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Source: *American Journal of Political Science*, May, 1996, Vol. 40, No. 2 (May, 1996), pp. 543-564

Published by: Midwest Political Science Association

Stable URL: https://www.jstor.org/stable/2111637

*Theory and Evidence for Counteractive Lobbying**

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Introduction

In Austen-Smith and Wright (1992), we developed a formal model to study the pattern of lobbying and influence when two rival groups contend to affect the voting behavior of a legislator over a given pair of alternatives. Subsequently, in Austen-Smith and Wright (1994), we analyzed comparative static results derived from that model. Our analysis focused on the relationship between the decision to lobby by two groups, A and B, and the prior probability, p, that the legislator favors a particular alternative from the given pair of alternatives. The comparative statics imply that, ceteris paribus, if p is less than .5, so that the legislator is expected to vote against A's interests and in favor of B's, then: (1) the decision of A to lobby the legislator depends on p but is independent of the decision of B; (2) if A does not lobby the legislator, then neither will B; and (3) if A does lobby, then B lobbies when p is sufficiently high.

We termed (3) counteractive lobbying. Intuitively the idea is that when legislators' prior positions are changeable (i.e., when p is sufficiently greater than zero or less than one), then we should observe interest groups actively lobbying to change the positions of legislators aligned *against* them; and under these conditions, we should also observe groups aligned *with* the same legislators lobbying to counter the potential influence of their rivals. We tested this prediction, along with results (1) and (2), in Austen-Smith and Wright (1994) with data drawn from the lobbying efforts of organizations on Robert Bork's nomination to the United States Supreme Court. Since different legislators can be associated with different values of p—the probability of voting for or against Bork prior to any lobbying activity—our three hypotheses are cross-sectional statements about the pattern of lobbying across legislators. We emphasize that (1)-(3), along with the conditions under which they occur, are implications, not assumptions, of the model.

Baumgartner and Leech (1996) contend that we specified our theoretical and empirical models incorrectly, coded our variables improperly, based

*We appreciate the comments and suggestions of Jeffrey Banks, Gregory Caldeira, Rebecca Morton, and Lee Sigelman on earlier drafts of this paper.

American Journal of Political Science, Vol. 40, No. 2, May 1996, Pp. 543–564

our conclusions on weak coefficient estimates, employed faulty measures, analyzed an unusual issue, and failed to place our results in proper historical context. Because they offer neither theory nor data to support these claims however, their contentions are merely hunches and intuitions that our results would change in significant ways had we taken account of considerations they deem important. We contend that their intuitions and criticisms are seriously misplaced. Indeed, their empirical criticisms illustrate the dangers and pitfalls of relying on guesswork instead of the data. Their theoretical criticisms betray misunderstandings of our theoretical model, and more seriously, of rudimentary aspects of game theory, the critical linkages between theoretical and empirical analyses, and important theoretical and empirical research on interest groups.

In what follows, we address the specific criticisms that Baumgartner and Leech raise about our paper, turning first to the theoretical issues and then to the empirical issues. We conclude by placing our results in the context of contemporary research on interest groups and lobbying—something Baumgartner and Leech do not attempt.

Theoretical Issues

Baumgartner and Leech raise several general objections to our theoretical conception of the lobbying process. First is their concern about the sequence of groups' lobbying decisions—whether behavior can be at once simultaneous and counteractive. A related concern is that our theoretical model does not consider that groups respond to expectations about the behaviors of other groups. We are also criticized for ignoring the ''social nature'' of lobbying and for modeling the lobbying process in a static rather than dynamic form. Let us consider each of these criticisms in turn.

Simultaneity, Counteraction, and Anticipation

How, Baumgartner and Leach wonder, can a group choose to lobby counteractively if both groups make their lobbying decisions simultaneously? Surely, they contend, behavior that is at once simultaneous and counteractive is logically inconsistent. An appropriate test of a theory of counteractive behavior, they argue, must reflect the timing of groups' lobbying efforts. They assert, moreover, that if groups do make their decisions simultaneously, they must do so without knowledge of their rivals' actions—an untenable assumption, Baumgartner and Leech believe, for groups do take into account expectations of the behaviors of other groups.

These criticisms are based on serious misconceptions about the equilibrium properties of the model, as well as about the type of data needed to test the theory. We ourselves were concerned that the term ''counteractive''

might confuse some readers, who would incorrectly infer that groups make their lobbying choices in a time-ordered sequence, first with one group making a lobbying decision, and then a second group deciding whether to counter the first group after observing its decision. To dispel this incorrect notion, we wrote (Austen-Smith and Wright 1994, 28): ''We emphasize that, in the model, both groups *simultaneously* decide to lobby, so that counteractive lobbying is an equilibrium result.'' We hoped that this sentence, together with a rudimentary understanding of game theory on the part of the reader, would clear up any possible confusion. Judging from Baumgartner and Leech's misunderstanding, we need to spell things out by briefly considering some basic elements of game theory.

Equilibrium and Comparative Statics. Predictions from game-theoretic models come from analyzing equilibria, the most fundamental of which are Nash equilibria. A Nash equilibrium consists of a decision rule for each agent, with the property that no single agent could adopt a different rule given the rules of all of the others and expect to do better in the game. Such a decision rule is called a strategy, and it provides a complete description of the choices of that agent under all logically possible circumstances in the game.

