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# *The Multiple Ambiguities of “Counteractive Lobbying”*\*

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We review an article recently published in this journal to show how errors of theory-building, measurement, and research design have contributed to a confusing state of research in the area of interest-group lobbying activities. Contradictory findings in this area have come from the use of cross-sectional variance models where theories have called for longitudinal ones, from inaccurate measurements, from incomplete models, and especially from a willingness to overgeneralize from case studies. Despite a resurgence of studies on lobbying strategies, this literature will remain contradictory and inconclusive unless researchers resolve some basic questions about their theories and the nature of the evidence necessary to test them.

## **Introduction**

The study of interest groups once dominated theories of American politics. Writers such as Schattschneider (1935, 1960), Truman (1951), Dahl (1961), McConnell (1967), and Lowi (1969) wrote about interest groups because they considered them to be at the core of the political system. Beginning in the 1960s, possibly in response to Mancur Olson's (1965) challenge of collective action, interest-group scholars increasingly turned their attention to the internal dynamics of groups, not their external activities. Progressively, the field strayed from the core questions of democratic accountability and processes. Recently, a number of scholars have begun to push the field back to the study of lobbying and other influence-seeking activities, a trend that we applaud (for a very partial list, consider Berry 1977; Gopoian 1984; McFarland 1984; Smith 1984; Wright 1985, 1990; Evans 1986; Schlozman and Tierney 1986; Caldeira and Wright 1988; Grenzke 1989; Vogel 1989; Browne 1990; Hall and Wayman 1990; Hansen 1991; Walker 1991; Rothenberg 1992; Ainsworth 1993; Ainsworth and Sened 1993; Browne and Paik 1993; Grier and Munger 1993; Heinz et al. 1993; Danielian and Page 1994; Grier, Munger, and Roberts 1994).

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In this resurgence, new research strategies are introduced and significant progress is being made in understanding interest-group influence and tactics. In spite of impressive theories, sophisticated statistical treatments, large-scale data collection, time-consuming observations, and inspired measurement techniques, however, contradictions abound in the new research on interest-group activities. Two types of differences in findings are especially important to note and to explain. First of all, recent quantitative assessments of group influence and activities concluding that groups often are unable to exert significant influence seem at odds with the more qualitative descriptions drawn from the literature on policy subsystems (compare, for example, Rothenberg 1992, 203–22 and Smith 1984 with Maass 1951; Cater 1964; Freeman 1965; Fritschler 1975; Bosso 1987; or Hansen 1991). Secondly, the recent literature is internally divided: studies of political action committees (PACs), for instance, sometimes note that contributions are related to congressional floor votes and sometimes find no such relationship. Our purpose here is not to call into question any particular finding or conclusion. Rather, we seek to understand the reasons for the contradictory state of the literature on interest-group activities that many others have noted (Berry 1989; Wright 1985; Evans 1986; Salisbury et al. 1987, 1992; Grenzke 1989; Hall and Wayman 1990; or Grier and Munger 1993).

Contradictions in a field of study may stem from the diverse and useful research methodologies being brought to bear on important new questions and, therefore, be a healthy sign of resurgence of interest in the topic. Or, contradictions may stem from incomplete models, inaccurate measurements, overgeneralizations from case studies, or inappropriate research designs. In the recent literature on lobbying and interest-group activities, we think that the second explanation for diversity of findings is more accurate than the first. In order to demonstrate what has led us to this conclusion, we focus on a prominent recent example of research on lobbying strategies conducted by two of the discipline's top scholars of the topic.

David Austen-Smith and John R. Wright (1994) propose a theory of counteractive lobbying. They contend that their "results suggest a substantially different interpretation of group influence over legislation from that which has dominated the field for the past 30 years" (1994, 26). Their findings provide little support for their assertions in spite of some impressive efforts at theory building, data collection, and statistical analysis. They study an unusual case, test an incomplete theory with imperfect measures, and commit a variety of other errors, as detailed below. The authors fail to place their findings in the context of previous studies of interest groups, but, most importantly, they reach conclusions far broader than those that could be supported with their empirical evidence.

Our article proceeds with three levels of discussion. First, we point to

specific problems of modeling and measurement in the Austen-Smith and Wright analysis. Second, we discuss a series of broader questions of methodological approach, particularly the choice of a one-shot cross-sectional variance design. This common research strategy has a variety of implications that are often overlooked when authors draw out the conclusions and discuss the implications of their studies. Finally, we conclude with a discussion of research strategies and findings on interest-group lobbying activities, attempting both to explain contradictions in the literature and to encourage ways of avoiding them.

### I. Problems of Modeling and Measurement

Austen-Smith and Wright (1994) present a model of strategic choice of lobbying targets by interest groups and test it with data from the case of the Senate battle over Judge Robert Bork's appointment to the United States Supreme Court. Using a data set indicating senators' stances toward previous Reagan nominees, other characteristics of each senator, and information on the lobbying activities of 75 interest groups, Austen-Smith and Wright claim to refute a prevailing wisdom that groups lobby legislators who already support them. They conclude instead that groups tend to lobby those who oppose them, and lobby their supporters primarily to counteract lobbying by other interest groups.

