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Political Institutions of the State



Legislatures: Individual Purpose and Institutional Performance

Michael L. Mezey

For students of the United States Congress, the primary unit of analysis is the individual legislator and the things that he or she does. Typically, legislative behavior is explained in terms of the purposes of the legislator, and questions of structure and performance are approached from the perspective of members' goal-seeking behavior. Although individual behavior also is a concern of those who study legislatures other than the Congress, it is not as central to that body of research as it is to congressional research. Instead, comparative legislative studies usually begin with the legislature as the unit of analysis. Questions of institutional performance and the relationship between the legislature and other political institutions are at the heart of the inquiry, and structural and behavioral issues tend to be explained in institutional rather than individual terms.

Like trains proceeding on parallel tracks but in opposite directions, the Congress approach and the comparative approach visit most of the same topics, but in a different order and therefore from a different perspective. Because the volume of research on the Congress is so large relative to the comparative literature, it makes sense to order a review of legislative research in terms of the individual behavior approach that has characterized work on the Congress rather than the institutional approach that has characterized much of the comparative literature. The implications of this disjunction between congressional and comparative legislative research can be taken up at a later point.

The discussion of the legislative research literature will proceed, therefore, through the following five topics: 1) the behavior of legislators outside the institution; 2) their behavior inside the legislature; 3) the effect of legislative structure on behavior; 4) the performance of the legislature; and 5) the status of the institution in the larger political system.¹

Behavior Outside the Legislature

At one time, most of the scholarly work on the Congress was done at a distance, relying for the most part on public records or journalistic accounts. During the 1950s, however, a new breed of political scientists, working more in the tradition of anthropology, went to Washington to observe the Congress, to interview its participants, and to begin the process of developing explanations for the behavior that they observed. Out of their work emerged a "textbook Congress" characterized by a strong committee system, powerful committee chairs, a rigid adherence to the seniority system and to other unwritten rules of the legislative game, and party leadership based for the most part on personality and persuasion rather than on sanctions and coercion. A similar research style was followed by those who studied European legislatures, although their conclusions were, of course, different from those of their American counterparts.²

These studies of what went on inside the legislature accounted for most of the legislative research literature through the 1960s and continues to be an important interest of scholars working in this field. Beginning in the early 1970s, however, a new perspective emerged, among Congress scholars as well as some comparativists, that sought to explain behavior inside the legislature in terms of the relationship between legislators and their constituencies. This "outside" approach directed increased attention toward what legislators did when they were outside the legislature and away from the seat of government.

The Mayhew Approach

The connection between representatives and the represented always had interested normative political theorists (see Pitkin 1967) and a few more empirically inclined political scientists had sought to assess the

relationship between legislative roll call voting behavior and either the demographic characteristics of constituencies (MacRae 1958) or rough measures of district opinion (Miller and Stokes 1963). But in a book and an article, both published in 1974, David Mayhew suggested a different perspective on representation.

In *Congress: The Electoral Connection*, Mayhew (1974a) argued that what legislators did both inside and outside the Congress could be explained by their re-election concerns. Specifically, members engaged in credit-claiming, advertising, and position-taking activities as a way to ensure their re-election. Credit claiming involved performing particularized services for individual constituents and working for resource allocation decisions that benefited the constituency as a whole. Advertising referred to activities that made legislators widely known among their constituents, that established a "brand name" loyalty among them. And position-taking involved votes, explanations of votes, and other statements designed to establish one's solidarity with the constituency.

The success of this strategy was indicated by the fact that districts in the House of Representatives that could be classified as "marginal" -- won in the previous election by narrow majorities -- were "vanishing." Incumbent re-election rates, which had been around the 90% mark for most of the post-World War II period, approached or exceeded 95% in some years, but Mayhew's key point was not so much an improvement in the incumbent success rate, but the fact that their margins of victory indicated that most incumbents were not having very difficult re-election races (Mayhew 1974b).

The textbook Congress of decentralized power, strong committees, and weak party leadership described by the research of the 1960s was, Mayhew argued, designed to serve members' re-election needs. The re-election goals of legislators explained how they acted inside the Congress, the manner in which congressional power was distributed, and the nature of the decisions taken. Members responded to the perceived needs and desires of their constituents, power was decentralized so that members could act independently of party leaders on behalf of these local interests, and policies that might well be inefficient in their use of resources were nonetheless enacted because they spread electoral advantages to as many members as possible.

Books by two other scholars, Morris Fiorina (1977) and Richard Fenno (1978), published during the same period gave further detail and structure to the Mayhew themes. Fiorina explained the phenomenon of the vanishing marginals by arguing that legislators ensured their re-election by intervening with the bureaucracy to gain favors for individual constituents as well as favorable treatment in the disbursement of federal funds for the constituency as a whole. And they

guaranteed their role in such activities by their consistent support for legislation creating an activist government with a wide array of favors and money to disburse. Thus, as Mayhew suggested, the re-election goals of legislators had consequences for what they did in Washington.

Fenno's contribution was to describe in rich detail exactly what legislators did in their constituencies. By following a group of House members as they traveled in their districts, Fenno identified various types of "home style" designed to gain the trust of constituents. Credit claiming, position-taking, and advertising were all apparent in Fenno's discussion of representatives constantly in touch with their constituents, interested in their concerns, willing to explain their own actions in Washington in terms of those concerns, and working to build trust both to ensure re-election and to permit them to pursue other goals, such as power and good public policy.

Although the approach suggested by Mayhew, Fiorina, and Fenno altered the research agenda for students of the Congress, an interest in the constituency activities of legislators had been apparent in the work of several scholars studying legislatures outside the United States. As early as 1960 and 1961, interviews with legislators in the Indian state of Orissa and in Austria had found that these legislators spent most of their time running errands for their constituents (Bailey 1960; Crane 1961). Others had written about the "surgeries" that British MPs conducted at which they heard and tried to deal with constituent problems (Barker and Rush 1970; Dowse 1972), and still others had written about the constituency activities of legislators in Tanzania (Hopkins 1970), Colombia (Hoskin 1971), and Thailand (Mezey 1972).

The purpose of this is not to say who got there first, but rather to underline the fact that the representational activities that became central to congressional research during the 1970s seemed to be pursued by members of all legislatures. This constancy of constituency concerns, especially when compared with the much more variable policy-making roles of legislatures, suggested that representation was a more appropriate dimension for comparative legislative analysis than lawmaking. A prerequisite for such an approach, however, was a concept of representation broad enough to include the various activities that had been identified in these different research sites. Eulau and Karps (1977) took on that task. Following Pitkin, they used "responsiveness" rather than representation, and disaggregated the term into four categories: service, allocation, policy, and symbolic responsiveness. Service meant responding to individual constituent requests, and allocation to the resource needs of the constituency as a whole. Policy responsiveness looked to the effect of

constituent policy preferences on legislative decisions, and symbolic responsiveness referred to those activities that conveyed the impression if not the substance of the other three forms of responsiveness.

The Agenda and the Data

The research agenda suggested by this "outside" approach was enormous. In the case of the Congress, had the marginal legislators really vanished? Did incumbency explain congressional election outcomes and, if so, how much of the incumbency advantage was explained by representational activities of the sort described by Mayhew, Fiorina, and Fenno, and how much was explained by other factors such as the weakening of political parties, the growth of political action committees, or the strategic decisions taken by those who ran against incumbents? Were legislators really single-minded seekers of re-election or did they have other goals that drove them? Could the structure and operation of Congress be traced in its entirety to the re-election goals of members? Was all of this peculiar to the U.S. House of Representatives, or did it extend in whole or in part to the Senate, to state legislatures, or to legislatures in other nations?

Work on this agenda moved faster and further in research on the United States Congress than on other legislatures in part because more political scientists were interested in the Congress and in part because there was during the 1970s a veritable explosion of new data provoked in part by the theoretical and empirical questions posed by the outside model. In 1972, the first edition of the *Almanac of American Politics* was published, and subsequent biennial editions contained, for each legislator, election data, roll call votes on key issues, interest group ratings, and information on constituency characteristics. Beginning in 1981, *Congressional Quarterly's Politics in America* provided similar data and, over the years, the *Congressional Quarterly Weekly Reports* grew in size and comprehensiveness. In 1980, the first edition of *Vital Statistics on the U.S. Congress* was published containing a wealth of time series data on elections, campaign contributions, staff, and workload.

Beginning in 1974, the Federal Election Commission began to collect data on campaign contributions and expenditures by congressional candidates along with information on the campaign finance activities of political action committees. These found their way in aggregate form into the other reference volumes, while some scholars mined the raw data when it became available. In 1978, the Survey Research Center began to include questions on congressional voting in its national surveys. In 1978 and

1980 a congressional district sampling design was used and, beginning in 1988, a series of surveys were designed to look at Senate elections. Finally, Congress itself contributed to the data explosion by its decision in the reform era of the 1970s to allow recorded teller votes, thereby increasing the number of roll calls available for analysis, to open most of its committee meetings to the public, and to record committee votes.

Some new data also became available to those interested in comparative legislative research. Survey research data existed in several European countries, and the Center for Comparative Legislative Studies at the University of Iowa went forward with survey-based comparative projects on several legislatures in Europe, Asia, and Africa. The Research Committee on Legislative Studies of the International Political Science Association was formed and brought together scholars working on legislatures in different parts of the world. The Consortium for Comparative Legislative Studies funded and otherwise encouraged country research on legislatures many of which had never before been studied.³ In every edition, *Parliaments of the World*, published by the Inter-Parliamentary Union, contained more detailed information on more legislatures. Nonetheless, truly comparative data proved elusive and therefore most of the new research on legislative elections and constituency service dealt with the Congress.

An Increase in Service?

Comparing survey data from 1958 and 1978, Fiorina (1982) concluded that there had been an increase in the proportion of constituents who expected help from their legislators either with individual problems or with broader constituency projects. On the legislator's side, the magnitude of constituency service activities only could be estimated, but all sources indicated a heavy and increasing service case load and a higher level of importance attached to constituency service by members (Johannes 1984; Parker 1986; Cain, et al. 1987). What was easier to document than the expansion of constituency service itself was the expansion of resources that could be devoted to it; increases were detected in staff size, the number of staff people permanently stationed in the district rather than in Washington, mailings from legislators to their constituents, and the number of days during the year that legislators spent in their districts (see Mayhew 1974b; Parker 1986, 18; Jacobson 1987a, 43).

Measuring Marginality

Attempts to connect indicators of constituency service activities to the vanishing marginals had to deal

first with the measurement problem posed by the dependent variable. As political scientists sought to operationalize the concept of marginality, attention focused on incumbent re-election rates, margin of victory in previous election campaigns, and the meaning of the incumbency effect itself. The problem with congressional re-election rates was that they had always been quite high, and therefore it was arguable whether or not the slight rise in the 1970s was significant (Jacobson 1987b). If one focused on margin of victory, the question was what percentage of the vote made a district marginal, with cut-off points of 55% and 60% used in most analyses (Mayhew 1974b). Although margins in both congressional and state legislative elections definitely were rising (Garand 1991), Jacobson showed that vulnerability to defeat had not decreased. Thus, an "incumbent elected in the 1970s with between 60% and 65% of the vote was just as likely to lose in the next election as an incumbent in the 1950s who had been elected with 55 to 60 percent of the vote" (Jacobson 1987b, 130).⁴

A more complicated methodological question was the measurement of the "incumbency effect" -- that is, how much of a candidate's margin of victory was explained simply by the fact of incumbency. This involved distinguishing the votes that a candidate received because of partisan affiliation and because of local or national issues from the "personal" vote received because of the candidate's own qualities or activities.

One approach was to analyze survey data and through regression analysis attempt to distinguish between the variance in the vote explained by attitudes toward the candidate and that explained by other variables, particularly partisanship (Cain et al. 1987; Jacobson 1992). Although this method detects ample evidence of a personal vote for the incumbent, the variability of the results from one survey to another suggests that this is not a particularly reliable way to measure incumbency advantage.

Others have relied on the analysis of aggregate election returns, focusing on the "sophomore surge" -- the difference between a candidate's first election in an open seat race and the candidate's first re-election campaign -- and the "retirement slump" -- the difference in the vote for the incumbent in his last re-election campaign and the vote for his party's candidate in the next open seat campaign (Cover and Mayhew 1977; Alford and Hibbing 1981; Garand and Gross 1984; Breaux 1990). Gelman and King (1990) have suggested that these techniques are biased measures of the incumbency effect because they are based on the small number of districts in each election that have open seats or are being contested for the first time by first term legislators. Their "unbiased" estimate of incumbency effect, using all contested races and controlling for

previous votes and partisan swing, produces a somewhat higher estimate of incumbency advantage than other approaches (see also Jacobson 1990a, 30-32).

Not surprisingly then, the consensus of all of this work is that there is indeed an incumbency advantage, but that its magnitude depends on how it is conceptualized and measured. Explanations for the incumbency advantage have focused on redistricting, on the use by incumbents of the perquisites of office to increase their name recognition, on the effect that the waning influence of political parties has on voter behavior, on the representational activities that legislators perform, and on the nature of the opposition that incumbents face.

Service and Incumbency

The hypothesis that incumbency was a product of redistricting was rejected (Ferejohn 1977; Cover 1977; Born 1985; Glazer et al. 1987), and the name recognition hypothesis faltered when no significant increase was found in the frequency with which voters could recall the name of their legislators (see Fiorina 1989, 99). And although the increase in service activities and the resources devoted to those activities had been documented, and although it seems eminently reasonable to believe that members use their enhanced perquisites of office to their electoral advantage, a direct connection between these changes and either re-election rates or election margins has not been conclusively demonstrated. Johannes and McAdams (1988) find in the 1984 National Election Study data no connection between the vote and a recollection of the incumbent having performed service activities. Percentage of the vote won in a district is also found to be unrelated to various measures of constituency service and attentiveness gathered from congressional offices. The only exception appears to be staff size; Cain et al. (1987) find a strong correlation between staff size and voting for incumbents, and Holbrook and Tidmarch (1991) find a connection in state legislative races between the size of staff and sophomore surge. Rivers and Fiorina (1989) suggest that the allocation of resources such as time and staff serve to strengthen the incumbent's reputation for service, and that it is reputation for service rather than service itself that is connected with re-election rates.

The impact of constituency attentiveness on election outcomes may be conditioned by the electoral context. Parker (1986) finds that increased attentiveness results in improved electoral performance for those members of Congress representing states or districts where the opposition party is particularly strong. Similarly, King (1991) argues in a methodological piece that the partisan predisposition of voters in the district affects the level of constituency service that members

perform. These findings fit nicely with Jacobson's conclusion that although victory margins of members have increased, their electoral security has not, and with Hibbing's (1991, chap. 6) finding that increased tenure in the House is not associated with lower levels of district activity. Thus, all members are "running scared," and they attend to constituency service as a way to insulate themselves from the swings in electoral behavior that may well characterize the next election. Arguably, such activities have their most visible payoff for those legislators facing the greatest level of electoral uncertainty, that is, members representing districts with contrary partisan predispositions.

Political Parties

One of the first hypothetical explanations for the vanishing marginals pointed to the weakening role of political parties in American politics in general and in congressional elections specifically (Ferejohn 1977). In the first edition of *Congress: Keystone of the Washington Establishment*, Fiorina dismissed the hypothesis, but twelve years later, he concluded that the decline in party loyalties among the American electorate and the weakening of local party organizations require candidates to establish their own direct ties to their constituents based on reputation and service (Fiorina 1989, 112-15).

On the other hand, recent research by Paul Herrnson (1986, 1989) suggests a revitalized role for political party organizations in congressional campaigns. Although recognizing the candidate-centered nature of congressional elections, Herrnson finds that to an increasing extent national congressional campaign committees provide technical services, research, and funding connections for candidates, and state and local party organizations help candidates gauge public opinion and mobilize the electorate. He also finds that Republican organizations, particularly at the national level, have been more active in this respect than Democratic organizations.

Issues

Even if parties are playing a greater role in organizing campaigns than had been thought, party identification is clearly playing less of a role in structuring the decisions of voters (Jacobson 1992, 109ff). The decline of party may mean an increase in the personal vote of incumbents, but it also can mean that issues are becoming more important factors in voters' decisions.

In a number of articles published in the 1970s, Robert Erikson and Gerald Wright maintained that issues are more important in House and Senate elections than analyses that focus entirely on incumbency and party

would suggest (Erikson 1971; Wright 1980). Working with National Election Study data, Alan Abramowitz and Jeffrey Segal (1990, 576) conclude an analysis of the 1988 elections by noting that although incumbency and party identification were the most important factors in explaining congressional voting, "evaluations of national economic conditions and attitudes toward government spending had significant effects on voting for Senate candidates and attitudes toward government spending, abortion, and assistance to blacks had significant effects on voting for House candidates." However, they find that issues had a larger impact on Senate races than on House races, particularly those Senate races characterized by a sharp ideological contrast between the candidates (see also Wright and Berkman 1986).

What still isn't clear is whether issue voting is retrospective, as Fiorina (1981) has argued, or prospective. Although most political scientists continue to accept Fiorina's emphasis on retrospective voting, Lockerbie (1991) provides recent evidence that prospective evaluations may have been underestimated and retrospective evaluations overestimated. He also points to the effect that retrospective evaluations have on prospective evaluations.

National issues as well as partisan factors are involved in assessments of the influence presidential candidates have on the electoral fortunes of their party's congressional candidates. As party loyalty among the electorate has declined, split ticket voting has become more common, and the effect of "presidential coattails" has diminished (Calvert and Ferejohn 1983; Campbell 1986). In off-year elections, the president's party generally loses seats in the Congress, but there is little consensus on the reasons for this. Some have argued for the withdrawing of presidential coattails, others for an explanation focusing on differences in turnout between presidential and off-year elections, and others on the possibility of a vote against the president because of perceived poor performance. Robert Erikson (1988) finds all of these explanations wanting and argues instead that the midterm electorate has a propensity to engage in negative, or protest voting, thus penalizing the president's party simply for being the party in power (see also Alesina and Rosenthal 1989).

Challenger Quality

Gary Jacobson concludes a recent review of work on congressional elections with the observation that incumbent success continues to be attributable in large measure to "inexperienced challengers lacking adequate funds and resonant issues" (Jacobson 1990a, 133). Jacobson and Kernell (1983) first suggested this perspective in their discussion of the strategic decisions of

politicians. They describe politicians as rational actors who make decisions in the context of their career goals. Potential challengers with political experience and a current office would be unlikely to risk their careers on a race that they had little chance to win. Ironically, it was exactly such experienced politicians who, if they entered the race, would have the best chance to win. What then do individual candidates consider as they decide whether or not to enter a race?

Perhaps the first point to make is that all inexperienced challengers are not necessarily weak challengers. Green and Krasno (1988) create a measure of challenger quality that includes factors such as celebrity or occupational status, previous candidacies for office, and party activism as well as previous office-holding experience. And Canon (1990) notes that in certain situations, inexperienced candidates do get nominated and do win elections. In implicit contradiction to the strategic challenger model, Canon further suggests that most amateurs know that they have no chance to win, but enter anyway, driven by a number of personal and ideological goals beyond rational calculations of electoral benefit. This view is supported by earlier research (Maisel 1982) on the decision to enter primary campaigns. Assessments of the likelihood of winning are important, but so is a desire to air views or to provide an alternative to the incumbent. Although a majority of those who enter primaries do so with neither the consultation nor the encouragement of party leaders, primary winners were more likely than losers to have had such consultation and support. Recent research with a small group of primary candidates suggests that most had considerable prior experience with the formal party organization at the local or state level (Kazee and Thornberry 1990).

Still, for the strongest potential challengers, the decision will be not to run against an entrenched incumbent unless there are other variables that encourage them to make the race. For example, McAdams and Johannes (1987) find that the incumbent's record of constituency attentiveness is less important than his ideological position in attracting or discouraging a strong challenger. Moderate incumbents in particular seem to scare off strong challengers, with liberal Republicans and conservative Democrats facing the weakest challengers. Jacobson and Kernell (1990), drawing on data from the 1986 congressional elections, show that strong challengers capitalize on favorable national issues while weak challengers do not. However, Krasno and Green (1988) argue that district level political forces rather than the national political climate have the most consistent impact on the emergence of quality challengers (see also Canon 1990, chap. 4)

The other dimension to candidate quality is money. Research on congressional as well as state

legislative elections demonstrates that the amount of money spent by challengers has a crucial impact on election outcomes (Jacobson 1990b; Gierzynski and Breaux 1991). Although the consensus is that the level of challenger funding is the key variable, Green and Krasno (1988) maintain that, properly modeled, incumbent spending is just as important. And incumbents typically are well funded because of the strategic decisions of those who contribute to campaigns. Those with funds want to contribute to winners, and interest groups particularly want to contribute to incumbents whom they view as powerful (Grenzke 1989b). Challengers are well funded only if they are thought to have a good chance to win, a conclusion that interest groups are more likely to reach if the candidate has had previous political experience and is in an electoral situation that seems to offer some prospect of success (Jacobson 1990a, 96-102). That few challengers find themselves in such a position is suggested by Alan Abramowitz's (1990a) analysis of the elections of 1986 and 1988. He shows that House challengers are increasingly disadvantaged by rising campaign costs and a declining ability to attract funds. The quality challenger perspective also helps us to understand the more uneven record of incumbent success in U.S. Senate elections. First, senators are more likely than representatives to attract strong challengers (Jacobson and Wolfinger 1990; Squire 1990). A strong record of constituency service will not necessarily protect an incumbent senator, especially one running in a large state, from a strong challenger. Certainly, incumbent senators do not ignore service; on the contrary, the evidence is that they too have become increasingly attentive to their districts (Parker 1986). But the sheer size of the constituency, the much greater media attention to senatorial campaigns than to House campaigns, the greater likelihood that election outcomes will be influenced by issues and national political forces, and the greater likelihood that challengers will be experienced and well funded, means that such a record of service may be overwhelmed (Abramowitz 1988, 1990b; Fiorina 1989, 115-18; Miller 1990; Wright 1990). This interpretation is supported by the finding of a stronger incumbency advantage for senators from smaller, more heterogeneous states than from larger states (see Hibbing and Brandes 1983; Westlye 1983).

The Electoral Connection in Comparative Perspective

The linkage between constituency activities and re-election was plausible in the case of the Congress because national political parties seemed to have relatively little to do with election campaigns. Legislators who wished to return to the Congress had to establish

their own strong ties to their constituents. As the single member in Congress from their district, individual citizens and local interests would go to them with their problems; to the extent that members responded well, they would cement their ties with the district and increase their likelihood of re-election.

But what of countries with strong political parties that play a more dominant role in selecting candidates, in financing their campaigns, and in structuring the choices of voters and where individual candidates and their campaigns count for less? Why would legislators be responsive in a service or allocational sense in such political systems? Would there be any political payoff for such activities? Similarly, what about proportional representation systems in which several legislators represented the same constituency? Would citizens come to any or all of these legislators with service demands, would legislators feel obliged to respond, and, again, what would they gain from responding?

Electoral Systems and Parties

A comparative study of Kenya, Korea, and Turkey, relying on interview data with legislators, with elites, and with mass publics, suggests that the electoral system might be a factor. Relatively high levels of constituency service and allocational activity were found in Korea and Kenya, countries with single member district systems, while legislators in Turkey, a country with proportional representation, reported somewhat lower levels of such activities. On the other hand, constituents in all three countries attached the highest priority to the constituency service aspect of the MP's role, a finding that seems to be repeated in virtually every Third World country, regardless of the strength of its parties or the nature of its electoral system (see Kim et al. 1984, chaps 5 and 6; Cohen 1977; Barkan 1979; Valenzuela and Wilde 1979; Mezey 1985, 740-44).

One comparative study, drawing on interviews with American state legislators, found that the level of constituency service activities varied across and within states and that no conclusive link could be established between such activities and electoral victory margins. However, legislators from single member districts placed a "substantially greater" emphasis on service than legislators from multi-member districts (Jewell 1982, 146-47).

Finally, a comparison of the Congress and the British House of Commons, which necessarily holds the electoral system (single member district in both cases) constant, tests the effect of the stronger British party system on constituency service activity. Relying on interviews with legislators and survey data on constituent

attitudes in both countries, Cain et al. (1987) document, in Great Britain, an increase in constituency service activities from the mid-1960s to the late 1970s, an increase in constituent awareness of these activities, and an apparent increase in the frequency with which evaluations of service form the basis for constituent evaluation of MP job performance. MPs who engage in constituency service are also better known and more favorably evaluated than those who do less. And some connection between such activities and electoral success is found, although not to the extent indicated by the United States data (see also Norton and Wood 1990). Cain et al. conclude that, like their American counterparts, MPs are driven to perform constituency service by the desire for a personal vote and that this tendency has become more pronounced as the British party system has weakened. The status and votes that MPs acquire in their districts as a result of constituency service allow them to gain a degree of independence from national party leaders. On occasion, the party now finds that they need the member and his popularity as much as the member needs the party.

This suggestion of an electoral connection in Great Britain is supported by David Wood's (1987) finding that a significant number of Conservative MPs viewed lobbying on behalf of local industries as an important part of their job and that this was particularly so for legislators from politically marginal districts. However, Donald Searing (1985) argues that for those British MPs who are especially active in regard to service activities the desire to secure a personal vote is less important than the sense of satisfaction that they gain from being of service.

In addition to the United Kingdom and the United States, evidence of a personal vote has been found in several other single-member district systems including Australia, New Zealand, Canada, and Korea (see Bean 1990; Anagnason 1983; Krashinsky and Milne 1986; Park 1988). And in Brazil, Barry Ames (1987) found that during the 1947-1964 period when the legislature was an active participant in the budgetary process, members who planned on seeking re-election were especially interested in maximizing expenditures in their districts.

In some countries with proportional representation, the service link between legislators and their constituents seems weaker. In the Netherlands, for example, where there are no local constituencies and all candidates run on nationwide lists, voters say that they are unlikely to go to MPs with problems or complaints and MPs, in describing their roles, place very little emphasis on dealing with voters' problems (see Gladdish 1991, 100-102). In Germany, where some legislators are elected from single-member districts and others from

party lists, service and pork barrel activities are more common among the former (Lancaster and Patterson 1990).

When proportional representation systems use local lists and candidate preference options, service activities are more evident. In Ireland, the single transferable vote version of proportional representation encourages voters to indicate their preferences among candidates, and in one study a plurality of voters indicated that constituency service was the most important criterion in their preference decisions (Farrell 1985). Irish parliamentarians apparently respond to these expectations as suggested by re-election rates that rival those of their American counterparts (Darcy and Marsh, 1990).

In Belgium, where voter preferences are recorded but seldom affect election outcomes, legislators still act as ombudsmen for their constituents, intervening with government ministers and officials, introducing locally relevant bills, and attending local functions (MacMullen 1985). A similar commitment to casework emerges from interviews with Israeli legislators all of whom are elected from national lists (Uslaner 1985). Partisan factors may be one explanation for such activity in multi-member proportional representation systems. After all, names still appear on party lists, and some percentage of the vote for the party's list will be attracted by the presence of names that are known locally. Whereas the incentive for performing constituency service activities in single member district systems may well be the rational legislator's desire to maximize a personal vote, in multi-member systems the incentive may well be provided by rational parties encouraging their incumbents to act in ways that will maximize the party's vote. This will be especially the case in systems where parties design their national lists to appeal to particular constituencies within the party. Legislators in Israel, for example, are aware of these *de facto* constituencies, and direct their service activities at these groups (Uslaner 1985).

Electoral Consequences

The consequences of constituency activity have not been studied as persistently as they have been in the United States, and the research that does exist leaves one uncertain about the magnitude and importance of a personal vote compared with party and issue factors. In Japan, for example, a system with multi-member constituencies but a tradition of aggressive constituency campaigning by individual candidates, party still accounts for significantly more of the variance in votes than candidate or constituency factors (Richardson 1988). In Canada, where incumbency has been shown to be a

factor, national issues still remain important. There, Happy (1989) finds that retrospective evaluations in regard to the economy have a significant and negative impact on votes for incumbents.

Behavior Inside the Legislature

Definitions and Measures of Behavior

To what does behavior in the legislature refer and how can it be measured? The most durable response to both questions has been the roll-call vote. The casting of a vote is clearly behavior, and clearly important behavior because a defining feature of legislatures is that they make decisions through a voting process. It also is easily measurable behavior. Because legislatures are public bodies that keep records of how their members vote, it is relatively simple to gather roll call voting data, develop indices that distinguish among the voting records of different legislators, and then search for variables that explain why they vote as they do.

Although there is much in the way of legislative behavior that is not captured by the roll call vote and there are limitations to what roll-call data can tell us about the congressional decision-making process, scholars have stuck with these data because they are "hard" and they are available in a number of different legislative settings, often over an extended period of time (see VanDoren 1990; Kingdon 1989, 10-11). Other measures of behavior are in contrast softer and therefore more difficult to quantify, more difficult to gather for a large number of legislators, and do not necessarily come in comparable categories across time and space. These include the speeches that legislators give, the bills and amendments that they introduce, the questions that they ask of the government in parliamentary systems, and the positions that they take and the strategies that they follow during less public phases of the decision-making process (but see Hibbing 1991, chap. 5).

This emphasis on roll call data has hindered comparative research. In many countries a legislator's vote is determined by his party leader or by executive elites outside the legislature. In other systems, legislatures have little apparent policy-making influence, and therefore the votes that members cast are not of interest. The more consequential aspects of legislative behavior in these systems are exactly those activities that are less public and less easily measured. Thus, the "softer" nature of research on many non-American legislatures is in part explained by the absence or irrelevance of the "hard" roll call data that students of the Congress favor.

Roles and Goals

A different category of behavioral data comes from the responses of legislators to questions about how they perceive their legislative roles and how they decide what to do in the legislature. Although role perceptions are supposed to explain behavior, they are often used as measures of behavior. The pathbreaking study in this genre was the analysis by Wahlke et al. (1962) of the roles of legislators in four American states. Through interviews with legislators, various orientations toward the purposes and processes of the legislature, toward the constituency, and toward representational style were identified. These categories have been replicated in a number of legislative settings. Interviews tapping purposive role orientations of legislators in African, Latin American, and Asian nations revealed the importance that these legislators attached to the performance of constituency service activities. This finding revived the interest of scholars in legislatures that had been ignored because their members did not seem to have any influence on the law-making process (Hopkins 1970; Hoskin 1971; Mezey 1972; Mezey 1985, 740-44). Research on role orientations associated with representational style has been conducted in the United States House of Representatives (Davidson 1969; Kozak 1984) and in legislatures in at least eleven other nations in Europe, Asia, Africa, and Latin America (see Mezey 1979, 172).

Although widely gathered, role orientation data have not been linked with actual legislative behavior. That is, the consequences of different role perceptions for what the individual legislator does are not clear. Nor is it clear what different collective role patterns mean for the legislature as a whole. A role consensus within one legislature different from that in another legislature may tell us something about the functions of each legislative body. But role consensus is difficult to find (Kim and Patterson 1988), and the functional implications of different role orientation patterns remain ambiguous (see Jewell 1985, 125).

Recent work on the United States Congress has retained the purposive perspective of role analysis but abandoned the role orientation categories in favor of a discussion of legislative goals. This work has divided between Mayhew's emphasis on the re-election goal and models that see legislators motivated by a more complex mix of goals. The multiple goal approach can be traced to Joseph Schlesinger's (1966) suggestion that behavioral differences could be traced to differences in ambitions. Legislators with progressive ambitions hoped to move to higher office; those with static ambitions planned to stay where they were; and others with discrete ambitions wished to complete their terms and leave office.

Politicians, Schlesinger argued, adjusted their behavior to their ambitions.

Richard Fenno (1973) talked about goals rather than ambition and suggested that legislators pursued three goals -- re-election, power, and good public policy -- and that the relative importance of each goal might vary during a member's career (Fenno 1978). This perspective was refined a bit further by Richard Hall (1987) who, drawing upon Dodd's (1977) analysis of the power goal, distinguished the goal of making a personal mark on public policy from the goal of good public policy. John Kingdon, in his comprehensive analysis of legislative decision making, used Fenno's categories differently phrased -- satisfying constituents, intra-Washington influence, and good public policy -- but seemed to agree with Mayhew that "the congressman considers the constituency interest first" (Kingdon 1989, 248).

Also in accord with Mayhew is much of the scholarship categorized under the rubric of "the new institutionalism" (March and Olsen 1984). Emerging from the public choice literature, this approach views legislators as rational actors with electorally induced preferences among policy alternatives. Public choice analysis develops models of decision making that assume that these preferences are heterogeneous among members of a collective body such as a legislature or a committee. Each legislator has an ideal preference based upon constituency and electoral factors, and behaves in a manner that optimizes that preference. However, what they are able to accomplish depends upon a number of factors: their uncertainty about the preferences of other participants, the voting strategies that they follow and believe that their colleagues are following, and the institutional factors that structure the choices that they and their colleagues must make.⁵

Constituency Influence

Speculating about the influence of constituency and electoral factors on legislative behavior has proven to be easier than empirically mapping that influence. One problem has been measuring constituency views. In the era before survey research, scholars used demographic data on the constituency as a surrogate for attitude data (Turner 1951; MacRae 1958; Froman 1963). Obviously, the attribution of attitudes to constituents based upon the constituency's aggregate demographic characteristics is at best problematic. The breakthrough in this area occurred with the work of Warren Miller and Donald Stokes (1963) who, using survey data, estimated constituency opinion in 116 congressional districts and coupled those estimates with the voting records and preferences of the House members who represented these districts. They found rather weak correlations between roll call voting

behavior and constituent attitudes and a similarly weak relationship between the views of representatives as expressed in interviews and the views of their constituents. Although criticized on methodological grounds having to do primarily with the small sample sizes upon which their assessments of constituency opinion were based (Fiorina 1974; Erikson 1978), the main contours of the Miller-Stokes findings never have been convincingly refuted and have been applied in at least one other national setting.

Using a similar model and techniques, Converse and Pierce (1986, chap. 19) find that the relationship between constituent views and the roll-call votes of members of the French National Assembly resembles what Miller and Stokes found for members of the U.S. House. They detect a slight connection between the relatively rare defections from party discipline that occur in the French National Assembly and district views, but not surprisingly, the most salient of their findings was that party was the strongest predictor of how French deputies vote.⁶

The empirical finding of weak correlations between constituency views and the behavior of legislators was puzzling in light of interview data suggesting that legislators always considered constituency opinion when deciding how to vote (see Kingdon 1989). One way to solve this puzzle was to ask how legislators conceived of constituency opinion. Although political scientists and legislators agreed that citizens ordinarily were not aware of very many public issues, legislators could not always anticipate those issues about which constituents ultimately might care. And when voters did care, as Miller and Stokes demonstrated with the high salience issue of civil rights, a stronger connection between constituency opinion and roll call voting obtained.

However, constituency also could be a factor on low saliency issues. In these cases, legislators said that even though their constituents might not be aware of how they voted, it was possible that a future opponent might turn the vote into an issue in the next campaign (Kingdon 1989, 60). Thus, a legislator's decisions could be influenced less by current constituency opinion than by an assessment of future constituency opinion and the retrospective judgments that citizens might make after the vote. Arnold (1990, chap. 3) argues that citizens are more likely to make these retrospective evaluations when they can trace the specific effects of a public policy decision back to the legislator. Such tracing requires, among other things, an identifiable action on the part of the legislator, such as a roll call vote. On this reasoning, the legislator will be less likely to be affected by the prospective views of constituents -- their attitude toward the issue when the legislator votes -- than by their

retrospective views -- how they will evaluate the vote after it is cast.