Actors in a game-theoretic model make their decisions at each stage of the game under one of two general conditions: either they know exactly the choices of other actors at a previous or comparable stage of the game, or they do not. For example, actor 1 may move first, and actor 2 second, but it is not required that 2 observes 1's move before 2 moves. When neither 1 nor 2 observes the other's choice before moving, they are in effect making their choices simultaneously. Each agent chooses its strategy to maximize its expected payoff conditional on some conjecture about other agents' strategies and, in a Nash equilibrium, these conjectures are correct.

Whether a model is dynamic, repeated, or static, the identification of an equilibrium is typically not sufficient to conduct empirical work. Empirical tests come out of comparative statics. Comparative statics refer to the way that predicted equilibrium behavior changes in response to changes in the values of one or more exogenous parameters of the model. An example of an exogenous parameter in our lobbying model is the prior probability that a legislator will vote for B. Given a unique equilibrium, empirical hypotheses are derived by varying a given parameter, identifying the consequent differences in the equilibrium decision rules, and then comparing the predicted behavior—to lobby or not to lobby, to vote for A or to vote for B—induced by the equilibrium decision rules before and after the parameter change. Thus, operationalizing and empirically examining a hypothesis

requires finding a data set in which the relevant parameter shows appropriate variation, attempting to control for other exogenous factors that have also changed in the real world, and examining whether the predicted variation in behavior occurs. At this empirical stage, there is no claim to examine the equilibrium decision rules themselves or the reasoning of the agents involved since such things are unobservable; the focus is on the observable correlation between some parametric variable and their agents' actions.

We now return to the issues at hand. The groups in our model make their lobbying decisions simultaneously and, in equilibrium, these decisions will be consistent in that each group's decision will be predicted on *correct* expectations about the other's decision. Baumgartner and Leech reveal their confusion about the equilibrium concept in claiming that "Austen-Smith and Wright's formal model predicts that counteraction will occur spontaneously—that is, without knowledge of rivals' actions—when the probability of an opposing group successfully misleading a legislator is sufficiently high." Their confusion is evident in their assertion that counteraction occurs "without knowledge of rivals' actions." This claim is inconsistent with the very definition of a Nash equilibrium, and the statement is not one that we make, or would have made, in the paper. By definition, in equilibrium each agent's decision rule is chosen taking complete account of the *expected* decisions of all other agents at every stage of the game. The existence of an equilibrium ensures that no group would alter its lobbying strategy if given the opportunity to so do after observing the other group's strategy. Thus, in equilibrium, groups correctly anticipate each other's decisions and act accordingly.

It follows that one's data need not be in any sequential order to test the comparative statics predictions of the model. When testing the comparative statics predictions, one is examining only how equilibrium lobbying choices—the choices that groups actually made, having anticipated the choices of their rivals—vary with changes in the values of the parameter p. An empirical test of the theory does not require an examination of the process groups go through in anticipating the actions of their rivals or an analysis of what the decision rules themselves look like. A test of the theory involves a test of its *predictions*, not its assumptions.

The Social Nature of Lobbying

Baumgartner and Leech characterize lobbying as a "social activity." Unfortunately, they make no effort to define what this means, so no one is going to have much luck exploring the implications of this claim. Part of what they apparently have in mind is groups taking account of other groups' behavior. As observed above, however, our model is intrinsically

one of social activity: by definition in a game-theoretic model, equilibrium behavior reflects a set of mutually consistent expectations across all players.

Whatever Baumgartner and Leech have in mind by "social activity," they argue that any satisfactory model of lobbying must include it. If this concept is to be incorporated into a game-theoretic model, however, then it has to be characterized in terms of the primitives of the model: the agents, their preferences over outcomes, the actions they can take, and their beliefs. Only then can the implications of the concept for behavior and outcomes be deduced. Unfortunately, Baumgartner and Leech provide no idea what such a characterization would look like. Hence, their insistence that "social activity" be incorporated into the model is tantamount to *assuming* that lobbying is a social activity, rather than *explaining* it as such.

What Baumgartner and Leech may have in mind by "social activities" is a varied set of activities in which lobbyists and legislators engage for fun and camaraderie. Birnbaum (1992, 22–6), for example, describes how interest groups organize weekend retreats for lawmakers and their families, complete with golf, swimming, bowling, trap and skeet shooting, and various activities for the children. This social aspect of lobbying, as one of us has argued elsewhere (Wright 1996, 76–82), is primarily about access rather than influence, and thus is quite distinct from the portion of the process where lobbyists explicitly seek legislators' support on particular issues. We have attempted to model only the narrow slice of the entire lobbying process that occurs at the very end, where groups explicitly attempt to influence legislators' votes, not the entire process that begins with access-seeking.

The particular slice of the lobbying process that we have modelled is admittedly a subjectively differentiated portion of a continuous process. Identifying such starting and stopping points, however, despite being somewhat arbitrary, is an essential and unavoidable aspect of scientific inquiry. Unless one aspires to a "theory of everything," one must specify clear boundaries to the events under investigation; otherwise, research questions, whether theoretical or empirical, are not tractable.

What makes for good models, and hence good science, is the selection of manageable events for study. William Riker (1957) noted that the greatest advantage natural science has over social science is in the analysis of small—even minute—events. Narrow slices of reality can be defined more precisely than large, sweeping events, resulting in a greater likelihood of replication and more precise generalization. This, of course, is particularly important when the application of statistical techniques presumes that identifiable events will occur again and again.

