Political Science: The State of the Discipline

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AMERICAN POLITICAL PROCESSES AND POLICYMAKING

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The Scholarly Commitment to Parties

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"American political parties" are the subject of this "state of the discipline" essay. The continuing tradition that constitutes my theme is the preponderant scholarly commitment to the desirability of parties in a democratic society, and particularly in a society that may now reject them. Although I shall discuss chiefly the views of party specialists, their views in this respect have also long been held by political scientists in related areas of American and comparative politics.

Enough talent and industry have been engaged in studying American parties so as to produce many more impressive works than can be noted in any essay. I hope that the portion cited to illustrate my theme is fairly representative of our profession's customary treatment of parties within the general area of American government and politics. Comparative studies are mentioned only tangentially.

My scope is "parties" instead of "parties and other political organizations." The latter, as I shall suggest at the end, may reflect a more fully appropriate intellectual perspective. But I want to stress the special concern of political scientists for the role of parties even when they also write about interest groups. In any case, the boundaries of the party field are not very firm, but expansible into other fields of America politics like voting behavior and congressional behavior as well as interest groups.

The professional commitment of political scientists to the desirability of political parties has a substantial history. In fact, the scholarly commitment to parties is old enough to have been at odds with strong American political trends well before the party decline of the last few decades. I am thinking of the first ten or twenty years of the century when progressive reforms first began to diminish the power of party organizations and when a good deal of respectable public opinion already displayed a preference for nonparty politics (Shefter, 1978). At the time, political scientists themselves were among the reformers of old-style political organizations, but they did not characteristically settle, at least in national or state politics, for the nonpartyism of municipal progressivism (Hays, 1964). Specialists in municipal administration seem exceptional in their nonpartisan preferences. More impressive is the fact that certain leading scholars even at the turn of the century admired conventional American parties, objecting only to plainly corrupt manifestations while treating their non-ideological and decentralized features as suited to the constitutional and social order. From that standpoint, the old American parties were successful enough to justify preservation. These defenders of the established parties, while important and prestigious in American political science, have probably attracted less attention than have the scholars who emphasized the inadequacies of existing organizational structures and advocated the development of more effective parties. The latter, belonging loosely to the responsible-party or responsible-party government school of thought, have conducted a familiar debate with the defenders of existing parties.

The debate has been a meaningful one, not yet ended, but it is of less interest to me here than the underlying agreement that effective parties are desirable and probably essential in American politics as in democratic politics elsewhere. I treat the scholarly commitment to parties as more than a matter of specialists valuing any subject they have chosen to study. The case for parties has an intellectual basis that can be seen in the positive evaluations of parties by many less specialized political scientists. I begin therefore with a brief examination of that intellectual basis before discussing the historical positions of the defenders of conventional American parties and the advocates of responsible party government. Subsequently, I shall come to recent manifestations of the common tradition and to a question about its survival.

I. INTELLECTUAL BASIS

The scholarly commitment to the usefulness of parties has both empirical and theoretical sources. The empirical is apparent at the start. When the systematic study of American politics began in the late nineteenth and early twentieth centuries, parties were well-established institutions whose development much earlier in the century had coincided with an enlarged electoral franchise. Parties thus appeared to be products and agencies of American democracy. Although seldom the subject of academic inquiry before the last decades of the nineteenth century, their importance was recognized once political science emerged as a separate discipline around 1890 or 1900. Accordingly, while Bryce's substantial description of parties was famously innovative, it was not long alone in its appreciation of parties as "the great moving forces" of American government (1891, p. 5). Like Bryce, many American scholars of the period thought that American parties were too strong, notably in their control of patronage, and too unprincipled. But a desire to purify parties was compatible with a belief that reform could make parties more useful. Even the direct primary, for the political scientists who accepted it, was not a means to destroy parties.[1]

To be sure, the acceptance of parties in political science was at first less enthusiastic than it became after the objectionable city machines had weakened. Nevertheless, the earlier scholars usually treated American parties as much more than necessary evils. The favorable tone is evident in the works of Wilson, Ford, Lowell, and Goodnow (Ranney, 1954). Moreover, in the early decades of the twentieth century, it was evident that parties accompanied democracy elsewhere and not merely in the United States. As Bryce wrote, "parties are inevitable. No free large country has been without them. No one has shown how representative government could be worked without them. They bring order out of the chaos of a multitude of voters" (1921, Vol. I, p. 119). This is the view of parties that has subsequently been dominant in our profession. Neither Croly's belief that we could do without parties, despite their previously useful services, nor Ostrogorski's belief that a true democracy should do without them, has commanded a large scholarly following.[2] The rejection of Ostrogorski's anti-partyism, despite respect for his detailed study of British and American parties, is most telling. In Macmahon's authoritative words, Ostrogorski's approach reflected a "vast naivete" (1933). Charles Merriam also specifically rejected Ostrogorski's views in his parties text (1922, pp. 380-81), and spoke for his fellow scholarly practitioners when he said that party "is one of the great agencies through which social interests express and execute themselves" (p. 391).

The observable fact that parties have everywhere developed when a large population participates in competitive elections, though it helps to explain the original commitment of political scientists, does not provide a fully satisfying theoretical basis for defending parties against their critics. Such a basis, however, soon developed, and by the middle and late years of the twentieth century it underlay the several otherwise differing arguments for parties. Specifically, the political scientists' case for parties involves a theory of democracy sharply different from the individualism that determined Ostrogorski's anti-partyism. Insofar as individualism, identified with direct popular government and sometimes with Rousseau, is regarded as the classical democratic theory, those who depart from it are said to be revisionists seeking to reconcile popular participation with respresentative institutions, intermediary groups, and regularized leadership. The word "revisionists" suggests a dilution of democratic ideals even when it is not prefaced with the pejorative "elitist." Naturally, however, those who are called revisionists do not believe that they are diluting democracy. Rather, they conceive of collectively organized representation as the only means by which the will of the people can be made effective. Town-meeting style democracy they see as impractical in a large national or regional community, and strictly individual representation in legislative bodies as less responsive to popular majorities than party representation. The latter tends to be built around a kind of program or set of policies, but the most severe revisionist might be content with a party competition involving no more than a choice of leadership teams without clearly differentiated principles (Schumpeter, 1950, p. 283). In either view, most citizens are assumed to play useful political roles by exercising a choice at periodic elections as long as their choice is made meaningful by party labels. Many citizens may also participate in party candidate-selection and policy-making, as some pro-party writers advocate, but such participation, whatever its advantages or disadvantages, is not required in all versions of the democratic theory justifying parties. The essence of that theory is that voters should be able to choose between recognizable competing leadership groups.

By mid-century, the theory was explicit in influential comparative studies whose treatment of parties, especially of strong British-style parties, was highly favorable. Finer (1949, pp. 274-282, 353-362), Friedrich (1946 pp. 257, 294, 347), and Neumann (1956, pp. 397, 421) are important examples. Parties, they asserted, were essential in order to organize effectively the multitude of diverse interests in a modern society. In particular, working-class interests needed such organization. Still more explicitly, Duverger perceived the scale, centralization, and discipline—even the oligarchical character—of the twentieth century's mass parties as well-suited to political purposes in contemporary society (1954, p. 427).

Significantly, neither Duverger nor most other political scientists, American or European, thought of interest groups as sufficient agencies of collective representation. They have not always attacked interest groups as overly powerful and narrowly self-serving, but even when recognizing them as legitimate and useful in a political system, they seldom regard them as adequate mobilizers of majority preferences on behalf of broad public purposes. Interest groups, often studied along with parties, retain much of the stigma of "factions" in relation to the general interest. On the other hand, parties, in political science, are distinguished from factions because of the broader purposes and roles that they are supposed to fulfill. The point is now spelled out most sharply by Sartori (1976), but it has been embedded in much of twentieth-century political science. A party, Sartori writes, is "a part of a whole attempting to serve the purposes of the whole, whereas a faction is only a part for itself" (p. 25). For Sartori, it is true, faction is by no means synonymous with interest group; rather he uses the term in an older and more limited sense. But what he says about party in relation to faction asserts the superiority of party to any more particularized group. That superiority follows from Sartori's definition of party as functionally meant to serve the whole community. He believes that party serves that purpose not so much by representation as by its provision of channels of expression. Parties, in his view, transmit demands backed by pressure (p. 28). So, it can be said, do interest groups. But parties, it is thought, are motivated to relate demands to public purposes if only because parties, by their nature, must seek broad electoral support.