#### *An Overview of the Results of the Model*

The model Austen-Smith and Wright present involves two equations, the first modeling lobbying by pro-Bork groups and the second modeling lobbying by anti-Bork groups. Since both equations are parallel, let us consider the results of the model predicting the number of groups supporting Judge Bork that would contact a given senator. The predictive variables are: 1) a dichotomous measure of the senator's stance toward previous Reagan nominees; 2) a dummy variable for membership on the Judiciary Committee (by far the strongest predictor of contact in the model); 3) the senator's 1987 voting score, based on 11 votes meant to distinguish liberals from conservatives; 4) the organizational strength of pro-Bork groups in the senator's state; 5) the number of anti-Bork groups that contacted the senator; and 6) an interactive term meant to capture the counteractive nature of lobbying. This last variable is defined as the number of anti-Bork groups contacting the senator for those senators who were predisposed for Bork and zero for those who were predisposed against Bork (1994, 40).

From their analysis, the authors conclude (1994, 41) that "significantly more groups lobbied their 'friends' as the number of opposition groups lobbying their friends increased. Thus, the hypothesis of counteractive lobbying is supported by the data." A glance at the coefficients reported in

Table 2 shows that one would expect a baseline of 4.42 groups in favor of Bork to contact each senator. This would be lower by 2.36 if the senator was predisposed for the nominee; higher by 3.85 if the senator were a member of the relevant committee; lower by .06 for each score on an ideology scale for the senator (so a 100-point difference here would indicate a difference of six groups, with the most conservative senators hearing from a greater number of pro-Bork organizations); higher by .36 for each group that indicates they are strong in the senator's state; higher by .08 for each opposing group that contacted the senator; and higher by .12 for the counteractive lobbying variable.

This counteractive lobbying variable (with a standard error of .07) is reported as being statistically significant at the .10 level on a one-tailed test. While perhaps we should not quibble about levels of statistical significance, several problems remain: the model excludes indirect strategies of lobbying; the independent variables include substantial problems of multicollinearity (1994, 40, fn 14); and several of the variables are measured crudely or inaccurately. Further, these are the results of a two-stage least-squares (2SLS) model where similar problems are likely to have affected the estimates of the first-stage model. To conclude from these findings that one has documented an important new element of interest-group lobbying strategies is to stretch far beyond what the evidence will support.

Even if these problems were not inherent in the interpretation of the results presented, the substantive importance of the findings would still be questionable. A coefficient of 0.12 implies that in a battle between two well-organized coalitions of interest groups, one should expect an increase of one group making contact with a given senator predisposed to vote with a set of groups for every eight opposing groups contacting him or her. This hardly seems strong enough to justify an entire theory of "counteractive" lobbying, especially when the coefficients for committee membership, ideological affinity, and organizational strength all point toward a different interpretation of how groups target members.

### *Estimating Senators' Prior Probabilities of Support*

One of the most powerful and intriguing elements of the data set reported in the Austen-Smith and Wright article is their "unambiguous" and "exogenously determined" measure of each senator's expected vote on the Bork confirmation prior to any lobbying activity, compiled by "the organizations that actually did the lobbying" (1994, 37). The authors argue that this measure is especially fitting to their needs, much more so than a simple estimate based on previous voting patterns. After reviewing the possibility of creating a score based on "previous behavior and general voting patterns" the authors conclude (1994, 37, fn 7): "we decided to

use the ACU's head count in order to avoid any ambiguities in senators' priors."

Although it was compiled by the American Conservative Union, which helped lead the pro-Bork fight, this measure is neither unambiguous nor exogenous to the model. Rather, it is simply a vote score, as their source clearly indicates: "For two years Dan Casey had tracked the voting patterns of the Senate on judicial nominees. Working with ten roll call votes from recent years, Dan divided the Senate into four basic groups" (McGuigan and Weyrich 1990, 16). In other words, with the exception of the assignment of 11 freshmen senators, the prior support score amounts to the very thing that Austen-Smith and Wright avoid compiling themselves owing to the supposed superiority of the independent ACU judgment.

Austen-Smith and Wright compound the problems associated with the interpretation of this variable by collapsing it into a simple pro- and anti-Bork dichotomy and by ignoring the judgments that could have been used to create a six-point scale of expected support for Bork before any lobbying began. McGuigan and Weyrich make clear that the ACU had targeted individual senators, even those who scored in one of the extremes, as potentially open to persuasion (or in need of reinforcement). Of 43 senators coded "99% sure for the Reagan nominee," seven were given a mark indicating that they were "caveats." Of 34 senators coded "almost never votes right—can probably be written-off," again seven were marked as "worth our attention if the time and resources could be spared" (McGuigan and Weyrich 1990, 16). Clearly, the ACU used its analysis of previous voting patterns to identify likely fence-sitters to target for its subsequent lobbying efforts. Indeed, they sent their list of senators to 300 leaders and activists in their group, along with notes suggesting themes to bring up in their contacts with the senators (McGuigan and Weyrich 1990, 17). Counting the seven "questionable" senators from each of groups 1 and 4 as separate groups, the ACU prior expectation score could be used as a six-point scale: almost exactly what would be useful in determining whether other groups focused on allies, fence-sitters (as has been found by Rothenberg 1992 and Smith 1993), or adversaries for their lobbying efforts. Unfortunately, Austen-Smith and Wright collapse this six-fold variable into a simple dichotomy of pro- and anti-Bork, missing the opportunity to test their theory and some simple rival hypotheses in a much more straightforward manner.

