservative opposition party, led by Violeta Chamorro. Since Nicaragua was a poor democracy (Nicaragua's Polity score is 6 in 1990), we expect both punishments and rewards to be more likely than in the average country. Nicaragua's GDP per capita hovered around \$2000 in the early 1990s, which is well below the mean of \$5000 in the sample. In 1991 the Chamorro government voted in support of the United States on resolution R/46/82A, which pertained to the Middle East peace process, and was rewarded. The United States had taken note of a much more cooperative Nicaraguan government (Serafino, 1990) and subsequently released additional aid funds after observing cooperation in several areas as well as a rare instance of cooperation in the UNGA.

In the following year, the Chamorro government took a more oppositional stance relative to U.S. interests in the UNGA, voting against the U.S. position on all but one important vote. Chamorro's opposition to U.S. positions, including on votes involving Cuba, was apparently intended as part of an effort to build bridges to the Sandinista opposition. The sole exception was a resolution that the United States supported on the situation in Bosnia, which passed unanimously. In response to this lack of cooperation, the United States reversed its aid policy towards Nicaragua in 1992 and punished the Chamorro government with significant aid reductions (The New York Times, 1992;

Krauss, 1992). The same pattern continued into 1993, with Nicaragua voting against a number of important resolutions and the United States continuing to withhold aid.

## 5.2 The Strategy of UN Voting: Recipient Behavior

We now turn to a discussion of recipient behavior. The model allows for voting behavior to be strategic, because voting decisions precede aid disbursements. Consequently, vote choices depend both on governments' underlying preferences and on U.S. disbursement strategies. Table 5 presents the substantive effect of each regressor on the probability that the recipient votes in opposition to the U.S. position. As in Table 4, the first row depicts the median case for all variables, while each subsequent row isolates the effect of changing one variable. The table indicates that democracies, more developed countries, and countries with weak trade ties to the United States are less likely to oppose the United States than autocracies, poor countries, and those with more substantial trade ties. Surprisingly, non-allies are less likely to oppose U.S. positions than are U.S. allies.<sup>13</sup> The effect of left-right partisanship is ambiguous, since the residual category (centrists and governments with unidentified partisanship) seems to be most oppositional.

The results in Table 5 indicate that opposition to the United States is widespread on votes that it designates as important, but varies substantially depending on the U.S. position on particular issues. As we discussed above, the baseline probability that an aid recipient votes against the United States when the U.S. position is "No" is 0.88, while the probability that an aid recipient opposes a U.S. "Yes" vote is only 0.03.<sup>14</sup> The United States voted "No" on approximately three-quarters of all votes that it defined as important. When the United States votes "Yes," it finds itself in the majority. These resolutions cover issues on which the United States takes less controversial positions and is able to craft a compromise that it is able to support. Since voting is very different on "Yes" and "No" votes, it is important to control for the U.S. position, which determines the level of recipient opposition and the U.S. propensity to punish and reward recipients. The existing literature on the relationship between UNGA voting and U.S. aid flows (e.g., Kegley and Hook

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<sup>13</sup>The only significant coefficient involving alliances is the one for the cost to the recipient of being punished, which is higher for allies, but the predicted effects for other utilities outweigh this effect, so the net predicted effect is an increase in opposition to U.S. votes. The standard errors for this prediction are very large, however, so we do not put much weight on this result.

<sup>14</sup>Since "No" is the median position of the United States, the first row represents a "No" vote with all other variables at their median.

(1991) and Wang (1999)) has missed the significance of the distinction between “Yes” and “No” votes, and our results suggest that this is an important omitted variable in those studies.

[Table 4 about here.]

As Table 5 indicate, democracies are less likely to oppose U.S. positions in the UNGA. We are now in a position to answer the question of why democracies are more supportive of U.S. positions in the UNGA than autocratic regimes. Is this because democracies are more sympathetic to U.S. policy positions, because democracies are more vulnerable to U.S. influence attempts, or because U.S. influence attempts are more credible among the set of democracies? Our estimates indicate that democracies’ preferences are inherently more oppositional than authoritarian governments’ preferences. The coefficient of -5.63 for polity in the government’s utility for being rewarded for compliance in Table 3 indicates that as a country becomes democratic, the benefits of opposing the United States rather than complying increase. (The reference category is voting in opposition with no consequences.) Democracies in developing countries may be more oppositional because their publics are critical of U.S. positions, and democratically elected leaders have incentives to cast symbolic votes against the United States. On the other hand, democracies are less susceptible than authoritarian governments to punishments: the coefficient of 2.75 in the first column of Table 3 indicates that the cost of punishment declines as polity increases, holding voting behavior constant. The only reason democracies are more compliant than moderately consolidated autocracies – in spite of more oppositional preferences and lower vulnerability to sanctions – is that democracies are more likely to be punished or rewarded. As we concluded above, autocracies cannot generally be punished because the aid they receive is tied to broader U.S. strategic objectives – Egypt is a classic example – and it is much more palatable to reward democratic countries.

