BASIC INTERESTS

THE IMPORTANCE OF GROUPS IN POLITICS AND IN POLITICAL SCIENCE

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and

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In Memory of Jack L. Walker, Jr.

CHAPTER 1

Progress and Confusion

SCHOLARS working in the area of organized interests in politics have made tremendous strides in the past two generations. Comparing the state of our knowledge in 1998 with that in 1948, for example, makes clear that our collective understanding of the roles of groups in politics has become considerably more complete, sophisticated, and accurate. We know more about the nature of political mobilization, about the political activities of organized interests, and about the contours of the group system, to mention a few areas of advance. Probably the most prominent example of progress is how far we have come in understanding the biases in mobilization that benefit certain types of groups, especially occupational ones, and discourage other types of potential groups from forming. Groups were once thought to spring naturally from society in response to disturbances, with little reference to any factors that might facilitate this process in some segments of society or inhibit it in others. David Truman's 1951 *Governmental Process* was rightly criticized for paying little attention to these issues, but his work represented the state of the art at the time. Today, two generations later, these issues are well recognized. The biases of mobilization contribute to a bias in the Washington interest-group community that has been amply and repeatedly documented over the past several decades. No understanding of the group system would even be attempted today that did not pay serious attention to these obstacles, but these were glossed over in the most prominent study of the topic in the early postwar period.

In this and other areas, scholars have made substantial progress in elaborating more complete, sophisticated, and nuanced views of the roles of groups in the political system. At the same time, however, serious gaps in our knowledge remain. While dramatic progress has been made in some areas of research on interest groups, other topics have either been ignored or have been the subject of inconclusive studies. We are not the first observers to note this unevenness. In this chapter, in fact, we rely heavily on the works of previous scholars who have reviewed the state of the interest-group literature in order to summarize our collective progress. We first look at areas that have proved quite fruitful, then turn to other areas of the literature that have either been avoided or have been investigated by many scholars while producing few conclusive results. The chapter concludes by drawing the lessons from these patterns of progress and confusion.

THE STATE OF THE LITERATURE

Virtually all those who have attempted to summarize the state of the literature in interest-group studies have noted certain areas where an accumulation of studies has led to real and important progress. A few of the most prominent areas of advance have included studies of the biases of mobilization; the collective-action dilemma; the occupational basis of most interest groups; the choice of direct and indirect lobbying tactics; the importance of long-term lobbying relations; the roles of groups in promoting new understandings of issues over the long run; the links between new social movements and the interest-group system; the effects of contextual factors such as laws, government subsidies, institutions, and patrons of group activities on group mobilization; and the roles of groups in elections, campaign finance, and the courts. On the other hand, most reviewers have paid closer attention to a series of problems. Published reviews have noted a long series of difficulties ranging from the choice of research topics to the theories and methods used to investigate them. In this section, we review the range of conclusions that other scholars have drawn in their efforts to state just what we do and do not know about the nature, activities, and effects of interest groups in American politics (for recent reviews, see Greenstone 1975; Salisbury 1975; Garson 1978; Knoke 1986; Schlozman and Tierney 1986; Walker 1991; Cigler 1991; Petracca 1992b; Heinz et al. 1993; Crotty, Schwartz, and Green 1994; Smith 1995; Baumgartner and Leech 1996a; Berry 1997).

In his review of the distribution of scholarly resources in political science in the early 1980s, Douglas Arnold singled out interest-group studies by noting: "Interest groups also seem to have attracted relatively little scholarly attention given their presumed importance. Here, surprisingly, the field is theory rich and data poor. . . ." In spite of a wealth of available theories, he continues, "there are relatively few empirical studies of how various groups operate politically" (Arnold 1982, 97). Arnold attributes some of these problems to the difficulties and expense of data collection: Whereas scholars in some areas can rely on the secondary analysis of large-scale data sets collected by others, interest-group studies require expensive, difficult, and time-consuming field work or original data collection (101). In the years since Arnold wrote, a vast outpouring of scholarly energy has transformed the field. Hundreds of studies have collected data on interest groups and their lobbying activities. The 1980s saw both a resurgence of large-scale surveys of interest-group behavior (see Schlozman and Tierney 1986; Knoke 1990a; Walker 1991; Heinz et al. 1993; Gray and Lowery 1996) and a tremendous number of smaller-scale investigations of the lobbying efforts of particular groups surrounding one or a few political decisions (see the works reviewed in Smith 1995; his bibliography lists 257 entries, and he focuses only on legislative lobbying). Since a lull during the 1960s and 1970s, a

resurgence has occurred in the study of interest groups, transforming the topic from one that was theory-rich but data-poor into one that is now rich on both counts. The 1980s constituted a period of rapid advance, at least in terms of the collection of vast amounts of new data on group activities.

Arnold was right to point out that scholars studying interest groups do not benefit from the large-scale and institutionally financed collection of data, as compared with other areas of political science, such as electoral behavior or international-conflict studies. Comparing how group scholars organize their research projects with how those in electoral behavior often do theirs is to compare an artisan working alone with a large corporation benefiting from a huge infrastructure. Even though we can note a great resurgence in data collection and empirical research into the roles of groups in politics, these projects have typically been of the scope that a single researcher could accomplish in a year with a modest budget. Exceptions include the studies by Walker, Knoke, Heinz and colleagues, Gray and Lowery, and a few others, as we will review in detail in chapter 8. Even Schlozman and Tierney's survey of groups was accomplished on a shoestring budget. One of the most prominent elements of empirical work on groups in the past few decades has been the modest scope of the projects.

Some of the largest collections of systematic data on groups come from the requirements of the Federal Election Campaign Act of 1971, as amended, or from congressional sources such as roll-call votes, interest-group rankings of members of Congress, and lists of witnesses at public hearings. These publicly available sources of information have led to massive literatures, to be reviewed in later chapters. This new and quantitative literature on groups shows that Arnold is right to point to the subsidization of research costs as an important determinant of scholarly agendas. The work that has stemmed from these studies has not been as coherent in its findings or as clear in its theoretical groundings as in other areas of political science because the data were not collected for a theoretical purpose, but rather constitute isolated bits of information without a set of complementary variables that would allow the systematic test of any particular theory. Contrast the collection of data on PAC contributions, to which the analyst must add a wide range of other bits of information, if possible, with the National Election Study, which is designed not to meet public demands of disclosure of a single piece of information but rather as a complete analytical tool in itself. In sum, interest-group scholars have not benefited from the collection of large, theoretically inspired data sets that would allow the field to base its research on a firm empirical foundation. Those data that have been collected, as in the areas of campaign contributions and roll-call votes, are simply not enough to test a complete theory of how groups behave. The burgeoning literature attempting to take advantage of these sources shows that scholars are willing to take advantage of publicly available sources of information and would likely make good use of more complete sets of information, were they

available. In any case, Arnold is right to point to the importance of subsidies and infrastructure in determining the growth of a field. Interest-group researchers typically work alone with little institutional support.

How much further advanced are interest-group studies in the 1990s than they were when Arnold wrote? Unfortunately, the consensus seems to be that the addition of vast amounts of new observational data has not led to a comparable increase in our understandings of the roles and impacts of groups in politics. The large-scale surveys of groups have led to a number of important and consistent findings about the mobilization strategies, lobbying tactics, and Washington activities of interest groups. On the other hand, there have been only a half-dozen such projects in recent decades. The more numerous small studies often are conducted in such a way as to hinder if not preclude comparison of results from one study to those of the next. A mixture of theoretical problems, measurement difficulties, the prevalence of the case study as the research design of choice, and other analytic shortcomings has rendered the development of a cumulative body of evidence an elusive goal.

One of the strengths of the literature on groups has always been the artisanal structure of the field. Scores of scholars have produced a great range of studies on important topics, sometimes from innovative theoretical perspectives. We will note in some detail below how research on the roles of groups in politics benefits from a methodological and theoretical eclecticism. The great diversity of research approaches has led to a number of insights. At the same time that we recognize the value of this methodological pluralism, it is important to see the potential for inefficiencies. With few shared data sources and with a diversity of research and theoretical approaches, scholars working alone often organize research projects that make an interesting new point but that cannot be compared directly with studies done by others because of subtle differences in measurement, theoretical questions, and empirical context. Diversity of approach must be balanced with some degree of shared theoretical perspective in order to produce a literature endowed with coherence and comparability.

In reviews of the literature conducted since Arnold's, scholars have been more likely to note the resurgence in data collection on groups than to complain about the paucity of data. Concern now focuses on the disparity between the degree of effort being expended and the scientific payoffs. In a substantial review of the political science and sociology literatures on interest groups published in the mid-1980s, David Knoke points to the beginnings of the resurgence of data collection. He concludes his review of the state of the literature in these terms:

> This brief review of the past decade's major research on American associations and interest groups reveals a diverse specialty that has continued to uncover interesting findings about these forms of social organization. The volume of factual knowledge at all levels of analysis has grown significantly. But association research as a field failed to achieve a sustained take-off into scientific maturity. Lacking consensus about the central issues and appropriate ways to study them, it remains a fragmented and unfocused enterprise at the margins of its parent disciplines. Sorely missing is an overarching paradigm that could crystallize attention and confer cachet upon the specialty. A fundamental theoretical goal must be to create coherence among the myriad empirical findings, particularly those bridging multiple levels of analysis from the individual, to the organizational, to the societal. (Knoke 1986, 17)

Knoke's assessment is echoed by virtually every political scientist who has attempted a significant review of this literature, as we will explain in some detail below. The literature may be divided into three areas: advance, avoidance, and confusion.

AREAS OF ADVANCE

In his review of the state of the literature on interest-group studies, Allan Cigler divided the literature into two parts: "demand aggregation" and "group impact" (1991, 100). The first set of studies cover those topics concerning how groups mobilize, how group leaders relate to their memberships, and how they recruit their members or otherwise maintain themselves financially. The second category covers what groups do, and to what effect, in the political arena. Cigler notes that almost all the areas of strength and progress can be put in the first category rather than in the second: We collectively know a great deal now about how many groups there are, how the diversity and bias of the Washington group system has changed over time, who joins groups and why, and how groups maintain themselves financially than we do about what groups do once they exist. In a later review, Cigler writes:

> I think it is fair to say that research on demand aggregation represents some of the most analytically and theoretically elegant scholarly work in all of political science. For example, the loosely integrated body of literature often referred to as incentive theory, ranging from formal models of the public choice theorists to the empirical tests of why and under what conditions individuals join groups, provides much insight into understanding collective action issues. (Cigler 1994, 32)

Among those who have reviewed the state of the literature, there seems a consensus that several areas deserve mention for significant advance. Within the broad area of what Cigler calls demand aggregation, we can note tremendous progress in elucidating the various processes of group mobilization, including work focusing on the individual's decision to join, the efforts

of group leaders to attract members, and the impact of social and institutional environment in facilitating the mobilization of some types of groups more than others. Within the broad area of group impact, there has also been considerable progress in documenting the structures of the Washington group system, noting the techniques of influence and access, and noting the different structures of relations among groups within various policy domains, issue-networks, and policy subsystems. Comparative studies of the relations between groups and government have led to many important findings, as have longitudinal studies of the efforts of groups to maintain access and generate favorable public policies over time.

There is no shortage of areas of advance; in this section we mention a few of the most prominent. We begin with those associated with the topic of mobilization and then consider some of those related to lobbying. Our choice of topics here should not be taken to indicate that any studies not mentioned are somehow not substantial. We focus here on broad bodies of research, not individual studies; many excellent individual studies are not mentioned merely for lack of space. Our notation of areas of advance certainly does not imply that all the important questions in these areas have been laid to rest. In subsequent chapters we will note some ambiguities even in the broad areas where advances have been substantial. Likewise, in our review of areas of contradiction below, we do not seek to be exhaustive, but rather to point to some general patterns.

There is no doubt that the various questions associated with the collective-action dilemma have been the focus of significant new findings, both regarding the biases in the group system and the many ways in which groups work around these problems (see, e.g., Olson 1965; Wilson 1973 [1995]; Hardin 1982; Ostrom 1990; Chong 1991). Research into this topic has been multi-layered, with significant progress in developing general theory, in testing components of the theory at the level of the individual who may be a potential member of an organization, in noting the roles of group leaders in attempting to recruit members, and in noting the importance of structural, institutional, and contextual factors affecting the mobilization of different types of groups. We will review significant portions of these literatures in chapters 4, 5, and 6. For the moment, it is worth noting that scholars have made a lot of progress and that they have worked at many different levels simultaneously.

The motivations for individual participation in the group system and the impact of this participation on the citizenry has been the subject of considerable research (see, e.g., Verba and Nie 1972; Verba, Schlozman, and Brady 1995). Survey research on this topic has focused on two important questions: What types of people are more likely to become active in the group system, and what effect does participation in groups have on a person's other political activities? Scholars have repeatedly documented both the social class

bias in group activities and the potential impact of participation in groups on an individual's subsequent political activities. We review this literature more substantially in chapter 5.

The various sources of financial support to which groups turn in their efforts to maintain themselves have become much better known over the decades. In contrast to early studies in the collective-action perspective, scholars rarely assume that mass memberships are the only, or necessarily even the most important, source of income for many interest groups (see Salisbury 1984; Walker 1991). The importance of occupations in structuring the group system has been largely confirmed, testimony to the importance of Olson's by-product theory of groups.

Scholars have developed much greater understandings of such topics as the roles of group entrepreneurs, of the importance of social movements, and of the roles of large institutional patrons of political action (see Salisbury 1969; Chong 1991; Walker 1991). Finally, the impacts of broad social, governmental, and environmental factors that foster the development of the group system have become much better understood (see, e.g., Salisbury 1984; Walker 1991; Gray and Lowery 1996). Groups do not develop in a social or legal vacuum; the contextual factors of American politics that promote the growth of some types of groups more than others is a topic that received little systematic attention two generations ago, but which is now the subject of some important studies.

A number of important studies have mapped out the structures of the national group system as a whole. Theories of group mobilization patterns have been developed, confirmed, and refined with the help of important empirical studies of the national group universe and the growth patterns of different types of groups (see, e.g., Schlozman and Tierney 1986; Walker 1991; Heinz et al. 1993). We review these studies more substantially in chapter 6; for now it is worth noting that their results have confirmed some elements of the theoretical work on the biases of mobilization—such as the advantage of business and of occupations generally—at the same time as they have led to further insights in other areas.

Studies of the Washington activities of groups have not only borne insights into the origins and maintenance of groups, but they have consistently documented a wide range of lobbying tactics and research activities that groups use in their attempts to affect policy outcomes (see, e.g., Milbrath 1963; Berry 1977; Schlozman and Tierney 1986; Walker 1991; Heinz et al. 1993). These studies have been supported by others focusing on the roles of groups within particular institutions of government, such as Congress (see Smith 1995), and in the courts, where the roles of groups have become an increasingly important part of the scholarly agenda (see for example Caldeira and Wright 1988, 1990; S. Olson 1990; Epstein and Rowland 1991).

Research into the policy activities of groups and the workings of Washington policy communities is one of the richest traditions in political science. Since the turn of the century, scholars have consistently paid attention to the structures and workings of various informal networks of policymakers. Research into policy subsystems, issue-networks, advocacy coalitions, and the like has expanded substantially over the past two generations. Our knowledge of the policy roles of groups has been expanded by substantial investigations of the activities of groups within single policy domains, such as Browne's studies of the agricultural policy domain (1988, 1990, 1995). Similar studies by Hansen (1991), laying out the development of close relations between agricultural interests and a supporting legislative coalition over seventy years, or by Bosso (1987) showing the impact of changes in the interest-group environment surrounding the use of pesticides over the postwar period, have paid important dividends. Massive studies into the structures of particular policy domains, such as those conducted by Laumann and Knoke (1987) or by Heinz and his colleagues (1993) have led to important improvements in how we understand the structures of Washington policy communities.