### *A Theory of Simultaneous Counteractive Behavior*

Austen-Smith and Wright posit that groups *simultaneously* decide whether to lobby a given target (1994, 28), and their data give no indication about time ordering of any lobbying efforts (1994, 35, 37–8). Even though

the authors emphasize in the discussion of the model that counteraction is an equilibrium result, they interpret their findings as though lobbying decisions were made by groups in response to previous decisions by rival groups: "Groups lobbied their 'friends' . . . in response to lobbying by opposition groups. . . . Significantly more groups lobbied their 'friends' as the number of opposition groups lobbying their friends increased. Thus, the hypothesis of counteractive lobbying is supported by the data" (1994, 41).

Considering that the data were collected simultaneously, to state that these decisions were made "in response" to the behaviors of others is impossible. Certain senators could easily have become the targets of intense campaigns because of widely known uncertainties about their voting intentions, a simple rival hypothesis that is not clearly discounted by the data. Austen-Smith and Wright's analysis indicates that the number of groups that lobbied an "unfriendly" senator did not change in response to a change in the number of opposition groups lobbying, but this does not satisfactorily discount the fence-sitting hypothesis for several reasons.

First, 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 (1994, 34). In the model, this probability is dependent both on the legislator's prior beliefs about constituency sentiment,  $p$ , and the cost,  $c$ , of the legislator acquiring information firsthand. The empirical test of the model, however, includes no variable that would correspond to  $c$ , thus making any spontaneous counteractions dependent only on the legislator's prior beliefs. This differs little from a prediction that groups will focus on fence-sitters. Second, the measure used to control for the senators' ideological proclivities, the 1987 vote score, is not transformed in a manner that would allow it to differentiate among those who would likely be clearly in favor, clearly against, or on the fence. The appropriate data for this test were of course available to the authors in the form of the six-fold expected vote score from the ACU analysis as discussed above.

Austen-Smith and Wright's use of data collected simultaneously to test a theory of logically time-ordered behavior introduces unnecessary ambiguities into the results. In particular, it renders it difficult to distinguish, even in theory, between behavior aimed at fence-sitters and that aimed at counteraction. This problem could have been addressed with a different, longitudinal data set, or by making more complete use of the ACU data used in the existing data set to distinguish between these fence-sitting and counteractive hypotheses.

*The Unit of Analysis*

Austen-Smith and Wright base their study on a mail and telephone survey of 75 groups active in the Bork confirmation battle. The unit of analysis in the Austen-Smith and Wright article, however, is the senator, not the interest group. That is, Austen-Smith and Wright take the reports on the lobbying activities of each of their 75 groups and link them to each of 100 senators. Each senator is associated in the data set with a number of pro- and anti-Bork lobbying efforts. From this, we can see which senators became the object of intense lobbying and which were left relatively alone by forces on either side of the debate. The analysis allows for no inferences about which groups lobbied which senators, even though these data were collected and at many points the authors seem to imply that we can reach conclusions on this topic. Let us consider the effects of this transformation on one variable: organizational strength.

Groups were asked to rate their organizational strength on a three-point scale, based on their "members, supporters, and their level of activism" in each of the 50 states (1994, 38). Each group that responded strong or medium was coded a one for that state. Then, two subsequent variables were calculated by summing the organizational strength variables for all the groups for each senator, separately for the pro- and anti-Bork groups. We end up, then, with two scores for each senator. These indicate the number of groups passing a minimum level of organizational strength opposed to Bork or in favor of Bork in the senator's state. The transformation of a data set about the activities of groups to one about the characteristics of senators has many consequences.

First, no logical or necessary link insures, as the authors imply, that those groups that are individually most active or powerful in any particular state are the ones contacting the senators from that state. This would seem to matter tremendously because the model discussed in the Austen-Smith and Wright article (1994, 29-30) gives great importance to the ability of a group to give the senator electorally relevant information about constituency sentiment. A second ambiguity stems from the lack of a control for population in the organizational strength variable, making it likely that many groups will identify the largest states as those where they are the most active. From the senator's point of view, however, the point of comparison is not the other states, but the other groups active in his or her own state. In sum, the possibilities for misunderstanding and spuriousness are very strong after these data are transformed to the level of the senator. Although such a transformation may ease computation by increasing the sample size (from 75 groups to 100 senators), the theory calls for group-level analysis. Rather than analyzing group behavior, the statistical compar-



isons in the article are actually about coalitions of groups: a different question entirely than that discussed in the text.