This logic is reflected as a non-monotonic relationship between polity and UNGA voting. Non-monotonicity occurs in strategic models because of countervailing strategic incentives, and in this case, U.S. incentives to avoid punishing or rewarding autocracies interact with the preferences of aid recipients. The result is a non-monotonic relationship between regime type and votes against U.S. positions, which depends on the recipient’s level of development. The graphs in Figure 5 depict the relationship between wealth, regime type, and the probability of voting in opposition to the United States. Figure 5(a) depicts the case of “Yes” votes. Aid recipients generally support the U.S. posi-

tion on important “Yes” votes, but relatively poor countries are increasingly likely to vote against the United States if they are not democratic (i.e., Polity Score $\leq$ 6). Opposition by authoritarian governments is maximized among very poor countries, which are most likely to strongly oppose U.S. preferences. However, the effect of polity is non-monotonic. Very authoritarian poor countries are highly compliant, because they have few incentives to oppose U.S. positions. Opposition increases and reaches a peak among weakly consolidated authoritarian countries (Polity Score $\approx$ -4), a range in which punishments and rewards remain very unlikely. As these regimes become more democratic, however, the incentives created by the increasing probability of punishments and rewards come to overwhelm the effects of increasing opposition and decreasing vulnerability to influence, and the probability of opposing U.S. positions again declines to very low levels. This effect is due to strategic voting.

Pakistan is an instructive example, because it experienced a transition to democracy and another back to autocracy within the time period we study. Pakistan was autocratic from 1984–1987 (i.e., Polity score of -4 to -7), was democratic from 1988–1998 under Benazir Bhutto and Nawaz Sharif (i.e., Polity score of 7 to 8), and reverted back to autocracy after a coup led by Pervez Musharraf in 1999 (i.e., Polity score of -6). The expectation of our model is that Pakistan should be punished and rewarded more frequently while a democracy than while it was authoritarian. Figure 3 shows that this is indeed the case. The United States punished or rewarded Pakistan in only one of seven years of non-democratic government, while it punished (6 times) or rewarded (3 times) Pakistan during nine of the eleven years in which it was a democracy.<sup>15</sup>

[Figure 3 about here.]

Pakistan was a good candidate for punishment because it frequently voted against the U.S. position on important issues. Pakistan voted with the United States position only 24% of the time, well below the sample mean of 35%, and this voting pattern was not systematically altered by transitions to or from democracy. Thus, Pakistan is a good case to use to isolate the effects of regime changes on U.S. aid disbursements. In the three years in which Pakistan was rewarded it voted with the United States on several important votes that pertained to the Israeli-Palestinian

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<sup>15</sup>Note that 2001, when Pakistan received substantial aid because of the U.S. war with Afghanistan, is not coded as a reward because Pakistan’s voting was uncooperative.

conflict (e.g., R/44/40A in 1988). In fact, it voted with the United States almost 35% of the time in 1988, 1991, and 1993. In contrast, during the seven years in which it was punished, it only voted with the United States 23% of the time on important votes. Several of the votes identified as important by the United States during the mid-1990s condemned nuclear testing of the kind Pakistan was conducting. For example, Pakistan voted against R/53/77G in 1997, which was one of the most popular U.S.-supported resolutions, opposed by only 8 other countries.

The effects of the political orientation of the government are similarly complex, and would likewise not be adequately captured by a non-strategic model. Recall that the comparison category for aid recipients is non-compliance without punishment. The significant negative coefficient for left-leaning governments, therefore, is attributable to punishment; in contrast, when right-wing governments vote against the United States, they are unconcerned about whether they are punished. This suggests that left governments are more sensitive to the fiscal or social effects of aid reductions. On the other hand, the utility in the third column combines the effect of compliance with the effect of the reward; these effects apparently cancel each other out for left-wing governments, because there is no significant effect. Right-wing governments benefit from agreement with the United States and from being rewarded, so they enjoy a significant positive effect. For its part, the United States is averse to rewarding left-wing governments and to punishing right-wing governments for their votes in the United Nations, presumably because right-leaning governments support policies that the United States finds beneficial on a wide range of other issues, such as economic reform. The net effect of government partisanship on UN voting is a combination of these factors, and we find that both left and right governments are marginally more likely to oppose U.S. positions in the UN than moderate or unclassifiable governments, but for markedly different reasons. Left governments oppose the United States on principle in spite of the costs they bear when the United States reduces their foreign aid and the high probability that it will do so. Right governments tend to support U.S. positions, in contrast, and benefit from U.S. largesse when they do. However, when they are inclined to oppose U.S. positions, they do not face a credible threat of losing access to U.S. support.

Figures 5(c)–5(d) explore the strategic effects of rewards and punishments by illustrating how the probability of recipient opposition changes as a function of GDP per capita and trade. Figure

5(c) depicts “Yes” votes while 5(d) depicts more controversial “No” votes. Again, recipient behavior is markedly different on “No” votes than on “Yes” votes. In both cases, however, poor countries and those that have a high volume of trade with the United States are the most likely to oppose the U.S. position. Poor countries have preferences that are inherently opposed to U.S. preferences (Kim and Russett, 1996; Voeten, 2000). The positive coefficient in the third column of Table 4 (4.42) indicates that as countries increase in development, they come to prefer receiving U.S. rewards to voting in opposition, which implies the opposite for poor countries. As countries become poorer, in fact, the increasing preference for opposing the United States overwhelms the fact that poor countries are more vulnerable to U.S. sanctions. The positive coefficient (9.87) in the first column indicates that the cost of aid withheld is greatest for poor countries, but they nevertheless oppose U.S. positions more frequently.

It is a counter-intuitive result that high-volume trading partners are more likely to vote against the United States. The explanation is strategic. It is not the case that U.S. trade partners are inherently more oppositional than other countries, and neither are they less vulnerable to U.S. punishments. Returning to Table 4, the coefficient in the third column (11.46) indicates that significant trade partners benefit more from being rewarded than from opposing U.S. positions. The negative coefficient (-15.86) in the first column indicates that being punished is very costly for significant trade partners. The explanation for their oppositional voting behavior, therefore, can only be that the United States is unlikely to punish or reward its major trading partners. We found above that this is the case: the United States is unlikely to punish its trading partners if they vote against it, but is also unlikely to reward them if they cooperate. We are now in a position to conclude that this outweighs the inherent sympathy and vulnerability major trading partners have towards the United States, so that the net effect of trade volume is to make them more likely to oppose U.S. positions. The substantive significance of this finding depends on the level of economic development, but trade volume has a monotonic effect that increases opposition.