But even if legislators know how their constituents will respond to their vote, they still might vote contrary to constituency wishes. Legislators think that they can deviate from constituency views on occasion, but that a "string of votes" against the expressed views of their constituents is riskier (Kingdon 1989, 41-43). Fenno (1978) argued that legislators might take such a risk if they thought that they could explain their vote in a satisfactory manner and if, through their attentiveness to the constituency, they had established a level of trust that would lead constituents to accept their explanation. Such explanations become particularly important when legislators vote in a sophisticated, or strategic manner -- that is, when they cast a vote against constituency views in order to achieve a final outcome that their constituents would prefer (see Denzau et al. 1985). The difficulty of making such an explanation, however, may well limit the number of cases in which members will vote strategically (Wilkerson 1990).

A second approach to the puzzle of relatively weak links between constituency opinion and roll call voting behavior focuses on the definition of constituency. Observers of the United States Senate noted that it was quite common for two senators representing the same state (and thus the same geographic constituency) to be from different parties and exhibit different voting patterns. Fiorina (1974) explained this phenomenon by suggesting that senators vote in accordance with the preferences of their own constituents, defined as their co-partisans. Thus, senators from the same geographic constituency could be representing different ideological segments of the constituency (see also Poole and Rosenthal 1984). This insight, combined with Fenno's (1978) parsing of the term "constituency" into a number of different components (personal, primary, re-election, and geographic) suggested that analyses of constituency influence had to specify which constituency was being discussed.

Preliminary support for this view came from Stone's (1979) finding that the relationship between the attitudes and votes of legislators and the attitudes of their partisans in the constituency was stronger than the relationship with the attitudes of the constituency as a whole. Converse and Pierce (1986) reached a similar conclusion in their study of representation in France. That various constituencies might be incorporated into the same geographic constituency was also suggested by the finding that heterogeneous states were more likely than homogeneous states to have mixed party representation in the Senate (Bullock and Brady 1983).

The availability of fresh constituency opinion data from recent National Election Surveys has permitted

a better test of these ideas. Drawing on such data Shapiro et al. (1990) conclude that the strongest explanatory variable for senators' voting records is the weighted opinions of their co-partisans in the constituency, but that the attitudes of independent constituents also have an effect. Gerald Wright (1990) agrees that the views of independent voters are important, but he discounts the influence of co-partisans. Instead, he emphasizes the impact of the ideology of elites in the senator's state party — convention delegates and county chairs — on the voting behavior of senators.

Interest Groups and Attentive Constituents

Wright's finding in regard to local elites makes the point that all constituents are not equal. Students of public opinion had long recognized the important role of attentive constituents — those who followed public policy issues and the activities of legislators and who might also serve as opinion leaders for other less informed citizens. Darrell West's (1988) study of legislative voting on the 1981 Reagan economic package shows that the opinions of activist constituents, defined as those who wrote or phoned about the issue, explained more of the variance in roll call voting than district opinion as measured by 1980 NES data.

Although relatively little research has been done on attentive publics, a great deal of literature exists on organized interest groups, a special class of attentive publics. These groups and the political action committees that they organize to contribute to congressional campaigns are typically viewed in the popular press as having a particularly strong influence on congressional decision making, but attempts to demonstrate such a connection have produced mixed results. Some have found a connection between votes and campaign contributions (Frendreis and Waterman 1985); others find it on low visibility rather than salient issues (Schroedel 1986), and still others have concluded that there is no connection at all (Welch 1982; Owens 1986; Grenzke 1989a). Wright (1985) finds that PAC contributions occasionally explain a significant amount of the variance in voting behavior and then only when other cues are particularly weak. In another research effort, he concludes that the number of lobbying contacts that legislators have with an interest group explains more about their policy choices than campaign contributions explain (J. Wright 1990). Hall and Wayman (1990) take a different approach, directing attention away from roll-call votes and toward participation in committee deliberations and more generally toward the resources that legislators devote to a particular issue. They argue that the purpose of campaign contributions is to mobilize the committed to activity and to demobilize opponents, rather than to

change legislators' minds on the issue itself. On this reasoning, a correlation between contributions and voting behavior would not appear.

Although it is important to recognize the impact of organized and attentive publics, the activities of such groups are still constrained by the views of disorganized and inattentive interests. Arnold (1990, 64-71) discusses the calculations that legislators must make as they choose between the usually unambiguous views expressed by attentive publics and the possibility that inattentive publics might be roused to attention by organized interests or by "instigators" with a contrary view. In a more formal approach to the issue, Dezaun and Munger (1986) argue that constituent preferences are represented even when they are disorganized. That is because interest groups concentrate their efforts on legislators whose constituents have no preferences on the policy that concerns the group rather than on those legislators likely to be constrained by constituency opinion.

In countries with stronger party systems, attentive publics and interest groups often seek direct representation in the legislature. In France, for example, it is possible to hold a parliamentary seat and a local elective office simultaneously. In 1988, nearly half of the members of the Chamber of Deputies were sitting mayors (Frears 1990). Group representatives also serve as legislators. During the period 1972-1987 more than half of the German Bundestag performed either full-time or honorary functions for associations and societies with an interest in government policy; these members often served on the parliamentary committee dealing with the concerns of their group (Muller-Rommel 1990). A similar pattern of interest group representation is found in Austria (Pelinka 1985). In Great Britain more than half of the Labor Party MPs serving during the 1980s were union sponsored and, in 1987, more than 80% of all MPs registered as having some pecuniary involvement with an outside interest group (Shaw 1990). A survey of interest groups found that 75% regularly contacted individual MPs, with the most common request being to put down a parliamentary question or arrange a meeting for the group (Rush 1990).

Political Parties

Parties can influence legislative behavior in a variety of ways. Authoritarian parties control the actions of legislators who are members of the party through their national party apparatus (see Nelson 1982). In other political systems, external party organizations may be less important, but the parliamentary party is highly disciplined. And finally, there are political systems where party organizations have little or no influence on what legislators do (see Mezey 1975). In the case of the United

States Congress, it is difficult to imagine a member considering the views of his party's national committee on a pending issue. In Italy, however, members view themselves as representatives of their party rather than their constituency and so national party positions count for a great deal (Pridham 1985).

As for legislative parties, they have been assumed to be stronger in parliamentary systems than in the United States. In the case of the British Parliament, for example, the traditional wisdom had been that party discipline was absolute and that those who rebelled against the leadership, especially in situations when they might deprive a government of its majority, risked sanctions running the gamut from expulsion from the party to a permanent career on the backbench. However, the most persistent student of dissent in the House of Commons demonstrates that this textbook description has not applied, at least since 1970. Members are much more willing to vote against their party leaders, and they have done so even when the effect of such dissent was to endanger the government's majority (Norton 1980, 1985). A more recent analysis even suggests that rebellious members may not necessarily be cutting themselves off from future cabinet posts (Piper 1991). Some of this deviation from the party line is done with electoral considerations in mind; Mughan (1990) finds that the incidence of rebellion is likely to increase as a party loses popularity, and as it nears the next general election. The connection between rebellion and electoral concerns lends further credence to the notion of a personal vote as a factor in British parliamentary elections.

Data such as these should not obscure the fact that party discipline remains the key element in structuring legislative decision making in most parliamentary systems. In Scandinavian legislatures, for example, Damgaard (1990) points out that even though legislators are locally elected, "they primarily consider themselves as representatives of their party." Even in Britain, Richard Rose (1986, 30) reminds us that "by comparison with the mid-19th century House of Commons, party cohesion and discipline are far stronger today than in the past." And when Hibbing and Marsh (1987) analyzed those votes in the British House of Commons on which party leaders did not impose the whip, they discovered that party remained the most important influence on these voting decisions.

On the other hand, discipline is not simply imposed from the top down; rather, party positions often emerge from discussions among leaders and backbenchers, and party leaders refrain from making proposals that will provoke opposition among their followers. Therefore, party discipline may be partially the product of anticipated reaction -- that is, leaders deciding not to propose policies that they know their followers will oppose (see Mezey 1979; Laegrid and Olson 1985).

While the theme of party discipline in parliamentary systems may be somewhat overdrawn, there is also evidence that the theme of weak party leaders, independent legislators, and strong committees that dominated much of the thinking about the American Congress during the 1960s and 1970s is in need of some modification. Analyses had tracked the decline of party voting during the 20th century and the rise of both cross-cutting and bipartisan coalitions (Brady et al. 1979; Collie 1988a).⁷ But after 1980, party discipline in the House rose and party leaders improved their capacity to lead. This happened because sectional differences within the Democratic party diminished and because the rules of the House changed to allow the majority party to control the agenda if it was so inclined (Rohde 1992). According to Barbara Sinclair (1983), leaders adopted a service orientation toward rank and file members as a way to increase the prominence of the party, included as many members as possible in the coalition-building process, and used the rules of the House to carefully structure the choices made on the floor. Bills were referred to several committees as a way to enhance leadership control of legislation (Davidson et al. 1988), and the use of special rules increased (Bach and Smith 1988). In David Canon's (1989a) view, these leadership changes in the House are institutionalized in the sense that they transcend the leadership of one Speaker. But a longer perspective warns against viewing changes of this sort as permanent; as Cooper and Brady (1981a) have noted, leadership styles and strategies are influenced by institutional contexts and the nature of party strength (see also Peters 1990).

There are also some indications of increased party unity in the Senate (Rohde 1992). This isn't surprising because party strength in both chambers is affected by the same set of external forces (Hurley and Wilson 1989) even though different variables will explain more of the variance in one chamber than in the other (Patterson and Caldeira 1988). However, the task of party leadership in the Senate continues to be daunting. Fenno (1989a) suggests that the Senate of the 1950s and 1960s about whose folkways Donald Matthews (1960) wrote was a "communitarian" body in which group norms were respected and the committee system was all powerful. But the modern Senate, in Fenno's view, has evolved into a dominantly individualistic body in which committees and leaders have much less influence than they had in the past and assignments to prestigious committees are more equitably distributed to satisfy senators who wish to be involved in a number of policy areas (see Sinclair 1988).

Senate leaders, unlike their House counterparts, have not responded with new procedures to control this norm of increased participation (Smith 1989, chap. 4). Multiple referral of bills has not become a leadership tool in the Senate; rather, leaders use it to serve the needs of

individual senators (Davidson 1989). Unanimous consent agreements have become complex documents meticulously negotiated so that the concerns of many individuals are addressed; they seem to allow leaders to move the business of the Senate forward, but they also enable individual senators to get a great deal of what they want (Krehbiel 1986; Smith and Flathman 1989).

Executive Influence

Discussions of the influence of political executives on legislative decision making divide between parliamentary systems in which legislative decision making is often assumed to be controlled by the executive and presidential systems in which power is shared between an executive and a legislature structurally independent of one another. The first point that needs to be made is that all parliamentary systems are not alike. As Richard Rose (1986) shows, British governments during the 1970s saw more than 93% of what they proposed pass the House of Commons, but the comparable figure for Canada was 64%. On the other hand, in Great Britain, approximately 30% of the private member bills proposed each session are passed (Marsh and Read 1988), while in Canada the comparable figure, at least through the 1960s, was only 2% (Mezey 1979, 88). Also, governments must contend regularly with actual or anticipated backbench opposition, and high success rates may occur because the government decides not to push legislation that will cause conflict among its supporters in the legislature or even unduly arouse the opposition party if the government's majority is fragile enough (see King 1976).

In the United States, presidents always have had a difficult time influencing legislative decision making (see Fisher 1991). Connected with their legislative co-partisans by only the most tenuous ties and often confronting a Congress controlled by the opposition party, presidents nonetheless require congressional support for nearly all of their policy initiatives. Their success in gaining such support has been uneven. The personal vote that enables legislators to maintain their distance from party leaders works just as effectively against the president. Of course, there are instances when the influence of the president is strongly felt (Lockerbie and Borelli 1989); presidents do have advantages when dealing with members of their own party (Bond and Fleisher 1990, chap. 4) and may exert more influence on foreign and defense policy than on domestic policy (Shull 1991). Nonetheless, the tension between the distributional policy outcomes favored by Congress and the more national perspective of the president are omnipresent (Mezey 1989). On most issues, most of the

time, presidential influence is felt "at the margins" (See Edwards 1989).

Discussions of presidential strategies for influencing legislators have for the most part relied on anecdotal or case study data (see Wayne 1978; Edwards 1980). A more rigorous game theoretic analysis by Terry Sullivan (1990) finds that presidential compromises produce relatively few votes for the president in Congress; rather, presidential success depends upon the conversion of the president's own partisans, who decide to give support largely because of strategic considerations. Cary Covington (1987) comes to a substantively similar conclusion, finding archival evidence that presidents devote their efforts to mobilizing their "core supporters" most of whom are their co-partisans.

Decision-Making Models

How do legislators decide, given the multiplicity of pressures and the large number of decisions that they must take? By the end of the 1970s, researchers had arrived at models that assumed that legislators looked for ways to simplify the decision-making process. They could do so by assessing opinions of the various actors who are important to them (Kingdon 1973), by relying on the cues of colleagues whom they respect (Matthews and Stimson 1975), or simply by voting as they had on similar issues in the past (Asher and Weisberg 1978). Legislators with intense policy preferences choose cues from policy specialists, but on issues with constituency implications, cues are sought from those with information about district preferences (Hurwitz 1988). More recently, Arnold (1991) has returned to the Mayhew and Kingdon perspective, arguing that all decisions begin with assessments of whom in the constituency cares or might care about the vote and whether or not those who care can trace the consequences of the vote back to the legislator. Those seeking to mold majority coalitions understand this calculus and derive a number of procedural and substantive strategies from it that will weaken or break the traceability chain, thereby insulating the legislator from electoral reprisals.

Another view of the decision-making process comes from the policy-voting perspective introduced by Clausen (1973) and utilized as well by Sinclair (1982). Their approach argues that legislator decision-making varies according to the issue, with some external and internal actors more important for certain issue categories than others. In recent years, this perspective has been challenged by Keith Poole and his colleagues (Poole 1981; Poole and Daniels 1985a; Poole and Rosenthal 1985b; Poole 1988) who have argued that roll-call voting in the United States Congress can be characterized by a single left-right dimension that has persisted since the

creation of the republic. This means that the ideology of legislators explains a great deal about how they vote, particularly in certain foreign and defense policy areas where it appears to explain more about voting behavior than either party or constituency (McCormick and Black 1983; McCormick 1985; Lindsay 1990; Overby 1991).

As is so often the case with roll-call studies, methodological questions arise. In this case, those taking the multidimensional perspective favor cluster analysis and those arguing for a unidimensional solution use multidimensional unfolding analysis (Koford 1989). At this point, the multidimensional school, represented by Clausen and Wilcox (1991), has conceded that the liberal/conservative dimension may in fact incorporate some of the separate dimensions identified by Clausen but that other dimensions remain distinct. More importantly, they argue that the process of change in Congress is more accurately tracked by a multiple dimension approach, and that such an approach allows the deviation of individual and group voting records from ideological consistency to be explained in terms of policy substance. For his part, Poole (1988) suggests that the multidimensional and single dimensional approaches are theoretically reconcilable under a "belief system" rubric which, he argues, characterizes individual members as well as legislative parties, the latter defined as individuals with similar patterns of constraints over issues.

Students of the Congress have not been very interested in voting behavior in parliaments outside the United States because they assume that these legislatures are characterized by strong disciplined political parties neatly arrayed on a left-right continuum. Although spatial analyses of voting have suggested that this is the case in, for example, the Netherlands (Wolters 1984), in other countries there have been multidimensional findings. In Ireland, for example, Sinnott's (1989) multidimensional analysis of preference data from a sample of parliamentarians uncovers a two-dimensional policy space -- left-right/nationalism and confessionalism -- along with substantial intraparty variation. Shaffer (1991) also finds a two-dimensional solution -- left/right and urban protest-rural traditionalism -- in his analysis of roll calls in the Norwegian Storting.

Structure and Behavior

How legislators reconcile or decide among the external and internal actors that seek to influence their voting decisions is in part a product of the structure of the legislature. In this case, structure incorporates the various decision-making arenas in a legislature and the formal and informal rules that constrain behavior within these arenas.

Committees

Modern research on the United States Congress has emphasized the committee arena ever since Richard Fenno reminded a generation of scholars of Woodrow Wilson's aphorism that Congress in its committee rooms was Congress at work. Because all legislatures have committees, these bodies also have been discussed cross-nationally. The difference has been that studies of the United States Congress have focused on the individual behavior and collective norms that characterized committees and on their relationship with their parent chamber while research in other countries has been concerned primarily with the power or importance of the committee system in the context of what is usually a stronger party system. Such studies typically have paid little attention to what happens inside committees (see Lees and Shaw 1979; Eulau and McCluggage 1985).

In regard to the internal workings of committees, the research approach pioneered by Fenno (1966) in his study of the Appropriations Committee viewed committees as social systems characterized by varying degrees of integration. Fenno's work on committees eventually moved to an emphasis on the goals of members and the joint impact of these goals and the environment within which the committee operated on its decisions and decision rules (Fenno 1973). More recent research, utilizing the rational choice perspective, models committee decision making in terms of the interaction between member goals and parent body constraints in the form of procedural rules.

Formal analyses of committee power begin with the benefits of the committee system for individual legislators. Drawing on Mayhew's view that legislative structure follows from the electoral motivations of legislators, these models argue that a committee's power is based on its control of policy initiatives that lay within its jurisdiction and therefore on its capacity to allow individual members to secure gains calculated in terms of electoral benefits. Legislators not on the committee defer to the views of those who are, either because they respect their expertise or because they expect this deference to be reciprocated when their own committee proposals come before the legislature. Such mutual deference allows legislators to maximize the electoral benefits that they gain from committee decisions. In this manner, legislative structure produces a stable decision point, or equilibrium, among the competing electorally driven demands of legislators (see Shepsle 1979). In a later paper, Shepsle and Weingast (1987) developed a more complete model of committee power that gives special prominence to the sequence of legislative policy making. They argue that members defer to committees not only because of the committee's agenda-setting power but also because of the

committee's power, at the conference stage of deliberation, to make final changes in the bill and then confront their colleagues with a straight up or down vote.

This view of committees as autonomous bodies composed of members with policy views quite different from those of the modal member of Congress has come under increasing criticism. Krehbiel (1990) finds little support for the hypothesis that the views of committee members are more extreme or more homogeneous than those of the legislature as a whole. And, in an earlier piece, Krehbiel (1987) suggests a number of ways in which the parent body can undermine the conference role of the committees. Smith (1988) argues that the Shepsle/Weingast model fails to take into account the different strategies that committees follow, strategies that are influenced by the goals of the committee members. And a more recent analysis of the House Appropriations Committee challenges the assumption that committees are autonomous from party leadership, arguing instead that committees are agents of party leadership and that the membership on the committee as well as the actions of the committee have become increasingly responsive to the views of party caucuses (Kiewiet and McCubbins 1991). Gerald Strom (1990, chap. 7) also suggests that committee decisions are heavily influenced by what they anticipate will be acceptable on the floor.

In contrast to a perspective on committee power which focuses on how committees can advance members' re-election goals, Krehbiel argues for a view of committee power based on the informational benefits that the Congress and its members gain from specialized committees. He believes that the electoral benefits (or distributional) explanation needs to be tempered by an understanding of the need that legislators have for information that will reduce their uncertainty about the link between policy and "outcomes," the latter defined as the effects of the policy adopted. From this perspective "committees earn the compliance of their parent chamber by convincing the chamber that what the committee wants is in the chamber's interest" (Krehbiel 1991, 256). This accords with the idea that legislators are motivated by multiple goals, that re-election is simply one of these goals, and that good public policy and making one's mark on legislation are also important goals. In this vein, Hall (1987), in a study of member participation on the House Education and Labor Committee, finds both electoral and non-electoral motivations for participation, with electoral motivations more prominent in public arenas and the legislator's desire to leave his mark on public policy more important behind the scenes. Strahan (1990), in his study of the Ways and Means Committee, also finds less evidence of distributional policy making and more evidence of the concern with policy substance that Krehbiel's analysis emphasizes.

Finally, some have taken the increased activity on the floor of the Congress, in particular an upsurge in amending activity, as an indicator that the power of committees over the content of legislation has eroded (Bach 1986; Smith 1989). But Weingast (1992) argues that much of the new amending activity is a reflection of subcommittee or committee efforts to hold its coalition together either by fighting opposition amendments with neutralizing counter-amendments, or by accepting amendments that are harmless or that are necessary to maintain majority support. He concludes that increased amending activity says less about the decline of committees and more about the decline of norms that had previously mandated that deals be worked out in committee rather than on the floor.

Parties and Committees

In parliamentary systems, deliberations within the legislative party tend to be more important than what happens in committees. Such an inverse relationship between the importance of committee arenas and the importance of party arenas (see Shaw 1979) is illustrated in recent research on the committee system in the British House of Commons. One study, drawing on interviews with MPs, finds that backbenchers have used the recently strengthened committees to monitor and criticize their party's leaders and policies. Thus, MPs see the committees as instruments for strengthening their own role at the expense of the party organization (Jogerst 1990). For this reason, British governments always have worked to keep parliamentary committees weak. On the other hand, in Germany and Italy, strong parties and strong committees co-exist. In both countries, they provide a more private arena than the chamber floor for government and opposition bargaining (see Saalfeld 1990; Di Palma 1977).

With certain exceptions, most of the bargaining in parliamentary systems takes place within and between political parties. Because data on intra-party bargaining is difficult to retrieve, more research has been done on the process of inter-party bargaining. Formal models of legislative decision making have familial connections with this research as it relates to the formation of cabinets in parliamentary systems. Long before formal models were applied to the American Congress, scholars had constructed game theoretic models with parties rather than individual legislators as actors, in an attempt to understand the conditions under which coalitions form and the size and durability of those coalitions. Unlike models of legislator behavior that needed to accommodate difficult problems such as a large number of legislators, information uncertainty, and repeated plays (i.e., many decisions among the same group of actors), the cabinet

coalition application was easier because there were fewer actors (parties), much less information uncertainty, and a decision-making process that might not need to be repeated until the next election. These factors allowed for the construction of formal deductive models of coalition behavior that were good predictors of the composition and size of the coalitions that would be formed in multi-party systems and of how coalitions would apportion cabinet portfolios and other goods among themselves (Groennings et al. 1970; Dodd 1976). But critics of these models have suggested that they neglect questions of coalition politics, alliance strategies, the nature of decision making within political parties, and the effects of different ideologies and motivations on party behavior (see Dodd 1984; Pridham 1986; Bogdanor 1983).

On the Floor

In contrast to intra-party deliberations, what happens on the floor of the legislature happens in public. Thus, there is a wealth of literature on the floor behavior of legislators, particularly in the United States Congress. The function of floor discussion raises some interesting comparative questions. Steven Smith makes the distinction between debate, which is arguing from fixed positions, and deliberation, which involves a more reasoned discussion of various options with a view to seeking consensus. Although Smith (1989, chap. 8) concludes that the House emphasizes debate over deliberation while the Senate does the reverse, most analyses of the American Congress take a deliberative perspective in light of the determinative impact of congressional decisions on public policy.

In other nations where, because of party discipline, legislative outcomes are seldom in doubt, floor discussions can be more accurately characterized as debates, but this too has a function. For example, one study of the Parliament of the French Fifth Republic concludes that debate is used as a way to move issues off the private agenda of cabinets and bureaucrats into the public realm (Baumgartner 1987). Although debate in Congress might well have the same agenda setting impact as in other countries, there has been virtually no systematic exploration of this point. One exception is Kingdon (1984) who finds little connection between committee hearings on a particular subject and the prominence of that subject on the policy agenda.

Rules

The operation of the legislature in its various decision-making arenas is governed by a set of written and unwritten rules that determine how the institution

does its work. Matthews' (1960) pathbreaking study of folkways in the United States Senate identified a set of unwritten rules of behavior, or norms, that governed the behavior of its members. These and other Senate norms were empirically explored in a series of articles by Ralph Huiitt (see Huiitt and Peabody 1969). Subsequent research traced the decline of many of these norms, including reciprocity, interpersonal trust, specialization, and apprenticeship (Asher 1975; Smith 1989; Uslaner 1990), although more recent research by Hibbing (1991) suggests that the death of the apprenticeship norm may have been exaggerated.

Rasmussen (1988) has investigated the behavioral norms of the House of Commons, especially those surrounding the maiden speech of members. He found widespread violation of the norms and, most importantly, few sanctions applied to violators by party leaders. In a comparative study of Belgium, Italy, and Switzerland, Loewenberg and Mans (1988) find that norms regarding courtesy and party have equivalent meanings in all three countries, but the extent to which each is subscribed varies across and within countries. Courtesy appears to be a reflection of the culture of the nation, while norms of party allegiance tend to vary by party within nations.

The problem with a focus on norms is that the concept is largely descriptive rather than explanatory. Norms are indicated by behavioral conformity, but what explains that conformity? Barbara Sinclair (1991) suggests that norms rise and fall as the balance between the costs and benefits of compliance change. A similar marginal utility analysis is offered by Krebbiel (1986) to explain why senators conform with the norm of not objecting to unanimous consent agreements.

Despite its theoretical weaknesses, the focus on unwritten norms of behavior was a change from the more traditional and descriptive research approaches centered on written rules of procedure that at one time had been typical of scholarship on legislatures the world over. More recently, American political scientists have displayed a renewed interest in the formal rules of the Congress. This has been sparked in part by the observed tendency in the House to move more decisions to the floor of the chamber and, in the Senate, toward greater individualism and therefore a decline in committee authority. In these contexts, rules are seen as instruments for managing and stabilizing a more uncertain decision-making environment (Bach and Smith 1988; Smith 1989; Bach 1990). Rational choice theorists have been interested in the impact of rules on the choices that legislators make. Weingast (1989), for example, shows that open rules, properly modeled, can protect the majority preferences of committee members.

Legislative Performance

Functionalism

At one time, functionalism was the major theoretical tool for treating the legislature as a collective institution and for assessing its impact on the larger political system. This approach suggested that legislatures and their members performed various representational and system maintenance functions in addition to the law-making function on which students of Congress tended to concentrate (Kornberg and Musolf 1970; Mezey 1979; Loewenberg and Patterson 1979). Functionalism provided common dimensions for the comparative treatment of legislatures and facilitated the discussion of the things that legislators did that were not connected with lawmaking. Although rich in description, functionalism did not lead to theory; it yielded nothing in the way of testable propositions that would help to explain the differences and similarities in legislative performance that were observed cross-nationally.

Legislatures and Change

Legislatures can be viewed as agents for change in a political or social system or as defenders of the status quo. In Mayhew's discussion of the Congress as well as more conventional analyses focusing on separation of powers, it is argued that the local orientations of members, conflicting electoral cycles, the decentralized committee system, and weak party leadership produce slow, inefficient, and ineffective public policy making favoring either the status quo or at most incremental change (see Sundquist 1981; Mezey 1986; Brady 1988; Fiorina 1989). Others, subscribing to the view of members motivated by multiple goals, give more attention to the legislatures' quest for good public policy (Maass 1983; Krehbiel 1991).

In the case of developing nations, Samuel Huntington (1968) suggested that their legislatures were largely conservative bodies that disproportionately represented the privileged and therefore were resistant to demands for political and social change. Others, however, have viewed individual legislators in Third World countries as important agents of change particularly in their role of linking citizens with central decision-making authorities (see Smith and Musolf 1979). John Sloan (1989) compares more democratic Latin American countries that have strong legislatures with more authoritarian countries with weaker legislatures and concludes that the former are more flexible and can achieve many development goals more effectively than the latter (see also Sloan and Tedin 1987). In Eastern Europe, legislatures seem to have been key arenas for

change as the authoritarian political systems that characterized those nations broke down (Hahn 1990). In China, the legislature has not played such a role, but there is some evidence that its prominence has increased (see O'Brien 1988, 1989, 1990).

Universalism

Mayhew (1974a, 88) suggested that in its policy-making activities, Congress followed the principle of "universalism" which meant a preference for distributive policies characterized by specific benefits and generalized costs. This tendency is reflected in the congressional taste for entitlements and formula grants (Arnold 1981) and for logrolling arrangements that achieve near consensual support. Such policies, however, are inefficient because their aggregate costs exceed their aggregate benefits; legislators nonetheless prefer such programs because of uncertainty about how their constituents would fare with more efficient policies (Collie 1988b). Such universalism is encouraged when decisions are made on the floor rather than in committees (Owens and Wade 1984) and by considerations of fairness to the various participants (Collie 1989). Even when Congress delegates authority to the bureaucracy, they do so in a manner that ensures that final policy decisions adhere to the preferences of the median legislator (McCubbins 1985).

There have been at least two types of response to the universalism argument. Some formal theorists argue that policy outcomes are governed by a competitive bargaining process that produces a minimum winning coalition rather than a universalistic coalition. Such collective choice models suggest that the most efficient projects will be selected (Ferejohn et al. 1987). However, empirical support for either universalism or minimum winning coalition arguments is difficult to find. As Collie (1988b) points out, roll call studies usually eliminate consensual votes, thereby emphasizing conflict rather than universalism. Also, such studies are not restricted to distributive policies, and few votes are actually decided by consensus. On the other hand, the minimum winning coalition perspective is equally difficult to defend because coalitions larger than those necessary to carry the day regularly appear. These oversized coalitions, which seem most typical of congressional action, can be explained, Baron and Ferejohn (1989) argue, by combining spatial modeling with game theory (see Krehbiel 1988).

A second response to the universalism argument is to point out that Congress on occasion *is* the source of major policy changes of a non-incremental and often non-universal sort. David Brady (1988, 12-16) argues that under normal conditions the Congress is disposed toward either the status quo or toward incremental policy decisions. This is so because congressional parties are

"weak and divided" and the preferences of their members are "heterogeneous and conflicting." These factors prevent parties from assembling coalitions that can overcome the structural features that Mayhew highlighted. Typically this means an absence of major policy changes in the Congress. Atypically, according to Brady, critical elections nationalize political issues, unite the different elements of the party, and thereby "increase the party's ability to legislate" and "decrease the committee system's ability to thwart policy initiatives." Even though Brady's overall perspective on Congress is consistent with Mayhew's, in a recent book Mayhew (1991) challenges Brady's argument. Divided government, he says, does not necessarily retard major policy initiatives, and lawmaking surges are not necessarily connected with electoral realignments.

Bureaucracy

The research of the 1960s and 1970s explained the lack of legislative attentiveness to oversight activities by noting that the electoral incentives to engage in such activities usually were slight. Critics argued that the Congress in effect abdicated decision-making authority to the bureaucracy; only when there was a major incident would Congress react. Essentially, Congress engaged in "fire alarm" oversight — when they had to — or when there was political advantage to be gained by doing so (Scher 1963; Ogul 1976; Lowi 1979; McCubbins and Schwartz 1984). And, as Fiorina suggested, electoral advantage could be gained by a collaborative rather than an adversarial relationship with the bureaucracy. Scholars identified subgovernments composed of legislators on subcommittees, the bureaucrats whom they were supposed to be overseeing, and affected interest groups all working together to produce mutually beneficial policy, with the legislator's payoff being calculated primarily in terms of funds and favors for constituents (Dodd and Schott 1979; Hamm 1985).

More recent research by Joel Aberbach (1990) raises some questions about this electoral interpretation of oversight activity. He detects an increase in congressional oversight activities that he attributes to a number of factors: congressional responsiveness to citizen frustration with the increased size and complexity of government; the limited number of things that legislators can do through conventional legislative means to attract and satisfy constituents, given the restrictions on government action imposed by large budget deficits and the scarcity of economic resources; the increased staff and committee resources that legislators have available to devote to oversight; and the increased rivalry between a conservative president and a more liberal Congress.

Others argue that effective control of bureaucratic "agents" by their congressional "principals" may not manifest itself in visible oversight activities. Rather, legislators make their expectations clear when they delegate power, and the regular reports that they require from bureaucrats ensure that these expectations are met. Thus, what appears to be abdication and little visible oversight may in fact be the executive knowing, anticipating, and responding to the wishes of the legislature (see Weingast and Moran 1983; Weingast 1984; Kiewiet and McCubbins 1991).

There has not been a great deal of comparative research on legislative oversight. Aberbach et al. (1981), using interviews with legislators and bureaucrats in several western democracies, find that in Britain, the Netherlands, and Sweden contacts between bureaucrats and legislators are mediated through senior ministers, whereas in Italy, West Germany, and the United States direct contact is more typical. In the latter cases, executive fragmentation and strong committees produce legislatures with more prominent policy-making roles and therefore direct linkages with legislators are important to bureaucrats. Recent comparative research on legislative oversight in regard to monetary policy in the United States, Great Britain, France, and Germany supports the view that legislators are motivated to engage in oversight primarily by constituency concerns, but considerations of the institutional balance of power between the legislature and the executive are also important (Woolley and LeLoup 1989; LeLoup and Woolley 1991).⁸

The Status of the Legislature

Discussions of the power and prominence of the legislature compared with that of political executives such as presidents and prime ministers fall into two broad categories. One tradition examines the institutional factors that explain legislative strength such as constitutional provisions and their history, the power of competing institutions such as parties and executives, and the nature of internal legislative resources such as committees and staff (see Blondel 1973). A second tradition focuses on political factors, particularly the nature, sources, and level of public support for the legislature and its members (see Mezey 1979).

Institutional Factors

Discussions of legislative power always have had a strong constitutional and historical flavor to them. But even though the power of a legislature rests to some extent on the basic law of the nation and the political traditions that surround that law, it is also the case that

constitutional provisions do not explain everything. Further, in less stable political systems, constitutions are subject to change, and in many nations there is a substantial gap between what appears on constitutional paper and what actually takes place.

The policy-making power of a legislature is indicated by its capacity to constrain the actions of the executive. For example, the Congress usually rejects or significantly amends more than half of what the president proposes; the House of Commons, in contrast, regularly accepts most of what the Cabinet proposes. But as we have seen, constraint also can be imposed prior to final votes by private discussions among party leaders and followers, by threats of revolt on the floor, and by the prospect of politically damaging debate.

Anthony King (1976) cautions against simplistic notions of executive domination in parliamentary systems, arguing that the legislative-executive relationship takes multiple forms. These include the government's relationship with the opposition leadership, with its own backbenchers, and with backbenchers of both parties. Each relationship affords a different set of opportunities for the exercise of legislative power. One such opportunity is provided by situations of minority government, a phenomenon that Kaare Strom (1990) shows is more frequent than conventional wisdom would suggest. His view is that minority governments strengthen parliamentary control and that opposition parties also allow such governments to form because they anticipate reaping future electoral benefits.

Two additional factors with implications for the legislature's policy-making prominence are committee systems and professional staff. Both speak to independent sources of information as a prerequisite for legislative influence. The congressional committee system originated in part from a desire to restrain the growing influence of the president on public policy (Cooper 1970), and the Congress's continued strength as a public policy-making body is connected by most observers with the strength of its committee system. Comparatively, the greater impact of the German and Italian parliaments on public policy decisions compared with the House of Commons and the French Chamber of Deputies can be attributed in part to the presence in the former two bodies of strong committee systems. Proposals for strengthening the House of Commons and other similarly situated legislatures almost invariably begin with proposals to strengthen the committee system (Crick 1968; Norton 1990; Arkins 1990).

