Tests of Vote-Buyer Theories of Coalition Formation in Legislatures

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Formal theories of "vote-buying" aim to explain legislative coalition building, and lobbying. While anecdotal evidence suggests that something approximating vote-buying occurs, these theories have not been subjected to substantial empirical tests. Using roll-call data from all House bills subject to votes on final passage in the House of Representatives in the 103rd Congress, this study tests the coalition size implications of vote-buying theories. The primary method of analysis is ordinary least squares. Variation in coalition size, as represented by the percentage of the chamber voting yea, is accounted for in a manner consistent with vote buying theories. By making an assumption about what kinds of legislation are likely to attract one, or multiple, vote-buyers, we are able to operationalize the number of vote-buyers lobbying on a given bill. Other theoretical predictors of coalition size are also entered into the analysis, and the results offer support for the family of theories. When controlling for different parliamentary procedures, bills that are likely to attract two vote-buyers generate coalitions that are approximately 6 percent larger than single vote-buyer bills.

A recent game-theoretic literature known as "vote-buying" or "favor-trading" theories (Groseclose 1996; Groseclose and Snyder 1996; Snyder 1991) has been developed that examines the strategic interaction between coalition builders and legislators. The analytical elegance of these theories has led to their incorporation in studies ranging from legislative organization (Diermeier and Myerson 1999), to political history (Jenkins and Sala 1998), to theories of institutions (Diermeier and Krehbiel 2003) to managerial strategy (Baron 1999, 2001). While the vote-buying theories have gained significant acceptance, surprisingly little empirical analysis has been conducted in order to test their implications.[1]

The primary reason no such testing has occurred is, that the theories are very difficult to test. Many of the actions taken by the actors in these models are unobservable; and, as will be discussed later, several of the theoretical parameters in the models are difficult to measure accurately. While these measurement complications make direct tests of vote-buying models exceedingly difficult, if we are willing to accept certain motivating assumptions about the data that we use, various indirect tests can be conducted which aim to detect more subtle implications of the theories. The analysis conducted in this study relies on certain specific assumptions about the sample that allow such tests to be undertaken. While it is admittedly undesirable to rely on strong assumptions to motivate data analysis, this study should be viewed as a first step toward investigating whether or not the implications of vote-buying models have empirical veracity. To the extent that they do, further scholarship should aim at developing more refined tests of the models beyond what is possible here given the relatively crude sample employed.

Taken together, the vote-buying theories treat coalition builders as calculating strategists, who entice legislators with favors in order to ensure the passage of their desired measures. The core features of these theories are that first, in most cases, if coalition leaders buy votes, the first mover will target those legislators who are indifferent, or nearly indifferent to the proposed policy and the status quo. This is true because legislators who are nearly indifferent to the policies are cheaper to buy than those who are strong supporters or opponents of a measure. Second, the theories also predict that a coalition leader's decision to engage in vote-buying depends on the level of resources at his disposal. Third, when compared, various versions of the vote-buying theories offer different predictions about coalition sizes depending on their assumptions about the number of vote-buyers (one vs. two).

On the first two points, anecdotal evidence leads us to believe that something approximating vote-buying occurs in the legislature. A clear example of a pivotal member being bought occurred during the 103rd Congress when the Clinton Administration persuaded Marjorie Margolies-Mezvinsky (D-Pennsylvania) to vote in favor of the President's budget package despite significant constituency disapproval of the measure. Margolies-Mezvinsky later noted that her vote had been conditional on Clinton's commitment to convene a conference on cutting entitlement spending in her district (Cloud 1993: 2125); Clinton used a favor to buy the pivotal voter.

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[1] Scholars have conducted some case-based empirical studies of vote-buying theories including Groseclose's (1996) study of the Clean Air Act.

Cases also exist in which lobbyists seem to be engaging in a political calculation to determine whether it is in their interests to lobby, given their level of resources. For example, prior to the passage of the Family and Medical Leave Act in 1993, it was observed that several business groups that had lobbied hard against such actions in previous congresses were surprisingly silent, "recognizing that Clinton's election changed the political arithmetic on the issue", and that they no longer had sufficient resources to persuade legislators. Tom Sawyer (D-Ohio) claimed that the lobbies were "lying low" and that it was "a night and day difference" in comparison to previous passage attempts (Galvin 1993: 222).[2]

While these two case studies support some of the predictions of the vote-buying theories, the last point, dealing with coalition sizes, requires large sample empirical analysis to evaluate its plausibility. This study provides such empirical support by analyzing coalition sizes on final passage votes for House bills in the 103rd Congress. Section I discusses two variations on the vote-buying theory as developed in articles by Groseclose and Snyder (1996) and derives testable hypotheses about coalition sizes. Section II discusses the data used to test these hypotheses, and Section III presents analysis and implications of the empirical results. Section IV concludes with caveats, implications, and suggestions for future research into vote-buying theories.