[Table 5 about here.]

[Figure 4 about here.]

[Figure 5 about here.]



### 5.3 Does Vote Buying Change with the End of the Cold War?

Until this point we have presented results of a model that pools data from 1985 to 2001, but one might ask whether the political economy of UN voting changed significantly with the end of the Cold War. The end of the Cold War might not be expected to substantially change the incentives of aid recipients, but it represented a sea change in U.S. geopolitical strategy, so it would be surprising if there were no adjustments in aid policy. Indeed, it is often argued that foreign aid became more development-oriented with the end of the Cold War. We divided our sample into Cold War (1985-91) and post-Cold War (1992-2001) subsamples to test the robustness of our results, and found several differences in the samples that shed light on the interpretation of vote buying in the United Nations. The results are presented in Table 6.<sup>16</sup>

[Table 6 about here.]

There are no significant differences in recipient preferences between the post-Cold War sample and the pooled model, which indicates that none of the pooled results described above were driven by peculiarities of the Cold War period. Comparing the estimates for the Cold War and post-Cold War periods, the end of the Cold War period does not seem to have changed recipient preferences significantly. Standard errors are larger in the smaller, Cold War sample, leading to fewer significant coefficients, but the signs of coefficients are largely unchanged. These results provide reassurance about our interpretation of the pooled results above.

On the other hand, several significant differences arise in U.S. behavior between the Cold War and post-Cold War periods. First, U.S. treatment of allies changed sharply with the end of the Cold War. We saw above that the United States was slightly more likely to punish allies than non-allies in the pooled sample, although this coefficient was not significant. During the Cold War, however, the United States was significantly less willing to punish its allies, which reflected the constraints imposed by a tense global rivalry. After the end of the Cold War, by contrast, the United States became more willing to punish allies than non-allies, as systemic constraints relaxed. Similarly, the pooled results indicated that the United States was unwilling to punish

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<sup>16</sup>Note that we do not show the utilities for which only a constant is estimated in the models, e.g.,  $U_R(\neg Reward)$  in table 3. Instead, we move this estimated coefficient into the player's utility for the opposite outcome and flip the sign, e.g.,  $U_R(Reward)$  for the constant included in  $U_R(\neg Reward)$ . This is done to make the table fit on one page.

substantial trading partners; in contrast, during the Cold War period, the United States was more likely to punish its trade partners than other countries, again reflecting the heightened stakes of superpower competition in the earlier era. Another sharp difference arises in the treatment of left-leaning governments. During the Cold War, the United States was much more willing to punish left-leaning governments than either right-leaning or centrist governments, but during the post-Cold War era, leftist governments were less likely to be punished for defying the United States than centrist ones. The reduced salience of left-leaning political orientation after the end of the Cold War reflected the collapse of the left as a global challenge to the capitalist economic model.

One central result that does not change with the end of the Cold War is the role of democracy. In both the Cold War and the post-Cold War subsamples, as in the pooled model, the United States is much more likely to punish and reward democracies than autocracies. There is no statistical difference between the coefficients in the separate samples. This suggests that, although many other features of U.S. foreign policy shifted dramatically with the end of the Cold War, the relationship between U.S. foreign aid and authoritarian regimes did not. Certain authoritarian regimes lost U.S. support because the Cold War ended, but the same logic applies during the Cold War and after: autocratic regimes that receive U.S. foreign aid do so because they play key roles in U.S. foreign policy. Consequently, they cannot be punished when they defy U.S. preferences. The U.S. public is sympathetic towards democracies, so democracies receive foreign aid that is not critical to foreign policy and can be used strategically to buy votes. Furthermore, it is politically acceptable to give democracies additional aid when that becomes expedient, so it is credible to offer to increase aid as an inducement. Consequently, it is more credible for the United States to punish and reward democracies.

## 5.4 A Direct Test of Strategic Voting

The results provide indirect evidence that recipient countries vote strategically in anticipation of aid-based punishments and rewards. However, it is possible to directly test the proposition that the relationship between the substantive regressors and recipient voting behavior is strategic. Does recipient voting behavior really depend on subsequent U.S. aid disbursement decisions? The obvious way to assess this is to use comparative model testing methods to compare the strategic model to

a non-strategic model of recipient voting behavior (Clarke, 2001, 2003). Such a test has two key implications. From a methodological perspective, a comparative model test will assess whether a strategic statistical model is the appropriate specification, given our data. From a substantive perspective, it represents a direct test of the hypothesis that recipient voting behavior depends on expected punishments and rewards.

