



Basic Interests: The Importance of Groups in Politics and Political Science.

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anxiety and defense of group privilege, or a conservative orientation to politics? To date there has not been an entirely satisfactory way to resolve such vexing and long-standing controversies. It is too much to expect this volume to resolve the controversy once and for all.

Nevertheless, some of the chapters do make a move in the direction of interdisciplinary discussion. For example, chapters 2 and 3 make incisive critiques of the psychologically based symbolic racism explanation, and show how it might be enriched by taking more explicit account of the changing fortunes of political and economic classes. It turns out that such psychological constructs as symbolic racism are in fact much more friendly to sociological interpretation than previously thought.

Perhaps it is reasonable for scholars to continue to divide along lines of discipline, with each discipline advancing its particular line of argument. If so, then certainly the sociological perspective, with its focus on the place of groups in the social structure and on the ways in which people make sense of their group position, requires as prominent a place in the literature as the other perspectives. As some of these chapters suggest, taking seriously the position of groups in society does not preclude paying attention to phenomena and explanations that other disciplines take to be important. In turn, scholars working in other disciplines would enrich their own work by taking into account the insights of the sociological tradition.

Frank R. Baumgartner and **Beth L. Leech**. *Basic Interests: The Importance of Groups in Politics and Political Science*. Princeton, NJ: Princeton University Press, 1998. 248 pp. \$15.95 (paper).

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Readers hoping to find original research about interest groups in this book are likely to be disappointed. Even though published by a major university press, this is not a research monograph complete with theory, hypotheses, and data. It instead is a review and critique of research by others in the field of interest groups over the past 50 years. What evidently accounts for the book's university press status is an effort by the authors to construct a theme and an argument about what constitutes good research in the field.

Baumgartner and Leech argue that the scholarly research on interest groups can usefully be divided into "areas of advance" and "areas of confusion." Areas of advance include research on mobilization and membership; descriptions of groups' lobbying tactics; studies of policy subsystems, issue networks, and coalitions; and cross-national studies of group-state relations. The area of confusion includes quantitative research on the impact of political action committee (PAC) contributions and lobbying activities on congressional policy. The literatures on PACs and lobbying

are “disappointing,” claim the authors, because there has been substantial growth in the amount of research in these areas, but little or no accumulation of knowledge. They claim that confusion and lack of accumulation result when scholars analyze only a few issues and groups at a time, fail to employ measures and definitions comparable with those used in other studies, conceptualize influence as occurring discretely rather than continuously through time, ignore cue-taking behavior among legislators, and do not account for “contextual” factors such as the salience of issues, the likelihood of winning, and degrees of conflict and consensus among groups. Only when scholars broaden their scope of inquiry and take these kinds of factors into account will contradiction and confusion decrease and knowledge accumulate.

Many of the claims put forth in this book are unconvincing; some because they lack sufficient evidence, and others because they fail to take alternative perspectives into account. The apparent confusion in the literature on PAC contributions and voting, for example, may have far less to do with research designs and model specification than with the possibility that the underlying relationship between money and votes is tenuous at best. One must suspect a tenuous relationship when statistical significance waxes and wanes as variables are shuffled into and out of the analysis, different measurements are employed, and different estimation techniques are applied. If PAC money were truly and generally a driving force in representatives’ policy decisions, then this fact should be plainly evident in study after study, despite analyses of different votes and different groups, different estimation techniques, minor differences in specification, and so forth.

Perhaps, as Baumgartner and Leech maintain, different research practices will reveal new and important insights about the connection between money and votes. This, however, is only a conjecture, and they offer neither evidence nor argument to render competing claims implausible. For this reason, I found this aspect of the book dogmatic. It is less a far-reaching and reasoned account of the state of research in the field than it is a limited appraisal advancing pet hypotheses that are never tested. Whether analyzing more groups, more issues, and more variables, as recommended by Baumgartner and Leech, would strengthen and clarify the research is an empirical question. Often, though, there is little to be gained from fine-tuning analyses where the fundamental relationships in question are tenuous at best.