Although legislators employ staff members to help them deal with their constituents, other staff, particularly those assigned to committees, assist them with their legislative responsibilities. Staff members provide legislators with expert information and advice on

complex public policy issues, help them to draft legislation, and are even the source of some public policy initiatives. This policy-making role that congressional staff play has been criticized; in some cases, staff are accused of acting independently of their superiors, and in other instances of prompting legislators to consider and pass legislation that they might otherwise have ignored (Malbin 1980). More recent research by Christine DeGregorio (1988) suggests that this critique is overdrawn and that the most aggressive subcommittee staff people act at the behest, and under the control of, their legislative masters. Although the role of staff may be debated, it still seems clear from comparative studies that the absence of professional staff is an impediment to the exercise of legislative power (see Mezey 1979; Blischke 1981; Arter 1984; Hammond 1985; Lock 1988).

Strong committees and large staff resources also may attract skilled and experienced politicians to the legislature. Nelson Polsby (1968) demonstrated that as the strength of the House committee structure grew, turnover in membership declined. In recent years, the percentage of members who have held no previous public office prior to their election has remained at about 25% for the House and about 10% for the Senate (Canon 1989b). Similarly, Shepsle (1988) has argued that as the power of the congressional committee system erodes people of talent will have fewer incentives to stay in the Congress (Cooper and West 1981; Hibbing and Moore 1991). Ultimately, this might result in a decline in the policy-making prominence of the Congress. Legislatures that are weak and vulnerable to attacks on their constitutional prerogatives are characterized by a large number of members without previous political experience and relatively high membership turnover rates (Mezey 1979, 249-51; Kim et al. 1984).

Legislatures and Support

Public support for the legislature is another component of legislative strength. In the United States, relatively high levels of support for individual members and relatively low levels of support for the Congress as an institution have been observed (Parker and Davidson 1979). Although members of Congress have been re-elected at overwhelming rates, evaluations of the job performance of the Congress as a whole have declined (see Davidson and Oleszek 1990, 422). Similar findings of higher support for MPs and lower support for the Parliament have been observed in Great Britain (Norton 1990).

An explanation for these findings may be found in the constituency-oriented activities of legislators. As Mayhew argues, legislators create and support a congressional structure that serves their electoral needs;

their high re-election rates testify to the success of this strategy. However, this structure may well produce poor public policy (Mezey 1986). This poor performance, combined with the isolated scandals about individual members that tend to attract a disproportionate amount of press attention, creates a public impression that the Congress is performing poorly. Members themselves contribute to this perception by deflecting public criticism of themselves toward the Congress, in effect running against Congress (Fenno 1978). However, recent research by Richard Born (1990), using National Election Study data over a number of years, suggests that evaluations of the Congress are strong predictors of how constituents feel about their own member and therefore attempts by members to distance themselves from the institution may be futile.

This approach relies on what Easton (1965) referred to as specific support — support based on performance. Wahlke (1971) argues that because citizens tend to know very little about what their political leaders are doing, measures of specific support are not as important as measures of support that are unrelated to performance. Such diffuse support is measured by support for the continued existence and powers of the legislature rather than by favorable evaluations of what the legislature is doing. Over the long term, specific and diffuse support are connected. Boynton and Loewenberg (1974) found, for example, that diffuse support for the German Bundestag increased as a result of the economic successes of the post-war government. And a more recent analysis suggests that perceptions of poorer government performance have led to a decline in trust for the Bundestag (Saalfeld 1990).

In Korea, Turkey, and Kenya, it is found that among more modernized sectors of these societies constituent satisfaction with the performance of the legislature is connected with diffuse support for the legislative institution, and satisfaction with the performance of individual legislators has little impact on diffuse support (Kim et al. 1984). Similarly, in Sweden a relatively high level of support for the parliament as an institution along with an increasing cynicism about its members has been noted (Arter 1990).

The Development of Legislatures

A legislature's status in its political system may vary. That is, the prominence of a legislature and the degree to which it is supported by the public may change over time. Drawing on the political development literature in general and on Huntington (1965) specifically, Polsby (1968) used the concept of institutionalization to describe the process by which the House of Representatives became more complex,

autonomous, coherent, adaptive, and universal. In so doing, he argued, the House increased its power and prominence in the political system and became a stable body in the sense that its status relative to other political institutions did not change substantially with changes in membership or in its political environment.

The theme of institutionalization has since been pursued by Gerlich (1973) in a discussion of several European parliaments, by Opello (1986) in a case study of Portugal's Parliament, and by Hibbing (1988) in an analysis of the British House of Commons. Hibbing replicates Polsby's categories and finds only spotty evidence of the institutionalization of that body. This finding leads him to question the assumption that the institutionalization process is unidirectional and monotonic, and he concludes that much depends on which indicators one selects and which time period one is examining. He also agrees with Cooper and Brady's (1981a) view that institutionalization is more descriptive than explanatory in the sense that it leaves open the question of what external factors explain the particular facets of institutionalization.

Explaining change is of course more difficult than describing change. Fred Riggs (1975) emphasizes the connection between the conditions under which a nation's legislature first emerged and its current status in the political system, while Sisson and Snowiss (1979) discuss the importance of a supportive ideology involving constitutionalism and individual rights in the creation and maintenance of strong legislative institutions. Organization theory, with its focus on the relationship between organizations and their environments, offers perhaps the strongest potential theoretical approach to understanding legislative change (see Cooper 1977; Hedlund 1985), although as Hibbing (1988, 710) notes, "no one has undertaken the work of empirically relating measures of environmental change to indicators of legislative change."

Conclusions: Behavior, Institutions, and Theory

Purposive Politicians

The reigning perspective for those who study the United States Congress centers on the constituency and electoral concerns of individual members and the impact of those concerns on their behavior and on the structure, performance, and status of the institution. Although the importance of constituency factors cannot be denied, there does remain the strong sense that electoral concerns explain only part of what legislators do. Fenno's view that legislators are driven by policy and power goals as

well as by re-election concerns, and that the relative importance of these different goals will vary over the course of a legislative career, continues to resonate as a fuller description of what legislators are about. In Fenno's short books on individual United States senators, the genuine policy commitments of those whom he is studying emerge as a consistent theme (see Fenno 1989b). Keith Krehbiel, in his critique of the view that legislators are driven exclusively by constituency concerns, argues that although a random list of bills passed during any congressional session might reveal some constituency dimension to many of them, "the claim that most or even many of them are predominantly distributive stretches the imagination" (Krehbiel 1991, 252-3).

Even when one discusses activities that are constituency related, it is not at all clear that these are motivated entirely by electoral concerns. As previously noted, Searing found that for many British MPs, constituency service activities yielded little in the way of electoral rewards; instead, members reported that they derived personal satisfaction from helping people. Interviews with members of Third World legislatures, institutions with notoriously high turnover rates and great vulnerability to military coups, also suggest that the electoral dividends of constituency service may be less certain than the personal status that legislators gain in their communities by doing favors for constituents.

Purposive models therefore need to give equal time to non-electoral factors and probably need to be expanded to include non-political factors as well. It would be useful in this connection to return to Schlesinger's notion of ambition theory and to remember that not all legislators have static ambitions -- the desire to stay where they are. Many may be hoping to move on, and others may be more willing than we think to risk loss of office in order to achieve policy goals. It is convenient but inaccurate to think of legislators solely as professional politicians driven to stay in office because the alternative is unemployment. Rather, legislators who leave office often do quite well; those who stay in office may have to be content with doing good.

What still has yet to be demonstrated in a convincing manner is the impact of service, distributional policies, credit claiming, and home style on the re-election of members of Congress. Surely these factors count for something, but we still do not know, and may never know, how much they count, and how much they count in comparison with party, issues, and national events. What we do know is that challenger quality does count in congressional elections. This knowledge should cause us to direct less attention at incumbents and what they do and more at political parties, their methods for recruiting candidates, the personal motivations that drive people toward seeking a political career, and issues of

campaign finance. Certainly, the political strength of incumbents and the campaign funds at their disposal will be important in these decision-making processes. But as David Canon (1990) has shown, challengers with a great deal to recommend them do turn up, and often for apparently hopeless races.

The role of political parties also needs to be reconsidered. The Mayhew perspective assumed that the weakness of political parties in the electoral arena meant that legislators interested in re-election would have to take care of themselves. But apparently the role of national political parties in such matters is increasing. Will that altered role affect the behavior of members of Congress in their constituencies? Certainly, in some countries, as parties have weakened, the service activities of legislators have increased. Is the inverse going to be true? Elaine Swift's (1987) discussion of the nineteenth century Congress suggests that the answer may well be no. She demonstrates that careerist behavior, calculated in terms of advertising and credit claiming, seemed to be unaffected by changes in the strength of political parties that occurred during that century. And as has been noted, in several countries with stronger party systems than the American case, legislators continue to engage in service activities, albeit less for their own personal electoral advantage than for the advantage of their parties.

All of this calls into question Mayhew's hypothesis that questions of congressional structure and procedure are dominated by the electoral motives of members of Congress. Just as constituency activities and strong parties can co-exist, Cooper and Young (1989) demonstrate that changes in procedures for bill introduction cannot be explained entirely in terms of the electoral self-interest of members. Rather, context mediates between procedure and self-interest and, more importantly, contextual factors exist apart from and beyond the control of members and their self-interest.

Institutional Performance

The purposive model, with its focus on individual behavior, has diverted the attention of Congress scholars from the broader institutional questions that are more frequently addressed by comparativists. More research needs to be done on the question of what difference the legislature makes for the political system and for the policies that it pursues. Are all legislatures inclined toward universal and therefore more inefficient policies? If the congressional policy-making process is slow, incremental, and dominated by monied interest groups, does that suggest that the hypothesis that legislatures are inherently conservative bodies disposed to support the economic and social status quo apply as much

to the Congress as to Third World legislatures? If so, would that reaffirm the point of view of those who have always looked to the executive as the catalyst for major political change? Does a legislative policy-making process that fosters universal, often inefficient policies, along with a politics of blame avoidance, affect the level of public support for the legislature as a whole? Do stronger legislative parties and more centralized legislative power lead to more efficient and effective policy performance by the legislature? And if parties decline, as they seem to be doing in some European nations, will that result in poorer legislative performance?

Comparing Legislatures Across Space and Time

To answer these questions we must look at more than one legislature, just as we must look at more than one legislator if we are to understand legislative behavior. And to look at how legislatures affect, and are affected by, political and social change, we must look at longer time periods. But the fact is that most of the legislative literature is firmly rooted in time and place, and for much of the subfield the place is Washington, D.C., and the time frames are usually current. The research decision to deal only with the Congress is often dictated by practical considerations: comparative research is more difficult and expensive to do. But ethnocentric factors also come into play. Most legislative scholars in the United States have congressionally centered research agendas because they view themselves as Americanists and they view the Congress as a uniquely important institution.

Comparative efforts are further impeded by the institutionally centered and often less quantitative approach that characterizes much of the research conducted on legislatures outside the United States. This is more than simply a matter of research training or taste. The implicit comparative argument is that legislative behavior is shaped by the status of the legislature and its role relative to other institutions such as the executive and political parties. The congressional argument is that institutional and structural factors follow from the goals and purposes of self-interested politicians. The comparative argument insists on the importance of context; the congressional argument persistently pushes contextual questions to the periphery.

All of this presents significant impediments to comparative research. And it should be obvious that only when we move toward comparative research will we be able to move toward theories of legislative behavior. As Samuel Patterson (1989, 461) reminds us, the "overarching theoretical and empirical requirements of political science are invariant as to country." Even if

comparative data are unavailable, "patterns of behavior and generalizations found to hold in one system ought to inform, or be tested in, another system." And even if one's research is confined to a single country, generalizations can be strengthened by testing them across time (Cooper and Brady 1981b).

A comparative and diachronic -- and therefore a more theoretically promising -- approach to legislative studies has not been characteristic of the research that has dominated the field. The "normal science" studies that constitute much of the research cited in this chapter reduce the complexity of the legislature by identifying falsifiable hypotheses, looking for data with which to test these hypotheses, and through these findings refining or advancing to some small degree the state of knowledge in a narrow subfield. This work is what doctoral students are encouraged to do; it is the work that most continue to do after their doctorates; and it characterizes the bulk of the literature published in our leading scholarly journals. Although this work is clearly valuable, its value is just as clearly limited. With each article on each topic, the questions seem to get narrower, the issues more arcane, and the findings less relevant to all but the small group of scholars pursuing that specific line of research. Ironically, congressional scholars seem to have replicated one of the major failings of the Congress itself: extreme specialization at the cost of a view of the "big picture." To use a well-worn metaphor, such work produces lots of trees, but few perspectives on what the forest as a whole looks like (Rieselbach 1983). And such a larger view is what theory is all about.

More recent attempts at theorizing have come from those who have sought to develop formal models of aspects of the legislative process. In contrast to the normal scientist, the formal modelers reduce complexity through a series of rules and assumptions, much in the manner of economists who model national economic systems. Such work compels both those who do it and those who read it to think rigorously about legislative decision-making processes. But the impact of this work has been restricted by technical and notational paraphernalia that are unfamiliar to most political scientists, by what some have seen as its tendency to minimize political factors (see Rieselbach 1992), by the difficulty that many of its practitioners have encountered in testing these stylized laboratory models against the more disorderly real political world, and by its refusal to move beyond the Congress.

All of this is unfortunate, because formal theories of legislative behavior hold out the hope of being truly generalizable. The idea that structure influences collective decision making is intrinsically comparative because all legislatures are collective decision-making bodies. For example, modeling decision making within a party caucus where members are bound to support on the

floor decisions taken by the caucus majority should not be more difficult than modeling decision making within a congressional committee. Similarly, the implications of decision-making sequences in parliamentary systems that place committee decisions after floor discussion should be at least as interesting as the power congressional committees may gain from their placement at the conference stage of deliberation. The point is that formal modelers have unnecessarily restricted their efforts to the Congress. The data-gathering reasons that influence the "America first" approach of normal science practitioners do not apply to those who design deductive models. And the cost of this decision is great because it is through such models that theories of legislative (as opposed to congressional) behavior may come.

In summary, normal science, in its quest for narrow questions with narrow answers typically avoids comparative and/or diachronic analysis; and formal models, at least at this nascent stage in their development, have dealt only with the Congress. This means that legislative research remains theoretically impoverished. Its lack of comparative focus is part of the problem; one can no more have a theory of legislatures that applies only to the United States Congress, than a theory of relativity that applies only to Chicago. Even a theory of Congress has proven elusive; few congressional scholars have had the inclination to put the various narrow studies together, and those who have attempted to do so have operated more at the level of description and speculation than explanation. This work has produced various "perspectives" or ways of looking at legislative behavior, but little in the way of theory.

Work on these perspectives, one hastens to add, still needs to be encouraged because as they synthesize and interpret the narrower work associated with normal science they can start us toward theory. Such efforts are at once creative, risky, and important. They involve imaginative leaps, and bold, often controversial pronouncements. They invite criticism from those committed to normal science, but if successful, such work can change the way all of us, including normal scientists, think about the field. Mayhew's *The Electoral Connection* is such a work. Drawing primarily on the empirical work of others, he attempted to step back from the "trees" and look at more general patterns. Although frequently criticized for its descriptive and non-empirical approach, there should be little debate about the extent to which it has shaped the thinking and the research agendas of a generation of legislative scholars.

Notes

1. What follows is a far from complete review of a large body of literature. Given space limitations and my editor's mandate to

incorporate research on legislatures outside the United States, I have had to make some difficult choices. These include the exclusion of virtually all of the research on state and other sub-national legislatures, as well as the slighting of several important areas of congressional research. Even work that I have included is sometimes summarized too briefly. Thus, apologies in advance to those who have been left out, to those who think that their work deserves more space than I have provided, and to those who think that they would have liked to know more. For the latter group, the most complete treatment of the literature is still found in *The Handbook of Legislative Research* (1985). I also can recommend other literature review articles that I have found helpful, including Collie (1988b), Krebbiel (1988), Ogul and Rockman (1990), Patterson (1989), and Rieselbach (1992). I am grateful to Aage Clausen, Ada Finifter, Malcolm Jewell, David Mayhew, and Marvin Overby for their comments on earlier drafts of this manuscript.

2. The essence of this research on the Congress can be gathered from the essays in Peabody and Polaby (1963), from Ralph Huit's essays in Huit and Peabody (1969), and from Matthews (1960). On European legislatures, see Jennings (1957) on the British Parliament, Loewenberg (1967) on the German Bundestag, and Williams (1968) on the French Parliament.

3. Much of this research is collected in anthologies. See Kornberg and Musolf (1970); Boynton and Kim (1975); Eldridge (1977); Smith and Musolf (1979). See also Kim, et al. (1984)

4. For a critique of Jacobson's view, see Hibbing and Bauer, 1989; also see Jacobson (1990a, 40) for a rejoinder.

5. There is a great deal of literature in this area. For an introduction, see Shepsle 1979; Krebbiel 1988; G. Strom 1990.

6. For a critical discussion of Converse and Pierce, see Eulau (1987) and Wahlke (1987).

7. See Cox and McCubbins (1991) for a recent critique of this view.

8. For excellent reviews of the literature on legislative-bureaucratic relations, see Rockman (1985) and Ogul and Rockman (1990).

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Public Law and Judicial Politics

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The Status of the Field

Public law, judicial politics, or law and politics today enjoys a highly problematic status as a subfield of political science. Even its name is in doubt. Once one of the five or six major subdisciplines, it has, to some degree, avoided the fragmentation of some of the others. It remains one of the larger subdisciplines in numbers of practitioners and publications. Yet it has become one of the more minor fields in terms of status within the discipline. Few, if any, public law political scientists are numbered among the stars of the discipline as Corwin and Cushman once were. Many departments have reduced judicial politics to a sub-subfield of American politics. During much of the 1980s, a striking proportion of the top departments, including Harvard, Yale, Chicago, Michigan, and Stanford had no senior person devoted to principally to teaching in the field. Through promotion, some senior lateral hiring, and the joint appointment of law faculty to political science departments, the situation is now somewhat improved. Nevertheless, several generations of Ph.D.'s are depleted, and in only a handful of major departments is there more than one senior person of national reputation devoting full time to political-science-based (as opposed to law-based) instruction in law and politics.

Vigorous efforts are being made to fill the gap by the Law and Courts Organized Section of APSA. A committee of the section, chaired by Austin Sarat, prepared a major report on the state of the discipline, and, in conjunction with the section, the *Review of Politics* has announced it will devote one issue a year for three years to law and politics studies (see the "Special Issue on Public Law," Summer 1992).

Public Law as Constitutional Law: Supreme Court Alone

Intellectual change typically is overdetermined and full of chicken and egg problems. A number of

paradoxes associated with the decline of the subfield can be noted even though describing them falls short of a causal explanation. The central paradox involves constitutional law and the Supreme Court. Until the 1950s, "public law" was usually thought of as containing three distinct entities that together formed the field while each also performed a vital service to another major field. The three were constitutional law, administrative law, and international law, linked respectively to American politics, public administration, and international relations. Moreover, another small but distinct field, "government regulation of business," called heavily on the services of public law political scientists.

Then, in a bipolar, cold war world, particularly as interpreted by the dominant Morgenthau school of international relations scholars, international law came to seem a meaningless facade. The number of international law teachers and scholars in political science declined to near zero. Even those international relations specialists seeking non-Clauswitzian facets somewhere in the family of nations, now concerned themselves, not with international law, but international organization.

Under the impact of the "behavioral revolution" the policy studies areas of political science such as government regulation fell away. Public administration became administrative behavior and organization, with administrative law, like all law, treated as one of those formal screens which investigators had to penetrate in order to discover real political behavior.

In American politics, too, behaviorism tended to denigrate formal legal and constitutional structures and language as mere appearances behind which real and quantifiable political behavior lurked. Yet the power of the Supreme Court to declare laws unconstitutional was too big and too dramatic to permit as great a decline in constitutional law as occurred in administrative and international law. Constitutional law remained as the eroded but still visible remnant of what had been "public law." Even in the remnant, however, much of "constitutional law" became "judicial behavior," and most of judicial behavior became the analysis of the votes of the justices of the Supreme Court in particular cases. In

the eyes of most political scientists public law was reduced to the study of the Supreme Court's Constitutional Law Opinions and Votes

Reduced in this way, public law encounters the paradox that it studies only one thing, and the thing is so big as to be unimportant to most political scientists. Supreme Court decisions declaring laws unconstitutional were few and far between even in the Warren court years, and they seem to come as bolts from the blue. American politics and public administration specialists immersed in the everyday, routine politics of Congress, the presidency, and the federal bureaucracy naturally treated constitutional judicial review as a kind of random act of God -- sporadic incidents to which the legislative-executive system they studied had to adjust occasionally. Reduced to constitutional law, law and courts were read out of the normal nitty gritty politics of pluralist bargaining and compromise that American politics people studied. And constitutional law and/or judicial behavior people were seen by American politics scholars as being, to mix metaphors, a set of Kremlinologists who sat in a corner trying to divine the intentions of the Olympian justices. Moreover, they did their divining either by an arcane lawyer-babble of case names or an equally arcane Guttman-scaling of judicial attitudes. So American politics people stopped talking to constitutional law and judicial behavior people, because they spoke only in a weird non-political-science language and because they studied things that had almost nothing to do with the everyday politics that American politics people studied.

This constitutional law-Supreme Court orientation also has curious cause and effect relations with certain internal tendencies in both the American and comparative politics fields. Nearly all congressional and American bureaucratic studies have always consisted of input and through put analysis. While economists routinely assess the performance of complex organizations by studying their outputs, political scientists almost never seek to understand the performance of Congress by reading its statutes or of executive branch organizations by reading the regulations and rules they produce. Comparativists, even those principally concerned with law-making processes and institutions, are almost invariably totally ignorant of the actual laws of the countries they study and of the courts of those countries. No doubt a major reason for this law avoidance is that law is complex and technical. Yet if Americanists and comparativists turned to their public law colleagues for technical help, there was no help to be had. Public law people only did constitutional law. They knew nothing about statutes or administrative regulations. And they knew nothing about foreign law or courts either because until very recently most foreign countries had little or no constitutional judicial review. Thus, politics people

defined law out of what they did and could not even ask law and politics people to do what politics people should have been doing themselves.

Public law and/or judicial behavior thus dwindled into a marginalized constitutional law-Supreme Court ghetto of little interest to other political scientists. And public law, judicial behavior graduate students were taught that what they ought to do was to stay in that ghetto and concentrate on learning its esoteric languages.

Two Breaks in the Constitutional Law Barrier: Urban Courts and Private Law

One early break in the ghetto wall occurred during the urban politics episode of American political science. Empirically oriented urbanists looking at what was really going on in cities kept running into local courts and judges. Because most of these political scientists were not public law trained, they did not know that they were supposed to ignore these courts and judges because they did not do constitutional law. Thus, a major opening occurred from constitutional law-Supreme Court-only studies.

A second, but so far much narrower, break occurred as some political scientists reconsidered the "public" in public law. The distinction between public and private law is drawn from nineteenth century European civil law. That distinction makes very good sense in a laissez faire era in which the state is a neutral policeman of private relationships themselves established by private contracts. Private law is then the essentially unchanging rules of the game of private enterprise enforced by an apolitical court in the absence of any government interest. Public law governs the internal processes of government bodies and their relations to one another and to the citizens. It embodies the public interest and is an instrument of public policies. Public law and government, as the distinguished name of the Columbia department tells us, go together. The political scientist has no interest in private law.

Once political science defines itself as dealing with the authoritative allocation of values, and the modern state is seen not as a neutral policeman but a positive achiever of welfare, the public law/private law distinction becomes less tenable. The "private" law of property and contract authoritatively allocate most of the values in a capitalist society. As the Supreme Court constitutionalized criminal procedure, somehow it became appropriate even for public law political scientists to study criminal law and courts, although criminal law sits uneasily on the boundary between public and private. And as urbanists studied the trial courts that loomed so large in city politics, eventually they had to discover that such courts had civil as well as criminal divisions. So

there has been a slight movement from public to private, as well as from Supreme Court to trial courts, most notably in Herbert Jacob's work on divorce (Jacob 1988) and Baum and Cannon's (1981) on tort. See also Scheppele (1988). Meanwhile, studies of the criminal justice process, essentially in local courts, continue to appear regularly (Heinz, Jacob, and Lineberry 1983; Nardulli, Eisenstein, and Flemming 1988; Casper and Diamond n.d.). The politics of criminal justice policy also attracted researchers (Scheingold 1991; Dalton 1985).

The political science study of one crucial area of private law, i.e., property, has been much distorted by constitutional law blinders. When the New Deal Supreme Court read property out of the Constitution, political scientists who defined their trade as constitutional law stopped studying property. Although still somewhat distorted by constitutional law concerns, there is now a significant return to property studies (Brigham 1990b; Coyle 1993; Shapiro 1986; Paul and Dickson 1990; Nedelsky 1990).

Three Major Movements "Outward" from Constitutional Law-Supreme Court Studies

(1) International Law Returns

Trial courts and private law have always been there. Political scientists were just slow to recognize them. The three largest political science movements "outward" from the constitutional Supreme Court have occurred because of massive changes in the real world that even "public law" political scientists could not go on ignoring indefinitely. The most recent change is in the international realm, and so the change it is engendering in political science law studies is still embryonic. If international law dwindled in an age of cold war bipolarity, it should follow that it might revive as that international condition changed. Because good old constitutional law had become so preoccupied with rights, it was natural that a few political scientists would bleed over from constitutional rights to international human rights. Given the post-Gorbachev world, it is also natural that some international relations specialists would see the need to shift from an exclusive preoccupation with power to some attention to the treaty law that is actually beginning to again define international relationships. Even here, however, the public law legacy will prove a handicap. Some political scientists are studying multinationals and international trade policy. Both public and private international law (the law of international business

transactions) play major roles in these areas. But even as law-oriented political science moves to take up international law again (Renteln 1990; Forsythe 1990), its preoccupation with public law and with individual rights analogical to our first ten amendment rights will prevent it from promptly rendering the assistance it ought to be giving to political scientists studying international political economy.

(2) Comparative Law as Comparative Constitutional Law

A second major change in the real world has led to a rather easy and natural, but still strangely small, movement of public law outward into broader concerns. If public law reduced itself to the study of constitutional judicial review, it would follow that if constitutional judicial review traveled, so would public law. Courts with constitutional judicial-review powers were established in many nations after World War II. As the actual impact of some of these courts has increased, the political science study of comparative constitutional law and courts has begun to flourish a little (McWhinney 1986; Beer 1992, 1984; Ellerman, Gawron, and Rogowski 1987). It is a bit difficult for political science to ignore such things as the German constitutional court, which has had major impacts on policy and governmental structure; the Israeli constitutional court, which has, in effect, written an Israeli constitution; and the French Constitutional Council, which today reviews nearly every major piece of French legislation. Indeed, major political science works on German (Kommers 1989) and French (Stone 1992) constitutional law and courts have recently appeared. Yet most comparative politics scholars remain woefully uninformed about constitutional law and courts. The group of comparative constitutional law political scientists remains small and rather isolated from the main body of comparativists. However, just as American constitutional law specialists have spread outward and downward, there is some comparative scholarship by political scientists or those heavily influenced by political analysis of law into non-constitutional comparative law areas (Willborn 1989; Stetson 1982; Goldberg 1990). Attempts are also being made to undertake general comparative analysis of courts comparable to systematic comparative work on legislatures, executives and bureaucracies (Shapiro 1981; Damaska 1986; Waltman and Holland 1988).

Vigorous attempts are being made by the Research Committee on Comparative Judicial Studies of the International Political Science Association to end the isolation of law from comparative politics, particularly in a series of major publications (Schmidhauser 1987; Jackson and Tate 1993; Symposium on "The Comparative

Study of Judicial Review" in *Policy Studies Journal* 1990, 19, 74-171). *Comparative Political Studies* currently contemplates a special issue on comparative judicial review. *The International Political Science Review* will shortly publish one symposium on judicial elites and another on judicialization of politics. *West European Politics* will devote an issue to judicial policy making. Mary Volcansek has been particularly active with a long string of recent publications — only two of which are noted here (1992, 1990).

At this very moment, not only is constitutional judicial review flourishing in some countries, but the writing of new constitutions is a booming industry worldwide. Surely, political science ought to have a very marked comparative advantage in this enterprise given that it is supposed to know about both political development and constitutional law. In fact, because almost all political scientists who do constitutional law are Americanists and almost all of them who do political development or comparative institutions or systems know nothing about constitutional law, American law school constitutional lawyers, rather than political scientists, tend to be central to this new export trade. Nevertheless it hardly seems possible that American political scientists who say they are students of constitutional law can continue to ignore the international constitution epidemic. Nor does it seem possible that political scientists who are country specialists can continue to carefully shield their graduate students' eyes from constitutional law when the countries they are studying themselves see constitutions and constitutional courts as pressing political questions.

American and Comparative Electoral Law Studies

One major exception to the aversion of Americanists and comparativists to law is to be found in the area of electoral laws. *Baker v. Carr* has engendered an endless stream of litigation over districting, now vividly augmented by the Court's anti-gerrymandering decision in *Bandemer v. Davis*. One product of this judicial initiative has been a group of Americanists who do districting studies connected to litigation (Grofman 1985). Partially in connection with the new binge of constitution writing, some comparativists have now been working on electoral laws too (Lijphart 1990).

Comparative Law and the European Community

Exactly the same story may be told about the European Community. American political science has had a hard time recognizing the Community. In spite of the name they take, most comparative politics people are not comparativists but single-country specialists. Even our "Europeanists" are nearly all French, German,

British, or Italian politics specialists. The Community was not a country so it didn't get studied by "comparativists." Most comparativists no doubt thought the Community somehow belonged in international relations and left it to "them." But it doesn't really belong there, and "they" did little about it. The Community is a federalism. But the small federalism subfield of political science was basically a set of Americanists. Compound all this by the refusal of public law people to do anything other than the American Supreme Court and everybody else in political science to know anything about law at all. The result was that American political science knew nothing of the European Court of Justice, although it is arguably the most significant judicial instrument of political development in the twentieth century. After some pioneering work by Sebeingold, Haas, and others, American political science largely ignored the Community. It can hardly continue to do so now. Comparative politics is being dragged kicking and screaming into Community studies, notably by the Brookings volume (Sbragia 1992) which contains a chapter on the Court of Justice. A small number of American political scientists of public law and/or federalism persuasions — most notably for the public law community, Sam Krislov and Don Kommers — were incorporated into a major European comparative study of Community and American law (Cappelletti, Seccombe, and Weiler 1986-89). Other political science work on the Court of Justice is beginning to appear (Volcansek 1992). Yet the greatest expertise on the intersection of law and politics in the Community to be found in the United States is not in the work of political scientists but in that of law professors such as Joseph Weiler (1986).

Comparative Non-Constitutional Law

Moreover, the whole small realm of political science comparative law is still very excessively constitutional law and constitutional court centered. While in Community law the distinction is somewhat artificial, much of what is of political interest in that law is administrative rather than constitutional. Nearly all British law comparable to American constitutional law is called administrative law in Britain. Yet the amount of comparative administrative law research in political science is minuscule, even now that administrative law is reviving as a subfield of public law (Sterett 1990). As to political science comparative study of other key bodies of law, such as environmental regulation, David Vogel's (1987) work stands nearly alone (but see Kagan 1989), and it is probably no coincidence that he is a political scientist on a business school faculty.

(3) Administrative and Regulatory Law Returns

Vogel's work on comparative regulation is a kind of link between the second and third great change in the real world I am attempting to highlight. That third change lies in the realm of the American politics of regulation. The 1960s, 1970s, and 1980s presented us with a major outburst of new government regulation of health, safety, the environment, education, and civil rights. At the same time there was an enormous surge of judicial activism, not only in the Supreme Court's constitutional decisions but in the decisions of all the rest of the American courts and in all kinds of regulatory law cases.

Not entirely coincidentally, a major change was occurring in political science itself. Perhaps Congress specialists could go on forever blithely ignoring the huge product that Congress was spewing forth, but it was difficult for all of political science to do so. Policy replaced behavior as the voguish catchword of our trade. The most important branch of policy studies immediately became implementation. And students of implementation, mostly trained in American politics and/or public administration, soon came upon law and courts all over the implementation game. Any implementation action immediately set off a series of lawsuits. A major tool of implementation itself was the lawsuit or the threat of suit. Those political scientists who were concerned not only with the implementation but with the making of policy now discovered that the old policy-making iron triangle of interest group-congressional committee-executive agency had become a rectangle with courts (most often the D.C. Circuit) at the fourth corner. Most of the new regulatory policy statutes entailed large amounts of supplementary law making by "informal rule-making," that is, executive agency enacted regulations. Judicial review of that rule making had grown to incredibly intrusive proportions. That review was not constitutional. The question asked by the reviewing court was not: "Is the regulation constitutional?" but instead "Is the regulation in accord with its parent statute and with the Administrative Procedures Act?"

The bodies of regulatory and administrative law -- statutes, regulations, court decisions, and redeisions -- involved here were extremely technical, voluminous, and complex. But because the political scientists confronted by them thought of themselves as policy specialists and not "public law" people, they did not think they had to blind themselves to non-constitutional law. And because there was so much law and it was so central, they did not think they could do what American politics and public administration people had been doing for years, blind themselves to all law. As policy studies, or to use the

old name, government regulation, became a centerpiece of political science, public law necessarily again began to grow outward from constitutional law to at least really public law, to concern itself with all the law and all the courts that were involved in public policy (Bryner 1987; O'Brien 1987; Rebell and Block 1982; Williams and Matheny 1992).

A Realm Between

Even here, however, the growth has been small compared to the great continuing weight of constitutional scholarship. There is a strong and easily identifiable return of administrative law to its old place as a recognized subfield of public law and a significant component of public administration (Carter and Harrington 1991; Cooper 1983; Shapiro 1988; Rohr 1986). Particular attention ought now to be paid to a particular "realm between" constitutional law and normal regulatory law. Certain statutes, for instance, the Administrative Procedures Act, the anti-trust statutes, and the civil rights acts are so sweeping and, by their general wording, leave so much to judicial interpretation, that they are semi-constitutional in character and may invite a different style of judicial action than either constitutional provisions or routine statutes, a style that emphasizes "prudential" interpretation (Bryner and Thompson 1988; Shapiro 1986).

Law and Policy Studies

A number of widely read case studies of policy development and implementation treat all relevant law and all relevant courts as a routine part of long, complex policy stories involving legislatures, executives, parties, interest groups, etc., that ought to be handled routinely by whatever political scientist is telling the story rather than shunted off to some separate law specialist. These policy stories may be about environmental, or transportation, or civil rights, or occupational health and safety law rather than only constitutional law (Rabkin 1989). They have produced some of the most important "public law" books of the 1980s (Katzmann 1986, 1980; Melnick 1983; Mezey 1988; Mutch 1988). Indeed Melnick's book is clearly one of the most important books of the 1980s for the future development of "public law," because it is the first to routinely treat all levels of courts and all kinds of law as integral parts of the politics of policy making (see also Melnick n.d.; Mashaw and Harfst 1990).

From the Supreme Court to All Courts

Such stories almost always involve both trial and intermediate appellate courts as well as the Supreme Court, and often state as well as federal courts (Orren 1991). The federal courts of appeal, and district courts which loom very large in regulatory law, little of which reaches the Supreme Court, are the subject of a small number of political science studies (Barrow and Walker 1988; Cooper 1988; Howard 1982; Wasby 1987; Wenner 1992). Policy implementation studies lead naturally to the federal district courts in an age of "institutional remedies" when district court orders seek to implement anti-discrimination and other national policies at local levels. Urban studies gave us a body of work on state trial courts. If political science did state trial courts and the U.S. Supreme Court, it followed naturally that it ought to do something about state supreme courts (Glick 1992). In this area recent developments in the old standby, national constitutional law, have encouraged more study of state supreme courts (Ducat, Wyckoff, and Flango 1991; Fino 1987). As an increasingly Republican Supreme Court has become increasingly noncompliant to the constitutional rights agenda of the left, the rights industry has shifted some of its productive energies to state constitutional law and thus to state supreme courts (Friedelbaum 1988; Sheldon 1988). The attention of constitutional law-Supreme Court political scientists is then painlessly drawn from the U.S. Supreme Court to state supreme courts. Constitutional, criminal justice, urban politics, and policy studies also move attention onward to lower state courts (Porter and Tarr 1982; Gates and Johnson 1991; Culver and Stumpf 1992; Feeley 1983; Cole 1986). Political scientists have also contributed to the study of alternative dispute resolution (Harrington 1985).