In our case, we can rely on a simple likelihood ratio test, which is appropriate for nested models. The model presented in Table 3 assumes that the recipient conditions its vote choice on its anticipated effect on the U.S. aid disbursement decision. However, if it is the case that the recipient’s voting decision is affected by the same covariates but is not strategic, we do not need to condition its choice on the expected response of the United States. Furthermore, if this non-strategic model is more appropriate, the results that we attribute to strategic interaction could be spurious. In this alternative model of recipient voting behavior we include the same substantive regressors included in the recipient’s utilities above without conditioning their influence on U.S. behavior. This simpler model is nested within the strategic estimator as it is technically the same model with the assumption that  $p_3 = 0$ ,  $p_4 = 1$ ,  $p_5 = 0$ , and  $p_6 = 0$ .<sup>17</sup> As Clarke (2001, 727–728) notes, two models are nested if the “unrestricted” model can be reduced to the “restricted” model by imposing a set of linear restrictions. The restriction here states that the probability the United States responds with a punishment or reward is irrelevant to the recipients’ vote choices. The likelihood ratio test comparing the two models rejects the null hypothesis that the restricted model performs equally well at a high level of confidence ( $p < 0.005$ ).<sup>18</sup> Thus, we conclude that the non-strategic model is indeed misspecified, as we assumed at the outset, and that a properly specified model must include strategic interaction. Substantively, this means that U.S. aid policy has a significant effect on voting in the United Nations General Assembly.

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<sup>17</sup>There are a few other possible assumptions that would lead to the restricted model. However, they all lead to an equivalent non-strategic version of the model. The linear restrictions should be written such that the strategic logit model is reduced to a single-equation logit model that includes one of each substantive regressor and a constant.

<sup>18</sup>The log-likelihood for the strategic version of the recipient vote choice model is -4171.45, while the log-likelihood for the restricted non-strategic model is -4220.37. Since the strategic model has 8 additional parameters, the likelihood ratio test statistic is 97.83 with 8 degrees of freedom. This indicates that the strategic model outperforms the non-strategic version, as the Chi-Square distributed test statistic of 97.83 is significant at any conventional level of statistical significance. The critical value for significance at the 0.005 level is only 21.96.

## 6 Conclusions

UN voting records are a uniquely informative data source on the policy preferences of most of the world's countries on a wide range of issues, and previous scholarship has frequently treated UN voting either as a measure of preferences or as an arena for vote buying. Using a strategic statistical model that explicitly allows for the possibility that voting is strategic, we find that the U.S. policy of influencing important UNGA votes with aid disbursements has important effects on the behavior of recipient countries. A model specification test rejects the hypothesis that voting decisions are unaffected by subsequent aid disbursement strategies with a high degree of confidence ( $p=.005$ ). UN voting on issues of political significance to major aid donors is not simply a sincere expression of country preferences.

One implication of our findings is to draw into question the use of UNGA voting as an index of states' preferences (Gartzke, 2005; Russett and Oneal, 2001; Stone, 2004). Roll call voting is only likely to prove a useful measure of preferences if there is substantial variation, or contention, in voting behavior. This is why it is commonplace to discard near-unanimous votes in the literature on ideal point estimation (Poole and Rosenthal, 1991). The U.S. State Department's list of politically important votes identifies votes that are contentious, neither expressing a shallow consensus nor carrying out simple procedural matters. Such votes are especially important in the estimation of states' preferences or "ideal points," but our analysis indicates that the use of such votes as a measure of sincere preferences is problematic, because important votes are important enough to be strategic. The information about countries' preferences contained in UN voting data are accessible, however, if we use a strategic model.

A further pay-off to using a strategic model is that it becomes possible to estimate strategic quantities that are not directly observable, and this makes it possible to sort out causal explanations that would otherwise be observably equivalent. For example, we observe that democratic countries vote with the United States more often than autocratic countries on important votes. Is this because democracies have inherently aligned preferences, because democracies are more vulnerable to U.S. pressure than autocracies, or because the United States punishes and rewards democracies more frequently than autocracies? These alternative interpretations have quite different normative implications, and standard regression analysis cannot distinguish among them. Our results, how-

ever, are able to reject the first two possibilities: democracies in the developing world are in fact more critical of U.S. positions in the United Nations than autocracies, and they are less vulnerable than autocracies to U.S. influence attempts, presumably because foreign aid plays a less central role in regime survival. Nevertheless, democracies comply more with U.S. voting preferences than do autocracies, and this is attributable to the credibility problems that frustrate U.S. efforts to link aid to autocratic countries to UN voting.

While our results indicate that U.S. policy influences countries' votes, they also point to the limits of that influence. For example, our results suggest that the poorest members of the General Assembly, although they are most vulnerable to sanctions, are nevertheless the most resistant to U.S. pressure to conform. Poor countries appear to resist because they have strongly held preferences that clash with U.S. objectives. Similarly, countries that trade intensively with the United States are highly vulnerable to U.S. influence attempts, but are nevertheless more resistant to U.S. influence. In this case, however, resistance is not due to deep-seated conflict of interest, because countries that trade with the United States tend to share U.S. preferences. Instead, countries that trade intensively vote against the United States more frequently because it is too costly for the United States to link their votes in the UN to punishments or rewards.

A simple strategic model reveals layers of interaction that lie beneath the radar of conventional regression analysis. The motivation for our model was to investigate strategic voting in the context of vote buying. We posited that vote buying should create strategic interaction that might obscure the underlying relationships, and in particular, the credibility of U.S. influence attempts should play a key role in determining how effective they can be. In fact, we find that credibility varies systematically across target countries and regime types, and even shifts between the Cold War and the post-Cold War period. These variations explain much of the variation in UN voting on important issues. The resulting picture of the political economy of UN voting is considerably richer than previous analyses have found.