Many important policy studies have been done through a focus on a particular issue domain, but others have chosen to focus on a particular group or process. McFarland's study of the national experiment at negotiated compromise in settling environmental disputes in the energy field (1993) netted many new findings, such as the impact of internal constraints on groups' external lobbying positions. McFarland's (1984) and Rothenberg's (1992) studies of Common Cause have made that group familiar to everyone knowledgeable about the interest-group literature and have illustrated many important theories of how groups recruit members, how they induce them to remain, how groups decide on their legislative priorities, how they lobby, and to what effect.

Though our focus in this book is on American national politics, some of the most influential work on the roles of groups in policymaking has been in comparative politics, where the literature on corporatism, pluralism, and group-state relations has grown substantially since the 1970s (for a recent review, see Schmidt 1996). Scholarly concern with the relations between business, interest groups, and governments is likely only to grow as economic and trade pressures increasingly cause governments to work as the representatives of their national business communities in international disputes. Studies of the various forms that group-state relations may take, of the impact of the organization of national interest-group systems on governmental actions, and of the impacts of governmental organization on how groups themselves are organized, are likely to be of increasing concern to political scientists. Here, those interested in American politics may learn considerably from their colleagues studying similar ideas in other countries.

In contrast to their colleagues in American politics, those interested in the comparative study of groups in politics have almost always focused on the structures of relations between groups and government (for a few examples see Lijphart 1968; Schmitter 1974; Heclo 1974; Richardson and Jordon 1979; Lehmbruch and Schmitter 1982; Katzenstein 1985; Hall 1986; Baumgartner 1989; Wilsford 1991; Richardson 1993; Knoke et al. 1996; Schmidt 1996). Government structures, policies, and the international economic context affect not only how groups are organized, but how they relate to government officials, according to this large and growing literature. In comparative studies of groups, the focus is almost always on the relations between groups and government agencies. In American-only studies, groups often are studied in isolation.

All in all there are many areas of group behavior where our collective understandings in 1998 are substantially and unambiguously far advanced over what we knew in 1948. Two generations of scholarly research into the roles of groups in politics have paid some large dividends. This review of a select few areas of substantial advance points to several elements that the areas have in common. First, scholars work within a theoretical framework that, if it does not bind them all to the same perspective on what is important, it unites them at least with a common set of concerns. In that way, when one scholar adds a new insight, other scholars recognize its relevance for their work. This is most clear in the literature on mobilization. Second, many of the advances in the study of group lobbying behaviors have been noteworthy for their relatively large empirical scope. This is particularly the case with the Washington surveys of the 1980s and some of the most influential studies of particular policy domains. Finally, many important studies are especially careful in their attention to the context of group activities. Longitudinal studies of group activities within a particular policy domain have paid careful attention to the changing relationships between groups and their external context, especially government agencies. Comparative studies have typically treated the group-state relationship as the focus of attention, not the internal dynamics of the groups themselves. In these areas of progress, the points in common seem to be shared theoretical perspectives, a large empirical scope, and/or attention to context.

AREAS OF AVOIDANCE

There are two basic reasons for the existence of large areas of unexplored territory in the study of organized interests. The first is simply that new research questions have been posed at such a rate that scholars have not yet organized research projects to solve them. The second is that some important puzzles have not been solved, and scholars have not figured out the best

way to approach these issues. Gaps that stem from the first set of reasons are likely to be filled by the mere passage of time, but other areas will require new ways of thinking about the issue before progress can be made.

Gaps that result from the rapid accumulation of new research questions are a sign of a healthy and growing literature. What motivates private and public patrons of interest-group activity? What are the effects of this outside patronage on the freedom of action of interest-group leaders? What proportion of public-policy issues feature a one-sided versus a multi-sided constellation of interest groups? What proportion of these conflicts can be accounted for by the collective- versus selective-interest dichotomy implicit in the Olsonian perspective? How do the roles of interest groups differ when dealing with issues on and off of the national political agenda? How do groups redefine issues in order to achieve their lobbying goals? How do coalitions of groups decide on their legislative priorities for a given year? How are interests represented through the group system vicariously? How do contextual factors such as economic growth, direct and indirect government subsidies and regulations, and political conflict affect mobilization patterns of groups? What are the impacts of various group lobbying tactics, and in which circumstances are they most useful? How have new communications technologies affected these strategies? How can we reconcile the findings that groups spend much time monitoring their environment and working with their allies with an expectation that they would use limited resources to lobby the undecided? Recent research poses dozens if not hundreds of useful research questions to be addressed in the years to come. Certainly there are enough gaps in the literature for several generations of dissertation projects. Many of these are currently the subject of study; advances in these areas are likely as scholars continue their work.

Perhaps the single most remarkable feature of the literature on interest-group activities in Washington is the paucity of large-scale work. Some of the greatest advances in the past decade's research on groups have come from the few large-scale Washington surveys of groups that have been done. The works of Schlozman and Tierney, Walker, Heinz and colleagues, and Gray and Lowery have generated some of the most important insights into the roles and activities of groups in Washington and in the state capitals, and are certain to be cited for years to come. In spite of their prominence and impact, the examples that these authors set in conducting their large and respected projects have rarely been picked up by others. There remain, therefore, a great number of unaddressed empirical questions concerning such questions as the usefulness of common group tactics, the reactions of groups to changing technologies, and the impact on groups of changes in presidential administrations or partisan control of the Congress. In fact, given the paucity of large-scale survey work on the Washington interest-group community, our knowledge of any changes over time must be

pieced together little by little from a variety of sources, as we will review in chapter 6. Nothing impedes this research agenda but a lack of willingness and effort.

Research into interest-group activities expands rapidly every year. New research projects promise to answer many of the questions mentioned here, and doubtless will continue to do so. Gaps in our collective knowledge will undoubtedly be filled more easily in some areas than in others, however. We turn now to consider some areas of interest group studies where even the investment of massive scholarly resources has shown little collective benefit.

AREAS OF CONFUSION

Some issues in the study of interest groups have been avoided not out of a lack of time or effort but because of the inability of previous generations of scholars to generate positive conclusions despite the investment of tremendous energies. This is most clear in the area of power and influence. As we will review in chapter 3, the 1950s and 1960s were marked in both political science and sociology by vituperative and ultimately inconclusive debates about the distribution of power in society, with the literature on interest groups at the center of these debates. Because of these difficulties, and because of the multiple contradictions that previous generations created, a sensible reaction seemed to be to move on to other areas of research where conclusions could be better substantiated. One of the results of this has been that scholars have avoided some basic questions of political power, or have studied those questions in such circumscribed ways that their carefully designed studies can often not be generalized beyond the case on which their evidence is based. We will see in the next section that one of the most important ways in which scholars in the 1970s and 1980s attempted to be more scientific in the study of power and influence was to isolate particular cases for intensive analysis, but this approach has often led to its own set of contradictions rather than to clear conclusions.

Robert Salisbury (1994) has suggested that the problem here is not so much in the disagreements among scholars about how to measure power but rather in posing the question in the wrong way. The problems of the literature in the 1950s and 1960s may have been not so much the inability to measure influence, but rather a set of research questions that required this in the first place. As we will review in later chapters, the more recent and quantitative literature on groups has largely steered away from these matters. The literature on influence is an interesting example of avoidance based on a recognition that previous studies had mostly generated more smoke than fire, more debate than progress, more confusion than advance. Scholars may be right to avoid questions that cannot be answered, but then

again it would be preferable to rephrase the questions so that they could be answered. Salisbury suggests that the literature has suffered from a view of influence as a game, where clear winners and losers can be identified. The political process, he points out, is continuous, with no clear resolution and no identifiable end point at which to sort out winners from losers. Looking at the question in the wrong way, Salisbury assures, "is likely to generate more misunderstanding than insight" (1994, 18). His solution? Greater attention to the context of group behavior, among other things. We will discuss in later chapters how this might work. In any case, some areas of research into interest groups have been avoided not because of a lack of time or interest, but because previous work has been inconclusive.

At least two broad areas of the literature related to the study of influence can be cited for a collectively inconclusive nature in spite of great numbers of well-conducted individual studies. These involve the effects of political action committees (PACs), and quantitative analyses of the impact of lobbying on congressional votes. As many scholars have noted, these two areas of research have generated a wealth of new empirical work since the 1970s, but there has been little corresponding increase in knowledge.

Allan Cigler points to the literature on PAC contributions as an area where massive efforts have generated few collective benefits. Since the Federal Election Commission began collecting data on the contributions of these organizations to candidates and legislators in the mid-1970s, scores of studies have been designed to show the impact of these contributions in elections or in Congress. Concerning electoral outcomes, such a range of important variables besides PAC contributions are typically excluded from the analysis that the literature is inconclusive. "The availability of funding data has not automatically produced good research or clear results. PAC money is analytically difficult to separate from all other sources of money in terms of its impact on elections, and money itself is only one of many political resources in a campaign" (Cigler 1991, 113). Similarly, the literature on PAC effects on legislative voting is tremendously confusing, supporting only the most general conclusions. Cigler summarizes the literature this way:

> What is the effect of PAC money? When one turns to the research literature on the relationship of PAC contributions to congressional voting, one finds that it "is filled with ambiguity and apparent contradiction" (Wright 1985, 401). Studies dealing with such issues as the B-1 Bomber (Chappell 1982), minimum wage legislation (Silberman and Durden 1976), the debt limit, windfall profits tax, wage and price controls (Kau and Rubin 1982), trucking deregulation (Frendreis and Waterman 1985), legislation of interest to doctors and auto dealers ([K.] Brown 1983), and gun-control legislation (Cleiber, King, and Mahood 1987), have concluded that special interest money does appear to make a substantial difference. Others find no simple, direct relationship between contributions and issues such as the Chrysler loan guarantee program (Evans 1986) or dairy price supports (Welch 1982). Two studies, each of which examines a large number of issues, make virtually no common generalizations (Ginsberg and Green 1986; Grenzke 1989). (Cigler 1991, 116)

Richard Smith reviews hundreds of articles on group activities in Congress and is scathing in his criticisms. Not only do we have tentative and conflicting conclusions in those areas of research where little work has been done but we have a similarly inconclusive set of findings even in the most well-trodden paths (Smith 1995, 122–23). Smith reinforces the comments of Cigler. First, he notes that the literature presents an unjustified but consistent divide between those studies focusing on PAC contributions and those focusing on other lobbying activities. Almost all admit that contributions and lobbying activities are linked, but few design their projects in a way to accommodate the potential spuriousness thus created. Smith notes the results of these research projects:

> Consider first the scholarly work on the relationship between campaign contributions by interest groups and roll-call voting on the floors of the House and Senate. Over 35 studies have been published in recent years, and these studies have produced a literature filled with conflicting results. At one extreme are [the authors of eight studies] who report that interest group campaign contributions seem to be largely unrelated to the voting decisions of members of Congress.
>
> At the other extreme are [the authors of seventeen studies] who report statistically significant relationships between interest group campaign contributions and the voting decisions of members of Congress. . . .
>
> Between these two extremes are [the authors of twelve studies] who report more mixed results. . . .
>
> These conflicting findings are present whether one looks at the House or at the Senate, whether one looks at single votes, at indexes of votes on single issues, or at indexes of votes across several different issues (typically ADA or COPE scores). Conflicting results also occur regardless of whether one examines the contributions of a single interest group or the combined contributions of several interest groups, whether one enters contributions in nominal dollars, as a percent of total contributions received, or as the ratio of contributions compared to another source of contributions (. . .), and whether one analyzes contributions in linear or logarithmic form. (Smith 1995, 92–93)

When a literature burgeons but does not produce a set of comprehensible findings, important conceptual issues apparently remain unresolved. PAC contributions are not the only area where such contradictions are common. Lobbying activities in general have seen a range of conflicting results. Those who have conducted large-scale surveys of interest-group behaviors

have been remarkably consistent in their findings of what groups do, as we will discuss in chapter 8. However, those who have focused, as in the PAC literature, on one or a few cases at a time have generated a wealth of contradictions.

Groups are known to use a wide range of tactics and to focus sometimes on legislative allies and sometimes on fence-sitters or on opponents. Contradictions abound in this literature based on such questions as the type of issue being discussed, the degree of salience of the issue, the inclusion of controls in the statistical models employed, the validity and inclusiveness of the measurements used, the dynamic versus cross-sectional nature of the research design, and other factors. The literature is filled with such a diversity of theoretical and methodological approaches that we are left with a bewildering array of findings rather than a coherent set of results (see Baumgartner and Leech 1996a, 1996b). Richard Smith gives the same dim assessment of the literature on lobbying as he did of that on PAC contributions:

> Taken together, the recent theoretical developments about lobbying and persuasion and the empirical evidence about changes in the conduct of lobbying campaigns—especially the evidence about the emphasis on contacting undecided members of Congress—suggest that interest group lobbying should substantially affect the roll-call decisions of members of Congress, and should do so considerably more than conventional wisdom implies. But is this the case? Do the recent statistical studies of the relationship between lobbying and roll calls suggest a strong linkage? The answer is rather unclear. On first reading, the evidence from the statistical analyses is mixed, and suggests that the impact of lobbying depends on the presence or absence of a variety of conditions. A closer examination, however, suggests that there are methodological reasons to doubt all the statistical results, and hence the actual relationship between lobbying and voting remains obscure. (Smith 1995, 104)

PAC contributions and lobbying activities represent two areas where the publication of hundreds of studies in the past two decades has not generated the type of advance that one would hope, in contrast to the other areas of research on groups, such as that on collective action. Hugh Heclo wrote some time ago that the concept of the iron triangle was "not so much wrong as . . . disastrously incomplete" (1978, 88). Many of the works that make up the literatures discussed in this section may be evaluated in a similar manner: Each of them when taken individually is probably not so much wrong as it is profoundly disconnected from other studies on the same topic. This disconnection makes each study incomplete and potentially misleading, even if each is well done individually. Clearly, the mere accumulation of more studies over time does not inevitably lead to increases in knowledge.

There are at least three important lessons in the review of these two disappointing literatures. First is the impact of the availability of data sources.

The public availability of vast amounts of information concerning quantitative indicators of group activities such as campaign contributions seemed too good to pass up. Scores of political scientists went about correlating these figures with others in the hope of showing an important set of relationships. Many of these studies were individually well done, but in retrospect the literatures based on the exploitation of these data sources have been inconclusive. The mere availability of new data does not guarantee progress. Second, the inability of scholars to compare their disparate findings suggests that the impact of a literature may stem more from the interrelations among the studies that make it up than from the strengths of the individual studies themselves. Building in comparability is an important goal in any literature. Third, effort counts. To take publicly available information about a partial set of indicators and hope to add one or two pieces to the puzzle is a tempting research strategy. Unfortunately, it is no substitute for starting with a clear theoretical framework and going into the field to gather the information necessary to test the theory completely. Taken as a whole, the studies that make up the literatures reviewed in this section are remarkable for the modest efforts in research that they represent. (Two important exceptions to the generally inconclusive literature on PACs include Sorauf's 1992 review of campaign finance issues generally, and Gais's 1996 large-scale study of the unintended consequences of campaign finance reform efforts; these studies differ from others reviewed here in that they focus their attention much more clearly on a small range of questions and marshal a large amount of evidence based on many cases and several sources of data.)