Important though Sartori and other comparativists have been in stating the theoretical basis for the scholarly commitment to parties, similar views may be found in works limited to American politics. I cite here one leading and prestigious example, Robert Dahl. Writing not as a party specialist but as an analytical theorist in his American government text, Dahl had no doubt that parties made substantial contributions to the operation of a democracy like that of the United States. They facilitated popular control over elected officials, helped voters to make more rational choices, and aided in the peaceful management of conflicts (1967, p. 243). These contributions were not to be found in the internal affairs of parties but in their external effects—among which was their assistance for "the many to overcome the otherwise superior resources of the few" (p. 250). The last point is an important one for believers in the democratic virtue of parties. They customarily see the mass of people, and notably poor people, as underrepresented in interest-group politics, and also likely to be underrepresented in an electoral politics conducted exclusively in terms of individual candidacies. Only by collective and organized action, meaning in practice a party capable of mobilizing majority support, can the many obtain governmental policies suiting their interests. Most commonly, that view is advanced by those who hope for a more coherent majority party than Dahl's own pluralist approach provides (James, 1974, pp. 6, 260-262). But Dahl, though far from denying either the legitimacy or the efficacy of the diverse interests that parties seek to combine in winning electoral coalitions, nevertheless shares with more majoritarian-minded advocates of parties the assumption that parties have a special value for those people whose own individual political power is limited.

There may be some quarrel with the adequacy of the intellectual case that political scientists expound in behalf of parties in a democratic society, but there should be no doubt about its widespread scholarly acceptance. As Samuel Eldersveld wrote in his major study of party organization: "Intellectually, we have become committed to the position in the twentieth century that parties are central to our system. . ." (1964, pp. 20-21). His page of quotations from eminent scholars makes it clear that the "we" of his sentence means political scientists.

II. DEFENDERS OF INDIGENOUS INSTITUTIONS

Constituting one of two loosely defined categories of pro-party political science, the defenders of conventional American parties respect the indigenousness of the very characteristics that believers in more responsible governing parties find objectionable. Decentralization, limited ideological or programmatic appeals, porousness, relatively noncohesive legislative contingents, and the absence of mass-membership organizations are appreciated as natural in American political development and often also as party qualities especially useful in the social and constitutional circumstances of the United States. Even the patronage on which American parties were originally built has occasionally been defended except in its plainly corrupting forms. Of course, not every defender of American parties admires all of their various characteristics. Some are critical enough so that their inclusion among the defenders might be questioned. The categorization is necessarily rough here as it is for the responsible-party government school, whose ranks include advocates of greatly varying degrees of change in existing party characteristics. And at least one important parties specialist, V. O. Key, is so hard to classify that he may deserve a category of his own.

In treating the appreciators of American parties, it is important to emphasize at the outset that their favorable opinions are based on the historical roles of parties in the nineteenth century and the first half of the twentieth. The defenders derive no satisfaction from the widely perceived recent decline in party efficacy, nor from the results of old or new progressive reforms designed to weaken parties. These are not the developments regarded as well-suited to American circumstances although they too might strike us as indigenous. No believer in the usefulness of parties applauds their decline and possible demise. Although the defenders of conventional American parties think that we cannot have (and often that we should not have) strong parties resembling foreign models, they definitely want parties that perform at least as well as those they have observed in American history.

The political scientists' defense of American parties, like their positive evaluation of parties generally, coincides with the emergence of relevant scholarly study near the turn of the century. Admiration for British parties, though established early among intellectuals, did not lead every scholar to follow Woodrow Wilson in urging that something like those parties be substituted for the American model. Even A. Lawrence Lowell, who shared Wilson's belief in the superiority of Britain's more responsible parties, thought the less cohesive American parties better suited to the American constitutional preference for a government in which no majority, acting through a cohesive party, could gain complete control (Ranney, 1954, pp. 48-69). Fuller praise of American parties came from Henry Jones Ford (1898), who can be regarded as the founder of the twentieth-century defense of American parties. Although Ford, like Wilson and Lowell, was critical of the limited degree to which American parties assumed policymaking responsibility, his hopes for developing that responsibility did not preclude an emphasis on the considerable accomplishments of the country's historic parties. Sharing the view, also held by Lowell and Wilson, that American parties owed much of their special character to the separation of executive and legislative powers established by the Constitution, Ford argued that parties in fact made that difficult system work. They bridged, to a useful extent, the separation. The extensive extragovernmental apparatus of the nineteenth-century boss-run machines impressed Ford as understandable given the circumstances in which parties had had to develop in the United States.[3]

Ford's emphasis on the usefulness of traditional party organizations became a familiar theme for many political scientists as those organizations slowly declined in the early twentieth century. This was notably so for a relatively conservative defender of American parties like E. M. Sait. In the first edition of his text (1927, pp. 159-164), Sait quoted Ford approvingly with respect to the success of American parties and, in a section labeled the "Peculiar Importance of American Parties," praised their accomplishments in harmonizing organs of government, enabling the electorate to function, advancing national unity, checking religious intolerance, assimilating foreign elements, softening the clash of economic interests, and adopting similar consensual platforms. Like Ford, Sait was skeptical about the claims for the direct primary, but after its first two decades of operation, he did not view its impact as so destructive of party responsibility as political scientists would later more often discover it to be. Nevertheless, Sait was already concerned with the apparent decline of the old organizational loyalties, whatever the cause. The decay of partisanship, he said, "cannot be viewed without disquiet. The democratic regime can no more function satisfactorily without strong parties than parties can function without strong organization" (p. 373).

By the 1920s, it was not only the formerly much criticized organizational strength of traditional American parties that non-reformist political scientists appreciated. The absence of well-defined distinguishing principles, as between the major parties, now also became a kind of virtue in the American system. For example, Sait included the moderating and unifying contributions of American parties among the praiseworthy accomplishments already cited. Similarly, Arthur Holcombe, in his well-known exposition of American parties (1924), responded directly to the "empty bottles" criticism that went back at least to Bryce. To have any chance of winning elections in the diverse American nation, parties needed to make broad rather than particularized appeals, such as those of an exclusively capitalist or socialist party. By their broad appeals in the United States, Holcombe said, parties tended to conciliate social strife. "The wide extent and diversified interests of the major parties," he added, "are the best guarantee which the people possess that power will be used with moderation" (p. 384).

In the political science literature of the next several decades, the defense of America's loose and compromising parties emphasized their decentralized, federative character as better suited to a pluralist society and to pluralist values than a more cohesive national party could be (Banfield, 1964; Grodzins, 1964). The general point was fully argued by Ranney and Kendall in their textbook's extended effort to present a theoretical basis for understanding mid-century American parties. Although the authors were attracted by the logic of absolute majority rule, which would in principle support a more cohesive national party, they were convinced that the present parties were "appropriate to the governing system the American people really want" (1956, p. 500).

With good reason, many defenders of traditional American parties found their case at mid-century most fully and persuasively stated by Pendleton Herring. In a sophisticated pluralist exposition of American institutions (1940), Herring—very much in the mainstream of postwar political science—appealed to many political liberals as well as conservatives. He treated the New Deal legislation of the 1930s as an accomplishment of existing institutions and as evidence of their workability. Herring saw in presidential leadership, within the existing structure, an effective means of obtaining positive action when required. "The New Deal," he said, "demonstrated that rapidity of action was possible within the limits of present institutions" (p. 421). Rather than deploring the fact that neither major party was a unified national entity, Herring found advantages in having only "a loose confederation of state and local organizations" (p. 245). He treated these organizations as meaningful, the professional politicians at their center as usefully in control, and their regular followers and supporters as significant despite—or because of—their diverse interests and principles.

So highly favorable a view of the performance of American parties was not characteristic of most of their defenders after World War II. Even scholars who shared Herring's evaluation of the usefulness of parties in earlier American experience, including the 1930s, often found contemporary parties inadequate as a result either of their decline or their failure to change sufficiently to meet new demands. The line between defenders of indigenous institutions and more-responsible party advocates becomes harder to draw. I do so hesitantly when I consider V. O. Key among the defenders. He is rightly well-known for his belief in strengthening and making more effective the

American parties that he studied so intensively and insightfully in the two decades after World War II. But he suggested no foreign models for American parties to emulate. Insofar as he had models, they appeared to be modernized variations of successful American parties of the fairly recent past. In fact, Key often deplored the twentieth-century decline in organizational strength, and most emphatically its absence in particular places. The sense in which he wanted parties to be responsible, or at least responsive to voters, was very much within the limits imposed by the constitutional structure. He did not advocate the full-fledged responsible party government that he thought unachievable in American circumstances.[4] Nor did he advocate a mass-membership party of a type largely unfamiliar in the United States. Rather, like most defenders of American parties, Key looked to the self-interest of party leaders to produce a responsiveness to the electorate.