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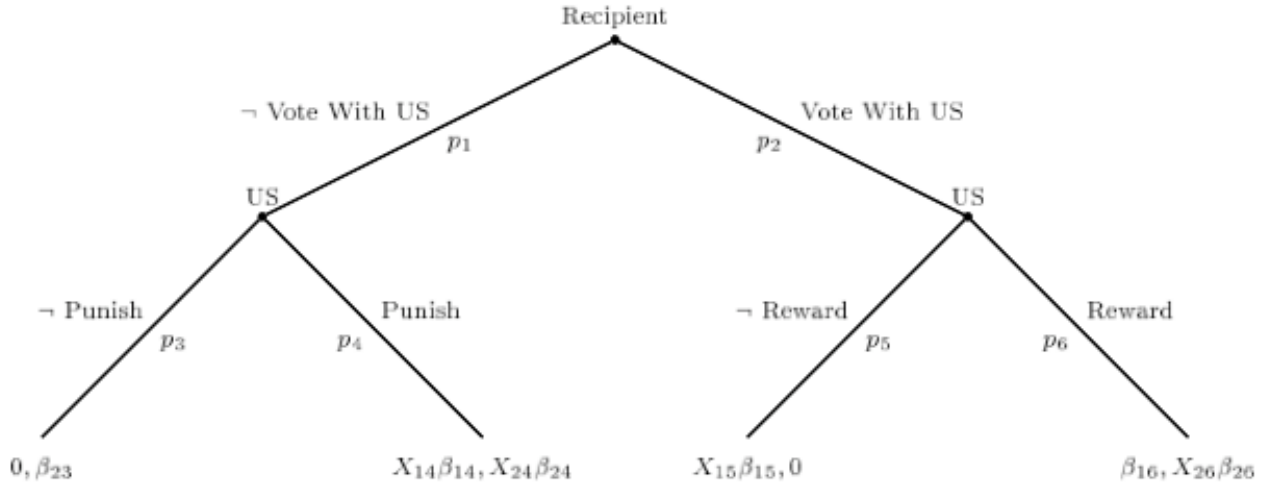


Figure 1: The Voting-Aid Game

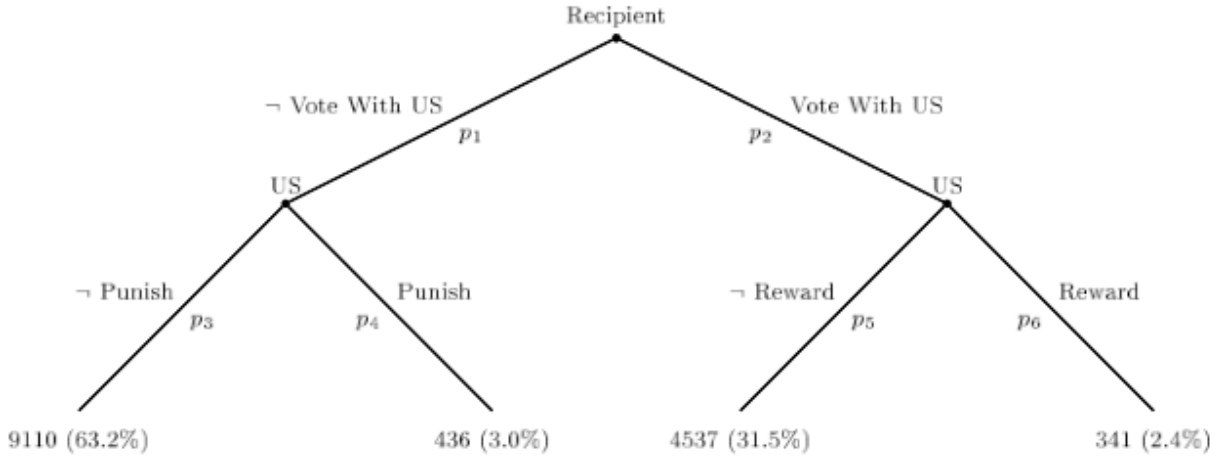


Figure 2: The Distribution of Data Across Outcomes

Table 1: Descriptive Statistics for Regressors

	Minimum	Median	Mean	Maximum	Standard Deviation
Recipient Polity	-10	-1	0.19	10	6.99
Allies	0	0	0.22	1	0.41
GDP pc	281.3	3358	4793	29170	4762.37
Trade	0	318.40	3869	259500	15051.09
Left-Wing Executive	0	0	0.31	1	0.46
Right-Wing Executive	0	0	0.18	1	0.39

Table 2: Results of Predictive Model

Variable	Estimate	Standard Error
Constant	9.186	1.407
$Commitments_t$	0.387	0.007
$Aid_{t-1}$	0.332	0.010

R-Squared Within Group: 0.53  
R-Squared Between Group: 0.99  
F-test for Fixed Effects:  $P > 0.006$

Table 3: Utilities for Statistical Strategic Model

	$U_R(Punish)$	$U_R(-Reward)$	$U_R(Reward)$	$U_{U.S.}(-Punish)$	$U_{U.S.}(Punish)$	$U_{U.S.}(Reward)$
Constant	<b>110.53</b> (22.59)	<b>0.32</b> (0.07)		<b>4.94</b> (0.24)		<b>-3.20</b> (0.13)
Recipient Polity	<b>2.75</b> (0.64)		<b>-5.63</b> (1.02)		<b>0.05</b> (0.01)	<b>0.13</b> (0.01)
Allies	<b>-30.69</b> (5.02)		-2.99 (5.79)		0.19 (0.13)	-0.17 (0.14)
Recipient GDP	<b>9.87</b> (1.90)		<b>4.42</b> (0.79)		-0.09 (0.16)	<b>0.06</b> (0.01)
Trade	<b>-15.86</b> (5.04)		11.46 (6.39)		<b>-0.05</b> (0.02)	<b>-0.11</b> (0.03)
Left-Wing Executive	<b>-9.85</b> (3.26)		<b>14.76</b> (4.59)		-0.07 (0.11)	<b>-0.44</b> (0.15)
Right-Wing Executive	6.96 (10.41)		<b>68.74</b> (8.85)		<b>-0.71</b> (0.15)	-0.28 (0.16)
U.S. Votes No	<b>-109.71</b> (21.39)		<b>-6.82</b> (1.25)		<b>0.22</b> (0.02)	<b>-0.16</b> (0.01)