A LITERATURE THAT GROWS BUT DOES NOT ACCUMULATE

Comparing the state of knowledge in areas of interest-group scholarship where much progress has been made with those where less progress has been apparent allows us to note whether more work inevitably and inexorably leads to greater collective knowledge. It is tempting to think that knowledge will expand by the simple accretion of greater numbers of empirical projects over time. A review of the literature suggests that such optimism is unfounded. As the previous sections have demonstrated, there are important areas in the literature where investments have paid off in better understandings; there are areas where not enough work has yet been done to answer the important questions; and there are large areas where few strong conclusions have been reached in spite of a great number of studies. How can a literature grow without accumulating? We would point to three causes: Theoretical incoherence, lack of comparability across studies that often comes from ignoring the context of group behavior, and the scope of the research effort.

Many who have reviewed the literature on groups have pointed to the great diversity of theoretical approaches being used in explaining how there can be such confusion. Diversity can be a strength, but it can lead to incoherence as well. Without a clear set of theoretical questions, the literature on interest groups has balkanized into such a number of small and unrelated areas that its collective impact is low. One recent book that epitomizes the disappointing cumulative impact of recent work on groups is that edited by Mark Petracca (1992a). Noting the "avalanche of new empirical data on various aspects of the interest group system" (1992b, xviii), the editor expected to summarize a new set of findings. What he found, instead, was "no single question and most certainly no single approach that unites the study of interest groups. Neither is there a unifying theory nor even set of theories to guide interest group research" (Petracca 1992a, 348). Reviewing the same volume, Lawrence Rothenberg notes that the book accurately reflects the state of the art:

> One . . . leaves the book with an understanding that the study of interest groups still has considerable room for growth. Reflecting the general state of organizational research, exactly how associations fit into the political world remains somewhat mysterious. In particular, what groups provide to politicians, the impact of organizations on public policy, and the relationship between group influence and the perpetuation of the group system are still issues open to debate. . . . While *The Politics of Interests* provides partial answers to such questions, it also illustrates that much work remains to be done. (Rothenberg 1993, 1167)

If, after these decades of work, how groups relate to government can be said to remain "somewhat mysterious," it is worth wondering how all that effort could better have been spent. One way to look at this question is to compare areas of advance with areas of confusion along the dimensions of theoretical coherence, context, and scope.

The literature on the dilemmas of mobilization has progressed notably from an initial focus on the calculus of joining from the perspective of the potential member of the group (M. Olson 1965), to an increased awareness of the potential roles of group leaders in offering services to these potential members, thereby altering the membership calculation (Salisbury 1969; see also Moe 1980a, 1980b; Rothenberg 1988, 1992), to consideration of the broader social environment within which groups operate and which affects their abilities to recruit (see Walker 1983, 1991), to a population ecology perspective on group mobilization that puts more emphasis on environmental factors than on the internal factors that had once been the only factors considered in the literature (see Gray and Lowery 1996). With Gray and Lowery's perspective on how groups mobilize, not only are we much further advanced from the days of Truman, who mostly ignored these dilemmas, but

indeed we are a long way from Olson's original focus on internal group dynamics. Theories of mobilization are an area of advance because the literature has progressively added new elements of theory and evidence at different levels. Though we do not yet observe a single unified theory in this area, we can see substantial progress in enunciating a perspective combining attention to individual behaviors, internal group dynamics, and the broader context of the group's social and political environment. The literature is vibrant and makes progress because many scholars work on different parts of the puzzle, but they all can see the relevance of the work of others. Progress is visible over the generations as successive theorists add new elements to the old.

Compare the cumulative nature of the literature on mobilization with the disjointed nature of the growing literature on lobbying. Working from many different theoretical perspectives, often taking advantage of small bits of publicly available data on one or a few cases of decision making, scholars often take refuge in the organization of very tightly organized case studies designed to address a limited theoretical question. To be sure, some large-scale work has been done. Considering the payoffs that have come from these few large studies, the continued willingness of scholars to organize small rather than large projects has been remarkable. As this proclivity for small projects is combined with a desire to address narrow theoretical questions based on case studies, this literature has grown without accumulating the record of progress that we can note in other areas of the study of groups.

The implications of the lack of a shared theoretical structure are much greater than scholars often realize. Many scholars hope to solve difficult problems of modeling group behavior by focusing on a limited theoretical question and using the tools of deductive analysis to isolate a set of behaviors to test a narrow theory. The hope, presumably, is that what the research projects suffer in generalizability will be offset by their internal coherence and by the internal coherence of other projects. To meet with success, this approach requires a level of comparability across research projects that is far from what we observe today. As scholars have increasingly followed the approach of limiting their research projects to narrow topics informed by a single and highly limited theoretical concern, the literature as a whole has shown less ability to generate a coherent picture of the political activities of groups. Each individual study may be well done, but it may use a slightly different set of indicators, a particular definition of key terms different from those used by others, or a precise model of external forces that makes it incomparable to other studies, even if these other studies would seem at first glance to be closely related. We will review this problem in some detail in chapter 2. Diversity of the type we observe there produces not an accumulation of results merging into an increasingly complete perspective but rather an incoherent cacophony of incomparable findings.

One potential route for improvement is the adoption of a more explicit deductive theoretical approach to the study of lobbying behaviors. This is, after all, what provided much of the impetus for improvements in the area of group mobilization. In an extensive review of the literature in economics relating to interest groups, William Mitchell and Michael Munger point to this problem in the political science literature. "In general, deductive theory has not provided the hypotheses and explanations offered by political science in studying interests. Accordingly, we have a vast and factually rich body of data but one that is analytically incoherent" (1991, 513). Their suggestion that economic models of groups may provide some of the theoretical structure that the literature needs fits with experience in the area of collective action, where the Olsonian dilemmas have indeed produced a range of important findings. Mancur Olson himself has described the problems of the literature on pluralism as stemming from its lack of deductive structure, making scientific progress impossible and guaranteeing the demise of the literature (1986, 166).

Many have noted the lack of a single structure, and many have proposed the adoption of a set of deductive approaches drawn from economics, as is implicit in Mitchell and Munger's review of the literature in that area. Interest-group research remains captivated by no single approach, however. On the contrary, as Cigler notes, "theoretical and methodological diversity is the hallmark of the interest group subfield" (1991, 125). David Knoke goes much further than Cigler, and makes clear that diversity has its costs:

> Research on associations expanded steadily during the decade. However, its surface diversity and richness mask the field's underlying anarchy. Put bluntly, association research remains a largely unintegrated set of disparate findings, in dire need of a compelling theory to force greater coherence upon the enterprise. Without a common agreement about central concepts, problems, explanations, and analytic tools, students of associations and interest groups seem destined to leave their subject in scientific immaturity. (Knoke 1986, 2)

A healthy diversity of theories, methods, and approaches to the study of group relations with government is sure to lead to an accumulation of important findings. However, a chaotic set of unrelated perspectives that do not produce comparable findings hinders the accumulation of knowledge. In looking at the development of the literature in the past generation, as we have begun in this chapter, we note that the literature suffers more from incoherence and chaos than it benefits from diversity. To be sure, not all areas of the literature are equally afflicted: Some show relative theoretical clarity and scientific progress. Other areas, however, have seen such a disjuncture between effort and progress that some serious questions should be posed. We pose such questions in the chapters to come and suggest some ways in which research projects can be organized to produce a literature whose constituent parts can be compared with each other.

There is not likely ever to be a single theoretical perspective to the study of groups in politics. Groups do many things, and depending on what elements of group activities they want to understand, scholars in the field will make use of a variety of theories. Progress will come from an increased willingness to be explicit in our theoretical perspectives, from clear statements of the limits of our chosen theoretical perspectives, from concerted efforts to build on the findings of others (even those working from slightly different theoretical perspectives), from constructing projects that are much larger in scope than is often the case today, and from ensuring the comparability of our research findings by paying more careful attention to the contexts of group behaviors. The accumulation of hundreds of small studies will not lead to a coherent literature if those studies cannot be compared with each other. In contrasting a few areas where progress has been apparent with some areas where more substantial problems remain, we find that attention to context, scope of the research effort, and theoretical relevance are the keys to accumulation.

CHAPTER 2

Barriers to Accumulation

SOME LITERATURES accumulate new knowledge and others merely grow larger. We saw in the previous chapter that some parts of the literature on interest groups have developed into relatively cumulative enterprises where scholars build on the works of others and collectively reach some important conclusions. In other areas, we noted a troubling tendency for the body of accumulated findings to grow larger and larger without generating a series of coherent and well-confirmed conclusions. One of the barriers to effective accumulation is the lack of a shared vocabulary. Interest-group studies benefit from the contributions of scholars in several disciplines operating from a great diversity of theoretical approaches. This diversity lends promise to the study of groups, but the lack of a shared vocabulary creates the risk that scholars might speak past each other. Scholars may hope that they are filling in small parts of a big picture, to be completed as others add to the collective work. Still, the troubling possibility remains that each may be toiling independently on a separate canvas never to be completed or even picked up by another. This chapter focuses on this lack of a shared vocabulary in order to show how the resulting confusion may constitute an important barrier to the accumulation of knowledge. These problems affect certain areas of interest-group research much more severely than others, of course, which helps explain the uneven patterns of progress pointed out in chapter 1.

For each topic discussed in this chapter, we describe how scholars working from different theoretical perspectives assign different meanings to the same terms, use different conventions on important elements of research design, and attach different levels of importance to different elements of their research projects. Each perspective, of course, offers a different set of findings. When it comes time to compare and perhaps to consolidate these diverse findings, we often find that subtle differences in vocabulary hinder the development of a body of evidence, arguments, and conclusions that would be directly comparable. The discussion in this chapter is kept relatively general; the themes introduced here are picked up in greater detail in later chapters.

WHAT IS AN INTEREST?

Defining an interest and an interest group would seem a prerequisite for a fruitful research program in this area, but it has yet to be done. Robert Salisbury shows some displeasure with what he sees as a trend toward the adoption of increasingly narrow definitions of interests and interest groups in recent years. In one of the most influential articles on interest groups of the 1980s, he had to remind scholars that many organizations that we think of as interest groups are not membership organizations but are institutions such as cities, local governments, universities, corporations, and hospitals (Salisbury 1984). More recently, he noted that we gain little from an overly restrictive definition of interest groups:

> The intellectual domain of the student of interest groups cannot be restricted to voluntary associations. . . . [O]ur scope must include every active unit, from the isolated individual to the most complex coalition of organizations . . . that engages in interest-based activity relative to the process of making public policy. I recognize that this constitutes a supremely imperial conception of our field. So be it. What should we leave out? What organizations and/or active individuals fail to qualify? I see no need to restrict our jurisdiction in advance and much reason to be ready to incorporate more rather than less organizational variety. Indeed, it seems clear to me that our research heretofore has suffered more from omissions than from too expansive a notion of what to include. Let us not be reluctant to extend our reach in the future. (1994, 17)

Salisbury's remarks are designed to encourage grander and potentially more influential research projects. They reflect an increasing feeling that many studies have been defined so narrowly that they have lost much of their interest for the broader profession. Salisbury encourages the revival of a research tradition of long ago. In the early years of the century, scholars were much more likely to adopt extremely encompassing definitions. Sometimes, the groups in question were formal organizations that would today be called interest groups, but in many cases the definitions were more amorphous. An interest group might be an occupational or demographic category such as consumers or farmers—not necessarily membership organizations and not necessarily formal organizations at all. Bentley, for instance, defines a group as any subsection of society "acting, or tending toward action" (1908, 211). He argues: "There is no group without its interest. An interest . . . is the equivalent of a group. . . . The group and the interest are not separate." David Truman, for his part, defined an interest group as "any group that, on the basis of one or more shared attitudes, makes certain claims upon other groups in society" (1951, 33). Truman's distinction between latent interests, which might be mobilized if sufficiently threatened, and manifest groups, which have an actual organizational presence, implies that interests are real even if unmobilized. Such broad conceptions of interests remain common in the economics literature, where broad groups such as consumers and taxpayers are often included in models of political influence (see Buchanan and Tullock 1962; Tullock 1967, 1988; Niskanen 1971; Stigler 1971, 1972, 1974; Posner 1974; Peltzman 1976; Becker 1983, 1985; Mueller and Murrell 1986;

for a review see Mitchell and Munger 1991). Social psychologists and others interested in voting behavior often use similarly broad definitions, analyzing the behaviors of such "groups" as ethnic minorities, women, and the elderly (see, e.g., Paolino 1995). Interests, for many, are defined by one's social or demographic position.

A solid tradition in the literature attempts to define interests in objective terms: Working-class people have certain interests with respect to tax rates, job growth, and other issues; wealthy people have different interests with respect to these issues. Kay Lehman Schlozman and John Tierney discuss the use of objective definitions of interest, and note the prominence of economic motivations in these definitions. For interests that cannot be defined in economic terms, such as those involved in the abortion debate, the issue of capital punishment, or reforms in the electoral system, it is difficult to impute any interest on the basis of observable social characteristics, so the definition of interests must be subjective. They review a series of problems in defining exactly what we mean by an interest: The degree to which individuals are motivated by different and potentially conflicting objective and subjective interests; the importance of material versus nonmaterial incentives for creating interests; the concept of support for the public interest; and the problem of weighing differential intensities of interest, for example (see Schlozman and Tierney 1986, chap. 2).

Interests may be felt for a variety of reasons. Some feelings, however intense, may not be interests, but mere preferences unrelated to public policy. John Heinz, Edward Laumann, Robert Nelson, and Robert Salisbury give the following definition of interests:

> It is at the intersection of public policy and the wants and values of private actors that we discover interests. What we call the interests of the groups are not simply valued conditions or goals, such as material riches, moral well-being, or symbolic satisfaction. It is only as these are affected, potentially or in fact, by public policy, by the actions of authoritative public officials, that the valued ends are transformed into political interests that can be sought or opposed by interest groups. . . . This means that, in analyzing what interest groups do and with what effect, the very conception or definition of a group must be framed in terms of the public policy goals and objectives it seeks. If we are adequately to understand how groups function, it is necessary to study them in the context of policy. We cannot abstract groups from the substance of their interests without losing touch with what defines those groups. (Heinz et al. 1993, 24–25)

According to these authors, then, interests are only created when private values come into contact with government. The same values may not be interests if they have no relation with government action. The profit motive, religious beliefs, desire to achieve some public end, or views on any social issue, then, are not in themselves "interests," but become so only when

those who share them make demands on government. As the scope of government has grown, therefore, many values have become interests, and many private organizations have become interest groups. According to Salisbury, Heinz, and colleagues, we cannot define the term *interest* without reference to government.

Scholars have used a variety of definitions of interests over the years. Each individual definition may make sense, but such a range of different definitions are used that the literature provides little guidance for the researcher attempting to reach a simple decision on what is an interest. Empirical research on groups has often used much more narrow definitions than might be expected, considering some of the conceptual work on interests just discussed. Schlozman and Tierney, like several other scholars, use a published directory of organizations active in Washington as their working definition of an interest group, even though their review at the conceptual level includes a much greater range of potential definitions of an interest.

WHAT IS AN INTEREST GROUP?

If scholars have achieved no consensus on what an interest is, there should be no surprise that they do not agree on what an interest group is. Defining an interest group seems to be simpler than defining an interest because many people think they can recognize an interest group when it attempts to influence government. Still, a variety of definitions are used, and the differences among them are substantial.

David Knoke provides the sociologist's definition: "A minimal definition of an association is 'a formally organized named group, most of whose members—whether persons or organizations—are not financially recompensed for their participation.' . . . Whenever associations attempt to influence governmental decisions, they are acting as interest groups" (1986, 2). The sociologist hopes to distinguish associations and interest groups from such primary groups as the family, the corporation, and the bureaucracy. Sociologists typically define the field of voluntary associations separately from those of mass behavior or organizational dynamics, which explains the distinction in the first part of Knoke's definition. An association is different from a corporation or a bureaucracy because its members are not paid; it is different from a family because membership is voluntary. The second part of Knoke's definition defines when an association becomes an interest group: whenever it attempts to influence government, it is an interest group, just as for Heinz and colleagues. Whether Knoke would extend his definition of interest groups to other types of organizations that attempt to influence government decisions is unclear. In any case, sociologists typically begin with a definition of groups that takes the voluntary association as the base, then extend

from there. As a result, they typically pay less attention to corporations, law firms, cities, and other organizations that are often involved in lobbying.