Indeed, it was crucial in Key's approach that there should be party leaders and specifically more than one set of leaders—that is, competing teams. In the American context, that meant two parties rather than one. His most famous study, of the South (1950), is an extended criticism of the shortcomings of traditional one-party politics in which voters lack the clear-cut alternatives posed by competing teams bearing different party labels. Factional conflicts within parties, though fought out in direct-primary elections, do not provide a satisfactory continuity for voters or a sufficient association between gubernatorial and legislative candidates. One-party politics is issueless and also, most significantly, without a plain electoral choice between well-defined groups of politicians, the ins and the outs. In the long run, Key believed, the have-nots in a society were the losers from a politics so disorganized as to preclude their effective influence.

Similar themes are stressed in Key's more general study of state politics. For the North as well as the South, party competition is the best means of enforcing accountability. Key associated its absence in certain northern states with the decay of party organization. The vigor of two-party competition had declined, he thought, during the first half of the twentieth century. One cause is the direct primary which often, Key believed, transfers competition from an inter-party basis to a less meaningful level of personalities. "The new channels to power placed a premium on individualistic politics rather than on the collaborative politics of party" (1956, p. 169). Here as in his other works Key reflected a good deal more dissatisfaction with existing American parties than is customary among their defenders. And, while he regarded them as having been more effective in the past, his praise for their historical record was unenthusiastic. On the whole, Key is best understood as defending traditional American parties only in the sense that, when organized and competitive, they were the best that we were likely to have. He found advantages in two-party competitive states and in such national two-party electoral competition as occurred despite the disadvantages of decentralized confederative organizations (1964, p. 334). But Key did not proclaim any virtue for the limits in strength and coherence that flowed from the indigenous development of American parties. Instead, he merely accepted these limits, or most of them, as painful necessities.

III. ADVOCATES OF RESPONSIBLE PARTY GOVERNMENT

Political scientists whom I categorize as advocates of responsible party government are chiefly distinguishable by their desire for American parties to transcend the limits associated with indigenous institutions. Unlike defenders of traditional parties, who accept those limits either enthusiastically or resignedly, members of the party government school argue for parties that would assume collective responsibility for governmental policymaking in a manner so far uncharacteristic of American experience.[5] Although they might praise American parties of the past as much better than no parties at all, the advocates of party government do not regard them as satisfactory models. They seldom suggest entirely new parties, but they want to transform the existing Republican and Democratic parties into essentially different political institutions. In their minds, a democratic society requires not merely parties of one kind or another, but two strong and cohesive parties each offering the electorate policy commitments which it could fulfill after winning government offices.

Party responsibility thus has a special import here that differentiates it from the ordinary meaning of the phrase. After all, the defenders of traditional parties—indeed, all believers in the usefulness of parties—prefer responsibility in some sense. But the advocates of party government want parties to be so much more responsible as to make for a difference in kind and not merely in degree. They sharpen this difference both by a thorough-going criticism of existing parties and by arguments for basic changes. These arguments are often cast as moral imperatives for overcoming the American institutional and social obstacles in the way of effective parties. The transformation that party government advocates have in mind is principally at the national level. Although they also deplore the frequent absence of party responsibility in state and local politics, their emphasis is on the weakness and incoherence of national parties. Attention is fixed on the inability of either Republicans or Democrats to promise and deliver policies through party control of Congress as well as of the presidency. They want *national* party government.

With its emphasis on national politics, the party government school has existed as long as American scholars have studied parties. Woodrow Wilson, early in his academic career, propounded a version of the doctrine, specifically drawn from the British model, and others near the turn of the century were attracted by it (Ranney, 1954, pp. 25-47). Its more recent presence is heavily identified with E. E. Schattschneider, a champion of stronger parties from the 1930s through the 1960s. As he said of himself, "I suppose the most important thing I have done in my field is that I have talked longer and harder and more persistently and enthusiastically about political parties than anyone else alive" (Adamany, 1972, p. 1321).[6] During his career parties declined rather than becoming stronger, as he had hoped, but Schattschneider did not lack academic followers or influence. If for a time these views had less prestige among political scientists than those of pluralist defenders of existing parties, they were never disregarded and their popularity revived in the late 1960s and 1970s.

That popularity may be partly attributed to the sharpness with which Schattschneider stated the majoritarian premise of the party government position. Unlike his pluralist opponents, he was not satisfied with direct interest-group representation and with parties that sought only to accommodate diverse interests in coalitional representation. Schattschneider assumed that there was a legitimate and definable majority interest that a party should mobilize and represent. Hence, he put into the conclusions of his best-known book: ". . . party government is good democratic doctrine because the parties are the special form of political organization adapted to the mobilization of majorities. How else can the majority get organized? If democracy means anything at all it means that the majority has the right to organize for the purpose of taking over the government. Party government is strong because it has behind it the great moral authority of the majority and the force of a strong traditional belief in majority rule" (1942, p. 208). The majority, in Schattschneider's writing, has a more public purpose than do minority interest groups, and its superiority in this respect derives from its inclusion of the great mass of people whom he believes to be unorganized and unrepresented by pressure groups (1960, p. 35).

Interestingly, Schattschneider did not seek to strengthen American parties by participatory reforms. He favored neither the direct primary nor any other large-scale democratizing of party organizations. Instead, he accepted, much as did Key, the sufficiency of the competing-teams conception of party. "Democracy is not to be found *in* the parties but *between* the parties" (1942, p. 60). Voters need not be burdened with party affairs. Nominations could be settled by organizational activists and by the leaders themselves. So could party policies and issue-positions. Voters would be well served if given a choice between sets of leaders, each united in its commitment to a party and its program. The trouble was that such a choice did not really exist in American politics. Neither the Republicans nor the Democrats presented coherent leadership teams. Thus the two-party system did not fulfill its advantage by way of a real majority winner. Neither party's majority was cohesive enough. The apparent success of major American parties in electing candidates was not followed by their effective mobilization to govern as a united force (1948, pp. 29-30).

Schattschneider, in his own writing, provided little by way of blueprints for change. But there was one prominent theme: build a national party leadership that would not be dependent on state and local politicians. Decentralization of party authority was the enemy of responsible-party government at the national level. The major American party, as "a loose confederation of state and local bosses," meant agreement for but limited purposes—chiefly patronage (1942, pp. 132-133). A party built around state and local bosses lacked policy coherence. Most emphatically, Schattschneider preferred a national leadership strong enough not only to control congressional party majorities but also to cut off patronage to local bosses. Before and after elections, presidential and congressional candidates would share genuinely public purposes. When Schattschneider spoke of party government, he meant party centralization (1942, p. 207).

Considerably more specific proposals for achieving party government are made in the famous report of the American Political Science Association's Committee on Political Parties (1950). Schattschneider chaired the Committee, and its report reflected his intense belief in the democratic need for two competing national parties to present alternative governmental programs. Although the report's proposals for intra-party democracy are at odds with Schattschneider's earlier acceptance of a policy-making leadership, many of its other detailed suggestions are consistent with his approach. So too is the report's basic and explicit assumption that parties can be made responsible without formally changing the U.S. Constitution (pp. 35-36). In that respect, the APSA Committee spoke for much of the responsible-party government school during the post-war years. This is not to say that it spoke for most political scientists or even most parties specialists. Despite the Committee's unusual status as an agency of the professional association, its report, published as a supplement to the APSA's journal, evoked immediate dissent from the defenders of conventional American parties. And the report was subsequently criticized because its advocacy lacked the kind of empirical support that political scientists came to expect in later decades (E. Kirkpatrick, 1971). Many of its proposals, however, continued to appeal to responsible-party advocates even in the 1970s. Interest in the report remained high (Pomper, 1971). Virtually all students of American politics at least agreed that effective parties of some kind were necessary, and many well may have preferred the Committee's kind of effective parties although they did not always share the Committee's belief that they could be established in the United States.