Baumgartner and Leech's advocacy of a more inclusive theory of lob-

bying incorporating access-gaining dimensions simply does not strike us as consistent with good scientific practice. That the world is a complicated place and a model necessarily abstracts from some of its key elements does not imply that the model is useless, wrong, or misleading. To the contrary, it means that the model has the potential to yield worthwhile generalizations and predictions.

A Static Versus Dynamic Model

Baumgartner and Leech make much of dynamics, social and otherwise, but they offer nothing but assertion about its importance. It may be important, and indeed with a dynamic model we can ask additional questions about the role of reputation and so forth. This does not say that the underlying incentives identified in Austen-Smith and Wright (1992, 1994) regarding counteractive lobbying will go away or be contradicted. In fact, our intuitions about the relative merits of a static versus dynamic model are just the reverse of Baumgartner and Leech's.

The underlying incentives confronting groups in the model that lead to our Hypotheses (1), (2), and (3) do not depend at all on the static setup. For example, at any stage in a dynamic process, a reputational effect is manifest in the beliefs that one agent, say the legislator, has about the reliability of another, say group A. The more reliable the legislator believes A to be, the more the legislator will trust what A says and the less incentive there will be to monitor A's claims. In our model, this is easily captured by varying the penalty for being caught dissembling, since this in turn changes the incentives to dissemble. Thus, while it is dynamics and repeated interactions that generate incentives to build reputations and so forth, any particular stage in the process will reflect such reputations in the extent to which legislators trust groups—an idea that is easily embedded in the static framework. Of course, if the focus of our model were on explaining long-run lobbying relationships, then a static model would be inappropriate. But this is not our focus.

Baumgartner and Leech clearly feel strongly that dynamic considerations are paramount for any understanding of lobbying in the legislative process. Until demonstrated, however, such a claim is purely armchair speculation. Their case will not be compelling until they specify an alternative model with dynamic features—e.g., a repeated game or a dynamic game—and show that the equilibria to such a game yield predictions that are at odds with those generated by the static model. Unlike Baumgartner and Leech, who offer only assertions, we have an explicit model from which we have derived and tested several empirical predictions against a new set of data.

Empirical Issues

The empirical issues that Baumgartner and Leech raise reflect a suspicion that our results are largely a consequence of our coding schemes, measurement, case selection, model specification, and interpretation of the coefficients. This is simply not the case. In fact, for many of the criticisms Baumgartner and Leech raise about the empirical analysis, the evidence in support of counteractive lobbying is no weaker, and is sometimes stronger, than what we originally concluded. We begin with their complaint that our coefficient estimates are weak.

The Sizes of the Coefficients

Baumgartner and Leech contend that the estimated coefficients for counteractive lobbying are far too weak to support our theory. They state that a "coefficient of .12 . . . hardly seems strong enough to justify an entire theory of counteractive lobbying . . ." (1996, 524), and that "the coefficients presented provide little support for the 'counteractive' argument because they are weak . . ." (1996, 530). Baumgartner and Leech appear to be calling into question the magnitudes of the coefficients in our original Tables 2 and 3, but in fact it is only the coefficient in Table 2, having a value of .12, that they address specifically. They have no specific comment whatsoever on the size of the coefficient in Table 3, which at 1.34 is more than 11 times the size of the coefficient in Table 2.

Baumgartner and Leech do not take issue with the statistical significance of the coefficients (both are significant at .05), but they do contest the substantive significance. We agree that substantive significance is generally an important criterion for judging coefficients; however, the major drawback to applying a substantive test is the problem of determining a meaningful baseline. The dependent variable in our case is the number of groups lobbying for or against Bork, and the substantive implication of an increase of .12 or 1.34 in the values of these variables is unclear. Had we used senators' votes as the dependent variable in a separate analysis and estimated the effect of a unit increase in the number of groups lobbying for or against Bork on senators' roll call decisions, we could have assessed our coefficients in terms of their ultimate effect on the nomination outcome. We did not do that. Neither, however, did Baumgartner and Leech. Nor do Baumgartner and Leech propose any criteria, or offer any substantive argument, about what would be an appropriate size for the coefficient. They merely complain that one of the coefficients is weak.

Baumgartner and Leech also imply that the counteractive coefficients are weak relative to the other coefficients in the model. We made no effort

in our original paper to compare magnitudes of coefficients within models, but it is instructive to do so now, especially given the absence of a clear substantive baseline for judging the coefficients. The standardized (beta) coefficients for the coefficients originally presented in our original Tables 2 and 3 (Austen-Smith and Wright 1994) are as follows:

Table 2 (1994)		Table 3 (1994)	
PRO, senator's *ex ante* position	.41	ANTI, senator's *ex ante* position	.24
C, Judiciary Committee	.47	C, Judiciary Committee	.39
V, 1987 voting score	−.69	V, 1987 voting score	.46
O_P, organizational strength of pro-Bork groups in senator's state	.26	O_A, organizational strength of anti-Bork groups in senator's state	.35
L_A, number of anti-Bork groups that lobbied	.21	L_P, number of pro-Bork groups that lobbied	−.18
PRO × L_A	.42	ANTI × L_P	.40

In the first model, the counteractive coefficient (the coefficient for the interaction of PRO and L_A) ranks third in relative size, not far behind the effect of the Judiciary Committee, which Baumgartner and Leech proclaim (incorrectly as it turns out) to be "by far the strongest predictor of contact in the model" (1996, 523). In the second model, the counteractive effect (for the ANTI and L_P interaction) ranks second in relative importance, slightly above the importance of membership on the Judiciary Committee. In models that explain 72 and 78% of the variance respectively, we think the performance of the counteractive coefficients is strong enough to uphold our conclusion that counteractive lobbying was an important aspect of the Bork campaign.