Economists and social psychologists interested in voting behavior often use extremely vague or highly abstract definitions of interest groups in their efforts to understand the workings of the electorate. Rebecca Morton gives an example of this type of analysis when she turns to the concept of groups in order to solve the paradox of voting. If each individual should know that the probability of affecting the outcome of an election is so small as to render irrational the act of voting, certain groups within the electorate may be large enough that their collective votes would matter. Therefore, perhaps, "the group provides a private benefit to the individual to induce voter turnout in the group's interest" (Morton 1991, 760). So the concept of groups may be useful in solving a puzzle in the economic analysis of voting: Incorporating the roles of groups "is a desirable approach to analyzing voting behavior" (774). But what is the concept of groups? For Morton, "it is assumed that individuals are divided into m mutually exclusive groups in which members of each group have identical policy preferences within each group" (763; see also Morton 1987). The economist's definition of a group clearly has little in common with the sociologist's view, but it is a widely adopted definition in that field.

Like Morton, Carole Uhlaner turns to the roles of groups in her attempt to solve the puzzle of voting. Models of voting should note that individuals exist within a social structure, she notes. "There exist group affiliations and layers of intermediary elites between politicians and potential participators" (1989, 391; cf. Huckfeldt and Sprague 1992, 1995). What is Uhlaner's conception of a group?

> A polity contains the set of all voters and a set of all candidates. For purposes of this model, the voters are divided into groups. By "group" we mean loose connections of individuals who identify with each other when they relate themselves to political life and who retain this identity over some extended period of time (so group membership has stability). Such groups need not correspond to organized interest groups; formal structure is not necessary. On the other hand, the groups must have enough structure so that one could identify leaders as distinct from ordinary members. Many "reference groups" are groups in the sense used here. (Uhlaner 1989, 396)

John Turner uses a similar concept of group membership. These definitions of groups make clear that a group exists to the extent that people think it exists. No action is necessary; only thought: "A social group can be defined as two or more individuals who share a common social identification of themselves or, which is nearly the same thing, perceive themselves to be members of the same social category" (Turner 1982, 15).

For many economists, voting analysts, and social psychologists, then, a group is defined within the mind of the potential member of that group. To the extent that people believe they have shared identifications or shared interests, they are members of a group. To the extent that these shared interests lead to similar behavior in the political realm, such as voting, then these groups can be considered to be interest groups.

A definition of interest groups often used in the economics literature identifies groups by objective interests imputed by the researcher. Empirically based studies in the economics tradition typically focus on labor unions, business organizations, political action committees, and other formal organizations, whereas conceptual works typically use demographic or social groups as their definitions of interest groups (see Mitchell and Munger 1991 and the works reviewed there). As mentioned in the previous section, social psychologists often use definitions of groups related to a person's inclusion in some demographic category, generally requiring no formal membership. For journalists, interest groups are law firms, corporations, coalitions, public relations firms, and individual lobbyists active in the policy process.

Among political scientists, definitions vary widely. Those operating in the pressure-group tradition were more likely to use a restrictive definition of groups focusing on active governmental lobbying. For them, groups were corporations, industries, and hired lobbyists (see, for example, Crawford's 1939 book, *The Pressure Boys*). David Truman (1951), of course, made the distinction between active groups and latent interests, where latent interests exist in society but have yet to be mobilized into an organizational form. Pendleton Herring (1929) discussed those organizations with Washington representation, including private firms as well as membership groups. V. O. Key's (1964) definition was broad enough to include associations as well as economic interests such as firms, utility companies, and the like. In general, those writing before 1965 tended to use a definition that might be thought of as whomever one sees in Washington. A lobbyist might be a hired representative, the employee of a private corporation or public institution, or the representative of an organization with or without members. In this view, an organization could be considered an interest group when it had a lobbyist.

As political scientists became more concerned with the dynamics of organizational mobilization in the wake of Mancur Olson's 1965 *Logic of Collective Action*, they became more likely to use something closer to the sociologist's definition. Groups became synonymous with membership organizations. Here, the literature divided between those wanting to study the new questions of organizational maintenance and collective action, and those in the pressure group tradition who remained interested in lobbying per se. Jack Walker attempted to bridge this gap, but his work was greatly influenced by the state of the literature in the 1970s, when he designed his

study. He describes his working definition: "My principal focus is limited to functioning associations in the United States that are open to membership and are concerned with some aspects of public policy at the national level" (Walker 1991, 4). This definition is very similar to that used by Knoke, above. Many types of organizations that others would think of as interest groups are excluded from this definition: Walker points out that it does not include corporations, public affairs and public relations firms, law firms and other lobbyists for hire; government institutions such as cities, states, and foreign governments; private and public institutions such as hospitals and universities, foundations, and philanthropic organizations; and many other entities that sometimes behave as lobbyists or otherwise become active in issues of public concern (Walker 1991, 5–6). Walker noted a number of important complications in the makeup of this universe of membership groups, such as the roles of institutions as members and the effects of large government and private patrons in shaping the interest-group system, but limiting his sample to membership groups had important implications for his ability to generalize about the interest-group system as a whole.

Salisbury reminded the profession that its increasing focus on membership organizations was distracting it from the important roles played by nonmembership organizations in policy making. His 1984 article focused on the roles of institutions in lobbying. Within public administration, a long tradition has noted the importance of the "intergovernmental lobby" made up of cities, counties, states, and other governmental institutions as they attempt to influence the federal government (see for example Farkas 1971; Haider 1974; Commisa 1995).

In their large study Schlozman and Tierney (1986, 10) are careful to use the term "organized interests" because they want to include organizations such as corporations, hospitals, and others that do not have members, as well as membership organizations. Like Walker, they use a published directory to locate those interests active in Washington. Heinz and colleagues (1993) similarly are broad in their definition of interest groups, basing their interviews with Washington insiders on a combination of published lists, media reports, government hearings, and interviews with government officials about who was involved in various decisions. Hrebenar and Thomas, who have compiled four edited volumes outlining interest-group activities in each of the fifty states (1987, 1992, 1993a, 1993b), instruct their contributers to reserve the use of "interest group" to associations of individuals or organizations who attempt to influence public policy; they include businesses within this definition (1993a, 363). Virginia Gray and David Lowery use a broad definition of groups, including institutions, membership associations, and other types or organizations that were registered to lobby in their large-scale survey of interest communities in six states (1996).

No matter what the conceptual definition of interest groups that an author might want to use, those interested in large-scale investigations have consistently noted that there are few alternatives to consulting some sort of published list of groups. As the authors of each of the major surveys of interest groups conducted in the 1980s found out, establishing a definition of groups, and then getting a list of the full set from which to sample, is no easy task. Most empirical projects rely on published directories, legislative registrations, financial disclosure forms, testimony before congressional committees or in the rule-making process, or on some other published list. (See, e.g., Schlozman and Tierney 1986; Sabatier 1988; Hall and Wayman 1990; Knoke 1990a; Walker 1991; Jenkins-Smith, St. Clair, and Woods 1991; Baumgartner and Jones 1993; Golden 1995; Salisbury 1995; Gray and Lowery 1996; Hojnacki 1997. Cf. Browne 1995, who uses a reputational approach rather than published lists, and Heinz et al. 1993, who use a combination of approaches.)

All in all, one can note at least the following types of definitions used by various scholars interested in the roles of interest groups in politics:

social or demographic categories of the population
membership organizations
any set of individuals with similar beliefs, identifications, or interests
social movements
lobbyists registered in legislatures
political action committees
participants in rule-making or legislative hearings
institutions, including corporations and government agencies
coalitions of organizations and institutions
prominent individuals acting as political entrepreneurs or lobbyists

The diversity of definitions represents an underlying diversity of theoretical concerns. Different scholars are trying to explain different things about interest groups, and therefore define their tasks differently. Ambiguity of reactions toward interest groups and lobbyists is not a scholarly affectation: it is widely shared in the public and among politicians. Lawmakers have adopted a wide range of definitions in their efforts to regulate or control interest-group activities. Edgar Lane (1964) reviews the periodic efforts that legislators have made to control "the lobby problem" in the states and at the national level. Some definitions of lobbying equated the practice with bribery; others were so vague that almost all activities were included. The results of these ambiguities have been an ineffectual series of laws, either because they are hopelessly restrictive infringements on free speech or because they are so narrow that only a few rare behaviors are affected. Neither legislators nor scholars have devised an all-purpose definition of interest groups. This

confusion explains not only a chaotic set of research findings but also a generally ineffectual set of laws.

Exactly how one defines an interest group can have important implications for one's findings. For example, a 1995 study of participation on federal rule making (Golden 1995) relied upon lists of those who intervened in a sample of rule-making procedures rather than sampling from a list of interests generally present in Washington. Golden's research showed a much greater range of participants than had typically been noted by others before her. How one defines an interest group is no mere detail. It can affect one's conclusions about the diversity of interests present in Washington as well as other important questions. Most seriously, however, the great variety of definitions used by different scholars makes it difficult to compare their results. When one scholar finds greater diversity of interests present in a policy area than another had found, is that because there is greater diversity or because the two scholars used different definitions or sampling frames? This problem is not limited only to the concepts of interests and interest groups. It extends much further into a series of basic concepts, including the concept of membership.

WHAT IS MEMBERSHIP?

Just as the number of definitions of groups used in the literature is high, so too is the number of different conceptions of membership. For some, belonging to a particular social or ethnic class is enough; for others, a commitment of time and energy is necessary. Sometimes it requires only a psychological commitment or feeling of solidarity; in other cases, it means formal membership complete with dues and attendance at meetings; and in still other studies, it means only contributions of time or money. Dramatically different estimates of overall levels of membership in mass surveys naturally result from these different definitions, as has been repeatedly found in the literature in American and in comparative politics for the past forty years. Wide variation in estimates of public involvement result from variations in question wordings, question orders, and other elements of survey design. Some scholars have made estimates of American public involvement as low as 36 percent; others have given figures from 80 to 90 percent (compare, e.g., Wright and Hyman 1958; Babchuck and Booth 1969; Curtis 1971; Curtis, Grabb, and Baer 1992). Baumgartner and Walker (1988) review some of the difficulties in ascertaining membership in groups in a survey of the public (see also Schlozman, Verba, and Brady 1995). In the most recent and exhaustive study of its type, Verba, Schlozman, and Brady (1995) report higher levels of participation in groups than others before them, partly because they use a more expansive definition of group involvement. Increasingly, as

groups make use of new fund-raising and mobilization techniques, they blur some common distinctions between members and nonmembers. Debates about the degree of public participation in the group system are often obscured by different definitions of interest groups and membership.

When discussing membership issues, scholars often adopt the perspective of the hypothetical citizens' group: members are predominantly individuals, and anyone may join for a nominal fee. Most groups, however, are not Common Cause. Even most environmental and public interest organizations limit their reliance on individual memberships by diversifying their sources of income. Christopher Bosso reports, for example, that the mean revenues coming from membership dues in a sample of large environmental organizations in the late 1980s was only 32 percent (1995, 107). Similarly, he reports that even among the major environmental organizations, only a few rely predominantly on funds from individuals to meet their annual revenue needs: grants from foundations and other sources of institutional support are increasingly important (108). With the rise of a variety of new fund-raising and political mobilization techniques, groups with access to large mailing and phone lists often reach well beyond their own memberships in their appeals for support or political action.

In adopting the perspective of the group relying on individuals for their membership support, scholars can address many important questions relating to how groups overcome the collective-action dilemma, the extent to which the group system is representative of the citizenry, and the impact of group involvement on individual feelings of political efficacy. Kenneth Goldstein (1995) reports, for example, that the single most powerful predictor of whether an individual will contact their representative in Congress is having been asked to do so by an organization. Clearly, individual members are important to groups, and groups have important impacts on individuals.

Individual members are the basis for much group activity, but most interest groups do not have only individuals as members, many groups have strict rules restricting the list of people who may join, and many interest groups have no members at all. A large part of the group universe is made up of organizations whose members are corporations, cities, hospitals, or some other type of organization. Others have strict limits on who may join, as is commonly the case among professional associations. These organizations often face quite different problems and opportunities for member development than those faced by the hypothetical citizens' group dependent on a large number of small dues-payments. The U.S. Chamber of Commerce, for example, has a complex membership structure allowing direct membership to individual people, corporations, state and local chapters, and other disparate units (see Walker 1991; Salisbury 1995). The Association of American Universities limits its membership only to the fifty or so largest research universities, thus distinguishing itself from other higher education

organizations such as the National Association of State Universities and Land Grant Colleges. Neither group has any individuals as members. In any case, no single definition of membership could be applicable to all types of interest groups. As in the case of the term *interest groups* just described, it is clear that no single definition of "membership" is likely to fit in all cases, adding further difficulties to comparison across studies when scholars are not careful in comparing only apples to apples.

Why limit a study of interest groups to a study of membership organizations? The main reason would seem to be a desire to address the dilemmas of collective action presented by Olson or to resolve some of the questions of how membership groups survive and prosper. As with all such decisions, this focus comes with some costs as well as some benefits. For those interested in how groups interact with government, there is no reason to limit the focus only to groups open to members. If one hopes to focus on lobbying, influence, and the power of organizations in Washington, certainly one would not want to define interest groups in such a way as to exclude General Motors from discussions of automobile and tariff policies, or AT&T, Netscape Communications, and Microsoft from discussions of telecommunications policies. This is exactly what many scholars have done, however, because of their preoccupation with membership mobilization as the most important analytic question of the 1980s.

A second limitation of many studies of membership organizations has been the belated discovery that even most membership organizations do not rely primarily on the contribution of dues from their rank and file. Membership dues constitute only a fraction of the means of support for the typical interest group. Even those groups with few material incentives or selective benefits to offer rely on a small number of loyalists for the bulk of their support. Professional fund-raisers routinely speak in terms of a "pyramid of support," in which a small number of very large gifts are expected, along with smaller contributions from larger numbers of members. In his primer for nonprofit managers charged with fund-raising, Peter Edles includes a section entitled "Expect the most money from the least people." He notes that in the typical fund-raising campaign, 80 to 90 percent of the support should be expected from 10 to 20 percent of the contributors (1993, 11). Any university development officer would corroborate these expectations. Why expect that interest groups would rely on a flat dues structure when fund-raising experience makes it clear that a wide range of levels of participation would be expected? Slowly, better understandings of the complexities of membership are coming into the literature. Walker noted the prevalence of split dues structures and the existence of "member patrons"; Bosso noted the limited reliance even of environmental groups on dues; Edles reported the accepted lore of the fund-raising industry.

Membership is a deceptively complicated concept. A focus on the dilemmas of membership recruitment has led interest-group scholars to study certain types of organizations more than their numbers warrant and to adopt a view of the typical group that relies more heavily on individual membership than is often the case. Depending on the theoretical issue being addressed and the type of organization being studied, different definitions of membership are appropriate. As with other areas of the literature on interest groups, these definitional ambiguities make it surprisingly hard to read a series of studies and answer some simple questions, such as the extent of public involvement in the group system or the number of members in various social movements. *Membership*, *interests*, and *interest groups* are complicated terms to define. So too is *lobbying*.

WHAT IS LOBBYING?