Public Choice and the Return to Administrative and Regulatory Law

Along with policy studies, the great rising star of recent political science has been public or rational choice, which requires no description here given its pride of place elsewhere in this volume. Given the virulence of the virus it could hardly be thought that even constitutional law could escape infection. Yet constitutional law's almost complete isolation from the rest of political science has meant, as has been typical of past epidemics, that it has been among the last areas of political science to experience the new methodology and has been least affected by it. Rational choice is, however, having quite a dramatic impact on political science concern for administrative law. "Principal-agent" problems are of central concern to rational choice

analysts. These problems are especially acute in the process of implementation of federal statutes. The principal, that is Congress, itself has typically issued a complex, ambiguous, and somewhat self-contradictory directive. The agent is an independent and coequal executive branch. Congressional oversight of administration thus becomes a key principal-agent arena. Again, precisely because public choice political scientists have not been trained in public law, they do not know that they are forbidden to look at non-constitutional law. Free of these blinders, they have quickly seen that the administrative procedures enacted into law by Congress in the Administrative Procedures Act and in most program-creating statutes are one of the major avenues by which the principal seeks to control its agents. And once this point is seen, in a twinkling it can be seen that the courts, which engage not in constitutional but in administrative judicial review enforcing these procedures, are also big players in this agency game. Thus today we have the usual paradox that, while most public law scholars hang back, it is rational choice Congress, bureaucracy, government regulation scholars who are working at forced draft to acquire administrative law expertise and to move administrative law and administrative, as opposed to constitutional, judicial review toward the center of American politics studies. It is also typical of public law *and* American politics political scientists that this new work is far better known among academic lawyers than among political scientists (McCubbins 1985; McCubbins, Noll, and Weingast 1988, 1989, 1990; Calvert, McCubbins, and Weingast 1989).

Statutory Interpretation in the Movement Beyond Constitutional Law

Similar forces and roughly the same group of scholars are moving political science toward actually looking at the substantive laws Congress passes. In the search for all the various devices by which congressional principal controls executive agent, it must eventually occur to someone that Congress just might sometimes seek to control its agents by telling them what to do in the language of the statutes it enacts. Moreover, in the same decisions in which courts uphold or strike down agency actions on the basis of whether the agencies followed the procedural language of statutes, the same judge approves or disapproves agency actions on the basis of whether they followed the substantive language of the statutes. Thus the study of administrative law and administrative judicial review inevitably leads to the study of the substantive law that Congress enacts. Attracted by agency problems, Congress specialists may finally be pushed to the last resort, actually reading the statutes that Congress enacts.

Meanwhile rational choice concerns, policy studies, and the more general incursions of economics into political science, are among the several forces leading public law political scientists to some concern for statutory interpretation. Law and economics types have pushed a so-called "economic" theory of legislation which is really the old Bentley-Truman pluralist theory dressed in economic language. Because the vast outburst of proactive regulatory statutes of the 1960s, 1970s, and 1980s have been accompanied by a proliferation of deregulation-oriented Republican presidents, statutory interpretation has become a key political battleground. Depending on the theory and practice of statutory interpretation, particularly of the D.C. Circuit and the Supreme Court, either the Democratic regulatory vigor of the initial statutes could be preserved through hard Republican executive times or Republican presidential economic sense could temper the rhetorical excesses of Democratic green position takings. As real world politics brought statutory interpretation to center stage, academics seized upon the subject. Economic theories of legislation generate theories of statutory interpretation (McCubbins, Noll, and Weingast 1992). Older conventional theories of statutory interpretations are revived. Literary theory of textual interpretation is commandeered. And again the disciplinary dialectic is typical. Academic lawyers, who for years had abandoned statutory interpretation as unworthy of attention, returned to it quickly as it became politically relevant. Given that real political relevance was the spur, one might have thought that political scientists would have beaten them to it. But statutory interpretation is about statutes, not constitutions. And it is practiced more by "lower" courts than by the Supreme Court. So, naturally, public law political scientists were late to awaken to statutory interpretation. They are barely awakening now. And typically again, they tend to drift into statutory interpretation as a kind of obscure cousin of the real glamour subject: constitutional interpretation. Studies in constitutional interpretation are enjoying a great revival to be described in a later portion of this chapter. Nevertheless, some public law political scientists, and some academic lawyers with political science ties, are now working on statutory interpretation (Rodriguez 1989; Sunstein 1989; Ball 1988).

Interest Groups and Non-Constitutional Law

There is a fairly long tradition of studying interest group litigation activities as a facet of constitutional law. At least this movement directed our attention outward from the constitutional Supreme Court as such to some other legal actors. There is no reason to limit ourselves, however, to constitutional litigation in the Supreme Court. Interest groups and many courts interact

in many areas of both constitutional and nonconstitutional law and in judicial politics more generally. Interest group studies are now beginning to reflect this realization (Caldeira and Wright 1988; Epstein 1985; O'Connor and Epstein 1985; Shapiro 1990a; Maveety 1991).

Beyond the Three Major Movements Outward From Constitutional Law-Supreme Court Studies

Interdisciplinary Studies by Political Scientists

So far we have been looking at a movement "outward" and "downward" from constitutional law and Supreme Court toward all law and all courts. And we have tracked that movement largely as one within "public law" or "judicial politics" itself or at least within political science as a self-contained discipline. Much of the movement in public law political science, however, has been even further outward, beyond the edges of the discipline.

Initially the very term "public law" was a claim by political science to do the law that the law schools did not do. They did common law, which denied there was such a thing as public law. In practice, however, political science departments and law schools both taught constitutional, administrative, and international law. Armed with the more acute sense of the political, which gave them a major comparative advantage in the age of New Deal judicial realism, political scientists like E. S. Corwin, T. R. Powell, J. A. C. Grant, and Robert Cushman were dominant players in the constitutional law game shared with lawyers. Many political scientists continue to do constitutional doctrinal analysis and, in this sense, political science constitutional law has always been and is now interdisciplinary, calling on skills in both legal and political analysis.

Beyond this root interdisciplinarity, however, there has developed a newer, major interdisciplinary thrust. This movement has been shared by both the law and political science disciplines. The law schools are full of "law and..." movements: law and economics, law and philosophy, law and literature, etc. Law professors working in constitutional and administrative law do not speak of themselves as in "law and politics," but that is only because they have so internalized political science teachings about political institutions and processes that they no longer think in terms of "and." Moreover, the critical legal studies and feminist legal studies movements in the law schools are themselves interdisciplinary in character.

The parallel movement in political science has a number of facets. Recent movements in public law political science toward philosophy (Barber 1984; Smith 1985) and toward literature are intimately connected to developments in political science constitutional law studies and will be considered further in the later sections on constitutional law. Here the central phenomenon to be considered is the law and society movement. In one sense, within political science, that movement is part of the outward and downward vectors we have already traced. It directed political scientists away from the Supreme Court and constitutional law and toward everyday courts and everyday law. From its initiation, political scientists like Victor Rosenblum, Sam Krislov, and Joel Grossman played major roles in the law and society movement. For a substantial number of public law, judicial politics political scientists, the annual Law and Society Association meetings, and the *Law and Society Review* and other interdisciplinary journals, are more significant than American Political Science Association [APSA] annual meetings and the *American Political Science Review* [APSR].

Initially in this presentation reference was made to a number of paradoxes that have tended to marginalize public law as a subfield of political science. One of the most central of these paradoxes is that precisely because political scientists have been highly successful at interdisciplinary legal studies, their status as political scientists comes into question. A political scientist who publishes in the APSR and the *Western Political Quarterly* and goes to APSA and Western Political Science Association meetings is indubitably a political scientist. What is a political scientist who publishes in the *Law and Society Review* and goes to Law and Society Association meetings? Precisely because most senior political scientists who are not in public law think of it as nothing more or less than constitutional law-Supreme Court studies, a young political scientist who is interdisciplinary and studying British judicial review of administrative decisions or work site implementation of OSHA rules (Rees 1988; see also Bardach and Kagan 1982) is in some danger of being told he or she is not a political scientist at all.

Yet it would be fatal for any serious political science commitment to the study of law and courts to denigrate interdisciplinary work in these areas. Such work is now central to the study of law and courts and constitutes the leading edge of law school scholarship. A political scientist who claimed to be a law and courts specialist and who knew nothing about law and economics or post-utilitarian moral philosophy or legal ethnography or alternative dispute resolution would simply be viewed as a fool by other law and courts specialists. Conversely, political scientists bring to

interdisciplinary legal studies a claim to comparative advantage comparable to all the other disciplinary claims. The political dimension of law is sufficiently obvious that the political scientist is clearly bringing as important and unique a contribution to inquiry about law as is an economist or sociologist. The political scientist's identity as political scientist ought not and need not be lost in the course of interdisciplinary work.

The range of interdisciplinary law and society work by political scientists is great. Rather than endlessly inflating the bibliography, I urge those interested to scan such leading interdisciplinary journals as *Law and Society Review*, *Law and Social Inquiry*, *Judicature*, and *Law and Policy* for work by political scientists. Feeley, Sarat, Krislov, Harrington, and a number of others contribute frequently. There are many other such journals including *Law, Economics and Organizations* and *Law and Politics*. The Jurisprudence and Social Policy Program at Berkeley is producing interdisciplinarily trained Ph.D.s and sending them into political science departments, among others, and so are various public policy schools and programs.

The point is that, using any definition of politics and any set of boundaries of the concerns of political science that you care to, all sorts of law, lawyers, courts and judges, not to mention law-crafting legislators and bureaucrats, pop up as major phenomena. Most of them are not the Supreme Court and constitutional law. And most of them are best understood through combining a number of modes of analysis -- some of which come from political science and some of which don't.

Political Science and the Interdisciplinary Study of Lawyers

Lawyers constitute one subject of interdisciplinary studies of particular concern to political science. In many political systems, lawyers constitute a fairly distinct political elite and, in most, a significant transmission belt for both public policies and political ideologies. Political scientists have been making some contributions in this area (Zemans and Rosenblum 1981; Kritzer 1990; Sarat and Felsteiner 1986, 1989; Kagan and Rosen 1985; Shapiro 1990b; Cain and Harrington n.d.; Provine 1986).

Another Brand of Interdisciplinary Political Science Studies

The law and society movement is not the only home of interdisciplinary political scientists studying law and courts. In part informed by various bodies of contemporary European social theory, and in part by the general suspicion of objectivism and positivism as

innately conservative that has gripped much of the American academy for the last three decades (Silbey and Sarat 1987), a number of political scientists have been central players in the "Amherst Seminar." The quotation marks are because the seminar has become a kind of symbol or shorthand for interdisciplinary work on law, lawyers, and courts whose subjects, boundaries, and field methods are about the same as those of the law and society movement but whose presuppositions and purposes are somewhat different. Focusing on law as a constitutive act, the Amherst group sees the study of law and courts not as a purely exterior observation of things that are "out there," but as itself part of the process of making the "there." At the most Heideggerian edge, this position becomes "What we say about the law is the law." Most often, however, the central thrust is that law is less determined by society and more self-creating than most social scientists have normally appreciated and that, therefore, those studying law ought to see themselves as part of the process of making law (see the replies to Trubek and Easer 1989 by various members of the Amherst group presented in *Law and Social Inquiry* 1990, 15, 135-180).

The New Institutionalism and Normative Theory: Interdisciplinary Studies in Law and Philosophy

Political science has recently been in the grip of "the new institutionalism" or "bringing the state back in." Quite obviously this movement brings renewed prominence to law and courts that are central institutions and central expressions of the state (Hall and Brace 1989; Nardulli 1991). We have already noted one aspect of the new institutionalism in the concern of rational or public choice analysts to deal with law and courts as significant aspects of principal-agent relationships. Another tack is taken by those who treat widely held stable values or ideologies as institutions that constrain and partly determine political choice. In this guise, the new institutionalism provides a new urgency to the study of the normative aspect of law.

"Political jurisprudence" has often served as a convenient tag for the 1960s through 1980s work of political scientists who worked on law and courts and sought to distinguish their work from the doctrinal analysis done by academic lawyers. This political jurisprudence emphasized the political nature of law, and an exterior perspective on law as opposed to the academic lawyer's interior participation in the improvement of law. In both these aspects, it was echoed by the later blooming critical legal studies movement. Largely positivist in approach, political jurisprudence tended to leave normative legal theory to others while never denying that

the values incorporated into and surrounding legal systems were themselves important facts to be studied. The relationship between political jurisprudence and the new institutionalism is a major agenda item for the 1990s. Early stages in the encounter may be traced in Stumpf (1983, 1988), Smith (1988), and the exchange between Sotirios Barber, Rogers Smith, and myself in volume three of *Studies in American Political Development* (1989).

"Legal theory" or "jurisprudence" has always been a recognized facet of the public law subfield. Public law political scientists have typically been either public law-American politics or public law-political theory types. Treated as political discourse, the opinions of the Supreme Court are a central body of American political theory. There are, of course, enormous overlaps between political and legal theory. Nevertheless, political theorists without public law training or interests are likely to ignore some portions of legal theory and some major aspects of the history of legal thought. Thus a specialization in legal theory is discernible, but the legal theory scholar is likely to straddle public law and political theory subfields. And both as legal and political theorist, that scholar is also likely to straddle political science and philosophy.

Starting in roughly 1960, moral philosophy or ethics has undergone an enormous post-utilitarian or "post-consequentialist" change. Moral philosophers now again assert that statements of right and wrong other than and better than "the greatest good of the greatest number" can be made. Normative political theory revives in such works as John Rawls's. Normative legal theory revives in such works as Ronald Dworkin's (Brubaker 1985). Legal theory political scientists join political theory political scientists in reading Rorty and other normative philosophers. Other segments of this book describe the currently flourishing state of normative political theory in the political theory subfield of political science. A parallel development inevitably must occur in public law as we return to normative legal theory (Fiskin 1983; Berns 1983).

The return to normative legal theory in public law political science is in one sense an aspect of the new institutionalism which takes values seriously as political phenomena (Epstein, Walker, and Dixon 1989). In another sense, this return is a manifestation of the law and philosophy movement also to be found in the law schools (Scheppelle and Waldron 1991). So here again, some public law political scientists must be interdisciplinary in order to do their jobs well.

The new institutionalism is then a vehicle for two interdisciplinary movements in public law political science, rational choice linked to economics, and normative legal theory linked to moral philosophy. The

new institutionalism's third major interdisciplinary thrust is toward history. As a field, American politics is currently experiencing considerable interest in American political history. The annual volume, *Studies in American Political Development*, is a major expression of this interest. It has already published some work in judicial politics. Legal history, including constitutional history, has been flourishing as a subdiscipline of history, and it may make little difference in most instances whether the producer of a given piece of work is a historian or a political scientist (Nash 1983; Jillson 1988; and the work cited elsewhere in the chapter on the origins of judicial review).

Things have come to such a pass, along both law and courts dimensions, that it is now actually possible that someone could be considered a public law specialist who has not written a constitutional law-Supreme Court dissertation and has never taught a constitutional law course.

Constitutional Law -- Supreme Court Studies in Many Forms

For the bulk of the readers of this volume, however, all the things said so far are mere idiosyncratic preliminaries to getting down to the real business of constitutional law-Supreme Court that continues to preoccupy most political scientists who identify themselves as public law or judicial politics specialists. Indeed, it is noteworthy, that with the outstanding exception of Maveety (1991), almost no recent work on the Supreme Court treats of both its constitutional and non-constitutional cases. All of the standard currents in the mainstream continue to flow.

Behavioral Studies

Behavioral studies of the attitudes, votes, and small-group participation of Supreme Court justices and other "jurimetric" studies continue (Brenner and Spaeth 1990; Segal and Cover 1989; Handberg and Tate 1991; Segal and Spaeth 1993; Baum 1989). These studies now extend to lower federal court judges and state judges, most notably in Goldman and Lamb (1985). Although, from the beginnings, these quantitative studies included concern for judges' votes in non-constitutional areas, they continue to present themselves largely as a methodological advance on conventional constitutional law studies. H. W. Perry (1991) has produced an outstanding analytical study of Supreme Court agenda setting. Analytical data studies will be much facilitated in the future by the recent development of extensive new data bases (Palmer 1990; Spaeth n.d.) Some of this work

moves toward the construction of more formalized theories of judicial decision making (Caldeira 1985).

Doctrinal Analysis and Advocacy

The distinction between scholarship and advocacy has always been uncertain or nonexistent in most of the legal scholarship produced in law schools. Much of that scholarship consists of doctrinal analysis that purports to yield the correct or a good, better, or best statement of the law. The central strategy is a massive and deliberate confusion of is and ought. The previous cases are examined to show that, properly interpreted, the body of existing law really adds up to the law as the author thinks it ought to be. In short, most such writing consists of expanded and embroidered legal briefs. The key question in understanding such work is "Who's the client?" In constitutional law scholarship of this sort, the writer really begins by deciding what he or she wants the constitution to mean and then works backwards shaping an interpretation of past history and legal doctrine and a policy analysis that will culminate in that meaning.

Political scientists who are constitutional law scholars have always participated in this brand of advocacy scholarship. Some of it openly presents itself as direct lobbying of courts and/or legislatures (Shapiro 1985; Levinson 1985; Schuck and Smith 1985). Most of it presents itself as a search for what the correct law is, which usually turns out coincidentally to be the law the writer thinks best (Alfange 1983; Binion 1983; Carmen 1985; Randall 1989). A bit of it adopts an exterior perspective, or "constitutional politics" approach in which the political scientist seeks to describe the various doctrinal moves made by various participants in a process that over time moves constitutional law, or rather the Supreme Court, from one policy position to another (Downs 1985, 1989). The task is not to arrive at the correct law but to engage in the standard positivist task of political science, describing who gets what, how, and why (Way 1989; Mendelson 1990; Scigliano 1988).

Post-Modern Constitutional Scholarship

Both the traditional legal scholarship of advocacy and the newer, positivist, constitutional politics scholarship is now being challenged by various "post-modern" approaches, including some feminist legal scholarship. (Much feminist legal scholarship is more conventionally positivistic. See Cook et al. 1987; cf. Binion 1991.) This post-modern scholarship either consciously denigrates the distinction between objectivity and subjectivity or adopts a "discourse-deliberation" or "practice" approach to constitutional law (Brigham 1990a;

Carter 1989; McCann 1986). The political scientist as constitutional scholar ought not to be either an external, objective describer of constitutional maneuver or a mere constitutional champion of particular social interests. Instead he or she ought to be a participant in the public discourse that continuously constitutes and reconstitutes constitutional and other law designed to achieve a more flourishing citizen in a more flourishing state (Carter and Harrington 1990; Carter and Gilliom 1989; Carter 1985; Levinson 1988; Bumiller 1988). Cass Sunstein (1990) and Bruce Ackerman (1991), both of whom are academic lawyers holding joint appointments in law schools and political science departments, and Sandy Levinson who is both a lawyer and a political scientist, are crucial figures in this movement. It is at the core of the Amherst interdisciplinary group.

Constitutional Interpretation

This movement is also to be found, often under the label "constitutional interpretation," among other political scientists of less post-modern bent (Murphy, Fleming, and Harris 1986). The renewed interest in textual interpretation of the Constitution is in part a natural reaction to the reductionism that characterized behaviorism and in part a piece of the new institutionalism (O'Brien 1991b). It is also in part the scholarly side of the partisan struggle to control the Supreme Court in which Republican and Democrat are translated into originalist and nonoriginalist, or mainstream versus natural law à la the Bork and Thomas hearings. Its intellectual vitality, however, is derived from the revivals of moral philosophy and of political philosophy that were alluded to earlier. Constitutional interpretation becomes a vehicle for doing real normative constitutional theory (Hirsh 1992; Baer 1983; Macedo 1990; Meister 1989; Arkes 1990; Miller 1985). And this constitutional interpretation political theory ranges from the Straussians and their "founding moment" to the champions of community, the reconstructors of liberal democratic theory, and a wide variety of egalitarians and social democrats (Smith 1985; Jacobsohn 1986; Kammen 1988).

Quite obviously, too, this renewed interest in textual interpretation is a facet of the current law and literature vogue and of the intellectual prominence of the array of European social theory from Heidegger to Habermas, Foucault, and Bordieu that focuses on language or communications practice. The constitution is a text and ought to be amenable to the whole gamut of literary interpretative techniques that have infested English departments for some time and are now colonizing law faculties. All political scientists know that constitutional law is not only or even principally the

constitutional text but is most centrally the interpretive practice of the Supreme Court and of the lawyers surrounding it. Why shouldn't political scientists' attentions return from scoring justices' votes to taking constitutional language seriously (Levinson and Mailloux 1988)?

The Eternal Question: The Legitimacy of Judicial Review

The new concern with constitutional interpretation is inextricably mixed with that constant of American constitutional concerns, the legitimacy of judicial review itself (Fisher 1988). New descriptive and prescriptive works on the role of the Supreme Court in American politics are added to the stream (Brigham 1987; Jennings n.d.; Lasser 1989; McDowell 1982; Steamer 1986; Wiecek 1988; Halpern and Lamb 1982; Carter 1985; Morgan 1984). Interesting new work has recently appeared on the origins and evolution of judicial review (Snowiss 1990; Goldstein 1986; Wolfe 1986). There is a now long-established tradition of study of the impact of Supreme Court decisions, which continues and broadens into more general studies of the capacity of the Supreme Court to achieve implementation of its decisions, most notably in Rosenberg's (1991) recent work (see also Dometrius and Sigelman 1988; Songer 1988). Constitutional implementation studies have also been influenced by the "outward" and "downward" movements since the pioneering work of Jack Peltason, so that we get studies of district court implementation of anti-discrimination decisions (Cooper 1988; Bullock and Lamb 1984), and even some consideration of the implementation of non-constitutional decisions (Johnson and Cannon 1984).

Following up earlier and current work by Murphy and Tannenhaus (1990), studies of public opinion and the Court continue to appear (Caldeira and Gibson 1992), as do works on interest group efforts to lobby the Court (O'Neill 1985). Led by Mason and Swisher, political scientists have long made a specialty of studies of individual justices as a tool of political analysis of judicial review. That work continues (Ball and Cooper 1992; Davis 1989; Murphy 1988; Silverstein 1984; Strum 1984; Lamb and Halpern 1993; Stephenson 1991). American politics specialists are now producing a great deal of analysis of divided government. The Supreme Court appointment process has been a uniquely dramatic public demonstration of that division, and public law political scientists, following pioneering work by Henry Abraham (1992), have begun to write about it (Danelski 1990; Shapiro 1990a; Grossman and Wasby 1990; O'Brien 1991b).

The Study of Law and Courts in Political Science

Thus, the outward and downward movement away from the constitutional law Supreme Court is counterbalanced by a major new vigor in constitutional scholarship. Moreover, the movement from advocacy scholarship toward an essentially positivist exterior perspective on law and courts has been balanced by a new self-confidence about normative constitutional theory or interpretation. Finally, a post-modernist jurisprudence tends to deny the distinctions between description and prescription, objectivity and subjectivity, and scholarship and participation. In this light, the political scientist is simply a participant in the interpretative community contributing to constitutional discourse.

The question may then arise of whether the political scientist has any special claim to expertise in this interpretive community. Those most exhaustively trained and most daily active in this discourse are lawyers. Those who provide its critical foundations are moral philosophers. Those who know most about what it is supposed to be doing, namely, interpreting text and constructing language, are literary theorists and theorists of social communication. The stance that law might well have a rich interior life of its own, but that the job of political science was one of exterior perspective showing how politics invaded law, and the degree to which law and courts were institutions of politics, allowed political scientists to claim that what they knew of politics gave them a unique competence to study law or at least certain aspects of law. Political scientists who claim that they are participants in the interpretative community of law, busy constituting the law through deliberative discourse, may have difficulty making any comparable assertion. Much may depend on the degree to which they can establish that law is not an entirely or even largely autonomous mode of discourse on the one hand or simply a branch of moral discourse on the other, but has some especially important political dimension that gives the expert in political discourse some claim to a separate and prominent role in legal studies.

The problem of a place for law-and-courts political scientists can be clearly seen in political science faculties. At the moment, a long-standing bipolarity of public law is reasserting itself. As we have noted, traditionally most public law scholars were either public law-American politics people or public law-political theory people with very small numbers connected to public administration or international relations. At a certain point in the history of political science such a collapse occurred in every other aspect of public law that it became, as it is now in many departments, a constitutional law-Supreme Court subfield of American

politics. Today the revived interest in constitutional interpretation and constitutional theory is recreating the political theory wing of public law.

Meanwhile the outward and downward and interdisciplinary movements have created some political scientists who are neither constitutional law leaning toward American politics nor constitutional law leaning toward political theory. There are non-constitutional (or rather all of public law rather than a little of public law) political scientists leaning toward American politics-policy-administration-regulation. They can conveniently fit into departments that treat judicial politics as a subfield of American politics or can fit easily into the public administration subfield, if political science departments can bring themselves to understand that a public law person need not be someone who sits in a corner mumbling *Dred Scott* and *Roe v. Wade*.

More serious is the problem that if public law is a subfield of American politics so that hiring is dominated by American politics faculty, there will be no access to political science departments for comparative judicial politics scholars. Most of those currently in this specialty are part-time converts from good old American constitutional law. But where can real "made from scratch" comparative law people go? American politics will not want them. Most departmental comparative politics groups will know so little about law that they will not want to "waste" a slot on a law type that they could use to get an Ethiopian or Estonian specialist. Only where public-law judicial politics has been retained as an independent subfield, or a set of particularly sophisticated comparative politics people exists, is comparative law and courts likely to have much of a chance.

It is to be hoped that international relations and state and local government or urban groups will fairly readily recognize their law needs given the renewed role of treaties in international relations and the pervasiveness of litigation and quasi-judicial administrative proceedings in state and local policy making. Certainly there is already a renewed acknowledgement of administrative law as an essential component of public administration studies. Yet in all these fields it is far easier to lobby for hiring someone in the public law subfield who will do international law or administrative law or state and local courts than it is to use one of your own precious slots for a law person. Thus, to perhaps a lesser degree but nevertheless quite tangibly, it will be harder for a department that treats public law as a subfield of American politics to develop the law support it needs in international relations, state and local government, and public administration than for a department with a separate judicial politics subfield.

The public law-judicial politics subfield today is flourishing along a larger set of dimensions than it ever

has in the past. The degree to which this flourishing will benefit political science as a whole depends upon the extent to which political science departments stop seeing their situation as one in which they are forced to spend one American politics slot on somebody to teach the constitutional law courses that pre-law students demand. Instead they might start seeing their real scholarly needs for dealing with law and courts, which pervade nearly every aspect of the politics they study.

In spite of all the talk about the conflict between research and teaching, in American universities teaching agendas set research agendas far more than is commonly understood. So long as political science departments think they *must* teach a constitutional law course and need not necessarily teach any other law courses, the judicial politics research agenda will remain excessively directed toward the constitutional law Supreme Court. Departments should learn that there is no single necessary undergraduate political science course in law and courts. Where courses on Congress and the president, or legislatures and executives are taught, there ought to be a comparable course on federal courts or courts in general that covers the same range of policies, processes, and behaviors as is covered in those other courses. Where comparative politics groups recruit faculty with an eye toward teaching not only country courses but courses on comparative parties, political economy, development, etc., then one of the courses to be contemplated is a comparative law and courts course. Departments that think they can give policy courses with only a constitutional law-Supreme Court person on hand, rather than one who looks at all kinds of law and courts, are wrong. Departments that teach bureaucracy and regulation courses need courses in administrative and regulatory law because that law is a major part of bureaucratic and regulatory politics. No undergraduate will die, or worse yet, fail to get into law school, because he or she hasn't had a constitutional law course. Departments ought to begin telling their students the truth — that law schools don't care at all about undergraduate constitutional law courses. A course on the legal thought of Bentham and Weber sure beats tracing the Supreme Court's move from the fair trial rule to selective incorporation. If one course there must be, then a course on comparative law and courts or on the whole law and politics of the U.S., not just the small and peculiar constitutional part of it, would certainly be better for the students' intellectual health and the departments' research agenda than a constitutional law course. The movement in the subdiscipline is toward a set of people who treat all of law and courts as interwoven with all the rest of politics. It is time that other political scientists demand public law scholars and teachers who can speak to them, not to John Marshall.

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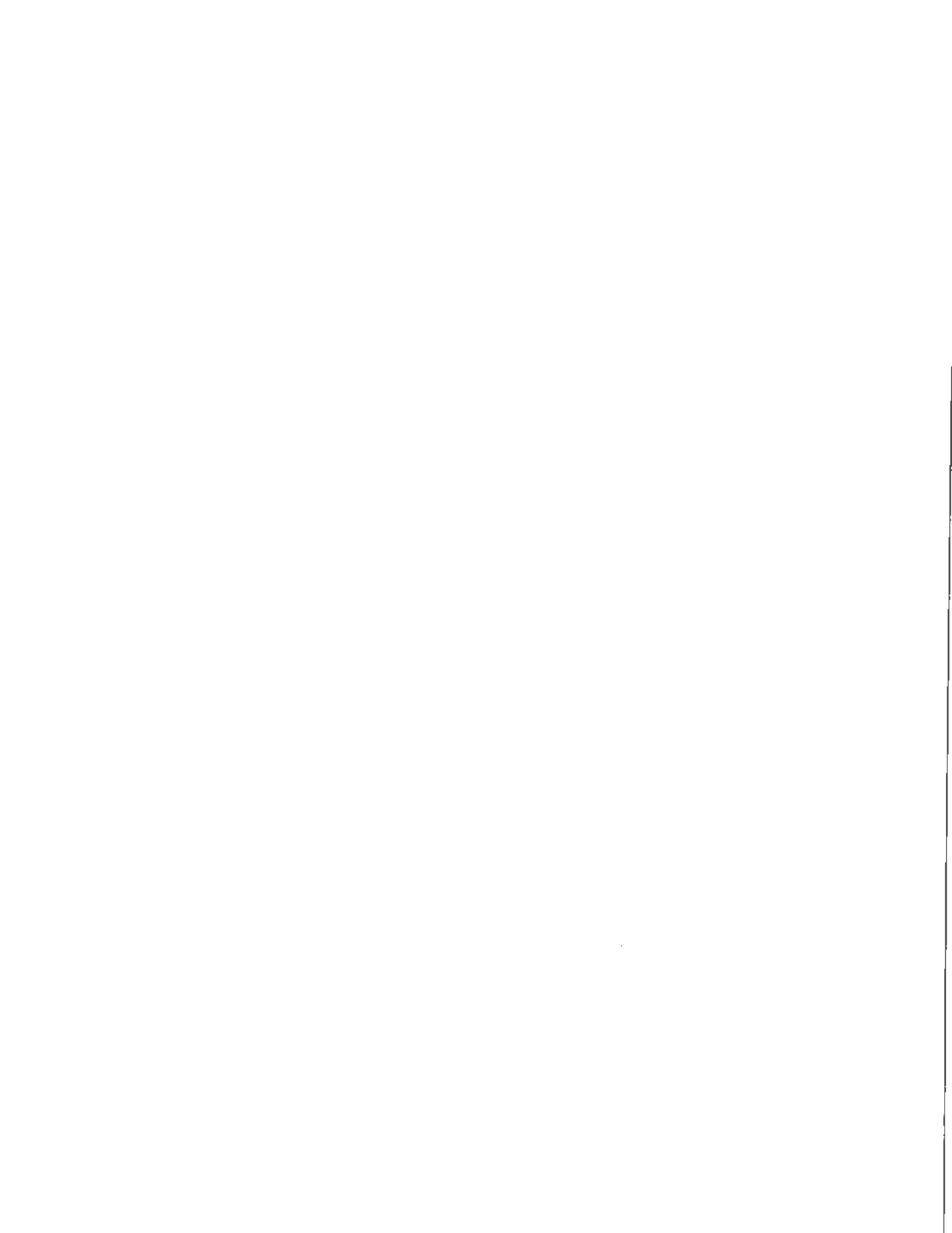
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Political Executives and Their Officials

Colin Campbell, S.J.

Unlike most work dealing with style, then, Campbell makes a serious attempt to move beyond the personalization of institutions, and he is explicitly concerned with theory-construction and other building blocks of social science. But [presidential] style, as always, proves to be a stumbling block rather than a vehicle for progress -- promoting conceptual confusion, the endless proliferation of relevant variables, and more complications than any analysis can make sense of (Moe 1990, 44).

The Campbell cited above is none other than your current author. One might legitimately ask whether someone prone to devising frameworks "promoting conceptual confusion..." is the appropriate person for presenting a distillation of the state of political science on the topic of "political executives and their officials." However, this area of inquiry is as old as political science itself. And its major debates have centered precisely on such issues as the complexity of models employed in analyzing executive leaders and their relations with their officials. This does not give one license for conceptual overkill. Yet, it should make us all attentive to the difficulty of reconciling our desire to capture the richness of this subject matter and the need to make sense of it all.

Political science by its nature attempts to focus on moving targets. And, as sub-fields go, executive leadership presents as much flux as any object of political science inquiry. This means that students of political executives constantly find themselves struggling with sharp learning curves as one administration takes over from another or executive leaders face crises or unforeseen circumstances which call upon their adaptive skills (Rockman 1984, 4-7). Also, just about everybody feels compelled to share views about executive leaders. For instance, my barber has become aware of the "lightening rod" theory of why George Bush kept John Sununu so long. My barber expounds his ideas for all comers -- regardless of whether they make their living studying presidents. Other burning issues to political scientists -- such as how Congress allocates committee

memberships -- would bore my barber and most of his customers to tears. Whereas other political science fields attract a lot of deference, presidency and executive leadership scholars compete with an overabundance of conventional wisdom. In this field, attentive publics often actually say on their own the types of things that one hears pronounced in panels at the APSA annual meeting.

One finds a tremendous amount of volatility in this field. From an institutional perspective, however, the presidency remains infinitely more vulnerable to the personality of the incumbent at a given moment than either house of Congress would be to the constellation of personalities that occupy it from time to time. Changes in the balance between the president and Congress due to party turnover or the peculiarities of personality can badly miscue even the most shrewd observers. The profession broadly endorsed John F. Kennedy's inattentiveness to organizational structure (Neustadt 1960). Political scientists had convinced themselves that excessive institutional rigor in the policy process had led to overcooking issues during the Eisenhower administration. Fred I. Greenstein's revisionist analysis of Eisenhower, however, contests the early critique of Eisenhower -- arguing that his administration achieved a highly adaptive balance between structural and personalized responses to issues (1982).

When students of the presidency piled on the anti-imperial-presidency bandwagon after Watergate (Nathan 1975), we contributed little foresight of the extreme decentralization that characterized the Carter administration. In the aftermath of Carter's failure as a political executive, we were the first to issue glum prognoses for the future of presidential leadership in the face of Congress's ascendancy (Hecklo and Salamon 1981). During the Reagan years, we began to point to the facility with which presidents can short-circuit institutional relationships with other parts of the policy apparatus in Washington -- Congress included -- by going public, pursuing partisan responsiveness, and using foreign affairs, to distract attention from domestic issues (Kernell 1986; Moe 1985; Rose 1988; Campbell 1986). With the end of the Reagan years and the minimalism of

the Bush administration, we began to focus upon "divided government" and its constraints on the executive (Sundquist 1988).