Senators' Prior Positions

Baumgartner and Leech offer several criticisms of our measure of groups' beliefs about senators' positions on the Bork nomination prior to any lobbying. They suggest that using senators' votes on previous controversial nominations during the second Reagan Administration—precisely what the groups themselves used—is inappropriate. They contend that our use of the American Conservative Union's breakdown of senators' expected support for Bork "amounts to the very thing that Austen-Smith and Wright avoid compiling themselves owing to the supposed superiority of the independent ACU judgment" (1996, 525).

Baumgartner and Leech are correct—we did avoid compiling our own

estimate of senators' prior positions, even though we could have done so using some combination of previous roll call votes. The reason we did not compile our own prior is that our choice of previous votes on which to base a prior may not have corresponded to the ACU's choice. We considered it prudent to defer to the ACU's judgment in this matter, for clearly, it was the ACU and its allies, not we, who did the lobbying. Our theory suggests that groups' lobbying strategies are predicted on *their* prior beliefs about legislators' positions, and we consider the ACU measure a fine indicator of lobbyists' prior beliefs.

Baumgartner and Leech also express concern that we have "missed the opportunity" to test our theory by collapsing what they believe should be a six-point prior into a dichotomous measure. In fact, the prior that the ACU devised was based on *four* categories, not six. The ACU's procedure was as follows. First, it assigned senators to one of four groups, ranging from expected support for Bork to expected opposition, on the basis of previous votes on controversial judicial nominations during the second Reagan Administration. Then, once these four categories were constructed, the ACU identified eight senators from Group 4 (not expected to support Bork), and two senators from Group 1 (expected to support Bork) as important lobbying targets. The original four groups, together with the emphasized targets, were reported in a memorandum to the 300 members of Coalitions for America on July 2, 1987, the day after Bork was nominated.

> Group 4 are those Senators who have always voted wrong in the votes we have targeted. However, a handful of even these Senators are worth your attention if you have the time and resources to spare. These include: Max Baucus (D-MT), Robert Byrd (D-WV), Tom Daschle (D-SD), James Exon (D-NE), Wyche Fowler (D-GA), Albert Gore (D-TN), Patrick Moynihan (D-NY), Harry Reid (D-NV) and Jim Sasser (D-TN).

The memorandum also noted that Senators Dan Evans (R-WA) and Bob Packwood (R-OR), both Group 1 senators, were of particular concern. On the second page of the memorandum where senators of all four groups were listed, however a '+' was attached to another five senators in Group 1, indicating "a normally 'predictable' senator, who, for various reasons, should be considered a possible switch on the Bork vote."[1] In total, then, seven senators from Group 1 (call them Group 1*) and eight senators from Group 4 (call them Group 4*) were identified as important lobbying targets in the action memorandum mailed to leaders of groups in the pro-Bork coalition on July 2. Baumgartner and Leech contend that these targeted

[1]Quotations are from the original memorandum.

senators actually constitute new categories of the prior, and they assert that analysis of these additional categories would have altered our conclusions.

In our judgment, the recommendations to lobby particular senators within Groups 1 and 4 really amounts to a lobbying *strategy*, not a refinement of the prior. Naturally, conservative leaders were more uncertain about the positions of some senators than others, but does that mean as researchers we should construct additional categories? Where, for example, should the new categories of senators be placed relative to the others? Should the seven senators from Group 1 be moved to a new group between Groups 1 and 2? Or should they be treated as undecided? It was not clear to us how they should be treated, and because the ACU chose not to designate them as undecided, neither did we. Reconstructing the prior on the basis of the lobbying strategy recommended by the ACU blurs the empirical distinction between a prior and a strategy.

Ultimately, we elected to collapse the four categories into two simply to make the presentation and interpretation of the results more parsimonious. A four-category prior yields little additional information, but several additional variables. The six-fold prior, which is questionable on both theoretical and empirical grounds, also yields little additional insight, but involves six additional variables. Although we still prefer the original specification, we ran the analysis using both a four and six-fold prior, and we present those results here in Tables 1 and 2.

In the analysis of the four-fold prior, pro-Bork groups in our present Table 1 and anti-Bork groups in our present Table 2, were both less likely, other things being equal, to lobby their legislative allies and more likely to lobby their a priori opponents. The magnitudes of the counteractive coefficients are very similar to those in the original analysis, and three of the four counteractive coefficients across both tables are statistically significant.

In the analysis of the six-fold prior, anti-Bork groups in Table 2 here were significantly more likely to lobby senators from Group 1* and Group 4*, other things being equal. Whether liberal groups were aware of the ACU's memorandum outlining its lobbying strategy is unclear, but the fact that they concentrated their efforts heavily on the same senators targeted by the pro-Bork forces suggests either that they had good intelligence, or that they too viewed these senators as important lobbying targets. The counteractive coefficients for these targeted groups of senators are *not* statistically significant, however. This result is consistent with Baumgartner and Leech's hypothesis that these senators were relative fence-sitters—thus each side lobbied these senators without anticipation of what the other side might do—but it does not alter our fundamental conclusion. Three of the four remaining counteractive coefficients from the six-fold analyses are

Table 1. Regression Analysis of Interest Group Lobbying By Organizations Supporting Robert Bork