The word *lobbying* has seldom been used the same way twice by those studying the topic. Lester Milbrath introduced his discussion of the topic by noting that "the words 'lobbyist' and 'lobbying' have meanings so varied that use of them almost inevitably leads to misunderstanding" (1963, 7). Robert Salisbury has written that we should avoid calling interest-group activities "lobbying" at all: "That much-abused word is so fraught with ordinary language meaning, most of it unsavory, as to defy rehabilitation anyway, but it is also true that none of its historic uses comfortably fits what many Washington representatives do" (1983, 71). If one were to compare the daily schedule of a lobbyist with the definitions of lobbying often used in the literature, one would find that much of what lobbyists do is not really at "lobbying" at all. Here we get to the problem of how to deal with indirect lobbying, research and data-gathering, and efforts to monitor what the government is doing. Lobbyists spend much of their time doing such things, but should we call those lobbying? The key point is that, whatever we call these activities, or however we decide to limit our definition of lobbying, we should be clear about what we mean, and we should compare findings in the literature based on similar usages of the term. The careless comparison of unlike definitions of the term generates considerable confusion. Unfortunately, the literature is home to a great variety of definitions of lobbying, and scholars are often tempted to compare the incomparable.

The word *lobby* originally referred to the entry hall in the British House of Commons, where those who were not members of government could meet those who were and plead their case. Thus "lobbying" has been used most often to refer to face-to-face individual meetings between legislators and representatives of an interest. In its most literal form, the word would

not even include testimony before Congress. Most interest-group scholars have used a broader definition. "Lobbying" has been used to refer to interest-group contacts in the bureaucracy, the office of the president, and the courts, as well as within the legislature. It has been used to describe grassroots campaigns, use of the mass media, and the creation of research reports, as well as face-to-face contacts. The common thread is that all of these activities must be used in an effort to influence the policy process for them to be called "lobbying."

Milbrath (1963, 7–8) was quite explicit in his explanation of how he reached a definition of lobbying. First, he wrote, lobbying must involve governmental decisions not private ones. Pressuring General Motors to increase minority hiring is not lobbying. Asking the government to pressure General Motors to increase minority hiring is lobbying. Second, lobbying must involve the intent to affect government decisions. Activities that have an impact, but are not necessarily intended to have an impact, are not lobbying. A scientist who discovers a clear link between smoking and cancer would undoubtedly have an effect on government decisions; however, her actions would not necessarily be lobbying. Third, lobbying goes through an intermediary, according to Milbrath. For this author, a citizen directly expressing his own opinion is not lobbying; lobbying occurs only when a group argues on behalf of someone else. (Milbrath sees the potential difficulty with this argument, since voting is the expression of a message often intended to affect government decisions, but we would gain little from a definition that expanded lobbying to include even the act of voting.) Finally, there must be an act of communication. Based on this explanation, Milbrath then arrives at his definition of lobbying: "Lobbying is the stimulation and transmission of a communication, by someone other than a citizen acting on his own behalf, directed to a governmental decision-maker with the hope of influencing his decision" (1963, 8).

The federal Lobbying Disclosure Act of 1995 defines lobbying activities as "lobbying contacts and efforts in support of such contacts, including preparation and planning activities, research and other background work that is intended, at the time it is performed, for use in contacts, and coordination with the lobbying activities of others." It defines lobbying contacts as "any oral or written communication . . . to a covered executive branch official or a covered legislative branch official that is made on behalf of a client with regard to (I) the formulation, modification, or adoption of Federal legislation . . . [or] of a Federal rule, regulation, Executive order, or any other program, policy, or position of the United States Government . . . [or] the administration or execution of a Federal program or policy (including . . . a Federal contract, grant, loan, permit, or license); or the nomination or confirmation of a person for a position subject to confirmation by the Senate." "Covered officials," in the terms of the legislation, are those in a policymaking or policy-advocating position, including congressional and presidential staff. This definition specifically excludes official testimony before Congress, indirect lobbying through the media, filings before the court (like amicus briefs), provision of information requested by the official, and responses to requests for comments published in the Federal Register.

While all would agree that contacting members of Congress and asking them to vote in a particular way is an act of lobbying, many other activities are subject to differing interpretations. Maintaining regular contact, good relations, and easy access to decision makers may involve only occasional efforts to sway votes, for example. Similarly, working to enhance the public image of an industry or a profession is generally not included in definitions of lobbying, but it can be an important source of power. Among the many ambiguities in defining lobbying, the following set of questions offers a sample: Does providing information for legislative allies to use in debate qualify? Working with allies outside of government? Recruiting a particularly influential ally within government to do most of one's lobbying? Keeping one's ties to that person a secret? Conducting membership or public education campaigns without directly asking those contacted to engage in specific lobbying activities? Providing information to a prominent member, who may be a university president, for example, and who in turn may contact a decision maker? Leaking information to journalists? Engaging in legal research for later use in law suits or amicus briefs? Monitoring the activities of government agencies in order to know what decisions may be under discussion? Contributing money to reelection campaigns or to political parties?

Studies of lobbying differ in the degree to which they adopt general or precise definitions. In the literature on policymaking in general, the more informal approach is more common; in the quantitative literature, of course, more precise operationalizations are necessary. The broad surveys of interest-group activities conducted by Schlozman and Tierney (1986), Knoke (1990a), Walker (1991), and Heinz and colleagues (1993) have consistently shown that groups use a tremendous range of tactics in various situations, and this finding is generally corroborated in the informal and case-study literature as well. We will review these findings in some detail in chapters 7 and 8. The range of tactics that groups use in their lobbying efforts is quite broad and has consistently been documented by every major survey of groups in recent decades. Whereas those conducting the broad surveys of groups typically find a great variety of lobbying activities being used, those conducting different types of studies often focus only on one or a few of these lobbying tactics.

Scholars show no consensus on what they mean by lobbying. For some, it represents the range of activities groups engage in as they attempt to maintain links with government officials. For others, it can be so narrowly defined that even many direct contacts with members of Congress would not be

included. As scholars have attempted to design more systematic and quantitative research projects, they have shown an increasing tendency to use very precise and often incomplete definitions of lobbying.

As in the other areas discussed so far in this chapter, there is no particular problem with a focus on the use of particular tactic of lobbying. Difficulties ensue when scholars using distinct and sometimes conflicting definitions attempt to compare findings that may not be comparable. In the literature on lobbying, this has often occurred. Economists sometimes discuss lobbying in terms that equate efforts to influence legislators with giving PAC contributions, on the assumption that those who contribute to particular legislators probably also lobby them. Within political science, the emphasis has been on measuring the impact of PACs as distinct from explicit lobbying contacts. Such conflicting definitions and research approaches guarantee that the substantive conclusions about determinants and consequences of lobbying will be difficult to compare.

WHAT IS INFLUENCE?

Scholars have long attempted to observe and document the exercise of influence in politics. They have yet to succeed. Anecdotes abound in the academic and popular literatures about how a particular vote or decision was manufactured by the skillful exertion of pressure. Never have scholars been able to organize a systematic study that would demonstrate the influence of any particular lobbyist when controlling for all rival factors that might have also affected the decision, however. Scholars continue to design their research projects around a premise that they will be able to "explain" votes or decisions by isolating all the forces acting on the decision makers, even though the process has not worked in the past.

In the last section, we reviewed some of the problems with overly precise and incomplete definitions of lobbying that are common in much of the literature. The appeal of these definitions has to do with a desire to isolate the particular forms of lobbying behavior that most closely resemble "pressure" in the hopes of linking these to subsequent decisions. As Robert Salisbury (1994) has pointed out, the hope to isolate and measure influence is the common thread that links together much of the work on interest groups in recent decades. Almost every study of PACs and lobbying eventually gets around to the linkage between the efforts or contributions of the lobbyists and the votes taken by the targets, Salisbury notes.

The general approach in much of the lobbying and PAC contribution literature is to estimate a baseline of expected behavior using a set of measured variables such as ideological predisposition, district interest, committee assignments, and party, then to ascribe any deviation in voting patterns to whatever lobbying activities might have been measured. Of course the approach requires that all relevant factors be included in the model. This is particularly difficult when dealing with such concepts of what a vote of a member of Congress might have been in the absence of any lobbying or when attempting to insure that all relevant facts that might have influenced the member's decision have been identified and measured. In the absence of a fully specified model, the scholar may hope that the effects of any unmeasured variables will be small and unbiased on average. When taken as a whole, however, the literature attempting to demonstrate these linkages between lobbying and subsequent decisions is characterized by a tremendous lack of consensus given the amount of work that has been done, as we noted in chapter 1 and as we will review in more detail in chapter 7. More important than any individual problems of model specification or measurement, the literature is organized around a chimera. Scholars have long attempted to isolate and measure the exertion of power and influence. In chapter 3 we will review their efforts in the 1950s and 1960s and note how they eventually gave up in the wake of vituperative and inconclusive debates. Rather than learn from this experience, after a brief lull when fewer lobbying studies were done, scholars have returned to the same doomed research idea. Many recent studies have been designed around the false premise that we can observe the actions of influence and power. There is little reason to organize a project on the chimerical promise of measuring the unmeasurable.

One recent approach to the concept of influence is to specify the conditions under which lobbyists would be influential under a set of assumptions concerning the information they control, the information government decision makers would like to have, and the behaviors of rival interests. These "signaling models" of group influence have been much more successful in devising interesting theoretical findings than in demonstrating the empirical usefulness of the approach, however (see, e.g., Ainsworth 1993; Ainsworth and Sened 1993; Austen-Smith 1993). To ascertain the validity of these models, one day scholars will have to solve the problem of how to measure whether these behaviors had the impact that the models predict. More important than only the problem of measuring lobbying is simultaneously measuring a variety of other variables that would allow one to consider a range of rival explanations for the same outcomes.

We can say a lot about the tactics and strategies of lobbyists without discussing influence in particular cases. Spectacular cases of pressure may make for interesting reading, but much of the important work in lobbying is in setting the agenda, in defining the alternatives for decision makers, in gathering evidence, and in convincing others that certain types of evidence

are germane to the decision at hand. To the extent that we attempt to define influence narrowly and in the context of a single decision, we inevitably fail in two ways. First, the models are rarely specified fully and are therefore doomed to fail. (This is especially true of the empirical models as opposed to the formal specifications, which are sometimes more complete.) Second, we are led to the adoption of overly narrow definitions of what lobbying is and how influence is wielded in politics. Most importantly, it leads us to study lobbying only at the very last stage of the decision-making process. Ironically, many scholars agree that this is where the possible exertion of influence is at its lowest point. Why search for influence where it is least likely to be found?

WHAT IS AN ISSUE?

To the extent that interest-group studies are linked with the policy process, they share a problem common to all policy studies (see Greenberg et al. 1977). That is, there is no clear way to determine when an issue or a policy conflict has begun or ended. Three fundamental research problems flow from this simple fact. First, there can be no universe of issues from which to sample. Second, there is no single definition of an issue when issues are easily aggregated into large and interrelated groups or broken down into minute clauses, as constantly occurs in the policy process. Third, issues rise and fall on the political agenda over time, being transformed and redefined in the process. There are no apparent solutions to the difficulties created by the fluid nature of issues in the political process. Scholars must be sensitive to the ambiguities of the concept of an issue if they hope to avoid the confusion that inevitably stems from using similar terms to mean different things.

Issues never begin and they never end. Particular controversies may dominate discussion during certain periods, but policymakers know that issues never disappear forever. Most scholars react to the fluid nature of the issues with which they deal by limiting their consideration of an issue to how it is defined at a particular point in time, often in relation to a particular vote or decision by a certain government agency. This approach allows the identification of a list of participants, the observation of efforts to affect the outcome of the debate, and sometimes the identification of winners and losers. Issues are constantly being redefined in government, however, and when one debate is lost it is often not long before the same issue rises again in another forum. This may be one reason why studies of particular controversies often seem to reach conclusions at odds with those where the author investigates an issue-area over long periods of time. The second approach allows the identification of long-lasting patterns of activity, whereas the first attempts to present a snapshot of an evolving process.

The second ambiguity associated with the definition of an issue is that issues may be aggregated in many different ways, none of which guarantees that the results of a given study will be representative of all issues facing the political system at any particular time. Any given issue is typically part of a broader set of related issues: For example, a decision about whether to modify a fire-safety regulation in poultry plants is likely to be considered in connection with other workplace safety issues. Is the issue the modification of standards as it relates to one industry or across many industries? For a congressman from a particular district, the issue may simply be how the standard would affect one factory. For another policymaker, the issue may be worker-safety standards in general. For others, it may be government regulation of the economy. Since different policymakers may be making their decisions based on different understandings of what the issue is, there is no clear answer to such questions.

The literature on interest-group lobbying and participation in the policy process combines studies at all levels of aggregation. Some studies focus on a single roll-call vote in a single committee whereas others focus on a series of decisions in a broad area such as health-care policy. Some focus on a single issue niche (e.g., how the crop subsidy program works for cotton farmers); others choose huge and heterogeneous issue domains (e.g., telecommunications, health, or defense policy). Some ask group leaders or lobbyists to generalize about their behaviors across a range of issues, or across the previous twelve months (see Schlozman and Tierney 1986 or Walker 1991); other studies are designed around activities in a single case of policymaking. Each approach is valuable but each would be most valuable if it were designed in a way that encouraged the comparison of its findings with those using contrasting definitions.

Andrew McFarland (1991) notes that interest-group scholars are fond of conceiving of the political system as an aggregation of hundreds of smaller issue-areas, typically defined by economic production: Cotton farming, pesticides, higher education, nuclear power generation, and the like. Since interest groups are known to be important in each of these issue-areas, then they must be fundamental in the political system more broadly. He notes, however, that large-scale, macropolitical events can affect hundreds of these issue-areas simultaneously, thereby making it important to understand these macrolevel events as well. Much of the accepted literature concerning the power of various types of groups (for example, Schlesinger's "reform cycles") can be restated in terms of the prominence of certain issues on the national political agenda, according to McFarland (1991, 276–77). In any case it is clear that we reach different conclusions about the nature of the policy process depending on the level of aggregation we choose. The more narrowly we define issues, the more likely we are to find the operation of policy subsystems operating with little public knowledge, partisan bickering, or

political oversight. The more broadly we define issues, the more likely we are to note the importance of elections, political parties, and the national political institutions.

McFarland points to an important issue for the study of groups in politics: can the political system be reduced to the aggregation of hundreds of issue-areas, each dominated by a set of groups? The answer is clearly no, for if it were yes, there would be no important role for the president, the political parties, or the institutions of national government. Certainly, "high politics" often matters. The linkage between the politics of issue-areas, where interest groups are commonly studied, and "high politics," where other actors are seen as more important, is the agenda-setting process. Interest-group scholars have provided many important analyses of the roles and importance of groups within their issue networks, but have not done as well in explaining the roles of interest groups in those cases where "high politics" is at work. Issues sometimes rise high into the public consciousness; all political issues are not decided within subgovernments. Giving explicit consideration to the agenda-status of the issues being discussed would resolve many of the difficulties in comparing the results of diverse studies and help reconcile the study of groups with the study of political parties, elections, and national political institutions. Often, these other institutions are seen to be less important in group studies, but this can be only because so many group studies are designed to focus on issues that are off the political agenda.

Baumgartner and Jones (1993) show that the roles of groups may be dramatically different depending on their goals of either destabilizing or reinforcing existing policy communities and depending on whether the issue is on or off the political agenda. Focusing only on those cases with little or no public controversy is incomplete and can be misleading, just as would be a focus only on highly salient issues. Comparison of lobbying strategies of interest groups in cases with little or no public awareness with those in such cases as the Clinton health-care plan, with its massive domination of the entire political agenda, leads to confusion rather than to analytic clarity. The integration of an agenda-setting approach with sensitivity to the roles of interest groups in the policy process promises to resolve many of the apparent contradictions in this literature (see also Heinz et al. 1993, 16–17, 302). In any case, scholars must be sensitive to the fluid nature of policy issues and to their movements up and down the political agenda.