The charge that research on PAC contributions and congressional voting is noncumulative is also narrowly conceived. An unusual aspect of research on money and voting is that because of the practical importance and popular concern about money in politics, null findings regularly make their way into the scholarly journals. In light of the general bias against publishing null findings, this fact alone makes it more likely that one will find contrasting findings in the literature, but it also makes for an informative research process. One gets a more accurate sense about the true reliability of reported relationships when null findings are disseminated rather than tossed into the files and forgotten. Moreover, contrary to what the

authors contend, comparability of research designs is not necessary for the accumulation of knowledge. Having many scholars simultaneously and independently hammering away at the data on PAC contributions and voting may not be neat and orderly, but it is undeniably an efficient way to gauge quickly the strength of the relationship between money and votes. Comparable or not, these disparate approaches have subjected the theoretically hypothesized relationship to quite a rigorous test of robustness.

Baumgartner and Leech also designate as an “area of confusion” those studies that evaluate the impact of groups’ congressional lobbying activities. Their evidence for confusion is based on findings from 14 studies that purportedly represent the universe of quantitative studies in “major” journals for which the effectiveness of lobbying can be evaluated. Their selection criteria, however, are highly arbitrary. Baumgartner and Leech offer no criteria or explanation for why this particular set of studies should make up the universe of relevant studies. Included among the 14 is one study in which the measure of lobbying activity is the AFL-CIO’s voting index of the percentage of time members of Congress voted as the AFL-CIO preferred (Kabashima and Sato 1986). If this study belongs in the relevant set, then so does every other study of congressional voting published over the past 20 years that includes a voting score as an explanatory variable.

Of the 14 studies, Baumgartner and Leech classify only two as demonstrating no influence of lobbying whatsoever. One of these, however, is clearly misclassified. In their analysis of lobbying on gun-control legislation, Langbein and Lotwis (1986, p. 424) report that direct congressional contacts by police groups and delegations were “highly significant in pushing members [of Congress] toward a gun-control position.” In the other study, the measure of lobbying activity is the percent of lobbyists registered with business associations, a measure that interest group scholars have long recognized to be fraught with problems. Thus, only one of the 14 relevant studies indicates that lobbying activities are ineffective; yet on the basis of this evidence Baumgartner and Leech contend that research on this topic contains a “maze of contradictions.”

In conclusion, this book, like most works that advocate a research agenda rather than execute one, ends where it should have begun. The authors provide numerous suggestions about how to advance the state of research on interest groups, and yet they are content simply to talk about what to do rather than do it. Why, if the purported dividends of their suggestions are so substantial, have they not conducted the analyses themselves? If the authors are unwilling to invest the time and effort to carry out their own research agenda, why should the rest of us?

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Colin Lacey and **David Longman**. *The Press as Public Educator: Cultures of Understanding, Cultures of Ignorance*. Luton: University of Luton Press, 1997. 228 pp.

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The authors, both members of an education faculty, are mainly concerned to advance an argument that the media in general have such a primary place in democratic societies as sources of information, awareness, and opinion that the media deserve to be treated in much the same way as the institution of education, which is almost universally regarded as too important to be left entirely to chance or to market forces and is nearly everywhere regulated or supervised. The book ends with a set of 10 recommendations, starting with the proposition that "newspapers should be recognized as major public assets with the acknowledged public function of informing and educating the public" (p. 204). To this end they also propose a national "regulatory body" to set and maintain standards and if need be issue operating licenses to newspapers in much the same way as television is licensed.

The body of the book comprises argument and evidence to support these proposals for gaining some public control over the informative functions of mass media. While mainly drawing on the situation in Britain, it aims to have a wider validity. The key steps in the argument draw on theory and research concerning the relation between mass media and public opinion. The starting point of the study is the work of Lippmann (1922) on propaganda and public opinion. The authors reflect on his pessimistic view of the manipulative tendencies of governments and propagandists, the power of the press and other forms of publicity, and the vulnerability and ignorance of the average citizen. Lippmann's hopes rested on the development of institutions devoted to improving the quality of information about social issues.

By way of Lippmann's concept of "manufacturing consent," the authors take on board Herman and Chomsky's (1988) Propaganda Model, which they find a convincing representation of how the media are currently working in Britain as well as in the United States. Lippmann's hopes for progress through increased education and rationality have not been fulfilled, in their view. The general public is still limited in the amount and quality of its information and in its powers of judgment. This leads to another foundation stone of the book, which is the view that, notwithstanding the near universal acceptance of liberal theory of the media, the free market has failed its audiences by delivering a weak, inade-