### *Measuring Lobbying Activity*

The theoretical definition of lobbying used by Austen-Smith and Wright does not conform to the empirical definition of the concept used in the analysis. The question posed to interest-group leaders was: "We would like to know about your Washington lobbying/advocacy efforts on the Bork nomination to the Supreme Court. By Washington lobbying/advocacy, we mean direct contacts with the senator, in person or by phone, or contacts with the senator's staff. For each of the following senators, can you tell us roughly whether your organization's efforts were strong, medium, or weak or none at all?" (1994, 37). This question, of course, tells nothing about the substance of the information, if any, being transmitted, nor about the direction of information transmission. More importantly, it taps many activities that are not in the theory. Groups might have answered that they had made strong efforts because they received many requests for information or research from senators' offices, because they mobilized constituents to contact their senators, or for many other reasons not included in the particular definition of lobbying used by the authors in the discussion of their theoretical model. The authors point to this problem when they write (1994, 36):

We define lobbying very specifically, and somewhat narrowly, as the transmission of information directly to legislators in an effort to reinforce or change their policy positions. Organizations that filled out the questionnaires, however, may have had somewhat different notions of lobbying in mind. 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. . . .

The authors attempt to close the gap between their theory and their indicators by introducing a new variable, the senator's voting score. This score is given the heavy task of providing "a general control for influences on lobbying that may not be captured by our theoretical model or our conception of lobbying" (1994, 36). There is little explanation of how one can use a measure that does not correspond to the theory, then include a control variable allowing for contact with supporters, and conclude that this control variable is a measurement correction rather than a change in the model. The revised model now holds that groups contact their adversaries if one controls for contacting their friends. Much more straightforward would be

an empirical approach featuring measures that correspond directly to the theory.

An additional problem with the vote-score measure is that it seems to replicate in many ways the ACU variable also included in the model. Both are based on previous voting behavior, and even if not perfectly correlated, are likely to involve the same or related dimensions (see Jackson and Kingdon 1992, 809). Thus, despite the authors' assurance (1994, 38) that "some important differences" distinguish the two variables, they are essentially measuring the same thing. This makes questionable the theoretical need for both variables as well as the statistical interpretation of either.

### *Direct and Indirect Lobbying*

Not only is the measurement of lobbying activity faulty, as discussed above, but even the conceptual definition is incomplete. The definition of lobbying used by Austen-Smith and Wright corresponds neither to the literature that they claim to refute nor to the facts of the Bork case. Austen-Smith and Wright (1994, 36) define lobbying as "the transmission of information directly to legislators in an effort to reinforce or change their policy position." While the theoretical model thus explicitly excludes indirect methods of lobbying—such as media campaigns, mobilizing members, information-gathering, and using allied groups or legislators to contact other legislators—such methods of lobbying are included in the definitions of lobbying put forth in the "service bureaus" finding of Bauer, Pool, and Dexter (1963) and the "communications" school of Milbrath and others that the authors cite (Milbrath 1963; Matthews 1960; Zeigler 1964; Dexter 1969). For example, Bauer, Pool, and Dexter (1963, 357) found that interest groups were more likely to work indirectly through allied legislators and members of the business community than to make direct contacts with unfamiliar legislators. Even Milbrath (1963, 8), whose definition of lobbying seems closest to that used by Austen-Smith and Wright, allows for many forms of indirect strategies of lobbying. Milbrath defines lobbying as "the stimulation and transmission of a communication . . . directed at a governmental decision-maker with the hope of influencing his decision." That communication, however, can be either direct or indirect (Milbrath 1963, 211), and may include such things as testimony at hearings, approaches through constituents, letter-writing campaigns, and advertising. The findings of these scholars, whom Austen-Smith and Wright claim to contradict, are thus based on much broader measures of lobbying.

The exclusion of indirect lobbying activities is particularly troublesome in the Bork case. According to Barbara Sinclair (1989, 188), "senators opposed to Bork, together with their civil rights, civil liberties, and labor group allies, persuaded the public that Bork was dangerously outside the

mainstream on issues of civil rights and the right to privacy.” Other accounts of the Bork case generally agree with this assessment of groups and senators working together (Bronner 1989; Pertschuk and Schaetzel 1989; McGuigan and Weyrich 1990). The anti-Bork coalition explicitly divided its efforts between disseminating information through the media and consulting with allied senators (Pertschuk and Schaetzel 1989, 41, 98). Far from a one-on-one lobbying process with each legislator carefully picked for efforts at “information transmission,” the Bork fight, like many battles, included well-coordinated and relatively public efforts at mass persuasion.

An exclusive focus on the direct strategies of influence has the potential to cause misestimation of any model of those strategies. Wright (1990) has argued persuasively that groups tend to coordinate their financial contributions and lobbying activities. Studying the impact of PAC contributions without controlling for the fact that those who receive contributions are likely also to be the targets of lobbying efforts would have the effect of overestimating the apparent impact of the contributions, Wright correctly pointed out. If the logic holds for these behaviors, then certainly it also applies here and in similar cases. There are many reasons to expect groups to coordinate their direct and indirect lobbying strategies. The huge public campaigns to convince Americans that Judge Bork was outside of the legal mainstream were an integral part of the lobbying strategies of those groups opposed to his confirmation. Certainly they coordinated their direct lobbying activities with these indirect activities. If this is the case, however, then the unmeasured effects of variables left exogenous to the model will distort the measured effects of those made endogenous. That is, without including these indirect strategies, any estimates of the causes or the consequences of the direct strategies will be unreliable and biased.