We have reviewed three problems related to the definition of an issue: issues are difficult to identify; issues may be aggregated in different ways; and issues rise and fall on the political agenda, often being redefined as this occurs. These difficulties are not of the type where a single "solution" is apparent. Rather, there are good reasons why scholars find different definitions of an issue best to suit their purposes. Just as with the ambiguities discussed earlier in this chapter, these differences in research approach will

always be with us; they simply must be handled carefully and explicitly by scholars so as not to hinder the development of a cumulative and comparable set of findings. Careless comparisons of research findings based on diverse definitions of the same terms renders virtually impossible the synthesis of a variety of findings into a single coherent description of how groups operate in the American political system. If tightly knit issue subsystems are in control in some narrow areas while the issues are off the agenda, but broad and conflictual issue networks obtain in others, especially when the issues are high on the political agenda, what do we conclude about the roles of groups in the system as a whole? Without a clear way to compare studies based on common definitions of key terms, we can have no answer.

WHAT IS THE NORMATIVE CONCERN?

Interest-group scholars have rarely made explicit the normative bases on which their conclusions might be interpreted. Great ideological and normative debates swept through political science in the 1950s and 1960s, as critics of pluralist thought charged that there were conservative biases in many of the most prominent works. However, as Greenstone (1975) pointed out, the most important debates even in this period combined normative and empirical disagreements. Those with relatively benign interpretations of the system noted certain observations; those who were more critical addressed a different set of empirical facts, never challenging the facts of the others. Rarely have scholars in the area separated the normative and the empirical; rather, both have been linked.

Despite a continued interest in normative issues associated with representation, few scholars have adopted an explicit normative framework. Many works concern the diversity of participation in the group system either through mass participation in groups or through elite participation in the policy process. Rarely does this literature make clear any ideal point toward which a democracy should strive. In the case of mass participation, it seems clear that more participation is normatively preferable to less and that participation should be equitable across social class and other distinctions. In this case, the normative question is relatively straightforward. In policymaking studies where we observe interest groups in their relations with government, however, there is no clear ideal with which to compare observed levels of participation. Scholars are concerned with diversity of participation, how this diversity changes over time, and how it differs from issue to issue. Few have paid attention to such questions as what degree of diversity would be appropriate for which types of issues, how to balance expertise and knowledge against demographic representivity, or how public officials may play a role in guaranteeing public representation, for example.

Studies of policy processes and lobbying often note changes over time in the diversity of lobbying communities. The rise of the consumer, women's rights, civil rights, and environmental movements in the 1960s and 1970s had important implications for the makeup of many policy communities. These were noted in the literature and generally were seen in a positive normative light: Each development seemed to indicate greater equality of participation. Beyond this general observation that greater diversity is to be applauded, scholars have typically not developed very sophisticated models of the normative appropriateness of various types of policymaking communities. If diversity of participation is often the result of a logic of conflict expansion, then do we conclude that only those decisions in government made after the eruption of conflict are made according to a process that is normatively beneficial? Scholars occasionally note the absence of certain interests who perhaps should be involved in important decisions, but without a clear idea of what is appropriate, these efforts are idiosyncratic and incomplete. To interpret the findings of the literature on group participation in government, we need a point of reference: What should be the proper roles of interest groups in policymaking? Does the answer to this question differ dramatically based on the nature of the question at issue? On these questions, the literature has remained resolutely ambivalent.

CONCLUSION

This chapter has reviewed a series of conceptual problems that has limited the ability of scholars to build a coherent literature on interest groups in American politics. For most of the issues discussed here, no single perspective could be applicable for all potential uses. The conclusion to be reached from this list of ambiguities is not that some single set of definitions should be imposed but rather that scholars should realize how their adoption of any particular usage affects their ability to compare their findings with those of others and to reach broad conclusions. Confusion and contradiction creep into a literature not when people use different definitions as they seek to address different theoretical concerns. Rather, they come when scholars overgeneralize from their studies, reaching conclusions on one set of topics when they have designed their projects in ways that best allow them to address another. Finally, the great range of definitions and legitimate usages of important terms in the literature serves to multiply the difficulties in generating a corpus of comparable findings. For quite some time, interest-group scholars have lived with the hope that the vast investment in empirical studies observed in the past twenty years would inevitably lead to a clear set of collective conclusions about group behavior. With this discussion of the myriad ways in which different theoretical and empirical perspectives produce subtle changes in research designs, measurements, and definitions of key terms, we hope to begin to explain how this accumulation failed to occur.

If we return to our distinction between areas of advance and areas of confusion in the literature, it is clear that if the areas of advance do not benefit from a complete absence of definitional ambiguities, scholars working in those areas have shown a greater ability to share common terms than those working in the areas of confusion. Those working on the collective-action dilemma have focused on a variety of elements of the membership calculus, and they have typically used relatively similar definitions of membership. This literature has grown richer as scholars have noted that not all groups have members, but when solving one particular puzzle most have chosen the appropriate set of definitions and limited their conclusions to those that can be supported by those terms. Looking at the areas where the most important contradictions have taken place, we note that many have to do with efforts to study lobbying in a quantitative manner based on one or a few cases. Notably, these studies have often adopted contrasting definitions of key terms. But the combination of new theoretical insights, new empirical points of reference, and new definitions of important terms can make it difficult indeed to compare findings. We will review these difficulties in detail in chapter 7. The point of this chapter has been simply to note the importance of care in generalization. Many useful definitions of a range of central terms are used simultaneously in the study of interest groups. We propose no imposition of a rigid set of standard definitions; different usages appear useful for different purposes. We do think it prudent, however, for scholars to pay careful attention to the precise terms they are using, especially as they attempt to compare their findings with those of others.

Having reviewed in chapter 1 the uneven patterns of progress in the literature and having noted a range of definitional ambiguities that may help explain these patterns in this chapter, we turn in chapter 3 to a historical overview of the rise and decline of the group approach to politics. To understand the state of the literature in the 1990s, it is important to see from whence it came.

CHAPTER 3

The Rise and Decline of the Group Approach

THE STUDY OF INTEREST GROUPS was once perhaps the most imperial of literatures, not only in American politics but in political science generally. Scholars of the generation of David Truman thought that a nation's political system could best be understood by looking at how groups formed and interacted with each other and with the government. Studies of interest groups were studies of the entire political system, and students of politics were students of interest groups, virtually by definition. Interest-group research of the postwar years took on the major issues of politics: who wields power and influence and whose views are represented in a democracy. Books focusing on the roles and influence of interest groups such as Truman's *The Governmental Process* (1951) or McConnell's *Private Power and American Democracy* (1966) posed central questions about the nature of representation in a democratic system. Similarly grand questions were the preoccupation in sociology, with books such as Mills' *The Power Elite* (1956), Hunter's *Community Power Structure* (1953), and Domhoff's *Who Rules America?* (1967). Scholars in both fields were concerned with basic questions about power, voice, and representation. The most vibrant research in both areas attempted to answer these questions by looking at groups, corporations, and other institutional linkages between the public and the government. These studies had in common an ambition to use the activities of interest groups as a lens through which to view all of politics. Dahl's *Who Governs* (1961) shared certain elements with this literature, though he disagreed with the view that groups alone could provide the entire picture; his coverage was correspondingly broader, focusing particularly on elected leaders themselves. Still, Dahl reserved an important place for the roles of groups in his study of New Haven, as did most prominent studies of the time.

THE GROUP APPROACH TO POLITICS

In this chapter, we review the dramatic rise and the precipitous decline of the "group approach to politics." The many difficulties and unfulfilled promises of the group approach affected the discipline for decades. The group approach offered many advantages over what had existed before. It represented one of the first truly behavioral approaches in a discipline that had been dominated by formal and constitutional analyses. On the other hand,

the approach foundered on a series of problems including how to measure power, how to devise a series of scientifically testable hypotheses, how to separate the normative from the empirical, and how to analyze a situation where organized interests work in close concert with allies within government, not so much lobbying them as helping them do their work. To understand the nature of the current scholarship on groups, it is important to see how much of it was a reaction against the work of previous generations.

The group approach to politics was important enough that Fred Greenstein and Nelson Polsby included two long chapters on groups in their *Handbook of Political Science*, published in 1975. Greenstone's eighty-page essay focused on group approaches to politics in general, especially the works of Bentley, Truman, Dahl, McConnell, and Lowi. Greenstone notes at the outset that "the group theories tradition has been the most important and sustained attempt to resolve two ancient issues:" the effects of groups on policymaking, institutions, and outcomes, and the effects of these processes and outcomes on the groups themselves (1975, 243). Salisbury's fifty-page essay reviews a broad range of issues dealing with groups, including the definitional ambiguities we discussed in chapter 2, research findings on group origins and growth, the Olsonian dilemma and subsequent research, internal group dynamics, and group relations with government. According to Greenstein and Polsby, then, the state of political science in 1975 required not one but two substantial reviews of research on groups. One focused on the broad theoretical questions of the "group approach to politics" and the other dealt with a range of issues concerning interest-group creation, maintenance, and influence.

Whereas scholars once considered groups to be central to our understanding of politics generally, more recent trends have pushed the topic of interest groups to the very margins of political science. Graduate-level seminars in American politics often overlook current research on interest groups. In stark contrast to the 1975 *Handbook*, a recent compilation of essays published by the American Political Science Association reviewing the state of the discipline includes no general essay on the roles of groups (see Finifter 1993). Clearly, the study of interest groups has receded from its position a generation ago at the core of the discipline (see Garson 1978).

We noted in the introduction that political scientists, philosophers, and commentators of all kinds have long had ambivalent views of the roles of groups in politics. On the one hand, they are seen as a vehicle for popular representation; on the other hand, the biases of group mobilization are acknowledged by continued emphasis on the dangers of granting access to "special interest lobbyists." Groups, at best, are necessary evils that must be controlled before they do too much damage.

Jack Walker noted that political scientists have been much more supportive of the roles of political parties in politics than of interest groups. He

noted that more than ten times the number of scholarly articles were published on political parties than on groups in the 1980s but that groups are more commonly active in Washington policymaking than parties are. Why the discrepancies in scholarly attention? "Political scientists devote so much of their resources to the study of political parties mainly because of their historic commitment to the task of convincing anyone who will listen that democracy cannot be successful without the existence of vigorous, competitive political parties" (Walker 1991, 20–21). Whereas parties are seen as encompassing organizations, interest groups are seen as fragmenters of the political process, encouraging the press of particularistic demands rather than helping to develop the conditions necessary for negotiation and compromise. Given these dubious effects, political scientists have abandoned the study of groups while lavishing more attention on parties than they may merit, according to Walker (1991, 20–40).

Kay Schlozman and John Tierney also noted the dearth of attention to the group system in the 1980s. Discussing the rise of the group approach to politics, they wrote: "No sooner had this perspective become dominant than it was questioned seriously" (1986, ix–xii). They foreshadow Walker's arguments that groups were increasingly important in the Washington community but strangely absent from the scholarly agendas of most members of our profession: "In the past two decades we have witnessed what seems to be a virtual explosion in demands by private organizations in Washington. While the media have made much of this development, political scientists have paid it less heed" (ix). In the face of this dearth of attention, they set out in their book "to compensate for the relative neglect by academic analysts of organized interests in contemporary politics by probing what they are up to and what their activity means for public life in America" (ix). For the authors of two of the most important large-scale reviews of the roles of groups in politics in the 1980s, then, part of the motivation was to fill in a perceived gap in the research agenda of the profession. This gap is all the more remarkable, as we will review in this chapter, because it followed a period when the group approach to politics was so dominant.

GROUPS AT THE CENTER OF POLITICAL SCIENCE

Every literature is in a way a reaction to and often against the work of a previous generation of scholars. Harmon Zeigler and Wayne Peak (1972, 2–6) describe the development of a group approach to politics in the early twentieth century as a reaction to the institutional/legalism of the nineteenth century. The work of the early pluralists clearly indicates that they were dissatisfied with the dominant formal/legal approach of their day. When they looked at the functioning of government, they found that constitutional

analysis and description could explain only part of what they observed. Corporations, trade groups, business executives, and others outside of government often played important roles but were absent from the descriptions of the time. Ernest Griffith was most explicit on these points, arguing that a group approach to the processes of government would be more revealing of how the system really works than any analysis of separation of powers or constitutional design:

> One cannot live in Washington for long without being conscious that it has these whirlpools or centers of activity focusing on particular problems. The persons who are thus active—in agriculture, in power, in labor, in foreign trade, and the parts thereof—are variously composed. Some are civil servants, some are active members of the appropriate committees in the House and Senate, some are lobbyists, some are unofficial research authorities, connected perhaps with the Brookings Institution or with one of the universities, or even entirely private individuals. . . .
>
> [H]e who would understand the prevailing pattern of our present governmental behavior, instead of studying the formal institutions or even generalizations in the relationships between these institutions . . . may possibly obtain a better picture of the way things really happen if he would study these "whirlpools" of special social interest and problems. (Griffith 1939, 182–83)

By focusing on the activities of actors inside and outside of government, Griffith and others could be called the first behavioralists in political science. Many of these authors would certainly object to being called behavioralists, since they did not share with the later behavioralists the tendency for systematic quantification or concern for the analysis of individual-level behavior. Still, the early group scholars, like the later behavioralists, studied political dynamics through observation and outside of the formal institutions of government, eschewing the formal/legal approach that was more common before their time. The foundations of behavioralism can be found at least partly in the rise of the group approach to politics, though the two developed differently in later years.

Griffith was not alone in this view of the greater realism of the empirical study of groups as opposed to the formal analysis of structures of government, as David Garson points out:

> In the early years of the twentieth century, the felt need to focus empirically on groups was widely shared among American political scientists. Summarizing popular ideas on government, for example, Albert Hart noted that "more and more people tend to accept the theory that all government in America—national, state, municipal or local—springs from one source, the American people as a whole, who choose to exercise their power through a variety of organizations (Hart 1907, 558)." (Garson 1978, 32)

This "organizational perspective" on political life offered many advantages over strict legalism, in that it allowed many more political variables to come into perspective, but it had the drawback of being all-encompassing as well. Group scholars like Bentley (1908), Odegard (1928), Herring (1929), Schattschneider (1935), Griffith (1939), and Key (1964) made possible some of the most important advances in our understanding of politics by introducing a broader view of the functioning of government institutions, including the informal relations among institutions and outside actors and the dynamics of engaging the public through elections and organized lobbying. Griffith's work is typical of his generation in two ways: Not only did behavioralism supplant formal/legalism as the dominant approach to the study of politics, but many of the first insights about the policy process that this broader view permitted are echoed with almost eerie similarity today. The remaining importance of this early work is of course more due to its broad analytic approach, which allows the simultaneous consideration of the roles of government and nongovernment actors in the policy process, than to any descriptive accuracy that the work might retain decades after it was written. Still, it is surprising to note the degree to which many of the descriptions of the "whirlpools" of policymaking in the works of Griffith and his contemporaries are similar to our more recent discussions of issue networks, policy subsystems, policy domains, and policy communities (see Heclo 1978; Kingdon 1984; Sabatier 1988; Berry 1989a; Browne 1990; Walker 1991; Baumgartner and Jones 1993; Heinz et al. 1993). Policy studies today typically borrow heavily from the work of interest-group scholars in the prewar years.

David Garson (1974; 1978) describes how interest-group theory became the central framework of analysis during the first sixty years of this century. Group theory grew in reaction to the narrow institutionalism of early political science and against assumptions about the absolute sovereignty of the state, he argues. Group theory instead relied on the pluralistic assumption that the best political outcomes would arise as a result of group conflict. Free and active group life was seen as a crucial to the functioning of a democracy. The role of the state was not to dictate outcomes, but rather to arbitrate among various interests. The best functioning democracy would not necessarily be the one with a certain constitutional structure but rather the one with the most balanced, active, and responsive group system. Groups accurately representing the views of their members would be a better guarantor of effective government than any particular institutional design. In the immediate postwar years, one of the explanations of America's success in maintaining democracy while other countries fell to fascism was the vibrant group system based on competition and independence from the state. Pluralism grew partly, then, as an effort to explain the perceived genius of American democracy, and it included both descriptive and normatively charged writings, especially in the postwar period.