I. THEORIES

Both the Snyder and the Groseclose-Snyder models assume that legislators have preferences that are single peaked over a unidimensional policy space, meaning that legislators have an ideal policy, and that they are less happy the farther a given policy lies from their ideal policy. A key assumption of these models is that coalition leaders desire to pass bills while expending as few resources as possible. Despite some similarities in set-up, these two models differ in the sense that the Snyder model considers only one lobbyist, while the Groseclose-Snyder model considers coalition formation with two competing lobbyists. An important point to note is that the term *lobbyist* has a very specific definition in the context of their model. In the context of vote-buying theories, a lobbyist refers to any coalition builder, either within (e.g., a party leader) or outside of (e.g., an organized interest group) the legislative chamber, who has distributive benefits at his disposal with which to persuade legislators to vote in accordance with his preferences. Hence, other activities conventionally attributed to advocates, such as information provision and legislative support, are not considered in these models and will not be addressed here. While such a definition is obviously a very narrow treatment of the term *lobbyist*, I will continue to use it in the text to stay consistent with Groseclose and Snyder's terminology.

Both models assume complete and perfect information on the part of the players; thus, all members and lobbyists

know exactly where everyone else lies in the policy space and a lobbyist knows if his offer for a vote will be accepted. Therefore, a lobbyist knows exactly whom he must "bribe" for a given bill to pass. Similar to the discussion of the meaning of the term lobbyist, it is important to note that referring to votes being bought by coalition leaders is not to imply that legislators are actually being bribed, and cash-in-hand transfers are occurring for votes. Buying should be interpreted, instead, as the general concept of exchanging political capital such as campaign contributions or legislative favors such as pork-barrel legislation or attractive committee assignments for votes (Groseclose 1995: 3). While such a definition is a very narrow treatment of the term *bribe*, similar to the term lobbyist, I will continue to employ it in the text to stay consistent with Groseclose and Snyder's terminology.

Given the existence of perfect information and only one vote-buyer, the result of the Snyder model is straightforward: if vote-buying occurs, only minimal winning coalitions ensue. After all, (assuming a simple majority is required for passage) if at least 50 percent +1 are predisposed to vote for the bill, then there is no reason to engage in vote buying because the bill will pass without expending any resources. Alternatively, if less than 50 percent +1 are predisposed to vote for the bill, then the lobbyist will buy only the votes of the least-expensive members whose votes are needed to attain a minimal-winning majority. Buying greater than a minimal-winning coalition wastes resources.[3]

While the Groseclose-Snyder model also assumes complete information, the presence of competing vote-buyers complicates matters. In this model, one lobbyist (B) favors the status quo (s), while the other (A) favors the new policy (x). The lobbyists move sequentially, with vote-buyer A moving first to bribe legislators, and vote-buyer B moving second to buy back legislators to maintain the status quo. Similar to the Snyder model, legislators will choose policy x over the status quo if they receive greater utility from voting for the new policy (plus some favor from lobbyist A) than from voting for the status quo (plus some favor from lobbyist B). An important feature of the model is that vote-buyer who favors the new policy, knows precisely the level of resources available to the status quo vote-buyer.

First, in cases where B has insufficient resources to buy back any votes bought by vote-buyer A, the result is identical to the Snyder model: if the ideological predisposition of the chamber favors the new policy, it passes without any bribes taking place. Conversely, if the ideological predisposition of the floor favors the status quo, A bribes just enough legislators to ensure the passage of the new policy, leading to a minimal winning coalition. Second, in cases where B

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[2] This example was suggested by Tim Groseclose.

[3] It should be noted that Snyder (1991) considers an extension of the one-sided vote-buying model with multiple lobbyists who engage in collusion to lower their total bribes to legislators. An ideal lobbyist policy emerges from some unspecified bargaining process; because only one policy is presented to the legislators, this extension can still be viewed as a de facto case of one-sided vote-buying.

possesses sufficient resources to buy back some legislators bought by A, A pays out enough bribes so that even if vote-buyer B were to exhaust all of his resources, he would be unable to form a blocking coalition to prevent the new policy's passage. In these cases, A will, in many situations, even bribe those legislators who are already ideologically predisposed toward the new policy to ensure that they are too expensive for B to buy back. Finally, while not explored formally by the authors, there must also be a third case: that being when vote-buyer A has meager resources in comparison to vote-buyer B, and is unable to buy a sufficient number of votes given B's resources to implement a new policy. In this case, we would expect to see the new policy proposal failing on the final roll call vote, despite the fact that there were two competing vote-buyers, one of whom favored a change in the status quo.[4]