### *Summary*

We think the results presented by Austen-Smith and Wright are largely uninterpretable because of the measurement and modeling errors discussed above. First, the coefficients presented provide little support for the “counteractive” argument because they are weak to begin with and are subject to a number of specification errors. Second, the measure of senators’ priors compiled by the ACU is, contrary to what is implied in the article, a vote score. This is problematic because a second vote score is already included in the empirical model. Third, because the ACU data have been made dichotomous and there is no measure of  $c$ , the cost of a legislator acquiring information, the evidence presented does not allow us to distinguish between counteractive lobbying and the rival hypothesis that groups lobby fence sitters. Fourth, data about groups have been aggregated to the level of the senator, making conclusions about the behavior of groups murky at

best. Fifth, the measure of lobbying used in the empirical model does not correspond to the theory, and adding a voting score to "correct" for that problem makes the results presented even more ambiguous. Finally, the definition of lobbying is incomplete in that it excludes indirect strategies, a particularly serious oversight in the Bork case—a case which featured indirect and grass-roots lobbying as an essential feature.

These problems are specific to the Austen-Smith and Wright analysis, and all but the first could have been corrected by using the full six-fold ACU measure, using a questionnaire that asked separately about indirect and direct lobbying activities, dropping the second vote score once the lobbying measure was improved, and analyzing at the level of the group rather than the senator. Even if these measurement and specification problems were corrected, however, there would remain several larger problems of research design that have become too common in studies of interest-group activities: a lack of attention to the dynamic and social nature of lobbying, and a failure to put what are essentially snapshot case studies in proper context. In the next section, we will discuss how these issues are illustrated in the Austen-Smith and Wright article before turning to a more general discussion.

## II. The Pitfalls of One-Shot Cross-Sectional Designs

Like many students of interest-group activities, Austen-Smith and Wright adopt a cross-sectional variance model of the process they seek to explain. Focusing on a single case, they attempt to explain how groups decide to target particular legislators for their lobbying efforts. This simple decision has many implications. Overlooking for the moment the impossible task of finding a "typical" case of lobbying activities, the cross-sectional variance approach obscures some important elements of the lobbying process. Most importantly, as the authors note (1994, 30), lobbying takes place over time and involves the creation and the maintenance of relationships of trust. Trust, reputations, threats, retaliation, and back-scratching are all important elements of the lobbying relationship, but they are not captured in a one-shot model.

In choosing to test a cross-sectional variance model of a single case of lobbying, Austen-Smith and Wright create conundrums out of behavior that would be expected in everyday life or in an iterated model. For example, Bauer, Pool, and Dexter's (1963) finding that groups act as information service bureaus for allied legislators may seem surprising if one adopts a cross-sectional model of the relations between groups and their targets. In the long run, however, groups would naturally attempt to maintain good relations with their allies, use indirect strategies of lobbying, counteract the lobbying strategies of opponents, and occasionally lobby those whom they

think might be persuaded. None of these activities are contradictory; further, no interest group would have to choose one to the exclusion of another, as Austen-Smith and Wright imply (1994, 26–7).<sup>1</sup>

Any number of studies will tell us that isolating a single movement out of an iterated process is likely to lead to dramatically different equilibrium outcomes (Trivers 1971; Axelrod 1981, 1984; Goodin 1984; Ostrom 1990; Hansen 1991). Further, many important elements of the policy process, such as cue-taking and the choice of legislative priorities, cannot be explained without explicit attention to the social and temporal context of lobbying. Heinz and his associates (1993) describe a Washington policy community where most actors spend large proportions of their time monitoring the activities of others (see also Browne and Paik 1993). If all the Washington policymakers are spending so much time monitoring the environment, and if they are all monitoring the same events, then their actions will be determined not independently, but often in rapid response to commonly perceived threats and opportunities as well as the actions of other groups (Wright 1990, 422–3). Longitudinal models with explicit attention to such cue-taking behavior can be inherently unstable (Bikhchandani, Hirshleifer, and Welch 1992), a point sorely missing in a cross-sectional design, and yet one that is central to the policy process (and to the Bork nomination in particular).

Models, such as that of Austen-Smith and Wright, that exclude the social nature of the lobbying game ignore that legislators often prefer to take voting cues from each other rather than from lobbyists (McFarland 1984; Schlozman and Tierney 1986; Kingdon 1989; Walker 1991). This can potentially be remedied by including discussion of indirect strategies of lobbying, but many cross-sectional analyses deal only with direct lobbying and ignore the social nature of the lobbying process. We should be careful about generalizing to the on-going relations among large numbers of actors on the basis of one-shot theories of individual behavior, especially in the case of lobbying: a highly social activity.