The group approach became dominant in these years not only in the study of American politics, but in comparative politics as well. A tremendously influential series of studies was initiated in the 1950s and 1960s, mostly at the urging and with the support of the Social Science Research Council, with the goal of understanding the functioning of democracies around the world through a description of how groups relate to the state. These decades saw an optimism in the possibilities of furthering our understandings of politics by using the broader lens of group politics rather than the narrower and previously dominant perspective of comparative legal institutions and constitutional structures. This approach and this optimism are reflected in such works as Ehrmann's *Organized Business in France* (1957), the essays published in Ehrmann's edited volume *Interest Groups on Four Continents* (1958), Almond's "Comparative Study of Interest Groups" (1958), Eckstein's *Pressure Group Politics* (1960), Brown's "Pressure Politics in the Fifth Republic" (1963), LaPolombara's *Interest Groups in Italian Politics* (1964), Eckstein's *Division and Cohesion in Democracy* (1966), and Lijphart's *The Politics of Accommodation* (1968). These studies shared the perspective that one could understand how a country "really" works not by analyzing its constitutional structures but by observing group-state interactions. In sum, interest groups were the vehicle that political scientists first discovered as they attempted to abandon the constitutional/legal framework that had previously dominated their thinking about politics, and as they attempted to adopt a behavioral framework. (The adoption of the "American" group approach for comparative analysis was not without difficulties, and these were debated at the time; see for example LaPolombara 1960 and Macridis 1961.)

In some ways the group approach became more influential in comparative politics than in American politics. One of the most important components necessary to sustain a vibrant democracy is a healthy network of organizations available to the public, according to Almond and Verba's five-nation study. Their focus on civic culture put organizational affiliations through interest groups at the center of democracy (1965). The importance of local voluntary associations has more recently been picked up by Robert Putnam as he has used organizational affiliations as a centerpiece of his explanation of the basis for a strong and working democracy in Italy and in America (1993; 1995). A research focus on the roles of groups in their relations with state officials is probably more central to much of the comparative study of public policymaking than in American politics today (see for examples Rokkan 1966; Heclo 1974; Suleiman 1974; Dogan 1975; Anton 1980; Aberbach, Putnam, and Rockman 1981; Olsen 1983; Hall 1986; Baumgartner 1989; Wilsford 1991). Major debates concerning the roles of groups and the nature of their relations with government officials dominate the literature on pluralism, corporatism, and related descriptions of the policy process, and these disputes have resonated throughout the field of comparative politics

for decades (see Lijphart 1969; Landé 1973; Schmitter 1974; Schmitter and Lehmbruch 1979; Lehmbruch and Schmitter 1982; Richardson 1982; Almond 1983; Keeler 1987; Wilson 1987; Smith 1993). The study of interest groups in comparative politics and in other democratic settings has consistently focused on the lobbying and government relations of those groups, rarely becoming preoccupied with the Olsonian dilemmas of mobilization for collective action. This may explain why group studies are considered more central in comparative politics than in American politics.

Despite its prominence, the group approach was not adopted by unanimous consent in either American or comparative political science. Neither did it ever generate the kind of consensus in its support that is sometimes imagined. Criticisms of the group approach were powerful and continuing. Further, the approach itself was mostly visible only from the outside, and more easily with hindsight. It was never a clear theory of politics with the requisite testable hypotheses, shared definitions, and neat operationalizations; rather it was a set of ideas about what was important to look for in politics, and an idea that representation took place through the group system as much as through the constitutionally mandated institutions of elections and judicial-legislative-executive interactions. Pluralism was so little developed as a theory that proponents of various pluralist arguments objected to being labeled as similar. While they shared certain general orientations, they did not share conclusions or anything as precise as a theory.

The group approach to politics reached its point of greatest acceptance in the 1950s but even then was not the subject of a broad consensus. Problems such as vague definitions, conflicting conclusions, unsystematic research techniques, inability to explain the lack of mobilization by disadvantaged social groups, and other issues had existed for a long time. By the late 1960s, however, the approach was in crisis, criticized on the one hand for failing to recognize the unfair imbalance of power among interests in society, and on the other hand for overstating the influence interest groups wielded over government. By this period, the approach had been largely discredited through a series of internal disputes, external challenges, and by the rise of a new set of research questions that left traditional group studies behind as outdated and irrelevant relics of a prescientific age. By the time of Olson's *Logic of Collective Action*, it was a research agenda on the road to oblivion.

THE DEMISE OF THE GROUP APPROACH

A great variety of forces combined to cause the demise of the group approach to politics. There were theoretical problems, methodological difficulties, ambiguities of definition, disagreements on how to interpret findings, difficulties of measuring important features such as power and influence, findings of limited group influence in government, and finally

the emergence of a new set of research questions based on the question of group mobilization rather than on group influence. We review each of these in turn.

A Normative Theory or an Empirical Approach?

The most troubling difficulty with the group approach to politics was the lack of clarity about its goals. The most prominent pluralists asserted that their studies were empirical ones, attempting only to explain how democracies operate, but ambiguities remained about whether they were merely explaining and describing, as they asserted, or holding up pluralism as a normative ideal.

In his review of group theories of democracy of the 1950s and 1960s, David Greenstone notes that theorists "disagree with one another over the ethical status of their subject matter. To some, notably Truman and Dahl in his work on New Haven, political interest group activities seem at least comparable with, if not essential to, the health of American politics. To others, such as McConnell and Lowi, interest group behavior often malignly subverts the highest ideals of liberal democracy. This issue, however, has apparently been less empirical than normative; the facts and observations adduced by each side have not ordinarily been challenged" (Greenstone 1975, 244). This confusion of the normative and the empirical rendered each disagreement potentially attributable to the biases or ideology of the author.

Many disputes arose because of ambiguities concerning whether pluralism was a normative or an empirical theory. When Jack Walker raised some of these questions in his 1966 critique of "the elitist theory of democracy," Robert Dahl was particularly forceful:

> One central difficulty with Professor Walker's paradigm is, I think, that he insists upon interpreting as if they were normative or deontological certain writings that were mainly if not wholly intended to set out descriptive, empirical theories. Most (though perhaps not all) of the works cited by Professor Walker are not attempts to prescribe how democracy *ought* to work but to *describe* how some of the political systems widely called by that name do in fact operate and to *explain* why they operate this way. Professor Walker may deplore the neglect of normative questions, as many other political scientists and political philosophers do; but he ought not to confuse attempts at empirical description and explanation with efforts at prescribing how these systems ought to operate in order to attain desirable or ideal ends. I would not argue that every writer cited by Professor Walker has always tried to maintain this distinction, or, if he did, has always succeeded; but I do think that it is a serious misunderstanding to interpret these writers as essentially normative theorists. (Dahl 1966a, 298, emphasis in original)

Whatever the conclusion concerning the original intent of the pluralists, the discipline showed considerable confusion toward these writings. Dahl's response indicates part of the reason: While most of the group studies focused on careful observation, description, and explanation, there was a strong strain of normative content in much of the work as well. To the extent that scholars focused on the diversity of interests active or potentially active in politics rather than on the biases and inequalities present in the pressure system, they seemed to offer an enthusiastic endorsement of the desirability and the justice of the American political system.

The pluralist view rested on an image of the group struggle in which no single group could have significant influence, but where a multitude of competing groups would be the safeguard of democracy through the free competition of ideas. David Garson notes this view by quoting from the presidential address to the American Political Science Association of Quincy Wright in 1950. Wright argued: "'A world with millions of small conflicts in the minds of individuals and in the discussions of small groups is likely to be more peaceful and prosperous than a world divided into two opposing groups each of which commands the exclusive, intense, and blind obedience of the population' (Wright 1950, 11). Thus, Wright's presidential address illustrated the then-common view that the politics of small group conflicts represented both description and prescription of democratic practice. This group politics was cast as the cornerstone of free society" (Garson 1978, 79).

To the extent that this image of competition among the multitude of groups could be challenged, one's conclusions about the nature of American democracy would be altered. In the postwar period, this image came under increasing scrutiny, largely as a result of the growth of large industrial corporations and the development of other large national interest groups. Henry Kariel gives voice to this feeling of unease. If pluralism was built on the notion of perfect competition among groups, as he asserts that it was, then it could no longer function as a justification for, or as a description of, our current form of government, he wrote. Technological innovation and economic change of the postwar years increasingly promoted the growth of organizational behemoths concentrating economic and political power as never before. The growth of such organizations as General Motors, the Teamsters, the Farm Bureau, or the American Medical Association, along with other corporations, unions, and groups boasting millions of members each seemed incongruent with existing notions of democracy. The new postwar economy called into question the competition among a myriad of small groups on which the pluralist perspective relied (Kariel 1961, 1–4).

The nature of pluralist competition among interest groups was the subject of great empirical debates, but these were never too far removed from the normative conclusions to which they led. As we will see in some detail below, the pluralist perspective led to benign normative conclusions regarding the extent to which the pluralist system could be compared to a perfectly competitive economic market. With competition, undue influence would be impossible. To the extent that scholars observed the development of extremely large corporations and other interests, they showed their unease with a theory of political representation that seemed to rely on free competition to ensure normatively defensible outcomes. The obvious links between scholars' justifications of the political system and economists' explanations of the benign genius of the free market only reinforced the suspicion that many pluralists were apologists for the status quo (and that many of those who attacked it were "radicals"). Though nothing about the pluralist approach would imply support for huge special interests able to dominate their parts of the political system, the explanation of the genius of democracy through competition was seen as a broad endorsement of a system that included significant disparities in influence, with some very privileged groups exerting great amounts of political power. The normative and the ideological became confused with the descriptive and the empirical.

Even among those who focused on the diversity of interests present in the pressure system, and who were therefore grouped together as the "pluralists," there was never a strong theoretical structure that joined all the work together. Pluralists lacked a single voice, as Robert Dahl noted (1966a). There was no pluralist "theory" in the sense of the development of testable hypotheses that could orient a school of researchers. Indeed, Mancur Olson used the group approach to illustrate his contention that "science attempts to go beyond descriptions, histories, terminologies, and typologies to genuine hypothetico-deductive theory. Schools of scientific thought that fail to develop deductive theories resting on tested hypotheses never last" (1986, 166). The approach was bedeviled by ambiguities about its goal—empirical description or normative evaluation—but it was further troubled by theoretical incoherence.

A Theory, an Approach, a School, a Perspective, or What?

Among the most serious intellectual problems for the development of a pluralist view of the roles and activities of interest groups was the fact that pluralism was never a theory at all, but rather a perspective. Those who worked within the approach disagreed with each other on significant points and objected even to being referred to as a school. This can be seen clearly in Robert Dahl's vehement rejoinder to Jack Walker's assertion that there was, indeed, a pluralist school of thought focusing on the competition among elites (see Dahl 1966a and Walker 1966a and 1966b). As Dahl pointed out (1966a, 297–98), those active in the approach espoused a great range of views.

While many of its most influential proponents denied that there was any such thing as a pluralist theory of politics, outside observers continued to note that it constituted at least a school or a perspective (see, e.g., Greenstone 1975). We reviewed in chapter 2 a series of definitional problems that would be unthinkable in a literature that had a clear theoretical structure. Even during the period when the group approach to politics was most widely accepted, it never constituted a clear theory of politics. It held within it the seeds of its demise as scholars proved unable to use the approach for the development of a set of testable hypotheses. We will see in the next chapter that this was one of the greatest attractions of the Olsonian perspective on collective action in the 1970s and 1980s.

Pluralism did not suffer only from a lack of theoretical cohesion. There were also important questions of descriptive accuracy. E. E. Schattschneider's oft-cited remark that "the flaw in the pluralist heaven is that heavenly chorus sings with a strong upper-class accent" (1960, 35) lays at the center of a debate concerning the observable bias in the group system. Was the degree of bias inconsequential, potentially reversible, and benign, or was it an unacceptable demonstration that social class predetermined entry into the group system? In short, how powerful were the barriers to entry into the pressure system?

Criticisms of the Pluralist Heaven

One of the most appealing elements of the pluralist perspective to many was the concept of the equilibrium of political forces based on the ability of "potential groups" to mobilize when their interests were threatened (Truman 1951). According to Truman's ideas of mobilization in response to threats, in the absence of any overt barriers to mobilization, those groups in society, be they workers, industries, social groups, ethnic groups, or whatever, would form interest groups and mobilize for political action whenever it was in their interest to do so. Of course each mobilization could set off a countermobilization by those with different views, and the end result would be a set of interest groups accurately reflecting the needs and desires of the population. Truman's ideas seemed to explain why many groups formed (and he gave many examples of how groups did indeed organize in response to economic crises, wars, and other threats). Further, they explained the important representational role of groups, casting groups in a more favorable light than was often done by the muckrakers and others who complained of the "undue influence" of the "pressure boys."

While no pluralist writers argued explicitly that the equilibrium that they described was normatively advantageous, many critics charged that these writers exhibited a certain satisfaction with the concept that any threat to

important interests would naturally lead those interests to mobilize. If they did not mobilize, the generally unstated but clear implication seemed to be, then it must be because they were relatively satisfied with the status quo. In this "benign view" of the pressure system, barriers to mobilization were treated as inconsequential, or, more often, ignored. V. O. Key describes the pluralist equilibrium in these terms:

> Political systems may exist in a stable, even static, form over long periods. The holders of power are unchallenged; the allocation of rights, privileges, and benefits remains acceptable to all sides; every man knows his place and keeps it. In modern states so serene a political condition does not prevail for long. The equilibrium—the balance, the ordered course of affairs, the established pattern—is disturbed from time to time by some change that generates discontent. Such dislocations tend to set off movements in demand of a correction of the balance or for the creation of a new order. Discontent may find expression through a political movement, a more or less spontaneous rising of the people concerned, or it may be manifested in the intensified activities of existing organized groups. (Key 1964, 40)

Disturbance theories or other equilibrium analyses of the interest-group system or of American politics in general tended to leave the reader with an impression that things are "fair" or "natural" if they are in their state of equilibrium. This is, of course, a misunderstanding of what an equilibrium implies, but it was a common one nonetheless. In a way, the fairness of the pluralist equilibrium was the point of normative disagreement between defenders of the pluralist approach and others. As Dahl argued in his response to Walker, nothing about the fact that an equilibrium may exist suggests the slightest conclusion about the desirability or fairness of that equilibrium: it could easily describe the overrepresentation of privileged groups and the under- or nonrepresentation of the disadvantaged. Still, many pluralist writings were taken to suggest that nothing would stop the disadvantaged groups in society from mobilizing to correct whatever difficulties they might suffer.

Disagreements about the nature of the pluralist equilibrium, centering on the question of barriers to entry to the pressure system, were the base of the combined normative and empirical debates among pluralists and their critics. This is what distinguished Truman from Schattschneider, Dahl from Hunter. Where some saw the mobilization of interests in American society as a guarantor of pluralism, they tended to overlook what Schattschneider called the upper-class accent of the heavenly chorus. One of the most important problems that the literature developed was an ideological one: the pluralists were accused of developing a theory that supported the status quo, which in the 1950s included segregation in the South, obvious advantages in mobilization for those of higher social status, huge organizational advantages

for business versus consumer interests, and a variety of other elements that no pluralist felt comfortable defending.