Bootstrapped Standard Errors in Parentheses  
 Number of Observations 1437  
 Bold Indicates Significance at the .05 Level  
 Log-Likelihood 6923.28  
 Percent Correctly Predicted: 84.9%  
 Modal Percent Correctly Predicted: 63.2%

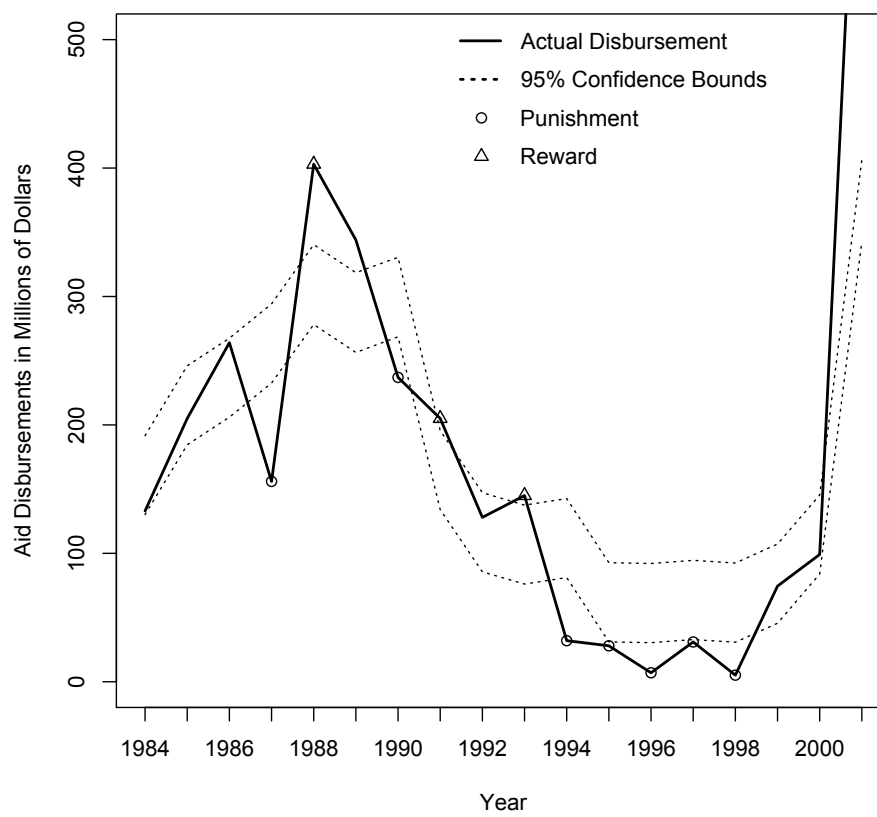
Table 4: Substantive Effects on Rewards and Punishments

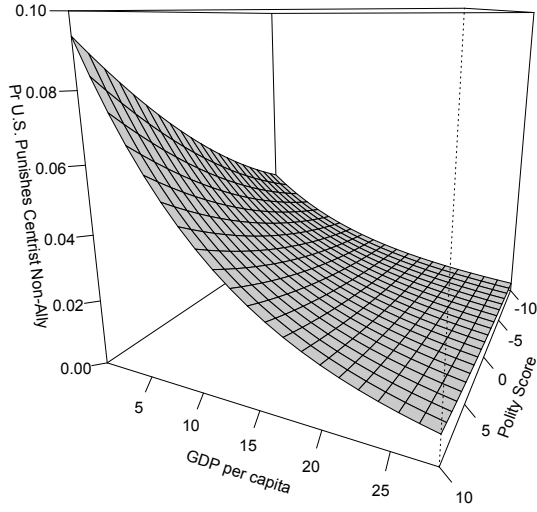
	$\Pr(Punish)$	Change in Pr	% Change in Pr	$\Pr(Reward)$	Change in Pr	% Change in Pr
Median Values	0.042	NA		0.008	NA	
Polity=-9	0.028	-0.014	-33%	0.003	-0.005	-63%
Polity=9	0.071	+0.029	+69%	0.029	+0.021	+263%
Recipient GDP=1,000	0.051	+0.009	+21%	0.007	-0.001	-13%
Recipient GDP=12,000	0.020	-0.022	-52%	0.013	+0.005	+63%
Trade=15 million	0.042	+0.000	+0%	0.008	+0.000	+0%
Trade=10 billion	0.040	-0.002	-5%	0.007	-0.001	-13%
Left-Wing	0.039	-0.003	-7%	0.005	-0.003	-38%
Right-Wing	0.021	-0.021	-50%			
Yes Vote	0.005	-0.037	-88%	0.042	+0.035	+438%

Table 5: Substantive Effects on Votes against the U.S.