The cross-sectional approach also leads Austen-Smith and Wright to miss one of the most important questions associated with their case: Why Bork? For interest-group leaders anxious to conserve precious resources, the choice of senators to contact is less important than what to contact them

<sup>1</sup>To conclude that because groups *often* deal with their allies certainly does not imply that they *only* deal with their allies. Even Bauer, Pool, and Dexter (1963, 357), often cited to support this view, wrote: “We are not saying that pressure groups never approach persons opposed to their position or those who are undecided and ripe for conversion. . . . We are saying that they do these things far less often than one might think and that pressure groups are more likely to organize and stimulate other people to perform these activities.”

about. Groups want to win, and the remarkable characteristic of the Bork nomination is that many anti-Bork groups came to the conclusion that they could indeed win the battle.<sup>2</sup> Clearly, expectations of the behaviors of others matter, as can be seen in such works as Chong's (1991) description of the civil rights movement, Marwell and Oliver's (1993) description of the role of social groups in overcoming the collective action dilemma, or in Rothenberg's (1992) description of how Common Cause chose to focus its lobbying efforts on the MX missile program after concluding that its traditional priority of campaign reform was not going to be successful for a time. Rather than fight a losing battle with few allies, groups and individuals often prefer to join a winning coalition. Browne and Paik (1993) point out that members of Congress also prefer to devote their resources to those issues that have a good chance of being enacted. Clearly, all those involved are sensitive to their expectations of the behaviors of others. Modeling these questions out of our explanations of the process of lobbying is to shift our attention from the important to the trivial.

In summary, the simplest element of the research reported in this article, the choice of a cross-sectional variance model of a single case, has two important implications insufficiently recognized by the authors. The dual fallacies of the one-shot, cross-sectional approach are to isolate a single play out of an iterated game and to ignore the social nature of lobbying decisions. This research approach helps explain the contradictions often seen between quantitative cross-sectional studies and the traditional literature on groups (including Bauer, Pool, and Dexter 1963 and the literature on policy subsystems). Although seldom phrased in such terms, the traditional literature has used a multi-person and iterated model of the policy process. These studies describe a process where groups spend considerable amounts of time contacting and consulting with their allies in government. If, as Hansen (1991) argues, legislators do not ally with interest groups if the alliance seems to have little future, it becomes less surprising that most literature concluding that groups contact their friends (and seem to have a

<sup>2</sup>Anti-Bork groups far outnumbered pro-Bork groups. The former listed more than 300 groups among its members, while the coalition supporting Bork claimed membership of about 100 groups (McGuigan and Weyrich 1990, 213). Austen-Smith and Wright's data set includes 16 pro-Bork groups and 59 anti-Bork groups. Their analysis (1994, 39) indicates that senators most opposed to Bork would have heard from four pro-Bork organizations and from 30 groups opposed to the nominee. Similarly, those most in favor of the nominee heard on average from six groups who agreed with them and 18 groups opposed to the nominee. All types of senators, no matter their predispositions, heard from a greater number of Bork opponents than from Bork supporters. The differences in probability of contact that Austen-Smith and Wright report are swamped by the element of the debate that they overlook: the huge mobilization of groups on one side of the issue.

certain influence with them) has taken a longitudinal approach, while the studies concluding otherwise have been cross-sectional to the point of focusing on a single vote. This brings us to another inevitable feature of the one-shot cross-sectional variance model: the choice of a case from which to generalize.

There are good reasons to study the Bork nomination. The case was so controversial and so high on the national political agenda that senators were unusually well informed about its intricacies, and hundreds of interest groups were active in highly visible efforts to sway senators. The authors base much of their research on the results of a mail survey of groups active in the debate; had the debate been less salient, this survey would have been impossible to conduct. So in a way the case seems perfectly suited to analysis. On the other hand, the very characteristics that make the Bork case interesting are the same that make it a dangerous base for generalization.

Interest-group strategies are likely to differ depending on the degrees of conflict and public salience of an issue (Hayes 1978; Price 1978; for an application of this problem to recent work on PAC influence, see Evans 1986, 115–6). This means that a finding in a salient case like that of Bork should not necessarily be construed as directly contradicting a finding in a less salient case like that of the trade legislation studied by Bauer, Pool, and Dexter (1963). Why should we consider there to be a contradiction when we find that groups behave differently when dealing with legislation that generates minimal conflict and little public attention from when they deal with issues where conflict is intense and public? Bauer, Pool, and Dexter (1963, 117–9) pointed out, after all, that in spite of the importance of the trade policy question that they studied, fewer than half the businesses contacted felt strongly one way or another about the issue.

The choice of a public and conflictual case is common in lobbying studies, especially in those that have found limited group impact (Rothenberg 1992 or Smith 1984). Others emphasizing the greater impact of group activities have been more likely to focus on long-term and quieter aspects of groups' relations with government officials (Bentley 1908; Herring 1929; Griffith 1939; Maass 1951; Bauer, Pool, and Dexter 1963; Freeman 1965; McConnell 1967; Redford 1969; Fritschler 1975; Hamm 1983; Bosso 1987; Browne 1988, 1990; Hansen 1991; Thurber 1991). While influence is not the focus of Austen-Smith and Wright's article, 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. At a minimum, those who conduct studies based on highly public cases of lobbying should be careful in their generalizations not to imply that their results apply to

other types of cases or contradict the findings of others based on more consensual issues.