In its proposals, the APSA report linked the responsible-party doctrine to the participation of issue-oriented organized activists both in selecting candidates and in party policymaking. Activists could be expected to endorse and help to nominate candidates willing to carry out party policies that their organizations had developed. The participatory membership would be a means for achieving coherently responsible party government despite the direct primary and other institutional obstacles. Accordingly, Schattschneider's acquiescence in the Committee's organizational recommendation was not incompatible with his principal purpose. It is true that he had said, in his previously published work, that his purpose could be accomplished without intra-party democracy—that large-scale citizen participation in a party's internal affairs was not a democratic essential as long as citizens had a clear inter-party choice. But he might well have viewed participatory activists as genuinely committed party organizational members, not so far removed from smaller leadership groups. Party activists were very different from mere party voters whose membership Schattschneider had treated as a fiction created by primary registration laws. At any rate, the Committee's preference for organizational activism remained an important element in the advocacy of responsible party government. Also in the 1960s and 1970s similar large-scale citizen participation in party affairs was advanced even by scholars who otherwise disassociated their arguments for more effective parties from certain other aspects of the APSA report (Saloma & Sontag, 1973).

Another persistent theme in responsible-party advocacy is presidential leadership. For some scholars, a national party is essentially the president's, or the presidential candidate's. Congressional members and candidates are treated as members of his team. The nature of the presidential-party relationship, however, is not perceived to be the same by all believers in responsible party government. Woodrow Wilson, for example, came to put so much emphasis on a president's own popular mandate, received directly and individually from the electorate, that he can be charged with a departure from party government altogether (Ceaser, 1979, p. 197). After all, party government implies collective rather than strictly personal leadership. Thus, stronger parties are often specifically advanced as a means to secure effective government without a too-powerful president governing independently of party or of other intermediaries between himself and the people. For the responsible party school, the president is important as the leader of a team, but he is responsible to others on that leadership team as well as to the voters.

Among recent responsible party advocates emphasizing presidential leadership, Burns is preeminent in his concern with both a strong party and a strong leader, each complementing the other (1978, chap. 12). Burns seeks a leader capable of mobilizing a party majority rather than a personal, independent, or bipartisan majority. And he conceives of such a party majority as effective only if headed by a strong president. Specifically, Burns wants a presidential party able to lead a congressional majority: ". . . each presidential party must convert its congressional party into a party wing exerting a proper, but not controlling or crippling hold on party policy" (1963, p. 326). He would also have his presidentially-led national party build a mass-membership organization, but undoubtedly most of his hopes for party policymaking rest on innovative leadership to which party members would actively respond by way of campaign efforts to elect congressional candidates committed to the program of the president's party (1965, p. 111; 1972, chap. 8).

IV. MAINTAINING THE COMMON TRADITION

By discussing Burns and also a few other currently active political scientists, I have not entirely neglected contemporary scholarship within the historical context. But I want at this point to demonstrate the continuity of the scholarly commitment to parties by separately considering a small portion of the massive literature of the last two decades. The common ground now occupied by parties specialists will be more apparent than it was in the historical narrative. Differences between the two schools of thought surely remain, notably over their preferred remedies, but the differences tend to be overshadowed by a mutual dissatisfaction with the state of American parties in the 1970s and 1980s. Perhaps dissatisfaction is not the best word for the views of contemporary students who do not themselves deplore the ineffectiveness of American parties but merely describe it. Yet they too represent the pro-party tradition insofar as their description of present-day parties measures them by the standard of more effective models derived from the American past or from elsewhere. Shortcomings are suggested even without any overt advocacy. And often there is at least an expressed hope or a search for stronger parties.

A good deal of the continuity in the scholarly commitment is explicable in the most literal sense. Not only do certain important mid-century carriers of the tradition—Ranney and Burns, for instance—maintain their positions, but other slightly earlier figures—notably Schattschneider and Key—are followed by former students, disciples, and admiring professional colleagues. In parties textbooks as well as in monographic literature, Schattschneider's and Key's themes persist. To cite only two of many contemporary examples, the responsible-party model is central in Everson's analysis (1980), and it is prominent in Sorauf's well-established text (1980a). Neither author, it is true, is optimistic about the model's prospects, and in this respect the presentation is like that of many contemporary scholars who retain the traditional frame of reference but not the faith. Hence, their perspective is closer to Key's than to Schattschneider's. So too has Key been followed in the now standard use of the analytical categories of electoral, organizational, and governmental parties that he had popularized in his textbook (1964, pp. 163-165).

To display more specifically the continuity of Key's research interests, the readiest example is provided by the literature on party competition. His influence here is unusually direct in that for over two decades numerous talented and highly trained political scientists have examined Key's hypothesis that two-party competition produced superior policy outputs to those of a single party's personal factionalism (Lockard, 1968). Using increasingly refined quantitative methods, they compare inter-party competitive states and one-party dominant states with respect to welfare, education, and other policies—often counting redistributional results as signs of superiority. By no means has all of the research tended to confirm the hypothesis; a good deal of it leads instead to the belief that socio-economic variables account for more of the differences between state policy outputs than do strictly political factors. This point is stressed in a methodological critique that also usefully summarizes much of the large volume of relevant literature (Lewis-Beck, 1977). Nevertheless, the quest for statistical confirmation has not been abandoned. Even if it were, the established preference for party competition in state as in national politics would not also be readily abandoned. For Key's followers as for other scholars, inter-party competition has advantages that do not require statistical confirmation. It must seem almost self-evident that a democratic belief in the usefulness of parties means a belief in inter-party competition.

Moreover, among American parties specialists, this means that two-party competition, not multipartism, is the alternative to a one-party system. Unlike students of European politics, where multipartism either exists or is thought likely to exist, observers of American politics, now even more than earlier, confront situations in which third parties are only interesting and often temporary aberrations. Accordingly, the American classification of state party systems ranges from degrees of one-party dominance to degrees of two-party competitiveness (Jewell & Olson, 1982, p. 27). In that context, no more than in American national politics, is it relevant to contrast two-party competitiveness, favorably or unfavorably, with multipartism. Instead, implicitly if not always explicitly, the literature treats two-party competitiveness as a desirable norm in contrast to one-party dominance. The treatment is striking in work on the development of Southern Republicanism (Bass &

DeVries, 1976). In presidential and congressional elections, increased Republican voting may suggest the emergence of more meaningful national party alignments; and in state elections, the slower but visible growth of Republican strength suggests the possibility of finally ending a century of one-party Democratic dominance. Indeed, the new Republican strength in the South, fitting as it does a generally more national and less sectional voting pattern, is one of the few recent developments encouraging to believers in effective parties. Understandably, however, close observers of the South, like most students of American politics, remain cautious about the extent to which recent Republican votes represent new party commitments as opposed to a decline in Democratic commitments and another version of candidate-centered politics (Strong, 1977). Rather than a new electoral alignment, or realignment, is there in the South, and elsewhere, mainly a dealignment?

The question leads to the vast and imposing literature on voting behavior. Although that literature belongs in a substantial field of its own, some of it is produced by party specialists and a much larger share is used in studies of parties. From its early days, the most influential research group analyzing sample-survey data has focused attention on party identification as a prime determinant of voting behavior (Campbell, *et al.*, 1960). I deal but briefly with work of this kind because of lack of space, not lack of relevance. After all, the analysts of voting behavior have been the definers of the electoral party, or the party-in-the-electorate, which is of such great concern to American parties specialists. Voters' party preferences were, and are, observed from election returns and official state registration totals, but they are most familiarly determined by party identifiers in sample surveys. Each of the artifacts called a major electoral party apparently consists of a substantial percentage of voters who identify themselves as Republicans or Democrats in response to survey questions. The standard form of these questions is now open to significant criticism (Dennis, 1981), but the elicited responses provide a widely accepted record of several decades of "membership" in the major electoral parties. With about three-quarters of American voters thus identified as Republicans or Democrats as late as the 1950s and early 1960s, electoral parties looked impressive, relative to their counterparts elsewhere, even if American organizational and governmental parties might not (Converse & Dupeux, 1962). Despite critical deviations, most party identifiers, it could be shown, usually voted for candidates similarly identified.