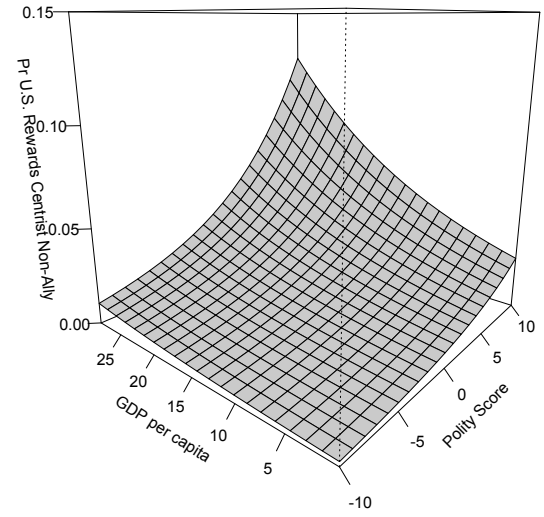
	$\Pr(\textit{Vote Against U.S.})$	Change in Pr	% Change in Pr
Median Values	0.880	NA	
Polity=-9	0.942	+0.062	+7%
Polity=9	0.821	-0.059	-7%
Alliance=1	0.960	+0.080	+9%
GDP=1,000	0.946	+0.066	+8%
GDP=12,000	0.695	-0.185	-21%
Trade=15 million	0.878	-0.002	-0%
Trade=10 billion	0.928	+0.048	+5%
Left-Wing	0.906	+0.026	+3%
Right-Wing	0.892	+0.012	+1%
Yes Vote	0.031	-0.849	-96%

Figure 3: Aid Flows to Pakistan, 1984–2001

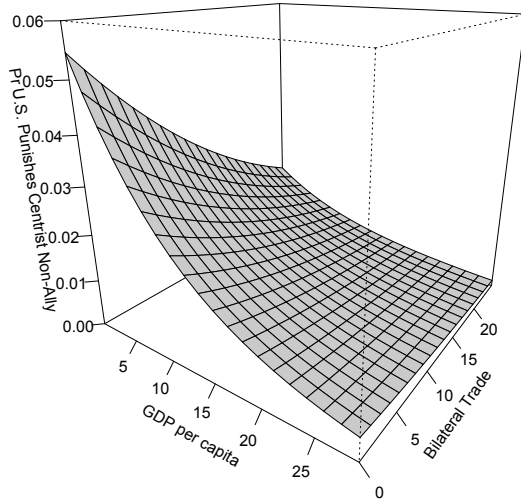




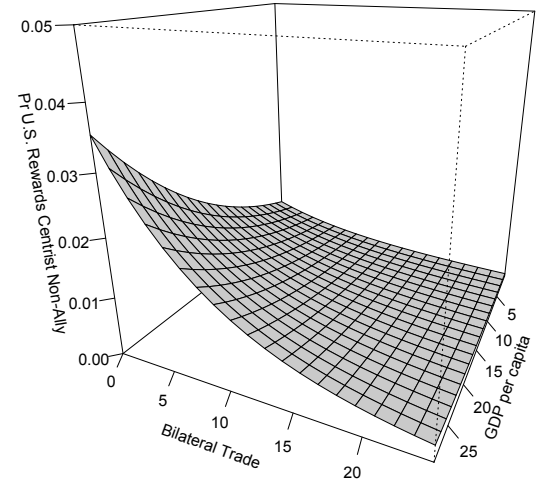
(a) Regime Type, Development, and Punishment