Dependent Variable: L_P, number of pro-Bork groups that lobbied

	Four-fold Prior		Six-fold Prior	
Explanatory Variable	2SLS Estimate	Standard Error	2SLS Estimate	Standard Error
Constant	3.22*	1.69	1.56	1.91
Group 1	−1.18	1.95	−0.10	2.08
Group 1*			4.58	5.20
Group 2	−2.96	2.70	−2.12	2.74
Group 3	0.44	0.57	0.78	0.60
Group 4*			0.90	0.66
C, Judiciary Committee	4.38*	0.70	3.97*	0.75
V, 1987 voting score	−4.20*	1.46	−3.45*	1.30
O_P, organizational strength of pro-Bork groups in senator's state	0.43*	0.09	0.40*	0.10
L_A, number of anti-Bork groups that lobbied	0.03	0.07	0.07	0.08
Group 1 × L_A	0.12*	0.07	0.14*	0.08
Group 1* × L_A			−0.15	0.21
Group 2 × L_A	0.12	0.10	0.11	0.10

N = 100; R-square = .73; R-square = .75
*denotes statistical significance at .05 (one-tailed test).

statistically significant, providing substantial support for the counteractive hypothesis. Importantly, the counteractive hypothesis pertains to *non-fence-sitters*, and that is the hypothesis we have tested. Evidence that fence-sitters are not lobbied counteractively does not contradict our theory; and, in fact, constitutes evidence in favor of it.

The Bork Nomination

Baumgartner and Leech contend that the case we chose for analysis is so unique that we cannot generalize from it. The Bork nomination, they argue, was conflictual rather than consensual, had high rather than low public visibility, and, like all confirmation votes, did not involve lobbying at the agenda-setting stage. Had we analyzed a consensual case with low public visibility, they suggest, our findings would have been quite different: "A tendency to focus on highly public and conflictual cases would lead to a distorted view of the roles and activities of interest groups in general" (1996, 534).

Table 2. Regression Analysis of Interest Group Lobbying By Organizations Opposing Robert Bork

Dependent Variable: L_A, number of anti-Bork groups that lobbied

	Four-fold Prior		Six-fold Prior	
Explanatory Variable	2SLS Estimate	Standard Error	2SLS Estimate	Standard Error
Constant	9.54*	3.21	11.43*	2.72
Group 1	2.16	2.29	−1.37	2.39
Group 1*			5.67*	2.11
Group 2	4.43*	2.05	3.15	1.92
Group 3	−3.07	3.96	−1.90	3.52
Group 4*			5.13*	3.02
C, Judiciary Committee	11.44*	3.62	11.03*	3.19
V, 1987 voting score	8.26*	3.56	5.36*	3.01
O_A, organizational strength of anti-Bork groups in senator's state	0.33*	0.07	0.33*	0.07
L_P, number of pro-Bork groups that lobbied	−1.14*	0.66	−0.88	0.59
Group 3 × L_P	1.95*	0.94	1.51*	0.84
Group 4* × L_P			−0.35	0.66
Group 4 × L_P	1.24*	0.38	1.27*	0.34
N = 100;	R-square = .79;		R-square = .85	

*denotes statistical significance at .05 (one-tailed test).

The claim that nonconflictual issues provide more generalizable results is highly debatable. Since nonconflictual, or consensual issues, by definition involve little or no conflict of interests, we would expect either no participation by organized groups, or else participation by groups on only one side of the issue. Politics, however, is about resolving conflict, and thus it is a strange recommendation to study consensual issues in order to understand and generalize about an inherently conflictual process.

Baumgartner and Leech also fault us for analyzing a highly visible issue. They claim that interest group participation will be greater, and the impact of their activities greater, on issues with low public visibility. They argue that "studies emphasizing the greater impact of group activities have been more likely to focus on long-term quieter aspects of groups' relations with government officials" (1996, 534). The claim is based on the assumption that, on very public issues, legislators will be more concerned about satisfying their constituents, who will be informed and attentive, than about pleasing special interests.

This argument implies that on a highly visible issue such as the Bork nomination, interest group activities and influence would have been marginalized as senators turned to their constituents or party leaders for voting cues. For such an issue, therefore, it should be relatively *difficult*, not easy, to find evidence of any distinct lobbying strategies, let alone counteractive ones designed to achieve influence over the outcome. By Baumgartner and Leech's logic, that we found evidence of counteractive lobbying when it would be least expected suggests that we should be even more likely to discover it on less visible issues. Thus, the case of Bork is a *strong* test case by the very criteria that Baumgartner and Leech propose.

Baumgartner and Leech also believe we have chosen an inappropriate issue for analysis: "confirmation votes in general are peculiar examples of congressional policymaking because legislators are not involved at the agenda-setting stage, the stage where interest groups may wield the greatest power." This claim is incorrect. Especially since the mid-1980s, but even in earlier periods, interest groups and senators have frequently attempted to influence the president's choice of a nominee (for details, see Caldeira and Wright 1995). Although the Bork nomination may have involved less legislative lobbying about who the nominee should be than other nominations—Bork's nomination in 1987 to replace Powell was seen as a forgone conclusion by some observers, especially after Bork had been passed over for nomination in 1981 and again in 1986—members of the Senate Judiciary Committee were still consulted prior to the nomination itself (Bronner 1989, 35–6). Gitenstein (1992, 36–7), in fact, suggests that a key reason that President Reagan followed through with his nomination of Bork was that Senate Majority Leader Robert Byrd indicated that he would not oppose Bork in a private meeting with Attorney General Ed Meese and White House Chief of Staff Howard Baker.