To be sure, party mobilization of voters was demonstrably less complete than it had been earlier in American history (Burnham, 1965), but the party identification figures suggested considerable stability. Hence, it was their drop after 1964 that most saliently signified the decline of parties. By the 1970s, with only about two-thirds rather than three-quarters of the electorate regularly identifying as Republicans or Democrats, and with a concomitant decrease in straight-ticket voting, something important had happened to the parties that were familiar to mid-century political scientists (Nie, Verba and Petrocik, 1976; Dennis, 1975). Not everyone agrees on the meaning of the change. It is variously interpreted as a prelude to realignment, a temporary though major break in an old alignment that can be largely re-established, a noncontinuing decline that leaves electoral parties only somewhat less pervasive, or telling evidence of a continuing process of dealignment or electoral decomposition (Miller & Levitin, 1976; Sundquist, 1973; Ladd & Hadley, 1978; Burnham, 1970, 1975; Lipset, 1981; Pierce & Sullivan, 1980). I note sources for the several interpretations without pausing to explore their variety. But I should emphasize the extent to which most scholars treat a regularized partisan alignment—old, new, or prospective—as a more or less desirable norm. Even while acknowledging an association of independent voting and high educational levels and thus a modernity about party switching and split-ticket ballots, political scientists rarely see virtues in weaker electoral parties. More often, they look hopefully for a renewed vitality, and when instead they find the likelihood of a continuing dealignment, doubts are expressed about its compatibility with a meaningful democratic process.

Relative to electoral parties, whose weakening in the last few decades has been so readily measured, extra-governmental party organizations occupy a less prominent place in the literature of recent party decline. Twenty or thirty years ago, these organizations, unlike parties-in-the-electorate, already looked relatively weak. With a few conspicuous exceptions, chiefly Chicago's, city machines—conventionally the most imposing American party organizations—were regarded as withering anachronisms where they had not completely disintegrated. It is true that some machine organizations, besides Chicago's, were shown to have more fully developed during or after the New Deal than earlier, thus contradicting established views of when and how the bosses had been destroyed (Stave, 1970; Dorsett, 1977). And patronage remained important enough to be studied in various settings (Tolchin, 1971). But by the 1960s the old machines ceased to be generally dominant. Moreover, they had not, in their heyday, been favorites of political scientists. Several studies had appreciated the usefulness of their organizational activities, but for the most part the machines themselves had not been attractive even to the general defenders of indigenous party institutions. And, as observed, responsible party advocates regarded local bosses as obstacles in the way of effective national parties. Accordingly, the decline of the old machines was not so much marked or mourned in the last few decades as was (and is) the still limited development of organizational alternatives.

A scholarly interest in new nonmachine organizations has been apparent since the late 1950s. Eldersveld's already cited study (1964) of Republican and Democratic parties in metropolitan Detroit is a leading example. Another is Wilson's book on Democratic activists in several large cities (1962). And there were several studies of the dues-paying membership organizations that developed in California and Wisconsin during the 1950s and early 1960s. Unlike Detroit's parties, these organizations started as voluntary or extra-legal parties as distinct from the highly regulated statutory parties which they sought to take over. Successful and persistent in only a few places, their ideologically-motivated memberships—amateurs rather than professionals—did not strike every scholar as so effective electorally as had the old patronage organizations. At any rate, by the 1970s it was apparent that the regularized dues-paying membership had not become a widespread party phenomenon in the United States and that ideological activists tended to be candidate-centered and issue-group oriented, rather than primarily party organizational in their commitments, even when they worked within party channels. Hence Wilson (1973, pp. 95-118), though less sympathetic to ideological activism than some other observers, represents a common view of its limited contribution to the effectiveness of late twentieth-century American party organizations. So too does Wilson present, in persuasive style, a widely accepted perception of the generally unsubstantial character of those party organizations relative to other kinds of political organizations in the United States.

This is not to say that students of parties have become content with the perceived limitations. Hopes for strengthening parties through organizational activism are apparent in very recent research on state parties. Three interesting examples appear in an important volume on party renewal (Pomper, 1980). Each of the three is about participatory efforts within established state party organizations rather than through extra-legal voluntary clubs alone. Lawson (1980) describes pro-party changes, including legal changes, in California; Marshall (1980) discusses the large-scale use in Minnesota of the caucus-convention system for policy resolutions and candidate endorsements; and Mileur (1980) explains the Democratic party's charter movement in Massachusetts as a means to secure meaningful convention decisions. The last of these developments, as the author notes, is related to the national Democratic party's charter of 1974. It too appealed to party revitalizers although it did not fulfill proposals for an enrollment of a regularized membership (Crotty, 1977, p. 252).

A related but different organizational hope at the state level is reflected in studies of party headquarters. Scholars have discovered welcome signs of greater professionalization and better-financed bureaucratic services. Huckshorn's work on state party leaders (1976, p. 254) shows that over 90 state parties maintained headquarters staffs in the mid-1970s and that over half of these had developed since the 1960s. Although Huckshorn does not believe that the new organizations dominate most campaigns, he describes their provision of services to some party candidates and their potential to provide technologically-advanced help that is too expensive for many individual candidates to buy for themselves (p. 263). The financial base need not and evidently does not usually rest on dues-paying members; rather, money comes largely from contributors who, while often numerous small givers, have a different status from organized activists. Party leaders and their staffs, in these circumstances, may well have considerable freedom from ideological constraints. Subsequent research concerning state party organization in the late 1970s tends to confirm Huckshorn's findings; professionalization and institutionalization continued to develop (Cotter, Gibson, Bibby, & Huckshorn, 1980). The evidence is not overwhelming, but it is thought significant particularly with respect to Republican developments (Jewell & Olson, 1982, p. 292).

More attention lately is being given to a similar but larger Republican development at the national level. Two scholars, Bibby and Cotter—who also study state party organizations—have stressed the importance of the growth of the Republican National Committee's headquarters since the late 1970s. In several works, they knowledgeably describe its staff, professional institutionalization, technical services, increasing involvement in congressional and state campaigns, help for state parties and candidates, close relations with the Republican congressional and Senate campaign committees, and thriving financial capacity thanks in large part to direct-mail solicitations (Cotter & Bibby, 1980; Bibby, 1979, 1980; Bibby & Cotter, 1980).

Although the RNC itself is still insistently a confederative party structure, its headquarters operation constitutes a much more substantial *national* party presence than any previously established. Significantly, the operation may be described as bureaucratic and leadership-dominated in that it does not rest on participatory activism (Longley, 1980). Money, to be sure, comes from numerous small contributions, as well as from larger ones, but the contributors are not members in the policy-making, activist sense. The RNC thus combines the conventional American cadre party's organization of leaders without dues-paying members, and the now characteristically successful financing of nonparty political organizations from solicited contributors. The pattern, as Kayden suggests in her analysis of nationalizing party trends, may represent the party of the future (1980). As such, it is more welcome among scholars content with strong leadership parties than it is among those preferring a more participatory model. Almost all party specialists, however, tend to appreciate any organizational improvement especially at the national level where parties have until the last decade seemed unsubstantial (Cotter & Hennessy, 1964).

Similarly, the different kind of strengthening of the Democratic national party has had a broadly favorable reception even among many political scientists critical of particular post-1968 reforms. The 1974 party charter, as noted already, is regarded as a positive development. So too is the establishment of the national party's authority over delegate-selection practices. For example, Ranney (1978, p. 226) treats the party's rule-making nationalization as a positive accomplishment although he dislikes the major results of the more general reform process of which the nationalization was a part. After all, national party authority, fortunately confirmed in U.S. Supreme Court opinions over-ruling contrary state regulations, might be used to achieve other preferred results. Apart from such possibilities, another more reform-minded political scientist sees the new power of the national party, relative to state parties, as "the most significant and far-reaching outcome of the entire reform era" (Crotty, 1978, p. 260). Not all scholars, it is true, are avid nationalizers; a few still prize the old confederative structure and, therefore, dislike overriding state parties. But the desire to strengthen national organizations is, as observed earlier, a long-standing one among party specialists.

However favorable the political science perception of the organization-enhancing potential of the Democratic national party's rule-making authority, it is not nearly so prominent as the adverse reaction to the development, or at least the accelerated development, of candidate-centered presidential nominating campaigns after 1968. Associating that development with the proliferation of primaries that was an evidently unintended consequence of party reforms, political scientists have become leading critics, especially within Democratic circles, of the party-weakening effects of the "plebiscitary" process of choosing presidential nominees (Ceaser, 1979, 1982; Ladd, 1980; J. Kirkpatrick, 1978; Ranney, 1975, 1978). The process, it is recognized, is used in practice by Republicans as well as Democrats. Although Republicans appear neither to have initiated the post-1968 reforms nor to have suffered electorally from the results of such reforms, their presidential nominees too have become the more or less popular choices of voters as expressed mainly in state-mandated primaries. Party organizational influences in determining nominations are thus observed by scholars to have been reduced in both parties. "Peer review" of candidates by leadership cadres—state and local party officials along with national figures in and out of Congress—is widely perceived as having been superseded by media-influenced campaigns among rank-and-file voters.