A central result of the Groseclose-Snyder model is that in cases where both vote-buyers have a significant level of resources, and more specifically, when B has sufficient resources to buy back votes, greater than minimal winning coalitions will occur. In fact, cases may occur in which A constructs a winning coalition that comprises 100 percent of the chamber if forming such a coalition exhausts the resources of B. Hence contrary to conventional wisdom, the presence of two vigorous opposing lobbyists can sometimes lead to extremely large voting coalitions, rather than minimal majorities that might follow from the two sides picking off each others' supporters from voting blocks.[5]

Comparing the results of these two papers, it is evident that a straightforward testable implication of these models is that the average coalition size should be larger for those bills that are subject to two-sided vote-buying rather than one-sided vote-buying. While it is true that the Groseclose-Snyder model allows for results identical to those of the Snyder model in certain cases, one would expect that there are many other cases for which two-sided vote-buying should lead to instances of supermajorities. Hence, by conducting empirical analysis on bills from the 103rd Congress, one might think that if something akin to vote-buying were occurring, it should manifest itself in the data in a manner consistent with the implication posited here.

II. MEASURES AND HYPOTHESES

To test whether the hypothesized relationship between Groseclose-Snyder model and the Snyder model holds, ordinary least-squares analysis is used to discern whether the average coalition size on a given bill in the U.S. House is larger in cases of two-sided vote-buying than one-sided vote-buying. The sample considered is a set of roll call votes of all House bills that were subject to vote on final passage in the 103rd Congress.[6] The sample is not truncated by the elimination of near-unanimous votes because the theories under consideration allow for large supermajorities.[7] The dependent variable in this analysis is the coalition size of those voting "yea".[8] Only bills that were voted on for final passage were considered, and no bills were omitted from the sample.[9]

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[4] Such a case would be tantamount to the proponents of the new measure engaging in position taking (Mayhew 1974), in which they proposed legislation and ensured that it made its way to the floor for a final passage vote, knowing that it would fail, but successfully getting a recorded roll call. Given the thousands of bills that fall by the wayside on the path from introduction to final passage vote in a given Congress, it must be true that (at least some) legislators are expending nontrivial amounts of effort to ensure that any bill eventually comes up for a vote on final passage.

[5] An alternative theory of coalition formation developed by King and Zeckhauser (2003) argues that coalition leaders hold options on certain members' votes. These "hip-pocket" votes are only called into play when their casting (one way or the other) will be crucial to the outcome. In such a scenario one would expect to see near-minimal winning coalitions forming in the presence to two competing lobbyists.

[6] Concerns might be raised that focusing only on House bills subject to vote on final passage might cause selection bias issues in that the sample of bills that finally reaches the floor is not necessarily random. While such data problems may generally occur in cross-sectional studies of roll-call votes, this concern is less problematic here. The theories being tested focus on one-shot coalition-builder-legislator interaction concerning one vote on one bill, rather than interaction on many votes for the same bill (e.g., committee votes, rules votes, amendment votes, etc.). Simply put, the Groseclose and Snyder vote-buying models only obviously address bills that are voted on for final passage. Hence in our analysis we are currently confining our attention to the entire population of votes that should be relevant to the theories—those votes on final passage—and as a result, our estimates should not suffer from the inconsistency or bias that conventionally accompanies nonrandom sample selection.

[7] As will be discussed below and presented in Appendix B, replicating analysis on a truncated sample where bills with coalitions greater than 95 percent were excluded from consideration yields substantively similar results.

[8] An alternative way to code the dependent variable is to measure the size of the winning coalition, even in cases where the winning coalition was a blocking coalition that led to the bill's defeat. Given the small number of bills that failed on final passage votes, such a coding scheme yields a sample nearly identical to the one being analyzed; and analysis on this recoded sample produces results that are substantively similar to the results reported here.

[9] Only votes on bills subject to final passage were considered in order to eliminate instances of "hurrah" legislation that is commonly associated with resolutions, and to eliminate amendment votes that do not fit well into the game-theoretic framework. In thinking about the theories, one could deduce that actors would have reason to vote strategically and not reveal their true preferences until the final round (e.g., the final vote on the bill). Therefore, one might conclude that vote-buyers, knowing that true preference revelation will not occur until the final vote, will opt to conserve their resources to buy votes in the final stage rather than in the amendment stage. Finally, from a practical standpoint, it is obvious that many amendments are proposed solely for position-taking, with little chance of actual passage. For example, the Crane amendment to HR 2518 sought to cut $292 million from the $292.64 million budget of the corporation for public broadcasting. The amendment, obviously a position-taking measure, failed miserably, attracting less than 15 percent of the chamber's support in the Committee of the Whole. Inclusion of such amendments would obviously serve to bias the results, but there is no definitive criterion that can be applied to the body of amendments to deal with such cases in a non-arbitrary manner. For this reason, only final votes were included; unlike amendments, it seems most likely that all of the final votes dealt with some form of substantive legislation that had a non-trivial chance of being passed.