Group scholars since the turn of the century had written dozens of books complaining about biases in what they derisively termed the "pressure system." The "undue influence" of wealthy individuals, large corporations (especially the oil and steel companies in the prewar period), and of business interests in general had been a staple of journalistic and scholarly discussion for decades. Such studies remained common even in the hey-day of the pluralist approach. At the same time as authors such as Truman were describing the great diversity of interests present in the group system and focusing on the possibilities of countervailing power, more narrowly focused studies often found only one side to be active. Garson describes the situation in these terms:

> At the same time [as the pluralists were writing], empirical studies of specific interest groups continued to come to conclusions disconcertingly in tension with the felicitous view of American democracy implicit in the group approach. Charles Hardin's study of *The Politics of Agriculture* (1952) was one example. In this study of the Farm Bureau, Hardin found not countervailing power, but corporatist collusion. Hardin found that the U.S. Department of Agriculture was working hand-in-glove with established farm interests through an extensive system of support (extension services, government advisory committees, appointments, local boards) to perpetuate a particular structure of power. (Garson 1978, 85–86)

The 1950s saw not only the publication of Truman's book describing the great diversity of interests present in America, but also a series of studies focusing on the ills of lobbying: Mason (1950) on the National Association of Manufacturers and the Liberty League; Bailey (1950) on the National Farmers' Union and the Chamber of Commerce; Schriftgiesser (1951) on *The Lobbyists*; Knappen (1950) on shipping; Shott (1950) on railroads. Early findings from mass surveys called the pluralist assumptions into question as well: "The very possibility of representativeness of interests in the group process was called into question by early opinion data showing that less than a third of Americans said they belonged to *any* organization taking stands on national issues" (Garson 1978, 82, emphasis in original).

Just as in liberal economics the best outcomes can only be achieved through active competition and the avoidance of monopoly power, so too did pluralism rely on competition among groups to provide the best political outcomes. The descriptive question of whether there were important barriers to entry in the political marketplace was therefore heavy with normative implications. V. O. Key notes how competition among groups can lead to better government decision making, as those in positions of authority are given freedom by the very conflicts they see about them. However, he notes

the corresponding difficulty if the government decision maker is faced only with a single side of a potential dispute:

> To some extent, the outrageous demands of pressure groups are checked by the demands of other groups that may be equally outrageous. In situation after situation legislators and administrators are confronted by groups pushing in opposite directions, a state of affairs that permits government to balance one off against the other and to arrive more easily at a solution thought to represent the general interest.
>
> Though the restraint of mutual antagonism is built into the group system, that check does not operate in many situations. Groups well disciplined and amply supplied with the matériel of political warfare often are countered by no organization of equal strength. The opposing interest may, in fact, be completely unorganized. The lobbyists for electrical utilities, for example, are eternally on the job; the lobbyists for the consumers of this monopolistic service are ordinarily conspicuous by their absence. The representation of these unorganized sectors of society becomes the task of politicians who, bedeviled by the group spokesmen on the ground, may succumb to the immediate and tangible pressures. In short, while group pressures often cancel each other out, this process restrains particularlism erratically and uncertainly. (Key 1964, 150)

The erratic and uncertain functioning of the pluralist equilibrium was emphasized in study after study focusing on particular issues. Not only was this the focus of periodic journalistic exposé, but an entire school of scholarship focused on what Griffith had called policy "whirlpools" in the early part of the century. These authors saw closed systems dominated by special interests with little competition, little room for public involvement or concern, and little in the way of pluralist competition. For Maass (1951), Cater (1964), Freeman (1965), McConnell (1966), Lowi (1969), Fritschler (1975), and other subsystem scholars, pluralism was not erratic and uncertain; it was inaccurate for the cases they studied, and seemed unlikely ever to be a good description of how most issues were decided in government. Lowi's work was especially influential on this score. Lowi's *End of Liberalism* made the case that the congressional practice of delegating authority to agencies inevitably benefits organized interests. These interest groups were seen as seeking narrow and particularized ends. Thus, even a fair and pluralistic fight among those interests would not lead to desirable governmental outcomes, since each group would seek its own selfish goals rather than what was best for the country at large. Lowi's argument convinced many people that the close involvement of interest groups in government would not be advisable even if the system were free of bias. This critical view of groups contrasted sharply with the more favorable pluralist view, and it helped turn many scholars away from the study of groups altogether. Rather than promoting democracy through conflict and competition, groups came to be seen

as a drag on the democratic process. From being part of the genius of democracy, potentially explaining the most basic elements of the representational process, groups became part of the problem. Scholars reacted in part by looking elsewhere for explanations of how representation occurred; clearly, they would not find it in a flawed group system.

Schattschneider (1960) was of course the most visible with his simple statement that the membership in the group system was so biased by social class that probably 90 percent of the public could not participate. A developing consensus that the pluralists had overlooked significant and systematic barriers to entry into the group system was one of the most serious difficulties for those who would support the group perspective. Since this debate, of course, the social-class bias of the group system has been studied repeatedly and has been consistently confirmed (for examples, see Verba and Nie 1972; Schlozman and Tierney 1986; Schlozman, Verba, and Brady 1995). In sum, a keystone of the pluralist perspective, that all groups ~~would mobilize~~ if threatened, was never well received and came under increasing attack. This attack was mostly on empirical grounds in the 1950s and early 1960s, but with Mancur Olson it would suffer a fatal blow. Before turning to Olson's criticism, however, it is worth noting that the group approach suffered from a variety of other difficulties. These included a failure to measure the central variables of power and influence and a series of studies that suggested that groups were not as important in politics as had been assumed. By the time of Olson's *Logic of Collective Action* (1965), a great variety of difficulties had disenchanted many with the group approach.

Problems of Measuring Power and Influence

One of the most troubling difficulties within interest-group research stemmed from the vituperative ideological and methodological debates about where power was located in the political system and how to recognize when, by whom, and how influence was wielded. As Robert Dahl wrote, power can be direct or indirect, reciprocal or unilateral. "One who sets out to observe, analyze, and describe the distribution of influence in a pluralistic democracy will therefore encounter formidable problems" (1961, 90). Dahl was particularly careful in his attempts to measure influence, and devotes an appendix to the discussion of how one can adequately gather indicators for the exertion of political influence. (See Dahl 1961, 330–40. See also his article on the topic, 1957.) Indeed, the 1960s saw vicious debates in political science and sociology based largely on the difficulties of measuring power and on the divergent conclusions that various scholars reached when they used different methods to address the topic.

The debates that separated Dahl (1961) from Mills (1956), Hunter (1953) from Truman (1951), and Domhoff (1967) from Polsby (1963) had in large part to do with their differing conclusions on how wide a stratum exerted significant political power and how permeable this group was to outside influences. The community power studies of the period often reached contradictory conclusions because some used observational techniques, some used reputational methods, and each was based on the intensive study of a single city yet attempted to reach general conclusions. Most importantly, the group of scholars involved in these efforts never was able to reach a consensus on how to measure power, the question at the center of all of their work.

Andrew McFarland (1966, 1969) reviews the literature on the concept of power. He notes the importance in pluralist thought of the dispersion of power across a great many actors, and of the importance of complexity in insuring such a dispersion. For each powerful actor, there should be a countervailing power, allowing no monopolistic situations to develop (see also Key 1964, 150, quoted above, and McFarland 1987). If power is the same as causation, as he argues it is, then "complex causation" is similar to pluralism. (That is, if there are many complex causes of a decision, then power must be relatively diffuse; if there is only one cause, then power is concentrated.) McFarland quickly notes, however, that one can almost always find a complex series of causes if one looks hard enough. He demonstrates the problem of "spurious pluralism" in which decisions appear to be affected by a broad range of actors but are really tightly controlled, with case studies of a Soviet firm and the U.S. Forest Service. Both organizations exhibit a great deal of decentralization on paper. On the other hand, powerful norms limit the nature of the bargaining that actually takes place within each institution. Some issues are just not put on the agenda.

McFarland points to an important and ultimately fatal problem for the pluralists. In the face of any findings of diffused power (or complex causality, as he puts it), critics may argue that the plural elites work within culturally defined parameters limiting the scope of the debate only to the relatively inconsequential. McFarland concludes that this problem is in the end impossible to solve. With no way to guarantee that the bargaining one observes concerns the full range of potential points of disagreement, one cannot be sure that power seemingly broadly shared extends across all the important elements of potential debate.

With the publication of an influential essay in 1962, Peter Bachrach and Morton Baratz showed how ticklish this measurement problem was always going to be. They pointed out that the most important exercise of political power might well be the power to exert agenda-control. The second face of power, as they termed the ability to limit certain items from ever entering the political debate, showed a great gap in most studies of power. However,

with a few exceptions (Crenson 1971 and Gaventa 1980), scholars have avoided the difficult question of how one could devise techniques of measuring this second and potentially more important face of power.

After the vituperative debates that characterized these disagreements in the 1960s, the demonstration by Bachrach and Baratz that tremendously important elements of power were potentially unobservable, many in the discipline seemed to agree with William Riker's assessment. In a 1964 article, he reviewed the difficulties and inconsistencies in five major conceptions of power. "The final question, once the full complication of the ambiguities is revealed, concerns the appropriate scientific attitude toward the conception of power itself. Ought we redefine it in a clear way or ought we banish it altogether? My initial emotion, I confess, is that we ought to banish it" (1964, 348). The concept of power was not banished from political science, but scholars for the most part reacted by abandoning their interest in these questions. Community power studies, once a common and prominent research approach in political science and sociology, were largely abandoned or ignored by the broader discipline. Urban politics, once the home of many influential studies, receded as a field within political science. Scholars moved on to other fields that did not have at their core such a difficult concept.

In important ways, one of the most significant failures of the group approach was self-inflicted. Many of its most prominent studies were designed in a way that required the author to devise a mechanism to measure the unmeasurable. Robert Salisbury points out some of the problems with this approach:

> For decades the most common question guiding interest group research has been the question of influence. What group or groups have how much influence over policy-making is the common thread that connects Dahl to Schattschneider, Bauer, Pool and Dexter to Lowi, McConnell to Browne, and Chubb to Stigler. Push it back a step to ask what factors affect group influence, and Truman is brought into the stream. Virtually every discussion of PACs sooner or later comes around to the question of how much influence PAC contributors have on electoral outcomes and/or policy choices.
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> At first glance, this may seem to be an altogether appropriate focus of inquiry. After all, in many areas of political science, from voting to President-Congress relations to Supreme Court decisions to international relations, a good deal of attention is devoted to who wins, who loses, and why. The trouble is that the game metaphor is profoundly misleading regarding the underlying character of much of the political process. Very often there is no clear resolution, no definitive conclusion to the process by which interests are articulated and pursued. "Play" continues, moving from one venue to another perhaps, the tides of success for particular participants ebbing and flowing, while the structure of the "game" slowly evolves. As the saga unfolds, individual episodes may be singled out for separate treatment, but unless they are seen in their larger historical/developmental context, any particular story, however, melodramatic it seems to be, is likely to generate more misunderstanding than insight.
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> Think of it this way. Does it make much sense to ask who is the most influential member of the U.S. Senate? Or, insofar as we would grasp the essential meaning and impact of their decisions, is it a high priority to determine the influence rank among the Supreme Court Justices? It is not that influence is irrelevant; it is simply not the best way to frame the central questions. (Salisbury 1994, 17–18)

Salisbury points out that one need not organize research projects that require the measurement of the unmeasurable, yet this was precisely what many of the most prominent group and community-power scholars did in the 1950s and 1960s (see also Heinz et al. 1993, 7–8). The failure to devise methods of answering a question that had been set up as the central topic in all these studies led to an abandonment of the community-power approach after it became clear that it would never succeed. It was one more in a series of difficulties for an approach that had been at the center of the discipline.

Findings of Limited Impact

If the group approach became for a time the dominant approach to politics, it was because of a general feeling that associations, businesses, and other lobbies were powerful. Study after study seemed to indicate that groups were more powerful than government decision makers themselves. The businesses and trade groups in Schattschneider's 1935 study dictated the wording of bills to a compliant Congress. Likewise, the Army Corps of Engineers and local government groups had easily obtained almost unlimited funding from Congress in Maass's 1951 study. Group propaganda could manipulate the public into acting against their best interests, putting pressure on legislators to do whatever the groups desired, according to Crawford's 1939 account. Government officials seemed merely to reflect in their actions those forces that the various lobbies were able to exert upon them. The assumption that groups were all powerful came under attack in the 1960s, however. An influential set of studies gave reason to think that we had overestimated the importance of groups in politics.

Several important studies published in the 1960s took issue with the view of interest-group dominance. Milbrath's (1963) survey of lobbyists, Bauer, Pool, and Dexter's (1963) study of tariff legislation, and Scott and Hunt's (1965) survey of members of Congress all concluded that interest groups wielded much less influence than was generally attributed to them. The

findings of these studies were not entirely novel. Even Schattschneider (1935) had showed evidence of interest-group apathy before most legislative issues. Although this book is best known for its description of strong interest-group influence, Schattschneider also noted that most manufacturers affected by the tariff legislation in question did not even take the time to express their support or opposition before Congress. Scholars had long found groups sometimes to be apathetic and ineffectual in some cases, but in the 1960s several studies were taken to indicate that groups were perhaps not the most important place to look for an understanding of the legislative process

Rather than using strong-arm tactics to get what they wanted, the primary role of interest groups was providing information to allies within government, according to this new body of research. Interest-group influence was thus essentially benign, since groups essentially served as adjunct congressional staff. Bauer, Pool, and Dexter's study (1963) is a complex work showing areas of strength and weakness, but it has come to be cited especially for its "service bureau" finding. The authors described the interests they surveyed as surprisingly poorly informed, badly funded, and ill-prepared to do battle in Washington, at least compared to the popular conception that somehow groups were "pulling the strings" in government. The popular scholarly conclusion drawn from these studies was that interest groups did not exert pressure, indeed were not influential. The informational role they played in Washington could easily be supplanted by information from congressional staff. Although these findings of limited impact were at odds with the arguments being put forth by the critics of pluralism, the implications of these contradictory findings were surprisingly similar. Both seemed to justify an abandonment of the previous focus on groups in the policy process.

CONCLUSION

This chapter has reviewed the rise and decline of the group approach to politics. As the first set of authors to move beyond formal legalism, group scholars offered tremendous advances to the discipline over the institutionalism of their predecessors. Descriptions of the policy process today focusing on such concepts as policy subsystems and issue networks owe a heavy debt to scholars of the generation of Griffith, who first wrote of policy whirlpools in 1939. Even as it developed a great number of important insights that remain central to our views of the policy process today, the group approach also suffered from a series of difficulties, as this chapter has reviewed. Only a fuzzy line separated the normative and the ideological from the descriptive and the empirical (and even this line was not always respected). No clear theoretical structure was ever developed, leading to a

wide range of contradictions in the literature. Huge barriers to entry were found to restrict participation in the pluralist competition among groups, challenging the view that politics could be understood through the observation of groups active in the pressure system. Fundamental issues remained unresolved about how to observe or measure the central concept of pluralism, influence. And findings accumulated with the suggestion that groups were not all that important to the governmental process in the first place.

Into this declining literature Mancur Olson added a crushing blow. *The Logic of Collective Action*, published in 1965, demonstrated convincingly that the group system could never be counted on for providing a fair system of interest representation, taking away any normative appeal that the group approach might once have had. Olson's elegant demonstration of the differential barriers to organization effectively put an end to the group approach to political science. Not only did Olson provide a devastating critique of the previous literature, which was suffering from the many problems we have reviewed in this chapter, but more importantly, he provided a new set of research questions for subsequent scholars to investigate. Olson's ideas paved the way for the creation of a new literature on interest groups. This new literature was quite different from the old, addressing a much narrower set of questions, but doing so with much greater analytic care and sophistication.