(b) Regime Type, Development, and Reward

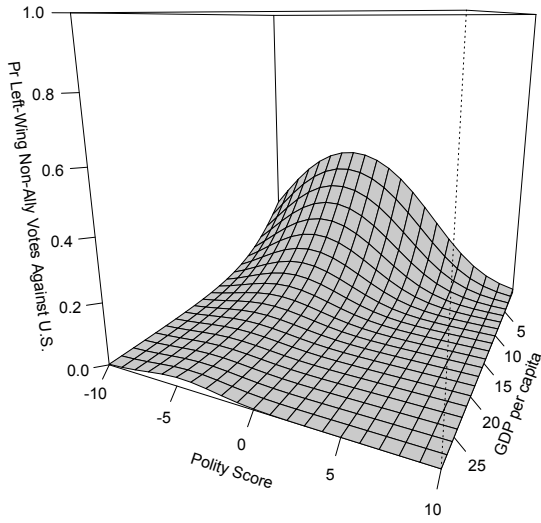


(c) Trade, Development, and Punishment

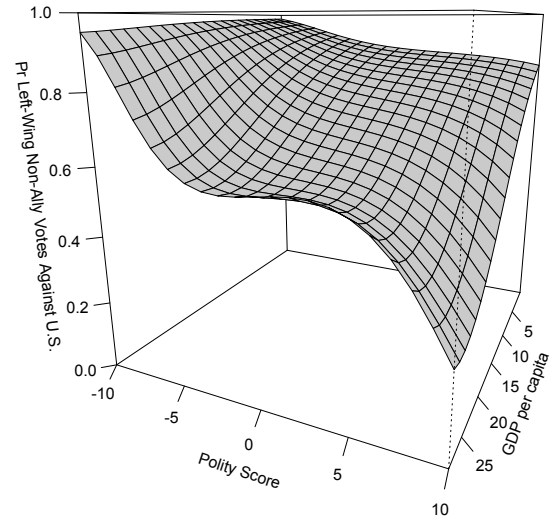


(d) Trade, Development, and Reward

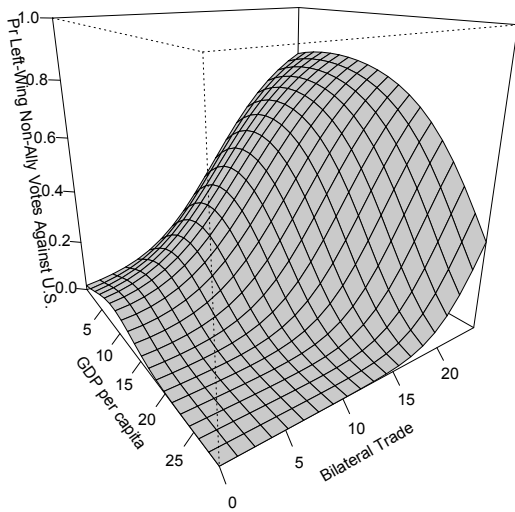
Figure 4: U.S. Behavior



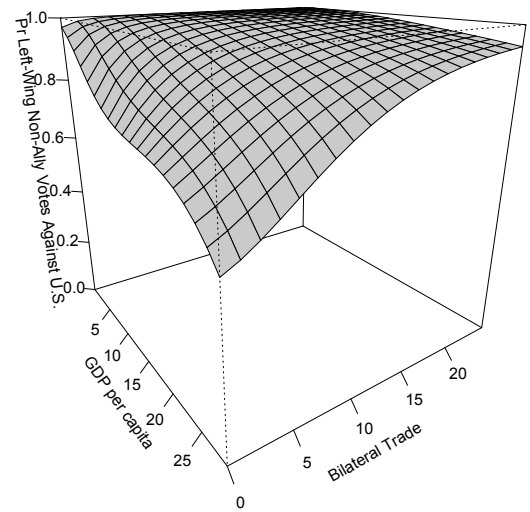
(a) Regime Type, Development, and “Yes” Votes



(b) Regime Type, Development, and “No” Votes



(c) Development, Trade, and “Yes” Votes



(d) Development, Trade, and “No” Votes

Figure 5: Recipient Voting Behavior



Table 6: Cold War and Post Cold War Effects

Variable	$U_R(Punish)$			$U_R(Reward)$			$U_{U.S.}(Punish)$			$U_{U.S.}(Reward)$		
	Cold War	Post-Cold War	Pooled	Cold War	Post-Cold War	Pooled	Cold War	Post-Cold War	Pooled	Cold War	Post-Cold War	Pooled
Recipient Polity	-0.18 (2.06)	<b>2.85</b> (0.63)	<b>2.75</b> (0.64)	-9.33 (14.12)	<b>-4.41</b> (0.97)	<b>-5.63</b> (1.02)	<b>0.06</b> (0.01)	<b>0.07</b> (0.01)	<b>0.05</b> (0.01)	<b>0.16</b> (0.02)	<b>0.13</b> (0.01)	<b>0.13</b> (0.01)
Allies	-6.40 (12.83)	<b>-33.37</b> (5.91)	<b>-30.69</b> (5.02)	-11.51 (28.96)	-2.48 (5.47)	-2.99 (5.79)	<b>-0.66</b> (0.30)	<b>0.51</b> (0.15)	0.19 (0.13)	<b>-0.61</b> (0.28)	-0.13 (0.17)	-0.17 (0.14)
Recipient GDP	<b>14.52</b> (5.67)	<b>10.26</b> (1.72)	<b>9.87</b> (1.90)	9.64 (8.69)	<b>3.69</b> (0.72)	<b>4.42</b> (0.79)	0.02 (0.02)	<b>-0.19</b> (0.02)	-0.09 (0.16)	<b>0.07</b> (0.02)	<b>0.06</b> (0.01)	<b>0.06</b> (0.01)
Trade	-50.77 (40.62)	<b>-16.59</b> (4.63)	<b>-15.86</b> (5.04)	69.21 (42.84)	<b>10.60</b> (5.27)	11.46 (6.39)	<b>0.19</b> (0.08)	-0.03 (0.02)	<b>-0.05</b> (0.02)	-0.10 (0.14)	<b>-0.08</b> (0.03)	<b>-0.11</b> (0.03)
Left-Wing Executive	0.27 (18.42)	<b>-10.16</b> (2.98)	<b>-9.85</b> (3.26)	-10.32 (44.05)	<b>13.92</b> (3.58)	<b>14.76</b> (4.59)	<b>0.66</b> (0.20)	<b>-0.57</b> (0.15)	<b>-0.07</b> (0.11)	-0.27 (0.26)	<b>-0.60</b> (0.18)	<b>-0.44</b> (0.15)
Right-Wing Executive	4.87 (24.80)	9.43 (9.26)	6.96 (10.41)	<b>158.02</b> (34.81)	<b>60.25</b> (8.31)	<b>68.74</b> (8.85)	<b>-1.49</b> (0.52)	<b>-0.52</b> (0.16)	<b>-0.71</b> (0.15)	<b>-0.14</b> (0.30)	<b>-0.44</b> (0.18)	-0.28 (0.16)
U.S. Votes No	<b>-189.79</b> (23.81)	<b>-90.94</b> (19.49)	<b>-109.71</b> (21.39)	<b>-5.93</b> (1.11)	<b>-7.48</b> (1.23)	<b>-6.82</b> (1.25)	<b>0.18</b> (0.04)	<b>0.24</b> (0.04)	<b>0.22</b> (0.02)	<b>-0.20</b> (0.02)	<b>-0.15</b> (0.01)	<b>-0.16</b> (0.01)
Constant	<b>195.03</b> (26.27)	<b>91.19</b> (20.46)	<b>110.53</b> (22.59)	<b>-0.59</b> (0.11)	<b>-0.28</b> (0.06)	<b>-0.32</b> (0.07)	<b>-0.51</b> (0.04)	<b>-0.48</b> (0.04)	<b>-0.49</b> (0.02)	<b>-0.27</b> (0.03)	<b>-0.34</b> (0.01)	<b>-0.32</b> (0.01)
N =	4966	9371	14337	4966	9371	14337	4966	9371	14337	4966	9371	14337

Bootstrapped Standard Errors in Parentheses

Bold Indicates Significance at the .05 Level