While measuring the dependent variable is straightforward, several problems arise in measuring the number of vote-buyers. First, it is difficult to observe when vote-buying occurs. Vote-buying in the sense of cash-in-hand-transfers for votes is illegal, and therefore not highly publicized nor open to measurement.[10] Along the same lines, while anecdotes of chits being traded for key votes make it into the press, it is obvious that more times than not, deals are made without drawing the attention of the media. Therefore the measure of vote-buyers that is adopted is indirect. Considering the legislation in the sample set, a protocol was created for coding bills as cases of one-sided or two-sided vote-buying. Embracing an argument employed in other theoretical studies of coalition formation (Baron 1999, 2001) that cases of client politics tend to attract one vote-buyer, while non-client politics tend to attract multiple vote-buyers, bills were coded 1 for two-sided vote-buying if they could be classified as non-client-politics legislation and 0 otherwise.[11]

More specifically, the protocol classified bills as cases of one-sided vote-buying if they satisfied one (or more) of the following conditions: (a) bill provided benefits for particular geographic regions, or specific subgroups of the electorate, (b) bill provided for the naming, or renaming, of structures, parks, or geographic regions or agencies, or provided for changes in the U.S. Code to allow for such renamings, (c) bill provided for expansion to an existing bureaucratic agency, (d) bill provided for non-controversial legislation that did not fall under the aforementioned categories (where "non-controversial" was defined as those issues that did not attract significant media attention during the 103rd Congress).[12] Bills were classified as cases of two-sided vote-buying if they fell into one (or more) of the following categories: a) bill provided for any form of regulatory action that would generate obvious "winners" and "losers" in the political arena (consistent with Moe (1989)), b) bill provided for major bureaucratic appropriations such as cabinet departments, c) bill was an omnibus bill and/or provided for block grants to several geographic regions for discretionary use, d) bill was an item of budgetary legislation, or dealt with the budgetary process, e) bill dealt with controversial legislation (where "controversial" was defined as those issues that attracted significant media attention during the 103rd Congress).

Using this coding protocol, the sample of House bills was broken into a group of 39 items of client-politics legislation and 124 items of non-client-politics legislation. Of the 39 items of client legislation, 18 bills provided for particularistic benefits to certain geographic regions or electoral groups, 7 provided for renamings, 7 provided for bureaucratic expansions, and 7 were generally noncontroversial items of legislation. Of the 124 non-client bills, 43 were regulatory measures, 50 bills were related to appropriations, 8 were omnibus/block grant bills, 6 were budgetary items, and 17 were relatively controversial bills such as handgun legislation. Appendix A provides a list of the first 15 bills in the sample being considered, as well as their classifications as client or nonclient legislation.

Having created a proxy for vote-buyers, one might seek to control for several other factors that might facilitate supermajorities. For this purpose, four additional variables are considered in the analysis. The first variable controls for whether or not the bill was considered under suspension of the rules. Including the suspension variable is relevant because bills called up under suspension of the rules require a two-thirds supermajority for passage, instead of the conventional simple majority, which effectively increases the threshold for a minimal-wining coalition.

Three other control variables are included to test competing theories of coalition formation. The first variable is whether or not the bill was considered under an open rule. Other theories of coalition formation (e.g., Baron and Ferejohn (1989)) have derived results showing that open amendment procedures lead to greater-than-minimal winning coalitions. The existence of such procedures decreases the incentive for members to agree to a given bill because of the possibility of a member's future recognition and being granted proposal power. As a result, a coalition builder must expand the size of his coalition by offering legislative provisions to those members who might otherwise hold up the process.[13]

The second control variable included in the analysis is a measure of plausible presidential veto threats. Consistent with Diermeier and Myerson (1999) and Krehbiel (1998), one should expect that the prospect of a president's veto would cause coalition leaders to build supermajorities in the hopes of overriding any possible executive action. Consistent with Matthews (1989), however, the simple expression of a president's displeasure for a bill is not sufficient information to determine whether there is a credible veto threat. Instead, one must be able to determine, first, if a president has expressed disapproval for legislation, as well as how far the legislation in question is from his ideal policy to determine whether any verbalized veto threat is credible.[14] The proxy for such credible veto threats is the distance in DW-NOMINATE scores (McCarty, Poole, and Rosenthal 1997) between President Clinton's ideal point in the 103rd Congress and the coordinate in DW-NOMINATE space of the

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[10] For example, in California, vote-trading is a felony punishable by up to four years in prison. A scandal arose in July, 1997, when a legislator was tape-recorded in a committee hearing admitting to trading his vote on tobacco legislation for another assemblyman's vote on a pet project. The tape that recorded the comments was confiscated (and was later found to be blank), and the California Attorney General investigated the matter. The charges were eventually dropped. The author thanks Justin Adams for noting this example.