Thus, political scientists have tended to extrapolate from the approach and circumstances of each administration in making more general assertions about the state of the presidency which soon fail the test of time. We have taken kernels of truth from analyses of individual presidents and overdrawn their consequences for the development of the presidency.

This chapter takes the broad compass of "political executives" literally. Thus, it will consider in some detail crossnational perspectives on executive leadership. In the U.S., "political executives" would take in most immediately presidents. Strictly speaking, cabinet secretaries -- as unelected appointees who serve at the pleasure of the president on the advice and consent of the Senate -- do not tap autonomous executive authority. Yet, some cabinet members might enjoy greater leverage and maneuverability because of the strength of their personalities, the degree to which the president defers to their expertise and prerogatives, the importance of their department, the salience of the issues they handle to the president's agenda, and/or their ties with sectors of the population whose support the administration requires for legislative and electoral success. Other presidential systems similarly construe executive leadership so that authority tends to focus on the individual who serves simultaneously as head of state and head of government (Riggs 1988, 247-78).

Parliamentary systems divide the political executive. Typically, they provide for a split between the head of state and the head of government with the former assuming the bulk of the ceremonial functions and the latter effectively serving as chief executive (Mackintosh 1977). More important, the head of government shares the day-to-day exercise of executive authority with a cabinet (Hennessy 1986). This body -- either because of a highly legitimized constitutional convention or a specific provision in the country's basic law -- defines the parameters of executive actions and initiatives in collective terms. Cabinet systems vary in the degree to which ministers actually deliberate collectively (Mackie and Hogwood 1985). And, the stylistic preferences of the heads of government can introduce great fluctuations in the role of cabinet dynamics in the process of executive leadership.

Americans might find it tempting to conclude that viewing political executives in the U.S. in light of executive leadership in other countries amounts to introducing beer to a wine tasting. However, a commonality has emerged in the tasks of executive leaders over the past 20 years. The fact that in many respects presidents and prime ministers face similar

problems and frequently employ comparable strategies underscores this phenomenon (Rose and Suleiman 1980; Plowden 1987). The electronic era has meant that both, more often than before, catapult to their positions without having passed through the conventional socialization lower down in public life (Campbell and Wyszomirski 1991, 13). Presidents and prime ministers tend much more than before to go above the heads of their cabinet colleagues and legislative supporters to make special pleas directly to the people (Seymour-Ure 1991).

Fewer prime ministers than presidents face divided government. However, some must work as hard as presidents to bring the necessary coalitions together to fulfill comprehensive agendas. Labour prime ministers in Britain must deal with an extremely fractious caucus; both Labour and Conservative prime ministers in the U.K. increasingly find their prerogatives constrained not only by their cabinet colleagues but by their opposition numbers in the European Economic Community (Nugent 1989, 88-109). German chancellors usually can only form coalition governments and must follow the rubrics of collective government mandated by the Basic Law (Mayntz 1987; Berry 1989). Canadian prime ministers might control Parliament. However, any legislation that affects the powers of the provinces must be negotiated through tortuous discussions with provincial governments (Simeon 1972).

To some extent, all advanced democracies have wrestled with similar fiscal constraints to those faced by the United States. Further, they have encountered comparable declines in public support of social programs and government intervention in the marketplace to those experienced in the United States. Thus, the increased difficulty of governance coupled with declining resources and support have introduced conditions every bit as likely to incline prime ministers toward minimalism as those which prevail in the United States.

Johan P. Olsen has done an especially good job of alerting us to the effects of economic conditions on our view of the state and, in turn, what we expect from political executives. His *Organized Democracy* argues that political executives operating within the corporatist-pluralistic model for governance that emerged in Norway during the period of economic expansion and growth of the welfare state lost all sense of proportion (1983, 31-33). Their efforts to respond to societal demands went beyond integration of broad and institutionalized groupings to accommodation of extremely particularistic and ad hoc interests. Following strategies of privatization and deregulation, the conservative-center government of the mid-1980s attempted to recapture the "institutional state" -- a style of governance which attempts to define in clear terms the boundary between the state and various sectors of society.

In a subsequent work, Olsen has further developed his thesis in ways which point up its applicability to the comparative study of executive leadership (1987). Here three points emerge. First, political executives must base their relations with the bureaucratic elite in ways which comport with the formers' preferred model for governance. Instrumental approaches will prevail when general agreement exists over the boundaries of the state; evolutionary modes when the political executive attempts to redefine these boundaries; an emphasis on adaptiveness characterizes the classic corporate-pluralist state as it adjusts to shifts in political power between social groups; and political responsiveness becomes the dominant value for the welfare state which -- like Norway in the 1960s and 1970s -- has become the "supermarket" for society's "great necessities."

Second, political executives can misread their circumstances to the point where a disjunction develops between their leadership style and the received image of the state. Canada's Pierre Elliott Trudeau ceaselessly pressed the sovereign-rational approach in a highly fragmented, consociational and corporate-pluralistic state (Aucoin 1986; French 1980; Hartle 1983). Britain's two Labour prime ministers during the 1970s -- Harold Wilson and James Callaghan -- failed to adequately address the shift of British thinking about the state from corporate-pluralistic to institutional (Keegan and Pennant-Rea 1979; Rose and Peters 1978; Hood and Wright 1981). Thus, their adaptive strategies were left unrewarded.

Third, Olsen's emphasis of administrative style as related to the boundary between the state and society invites us to make a distinction between moderate and radical views of the proper role of government. In other words, we must allow for thinking which crosses the line between reorienting and dismantling the state. As represented by some of the thinking behind Ronald Reagan's and Margaret Thatcher's form of market-oriented governance, a distinct movement emerged in the 1980s toward recrafting the entire context of the state in ways which would limit it to only a fraction of its former functions (Savas 1982; Riddell 1983, chap. 8).

The remainder of this chapter will serve as a guide for those wishing to pursue some of the central issues concerning political executives and their relations with their officials. It will not contain an American bias because your author is a Canadian who has cut his teeth on comparisons of executive leadership in the U.K., Canada, and Australia as well as the United States. However, it will have an Anglo-American bias. It will make appropriate reference throughout to issues and literature emerging from beyond the Anglo-American context. But it cannot lay claim to the same familiarity

with the materials. The sections which follow will place the issue of political executives in the wider setting of the current direction of the discipline in this field in the United States. Tremendous pressures have emerged for greater rigor in the analysis of the presidency. We will probe what realistically can be done to address these concerns and the degree to which what we learn might be applied in other systems.

The reader will find that I have not departed from the multi-dimensional approach which occasioned Terry Moe's response noted at the very beginning of this chapter. I will, however, take extra pains to meet his concerns half way and call for a recognition among students of executive leadership that the accessibility of various dimensions of our subject matter to rigorous theory and analysis differ considerably. Investigators should always be able to locate themselves in this matrix and to exercise due restraint -- especially when making connections from one body of knowledge to another.

The Search for Rigor and Trends in the Discipline

One cautionary note might guide our reflections as we consider the search for rigor in the study of executive politics. Above, I have reviewed the swings in the discipline's view of the presidency since the emergence of the behavioral era by reporting the reaction to various incumbents. These gyrations should alert us to a shortfall in the discipline's capacity to retain distance from its highly volatile subject matter.

Some of the swings owe more to episodic events than longer-term distortions in professional vision. In their own ways, Nixon and Carter scared us -- the former because he exposed the vulnerability of the system to imperial behavior, the latter because his performance pointed up the bias toward paralysis in the system -- and we tended to project from these experiences to our view of the presidency. However, other swings stem from changes in political scientists' criteria for assessing governance.

The initial absorption of the discipline with the divided government issue emerged in the late 1940s and early 1950s. The recent focus on divided government often appeals most strongly to authors inclined to interpret Bush's minimalism as reflecting the constraints of the institutional context of the administration. Thus, Bert A. Rockman has attributed Bush's tendency to "syndicate responsibility" to the exigencies of divided government (1991, 13-14); Charles O. Jones has maintained that we cannot understand Bush unless we employ the standard of how he handled "diffused

responsibility" rather than examining his performance according to the unrealistic expectations of "party government" (1991, 38); and Paul Quirk has allowed that the institutional context faced by Bush put the onus on his ability to pursue mediation and cooperative negotiation (1991, 72).

This latter-day response to divided government departs from that which prevailed in the 1940s and 1950s. In that period, the discipline took a strongly prescriptive stance based on the view that it should work for the reform of the system. In a 1950 statement, an American Political Science Association committee on political parties called for changes which would foster the development of a responsible two-party system in the United States. Here parties would identify realizable goals and maintain sufficient discipline to actually deliver on their promises. This perspective took root in a fascination at the time with the British parliamentary system.

Unlike the U.S., Britain had moved decisively after the war to advance further the welfare state. Reflecting upon the incapacity of the U.S. system to press the social agenda, scholars became enamored of party discipline. And, their commitment to addressing the need for leadership capable of achieving adequate policies for social maintenance flowed over into other realms later in the 1950s and early in the 1960s. Indeed, James MacGregor Burns -- writing in a period during which the Democrats controlled both houses of Congress -- lamented the intractable nature of the American "four-party" system. Due to the different coalitions of forces which produce even administrations and congresses of the same party, presidents -- Burns believed -- had proved incapable of tackling the big issues -- "urban decline, conservation, tax reform, medical care, government organization" -- even when the same party ostensibly controlled all the levers of power (Burns 1963, 3). One author of Burns' generation -- James L. Sundquist -- continues to press this view (1986, 78-79; 1988, 613-635). Today he focuses his critique more on the need for the type of executive management capable of reestablishing national purpose and direction than upon concern about the stalled social agenda. Yet, his prescriptions for constitutional reform fit squarely within the responsible two-party tradition.

A *realpolitik* emerged within the profession in response to those disillusioned by the U.S. tendency toward indecisive government. The first of this wave became known as the pluralists. Led by the seminal contributions of Robert A. Dahl and Charles Lindblom, authors within this tradition argued that we should lower the bar and expect much less comprehensive decision-making in the U.S. system (Dahl 1956, 1971; Lindblom 1965). This perspective had a pervasive effect on all fields of the discipline -- including the dissemination of

the view that the system by its nature would provide most presidents with opportunities to make only incremental changes to existing policies. Working from shared analytic perches elsewhere within the behavioral tradition, authors such as Theodore J. Lowi (1969) and Jack Walker (1966, 1969) took issue with the implications of the pluralist analysis. Nonetheless, the approach set the agenda and tone of most empirical research in the field of executive politics throughout the 1960s and 1970s.

Beginning in the 1960s, two-party responsible government began to encounter more direct and damaging assaults from public choice theorists. Such scholars as James Buchanan, Gordon Tullock, and William Niskanen effectively pressed the view that the U.S. should have less government, not more (Buchanan and Tullock 1962; Niskanen 1971). To such authors, the great debates being waged between the proponents of two-party responsible government and incrementalist models missed the point. Given greater knowledge about their options, voters and their representatives would make only those choices that correspond to the highest-ranked selections on their preference schedules. They would not understand, much less support, comprehensive policy programs. Nor would they -- given full knowledge of their options -- tolerate incremental augmentation of programs which do not serve their immediate interests.

Public choice theorists ushered in a new perspective on political leadership which greatly influenced expectations for executive politics. They anticipated by several years the limits of campaign appeals based on candidates' promises of social programs for which ultimately voters had to pay. It was not until the late 1970s that more conventional analysts began to observe that executive politics faced new constraints. Social spending had become such a drag on advanced economies that its net effect no longer contributed that clearly to citizens' disposable income (Rose and Peters 1978, 33-34).

The public choice perspective's link with monetarist economics was reflected in the politics of the two most dominant chief executives of the eighties -- Ronald Reagan and Britain's Margaret Thatcher. Thatcher achieved considerable success in pressing the public choice agenda. She so cut spending that the U.K. government operated in surplus through the latter years of the 1980s. She privatized several government enterprises and introduced a series of reforms designed to make the public service more accountable for its resources. On the other hand, Reagan's experience suggests that the U.S. system lends itself to public choice approaches as poorly as it does to the expansive and comprehensive welfare state. Conflict and inaction plagued the administration throughout over such issues as how deeply supply-side economics should dig into taxes, the extent of cuts in

social spending, and the degree to which the Pentagon should "give at the office" and absorb its share of parsimony (Stockman 1986; Roberts 1984).

The Public Choice Thinking: Consequences for the Study of Political Executives

The mixed results of public choice in the U.S. constitute a true paradox. Its proponents developed the approach in response to elements of fragmentation in the U.S. system which made it very hard for executive leaders to reverse the incremental growth of bureaucracy. Niskanen's work addressed most directly the perceived crisis of executive politics in which presidents find it impossible to break down the coalitions of officials, legislators, and interest groups which fuel the proliferation and expansion of government programs. He asserted that the budget-maximizing tendencies of government officials -- appointive as well as career -- stood at the root of the problem (Niskanen 1971, 21-22, 38). The fragmentation of the system sets officials up in a perfect climate in which to pursue their personal utility. This involves seeking higher salaries, better perquisites, greater reputations, and more power; dispensing more patronage; increasing programmatic outputs; and it adds up to immense pressures to expand organizations and increase budgets.

In suggesting how political executives might stem budget maximizing, Niskanen departed from the received wisdom of the time. This held that those seeking to control bureaucracies should acquire more detailed information on the actual operation of policy programs. Niskanen argued, on the other hand, that the surest way to take political executives off the scent of budget maximizers was to get them absorbed by issues associated with comprehensiveness, detail, procedural rationality, and control (Niskanen 1973a, 6-8; cites Wildavsky 1961).

Niskanen proposed a number of structural changes in the budget process which would replace ad hoc mechanisms with automatic rules which would force tough decisions. This automation of budgeting included a committee of congressional leaders which would recommend "target" outlays to both houses and the president, a requirement that Congress would have to offset spending beyond that recommended in the agreed budget with dollar-for-dollar increases in personal income taxes, and the assumption by the Executive Office of the President of a more active role in providing express guidance to the Office of Management and Budget on budget priorities -- thereby, making "the proposed budget a more effective instrument of the political interests of the

President" (Niskanen 1973a, 17-19, 55-57). During the chronic fiscal stress which began in the mid-1970s and still persists, Congress and presidents have engaged in an iterative process gradually moving toward the type of budgetary discipline envisioned by Niskanen. Once again, however, the fragmented nature of the system seems to preordain that many pockets of government activity will prove highly resistant even to potential draconian automatization of budgeting.

As mentioned above, public choice has found much more fertile soil in parliamentary systems. This is ironic. Several qualities of such systems already make them less susceptible than the American system to afflictions -- such as budget maximizing bureaucracy -- which so absorb the attention of Niskanen and others. First, political appointees and career civil servants in the U.S. -- especially the latter -- tend to focus their careers much more narrowly than their opposite numbers in parliamentary systems (Hecl 1977, 116-120; 1984, 18-20). For U.S. career officials, movement from one specialized section of a department to another occurs rarely. Much less would we expect transfers from department to department. This inclines U.S. officials toward blinkered perspectives. They, therefore, become much more territorial than their counterparts in parliamentary systems.

Second, parliamentary systems do not have truly transformative legislatures. That is, legislators lack the capacity to mold and reshape laws and budgets independent of the guidance provided by the political executive as embodied in the leadership of the governing party or coalition (Polsby 1975, 277). The absence of long and reasonably secure tenure in specialized fields among bureaucrats and transformative legislatures inhibits the development of strong client-patron relationships in parliamentary systems. In the U.S., the currency that such terms as "sub-government," "iron triangles," and "atomization" have attained suggests the extent to which permanent officials can play both the political executive and Congress off the middle to get their own way (see, for instance, Olson 1982, 50-52; and Aberbach, Putnam, and Rockman 1981, 94-100).

In the English-speaking world, parliamentary systems began implementing public-choice approaches to budgeting in the mid-1970s. As a condition of its 1976 rescue of the pound, the International Monetary Fund prompted the U.K.'s Labour government under James Callaghan to greatly extend the application of "cash limits" to public expenditure (Keegan and Pennant-Rea 1979, 205). Here the Treasury imposed cash limits on the estimates submitted to Parliament (Campbell 1983, 185-186). Departments received from Parliament authority to spend a given amount of cash, not an open-ended remit to pursue specific programs. In theory, this procedure

greatly reduced cost overruns. However, few parliamentary systems concentrate budget power in one department to the degree that the U.K. does. The imposition of cash limits — especially without external pressure — becomes especially difficult if control over economic policy is divided between a finance department and a budget department — as is the case in Canada and Australia. It also would encounter much more resistance if ministers have become used to making budget decisions through collective consultation rather than deferring largely to the judgment of the minister responsible for preparing the budget.

Canada in the mid- to late-1970s presented a clear instance of a system whose budgeting process had become extremely fragmented. Since Confederation in 1867, a committee of cabinet — the Treasury Board — has given collective review of the expenditure budget. In the 1960s, the cabinet increasingly did its business through specialized policy committees. In the same period, the Treasury Board Secretariat emerged as a department separate from the Finance Department. As well, the cabinet secretariat — called the Privy Council Office — began increasingly to present independent advice to the prime minister and cabinet committees rather than simply leading logistical support to the collective decision-making process. Paralysis arose in the executive-bureaucratic arena — especially in connection with questions which related to the budget. The Treasury Board found it difficult to impose discipline. As a recent offspring of the Finance Department, it lacked standing. Meanwhile, the Privy Council Office remained very much within the expansive mindset of the 1960s — tending to give moral support to cabinet committees as they continued to dream up expensive government programs and regulations (French 1980).

Through an iterative process beginning in 1978 and involving governments of both the Liberals' Pierre Elliott Trudeau and the Progressive Conservative's Joe Clark, the Government of Canada totally revamped its budget-making apparatus. Public-choice thinking operated behind its moves. Key public servants had become convinced that cabinet committees might be used to break down the corner-fighting tendencies of ministers (Borins 1982; Doern 1982; Van Loon 1983; Campbell 1983, 194-200). Here ten expenditure envelopes with specific sums of cash were distributed among the policy committees of cabinet. If ministers decided to allocate additional funds for a new or existing program, they would have to pay for it with savings found in the salient envelope.

As the theory went, ministers would develop a sense of sectoral responsibility allowing them to identify potential savings in their departments. That is, they would more willingly give up funds if these went to their sector rather than back into the general treasury. This higher-level game theory was lost on ministers who proceeded to

logroll in precisely the way that congressional committees do in the United States. That is, they would trade off their support of one another's programs and offer to the government politically unacceptable savings — such as eliminating the Royal Canadian Mounted Police Musical Ride, Canada's equivalent of the Washington Monument. By fiscal year 1982-83, the deficit had ballooned to fully 50% of revenue. The experience points up the possibility that — in fragmented policy arenas, presidential or otherwise — facile adoption of public choice solutions can actually backfire and exacerbate the incapacity of the system to make tough decisions.

As already noted, the Thatcher government in the U.K. so tightened fiscal policy that it ran surpluses through the latter part of the 1980s. Rational choice perspectives played an important role here. In 1973, William Niskanen published a monograph in the U.K. which hoped to alert Britons to the need to control the growth of bureaucracy (1973b). This work argued that prime ministers should eschew particularistic considerations — such as personal and regional backgrounds — in the assignment of cabinet secretaries and insure that they not go native by shuffling them occasionally on a random basis (Niskanen 1973b, 60). In making such proposals, Niskanen displayed a lack of understanding of the dynamics of cabinet systems of government. However, his book's focus on the evils of bureaucracy worked a tremendous effect on Mrs. Thatcher — who urged all of her ministers to read it when she formed her first government in 1979.

Effectively adapting Niskanen's message to the circumstances of the British executive, Mrs. Thatcher stressed innovations which made budgeting less ad hoc and more automatic (Campbell 1983, 186-189; Wildavsky 1983, 164). She extended cash limits to several previously exempt programs; she created a "Star Chamber" of cabinet — a group of her most prominent ministers — to impose spending constraints in cases where the Treasury and line departments had become deadlocked; she abolished Policy Analysis and Review — that is, interdepartmental studies of programs which as often as not had failed to recommend the tough options necessary to limit the size of the budget.

Some schizophrenia has emerged among those who style the central task of political executives as taming the bureaucracy. Public-choice-oriented political executives have embraced as a primary goal reduction of the bureaucrats' discretion and latitude for independent advocacy. In this respect, they have tried to reestablish the dichotomy between policy and administration which dominated thinking about the relationship between politicians and bureaucrats earlier in this century (Wilson 1941 (reprint); Goodnow 1900, 92-3; Gulick 1937, 10). Yet, many of the same political executives have sought to devise ways of making bureaucracy run more like

organizations in the private sector. This would involve more creative and risk-oriented management. Niskanen argued:

Bureaucrats...would be permitted to offer a wide range of public services....There would be no "strong" departments or "strong" secretaries. The choice of which bureau or combination of bureaus to supply a specific service would be forced to the level of the executive review (1973b, 61).

Peter Aucoin -- a Canadian scholar who has followed closely managerial reform in his country over the past 20 years -- has argued that the automatic decision criteria and the managerialist paradigms of public-choice political executives derive from different and potentially conflicting premises (1990a, 125-126). The former style the problem of the relationship between political executives and bureaucrats as one of control and seek measures through which elected politicians might "tame" the public service by putting it under tighter constraints. Managerialist paradigms, on the other hand, start with the proposition that, intramurally, bureaucracies run overly hierarchically -- thereby, stifling imagination and initiative.

In the U.S., the two premises have not done much hand-to-hand combat. This owes largely to the difficulty of introducing managerialist concepts in a highly fragmented bureaucratic culture (Campbell 1986, 192-93; Hansen and Levine 1988, 267-68). On the other hand, the U.K., Canada, Australia, and New Zealand have all initiated managerialist programs with relatively far-reaching objectives (Aucoin 1989; Boston 1987; Considine 1988; Fry 1988; Scott, Bushnell, and Sallee 1990; Keating and Holmes 1990; Kemp 1990). As with the introduction of automatic budgeting techniques, however, the level of fragmentation in the political/bureaucratic system appears to affect the degree to which political executives can guide public service organizations toward managerialist approaches. Here Britain and New Zealand have achieved the most sweeping changes -- which is not to say that these have all worked; Australia has taken a more paced approach and achieved a great deal, and Canada has moved cautiously and made only modest progress (Aucoin 1990a, 1990b; Hood 1990; Campbell and Halligan 1992).

The New Zealand case represents by far the most conscious use of public choice theory as a justification for managerial reform in bureaucracy. For instance, Graham Scott and Peter Gorringer (1988) -- both Treasury officials -- have explicitly employed agency theory in justifying their department's initiatives toward reorganization of the public service. Relying heavily upon the work of A. A. Alchian (e.g., Alchian and Woodward

1987), they fully embrace the view that bureaucrats serve as the agents of politicians who, in turn, function as the agents of the populace. In this regard, Scott and Gorringer distinguish between strategies for improving "performance" and those attempting to enhance accountability (1988, 6). The latter focus on whether "politicians...buy the right services to achieve social goals like wealth, justice and the relief of suffering." Politicians need not purchase all the services which advance social goals from the bureaucracy.

In all of this, bureaucrats often perceive constraints on their agency. They welcome the opportunities for greater flexibility that managerialism holds out. However, many find themselves tempted to conclude that political executives have thrown them crumbs after sharply curtailing their ability to exercise discretion and to operate as advocates. A civil-service head of a large department in Britain has put it as succinctly as any:

I said to Treasury, and so did many others, "This is a fine doctrine, but unless you believe it and are prepared to implement it and accept what goes with it, it won't work." ...you can't say to someone, "You're responsible for your budget and you won't get any more money, and you can carry your own account," if at the same time you are saying, "I will determine your staff, what they will be paid, how much you will pay for accommodation...." And, this is exactly how it worked out. The amount of delegation of eventual control was at most five per cent (Campbell 1992).

Public choice theory concerning political executives emerged first in response to the fragmented conditions of governance in the United States. However, policies deriving from public choice have not taken particularly strong root here. That is, political executives pressing public choice approaches have run into the same types of systemic obstacles to coherent policies encountered by those attempting to expand the role of government in the 1960s. Advocates of public choice in some parliamentary systems, on the other hand, have advanced the cause with considerable success. Here the U.K. and New Zealand stand out. In each case, strong majority governments pushed the reforms. The fact that each country has only one department, the Treasury, which controls both economic and budget policy, greatly assisted the process. Australia presents a case of a more fragmented system, with a Labor government highly dependent upon mood swings in the party's parliamentary caucus and divided authority between the economics and budget departments. However, the Labor government negotiated these tricky waters deftly -- largely through the

exceptional commitment of time and effort that Bob Hawke (prime minister, 1983-1991) made to deliberations on the budget, and to the discipline of a group of "economic rationalist" ministers and a like-minded cadre of senior officials (Campbell and Halligan 1992).

Some observers have concluded that public choice approaches have gone too far in systems which have accommodated them more readily than others (Pusey 1991). In this respect, it appears that governments can engage in the practice of overtreatment. This prospect arises especially when they start to chalk up fairly substantial surpluses. It also presents itself when it appears that the government has embraced managerialist rhetoric only for cosmetic reasons. That is, some programs designed to give officials more discretion in the use of their resources involve such a small proportion of their ever-shrinking budgets that they serve only the symbolic goals of the government.

The Presidency, Public Choice, and the Unresolved American Debate

Unlike the study of Congress, the U.S. presidency sub-field has remained relatively resistant to scholarship which employs formal theory. Many have attributed this fact to differences in the data base for presidential studies. At any time, there is only one case of "president." Widening the scope to take in executive branch behavior does not help much. The executive branch operates informally and iteratively, and much of what it does and how it functions remains obscured from public view. Congress, on the other hand, provides clear units of behavior such as votes and the allocation of committee assignments.

Nonetheless, formal theory has pervaded mainstream political science and increasingly dominates the journals. Some practitioners of the approach have already cast their eyes upon presidential studies. For instance, Terry Moe -- even though the presidency field is not his main specialty -- has sparked lively debate with a few cogent and strategically placed challenges. One of his recent contributions put the case starkly: "the positive theorists are coming anyway. They are going to invade presidential studies, just as they invaded legislative and electoral studies" (Moe 1993).

The current debate on the role of institutional approaches to presidential studies goes back several decades. In many respects, it concerns the relative effects of individual presidents and the institution of the presidency in American governance. Should we focus our attention on the most volatile part of this equation -- the personality, character, and style of each incumbent? Or should we concentrate on the more tangible and concrete

dimensions -- such as the organization and operation of the White House, the Executive Office of the President, along with the many agencies embedded in it, and/or the many organizations which constitute the "line" side of the executive branch?

Any attempt to pursue an integrative approach will immediately involve us in two tasks. First, we as analysts have to devise ways of relating what we know about presidents generally to the organization and operation of the formal apparatus. Second, we have to examine how individual presidents adapt to their own needs and engage the apparatus which they inherit with incumbency. In neither case have presidential studies achieved a high degree of rigor. However, we have spent more effort on the second task than on the first. Indeed, we have not explicitly addressed the first in any systematic way until very recently.

The Apparatus and Two Gearbox Issues

Broadly, Eisenhower stands as a high-water mark for blending the routinized and hierarchical presidency. Here "routinized" refers to the reliance upon structured procedures for conducting the business of the executive branch; "hierarchical" refers to the practice whereby control is sought through pyramidal and centralized organizations. Since 1960, several presidents either have betrayed ambivalence about engaging the standing apparatus to accomplish their objectives (most notably Kennedy and Reagan) and/or they have employed insufficient routinization and hierarchical structure to accomplish their objectives (clearly the case with Carter).

Two concerns have prompted presidents to be ambivalent about the standing apparatus, routinization, and hierarchical structure. First, along the lines pursued by Richard Neustadt's caveats about presidential leadership, incumbents and those advising them about organizational matters began to prize flexibility and adaptability (Neustadt 1960). Thus, Kennedy ran his administration without cabinet meetings (Hess 1976, 10). He also virtually ignored the National Security Council -- a statutorily mandated cabinet-level apparatus patterned after the British executive's collective approach to foreign and defense policy (Hess 1976, 78; Desler 1981, 267; Nelson 1981, 235). Johnson paid slightly more attention than Kennedy to routinization and collective bodies (Hess 1981, 106; Destler 1981, 269-71). However, the core of his approach centered on achieving flexibility and adaptability by creating in the White House and the Executive Office of the President a counterbureaucracy which would generate innovative ideas and force departments to respond to presidential initiatives (Redford and Blisset 1981, 204-14; Rose 1976, 46). Nixon became so frustrated with the seeming intractability of the

bureaucracy that two years into his administration he began to pursue the Johnson strategy with a vengeance -- drawing every conceivable issue into the White House and the Executive Office of the President and purging appointees in the departments and agencies who had "gone native" (Nathan 1975).

The second concern which has fueled presidents' ambivalence about the state apparatus and routinization and hierarchical structure became dominant in the aftermath of Watergate. It is one thing for presidents to avoid circumstances in which they, their immediate advisers, and appointees in the departments and agencies become subject to the tendency of career officials to control the agenda, stifle innovation, and expand their empires. This all concerns the gearbox between an administration and the state apparatus that it inherits.

Yet, there is another gearbox at issue here -- the one that connects the president with his ever-expanding apparatus for controlling and guiding the entire state apparatus. As John Hart (1987) has argued, the White House and the Executive Office of the President gradually have taken on the size, complexity, and moment of a "presidential branch" of government. Similarly, Hugh Heclo (1984) has identified a "public careerist" culture among the appointees to line departments and agencies. Operating as a surrogate for the type of permanent mandarin found in other advanced democracies, this culture produces individuals well schooled in the ways of Washington and just as capable as not of identifying and pressing goals which comport neither with what the president wants nor the permanent bureaucracy will readily embrace.

Even while disentangling their administrations from the imperatives pressing down from the state apparatus and inertia of bureaucratic routinization and hierarchy, presidents since Nixon have wrestled at least as much with their own presidential branch. They want to devise ways of staffing it and organizing it so that it can help them master the rest of the executive branch. However, they frequently have become painfully aware that rise of the presidential branch poses two dangers -- either that of a monster which plays into its "master's" weakness for imperialization or a leviathan which pulls him apart in a death by a thousand conflicting agendas.

Presidents in the post-Nixon era have achieved only limited success in coping with the second gearbox issue. Ford implemented a spokes-in-a-wheel design for the White House which placed considerable value on "multiple advocacy" (Porter 1980; see George 1980 for full discussion of multiple advocacy). Here White House advisers enjoyed relatively equal access to the Oval Office. As well, the president encouraged cabinet secretaries to resolve as many problems as possible among themselves so that he and his staff would be able

to focus attention on the major issues of the day. However, Ford modulated his approach and encouraged the use of coordinative structures -- such as the Economic Policy Board -- to midwife multiple advocacy among cabinet secretaries.

Carter pushed the Ford model too far. He refused to appoint a chief of staff until summer 1979; he allowed his White House Staff to function virtually without any routinized procedures and hierarchical structure; he let his cabinet secretaries select appointees for top departmental positions with practically no review in the White House; and he used ad hoc and overly inclusive meetings whenever trying to develop agreed administration positions (Campbell 1986).

Reagan went through three phases. The first and mostly successful stage corresponded with the first term. It involved a modified spokes-in-a-wheel format in which James A. Baker kept government business running on schedule and managed legislative strategy; Edwin Meese III served as Reagan's ideological conscience; and Michael K. Deaver nurtured the president's ego and public image. It also entailed very close scrutiny of nominations of sub-cabinet appointees by the White House and an elaborate system of "councils" of cabinet secretaries which -- while very constricted by the budgetary discipline imposed by the administration -- worked together on the development of administration initiatives. Reagan's second phase lasted from 1984 to 1986 and saw the imposition of hierarchical direction by James A. Baker's replacement -- Donald Regan -- and a marked de-emphasis of cabinet councils. The backlash to Regan, capped by the events of the Iran-Contra Affair, led to his replacement by Howard Baker -- the former Senate Republican leader -- and, ultimately, Kenneth Duberstein -- a Republican operative and former assistant to the president for congressional liaison. Both of the latter managed to run the White House in a nonhierarchical way and neither incurred wrath either inside or outside the Beltway. However, 1986 to 1988 hardly constitutes a period of activist or interventionist leadership.

Bush obviously encountered a great deal more difficulty than he perhaps bargained for in the gearbox between himself and his administration (Campbell 1991). This owed significantly to his selection of John Sununu as chief of staff. First, this move meant that others who might potentially prove tremendous assets in the White House -- such as Robert M. Teeter and Craig Fuller -- ended up not joining the administration. They believed that Sununu would not operate as a team player. Second, it preordained that the administration would not run on the basis of multiple advocacy. Sununu clung to deep ideological commitments which he did not readily concede; he managed in an ad hoc and personalistic

fashion; and he brought to the West Wing virtually no experience either in how Washington functions or how a president might consolidate and maintain his electoral appeal. The immediate post-Sununu period saw a flurry of activity associated with the gearbox between the president and the administration.

The Limits of Institutionalism

Undoubtedly, some readers will believe that they have seen some relationship between presidents' experiences with gearbox problems discussed above and presidents' personal styles. Yet, such connections run into increasingly stiff opposition from within mainstream presidential studies. This comes through very clearly in several chapters of a recently published mid-term assessment of George Bush. Most scholars writing in that volume maintain that institutional factors -- especially those associated with divided government -- do most of the work in explaining the organization and operation of the Bush administration.

For instance, Charles Jones takes issue with the premise that presidents must act decisively and energetically the minute they assume office (Jones 1991, 52). He notes that even James P. Pfiffner -- the scholar who has defined in the clearest terms the hitting-the-ground-running imperatives faced by presidents at the outset of an administration (1988) -- absolves Bush for not adopting an aggressive approach to his domestic agenda on the grounds that Bush lacked a mandate for anything besides maintenance of the Reagan legacy (Pfiffner 1990, 70). George C. Edwards III -- looking at the factors which maintained Bush's approval ratings at such heights -- asserts that voters, not analysts, determine what they expect from presidents (1991, 20, 26). Writing before the economy became an issue in fall 1991, Edwards underscores the extent to which voters were not factoring in consideration of whether Bush's performance matched the challenges which many analysts believed the nation faced. The issues which exercised such analysts had not become salient to the electorate.

In the volume mentioned above, Barbara Sinclair takes on even more directly Bush's critics' hand-wringing over whether his personality lends itself to the challenges faced by the nation implying that they do not understand democracy:

The behavior of the president and members of Congress is shaped and constrained by the context in which they act; what commentators interpret as playing politics or a lack of backbone is, given the context, often the best strategy.... That is, it is the best way to advance the politician's goals of policy results and electoral success. While

some may argue that the goal of electoral success is disreputable and that what is needed are principled public officials indifferent to being voted out of office, this argument ignores the crucial role that wanting to be reelected plays in keeping public officials responsive to the wishes of the electorate that chose them (Sinclair 1991, 155).

Even though authors such as Jones, Edwards, and Sinclair carefully style their treatments of Bush as specific to the conditions of his administration, their analyses comport with a two-fold transformation in presidential studies which begins at least with the Reagan administration. The transformation has addressed a simple problem: How can we say nice things about a president who does not fit conventional standards for presidential performance but nonetheless attains high levels of popular support? It has concluded, first, that a president's continued support among the electorate is the only valid measure of his performance and, second, institutional factors rather than personality, style, and character determine what a president ends up doing. I will maintain that both dimensions of this transformation reflect more the degree to which the discipline had become mesmerized by the approval ratings and electoral successes of Reagan and Bush than a scientific breakthrough.