It is true that the old peer review had never won the support of those pro-party advocates who champion participatory activism by the ideologically committed. They prefer open caucuses in which activists, not merely leaders, can influence nominating contests. But they share with the defenders of nomination by regulars and professional politicians a dislike for the now-dominant presidential primaries—indeed for primaries altogether (Burns, 1980, p. 198). Whatever their other differences, almost all political scientists want to strengthen, in one way or another, party organizational roles in choosing nominees. This generalization holds even in the case of a scholarly observer who, untypically, does not plainly deplore the new nominating process; thus Kessel, while recognizing the problems of candidate-centered politics, finds in recent presidential nominating and election campaigns hopeful indications of continuing, meaningful national followings. He sees these "presidential parties"—campaign workers carrying over, often as local party officials, from one presidential candidate to another—as positive developments along with the increasingly important national party committees (1980, pp. 245, 265).

In the more usual pessimistic view of the plebiscitary nominating process, political scientists stress its impact on the capacity of presidents, once nominated and elected, to provide effective governmental leadership. Polsby and Wildavsky make the point sharply and familiarly when they discuss "the decline in the vital function of intermediation by parties." Parties are less able to perform this function "because the formal properties of plebiscitary decision-making, such as occurs in primary elections, leave so little room for the bargaining process" (1980, p. 281). A presidential nominee, not the choice of a coalition of politicians within a party, is unable to count on such a coalition to sustain his policies, notably in Congress after a winning election campaign. The political scientists' concern here is the traditional one of using party to bridge the constitutional gap between the two elected branches of American government. The old presidential nominating convention, while never helping as directly as the still older congressional caucus nomination might have done, now appears less unsuccessful than the plebiscitary system (at least for those more attentive to Carter's than to Reagan's experience). Because of the understandably special concern that there should be effective presidential leadership of congressional parties, or at least an effective cooperative relationship, some degree of return to peer review and a brokered convention is appealing. Presidential primaries, unlike direct primaries for most other party nominations, seem recent enough in many instances so that they or their influence might yet be curbed. Contemporary political scientists might also like to roll back direct primaries for other offices; more than earlier scholars, they are likely to regard all primaries as harmful to party organizations. But, realistically, political scientists concentrate on trying to limit the use of primaries in selecting the distinctively important presidential nominees. They hope thereby to make the presidential nominating convention again a striking exception to the prevalence, in this century, of the direct primary for choosing nominees for most American elective offices.

With or without the impact of plebiscitary presidential nominations, the problem of establishing effective governing parties has troubled political scientists recently as it has in the past. Perceived declines in effectiveness are attributed to many causes apart from the new presidential nominating procedures. From the imposing literature on congressional behavior and elections during the last two decades, a picture emerges in which individual Representatives and Senators, never entirely cohesive party followers, have become increasingly independent actors. Congressional parties still organize each house, still account for the roll-call votes of most members more often than anything else, and still maintain elaborate structures (actually more elaborate than before) to mobilize their forces. But members especially of the House have strongly candidate-centered relationships with their constituents and are able to conduct their own campaigns with the help of substantial non-party contributors (Mayhew, 1974; Fiorina, 1977; Mann, 1978; Hinckley, 1981; Jacobson, 1980). Moreover, the specialized professionalism of members, institutionalized in the decentralizing committee and subcommittee structure of Congress, contributes to independent behavior (Patterson, 1978). The tasks of party leaders are recognizably difficult. Ideological agreement plainly helps; party cohesion in roll-calls on certain sets of policy issues is greater than on others (Clausen, 1973).

I should not leave the impression that the authors of specialized studies of Congress are generally disappointed seekers of greater party cohesion. Many explain why Congress is not and perhaps cannot be dominated by stronger parties, and their studies sometimes emphasize the remarkable degree to which parties persist and effectively function within the limits imposed by the American system. Often, however, there is a note of satisfaction with reference to successful party performances of the past (Brady, 1978) and an apparent anxiety about any party decline. Furthermore, party specialists, when writing on Congress, tend to be alarmed about any diminished party cohesion (Burnham, 1975), and to search for new sources, notably an ideological realignment of members and voters, that would facilitate greater cohesion (Everson, 1980). Parallel concerns appear in the less well-known literature on state legislative parties (Jewell & Patterson, 1977, pp. 383-388).

V. AGAINST THE TIDE?

However incomplete my review of the recent literature relating to parties, it reveals, I trust, the continuity of the scholarly commitment established earlier in the century. The demonstration is hardly remarkable or even necessary insofar as it merely confirms that those who study parties think them important. But in discussing recent work as well as in the earlier intellectual history, I have also stressed how the interest in the subject is accompanied by a belief in the usefulness and desirability of parties. That belief, though also familiar, strikes me as a significant characteristic of our field. Not every other area of study is marked by anything like a parallel to the persistent strength of the pro-party commitment. Interest groups present an especially telling contrast; studies of that subject, while often accepting the usefulness as well as the inevitability of interest groups in the political system, are far from agreement on the desirability of making them more effective in determining public policy. It is not unusual, now as in the past, for political scientists to think that interest groups are already too effective in this respect. Parties, on the other hand, are treated by our profession much as are those governmental institutions—Congress, presidency, courts—whose strengthening is also frequently advocated by their specialized scholars.

The relative *in*effectiveness of contemporary American parties is now our field's principal concern. Far from ignoring trends toward candidate-centered politics, party scholars emphasize the decline in the party identification of voters, the weakening of the organizational power of state and local leaders, the growing role of Political Action Committees especially in financing campaigns, the personalizing impact of mass media and communications, the increasingly nonparty electoral bases of congressional incumbents, and the new nonorganizational routes to presidential nominations and elections. Disputes occur over the reversibility of these trends and their potential for destroying parties altogether, and over ways of maintaining or rebuilding parties. But common ground exists here as it has more generally for the defenders of indigenous party institutions and the advocates of responsible party government. These schools of thought, I have argued, represent two strands of a scholarly pro-party tradition reacting to the candidate-centered politics that has become increasingly popular among other Americans during much of the twentieth century. Their differences, as the relevant literature indicates, lie in the means for strengthening parties. One group would restore and rebuild party leadership structures (caricatured as the "back-to-the-bosses" movement). The other would mobilize issue-oriented activists to exercise influence through caucus or other party participatory channels.

The differences, though consequential as they have always been, are contained within a Committee on Party Renewal that party specialists of various persuasion formed in the late 1970s. Members include scholars who (like Ranney) look primarily to a restoration of the traditionally competing leadership coalitions, others (like Burns) who remain identified with the advocacy of responsible party government under presidential leadership, and still others who emphasize large-scale participatory organizations. The Committee has made its case for strengthening parties both within and outside the profession. For example, it appeared before the Committee on House Administration (1979, pp. 392-393) that was considering proposals for government financing of House election campaigns, and urged that at least a substantial share of any such financing be provided to parties rather than to candidates alone (as the Representatives' own bills specified). In this as in other respects—like presidential nominating conventions—the Committee on Party Renewal seeks to rebuild parties as a counterforce to the now established candidate-centered politics. A leading member of the Committee, Gerald Pomper, edited a book devoted to arguments for party renewal as well as to useful studies, previously cited, of actual renewal efforts. The tone is clear in Pomper's introductory chapter: ". . . we must either acknowledge the mutual reliance of our parties and our democracy—or lose both" (1980, p. 5).

Pomper's language is more typical of responsible party advocates than it is of defenders of the old institutions, but the two schools of thought surely stand together as embattled champions of institutions which in one form the American public has never accepted, and which in the other form it has increasingly abandoned. Seldom do political scientists accept the decomposition of parties with equanimity. Now as always it is rare to find a scholar who rejects parties altogether—along with representative government—in the manner of Rousseau (B. Barber, 1980); it is nearly as infrequent for political scientists to express anything like satisfaction with relatively weak parties. Democratic politics without parties, or with much weaker parties than we now have, is almost always contemplated regretfully or fearfully when it is considered at all (Crotty & Jacobson, 1980). New kinds of political organizations, it is recognized, may be developing to perform certain party-type functions (Sorauf, 1980b), but these organizations are rarely seen as fully replacing parties even when they are regarded as potentially useful. Among political scientists, virtually no anti-party, or nonparty, school has arisen to correspond to the anti-party sentiment of a substantial portion of the larger community. Scholars do not reflect that sentiment though they may now have limited expectations with respect to party activities and accomplishments, particularly in policy-making spheres (King, 1969).