[11] The notion of "client politics" is borrowed from Wilson's (1980) classification of political competition, and is loosely defined as that legislation for which the benefits from implementation are highly concentrated while the costs associated with it are widely dispersed.

[12] More specifically, legislation was classified as "noncontroversial" if it did not fall into the aforementioned categories, and was also not identified as a House "Key Vote" by CQ Weekly Report for the 103rd Congress.

[13] Consistent with Krehbiel (1991) and Sinclair (2002) a bill was coded as open if it received an open or modified open rule.

[14] The author thanks a referee for this insight.

roll call in question, multiplied by an indicator variable that takes a value of 1 if President Clinton took a "nay" position on a given roll call and 0 otherwise.[15] More formally, the veto threat variable is formulated as follows:

Veto threat = I × |(Clinton's ideal policy) − (bill coordinate)|

$$\text{where } I = \begin{cases} 1 \text{ if President Clinton took a "nay" position on} \\ \quad \text{the roll call} \\ 0 \text{ otherwise.} \end{cases}$$

Hence, if Clinton did not express disapproval for a measure publicly, the veto threat variable equals 0. Conversely, if Clinton did express disapproval for a measure, and the policy proposal was very far from Clinton in DW-NOMINATE space, the veto threat variable is large and positive value. Such an operationalization effectively captures whether Clinton issued a veto threat over a given bill, as well as how credible that threat was.

Finally, a trend-variable in the form of the Congress quartile in which the roll call occurred is included in the analysis. Another alternative theory to the vote-buying models is that uncertainty with respect to the preferences of the entire chamber is the driving force behind the presence of super-majorities (Koehler 1975, Riker 1962). Coalition leaders, uncertain of members' true preferences, might pad their coalitions with extra members to ensure that last minute defections do not lead to a measure's defeat. Groseclose and Snyder (1996) suggest that a way to test this competing conjecture is to examine whether or not a negative relationship exists between legislative turnover and average coalition sizes between Congresses. "Assuming less is known about the preferences of new legislators than about veteran members, a large amount of turnover should lead vote buyers to pad their majorities more heavily" (Groseclose and Snyder 1996: 312).

An alternative method that is consistent with Groseclose and Snyder's suggestion but is feasible in same-Congress analysis is to assume that the preferences of members are better known at the end of a Congress than at the beginning. If so, it follows that coalition size should be negatively related to time elapsed in a Congress. Incorporating a simple trend variable serves to test for this hypothesized relationship.[16] Table 1 summarizes the variables and presents descriptive statistics.

Ordinary least squares analysis with Huber-White standard errors is conducted on variations of the following model[17]:

$$Y = \alpha + \beta_1 X_1 + \gamma Z + \epsilon \tag{1}$$

Y = Percent of Chamber Voting "Yea"

$$X_1 = \begin{cases} 1 \text{ if Case of Nonclient Politics} \\ 0 \text{ otherwise} \end{cases}$$

Z = Matrix of Control Variables.

Using the above variables, the vote-buying theories suggest that the coefficient on X_1, β_1, would be positive. In other words, the average coalition size should be greater in cases when there exists two-sided vote-buying (Groseclose-Snyder bills have larger coalitions than Snyder bills).

The following hypotheses relate to the control variables. First, the coefficient on the suspension of the rules variable should be positive. Specifically, it should be approximately 17—the difference between two-thirds and one-half. Similarly, the coefficient on the open rules variable should also be positive. Consistent with alternative theories of coalition formation we would expect open amendment procedures to encourage large coalitions. Third, we should also expect that the coefficient on the veto threat variable would also be positive, indicating that veto threats increase coalition size, as coalition leaders seek to ensure that they can overcome the presidential hurdle (Diermeier and Myerson 1999) and/or maintain the veto pivot in their coalition (Krehbiel 1996). Finally, we should expect that the coefficient on the congressional quartile variable would be negative, indicating that the longer a Congress continues in length, the smaller the coalition sizes that should emerge, which would be consistent with arguments that uncertainty over the chambers' preferences drives variations in coalition size.

III. FINDINGS

Results of the regressions of variations of model 1 are presented in Table 2. The first model is a simple estimation of coalition size as a function of the number of vote-buyers. Models 2-5 incorporate additional control variables. Because all hypotheses tested yield signed predictions, a one-tailed test of significance is appropriate.

Results for model (1) show that the coefficient for *nonclient politics* is of the hypothesized sign, but statistically insignificant when not controlling for other factors. This is not particularly surprising considering that the model is most likely suffering from omitted variable bias. However, as is evident in model (2), when one controls for whether the bill was called up under suspension of the rules, the

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[15] President Clinton's positions on the bills sampled was determined by consulting the *Congressional Quarterly Washington Alert* database.