Much of the ongoing debate centers on how we ascertain whether a president has led competently. Terry Moe accurately identifies one strain in the profession which gives, in his view, too much attention to "neutral" competence (1985). This criterion for presidential performance relates to the first of the gearbox issues considered above -- the extent to which an administration effectively engages the permanent state apparatus and gets it to do its bidding. Moe correctly notes that this concept can take on romantic proportions in the minds of some analysts.

More recently, Colin Campbell and Margaret Wyszomirski amplify this view by asserting that the concept of neutral competence remains ill-defined by those who regret the underutilization of the state apparatus which has characterized recent administrations' encounters with the executive branch:

Their critique might simply be a lament for a neutrally competent ...[civil service] that never existed -- one in which officials offered their advice and conducted the affairs of state without any personal commitments of a partisan or political nature (Campbell and Wyszomirski 1991, 15).

This does not mean that presidents have nothing to gain by tapping into the expertise and institutional memory of the permanent bureaucracy. However, they must engage it or ignore it for the right reasons. Administrations that dismiss the capabilities of the executive branch become error prone about small things which, over time, can add up to major crises. Yet, administrations which defer too much to the ongoing bureaucracy and let it ponder interminably the myriad options will lose sight of the big picture and doom themselves to electoral defeat. In either case, such administrations would betray a misunderstanding of the meaning of "neutral." It should not imply that public servants have abandoned all ideas and convictions about the range of possibilities suggesting themselves in a given issue. It simply should convey the notion that their career commitments prescribe a capacity to adapt what they supply -- expertise and knowledge about the system -- to the needs of their new principals each time the executive branch passes from one party to the other.

The term *neutral* does not connote the provision of value-free analysis. It relates rather to the degree to which career officials adjust their agendas and priorities in ways which reflect changes in the political leadership. In this regard, recent work by Joel D. Aberbach -- based on surveys he has conducted with Bert A. Rockman -- suggests considerably more adaptability of the career public service to the Reagan/Bush agenda than we might expect. Aberbach's research serves as a strong corrective for those who continue to maintain that the bureaucracy remains enamored of the expansive welfare state and hostile to all attempts to constrict it (1991).

Moe's earliest contribution to the debate over assessment of presidents focused on the first gearbox issue -- that centering on the relationship between an administration and the standing state apparatus. He posits a clear linear process whereby successive presidents have turned a blind eye to or positively advanced the de-institutionalization of the executive branch (1985, 258). They have done so because they do not prize what the standing apparatus has to offer. Instead, they have placed the highest value on "responsive competence" (p. 239). They advance this by centralizing their institutional resources in the White House and politicizing the federal bureaucracy -- both of which strategies provide them with an enhanced capacity to "circumvent established organizations and vested interests" (p. 244-45). Insofar as the desire to maintain their political support motivates them, evaluation of presidents from the standpoint of their engagement of the state apparatus becomes -- at best -- a secondary concern.

Moe's 1985 work provided important theoretical bearings to a growing body of scholarship which has reflected the ambivalence of administrations toward the

bureaucracy. Thomas Cronin has termed such works "presidentialist" in that they place a great deal of stock in the capacity of presidents to override "complexity, diversity, jurisdictional disputes, and bureaucratic recalcitrance" without becoming bogged down trying to master and restructure the state apparatus (1980, 248). Bert A. Rockman -- writing a year before Moe -- cautions that "policy" competence should form an integral part of presidents' pursuit of "political" competence (1984, 195). Policy competence allows presidents to maintain an historical perspective on their options and the likely consequences of their choices. It also gives rise to presidents' activation of their "managerial capacity" whereby they can "move decisions along, effectively coordinate them, and have a sufficient information base to make them." Thus, even before Moe's articulation of the de-institutionalization thesis, some scholars had raised serious doubts about the degree to which presidents had solved the first gearbox problem by trying to override the state apparatus.

In two more recent works, Moe adjusts his nomenclature so as to accommodate his view that -- whereas the bureaucracy has de-institutionalized -- the presidency itself has institutionalized (1990, 1993). Thus, the strategies -- centralizing of power in the White House and politicizing the bureaucracy -- which reduce the autonomy of the state apparatus also increase the institutional leverage of the presidential branch (1990, 8). Yet, Moe does an inadequate job of defining the boundary between presidential and bureaucratic institutions. For instance, he gives the National Security Council (NSC) as an example of the "agencies" of centralization in the institutionalized presidency.

In fact, the NSC originally took shape under Truman as a U.S. attempt to routinize -- and, therefore, make less subject to the management styles of individual presidents -- interdepartmental consultation on national security issues (Nelson 1981, 230). Since its creation in 1947, the centralizing function of the NSC has ebbed and flowed, depending on factors such as each president's interest in foreign affairs, the forcefulness of key participants -- including the secretaries of defense and state and the director of the CIA -- and their mutual working relations and relative power, and the approach of the principal White House adviser responsible for advising the president on national security affairs (George 1980; Nelson 1981; Destler 1981; Greenstein 1982; Mulcahy and Kendrick 1991). To assert that the NSC has worked a consistent and continually intensifying centralizing function would ignore dramatic differences in its trajectory from president to president. It would also obscure the fact that forceful presidents with strong aptitudes for and interest in foreign affairs would as likely ignore the NSC apparatus as engage it. Indeed, as

was the case with Kennedy, they might consciously eschew institutionalization in favor of flexibility (Allison 1971).

One finds an element of ambivalence toward the role of the state in Moe. This fits well within the non-statist tradition of American politics (Rockman 1984, 49-52). It also represents the prevailing stance of presidential studies. It does this insofar as it accepts as a contextual given that presidents should confine their efforts to objectives which they can actually attain -- assuming recurrent paralysis owing to divided government. Along the line pursued by Olsen (1983, 1988), Moe correctly implies that Franklin D. Roosevelt -- through the dual effect of his leadership during the depression and WWII -- bloated expectations for presidential leadership. Without employing Olsen's concept explicitly, Moe argues that Roosevelt turned the presidency into a supermarket for the great necessities:

...all presidents would be held responsible for addressing every conceivable social problem -- however gargantuan ... intractable ... far removed from the president's actual sphere of power -- and they would be expected, through legislative leadership and executive control of the administrative apparatus of the government, to take action (Moe 1990, 8).

Yet, Moe fails to accommodate the possibility that models of the state change in their appeal over time. We can no more argue that Roosevelt's approach did not fit his times than we can affirm that it should have prevailed during the relatively cautious and nonexpansive administrations of Eisenhower, Ford, Carter, or Bush. Two difficulties arise if students of the presidency fail to adjust their models to reflect the dominant expectations for the state. First, they can become bound within a specific methodology. Second, they can focus on one dimension of presidential politics to the detriment of their coverage of others.

As we have seen, the 1980s became a definitional decade in advanced democracies. Political executives in virtually every system -- even left-leaning governments such as Australia's -- sunk or swam on their ability to adjust the contours of the state to the more constricted circumstances of the increasingly globalized economy and public disenchantment with the fiscal and regulatory burdens of the interventionist, regulatory/welfare-oriented state. We have noted that public choice theory soon migrated from the U.S. -- where it first emerged -- and actually found more fertile soil in parliamentary systems -- where usually the regulatory/welfare state had become more entrenched than in the U.S. and the political institutions proved more

malleable to decisive leadership (just as they had during the expansion of the interventionist state).

In fact, the lessons from the unintended consequences of simplistic adherence to public choice have now had considerable time to sink in. For example, revolts in two parliamentary systems recently have ousted prime ministers who had spearheaded public choice approaches to governance. Now their replacements -- Britain's John Major and Australia's Paul Keating -- have attempted to salvage each government's political fortunes by, respectively, promoting a "citizens' charter" of baselines below which government services will not fall and "creative" guidance of the market economy. The more governments retreat from the neo-liberal stances which prevailed in the 1980s the more we should begin to anticipate the possibility that public choice will join Keynesian economics in the rogues gallery of theoretical approaches.

This will not at all challenge the gradual adaptation of the discipline to the rigors of formal theory. Formal theory has been the streetcar whereby public choice disseminated through the profession. However, it can just as easily serve scholars operating from different assumptions about the nature of the state. Moe accurately observed that public choice gained appeal as the "new economics" of organizations emerged as a focus of scholarship in the 1980s. This centered attention on the elements of political behavior which follow the conditions of "voluntary actors in the market place" and "the conditions under which they will find it mutually beneficial to cooperate -- that is, to organize their behavior" (1993).

If we have a return -- some might prefer "relapse" -- of interest in and expectations of the interventionist state, overly transactional perspectives might fail to capture forms of expectations for political executives which tap more than public support of the integrity of the market. The demands from rational choice scholars that we focus analysis on issues amenable to formal theory go beyond simply limiting the compass of presidential studies to institutional factors. They also place a straight jacket on an approach capable of enhancing our investigation of many other dimensions of the presidency. They do so by defining formal theory so narrowly that it appears only applicable in market-like, transactional circumstances. In this regard, we would do well to remember that Robert Dahl -- a pluralist -- presented cogent formal theories about democracy before the major public choice works first emerged (Dahl 1956).

Management Style and the Second Gearbox

Much of the definitional thrust of executive leadership through the 1980s focused on the first gearbox problem --

the interface between administrations/governments and the bureaucracy. The second gearbox problem -- that associated with the interrelationship between chief executives and their own administrations received little attention.

Thus, we forgave a multitude of lapses on the grounds that the main game concentrated on the effort to control bureaucracy. We excused Ronald Reagan's lack of engagement in the day-to-day affairs of state and vulnerability to manipulation by the White House staff as part of painting with broad strokes. The president had become so detached from the management of his own administration by his second term that he took no role in consultations which led to James A. Baker, the first-term chief of staff and Donald Regan, the first term treasury secretary, switching jobs (Schieffer and Gates 1989, 192-196).

Only the Iran-contra Affair swung observers around to contemplating the possibility that leadership in the new context of the institutional presidency required more than the president clinging to a few immutable ideas about the world and his aides filling in the details. The Tower Commission minced words neither in its assigning much of the blame for emergence of the scandal to Reagan's "management style" nor its attributing to Donald Regan responsibility for not living up to his control-oriented approach to running the White House (1987). Of Reagan, the commission said:

The President's management style is to put the responsibility for policy review and implementation on the shoulders of his advisors. Nevertheless,...the President should have ensured that the NSC system did not fail. He did not force his policy to undergo the most critical review of which the NSC participants and the process were capable. At no time did he insist upon accountability and performance review (p. 79-80).

Regarding Regan's role, the commission noted:

Mr. Regan also shares in this responsibility. More than almost any Chief of Staff of recent memory, he asserted personal control over the White House staff and sought to extend this to the National Security Advisor. He was personally active in national security affairs and attended almost all of the relevant meetings....He, as much as anyone, should have insisted that an orderly process be observed....He must bear primary responsibility for the chaos that descended upon the White House...(p. 81).

A committee of privy councillors documented in indisputable terms the degree to which Mrs. Thatcher's ad hoc utilization of cabinet machinery had contributed to the U.K. not responding in time to prevent the Argentinean invasion of the Falkland Islands (Franks Report 1983). Yet, she was quick to cover her lapse with jingoism and a decisive -- though almost foolishly hazardous -- campaign to recover the islands. Eight years later, George Bush virtually replicated Mrs. Thatcher's negligence by letting the response to Saddam Hussein's bellicose threats fall victim to in-fighting and indecisiveness among his principal foreign affairs advisers (Berman and Jentleson 1991, 118-21; Campbell 1991, 209-10).

The Reagan, Thatcher, and Bush experiences in the foreign policy field suggest the degree to which we should exercise caution in making generalizations about the aptitude that systems display for coordination between chief executives and their advisers and cabinet members. We can certainly anticipate circumstances -- either under presidential or parliamentary government -- in which chief executives achieve a high degree of effectiveness or very little. For instance, in a recent paper introducing the central themes of a Brookings Institution conference on the institutional effects of the two systems, Kent Weaver and Bert A. Rockman point up conditions under which parliamentary systems can be either more or less conducive to strong executive leadership. At one point in the paper, they note that parliamentary systems often allow a "greater potential for concentrated power if a prime minister has a firm majority in the legislature and surrounds him (or her)self with weak and compliant ministers..." (Weaver and Rockman 1990, 9). Later, they probe the opposite possibility: "...cabinet ministers in parliamentary systems may (unlike those in the U.S.) have political standing and power independent of the prime minister, making it difficult to challenge them in their own domains" (Weaver and Rockman 1990, 20-21).

Similarly, Peter Hennessy has recently compared the Overseas and Defence secretariat of the British Cabinet Office unfavorably with the U.S. National Security Council staff on the grounds that the former relies too heavily upon secondees -- mostly from the Ministry of Defence and the Foreign and Commonwealth Office, and the military (Hennessy 1991, 312). He argues that Overseas and Defence should be able to develop its own career structure and bring in people from outside government. In fact, the NSC staff relies heavily on secondees as well. Certainly, it does not have a permanent staff structure. And, its track record of bringing in secondees from outside government has not always proven successful.

On domestic fronts, political scientists have bent over backwards to give chief executives the benefit of the

doubt whenever seeming dysfunctions emerge in the management style. A recent edited collection containing ten assessments of Bush by twelve students of executive leadership made several references to John Sununu, all of which characterized his style as confrontational (Campbell and Rockman 1991). Yet, the authors proved loath to attack directly the "lightening rod" theory whereby analysts had concluded that George Bush was simply using Sununu to serve as a focal point for criticism of the administration's tougher stances on the domestic front. Analysts, apparently, found it hard to disentangle the rhetoric early in the administration heralding a new cooperative era with Congress and the evidence of deepening hostility between some White House officials and congressional leaders.

For instance, Paul Quirk -- whose chapter proved broadly critical of Bush's record -- took pains to distance Bush from Sununu's approach and that of the equally confrontational Richard Darman:

...Bush was well known and generally liked in Congress. To maintain his relationships and show respect for the institution, he came to Capitol Hill for personal visits with congressional leaders. In a period of a few months he invited every senator and most of the representatives to the White House. Through countless phone calls and handwritten personal notes, he kept in touch with members of Congress of both parties and a host of other Washington acquaintances. Two of Bush's senior aides...Sununu and...Darman...sometimes interfered with the good feelings by taking positions in a manner that members of Congress found arrogant. But after the incessant hostilities of the Reagan years, members of Congress and especially Democrats appreciated the atmosphere of harmony (1991, 75).

However, Quirk's description of Bush's substantive moves vis-à-vis Congress corresponds more with our image of the machinations of Sununu than that of harmony in executive-legislative relations:

...Bush failed to take positions and adopt strategies well suited to a cooperative approach...Bush vacillated -- and so was, by turns, flexible and rigid about everything. At times he quickly gave up whatever was needed to get a deal...At other times, Bush refused to make significant concessions...Some of his occasional rigidity was perhaps a matter of acting tough just to show his capacity to do so -- a way of dealing with the "wimp" factor (1991, 75-76).

Either Bush had ceded responsibility for domestic policy to the volatile Sununu and deferred to him because he wanted to stay above the fray or he consciously pursued a duplicitous strategy toward Congress. Whatever the case, a disjunction existed between the administration's putative approach to domestic politics and its actual performance which went largely undetected until the collapse of Bush's approval ratings in fall 1991. This, of course, occasioned Sununu's departure from the West Wing and an effort to revamp the White House.

Focusing on administrations' relationships with the permanent bureaucracy also has lulled observers of political executives in parliamentary governments into giving inadequate attention to the dynamics by which prime ministers have interacted with their political advisers and cabinet colleagues. George Jones -- rejoining an old debate which he has been conducting for several years now with Patrick Weller (Weller 1983; Jones 1983; Weller 1985) -- defended Mrs. Thatcher to the end as not running her cabinet in a "presidential" way (Jones 1991).

Weller has led a growing number of students of executive leadership in Westminster systems who assert that the complexity of modern government and the availability of electronic means for making direct appeals to the public have introduced an era of convergence between the devices employed by prime ministers to get what they want from their governments and those used by presidents with their administrations. In this context, he has construed the growth and specialization of staffs reporting directly to prime ministers as fitting within the evolutionary development of Westminster systems toward more central guidance by the chief executive.

Jones denies such an evolutionary process. Even in the case of Mrs. Thatcher, he argues, cabinet maintained the potential -- assuming the support of Conservative MPs -- to remove her if her approach appeared so much at variance with the conventions of British government. However, Jones's case only takes us so far. In fact, the 1990 leadership challenge which unseated Mrs. Thatcher came initially not from within cabinet but from a group of disenchanting MPs who had rallied around Michael Heseltine whom Mrs. Thatcher had forced from cabinet more than four years earlier. That cabinet did not register their concerns with Mrs. Thatcher until she lost the confidence of her party members in the House of Commons hardly means that the seeds of her own demise had not begun to germinate long before. As *The Times* of London had observed as early as 1984, Mrs. Thatcher had neglected cabinet consultation and manipulated ministers to an unprecedented degree:

Ministers increasingly voice concern about the way key decisions are taken by Mrs. Thatcher and small groups of ministers

without reference to the full Cabinet – a practice which they say has contributed to failings in the presentation of policies. One minister said privately last week that Mrs. Thatcher probably has used Cabinet less than any prime minister since the war. Some ministers are calling for a return to genuine cabinet government (*The Times*, March 5, 1984).

Scholars' assessments of Canada's Brian Mulroney present another case in which they have paid too little attention to a chief executive's ability to handle the second gearbox problem. When Mulroney took power in 1984, scholars simply had to consult those who had worked closely with him as leader of the opposition to find that he had abdicated management of the shadow cabinet to Erik Nielsen – a wily parliamentary strategist (Campbell 1988, 322-328). Part of Mulroney's reliance upon Nielsen stemmed from the need to build bridges with the right wing of the Progressive Conservative Party after a fractious leadership race in 1983. However, during Mulroney's year as leader of the opposition he demonstrated virtually no aptitude for the collective decision-making processes so central to cabinet systems of government. The pattern had established itself so firmly that the leaders of Mulroney's transition team enshrined the principle that the prime minister would require a deputy who would actually run cabinet. This practice even survived the transfer of the deputy prime ministership to another minister in 1986 when Nielsen left the cabinet in the wake of a conflict of interest scandal involving a cabinet colleague.

Through his first term (1984-88), Mulroney mostly received the benefit of the doubt in scholarly assessments of how he was handling the second gearbox problem. Two points emerged. First, a government trying to reverse the expansion of the state should limit the opportunities for officials – either through their ministers or through direct access to collective decision-making bodies – to fight for their corners (Campbell 1988, 324). We would expect, thus, that Mulroney's government – as was the case with Thatcher's – would rely less on cabinet committee deliberations than had the more expansive and statist Trudeau governments. Second, the prime minister clearly felt awkward with formal cabinet-level meetings and, in fact, rarely attended such sessions. Instead, he preferred bilateral discussions with individual ministers about specific problems – often conducted on the telephone. As Peter Aucoin asserted, this did not mean that he did not want teamwork in his cabinet. It simply meant that he would limit his direct participation in collective deliberations and, therefore, place less emphasis on collegial dynamics as he sought to establish teamwork:

...Mulroney's leadership style [in contrast with Trudeau's] is transactional rather than collegial. His preference is to deal with individuals on a one-to-one basis rather than on a collective basis. The logic here, of course, is that this transactional style better facilitates the negotiation of compromises among differing points of views than does the collegial process, where the checks and balances more readily lead to stalemates if different points of view are strongly held (Aucoin 1986, 18).

Three difficulties have presented themselves with this approach, and each has persisted throughout the Mulroney years. First, the transactional approach has always operated in conjunction with the collective machinery. This has proven a constant source of confusion and tension. The emphasis given to the transactional approach as against the collective one ebbs and flows notably depending on the perceived electoral vulnerability of the government. In other words, it will adopt more collective approaches when it becomes necessary to mobilize cabinet in a major effort to pull the government out of especially threatening predicaments. Second, players – cabinet ministers and senior officials alike – never know for sure which rules will prevail in the resolution of specific issues. Each return to collective processes has involved significant changes of the machinery with which players take some time to familiarize themselves. Further, some ministers will under any circumstances retain – due to personal ties with the prime minister or their importance to the coalition of interests which sustains the government – the ability to short-circuit collective processes. This stems from that fact that the prime minister's fundamental approach still invites special pleading outside the confines of cabinet-level bodies.

Third, the consequences of a dysfunctional collective decision-making process have proven somewhat more severe for the legitimacy of Canadian government under Mulroney than they did for that of the U.K. government under Thatcher. A very strong representational imperative weighs down on cabinet operations in Canada (Campbell 1985). This owes largely to the fact that disciplined collective government fits much less well in a federal system attempting to resolve differences emerging from a highly fragmented society than in a unitary system coping with pressures in a relatively homogeneous society.

In Canada's case, the representational imperative has operated behind two especially distinctive patterns in cabinet building in the federal government – cabinet has swelled to some 40 members and the cabinet committee system has become both intricate and highly institutionalized. Obviously, prime ministers – especially

ones trying to trim nonproductive elements of the state -- should take any opportunities which present themselves for rationalization of the cabinet machinery. However, all indications suggest that the apparatus cannot long be ignored or overridden without serious consequences both in the coordination between ministers and the legitimacy of the government's decisions.

We have focused above on instances in which analysts have suspended their disbelief in their critiques of chief executives' handling of the second gearbox problem. Scholars have done so because they have seen the key task of the chief executive as gaining control of the standing bureaucracy. However, one case presents itself in which the received wisdom -- informed by highly statist social theory -- fails to give due credit to a prime minister. Here Australian social theorists have argued that Bob Hawke and his cabinet became captured by a segment of the career public service which actually had renounced expansive views of the state (Pusey 1991, 154-55). This interpretation sells short the contribution of the political executive to the radical reforms which occurred under Hawke. To be sure, a symbiosis took place between the right of the Labor party and top officials in the central coordinating agencies of the public service. However, this symbiosis would not have held together as long nor accomplished as much without Hawke's aptitude for and commitment to collective cabinet processes (Campbell and Halligan 1992).

The various cases cited in this section suggest that analysts could profitably spend more time assessing how executive leaders handle the second gearbox problem -- the fact that presidents and prime ministers often have to worry as much about their relationship with their "own" as about that with the permanent bureaucracy. Some authors seem to imply that it amounts to an area of executive leadership which takes care of itself. For instance, Terry Moe bases a significant proportion of his portrayal of the potential institutional autonomy of the presidency on the assertion that it does not experience "collective action problems" and functions as a "team" (1992, 50-51). In fact, the internal capacity of administrations and governments for harmony often falls well short of what we might expect.

A Pinch of Personality?

During the era in which institutional factors have taken center stage, we have seen that presidency scholars have become especially hesitant to probe personality and the effects it might work on presidential performance. This is ironic. Whatever the force of institutional factors, we might expect U.S. administrations -- given the presidency's emphasis of monocratic leadership -- to prove somewhat more susceptible to differences in the

personalities of incumbents than would cabinet governments. The latter -- however they might have strayed toward the monocratic format -- still call upon chief executives to win the approval of their cabinet colleagues. In addition, prime ministers traditionally have had to pass through a stronger socialization process in "insider" politics (Rockman 1991, 53-54). In fact, we might attribute some of Brian Mulroney's difficulties discussed above to his lack of previous exposure to collective decision-making processes.

In comparison to the presidency literature -- which supplies an abundance of studies centering on the personalities of presidents -- we find relatively little work which explicitly examines prime ministers from the standpoint of personality. This does not mean that scholars do not have available biographies of prime ministers which develop profiles of incumbents' personalities and make links between these and their performance in office (Young 1989; Radwanski 1978; Gwyn 1980; d'Alpuget 1982). However, political scientists normally leave this type of work to journalists who have both the luxury of knowing a great deal more about incumbents' foibles and having to adhere to somewhat relaxed standards for analysis. Some expressly psychoanalytic works have appeared which reviewers have believed to make strained connections between prime ministers' personalities and performance (Little 1985; Anson 1991). Political scientists' efforts to incorporate some material which relates to personality have tended to employ this only within a context in which the leadership style of a prime minister serves as an especially engaging topic (Campbell 1980; Weller 1989).

I have already cited Barbara Sinclair's strong caveat against the introduction of personality issues in the assessment of presidents (1991, 155). Her position fits within the institutionalist perspective which broadly views presidential behavior as stemming much more from the exigencies of office and the maintenance of electoral support than from personality. Moe follows a similar line of argumentation but adds considerations associated with the difficulty of operationalizing personality factors.

Differentiating presidential behavior and the presidency, Moe asserts that -- with the institutionalization of the latter -- the former has become subject to all the individuals that make up the presidency "collectively" and, therefore, "an institutional phenomenon, not a personal one" (1990, 7). He adds, however, that if we did include personality factors in our models this would immensely complicate our analysis:

The explanatory focus is no longer on general issues of organization and structure, but rather on the behavior of a single person in the full flower of his uniqueness (Moe 1990, 10).

Moe, in fact, believes that our research can take into account what we know about presidents' personalities. This type of work can provide "the empirical foundation that promotes sound judgments about what presidents have actually done and why" (Moe 1993). However, personality does not lend itself to the "coherent theories with the degree of generality and explanatory power" which we seek in rigorous analysis.

We -- as a sub-discipline -- must disentangle what scholars such as Sinclair and Moe have argued. On the one hand, they seem to be asserting that personality lacks salience because institutional factors largely cancel out its effects. On the other, they seem to base part of their reluctance to include personality in analysis on the grounds that its operationalization presents too many difficulties. The first rationale would absolve us of any responsibility to continue to consider personality as a major element of presidential behavior. The second, however, should prompt at least some of us to redouble efforts to develop theoretically cogent ways of incorporating personality factors in our analyses.

A simple illustration should help make the point. During the 1988 primaries, George Bush became subject to a great deal of criticism for not providing a "vision" which would guide his presidency. During the 1988 presidential campaign, he embraced a strategy of attacking his opponent with innuendo and ridicule rather than promoting a discussion of the issues which would have provided him with a mandate. Yet, after the election the spin doctors got to work and built Bush up as an interventionist president who -- now that he no longer had to work in the shadow of Ronald Reagan -- would energetically pursue his own (still undefined) domestic agenda (Campbell 1991, 194-97). As analysts, how much should Bush's record in public life, and campaign performances during the primaries and the election, have informed our efforts to project his presidential style? Or, how much should we have listened to the spin doctors who, in the case of one observer, asserted that Bush had transformed himself from a "pit bull" to a "statesman far removed from the fray of the campaign"? Were we to have believed that George Bush did not, after all, reveal a dark side to his personality during the primaries and the election campaign, or, if he did, that being president meant that he could put it all behind him?

Erwin C. Hargrove has done more than any other scholar to mediate between students of the presidency focusing on institutional factors and those seeking to introduce a systematic consideration of personality (1993). He argues that a great deal of presidential behavior, in fact, does not fit neatly into what we might expect from incumbents' institutional context. Such behavior includes how presidents present themselves to mass audiences, how they devise their initial policy

agenda, how they manage decision processes, and the ways in which they persuade independent power holders in the policy arena.

Hargrove registers a strong and, I believe, legitimate question about how much we can attribute to the institutional constraints of presidents. History, he suggests, presents us with several cases of presidents who engaged in behavior stemming more from their "internal vulnerabilities" than from their objective circumstances (1993). Due to the monocratic structure of the American executive, such tendencies when present pose a greater threat than they would under collective formats for political leadership. Hargrove asks essentially how the system protects us from "inappropriate ego-defensive actions" especially given the "great scope for autonomous action in our 'elective monarchy'"? He does not call for psychoanalytic approaches -- we would normally lack the evidence necessary to adequately probe our hypotheses. Rather he urges us to use biography to identify repetitive patterns in individual presidents' behavior and the contexts in which these tend to occur.

In the literature, the issue of personality links most strongly with the assessment of how presidents will handle the second gearbox problem -- their relationship with their White House staff and the cabinet. We only have to consider the clearer instances in which a dysfunctional match emerged. For instance, Bob Haldeman's hierarchical White House would not have achieved such notoriety had it not fed Nixon's passion for control. Similarly, Donald Regan would not have arrogated to himself so much power had not Ronald Reagan slumped in the second term from a relatively detached to alarmingly disengaged president. And, only an incurable optimist like Reagan would have placed so much trust in one adviser.

The *prima facie* evidence suggests, thus, that presidential character and style work strong effects on the organization and operation of presidential staff and the cabinet. Yet, presidency scholars have strived with only modest success to find out how exactly the personal qualities of presidents link into organizational and operational issues surrounding administrations. Virtually everyone accepts the existence of a relationship. However, the exact functioning of the gearbox connecting one to the other has largely escaped rigorous empirical inquiry. First, let us examine what we mean by character and style.

In his treatment of presidential personalities and the management of the foreign policy process, Alexander George discusses at length the need to tailor the design and management of the national security process to each individual president:

...each president is likely to define his role in foreign-policy-making somewhat

differently and to approach it with a different decisionmaking and management style. Hence, too, he will have a different notion as to the kind of policymaking system that he wishes to create around him, feels comfortable with, and can utilize (George 1980, 146).

Presidential self-knowledge rests at the heart of this process and it does not always get it right. As George recounts, Richard Nixon tried at the start of his administration to encourage the type of multiple advocacy that had worked so well under Roosevelt and Kennedy. But he soon abandoned the system in favor of "by far the most centralized and highly structured model yet employed by any president (George 1980, 155)." If Nixon had been able to acknowledge some of his deepest character flaws, he would have recognized that his embracing this highly formal model would simply exacerbate his tendency to seek excessive control.

In my own research, I have pointed out the degree to which Carter overburdened himself by not seeking structures in the White House and cabinet that would serve as checks for his passion to master every conceivable detail before making a decision (Campbell 1986, 60-63, 83-84). As we have noted above, observers have told us that Ronald Reagan gave virtually no personal reflection to the swap between James A. Baker and Donald Regan that saw the latter become White House chief of staff (Schieffer and Gates 1989, 193-195). Had he given some time to the question, he might well have seen that Regan would arrogate power in areas where Reagan -- either because of his view of government or declining interest in his job -- had left a vacuum.

The frequency with which presidents seem to lack adequate self-knowledge comprises just one dimension to the issue of personality. More fundamentally, the issue takes us into the difficult task of defining the parameters of presidential character. A bitter debate broke out among political scientists in the 1970s in response to James David Barber's classification of presidents according to four personality types -- active-positives, active-negatives, passive-positives and passive negatives (Barber 1972, 1977; George 1974; Qualls 1977). According to Barber, active presidents come across as "human cyclones" with boundless energy while passive presidents evoke memories of Calvin Coolidge -- who indulged himself in 11 hours of sleep each night plus a nap in the afternoon. Positive presidents find a great deal of fun in their job while negative ones at best take grim satisfaction in tasks well done.

Most of Barber's problems with the rest of the political science community stem from his assertion that he has developed a predictive model of presidential

performance (Barber 1977, 213, 218). At best his model simply helps us understand the psychological factors associated with presidential performance. We can detect some early warning signals of potential behavior. However, we must continually guard against the expectation that a president with particular psychological traits will behave in a specific fashion.

When we turn down the volume in the heated scholarly exchange, however, we find some common ground regarding presidential character. For instance, Richard Neustadt faults Barber for appearing to force presidents into the boxes of his paradigm without sufficient accommodation of differences and nuances (1990, 206-207). However, in trying to diagnose what made the presidential experiences of Nixon and Johnson so unhappy, he does agree with Barber's assertion that both embraced the challenges of office with "relatively intense effort and relatively low emotional reward (1990, 206)." Neustadt argues that both presidents' behavior betrayed deep personal insecurity:

Back of their bad grace when things went wrong lay insecurity, or so it seems, a stressful inner turmoil that would go away only when things went right. Apparently both men were in the grip of human hungers they endeavored to appease by being President... (Neustadt 1990, 206).

Neustadt maintains that we should have seen the ways in which Johnson abused his Senate staff and Nixon let Bob Haldeman run his campaign staff as harbingers of how they would approach the presidency. Along the same lines, George attributes Nixon's proclivity for formalistic approaches to decision making as rooted in the fact that he had "developed a cognitive style that enabled him to cope with deeply rooted personal insecurities by adopting an extremely conscientious approach to decision-making." Whereas we might quibble with Barber's labelling (that is, the positive-negative dimension), it appears that just about everyone recognizes that presidents range from those with strong self-esteem to those who, for some reason, betray insecurity. Neustadt characterizes Roosevelt and Kennedy as having the former when he says both "approached the office as their natural habitat and drew security from being themselves (1990, 207)."

This leaves us with Barber's active-passive axis. At first blush, it appears that Barber has focused on a dimension which taps solely the degree to which the president has engaged himself in his job. However, it becomes clear that, in Barber's mind, presidents who put activity together with a sense of efficaciousness maintain perspective -- and, their effectiveness -- while those who do not lose both. The former type of president:

...shows an orientation toward productiveness as a value and an ability to use his styles flexibly, adaptively.... He sees himself as developing over time toward relatively well defined personal goals -- growing toward himself as he might yet be (Barber 1972, 12).

There are two problems with this formulation. First, it seems to assume adaptability and growth during the entire length of office when, in fact, presidents -- especially as the job begins to wear on them -- often become less flexible and more turned in on themselves. In this regard, analysts should always ask questions about a president's resilience. For instance, what would Kennedy have looked like as president in January 1969 after completing two terms and weathering the similar problems associated with Vietnam and civil rights that Johnson faced?

Second, we have to take care to delineate different types of activity. For instance, some presidents become cautious in their second term as they begin to think of how they will stand in the history books. But, Reagan, prodded on by his wife, actually gave greater attention to one policy area when he began worrying about the type of legacy he would leave (Cannon 1991, 507-510). Thus, we saw in his final years a number of overtures to the Soviets which contributed immensely to the end of the Cold War. Activity is often in the eye of the beholder.

This assessment, thus, construes activity as a multifaceted dimension. Presidents like Roosevelt, Kennedy, and Johnson earn high ratings as actives because they seek to establish a *new order* either in domestic or international affairs. Thus, Roosevelt brought in the New Deal and then led the U.S. effort to make the world safe for democracy; Kennedy sought to focus American attention on the "New Frontier" and urged that they "ask not what their country can do for them but what they can do for their country;" Johnson advanced civil rights in this nation more than any other president and pressed the "War Against Poverty."

Jimmy Carter embraced no overarching objectives that called for establishment of a new order. Carter's concerns focused much more on pragmatism. His activity was *executive*. The assumption that "there must be a better way" drove him to probe in exhaustive detail how the government worked and how it might become more efficient and effective.

Ronald Reagan -- whom most would rate as a passive president -- in fact proved to be a *being-there* active. He threw himself massively into the symbolic dimensions of the job. But he took no real interest in the executive functions. Further, the new-order goals he presented -- tax cuts, reduced spending, and increased defense expenditure -- played into the minimalist view of

the state's domestic responsibilities and the anxieties about America's decline as a world power which prevailed at the time. Nonetheless, Reagan utilized and exploited the immense prerogatives of the head of state function -- becoming a sort of high priest of the national liturgy. Ultimately, he turned Americans around so that they felt as good about their country as Ronald Reagan did about himself.