Are professional students of politics champions of a lost cause, trying with words to roll back a tide of American anti-partyism? So it sometimes appears as the scholarly devotion to parties has become more pronounced since mid-century while the power and public status of parties have visibly diminished. It might then follow that much less attention, in research and teaching, should be given to parties and more to other means of representation. The idea must occur to many political scientists, but, speaking now for myself, there are two reasons for not rushing to select this option.

First, I doubt that the pro-parties cause is irrevocably lost. Signs of decline can be acknowledged without thinking of them as overwhelming; newly developing national electoral alignments and organizational forms could suit the late twentieth-century American environment in ways that neither the old structures nor certain responsible-party models have done. I am thinking of the "presidential parties" and the broadly financed professional bureaucratic headquarters that scholars have recently examined, and also of the now-familiar evidence of a cohesive Republican governing party in 1981. Taking note of such developments, Harmel and Janda (1982, p. 132) write of what may be "the first steps on a path leading to a new vitality for the American party system." The suggestion is no less intriguing because it is made in the context of a work that typically and persuasively explains the constraints under which parties operate when trying to strengthen themselves. Possibly too there will be a broader tolerance for party strengthening than could have been expected when the old power and corruption of machine politics were more salient. No doubt, the earlier bad reputation of parties lingers generally, in middle-class attitudes particularly. But American parties may now have declined enough so as to allow a sympathetic public response to their revival in modern form. The signs of that kind of response are admittedly limited. A little pro-party sympathy may be detected in congressional legislation concerning campaign finance; although the major effect of that legislation has been to confirm candidate-centered financing, a few regulatory and public-funding provisions explicitly support parties (Kayden, 1980, p. 261; Alexander, 1980, p. 176). And there are proposals for more fully helping parties. Several states already do so through public funding (Jones, 1980). The popularity of the cause is enhanced by the greater contemporary reaction against Political Action Committees than against the weakened parties.

Secondly, I share, though a little skeptically, our discipline's long-established conception of the special importance of parties in a democratic society. I have used "they" for its various exponents principally to maintain a stylistic distance. I am myself impressed with the theoretical argument that parties are essential intermediaries for effective representation of a large and diverse electorate. Like other political scientists, I see no examples of a democratic political system, now or in the past, working without parties, or with much weaker, more porous parties than we now have.

Nevertheless, I do not believe that the established intellectual perspective or the limited indications of party revival should preclude the exploration of institutional alternatives to parties. Perhaps such exploration is already encouraged under the rubric used for APSA conventions, "Political Parties and Other Political Organizations." We know that Political Action Committees, in particular, are subjects of able scholarly work. They are not neglected even by party specialists who do not ordinarily conceive of them, any more than they conceive of the news media, as adequate to serve all of the purposes of parties. At most, it seems to me, the continuing scholarly commitment to parties may mean that they receive, compared to other political channels, more of our attention than their contemporary roles would seem to merit. The special attention is reflected not only in research but in courses and texts that concentrate on parties within an increasingly specialized discipline.[7] Concentrating in that way myself, I do not depart from the intellectual context of the continuing commitment that I have described. But the context along with the commitment may not be immutable.

FOOTNOTES

1. See Merriam and Overacker (1928) and Hannan (1923), reporting the views of political scientists among others in a special issue of the *Annals* devoted to the direct primary. Significantly, in 1923 a slight majority of the political scientists replying to Hannan's question favored convention nominations over the direct primary. Also among the several scholarly contributors to the special issue there were critics of the direct primary. Moreover, the political scientists who wrote in support of the direct primary did not make their case, as did Senator George Norris (another contributor to the special issue), on anti-party grounds. Altogether it seems from the 1923 col-

lection that political scientists already reflected a good deal of the skepticism about the direct primary's benefits that had been expressed earlier by the influential Henry Jones Ford (Ranney, 1954, pp. 85-87).

2. Ranney (1954) summarizes the arguments of Croly and Ostrogorski. The latter's major work, first published in English in 1902, is still widely admired for its scholarship by social scientists who nonetheless reject Ostrogorski's hostility to parties. Note, for example, S. M. Lipset's Introduction to an abridged paperback edition of Ostrogorski's study (1964). The other well-known European critic of parties, Michels, seems to have been no more influential than Ostrogorski in turning American political scientists away from their democratic hopes for parties; his argument, in any event, was not to dispense with parties but to realize their inevitable oligarchical nature (1949). Hence, in this essay I do put aside the interesting arguments against parties. By no means, however, am I suggesting that they have disappeared. Even if now rare among American political scientists, as I suggest in a later reference (B. Barber, 1980), the plainly widespread public hostility to parties has its contemporary intellectual champions as well as its staunch Washingtonian tradition. It may be uninfluential only among political scientists, but they and their study of parties constitute my subject.
3. I draw here not only on Ford's 1898 book, which I have cited, but also on Ranney's summary (1954, pp. 70-91) of Ford's later work.
4. Accordingly James (1974, pp. 9-28) treats Key as a proponent of a "responsible party" model rather than of a "party government" model in a classification that is probably more useful for some purposes than the simpler two-fold classification that I have adopted.
5. I have elsewhere discussed the part that British experience played in this advocacy (Epstein, 1980).
6. Adamany, in an appreciative review essay, cites *The Wesleyan Argus* (March 5, 1971, p. 2) as the source of the revealing quotation. For another insightful review of Schattschneider's work, see Boyd (1979).
7. Unlike Key (1964), several recent texts for parties courses do not include pressure groups or interest groups in their titles. Sorauf (1980a) is a case in point, but it should be noted that without devoting any chapters explicitly to interest groups, he does emphasize the limited roles of parties.

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6

The Forest for the Trees: Blazing Trails for Congressional Research

Leroy N. Rieselbach

Research on legislatures is essentially a post-World War II phenomenon. As recently as the early 1950s, the legislature—its practices and politics—was an unexplored wilderness.[1] Much of the then-extant knowledge about Congress, for example, was codified in three influential texts that practitioners had produced (Gross, 1953; Galloway, 1953; and Griffith, 1951).[2] In the ensuing years, an ever-growing army of scholars marched boldly into this virgin territory, producing literally thousands of studies that illuminated many dark corners of the forest. This chapter, of course, is an attempt, no doubt idiosyncratic, to specify what these explorers have uncovered and, more importantly perhaps, to identify those obscure portions of the legislative terrain that remain largely unmapped.[3]

These forays into the unknown, mostly the efforts of individual researchers doing individual projects, have surveyed much of the legislative landscape, at least to a limited degree. Most have produced discrete, narrowly focused, analyses of particular features of congressional structure and performance (the "trees"). With respect to some topics—congressional elections and roll call voting, for example—this research cumulates to provide substantial insight; in other areas—committee behavior and political party influence, for instance—much remains uncharted.

At a more general level, in the past decade or so, some genuine progress at integrating the bits and pieces of single studies into "middle range theories" has been made. Not surprisingly, the best cartography pictures those portions of the legislature most thoroughly investigated (elections and roll call voting), but some insightful contributions deal with less malleable topics such as committee processes and political representation. It is particularly difficult to

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theorize about such complex but critical matters as member motivation, political communication, and power and influence.

Finally, at a still broader level, there remains little "grand theory" that encompasses the totality of congressional politics (the "forest"). There are some traces of both empirical theory ("purposive," organizational, and systems-role paradigms) and normative theory (executive force, responsible parties, literary, and congressional supremacy models), but neither front has witnessed much forward movement in recent years. Existing theories have not led to a clear formulation of Congress' place and performance in national policy making. The forest has proved difficult to penetrate because it is not fixed or immutable; no sooner is the underbrush cleared from one corner or another than there appears some new growth that makes existing maps obsolete.[4] Congress refuses to stand still for easy and permanent exploration.

I. THE TREES: WHAT WE HAVE DISCOVERED

Research on Congress has, of late, emphasized congressional elections and roll call voting in particular. There has been a cumulation of knowledge here, absent in other areas of concern, that reflects scholars' access to data as well as their perseverance in exploring variations on central themes. On other topics, by contrast, difficulties in data collection and the centrality of less tractable concepts (influence, for example) have made progress beyond description hard to achieve. Thus, we know considerably less about committee processes, political party performance, and the impact of informal norms on member behavior than we do about elections and roll call voting. Similarly, the interactions between the executive and interest groups, external to the legislature, and the members of Congress remain only dimly illuminated.