[16] It is questionable whether it is appropriate to include a variable to test a multidimensional theory (divide-the-dollar) or a dynamic theory (evolution of coalition builder knowledge about legislator preferences) while at the same time testing a theory whose dimensionality is confined to R^1 and is static by assumption. However, inclusion of such variables will nonetheless control for other tendencies underlying amendment procedures and coalition builders that might serve to affect coalition size, regardless of the number of vote-buyers.

[17] Huber-White/robust standard errors are employed to account for the heteroskedasticity that commonly accompanies similar cross-sectional

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bill data. While the truncation of the dependent variable at 100 percent makes ordinary least-squares analysis somewhat inappropriate, the theoretical model postulates no specific distribution for the error structure that might be employed for empirical analysis. To address these issues, this analysis was replicated with tobit regression analysis, and the results produced are not substantively different from the results reported here. Similarly, the results reported here do not substantively differ from those that emerge when conducting OLS without using Huber-White standard errors.

≡ TABLE 1
DESCRIPTIVE STATISTICS OF INDEPENDENT VARIABLES

Variable	Description	Mean	Standard Deviation	Min.	Max.
Dependent Variable					
Coalition Size	Percent of Chamber voting yes	80.07	16.51	35.58	100.00
Independent Variables					
Two-sided Vote-Buyers	Coded "1" for non-client politics, and "0" otherwise	0.76	0.43	0.00	1.00
Open Rule	Coded "1" if bill was subject to open amendment procedure on floor, "0" otherwise	0.29	0.45	0.00	1.00
Suspension of Rules	Coded "1" if bill was called up under suspension of the rules, "0" otherwise	0.42	0.49	0.00	1.00
Veto Threat	Dummy variable coded "1" if President took a position of "nay" on vote, multiplied with the DW-NOMINATE Distance between Clinton's ideal point and the vote coordinate	0.02	0.13	0.000	1.307
Congress Quartile	Quartile in 103rd Congress in which the roll call vote on the bill occurred	2.34	1.09	1.00	4.00

N = 163

significance level of the coefficient for *non-client politics* increases to the 95 percent level, still maintaining the hypothesized positive sign. Further inclusion of additional control variables only serve to increase the significance level of the coefficient for *non-client politics*, until model (5), when all controls are accounted for, and the coefficient for *non-client politics* is still positive and statistically significant at the 95 percent level. Such results are encouraging in supporting the hypothesis that cases of two-sided vote-buying yield larger coalitions than cases of one-sided vote-buying. Specifically, when controlling for whether a bill was considered under suspension of the rules, cases of two-sided vote-buying should result in coalitions that are between 5.5 and 6.4 percent larger than legislative coalitions following from one-sided vote-buying.[18]

The control variable coefficients also yield some interesting insights. First, with regard to suspension of the rules, it is evident that the coefficient for suspension of the rules is significantly positive in all of the models. Furthermore, in models 2-5, controlling for other factors, it is clear that suspension of the rules corresponds to coalition sizes that are larger, on average, somewhere between 16.5 percent and

17.7 percent—nearly an exact fit to the hypothesized point estimate above. Surprisingly, however, the estimate of the constant term in all five models is significantly greater than 50 percent +1—the bare minimal majority that one would theoretically expect in the absence of suspension of the rules. While this large intercept confounds conventional wisdom about what constitutes a minimal-winning-coalition, it is unclear what its implication is towards assessing the veracity of the vote-buying theories.

One possibility is that this large coefficient follows from the bicameral nature of the legislative process in the United States. Because legislation must pass both houses of Congress in identical forms before it can be signed into law, any bill that is considered under final passage in the House is likely (nearly) the same bill that will be considered in the Senate. Because legislation must be able to secure at least a 60 percent majority in the Senate to overcome filibuster threats, it is fair to say that minimal-winning coalition in the Senate is actually 60 percent, not 50 percent. If there is sufficiently high correlation between the ideal points of the members of House and the Senate, then the consistently high constant term might simply follow from proposals having been strategically tweaked by their proponents to ensure that they will safely pass the Senate.[19]

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[18] Besides supporting the Groseclose-Snyder family of vote-buying theories, this result is also encouraging in that it offers support for another, similar theory of coalition formation offered by Carrubba and Volden (2000) which predicts that supermajorities will emerge over contentious logrolls. At the same time, this finding seems to stand in contrast to King and Zeckhauser's (2003) vote options theory.