Further evidence of the involvement of senators at the agenda-stage in confirmation battles is provided by a former staff member of the Senate Judiciary Committee, responding to a direct question about senatorial involvement in the selection of nominees:

> There is a formal start [to the confirmation process] when we receive the formal nomination, that is presented to the Secretary of the Senate. It's a formal document that is walked into the chamber, presented to the Senate and the Senate refers it to the committee. But almost always, it starts before that. It will always start, at the earliest stages, with rumors of who's involved in the potential nomination process—who may become the next nominee. And sometimes the Senators get very much involved in trying to influence, one way or another, the selection by the President. We see a lot of deals cut.[2]

[2]Confidential interview, 1990.

More importantly, our analysis is explicitly *not* about lobbying at the agenda-setting stage. In Austen-Smith and Wright (1992, 246), we clearly emphasized that our predictions "apply only to lobbyists attempting to persuade legislators to vote for or against a fixed agenda." Whether counteractive strategies would be prominent at the agenda stage is a question about which we can only speculate, and because Baumgartner and Leech bring no additional theory or evidence to bear on this question, it is also one about which they can only speculate.

Direct and Indirect Lobbying

Baumgartner and Leech are concerned that we have not properly taken into account the possibility that groups lobby indirectly; i.e., that they lobby friendly legislators in order to have them, in turn, lobby other less friendly legislators. We recognize that interest groups may engage in various indirect efforts to persuade legislators, and we have tried to be very explicit about this in both our theoretical and empirical analyses. In the conclusion to our theoretical article (Austen-Smith and Wright 1992, 246), we emphasized that groups might lobby indirectly even when they choose not to lobby directly.

> In those cases when the group chooses not to lobby, it may still be worthwhile for it to solicit a "friendly" legislator to act on its behalf—that is, to enage in indirect lobbying. "Friendly" legislators are in a position to offer trades (e.g., future logrolls) with key figures on a given issue and so induce "unfriendly" legislators to vote as the group wishes, even though the group could not anticipate influencing the decision directly. The results of the model suggest two conjectures. First, that groups will attempt such indirect lobbying when the prior probability of the "unfriendly" legislator changing his or her mind on informational grounds alone is low; and second, that groups engaged in indirect lobbying will solicit help from legislators whose prior beliefs strongly favor their own position.

In Austen-Smith and Wright (1994), we were even more explicit about facets of lobbying that involve contacts with one's legislative allies that are not motivated by a strategy of countering the opposition.

> Groups may, for example, consider lobbying to involve consultations with their legislative friends in order to have them indirectly lobby other less sympathetic legislators. Or groups might consider lobbying to involve social visits with legislators in order to maintain channels of access (e.g., Milbrath 1963, 255–94). It is also possible that groups contacted senators of a particular general ideological orientation, depending on the ideological orientation of the groups' members, in order to achieve visibility and raise money (e.g., Hayes

1981, 86). We expect that there may be influences such as these operating systematically outside the scope of our model (36–7).

We have no explicit empirical measures of any of the various forms of indirect lobbying that might have occurred on the Bork nomination. Our theory, however, is about direct lobbying, not indirect lobbying, and therefore our empirical measures of lobbying are also about direct lobbying, not indirect lobbying. We asked groups about their "Washington lobbying/advocacy efforts" on the Bork nomination, specifying that: "By Washington lobbying/advocacy, we mean *direct contacts* [our emphasis] with the senator, in person or by phone, or contacts with the senator's staff." That direct lobbying took place on the Bork nomination is an observable and indisputable fact, despite Baumgartner and Leech's assertion that our measure of lobbying does not correspond "to the facts of the Bork case."

We have no systematic nor even anecdotal evidence that indirect lobbying was a key feature of the Bork nomination, but still we cannot, and did not, rule out the possibility that indirect efforts took place. Indirect lobbying, although not part of our theory, is an unavoidable aspect of our empirical analysis in that its omission from the analysis could have affected the consistency of the other coefficient estimates of direct and counteractive lobbying. When we conducted our analysis, we suspected, for example, that indirect lobbying by pro-Bork groups might be positively correlated with direct lobbying by pro-Bork groups and negatively correlated with direct lobbying by anti-Bork groups. Given this possibility, we sought to control indirect lobbying by including an ADA score (designated V in our tables), which theoretically was motivated by our conjecture, quoted above, that "groups engaged in indirect lobbying will solicit help from legislators whose prior beliefs strongly favor their own position." Importantly, the ADA variable clearly captures effects distinct from those of the ACU prior: both variables demonstrate statistical significance despite their correlation.

This approach to controlling for indirect lobbying is hardly ideal. We have no clear *prima facie* evidence that various forms of indirect lobbying were especially pronounced on the Bork nomination, however, or at least were pronounced enough to threaten our statistical analysis. Until Baumgartner and Leech produce concrete evidence that major indirect efforts did occur outside of our model, their assertion that our model is incomplete and misleading is premature. Ultimately, concerns about the accuracy of our point estimates can be resolved only through additional empirical analyses. We invite Baumgartner and Leech to conduct such analyses rather than asserting what the results must look like without any supporting evidence.