This leaves us with the final type of active president. I have developed more fully elsewhere the view that George Bush -- even if we grant that he might have been a new order active in foreign policy -- exemplified the "*let's deal*" active on the domestic side (Campbell 1991). As the president who never provided the nation with a vision of his domestic goals, Bush eschewed so much as a hint of a new-order activeness. He picked and chose the parts of the executive branch in which he took an interest. He was not an executive president. Even in foreign policy, we have all seen that -- although Bush would not shy away from exercising his prerogatives as commander in chief -- he found it difficult to express to the nation why, for instance, he dispatched such a huge U.S. force to the Persian Gulf. He was not, thus, a *being-there* active.

In linking presidential personality to how incumbents organize and operate their advisory systems, we must look at their management style. Here we refer to the way in which they prefer to do business (Campbell 1986). *Priorities-and-planning* leaders seek an advisory system which will optimize choice and creativity. They will tend to expand White House and Executive Office of the President resources so that they can tap alternate views to those issuing from the various departments and agencies. However, they will not stifle the competition of viewpoints.

Priorities-and-planning presidents will -- minimally--give assignments and/or create units in ways which will optimize choice. They might -- as well -- foster competition between departments and agencies by establishing cabinet councils and task forces that will expose secretaries and their top aides to the purifying fires of face-to-face criticism. Both Roosevelt and Kennedy were *priorities-and-planning* presidents. However, neither took their approach so far as to establish regularized mechanisms for engaging cabinet secretaries in a collective process of canvassing alternatives. The style requires the president to think big and to enjoy enough self-esteem to enter into the give and take of an open advisory system. Thus, only new-order actives with a high degree of personal security need apply to be *priorities-and-planning* managers.

Broker-politics presidents seek the countervallance provided by multiple points of view. But, they tend much less than *priorities-and-planning*

presidents to institutionalize — either through creating advisory positions or utilizing collective decision-making bodies — the process whereby they obtain alternative advice. As well, they shy away from "big picture" scenarios. They prefer instead to work from relatively modest game plans. They will tend, thus, to channel their energies more into resolving crises than probing issues which do not cry out for immediate attention. Reactive-active presidents — such as Eisenhower and Bush — would find in broker politics a good fit between their personality and style. So would executive presidents like Carter. *Administrative-politics* presidents neither seek to tap a multiplicity of views nor to institutionalize countervailance. They prefer to devolve as many issues as possible down to departments. Thus, they see themselves as engaging in the process only in cases where problems prove too difficult to resolve lower down. Jimmy Carter organized his White House and cabinet systems as if he were an administrative-politics president. But his passion for detail undercut the appropriateness of this approach. That is, a serious disjunction emerged between the frequency with which the president immersed himself in issues and the relatively meager institutional apparatus available to handle the resultant case load. Normally, we would expect a being-there president to embrace administrative politics. Until Fred Greenstein's revisionist assessment of Eisenhower, presidency scholars had pegged Eisenhower as following administrative politics (1982). This owed in large part to the "hidden hand" quality of much of Eisenhower's pursuit of broker politics.

Finally, *survival-politics* presidents appear mostly to resort to this approach only when the other styles have failed. If they started out fostering countervailance, they increasingly cut down on the number of advisers whom they consult and their reliance upon collective consultative bodies. If they sought to devolve decisions to departments, they would turn more and more to specific advisers or units in the White House or the Executive Office of the President. These would increasingly operate as a counterbureaucracy to departments and agencies. Nixon gradually slid into survival politics even though he started in broker politics. Carter adopted survival politics in summer 1979 when concerns about the intractability of the American policy process and reelectability concentrated his mind on giving some direction to his administration.

Conclusion

This chapter began by locating the political executive sub-discipline within the wider context of political science. It asserted at the outset that the members of the sub-

discipline have always encountered considerable difficulty reconciling analysts' desire for simple explanatory theories and the innate complexity and unpredictability of political executives' behavior and performance. It focused its assessment of the state of the sub-discipline on Anglo-American systems. This allowed us to give special attention to factors emerging from one presidential and four considerably different parliamentary systems. But, it avoided the inevitable overload which would occur if we had sought an inventory of the entire canvas of executive leadership. And, several of the lessons which emerge from this analysis might prove of use to those more intimately concerned with political executives outside the compass of this review.

First, following upon Olsen's important work, analysts within any setting should give greater attention to secular changes which have altered publics' expectations for executive leadership and, in turn, political executives' views of their own roles. Similarly, they should eschew the tendency to overcompensate for the strengths and/or failings of current political executives by projecting immutable changes in the nature of the entire system.

Second, we should become more aware of the degree to which one researcher's rigor is another's subjectivity. The pursuit of the divided government thesis presents a case in point. In its 1960's U.S. incarnation, it emerged from a school of political science which had become disenchanted with the indolence of the system in addressing social problems. The more recent institutionalist variant has employed the concept — perhaps too eagerly — to excuse the inaction of presidents in grappling with key domestic issues.

Third, we should keep a cautious eye on the copycat effect in the world of political executives. We forget all too often that presidents and prime ministers and their many men and women view their counterparts in other countries as peers. Presidents and prime ministers tend to borrow ideas from one another — about inflation, deficits, the size of public services, privatization, and many more issues (see for example, Putnam and Bayne 1984). They and their advisers talk shop about how to organize presidents' and prime ministers' staffs, cabinet, economic policy making, budget review, management reform, and many similar topics. Furthermore, entire units within the World Bank, the IMF, and the OECD proffer advice on such matters and develop working groups designed to advance across-the-board acceptance of reforms. Yet, the copycat effect can become a poison pill. This chapter has dwelt on the degree to which unquestioning acceptance of public choice solutions to the intractable problems facing political executives — in some systems — have amounted to overtreatment or simply created side effects worse than the original affliction.

Fourth, this chapter has tried to disentangle two dimensions of the perennial institutionalization issue in the study of political executives. It has identified two, rather than one, gearbox problems. The first of these concerns the political executives who make up an administration or government and their relations with the standing bureaucracy. We found abundant evidence that the institutionalization of the latter works a diminished effect as chief executives and their colleagues heighten efforts to reassert political control. Yet, the tug of war between an administration/government and the bureaucracy forms simply one part of the policy competence equation. Institutionalization inevitably rears its ugly head. And, in the case of the second gearbox problem, it involves the increased complexity, unwieldiness, and even willfulness of the cadre of presidents' and prime ministers' men and women who make up an administration or government. Nobody -- least of all presidents or prime ministers -- can assume that just because two or more gather in a chief executive's name that they actually will work in harmony.

Finally, this chapter attempted to demonstrate the continued utility of examining presidents' and prime ministers' personalities as critical factors contributing to their management style and performance. The case presented here places a strong emphasis on the special salience of the issue to presidential systems -- owing, of course, to their monocratic nature. However, personality has worked distinctive effects in parliamentary systems. This assessment readily admits that inclusion of personality in analyses presents difficult problems of operationalization. However, we can lull ourselves into some seriously error-prone analyses if we rule out personality as a factor altogether or construe it as an optional add-on.

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Public Administration: The State of the Field

Donald F. Kettl

The scholarly study of bureaucracy and the administrative process has long been a contentious business. Scholars, in fact, have pointedly chosen to label the field in different ways. For traditionalists, the term of choice is "public administration." Many political scientists prefer "bureaucracy" instead, while others embrace "the new economics of organization" or "the new institutionalism." "Implementation" has its followers, while some scholars in public policy schools pursue "public management" and explicitly separate themselves from political science, in both traditional and new forms. Scholars from these different approaches rarely cite each other. They frequently suggest, at least implicitly, that those from other approaches have little to contribute to the really important questions. Of course, they rarely agree on what those important questions are. If anything characterizes the study of public administration, it is fragmentation.

Important questions in public administration, not surprisingly, have long revolved around problems of boundary-drawing. Woodrow Wilson, in "The Study of Administration" (1887), drew public administration's most famous boundary in stipulating a distinction between politics and administration. The Wilsonian politics-administration dichotomy has long dominated the way scholars have attacked the basic questions in the field. It has also directed the way they have answered them. It has allowed some public administrationists to distance themselves from indelicate political battles, and political scientists to immunize themselves against administrative complexity. The dichotomy ultimately has led many within political science to abandon the unruly child of public administration. It has also played into the cause of elected officials who were eager to use "administration" to pursue political ends and to blame administrators for political failures. The politics-administration dichotomy has fueled the struggle but not enhanced the debate. The challenge of drawing these boundary lines led to often difficult battles — indeed, estrangement — between public administration and political science.

The different approaches, as a result, are struggling for the very soul of the field. Some of the

conflict comes from fundamental disagreement over defining the basic questions. Some comes from stark differences in method, which especially distances newer, mathematically based approaches from older, descriptive approaches. Most fundamentally, however, the controversy flows from three fundamental problems.

First, different approaches to the study of administration usually come from one of two conflicting traditions in American politics — and each tradition leads to a very different perspective on the role of administration in American democracy (see Table 1). Some students of administration come to the subject with a fundamentally Hamiltonian bent. Like Alexander Hamilton, they seek a vigorous state vested with a strong administrative apparatus. They see the task of administration as carrying out publicly defined goals effectively; they see an energetic government doing good. Other students of administration, however, are fundamentally Madisonians. Like James Madison, they are wary about too much government action, and they are cautious about the concentration of governmental — especially administrative — power. Like Madison, they see in a delicate balance of power the best protection against tyranny. The competition of political interests, in their view, lessens the risk that bureaucracy can abuse individual liberty.¹

Second, different scholars have pursued very different ends in their study of administration. Some scholars have sought to build a body of theory that would explain the role administration plays in society. Their central goal has been to establish the study of administration firmly among the respected social sciences. Other scholars have recognized the importance of theory-building, but for them theory was just a step toward a more important goal: understanding the administrative process so that its functions can be improved. The distinction is based on the traditional theory-practice issue, but it is more than that. The two approaches differ sharply in how important finding practical solutions to administrative problems ought to be. They also differ on how important these problems ought to be in defining the central questions for the study of administration. Some

**Table 1. Strange Bedfellows:
Political Heritage in the Major Approaches**

Heritage

Hamiltonian

- Public administration
- Public management

Madisonian

- Bureaucratic politics
- Implementation
- Economic theories
- Bureaucracy

scholars have been content to reason abstractly, while others insist that theory speak directly to practical problems.

The Hamiltonian and Madisonian traditions enrich much of the study of American government, but perhaps no other area of political science so regularly finds itself in such deep conflict over the theory-practice problem. Since the very beginning of the American Political Science Association, public administrationists have threatened to holt from the fold. Yet, as political science nears the end of its first century, the issue of public administration's relationship with political science remains central. This relationship defines not only public administration's intellectual home; it also defines which questions will be preeminent.

Because of these fundamental differences, public administration has long struggled with a third basic problem -- the accumulation of knowledge. Other fields, from voting behavior to comparative politics, have integrated disparate approaches, accumulated knowledge, and built theory. For several generations, public administration followed this course. Since World War II, however, public administration has lurched from one fad to another, stumbled among several policy disappointments, and found itself fragmented because of alluring new approaches in sister disciplines. As a result, the field has shown a constant tendency to reinvent itself, only to discover that "new" approaches frequently rehash old ideas.

Thus, it is no exaggeration to say that public administration is in crisis -- and that, in fact, it has been in crisis since 1950 (Ostrom 1973; Waldo 1990). Important questions plague the field. Should there be a relationship between theory and practice? Does the pursuit of theory tend to have higher status and win the intellectual high ground? Is there room for the development of practical solutions to administrative

problems in the evolving scientism of political science? Does political science maintain any interest in what public administration has to say? Do other disciplines, from the interdisciplinary study of policy analysis (Weimer and Vining 1992) to the scholarly pursuit of organization theory in sociology and social psychology (Perrow 1986a), sometimes contribute more to basic administrative questions than public administration? Ultimately, does public administration still have -- and need -- a home in political science?

To each of these questions, I say "yes." Neither the study nor practice of administration -- or of politics -- can be complete without a theory that links them. As John Gaus (1950) argued, "A theory of public administration means in our time a theory of politics also." To that I would add, "No theory of politics is complete without a theory of administration." If administration is central to government, as Wilson argued in the less-cited portion of his famous 1887 paper, neither political science nor public administration can be complete without embracing the other.

Nevertheless, tensions among competing approaches to administration, and between these approaches and political science, have plagued the attempt to build a theory of administration. To examine these questions, I begin with a study of the field's creation. I consider how and why the field became fragmented. I continue by analyzing the emerging problems that theories of administration and politics must address and then conclude by examining public administration's greatest dilemma -- how to elicit effective performance and political accountability in the public sector. I conclude by examining the issues that, in the end, tie political science and public administration together.²

An Uneasy Marriage

Public administration developed as a field through four stages: the assertion that administration plays a central role in government (from 1887 until 1915); the era of scientific management and administrative principles (from 1915 to 1940); a period of critical self-examination (from 1940 to 1969); and a generation of centrifugal forces (from 1969 to the present).³

The Centrality of Administration: 1887 Until 1915

Although Woodrow Wilson is generally credited as the father of the *study* of American public administration, it really was Alexander Hamilton a century earlier who established the *practice* of American public administration⁴ (see Van Riper 1987; see also White 1948, 1951, 1954, 1958). Hamilton's famous reports on public credit and manufactures, written during his tenure as Secretary of Treasury, set the stage for policy making and administration for decades. Nevertheless, American administrative process evolved without a self-conscious view of itself or of its mission until Wilson formulated it in "The Study of Administration" (1887).

Wilson's work, as well as that of other early leaders like Goodnow (1900), defined the Progressive tradition. The Progressives were interested in two things. First, by establishing a professional administration they wanted to build a new American state (Short 1923; Skowronek 1982). They sought a strong and energetic government, a government that used science to pursue efficiency. Second, they sought to distance administration from the political spoils and scandals that had undercut administrative effectiveness, especially in the last half of the nineteenth century. Wilson and Goodnow are both remembered most for their commitment to separating politics from administration, Wilson for articulating it and Goodnow for developing it into a doctrine. "If I see a murderous fellow sharpening a knife cleverly," Wilson wrote, "I can borrow his way of sharpening the knife without borrowing his probable intention to commit murder with it; and so, if I see a monarchist dyed in the wool managing a public business well, I can learn his business methods without changing one of my republican spots" (Wilson 1887, 220; see also Doig 1983). Wilson suggested that administrators could create a set of tools that could be used for any public purpose.

Even though the politics-administration dichotomy suggests a highly stylized and inaccurate view of the role of administration in American government

(Henry 1987), it has established the importance both of the executive establishment and the systematic study of the field. If Wilson and Goodnow are best known for arguing the policy-administration dichotomy, however, they deserve to be equally known for believing in the centrality of administration in the political process (compare Holden 1991). The American Political Science Association's first president, Frank J. Goodnow, devoted his presidential address in 1904 to "the work of the American Political Science Association" and set out the fundamental questions to which he believed the new association ought to address itself (1905, 37): "the expression of the State will," "the context of the State will as expressed," and "the execution of the State will." He saved discussion of public administration, in fact, for his penultimate point. In Goodnow's view, APSA ought to address itself to helping government achieve "what is the best attainable" (p. 46). Public administration was the crucial link between the abstract study of politics and the process of improving the way the political system worked.

The importance of administration lay at the very core of the creation of the new American Political Science Association [APSA]. Five of the first eleven presidents of the association came from public administration and played important roles in framing the new discipline.⁵ One of them -- Woodrow Wilson -- went on to even higher office. As subheadings in the first *American Political Science Review's* book review section shows, public administration was one of five fields comprising the new discipline of political science; the others were comparative government (in the form of the study of colonies), public law (comprised of constitutional law and jurisprudence), international law, and political theory. From its very beginning, public administration was one of the critical foundations of political science, and political science was the natural home of public administration.

Despite their affinity, public administration and political science soon nearly divorced. Public administration promoted a "training movement," devoted to preparing students for government service with a curriculum independent of political science. Others in the APSA argued the association should devote its efforts to building theory. Did practical training belong -- or could it perhaps comfortably fit -- within a discipline devoted to the development of theory? Or did political science's more practical side belong in a separate discipline, in which its approaches could be integrated with those of related disciplines? The "training movement" proved short-lived, because mainline public administrationists saw their home in the new APSA and were committed to maintaining it. The truce proved uneasy, though. Many public administrationists were unhappy about sharing the

discipline with more theoretical political scientists, and some political scientists found practice-oriented public administration an undesirable partner.

Problems recurred constantly. Public administrationists never felt that political science recognized the importance of education for the public service. The natural corollary of the Wilson/Goodnow movement toward a professional administration was to train professionals to staff it. Many political scientists, struggling on a different front to establish the discipline as a social science to be taken as seriously as economics, focused their attention instead on developing advanced theories.

Scientific Management: 1915-1940

Having established the importance of administration in the American system, and having uneasily accepted political science as their home, public administrationists began to develop their craft. The gospel of separating politics from administration, which Goodnow and Wilson preached, became a strategy instead for separating administration from politics. Following the "scientific management" approach charted by Frederick W. Taylor (1911), analysts sought the "one best way" to perform administrative work efficiently, free from the meddling of partisan politics. As Roscoe Martin described:

In the atmosphere provided by scientific management, a mechanistic concept of public administration came to prevail widely and in important circles. Administration was separated severely from the legislative body, toward which its spokesmen frequently manifested not only impatience but also profound distrust. "Politics" was anathema -- not the politics practiced by administrators, but the politics of the "politicians." The emphasis seemed to be on answers, not on questions (1952, 67).

Advocates of the scientific management approach to public administration saw virtually no barrier to its ability to improve government -- if only managers could be protected from political meddling. They advanced the creation of new regulatory institutions, such as the Federal Reserve Board, new tax systems, such as the income tax, and new budgetary processes, such as the executive budget. It was an era of remarkable success and self-confidence.

Despite its rocky first years within political science, public administration gained remarkable prestige in government and, in fact, within political science. By 1940, one-fifth of all political science doctoral degrees

were in public administration (Martin 1952, 662). President Roosevelt had just implemented the recommendations of the Brownlow Committee (1937), which had proposed sweeping changes to transform the presidency. The committee's three members -- Louis Brownlow, Charles Merriam, and Luther Gulick -- occupied the pantheon of public administration (Karl 1963). Brownlow had helped establish the city manager movement, while Merriam was a vigorous proponent of scientific management. Background papers prepared for the committee, notably Gulick's "Notes on the Theory of Organization" (1937), came to define the field's orthodoxy for a generation. Meanwhile, young committee staff members were soon to establish themselves as the leaders of public administration's next generation.⁶ As a field, public administration had things to say to government, and it did not hesitate to say them. When World War II broke out, it was scarcely surprising that many of the nation's leading public administrationists went off to Washington to help manage the war effort.

Public administration reached its high point during World War II. Its leading scholars had helped found APSA and had trained government managers. They had generated a "scientific" approach to management that tried to elbow politicians aside. They had developed principles to guide executive officials, and they had put them to work in the Roosevelt administration.⁷ They had gained important seats in the central councils of American government and continued to play an important role within political science.

Critical Self-Examination: 1940-1969

Soon after the war ended, the focus of political science itself began to change. Political scientists began developing new theoretical approaches that undermined the scientific management approach to public administration. Norton Long (1949) contended that political power, not administrative efficiency, was necessary for effective administrative practice. Even as political science sought to discover how to become more scientific, Robert A. Dahl (1947) argued that the study of administration would never be able to achieve that goal. A new behavioralism, devoted to the understanding of how *individuals* (like voters) instead of *institutions* (like bureaucracies) behaved, began displacing more traditional approaches. The questions were not ones that public administration either recognized as important or was equipped to answer. Statistical methods were a whole new language for most public administrationists -- and not one they understood well or could speak clearly (Fesler 1975; Schick 1975).

For their part, many public administrationists returned to their universities from Washington with a

fresh sense of realism. Simple principles of efficiency, based on scientific management, seemed unacceptably shallow. They developed instead a new perspective on public administration based on the relationship between administration and democracy (Appleby 1945; Waldo 1984). They also struggled for new principles to replace the old -- and for ways to train public servants to face the nation's new challenges, which seemed not well answered by traditional theory. The idea of training reappeared, but this time the separatist tendencies were far stronger.

Meanwhile, Herbert Simon helped create a new management science. He argued (1946) that the old public administration produced principles which were frequently conflicting and hence useless. His attacks on traditional administration were against a straw-man target instead of the rich theories that public administration had developed. Nevertheless, his criticisms reflected growing dissatisfaction with the old theories at the same time that other theorists were defining a new, politically aware approach to public administration (Fesler 1975). Even more important, Simon redefined the *process* of decision making as the critical problem of administration, instead of finding the best organizational *structure* (Simon 1947; March and Simon 1958). With its traditional approaches under attack, public administration suddenly found that its intellectual foundation was slipping (White and McSwain 1990).

Just before World War II, leading public administrationists created "an association of public administration for public administrators" (Pugh 1988, 17). The movement, spearheaded by Syracuse University's William Mosher, soon led to the creation of the new American Society for Public Administration [ASPA]. ASPA's organizational meeting was held at APSA's 1939 annual meeting, and more than 150 APSA political scientists decided to join the new organization. Leonard D. White, well-known for his authorship of the leading public administration textbook (1926), became the editor-in-chief of the new association's journal, *Public Administration Review*. Even though ASPA's first meetings were held in conjunction with APSA's annual meeting, the new association soon decided to split completely from APSA. Otherwise, its founders feared, ASPA would never be more than a section of the political science association and its goal of establishing a society for public administrators -- as opposed to those who studied public administration -- would never be realized (Pugh 1988). War-time restrictions on travel and heavy demands on ASPA's founders to help manage the war effort slowed ASPA's early progress. With the end of the war, however, the separatist movement sparked by ASPA's creation took firm hold.

The three trends -- the rise of a new political science, the separate creation of a new management

science, and the separation of ASPA from APSA -- aggravated the tensions between public administration and political science. Political science as a discipline strongly fought the separatist tendencies. "We recommend that the tendency toward splintering the field of political science be stopped at once," the association's Committee for the Advancement of Teaching vigorously argued in 1951 (p. 129; see also Martin, 1952). Its Committee for the Advancement of Teaching wrote:

A few fields are mentioned again and again as constituting the core of political science. They are: political theory, public law, international relations, public administration, and politics, either as subjects unto themselves or as set forth in courses in American or comparative governments. It is noteworthy that the two fields that have had a tendency to become separated from political science -- international relations and public administration -- are considered by these respondents [teachers of political science around the country surveyed for the study] to be at the very heart of the discipline (APSA 1951, 126).

The battle against separatism, however, was a losing one. Political science was moving toward new theoretical questions that public administration was ill-equipped to answer. The drive of many within public administration to link its practice and its study helped drive some scholars out of political science and into ASPA (Caldwell 1965, 58). As Allen Schick observed, "Public administration had come apart and could not be put back together" (1975, 157).

In 1904, public administration had been a critical pillar in Goodnow's vision of political science. In 1951, it was one of the "core fields" of political science. By 1962, when APSA issued "Political Science as a Discipline" (1962, 417, 421), a special report on instruction, public administration was mentioned only in passing as a subfield of American government. The report discussed teaching in public administration as part of a broader look at "professional fields of instruction," along with other approaches including law, business administration, school administration, social work, and foreign affairs. The American Society for Public Administration was drawing important public administration scholars away from political science, while some scholars in political science wondered whether public administration properly belonged within political science. Public administration had lost both of its traditional anchors, its theoretical base and its disciplinary home. Dwight Waldo wrote sadly:

It is now unrealistic and unproductive to regard public administration as a subdivision of political science....The truth is that the attitude of political scientists (other than those accepting public administration as their "field") is at best one of indifference and is often one of undisguised contempt or hostility. We are now hardly welcome in the house of our youth (quoted in Schick 1975, 160).

Centrifugal Forces: 1969-Present

Many competing answers rose up in the struggle to replace orthodoxy. In the leading textbook of the era, Leonard D. White noted, "There are many ways to study the phenomenon of public administration. . . . All of these approaches are relevant and from all of them come wisdom and understanding" (4th ed, 11; quoted by Storing 1965, 50). While some of this diversity reflected a lively intellectual search for new ideas, some students of administration saw it as a "complacent, indiscriminating eclecticism" (Storing 1965, 50). If any approach could be useful, then no approach could be central. So great was the movement away from central theoretical questions, in fact, that White, perhaps the leading student of administration from the 1920s to the 1950s, ended his career not with grand theories but with careful histories (White 1948, 1951, 1954, 1958). It was almost as if he felt compelled to begin again at the beginning, to rebuild the field on a new foundation of fresh interpretations.

During the 1950s and early 1960s, public administration suffered from more than just the rejection of the politics-administration dichotomy. It suffered from the lack of a theoretical guide and a comfortable disciplinary home. It had come to the realization that politics mattered, but did not know *how*. Meanwhile, much of political science had convinced itself that public administration was stuck in old questions and crippled by outmoded methods.

The decline in public administration's status could scarcely have been greater. From a place of supreme confidence, reflected in the Brownlow Committee report, it was in uneasy search for itself. From having an important role in the APSA's governance and in the production of its young scholars, it had faded to the background. Public administration fell into a state of critical self-examination and faced a serious intellectual crisis (see Ostrom 1973).

Public administrationists have disagreed among themselves about the prospects for resolving this crisis, especially within the discipline of political science. Dwight Waldo's 1987 John M. Gaus Lecture -- delivered upon bestowal of the association's most prestigious award

to leading senior scholars in the field -- argued that "estrangement is perhaps too mild to characterize the relationship of public administration to other fields of political science." Waldo regretfully suggested that, for most political scientists, "public administration concerns the lower things of government, details for lesser minds" (1990, 74; emphasis in original). Herbert Kaufman, in his 1990 Gaus Lecture, worried that public administration and political science were reaching "the end of alliance." For Waldo and Kaufman, the battle was over; public administration was and ought to be separate from political science.

Allen Schick had presented a far different opinion years before. "[P]ublic administration can no more escape political science than it can escape politics," as Schick put it. "Until it makes peace with politics, public administration will wander in quest of purpose and cohesion" (1975, 160). In a third Gaus Lecture, James W. Fesler agreed with Schick. The worlds of governance and of political science "should not be far apart," he contended (1990, 85).

These disagreements reflect great uneasiness about whether the rift between public administration and political science can -- or indeed should -- be healed. The answer to this puzzle lies in developing a theory of administration that, as Gaus suggested, is a theory of politics also.

New Approaches

Graham Allison's "Conceptual Models of the Cuban Missile Crisis" (1969) undermined traditional studies of public administration more than any other work of the behavioral era. Instead of focusing on how to structure bureaucracies to produce desired outputs, he sought to explain why certain structures produced observed outputs. In doing so, they rejected traditional public administration and reached past political science. His was perhaps the first product of an interdisciplinary environment, then being nurtured at Harvard by Richard Neustadt, to understand policy problems.

Allison contended that three different models helped explain the crisis. In Model I, the "rational actor" model, the crisis policy makers attempted to develop the best, most rational answer to the problem of nuclear-armed missiles in Cuba. In Model II, the "standard operating procedures" model, the rules of the diplomatic and military bureaucracies led to predictable behavior that sometimes frustrated policy makers. In Model III, the "bureaucratic politics" model, political bargaining among top officials explained outcomes.

Allison was very careful not to say that any of his models represented the "correct" explanation of the

crisis. He presented them as complementary, not exclusive alternatives. Some scholars who read his work, however, took a different lesson from it. They viewed Model I as a proxy for the rational approach of economics, and they rejected it as unrealistic. They saw Model II as a proxy for conventional, principles-bound public administration, and they discounted it as woefully inadequate. They saw in Model III a new and lively approach to public policy based centrally in political science. Model III took a problem -- explaining outcomes -- and solved it by applying the central political process -- bargaining -- to the behavior of top officials.

Allison's work, perhaps unintentionally, had two important effects on the study of administration. First, for readers who saw far more virtue in Model III than in Models I and II, it further widened the split between politics and administration. Because the focus on political bargaining at the top seemed more illuminating -- and exciting -- than the study of administrative processes toward the bottom, it became easier for some political scientists to dismiss the study of administration in favor of Model III. Traditional public administration seemed even more remote from issues like the Cuban Missile Crisis and decision making in organizations. A lively literature devoted to bureaucratic politics sprang up soon after the publication of Allison's article and his subsequent book (Allison 1969, 1971; Halperin 1974; Steinbruner 1974).

Second, and far more subtly, Allison's work launched the study of new phenomena in the administrative world. The traditional approach to public administration was devoted to the *organization* as the unit of analysis: how it was structured; what processes and problems administrators had to solve; and how the entire process could work more efficiently. Like the broader movement in political science, Allison's three models focused attention instead on the *behavior* of political actors, of which administrators were only one group. Unlike the behavioral movement, he concentrated on power relations among actors. These actors bargained among themselves to shape policy decisions; the actors with the most political power won. In the bureaucratic politics perspective, administration was a "political resultant," not the search for efficiency.

Within the political science community, the bureaucratic politics perspective thus provided a fresh look at administration. It established a forum for studying administration outside traditional public administration channels, and it focused on a unit of analysis different from the ones that public administrationists had studied. The new approach was most important because of its departure from the Hamiltonian vision of bureaucratic power and efficiency that had characterized public administration for its first

century. Bureaucratic politics was implicitly Madisonian, full of competing interests that struggled to shape administrative action. Following close on the bureaucratic approach were four new approaches: implementation, public management, economic theories, and public bureaucracy. They had little in common but their rejection of traditional public administration.

Implementation

The 1960s brought the Great Society, ambitious promises, and major disappointments. Many political scientists found public administration sorely lacking: It neither produced success nor explained failure. If program results did not live up to their ambitions, administrative failures seemed to be the cause -- and if existing administrative patterns were not working, it was time to reexamine the old principles. Part of the reason was also theoretical: public administration seemed to many observers to leave major gaps in the explanation of the process. A new "public policy" approach grew up within political science, an approach that explicitly sought to build a bridge between old administrative theory and the new administrative realities forcefully brought home by the Great Society programs (Jones 1976). The bridge was a new theory -- implementation -- designed to be the "missing link" between policy making and policy outcomes (Hargrove 1975).

"There is (or there must be) a large literature about implementation in the social sciences," Pressman and Wildavsky wrote. "It must be there; it should be there; but in fact it is not" (1973, 166).⁸ Traditional public administration scholars were surprised to read this bold statement, for they believed that the entire field was dedicated to precisely this question. Leonard D. White, in the first edition of his public administration textbook, wrote, "Public administration is the management of men and materials in the accomplishment of the purposes of the state." John M. Gaus, a giant in public administration, added in the margin of his own copy of White's text: "and a reconsideration of the policy being implemented." Charles O. Jones has concluded, "Unquestionably Gaus viewed administration as comprehensively as Pressman and Wildavsky define implementation" (Jones 1977).⁹

What was truly new about implementation was not its focus but its rejection of traditional public administration prescriptions for the study of administration. Unlike public administration's concentration on the organization, implementation fixed instead on the *program* as the basic unit of analysis. It borrowed as well from the systems approach then gaining ground in the economists' study of public policy analysis: Goals and resources are put into the political system;

outcomes, often at variance with the original goals, result. Government agencies and their employees are means to an end, not an end in themselves. All too often, in the view of most of the early implementation scholars, they are, in fact, the source of the problem.

From this foundation, the implementation movement evolved through three stages (Goggin, Bowman, Lester, and O'Toole 1990). In the first stage, launched by Pressman and Wildavsky's study, implementation was singularly devoted to the study of government failure, with failure understood as the inability of a public program to achieve its legislative goals. Problems were everywhere. (See Derthick 1972; Bardach 1977; Berman 1978; Elmore 1978; Van Horn 1979.) The causes of failure were legion, rooted especially in fundamental pathologies of public bureaucracy (Hogwood and Peters 1985).

Pressman and Wildavsky identified three major problems. First, in their view, programs were plagued by the "complexity of joint action" (1973, 107). With so many steps in the policy process, the opportunities for disgruntled interests to derail or deflect implementation were endless. Second, there was the problem of control. Implementation tended to take a top-down view, and policy makers at the top had great difficulty in regulating the behavior of implementers at the bottom through the long and complex policy chain.¹⁰ Finally, the implementation environment was complex, with many opportunities for participants to play games that distorted and delayed implementation (Bardach 1977).

The behavior of government bureaucrats posed, for implementation theorists, a special problem. Bureaucrats delay, obstruct, vacillate, hesitate, and regulate. They get caught up in their own routines and orthodoxies, becoming blinded to new signs and signals. Bureaucracy demonstrates "a preference for procedure over purpose" (Pressman and Wildavsky 1973, 133). In fact, of all the impediments to success, early students of implementation rated bureaucracy the worst. "No one is clearly in charge of implementation," Ripley and Franklin conclude. It was little wonder, therefore, that program performance so often fell short of expectations (1986, 12).

Two important weaknesses characterized much work in implementation's first stage, however. First, the results were largely creatures of the cases scholars studied. Scholars were naturally drawn to interesting problems, but the most interesting puzzles often tended to be failure stories. The failure focus resulted in a depressing body of literature that promoted the perception that lack of success was the norm. The focus on failures explained neither how to achieve success nor why many programs, from the early space program to the construction of flood control dams, were so manifestly successful.

Second, there was a more subtle bias toward studying intergovernmental programs. Nearly 80 percent of all of the entries for programs and organizations in one standard implementation textbook are for program and organizations that have an important intergovernmental dimension.¹¹ Such an overwhelming focus on intergovernmental programs is far out of proportion to the role that such programs actually play in American public policy. Most of the national policy initiatives of the Great Society and of the next decade were intergovernmental, so scholars naturally were drawn to these programs. Most administrative activity, however, was not intergovernmental. Implementation theory that concentrated on studies of intergovernmental activity resulted in a distorted view of the administrative process.

These problems inspired a clever subtitle for Pressman and Wildavsky's now-classic book (1973): *How Great Expectations in Washington Are Dashed in Oakland; Or, Why It's Amazing That Federal Programs Work At All*. The implementation approach argued that Washington's goals too often went unrealized in the states and cities. That approach, however, did not often take account of one central fact: that the primary objective of many of the programs was to adapt broad national goals to specific local conditions. A secondary objective frequently was to shift power from the national government to the cities and, especially, to citizens in their neighborhoods. Conflicts over how to spend the money were the natural result of this process (Kettl 1980).

Inferences about "success" and "failure" were far too simpleminded, out of step with the shifts in political power and the complex organizational arrangements in many of these programs. A success to a neighborhood group might seem like a failure to a federal official. Judgments about program results, moreover, often changed over time. Delayed programs sometimes became eventual successes, and some early successes turned sour. Even a program brought to a standstill, such as construction of a new highway, may well be a success for an interest which did not want it finished.

By the mid-1980s, a second stage of implementation research emerged to tackle these problems. Marked by important syntheses by Ingram and Mann (1980), Mazmanian and Sabatier (1983), and Ripley and Franklin (1986), scholars searched for systematic variations among public programs. These authors contended that success was possible, that conditions for successful implementation varied over time and across levels of government, and that the political context of implementation significantly affected results. As critics have pointed out, however, the second-stage analyses *illustrated* but could not *prove* the value of their formulations. The case studies on which their

conclusions were based were never replicated, and their theoretical propositions were not validated. The studies produced useful synthesis but, in the end, few firm conclusions about which variables were most important in determining program success (Goggin, Bowman, Lester, and O'Toole 1990).

The third stage combined both top-down and bottom-up perspectives on implementation with theories about communication to seek an integrated theory of implementation. It is explicitly more scientific, seeking a systematic understanding of how different conditions produce different results (O'Toole 1986, 1989a, 1989b; Goggin, Bowman, Lester, and O'Toole 1990; Ingram 1990). This work represents an important advance past the first two stages, but it is still in its infancy: a broad sketch with only some details is just beginning to appear. It offers insights into how managers are likely to affect the odds of success and a plan for more research to improve the state of implementation theory. It is handicapped, however, by what preceded it: a cynical sense of inevitable failure that discouraged many scholars from even exploring implementation.