[19] The author thanks a referee for this insight. Building on this point, it should be noted that while the point estimate for the effects of non-client legislation on coalition size (around 6.5 percent) might seem

≡ TABLE 2
DETERMINANTS OF COALITION SIZE*

Variable	1	2	3	4	5
Non-Client Politics	1.95	5.56	5.79	6.16	6.40
	(0.59)	(1.86)	(1.86)	(1.99)	(2.02)
Suspension of Rules		16.48	16.16	17.93	17.71
		(7.38)	(6.30)	(6.27)	(6.26)
Open Rule			3.29	2.72	2.40
			(1.11)	(0.91)	(0.81)
Veto Threat				−15.79	−15.63
				(6.10)	(6.22)
Congress Quartile					0.90
					(0.84)
Constant	78.61	68.89	67.05	67.33	65.20
	(26.42)	(23.81)	(17.77)	(17.88)	(14.29)
Number of observations	163	163	163	163	163
R^2	0.00	0.24	0.25	0.26	0.26

*Ordinary least squares estimates with t-statistics based on Huber-White standard errors in parentheses.

Similarly, another possibility (consistent with Krehbiel 1996, 1998) is that coalition-builders, anticipating the need for large coalitions to overcome supermajoritarian pivots downstream might consider a de facto minimal winning coalition as one that could overcome a president's veto in all situations, and recruit voters accordingly. The fact that this coefficient does not significantly decrease in models (4) and (5) when plausible veto threats are controlled for, however, make such an interpretation problematic.

With respect to the other control variables, the findings are mixed, and less resolute. The coefficient on the open rule variable is positive as hypothesized, but statistically insignificant by conventional standards in all models. Similarly, the trend variable introduced in model (5) also fails to be either the predicted sign, or statistically significant—implying that uncertainty as measured by Congress Quartile is not a significant factor in determining coalition size.[20] The cross-congress methods described by Groseclose and Snyder (1996) might more effectively measure preference uncertainty than the within-congress approach taken here, but such methods are obviously impossible given the current data source. Such questions warrant further examination.

substantively small, it would be interesting to consider what its effect would be if we were solely considering a unicameral legislature, where a simple majority guaranteed legislative success downstream. In such a scenario, it is reasonable to suspect that the effect of nonclient legislation on coalition size might be substantively larger.

[20] Replicating the above analysis with Congress Quartile variable transformed into a continuous time trend does not substantively affect the results.

With respect to the presidential veto threat variable, an interesting finding emerges. While one would expect that the coefficient on this variable would be positive, the analysis demonstrates that the credible threat of a presidential veto actually leads to smaller, not larger, coalitions when controlling for other bill characteristics. This finding is statistically significant by conventional standards, implying that the average legislative coalition on a bill that President Clinton expressed disapproval for, which was one unit away from his ideal point in DW-NOMINATE space, would be approximately 16 percent smaller than bills that faced no opposition from the White House. At face value, this finding seems problematic for pivotal politics theories that would predict the opposite finding. At the time, however, this finding also offers problems for Groseclose-Snyder vote-buying models. Given that it seems obvious that there were two competing interests struggling over the legislative matters for which a veto threat existed, Groseclose-Snyder models would predict there to be larger-than-average coalitions on these bills. At the same time, this finding would be consistent with theories of presidential persuasion (e.g., Neustadt 1990) that would predict that on issues that are important to the president's agenda, he can exert influence and persuade legislators to vote in accordance with his preferences.

The extent to which this finding undermines the vote-buying models deserves further consideration. That being said however, the consistently significant finding that cases of non-client politics yield larger coalitions than cases of client politics still provides a modicum of support for the vote-buying theories. As a first-order approximation of legislator-coalition leader strategic interaction, the theories

seem to perform well. To reinforce this point, a final robustness check on the data is instructive.

Building on the points above, it is plausible that the pervasively large constant term is simply a statistical artifact, driven by the fact that all House bills are analyzed, including legislation that is relatively trivial and uncontested, and have very large coalitions (even though vote-recruitment/vote-buying may not be occurring). As such, by conducting ordinary least squares analysis the regression line produced is effectively trying to provide a best fit for the data, which is likely leading to an extremely large constant term, as it tries to fit data that include several unanimous, or near-unanimous votes. If one adopts the conventional standard of bill selection in which bills that have coalitions greater or equal to 95 percent of the chamber are dropped from the sample, the constant term decreases from what follows from analyzing the entire sample, yet the theoretical predictions pertaining to coefficient on nonclient politics still hold.[21]

IV. SUMMARY AND CONCLUSION

The purpose of this study is to test coalition size implications of vote-buying theories. As noted above, these theories have gained significant acceptance in the political science and economics scholarship, but have yet to be subjected to empirical tests. Because of a variety of measurement difficulties that would accompany a direct test of the theories, this study made several important assumptions to facilitate the indirect testing of these models and the results of this analy-

sis provide a body of support for this group of theories. Specifically, controlling for whether the bill was called up under suspension of the rules, cases of non-client politics (two-sided vote-buying) yield larger coalitions than cases of client politics (one-sided vote-buying). Furthermore, this result holds when several variables that might control for other theoretical determinants of coalition size, such as open amendment procedures and preference uncertainty, are added to the analysis. While several of these control variables fail to achieve statistical significance, the initial finding that cases of non-client politics yield larger coalitions seems robust, offering support for the vote-buying theories.