The Unit of Analysis

Baumgartner and Leech argue that our "theory calls for group-level analysis," not the analysis of coalitions of groups. Yet, our theory is completely agnostic on the matter of whether the unit of analysis is a coalition or an individual group. From an empirical standpoint, however, coalitions of groups are clearly the preferred unit of analysis. In the first place, to conduct our analysis at the level of individual groups would be impractical. To do so, we would need to analyze the behavior of each of 59 groups that lobbied against Bork in relation to each of the 16 groups that lobbied for Bork. Thus, we would have $59 \times 16 = 944$ regressions, and for each of these regressions it would be imperative to control for the lobbying activities of each of the other groups in the coalition.

More important than its impracticality, however, is the inappropriateness of this approach. It implies that each group made its lobbying decision independently of other groups in the coalition, and that each group lobbied in response to a particular group on the other side, not to the overall activities of that coalition. The lobbying efforts on the Bork nomination were truly coalitional however. Coalitions formed at the national level, and variants of these coalitions also formed at the state level. Seldom did groups act unilaterally; even direct Washington contacts were generally made by small delegations of lobbyists representing different groups. Thus, it would be artificial and inappropriate to analyze lobbying behavior at the level of the individual group, as Baumgartner and Leech recommend. A fundamental assumption of statistical analysis is that one's observations are independent of one another. If groups were chosen as the units of analysis, that assumption would be inconsistent with the facts of the Bork case.

Baumgartner and Leech also express concern that there is no one-to-one correspondence between our measure of organizational strength and our measure of the number of groups that lobbied. What they fail to recognize, however, is that one of the main reasons groups form coalitions is to assure a more uniform distribution of strength across districts and states. A group that is organizationally weak in a senator's state can enhance its credibility in the senator's eyes by joining with groups that are oganizationally strong in that state. This can provide a basis for lobbying the senator that the group would otherwise lack. Consequently, that there is no one-to-one correspondence between our measure of organizational strength and our measure of the number of groups lobbying accurately reflects the way coalitions operate. The coalitional structure allows for information about electoral circumstances to be freely transferred among coalition members, regardless of which groups actually have the organizational resources to generate the information.

Finally, Baumgartner and Leech's concern about the lack of a control for population in the organizational strength variable is unfounded. Our measure of organizational strength is simply the total number of organizations among those that lobbied reporting strong or moderate bases of organizational strength in a given state. Larger states, because of the diversity of their populations and economic interests, tend to have more organizations than do small states. Why we should control for this fact of political life is unclear to us. Our hypothesis is simply that the greater the number of organizations active in a given state, the greater the number of groups that will lobby the senators from that state. Our analysis is from the point of view of the coalitions of groups, not senators, so the concerns Baumgartner and Leech raise about the "senators' point of view" (1996) are simply irrelevant.

Contradictory Findings

Our results on counteractive lobbying suggest a distinct modification in the conclusions that scholars have traditionally held about interest group lobbying. The unique prediction of our model is that groups are expected to lobby legislators who a priori do *not* agree with them, whereas all other predictions of which we are aware assert that groups will lobby only undecided legislators or legislators who a priori support their position. The statements of Milbrath (1963), Bauer, Pool, and Dexter (1963), Matthews (1960), and others are very clear on this latter point. There has been no substantial revision of this basic conclusion, moreover, even in contemporary discussions of lobbying. Berry (1989, 145), for example, while noting that an important aspect of the lobbyist's job is trying to persuade undecided legislators, stops far short of asserting that groups actively attempt to change the positions of legislators aligned against them.

Given the evidence of counteractive lobbying, it follows that legislators do sometimes change their positions on the basis of interest group lobbying. Although we did not conduct a direct test of the influence of lobbying in Austen-Smith and Wright (1994), our findings nevertheless are consistent with the notion that groups are influential in the legislative process. As such, our findings fit within a growing body of empirical and theoretical results that point consistently to significant effects of lobbying on legislative outcomes.

Within the theoretical literature, important developments in game theoretic approaches to information problems have helped place the long-recognized relationship between information and lobbying on firm theoretical footing (Austen-Smith and Wright 1992; Rasmusen 1993; Potters 1992; Ainsworth 1993; Ball 1991; Wu 1992; Austen-Smith 1993, 1995; Ainsworth and Sened 1993). These studies generally demonstrate the existence

of equilibria in which lobbying is influential. Within the empirical literature, scholars employing different analytical techniques, different measures of lobbying, and different legislative issues have repeatedly found significant but moderate effects of lobbying on legislative outcomes. Wright (1990) found significant lobbying effects even on relatively obscure votes within House committees. Langbein and Lotwis (1990) reported in their analysis of voting on the McClure-Volkmer Bill that "The NRA's extensive grass roots lobbying appears to have been effectively countered by the activity of Handgun-Control-inspired local and national police groups" (431). Rothenberg (1992) concluded from his analysis of congressional voting on the MX missile that "lobbying matters," that "lobbying is a force," and that "lobbying had a significant but moderate effect on vote outcomes." Wu (1994), using Wright's (1990) lobbying data and a measure of committee-level participation similar to that of Hall and Wayman (1990), found support for "the conclusion that lobbying influences are an important medium through which interest group influence is translated into the legislative process."