Implementation did, however, establish a very different approach to administration. Like bureaucratic politics, it implicitly rejects the Hamiltonian vision of bureaucratic power. With its Madisonian vision of competing interests, implementation has helped to explain why success in government programs seems so elusive. It has not provided the interdisciplinary approach that the early founders of the public policy movement hoped it would. It has, however, capitalized on declining public confidence in government effectiveness by explaining the difficulty of making government work. If the approach has not matured theoretically, it has nevertheless leveled a far more serious blow to traditional public administration than the bureaucratic politics approach ever delivered.

Public Management

From the seeds of the bureaucratic politics approach came a new public management movement. Unlike bureaucratic politics, it was more Hamiltonian, devoted to the study of administrative leadership and power. Unlike implementation, public management was devoted to finding prescriptions for making public programs work better.

The public management approach has developed in two tracks: as a counterbalance to policy analysis in the public policy schools; and, in the field of institutional theory, as an elaboration of Simon's theory of decision making.

Public Management in the Policy Schools

The public management movement emerged in the 1970s with the development of public policy schools. It began with a focus on the importance of the environment and of strategy-setting by top executives (Bryson 1988). It borrowed the business school case method as the basic research and teaching tool. In fact, the public management movement has its foundation on writing *about* public managers by former public officials. At Harvard, the movement grew from work by Richard Neustadt (1960; Neustadt and Fineberg 1978) and his students Mark Moore (Moore and Stephens 1981; Fleishman, Liebman, and Moore 1991) and Philip Heymann (1987). At Berkeley, a separate public management movement built on the Pressman and Wildavsky implementation approach. Together, they conceived of public management as an intellectual balance for the policy analysis approach developing at the public policy schools. If policy analysis was to help government officials make better decisions, public management would help them achieve better results.

Public management has since become a term for all manner of administrative studies, from organization theory and behavior to old-style public administration dressed up in new clothes to avoid the old problems. Scholars sometimes find it easier to win grants and publish materials under the more lively sounding public management banner than under the tattered public administration flag. Top-rank research universities, which had rejected the vocational overtones of public administration, sometimes found public management a more high-minded approach.

The irony, of course, is that the public policy school approach to public management has innumerable echoes from public administration's past. It is essentially Hamiltonian, as practical minded as public administration ever was. Its approach builds on the same case-study tool that characterized public administration in the post-World War II years (Stein 1952), and for which public administration was roundly criticized because case studies were believed to be unscientific (see Behn 1988; Barzelay 1991). It seeks principles that many political scientists rejected in the late 1940s as outmoded. The main difference with traditional public administration is its heavy focus on top leaders, its argument that it is onto something new, and its rejection of the public administration work that preceded it. Two Kennedy School public management experts pointedly argue that "public managers are negotiators" and "public managers are leaders." By contrast, "public administrators are experts" (Barzelay and Kaboolian 1990, 600).

This approach to public management offers great potential, especially in coupling management strategy with the manager's political environment, but progress has been halting. It grows from Simon's argument (1947) that decision making is the central administrative act. Like implementation, however, the public management movement has largely been a creature of what its students have studied. The case studies have been interesting but the knowledge gained from them is rarely cumulative. Insights drawn from them have often been more the result of the experiences of the authors than of their theoretical contributions. Nevertheless, this approach to public management has produced absorbing studies devoted to new visions for improving government (Lynn 1981, 1987; Barzelay 1993). Despite the self-conscious attempt to distance itself from public administration, moreover, the similarities -- in both method and basic questions -- are striking.

Institutional Theory

During the 1980s, public management theory developed a separate branch, christened "institutional theory," quite independent of the Harvard/Berkeley public policy school approach. The institutional theorists started from Simon's decision-making theory, but from there they departed significantly from the public management scholars. Unlike the public management approach, institutional theorists were more focused on the behavior of organizations. Unlike traditional public administrationists, institutional theorists built more on interdisciplinary work, especially research by organizational sociologists like James D. Thompson (1967) and Philip Selznick (1949, 1957). They build on previous work as diverse as Herbert Kaufman's classic study of the forest ranger (1960) and Wamsley and Zald's analysis of the political economy of organizations (1973). With the "new institutionalists" of political science (Skowronek 1982), they share the belief in the importance of institutions in government and the importance of bureaucracy as a political institution (March and Olsen 1984, 1989; Powell and DiMaggio 1991).

Wamsley and Zald defined the central questions to which the institutional theorists have devoted themselves:

How does the level of required coordination among the units of an organization and the constitutional norms about the interrelations among units affect the degree of centralization of the internal policy? What are the conditions under which internal succession systems emerge, essentially limiting the choices of the nominally

superior appointing officer? How do variations of accounting systems limit the surveillance of external actors? How do accounting systems shape perceptions of power and allocate resources to avoid accountability? How does the mode of funding interact with agency goals so as to displace agencies from pursuit of their original goals? (1973, 81)

These critical questions contain echoes of traditional public administration issues, such as centralization vs. decentralization, accountability, oversight of career officials by political appointees, budgetary control, and organizational norms. They present the traditional questions in a new light, however. The institutional theorists have devised far more rigorous experiments to test their hypotheses. They have also worked far harder to aggregate their findings. The result is an impressive body of work that offers sustained attention to the critical issues of public management (see, for example, Rainey, Backoff, and Levine 1976; Bozeman 1987; Rainey 1983, 1991; Romzek and Dubnick 1987; Perry 1989; Provan and Milward 1991; LaPorte and Consolini 1991; and Milward, Provan, and Else 1993).

While still in its adolescence (Scott 1987), institutional theory offers some of the most useful new contributions to administration. Organizations lie at the center of their work. They are Hamiltonians at heart, seeking to invigorate bureaucracies and make them effective. Although the institutional theory approach is in relative infancy, it has already produced some of the most stimulating work in public management. It offers a connection between the critical questions that public administration defined in the past and at least the foundation for developing useful answers.

Economic Theories

Out of economics came a third approach to studying administration. This approach sought to derive a cogent set of predictions from a limited set of assumptions, based fundamentally on a theory of rational action. The economic theories begin with the assumption that participants in the political process, including administrators, seek to maximize their utility (Buchanan and Tullock 1962; Tullock 1965; Downs 1967; Niskanen 1971; Romer and Rosenthal 1978). As they gain more experience, they are likely to become more risk-averse because they have more to lose and because they become more entrapped in the organization's routines. Bureaucrats are therefore less likely to be entrepreneurs and more likely to become "conservers" as their careers develop. Whole bureaucracies likewise tend to become

more conservator-dominated as they age (Downs 1967). Administrators seeking to maximize their utility also work to increase their power and their discretion (Niskanen 1971; Blais and Dion 1991).

The assumption of utility-maximizing bureaucrats has produced a host of formal models of bureaucracy (Bendor 1988). Four branches of theory have dominated: principal-agent theory; bureaucratic-outcomes theory; institutional-choice theory; and transaction-cost theory.

Principal-Agent Theory

The economic theories huld upon a basic argument that the critical relationship is the one between elected officials and administrators. As policy makers, elected officials are the *principals* in the relationship. Administrators act as their *agents* in implementing policy. The central problems of such principal-agent relationships are how principals can choose the best agents, how faithfully agents are likely to pursue their principals' policies, and how principals can improve the odds that their agents' actions will coincide with their desires. Information asymmetries, where agents know more about their tasks and about their own behavior than principals, complicate the relationship. Principals have a difficult time telling agents what they ought to do because agents know more about the technical details. Principals therefore find it hard to pick good agents or to discover precisely what their agents are doing and how well they are doing it. When agents know more about their behavior than the principal ever can, and when agents are self-interested, then shirking and opportunism can result (Moe 1984; Arrow 1985; Bendor 1990).

Agency theory offers basic insights into the problems of managerial control and organizational effectiveness (White 1985). It helps explain implementation problems, such as why the Department of Energy had such difficulty in managing the contractors that manufactured the nation's nuclear weapons. It is difficult for principals to know enough to choose good contractors as agents (the "adverse selection" problem). It is also hard for the department to know enough about what its contractor-agents are doing and thus whether their performance matches the department's goals (the "moral hazard" problem). Agency theory identifies the flow of information as the critical problem. Principals can improve their selection of agents if they can learn more about them before hiring them. Principals can reduce information asymmetries by altering their agents' incentives and by improving the monitoring of their agents' behaviors.

Bureaucratic Outcomes Theory

Agency theory has been most powerful, however, in two applications: theories of bureaucratic outcomes, and theories of institutional choice. In the bureaucratic outcome approach, researchers have set out to reform the bureaucratic politics literature. Bureaucratic politics, along with much early public choice literature, has suggested that American bureaucracies resist change. Controllers, like members of Congress, have little incentive for oversight because the rewards are few (Mayhew 1974). The bureaucratic-outcomes approach, however, argues from the experiences of the Reagan years and from sophisticated quantitative analysis that the course of bureaucracies can indeed be changed. The argument goes like this (see, for example, Wood and Waterman 1991): Institutions headed by elected officials, such as the presidency and Congress, create bureaucracies; that is, bureaucracies can be viewed as agents for the principals' -- elected officials' -- wishes. The principals design incentives and sanctions to enhance their own control. When the principals detect bureaucratic behavior that does not match their policy preferences, they use these incentives and sanctions to change that behavior. Among the important sanctions are the president's appointment power and the budgetary leverage that the two branches share. Bureaucrats' behavior can thus be influenced by shaping their incentives and sanctions.

Using quantitative data gathered from a number of federal agencies, researchers demonstrated that agency outcomes vary with political preferences, especially changes in presidential administrations (see Moe 1982; Weingast and Moran 1983; Moe 1985a; Wood and Waterman 1991). From that they conclude that "elected leaders can and do shape bureaucratic behavior in systematic ways" (Wood and Waterman 1991, 801).

There is a fundamental irony, of course, in using rational-choice models, first proposed as a way to explain bureaucratic intransigence, to demonstrate bureaucratic responsiveness. In rejecting the obviously too simple assumptions of the most basic rational-choice approach and the arguments about immovable bureaucracies, however, the bureaucratic outcomes approach has struggled with a more fundamental problem. The tests of the model are statistical, and the dependent variables employed tend to be *process*, rather than *outcome* measures, such as the number of seizures by drug enforcement agencies or the level of enforcement activity by regulatory agencies. Separating activity from results is an old problem in measuring bureaucratic performance. In drug enforcement, for example, thousands of small dealers can be put out of business without affecting the large suppliers; a large number of seizures can produce

high levels of activity without demonstrating effectiveness. Likewise, hundreds of small anti-trust cases can pale by comparison with one single case, such as the divestiture of AT&T, which took years to accomplish but had profound implications.¹² Covariance between independent variables, such as presidential administrations, and process measures, such as the number of seizures or inspections, may in fact say very little about bureaucratic outcomes. That weakens the argument for a clear principal-agent connection between the preferences of elected officials and the activities of government bureaucracies. Furthermore, it underlines the critical information problems that undercut inferences about the whole process.

Institutional-Choice Theory

The impact of institutional-choice theory has been far greater. It seeks to examine the basic questions of bureaucratic politics – the three-way interactions among bureaus, politicians, and interest groups. Unlike bureaucratic politics, however, it attempts to model these interactions formally. It borrows heavily from agency theory in postulating the bureaucracy as agent of political forces and in incorporating serious information asymmetries. It assumes that the players are self-interested, but it also builds on Simon's (1947) argument about bounded rationality. Participants would like to maximize their utility – to optimize – but they cannot because of information constraints. Thus, they must sacrifice and adapt their choices to the constraints they face. The result is a tight argument, based on mathematical (but not necessarily statistical) models, that produces a pluralist outcome: the power of bureaucracies is the result of the equilibrium that contending political forces produce (Bendor and Moe 1985; Knott and Miller 1987; Moe 1989).

Institutional-choice theory thus completes the steps, in rigorous form, first made by bureaucratic politics. It replaces the traditional public administration view of bureaucracy-as-actor, as independent variable, with a new view of bureaucracy-as-acted-upon, as dependent variable. Organizations are not designed to promote efficiency but, rather, to reflect the power of political interests. Thus, it is scarcely surprising that bureaucratic structures often do not seem designed to promote efficiency or that they often produce ineffective results. Institutional-choice theory contends that they are not fundamentally designed to do so. Rather, they are the result of rules, implicit and explicit, that are the result of political forces. These rules can be discovered, influenced, and changed. Any attempt to reform bureaucracy thus must take account not just (and perhaps not even) efficiency but, rather, the constellation of political forces that will create the rules under which the

bureaucracy must operate. Some studies in this tradition, such as Chubb and Moe's controversial study on reform of local schools (1990), build on economic theories to recommend more choice as a way to make bureaucracies more responsive.

Transaction-Cost Theory

Transaction-cost analysis has revolutionized organization theory but has surprisingly little impact within political science or public administration. Based on path-breaking work by Nobel laureate Ronald Coase (1937) and important elaborations by Oliver Williamson (1975), this theory sees the environment as a network of organizations (compare Landau 1991). It begins by assuming that self-interested individuals work within organizations to promote their own individual goals. The environment, however, is uncertain. To stabilize it, organizations tend to develop long-term relationships with other organizations, notably suppliers, which cannot easily be replaced if the relationship becomes strained. Other organizations in the network gain bargaining power because of this interdependence.

All of these factors combine to create boundaries on rational behavior. In such interdependent organizational networks, the high level of uncertainty makes it hard to predict outcomes. Instead, administrators consider the costs of their transactions among organizations and conclude that it is most rational for their organizations to become bigger. When they cannot grow larger, they seek to integrate other organizations in the network into their organization. To minimize the costs of transactions among organizations, the theory thus argues that it is most efficient to substitute *authority relationships within the organization* for the *uncertain market relationships in the outside networks*. Vertical integration, not market competition, is the result.

Despite the enormous impact of transaction-cost analysis on generic organization theory, it has yet to be applied widely within the public sector (Rainey 1991, 44). Nevertheless, the potential appears significant. Researchers have discovered that many government organizations work through networks. Local mental health programs, for example, are managed through complex interrelationships among government bureaus and nonprofit organizations (Milward, Provan, and Else 1993). At the federal level, contracting out has proliferated, in everything from telecommunications policy to toxic waste cleanup. The management networks have become ever more complex and interdependent as a result (Kettl 1993). While these examples are suggestive, links between such service networks and transaction-cost economics have yet to be drawn.

Summary

These theories have appeal because they provide a foundation for understanding the unresponsiveness that nearly everyone has criticized in bureaucracies. When they have been incorporated into conservative political rhetoric, they have also helped help explain why bureaucracies, bureaucratic power, and public budgets have tended to grow over time. From the right, they have produced a voluminous literature of why privatization, by replacing governmental power with market competition, can improve governmental effectiveness (Savas 1987; Hanke 1987; Donahue 1989; compare Salamon 1981; Smith 1983; Seidman and Gilmour 1986; Moe 1987; Salamon 1989). From the left, they have generated calls for beefing up government by making it more entrepreneurial (Osborne and Gaebler 1992). Few fundamental ideas in public administration have ever struck more intellectual sparks.

The economic approach has come under heavy criticism, especially from theorists who contend that the basic focus on rationality robs the study of organizations of their very life. Economic theories of organization, Charles Perrow contends, represent "a challenge that resembles the theme of the novel and movie *The Invasion of the Body-Snatchers*, where human forms are retained but all that we value about human behavior -- its spontaneity, unpredictability, selflessness, plurality of values, reciprocal influence, and resentment of domination -- has disappeared" (1986b, 41). Terry M. Moe agrees, contending that bureaucracies tend to be omitted as major features of these models. Instead, they appear "as black boxes that mysteriously mediate between interests and outcomes. The implicit claim is that institutions do not matter much" (Moe 1987, 475).

One tenet common to all of the modern theories of organization is that institutions -- especially bureaucratic ones -- do matter. What varies is how. The more formal economic models of bureaucracy answer this problem by actually reducing bureaucracies to black boxes, which allows the creation of sophisticated models of how, for example, Congress can dominate bureaucracy (see, for example, Weingast and Moran 1983). In the process of building such models, however, some of what we do know about bureaucratic behavior is cast aside. In seeing the bureaucracy as a black box, for example, some formal models ignore the self-evident political importance of bureaucratic decisions (Moe 1991). Some branches of economic theory stray far from practice. Many are hard to test empirically and even harder to follow as practical prescriptions. Lowi contends that they rob the study of administration of its very life and emotion (1992). The rational-choice theories do raise interesting questions often missing from more traditional approaches.

Potentially useful cross-fertilization rarely occurs, however. Those who promote rational choice view the methods pursued by other approaches as inadequate. Students of other approaches argue that the assumptions of the rational-choice models are untenable.

The rational-choice branch of economic theory has produced a schism between traditional public administration and newer approaches in political science. In part, this is because economic theory of bureaucracy begin from assumptions of rationality and highly structured relationships that traditional public administrationists find implausible. In part, this is because the abstract mathematical language of formal economic theories is inaccessible to many scholars trained in traditional public administration. Most of all, this is because the formulations, while elegant, speak little to the politics in administration. Having struggled for generations to escape the simplistic allure of the politics-administration dichotomy, public administrationists have struggled ever since to combine administration with a theory of politics. The formal theories run in the opposite direction.

The other branches of economic theory, notably transaction-cost economics, as well as its close cousins in institutional theory, offer far more promise. They have a rich sense of the complexity and uncertainty of the administrative environment. These theories are still underdeveloped, and their full potential has yet to emerge. Because they are fundamentally Madisonian, moreover, they contrast with the Hamiltonian vision of much of traditional public administration. The often stark differences between the two visions sometimes make conversations difficult, but the insights of the theories suggests that the conversation is well worth pursuing.

Public Bureaucracy

Although the behavioral revolution shook political science to its roots, it never completely obliterated the study of public administration within the discipline. A small, eclectic, but gifted group of political scientists have since focused centrally on the role of bureaucracy in government. They concentrated not on administration but on bureaucracy, not on agencies but on institutions, not on micro-principles but on mega-phenomena. Norton Long's classic article, "Power and Administration" (1949), is the classic text for the public bureaucracy movement: It asserts that the influence of bureaucracies grows from their ability to build political support.

From Long's foundation came Theodore J. Lowi's challenge to create a "juridical democracy" designed to replace the power that the interest-group/bureaucracy alliance had forged (1969). Hugh

Heclo elegantly traced the linkages between political executives and the rest of the Washington establishment (1977). He warned that a "government of strangers," based in the very structure of American bureaucracy, posed serious problems for government performance. Francis E. Rourke found in the bureaucracy's attentive and mass publics the source of much independent bureaucratic power (1984; see also 1991). The public bureaucracy movement is defined preeminently by James Q. Wilson's synthesis, *Bureaucracy* (1989), as well as by his earlier work (1967, 1973, 1978). Supporting Wilson's view is work by his students and colleagues, such as books by Derthick (1990), DiIulio (1987), and Kelman (1990).

Three basic principles define the public bureaucracy movement. First, public bureaucracy theorists assert that bureaucracy matters. For traditional students of public administration, this assertion often seems strange. Woodrow Wilson's fundamental argument a century before had been precisely the same, and public administrationists had worked hard to develop its implications. Public bureaucracy scholars, however, are writing not for public administrationists but for other political scientists. As political science moved away both from the study of institutions, the public bureaucracy movement has labored to remind political science of the importance of each. Unlike public administration, its proponents are devoted to theory, not practice.

Second, they place bureaucracy within a network of political forces. Heclo's theory of "issue networks" is a classic statement of how bureaucracies are woven into the cloth of interest-group politics (1978). These networks provide independent sources of political power for bureaucracies. This argument challenges traditional public administration, which sees the delegation of authority from elected officials as the foundation for administration.

Third, they assert that bureaucracies are important because of their role in the state. They are institutions with independent sources of power and influence on the conduct of public policy. The public bureaucracy movement, in short, seeks to use a base in political science to explain the growing power of bureaucracy in the United States. The movement resonates with a troubled note: How, given bureaucracy's independent power, can it be made accountable in a democracy?¹³ The intersection of these two issues -- the independent *political* power of bureaucracies and the legal base of bureaucratic power in *law* -- has helped fuel a rebirth of interest in administrative law. The issue was an important field in traditional public administration, but with work by Mashaw (1983, 1985) and Melnick (1983), for example, it has produced important new insights into the challenges that such independent political power poses

for bureaucratic accountability (Finer 1941; Friedrich 1940; Burke 1986).

The work of the public bureaucracy scholars builds on many of the same fundamental issues raised by early public administration scholars. In writing about James Q. Wilson's *Bureaucracy*, however, David H. Rosenbloom notes that "in an odd way, his intellectual path and that of public administration seem to parallel each other more, and to intersect less, than one might reasonably expect" (Rosenbloom 1991, 191). In fact, many public administrationists fail to recognize and cite the work of the public bureaucracy scholars. They, in turn, often self-consciously separate themselves from traditional public administration by writing under the banner of bureaucracy, instead of administration (Hill 1992b).

The differences between bureaucracy and public administration are at once profound and trivial. At its most basic, the difference is between Madisonian and Hamiltonian views of American politics that are rooted in the Federalist papers. In *Bureaucracy*, for example, Wilson self-effacingly suggests to his readers that, if they "want to get immediately to the 'bottom line,'" they "can spare themselves the hundreds of pages that follow and turn immediately to Federalist Paper number 51, written two centuries ago by James Madison" (p. 28). Federalist 51, of course, is Madison's famous exposition of the fundamental principles at the core of republican government. These principles about power relationships in a democracy lie at the core of the theory of public bureaucracy. Traditional public administration, on the other hand, builds on Hamilton's argument for a strong executive.

The public bureaucracy movement, furthermore, can never quite embrace public administration because public administration can never quite seem to escape the old politics-administration dichotomy. Public bureaucracy begins by stipulating the essentially *political* nature of bureaucracy, while vestiges of the *instrumental* nature of bureaucracy -- the use of administrative tools to reach certain policy ends -- lurk in every cranny of public administration. This conflict is one which the two approaches have never managed to reconcile.

In *Bureaucracy*, Wilson contends that if we want to learn about government agencies, we must stick "as close as possible to what actually happens in real bureaucracies" (p. xii). This feet-on-the-floor orientation separates public bureaucracy theory from many of the economic theories and links it with public management, in particular. To learn what happens, Wilson favors a bottom-up view. "By looking at bureaucracies from the bottom up, we can assess the extent to which their management systems and administrative arrangements are well or poorly suited to the tasks the agencies actually

perform" (p. 12). Organization matters because it determines the relationship between government agencies and their clients and, in the end, the success of the agency. Complicating this relationship, however, are the difficulties in managing the functions of and relationships between different levels of the bureaucracy — operators (the rank-and-file employees), managers, and agency executives. The behavior of bureaucrats at each of these levels, and thus the agency's overall performance, are the result of incentives created by both the organization's structure and political influences from the outside. Structure matters, but political power matters more (Nivola 1991).

For many students of bureaucracy, therefore, the study of public agencies is most fascinating because it serves as a prism for the central questions Madison raised in *Federalist* #51. Government performance matters, but the study of performance sits uneasily beside a fascination with the great forces of American politics. It is the former issue that defines the common ground between bureaucracy and public administration. It is the latter that defines the differences.

The bureaucracy approach sees administration in an untidy, pluralist way. It also fears the accumulation of bureaucratic power and worries about the effects of such power on bureaucratic effectiveness. Like some economic theorists, in fact, public bureaucracy scholars worry that the incentives of the political system pervert bureaucratic performance. They often end up favoring many of the same prescriptions — such as privatization, decentralization, deregulation — if for different reasons (Wilson 1989; Derthick and Quirk 1985; Kelman 1990). Bureaucracy might be troublesome, but "you can have less bureaucracy only if you have less government. Many, if not most, of the difficulties we experience in dealing with government agencies arise from the agencies being part of a fragmented and open political system," Wilson concludes (1989, 376). In fact, when problems of bureaucratic performance arise, they tend to be due not to bureaucratic malfeasance but to fundamental problems of governance in American democracy (Wilson 1989; Derthick 1990).

The bureaucracy scholars have thus brought the study of administration to a position both ultimately consistent with and fundamentally in conflict with public administration's early theorists. Like Wilson and Goodnow, they argue for the importance of administration, both for making democracy work and for shining a light on democracy's critical processes. Unlike the early theorists, however, they contend that bureaucracy can be understood and can function only as a piece of the larger fabric of politics. One can no more separate politics from administration than one can

separate, say, politics from Congress or the presidency. Thus, despite their close links, the fundamental Hamiltonian-Madisonian differences segregate the two approaches from each other and hinder useful cross-fertilization that might otherwise occur.

Strange Bedfellows

This review of administrative theories reveals two features. First, the fundamental issues to which each approach has devoted itself have been remarkably the same. Each approach has sought to reconcile the different sources of administrative power: delegation, how elected officials grant bureaucrats the authority to make decisions on their behalf; bureaucratic expertise, how professional know-how gives administrators authority independent of delegated power; and politics, how relations with outside forces can give bureaucrats a source of power independent of the others. Scholars who have studied administrative systems in other nations have come upon the same basic questions. As Aberbach, Putnam, and Rockman found in a major survey of western democracies, the relationship between party politicians and professional bureaucrats, in fact, stands at the center of many issues in the modern state (1981). Bureaucratic power depends on the relationship between outside groups and government officials, as Crozier discovered in his study of French bureaucracy (1964; see also Riggs 1964, 1991; Heady 1979; Peters 1989, 1992). Different historical and constitutional traditions in different countries, moreover, significantly affect the role of the executive (Rohr 1991, 1992).

Second, even though the basic questions are similar, there has been remarkably little accumulation of knowledge. The reasons are complex. In part, public administration's original focus on the politics-administration dichotomy has struck many scholars from other approaches as completely unrealistic. Because they focused on the first formulation instead of subsequent revisions, these scholars dismissed much of what public administration had to say. In part, public administration's relatively soft qualitative methods struck scholars trained in statistical methods and formal modeling as woefully inadequate. In part, some scholars rejected public administration's effort to link theory and practice. They believed that important mega-theoretical issues, such as the role of administration in the state, were neglected in the push to find practical solutions to management problems. They also suspected that practical training was not so high a calling as theory building. In many universities, the pursuit of theoretical elegance has often received more support than the more mundane

training of future public servants. Traditional public administration has suffered because its pragmatic interests have often been seen as low-status, low-priority items.

Finally, the fundamental Hamiltonian/Madisonian differences underlining many of the basic disputes in the field have rarely been confronted. Some approaches, like traditional public administration and public management, have in Hamiltonian fashion sought to discover how to make administration more effective. Other approaches, notably bureaucratic politics, implementation, and some parts of the economic and public bureaucracy theories, have been far more concerned about the risks of administrative power and have sought to contain it. With such basic disagreements, cross-fertilization has proven difficult, even though the approaches have much to say to each other.

As a result, the public policy school version of public management and the public bureaucracy approach developed independently, even though they were using many of the same methods to investigate many of the same questions of traditional public administration. Traditional public administration, meanwhile, meandered somewhat aimlessly even though some of the formal economic approaches and, especially, institutional theory offer extremely useful answers to questions that have hamstrung public administration for a generation.

Cross-fertilization among these approaches is critical. It can avoid needless repetition. It could add rigor to those parts of administrative research, especially in parts of public management and public administration that have been criticized for their methodology. It could provide fresh answers to old questions. It could help clear up internal contradictions that muddy policy prescriptions. For example, economic theories lead in different directions: toward greater reliance upon competitive markets and decentralization, in principal-agent theory; and toward more centralization and less reliance upon external markets, in transaction-cost analysis. Most important, it could point out the blinders that ageless assumptions, especially those coming from Hamiltonian or Madisonian predispositions, create.

The third stage of implementation research (Goggin, Bowman, Lester, and O'Toole 1990) provides a most useful model for cross-fertilization. In early implementation research, as in most of the public management and some bureaucratic politics literature, bureaucracy was yet one more impediment to success. Innovative researchers have begun taking a more positive view of bureaucracy's instrumental role and have tied it more closely to the course of program implementation. Through game theory and other creative techniques, the second generation of implementation theory has begun moving the approach from its preoccupation with failure to the search for sources of success. Its successes have been built on a careful integration of basic questions with

a survey of what can be gleaned from public administration. This model promises important implications for other approaches as well. Scholars engaged in this form of research have much to learn from public administration — and public administration has much to learn from them.

Shared Questions

Despite the differences among these approaches, each approach tends to ask questions closely related to the others: How can we design organizational structures that produce the desired results efficiently? How can we create incentives for bureaucrats to do the same? How can we make good decisions within bureaucracies and gauge the results of those decisions? What are the networks of organizations — and of political forces — that shape administrative behavior? And, ultimately, how can that behavior be made accountable to politically responsible officials? These questions lead to important insights.

1. *The difficulty of reconciling traditional public administration with political science.* As long as the discipline has existed, public administration has not occupied a stable position. Part of the reason is the conflict between education for the public service, which has always preoccupied public administration, and the more theoretical and academic concerns of political science. More fundamentally, however, public administration and public management have been based in the Hamiltonian tradition, while the other approaches have grown from the Madisonian perspective. Much of the conflict between political science and public administration, including the public service education issue, can be traced to this critical difference.

2. *Different approaches for different problems.* The administrative literature has been a contentious one, with different approaches vying for the prime role. The analysis of the role and heritage of bureaucracy in each model, however, suggests that different approaches to administration yield different answers that are appropriate for different problems. From an overall "systems" point of view, public administration and public management have focused on the organization, either looking predominantly inward (public administration) or outward (public management). Implementation research, by contrast, tends to focus on policy outcomes. Bureaucratic politics, economic theories, and bureaucracy seek linkages between the bureaucracy and the wider political environment. Each approach has its primary value in attacking a characteristic set of questions. Put differently, different questions are best answered by different approaches.

3. *Combination of approaches for crosscutting questions.* Most important and interesting administrative questions, of course, do not reside neatly within one of these cells. Rather, administrative problems increasingly spill across two or even three of the issues. That leads to a clear conclusion: obtaining a full picture of administrative issues usually requires several different analytical approaches. One approach has the virtue of asking an important question that other approaches would neglect.

4. *Combining approaches rarely produces stable answers.* Our experience, both pragmatically and theoretically, is that such cross-fertilization is difficult. Each major approach has taken great pains to separate itself from the others. Just as the Hamiltonian and Madisonian traditions have long struggled for the soul of American politics, it is unlikely that competing approaches to administration based on these disparate traditions are likely to find an easy accommodation. Cross-fertilization among the approaches is essential; common ground among them is unlikely. From the inevitable conflict, however, can still come useful insights, just as the struggles between Hamiltonian and Madisonian traditions have yielded useful -- if often untidy -- patterns of governance.

5. *Profound normative and educational differences.* The inevitable struggles among the disparate approaches produce starkly different normative prescriptions, from strengthening bureaucracy to weakening its hand, from encouraging market competition to improving the power of government executives. These normative differences likewise produce widely varying approaches to education. Some approaches, especially the organizational ones, seek to give clear guidance to impart to future government managers. Others, such as those focused on the political environment, seek theoretical understanding of why bureaucracies behave as they do, but are largely divorced from public service education. Even approaches pursuing the same goal -- notably the organizational approaches of public administration and public management -- have sharply different views of what is important, how what is important can be learned, and how to transfer that learning to students. Cross-fertilization undoubtedly would bear much fruit, but the search for common normative or educational ground is likely to be contentious. Subdisciplines, as well as whole professional schools, have been founded on the principle of exceptionalism, not commonality. That fundamental self-definition would be hard to shake.

The Politics-Administration Dichotomy Reinterpreted

These comparisons and contrasts mirror the ancient dichotomy between politics and administration on which the field cut its teeth. The study of administration, of course, has moved far beyond the early search for a clear line between politics and administration -- that is, for a Hamiltonian vision based on bureaucracy as an independent actor. In discarding that well-meaning but poorly focused search, however, other approaches have risked committing the same error. Within the rich politics of the American political system, it is no more possible to solve the problem of bureaucracy by embracing the Madisonian tradition than by following the Hamiltonian vision. Americans' strange love-hate affair with bureaucracy is the direct result of America's conflicting traditions about what role bureaucracy ought to play. We want bureaucracy to be powerful enough to do -- effectively and efficiently -- what needs to be done. We also want its power carefully constrained so that it poses no threat to personal liberty (Goodsell 1985).

The academic study of administration began by asserting that the phenomenon was worthy of study in its own right. In making the case, early public administrationists overdrew that argument by ignoring important complexities, especially Madisonian influences and the effects of other political forces on bureaucratic power. The reform efforts, however, sometimes replicated the mistakes of early public administration by ignoring the Hamiltonian traditions and the independent power of bureaucracy. Having solved the first problem of Wilson, Goodnow, and the other early founders of public administration -- that administration had become too important to ignore -- public administration could not solve the second -- the troubled relationship between administration and politics. Administration is both a dependent and an independent variable, a product of Hamiltonian and Madisonian traditions. It is virtually impossible to study administration without viewing administration as the result of one of the competing political traditions. It is certainly impossible to produce an adequate theory based on one set of choices. Thus, only a catholic approach, fully informed by the cross-fertilization of competing traditions, is likely to advance our understanding of administration. That is the lesson that the struggles over the politics-administration dichotomy ultimately teaches.

Notes

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1. In this paper, I will use "bureaucracy" to refer to the government's administrative apparatus. "Administration" will include "bureaucracy" as well as the broader political and social processes surrounding it.

The differences between Hamilton and Madison's approaches are reflected in basic tensions between Hamiltonian and Jeffersonian administrative theories. For a fascinating look at this question, see Caldwell (1988).

2. Readers interested in comparing other analyses of the state of administration can consult Chandler (1987), Perry (1989), Lynn and Wildavsky (1990), Stillman (1991), Rainey (1991), and Hill (1992a).

3. The first three of these historical periods match an analysis developed by Martin (1952).

4. Not everyone, however, credits Wilson with the influence usually ascribed to him. See Van Riper (1987). The best guide to this, and most other questions involving the literature of public administration, is Martin (1989).

5. They were, in addition to Goodnow, A. Lawrence Lowell, Woodrow Wilson, W.W. Willoughby, and Ernst Freund.

6. Members of the staff included: Joseph P. Harris, director; G. Lyle Belsley; A.E. Buck; Laverne Burchfield; Robert H. Connery; Robert E. Cushman; Paul T. David; William Y. Elliott; Herbert Emmerich; Meric Fainsood; James W. Feaster; Katherine Frederic; Patterson H. French; William J. Haggerty; James Hart; Arthur N. Holcombe; Arthur W. Macmahon; Harvey C. Mansfield; Charles McKinley; John F. Miller; John D. Millet; Floyd W. Reeves; Leo C. Rosten; Spencer Thompson; Mary C. Trickett; Schuyler C. Wallace; and Edwin E. Witte (see President's Committee on Administrative Management 1937, viii; and Feaster 1987).

7. For a useful catalog of the enduring administrative principles, see Hood and Jackson (1991).

8. In fact, there already was a small but important literature on implementation when Pressman and Wildavsky wrote their path-breaking book. See Bailey and Mosher (1968) and Murphy (1971).

9. I am indebted to Charles O. Jones for finding and sharing this nugget.

10. A notable exception to the top-down approach is Elmore (1982).

11. These figures were calculated by the author from the index in Ripley and Franklin (1986).

12. I am indebted to Steven Kelman for making this point.

13. A very useful history of the public bureaucracy movement is Hill (1992b).

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