Not only was the Bork confirmation vote extremely conflictual and public (a growing trend in judicial nominations, as Austen-Smith and Wright note), but 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 (see Hall and Wayman 1990). Austen-Smith (1993) argued persuasively that group activities and influence should be expected to vary at different stages of the legislative process, and he cautioned that models of group influence should be careful to consider the different stages of the legislative process. Similarly, Austen-Smith and Wright (1992) argued that lobbying one's friends may be an effective and expected strategy during the agenda-setting stage of legislative decision-making. Considering these previous findings, it is surprising that the authors pay so little attention here to the consequences and the limitations of studying a case where the agenda-setting powers of groups are at their nadir.

By adopting a cross-sectional variance model of a continuing process, Austen-Smith and Wright inevitably distort the process they attempt to explain. While all models must necessarily involve simplification, the authors discount the degree to which the particular simplifications that they accept actually mislead. Lobbying is a social activity where groups pay careful attention to the behaviors of other groups (both rival and allied), where they have long-standing relations with their targets, where both indirect and direct strategies of influence are coordinated, and where the strategies chosen in one particular case may or may not be similar to those that will be chosen in the next. To the extent that these are important elements of the lobbying process, and we believe they are central, their exclusion from this model is more than a simple oversight. It renders the model so incomplete as to be misleading.

### **III. Explaining and Avoiding Contradictions in Interest-Group Studies**

The Austen-Smith and Wright article has many elements that make it the type that others may want to emulate: it has a rigorous theory, careful data collection, and an empirical test derived at least partially from the theory. It also, however, harbors many ambiguities that make the importance of its findings unclear. We can say with assurance that the current article will fit into a pattern of contradictory findings because it already does. Wright (1990) tested explicitly for counteractive behavior in a previous article and concluded that a direct model of lobbying strategies was



appropriate. In his study of congressional voting in which he controlled for party, ideology, constituency characteristics, financial contributions, and lobbying activities, Wright (1990, 423) wrote:

Finally, I assume . . . that decisions to lobby by groups on opposing sides of the issue are made independently; that is, lobby<sub>i</sub> does not depend on lobby<sub>a</sub>, and vice versa. This is a reasonable assumption if groups base their lobbying decisions primarily on jointly available exogenous cues such as partisanship and constituency and not on expectations about what their opponents might be doing.

Wright (1990, 436, fn 9) pointed out that his assumption of no counteractive behavior was based partly on the lack of available theory on the topic, but also that he “estimated an alternative specification in order to test this assumption and found only weak relationships between the two lobbying variables.” Of course, this is precisely the opposite of the counteractive logic used in the 1994 article with Austen-Smith, but neither the contradiction nor the possible reasons for the contradiction are discussed in the later article.

That the literature is contradictory does not necessarily imply that many studies are based on errors, nor does it necessarily mean that one study is wrong and another right. Rather, it points to the importance of noting the contradictions and determining the limits to each study’s generalizability. As Evans (1986, 115–6) has written, different studies reaching contrasting conclusions often differ in their usage of statistical controls or in their choice of conflictual versus consensual issues for analysis. First-order correlations often find, for example, that congressmen receiving money from dairy-industry PACs often vote with the industry. They may also, however, represent districts where dairy farming is prevalent, rendering the independent impact of the PAC contributions much less obvious. Since few studies use the same techniques, study a similar range of issues, or control for the same competing influences, their conclusions are often not comparable. Given this state of affairs, we should not be surprised by contradictions.

The full value of the accumulation of many studies of lobbying efforts will only be realized if researchers think carefully about the limits and the peculiarities of their own empirical and theoretical designs. The idea of counteractive lobbying, for instance, conforms with common sense. There is little reason to suspect that groups would not mobilize in response to the actions of rivals. But how often? In what situations? To what effect? Along with what other strategies? In order to interpret the results of a case study, the findings must be put into context by comparing them with findings from other studies. Studies in an area as rich as this one should not stand alone

or in contrast to a single work completed 30 years ago. Their results should be compared to those of others, and contradictions should not simply be pointed out as though to imply that a previous researcher got it wrong, but explained by reference to different methods, different context, different models, or other substantive and methodological concerns. Here is where we can see the difference between a cacophonous anarchy and a cumulative science.

How can a study be put in proper reference in order to encourage comparison? First of all, the literature to which the theory and findings are compared should be complete and fairly chosen. Outdated empirical reference points should be avoided. Conflictual and highly salient cases such as the Bork nomination should be compared with both similar and less salient issues from the literature in order to establish patterns of findings. Austen-Smith and Wright refer to Bauer, Pool, and Dexter (1963) and Milbrath (1963) as though little had changed in the United States interest-group system since those authors wrote their influential works. Jeffrey Berry (1984, 44) points out that many things have indeed changed. After summarizing the findings that Austen-Smith and Wright use, he writes: "Two decades later it is inconceivable that anyone doing research on any important public policy issue would find many congressmen who had heard nothing from interest group lobbyists or the constituents they represent back home." Similarly, in the second edition of his book, Berry points out: "Few lobbyists today see their role as strictly one of supplying information to their strongest supporters" (1989, 145; see also Tierney 1992, 218; Scholzman and Tierney 1986; Walker 1991; Heinz et al. 1993). In spite of dramatic changes in the Washington lobbying environment over the past generation, many authors still refer to historical findings as if they remained as empirically relevant as they do theoretically important today.