In light of these studies, it is unclear to us exactly which studies Baumgartner and Leech have in mind when they refer to "recent quantitative assessments of group influence and activities concluding that groups are unable to exert significant influence" (1996, 522). To the contrary, quantitative research on lobbying yields consistent and fairly strong evidence that groups are indeed able to exert significant influence. With the notable exception of Bauer, Pool, and Dexter and others writing in their tradition, moreover, the overwhelming conclusion of other scholarly approaches to the study of interest group research also holds that groups are influential. Different models and interpretations of why groups are important have been presented over the years, ranging from notions of pressure (e.g., Latham 1952) to subgovernments (e.g., Cater 1964; James 1969) to issue networks (e.g., Heclo 1979; Jones 1979) to "presence politics" (Bosso 1987), but the central conclusion has been consistent: interest groups often matter a good deal in the legislative process. In fact, Bosso questions whether Earl Latham's assertion that the legislature is a "referee" in the group struggle is "not more apt today than it was then." Even the conclusion by Heinz et al. (1993) that elites do not attempt to mediate interest group conflict outside the institutions of government is consistent with the idea that interest groups compete and win or lose in the Congress, the courts, and executive agencies. Exactly why Bauer, Pool, and Dexter (1963), and even Milbrath (1963), to some extent, perceived so little influence by organized groups is not entirely clear. What is clear, however, is that their conclusion is contradicted not only by counteractive lobbying, but by most other perspectives of interest group influence as well.

We also dispute Baumgartner and Leech's contention that "the recent literature is internally divided" (1996). The primary basis for this claim seems not to be the literature on campaign contributions, which they do not address at all, but rather a minor aspect of an earlier article by Wright (1990), which was neither intended, nor presented, as a paper on counteractive lobbying. Baumgartner and Leech claim that "Wright (1990) tested explicitly for counteractive behavior in a previous article and concluded that a direct model of lobbying strategies was appropriate." This assertion is drawn from an endnote (9) to Wright (1990, 436), which noted that lobbying by one side only weakly affected lobbying by the other. This result, however, did not include an interaction term for counteractive lobbying and therefore was not an explicit test of counteractive behavior as Baumgartner and Leech claim. In fact, according to Hypothesis (2) of Austen-Smith and Wright (1994), one should *not* expect to find a significant direct effect of lobbying by one side on the other. It is only the interactive term that provides the correct test of counteractive lobbying, but this did not become clear until the theoretical properties of competitive lobbying were developed in Austen-Smith and Wright (1992). Hence, the first explicit test of counteractive behavior did not appear until Austen-Smith and Wright (1994), and the appropriate specification for this test was realized only after the theoretical properties of counteractive lobbying had been developed in Austen-Smith and Wright (1992). Subsequently, Wu (1994) has reformulated Wright's (1990) model and concluded that the results "lend some qualified support to the counteractive lobbying story modeled and tested in Austen-Smith and Wright" (23).

We recognize that in analyzing counteractive lobbying on the Bork nomination we have analyzed just one issue, and that it is quite different from many of the issues analyzed by earlier generations of interest group scholars. Baumgartner and Leech, however, allege that we "fail to place [our] findings in the context of previous studies of interest groups" (1996). They also allege that we "refer to Bauer, Pool, and Dexter and Milbrath as though little changed in the U.S. interest-group system since these authors wrote their influential works" (1996). Yet, in the conclusion to our paper, we wrote:

> The idea that groups achieve influence by lobbying counteractively may have escaped Bauer et al. simply because of the nature of the issue they examined. Being a distributive issue, tariff policy is not one that typically engenders a great deal of conflict between opposing interests. Consequently, one is not likely to observe the push and pull among groups that is characteristic of judicial nominations. Open confrontation among organized interests, however, is far more typical of policy issues today than was the case at the time Bauer and colleagues

made their observations. Due largely to the growth of citizen and public interest groups during the 1960s and 1970s, consumers and producers now clash regularly over environmental issues, health-care policy, energy policy, and so forth. Public interest groups are more likely to pursue confrontational strategies than the private interest groups studied by Bauer, et. al. (Berry 1977, 218–9).

Whether these changes in the interest group system account for differences in research findings is not entirely clear. They might. Alternatively, methodological approaches have also changed over time, and they, too, might account for some or all of the differences. Our primary purpose in Austen-Smith and Wright (1994) was not to explain *why* our results differed from previous results; our purpose was simply to report that the prevailing description of, and explanation for, interest groups lobbying strategies was incomplete. Our revised description and explanation is much more consistent with the contemporary evidence that groups are influential actors in the legislative process.

Conclusion

Baumgartner and Leech criticize Austen-Smith and Wright (1994) on both theoretical empirical grounds. With respect to the theory, they complain that (1) we ignore expectations; (2) we ignore the "social nature" of lobbying; and (3) we adopt a static rather than a dynamic model. In response, we point out that (1) is without foundation, reflecting an erroneous understanding of the Nash equilibrium concept; that (2) either requires us to assume what must be explained, or else involves some ambiguous aspects of the lobbying process that are not the concern of Austen-Smith and Wright (1992, 1994); and that (3) is purely armchair speculation until Baumgartner and Leech write down a dynamic model and demonstrate that it yields qualitatively distinct results from ours. With respect to the empirical work, Baumgartner and Leech report a litany of sins of omission and commission. In response, we observe that many of these are simply false representations of what we did, many are irrelevant, and many rest on empirical assertions that are demonstrably at odds with the facts.

Assuming the deductions from a formal model are correct, useful criticism proceeds either by examining the extent to which the predictions are robust to variations in assumptions, or by refuting the derived empirical hypotheses. The former requires writing down an alternative model and showing that it yields distinct results; the latter requires exploiting a new data set, or using an alternative methodology to generate contradictory findings. While we welcome such criticism, Baumgartner and Leech do not provide it. Their critique contains neither theory nor data nor methodologi-

cal variation. Until they replace assertions with replicable arguments, their complaints are vacuous.

Final manuscript received 9 June 1995.

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