Future research on these theories can take any of several directions, depending upon which implications of the theories a scholar wishes to test. One possibility is to replicate this analysis using other Congresses to ensure that the results presented here are not Congress-specific. Another possibility is to adopt other approaches suggested by Groseclose and Snyder (Groseclose and Snyder 1996), such as conducting cross-time analysis to measure average coalition size in relation to interest group growth. Do coalition sizes increase as more interest groups (potential vote buyers) emerge over time? Finally, one might investigate whether campaign contributions of particular interest groups respond to the growth of competing interest groups. Vote-buying theories would predict a strong positive correlation between campaign contributions and the growth of potential competitors as incumbent lobbyists pay out more money and favors to legislators to discourage any new entry in the market for influence.

Regardless of which direction is taken, this study effectively tests a straightforward prediction of the vote-buying theories of coalition formation, and demonstrates that these theories, in addition to being analytically elegant, possess real-world bite. Their incorporation into further studies of legislative politics can only serve to enhance our understanding of the strategies employed by coalition leaders both within, and outside, the legislature.

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[21] Appendix B replicates the analysis in Table 2, focusing only on those bills that have coalitions less than 95 percent of the chamber. As can be seen, the results for the coefficient on *nonclient politics* are still positive and significant by conventional standards. The fact that the constant term is very close to 60 when controlling for other factors lends support to the explanation that the consistently high constant follows from legislators strategically designing proposals to appease the Senate's filibuster pivot.

≡ Appendix A

Abridged List of Bills Sampled

H.R. Number	Subject Matter	Classification
1	A bill to grant family and temporary medical leave under certain circumstances.	Nonclient
2	A bill to establish national voter registration procedures for Federal elections and for other purposes.	Nonclient
3	A bill to amend the Federal Election Campaign Act of 1971 to provide for a voluntary system of spending limits and benefits for congressional elections.	Nonclient
4	A bill to amend the Public Health Service Act to revise and extend the programs of the National Institutes of Health, and for other purposes.	Nonclient
5	A bill to amend the National Labor Relations Act and the Railway Labor Act to prevent discrimination based on participation in labor disputes.	Nonclient
6	A bill to extend for six years the authorizations of appropriations for the programs under the Elementary and Secondary Education Act of 1965.	Nonclient
8	A bill to amend the Child Nutrition Act of 1966 and the National School Lunch Act to extend certain authorities contained in such Acts.	Nonclient
20	A bill to amend title 5, United States Code, to restore to Federal civilian employees their right to participate voluntarily, as private citizens, in certain activities.	Nonclient
38	A bill to establish the Jemez national Recreation Area in the State of New Mexico and for other purposes.	Client
51	A bill to provide for the admission of the State of New Columbia into the Union.	Client
175	A bill to amend title 18, United States Code, to authorize the Federal Bureau of Investigation to obtain certain telephone subscriber information.	Nonclient
239	A bill to amend the Stock Raising Homestead Act to resolve certain problems regarding subsurface estates, and for other purposes.	Nonclient
322	A bill to modify the requirements applicable to locatable minerals on public domain lands, consisted with the principles of self-initiation of mining.	Nonclient
328	A bill to direct the Secretary of Agriculture to convey certain lands to the town of Taos, New Mexico.	Client
334	A bill to provide for the recognition of the Lumbee Tribe of Cheraw Indians of North Carolina, and for other purposes.	Client

≡ Appendix B

Determinants of Coalition Size for Bills with Coalitions Smaller Than 95 Percent of House Chamber*

Variable	1	2	3	4	5
Non-Client Politics	5.46	7.39	8.07	8.37	8.59
	(1.81)	(2.51)	(2.57)	(2.66)	(2.59)
Suspension of Rules		8.09	10.39	10.46	10.22
		(2.93)	(3.20)	(3.24)	(3.23)
Open Rule			4.33	4.00	3.79
			(1.60)	(1.47)	(1.40)
Veto Threat				−8.60	−8.51
				(2.98)	(3.09)
Congress Quartile					0.57
					(0.51)
Constant	68.17	64.43	61.71	61.80	60.46
	(25.40)	(23.87)	(16.93)	(16.93)	(12.28)
Number of observations	116	116	116	116	116
R^2	0.03	0.10	0.12	0.13	0.13

*Ordinary least squares estimates with t-statistics based on Huber-White standard errors in parentheses.

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