Although we have noted the benefits of longitudinal designs for capturing the iterative nature of interest group-government interactions, we do not mean to suggest that there is one best way to study interest-group activities. The role of interest groups can also be studied cross-sectionally across multiple decisions either within a national government (i.e., Kingdon 1984; Baumgartner 1989) or across states (i.e., Haider-Markel and Meier N.d.). At a minimum, however, whatever design is used must provide variance and a reasonable indication that the results obtained are not spurious. Just as important as these internal validity questions, the studies must be placed in context by comparison with a broad range of previously conducted studies, not only a narrow range of selectively chosen ones.

Considering that we are trying to explain iterated behavior within communities where each actor is highly sensitive to the behaviors of other actors, the current popularity of one-shot, independent-action models is cu-

rious. Certainly models of complex political processes must always involve simplification. Important results are indeed likely to come through the study of partial and incomplete theories focusing on particular elements of a larger process. In building these partial theories, however, we must remain aware that our generalizations should be just as limited as our simplifying assumptions are severe.

All models involve simplification, of course, and one can learn a great deal from some obviously incomplete models when their interpretation is handled with care. A serious problem arises, however, when a model is presented as being fully specified (or even reasonably close to being so), but when in fact it omits measures of crucial variables or includes measures that do not correspond to the theory. A well-specified model would allow us to tell if groups target their allies, other things being equal, or if this were merely a consequence of other strategies.<sup>3</sup> A model that includes some important variables but excludes others, on the other hand, will tell us very little. Variables will achieve or recede below levels of statistical significance based more on their variance in the sample and on their shared variance with other variables than because of their substantive importance.<sup>4</sup>

The conclusions that one reaches should be in proportion to the empirical evidence and to the limitations of one's model. Austen-Smith and Wright do not hesitate to draw broad conclusions concerning the role of lobbying in a legislative system in spite of the limitations of their study. Indeed, they argue that their model has particularly salutary implications about the roles of groups in a democracy, much like a model of perfect competition in an economic market. If lobbying is counteractive, they argue, and if legislators can even only occasionally check the facts, then any group that dissembles will be discovered and punished. "Thus, counteractive lobbying induces truthful behavior by both groups, and legislators will be better off—that is, make the correct decision in terms of their re-election

<sup>3</sup>The literature that Austen-Smith and Wright claim to contradict did not use any controls, but reported the simple bivariate relations. Bauer, Pool, and Dexter (1963) and others did not argue that groups lobby their allies when controlling for anything. To argue that groups target their friends only in response to anticipated contacts by their rivals, when controlling for the fact that they often contact their friends, is not to contradict the earlier first-order finding. The bivariate results Austen-Smith and Wright report (1994, 39) conform with those of the previous studies.

<sup>4</sup>Another feature of many recent cross-sectional variance lobbying studies is their use of multivariate statistical models with very small numbers of cases. Wright's (1990) analysis had 36 cases for most regressions; the current Austen-Smith and Wright analysis has 75 groups, and uses the 100 senators as a unit of analysis. (Further, the response rates make clear that problems of data collection in this area are severe.) The limited degrees of freedom available require extremely simple statistical models, models that are likely to exclude important variables as we have discussed in detail here.

chances—when counteractive lobbying takes place" (1994, 34). If the intention is to support such a serious and broad conclusion about the process of lobbying, then we should be careful to ensure that our models, our indicators, and the cases that we choose for study are accurate reflections of how the process operates. When we isolate small parts of a picture for careful theoretical treatment, we should also limit our theoretical conclusions to those that can be supported with a partial test.

If we can point to a single important question that should cause researchers to restrain themselves in their urge to generalize, it is this: is the model cross-sectional and individual, or is it iterative and social? Both types of models have important roles to play in helping us understand the lobbying activities of groups, and certainly the clarity of a partial model can allow us to isolate important elements of a broader theory of group behavior. Many ambiguities are created, however, when scholars use one type of model or evidence to justify conclusions that can logically be supported only with the other. Further, cross-sectional models of single cases pose much more serious problems of generalization than most authors admit. Should they choose a conflictual case or a consensual one? A highly salient case or a quiet one? A large-scale decision or a smaller one? Foreign policy or domestic? In sum, we can learn a tremendous amount from the accumulation of a variety of studies of interest-group behavior. This knowledge is more likely to come from a comparison of many existing and future studies than from any one in particular. In this process, we hope that researchers will clearly point out the limits of their own studies and avoid claiming that they have demonstrated something far broader than what has been shown. In the meantime it is up to readers and reviewers to take articles for what they are worth rather than for what they claim.

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