Congressional Elections

Voters. The pioneering efforts of the Survey Research Center led, in the 1960s, to the view that voters in congressional elections were relatively uninformed about candidates and their campaigns, tending to cast their ballots in keeping with their personal partisan identifications (Campbell *et al.*, 1966). When they defected from partisanship, citizens seemed to respond, in the aggregate at least, to national political trends, particularly the state of the economy and the standing of the president in the polls (Tufte, 1978; Kernell, 1977); thus the "outs" prospered at the midterm. More recently, individual level studies, however, found that voters were curiously indifferent to their personal economic circumstances in choosing congressional candidates (Kinder & Kiewiet, 1979).

These more current studies, which often use 1978 and 1980 Center for Political Studies congressional election data, suggest that local conditions, the essential features of single House or Senate contests, are more important than national developments. The voters respond, by this evidence, to a particular pair of candidates, influenced as much if not more by their familiarity with and evaluation of the opponents than by partisanship or national trends

(Mann, 1978; Mann & Wolfinger, 1980). The more they know about candidates and the more they admire them, the more likely they are to vote for those nominees, even if to do so requires abandoning their partisan commitments. Moreover, incumbents rather than their challengers are most likely to benefit from these voter propensities (Beth, 1981-82).

Campaigns. The primacy of local conditions and candidates squares with the major findings about campaign processes, strategy, and tactics (Kingdon, 1966; Leuthold, 1968; Hershey, 1974; Fishel, 1973). Given the general weakness of the national political party organizations, congressional candidates take nearly complete responsibility for their own campaigns.[6] They devise their own strategies, recruit their own volunteers, and raise their own funds. If they win, they feel beholden to no one, though they will act to keep their victorious coalitions intact. Such control of campaigns, however, does not breed a sense of security: on the contrary, candidates, including incumbents, run with considerable trepidation (Fenno, 1978). They are always worried, even if their seats seem safe, that disaster lurks around the bend; defeat comes often enough to arouse fear among those who survive (Erikson, 1976; Collie, 1981).

Incumbency. Incumbents most often succeed in locally focused, personally managed campaigns. Several factors seem to account for this result. While Tufte's (1973) claim that redistricting is a central explanation—state legislatures drew the lines to accommodate incumbents—has been discounted (Bullock, 1975; Ferejohn, 1977), other possibilities abound. For one thing, voters' orientations and attributes may have changed. They may vote the issues rather than the party (cf. Nie *et al.*, 1979, with Converse & Markus, 1979). Alternatively, while they may not be more "rational," citizens may simply recognize—when asked or when voting—the incumbent and cast their ballots for the more familiar figure. Moreover, incumbents have substantial advantages in making themselves well known: they can more easily raise campaign funds (Jacobson, 1980); they can use the perquisites of office—the frank, a monopoly of media attention, their ability to conduct "casework"—to "advertise" and "claim credit" (Mayhew, 1974; Cover & Brumberg, 1982; but see Johannes & McAdams, 1981); they can obtain the collaboration of cooperative bureaucrats (Fiorina, 1977; Arnold, 1979).

Finally, the explanation for incumbent success may be more a function of the absence of strong challengers than anything else. Unable to match the incumbents' resources, the out-party candidates cannot make themselves visible, and thus viable, alternatives to sitting members of Congress (Abramowitz, 1980; Hinckley, 1981). Whatever the reasons, truly competitive contests remain rare, especially for House seats (Cover & Mayhew, 1981). More than nine of ten incumbents seeking reelection to the House have won in recent years. Only when challengers can overcome all the obstacles, and become both visible and attractive, do they threaten incumbents. This possibility is greatest in Senate races—where new campaign technologies can be used to reach heterogeneous electorates—and since 1976, sitting Senators have become increasingly vulnerable.

In sum, two decades of research on congressional elections has revealed a good deal about the ways in which members of Congress win and retain their seats. We know little, however, about the forces that impel them to run in the first place; studies of recruitment have lagged well behind those of campaigns. Some candidates are presumably "self-starters," seeking a seat in Congress on their own initiative; others are reluctant and respond only to the importunings of local party and other interests. We would understand more about elections if we had better information about who runs and what motivates them to do so. In any case, the efforts of those who win nominations reflect individual actions in particular districts, characterized by varying voter configurations. Senators and representatives tailor their campaigns both to the district and to the opponent who emerges to challenge them at any particular time. Moreover, as Mayhew (1974) indicates, they structure their institution to meet these electoral needs.

Congressional Roll Call Voting

If scholars have obtained some understanding of the ways congressmen and women arrive and remain in Washington, they have also staked out some claim to comprehending the ways members make that most elemental of decisions—the roll call choice among various voting alternatives. Here, too, the availability of "hard" data as well as the existence of historical precedent (see, for example, Lowell, 1901; Rice, 1928; Turner, 1951) stimulated explanation of the *patterns* into which floor votes fall. Votes do not, it is important to emphasize, reveal members' motivations or preferences precisely, but they do record the basic alignments of supporters and opponents. (See Matthews & Stimson, 1975, pp. 5-12 on the rationale for studying roll calls.)

Member Characteristics. Votes on the floor of Congress may flow from members' personal attributes and experiences, which presumably antedate their election. Background characteristics give what Asher and Weisberg (1978) call a "long-term component" to legislators' outlooks and, thus, voting choices. Partisanship is most central; party has both psychological and organizational reality, and lawmakers prefer, other things equal, to support their party. Party "constitutes the single most important group loyalty for members of Congress" (Davidson, 1969, p. 147), and numerous studies document that they do indeed vote along partisan lines regularly (MacRae, 1958; Truman, 1959; Mayhew, 1966; Kingdon, 1981). Electoral realities, the need to satisfy similar sorts of constituents (Cooper *et al.*, 1977) or "shared policy attitudes" (Norpoth, 1976) may undergird partisanship, but whatever the motive, party retains a firm grip on its adherents even though that hold may have become somewhat weaker in recent years (Brady *et al.*, 1979).

When party cohesion declines, cleavage is likely to fall along ideological lines. One study (Schneider, 1979; see also Shaffer, 1980) argues that ideology provides the focal point around which most, if not all, congressional voting revolves. Whether this is, in fact, the case, remains problematic; it is clear, however that on some issues at least (Clausen, 1973), ideology is decisive. The conservative coalition, of course, is the most clearly visible manifestation of an ideological alignment that transcends partisanship (Manley, 1973; Brady & Bullock, 1980). It appears, in any event, that member attributes, party affiliation and ideological orientation, provide a benchmark—call it "judgment" or "conscience"—against which legislators evaluate issues that appear on their agendas.

Constituency Characteristics. Members also assess these matters as the representatives of particular constituencies; they act in the face of the influence, real or potential, of those "back home" whose support they need. Members may define constituency in different ways (Fenno, 1978)—as all voters (the "geographic constituency"), hardcore supporters (the "primary constituency"), or a small set of personal "intimates"—but they presumably cast their votes with a careful eye on the constituency as they perceive it.

Early roll call studies tended to adopt the broadest definition and uncovered some modest relationships between the demographic attributes of legislative districts and the voting stances the representatives of these constituencies adopted. For instance, differences appear among rural and urban legislators, Northern and Southern congresspersons, and those whose districts vary in class or ethnic composition on one or more issues confronting Congress (MacRae, 1958; Shannon, 1968; Turner & Schneider, 1970; Clausen, 1973). The reelection requisite apparently compels members to pay at least minimal attention to their constituencies, broadly conceived.

More recent research has sought to delineate constituency more narrowly and precisely. Jackson (1974) constructs measures of citizen preference and intensity, and finds constituency to be the most important correlate of senatorial voting, especially on issues salient to voters (see also Miller & Stokes, 1963). Where constituency pressures decline, members will join party coalitions, follow party leaders, or trade their votes in anticipation of reciprocal support on matters of significance to them. Fiorina (1974) goes a step further, arguing that, logically at least, members calculate their courses carefully in terms of constituency. Those from homogeneous, presumably safe districts, protect themselves electorally by voting consistently with the sentiments of the dominant local majority. Those from heterogeneous districts face more difficult choices; the best they can do is to vote with the constituency interest(s) they perceive as strongest.[7] In any event, congresspersons seem to cast their votes with a clear eye on district realities.

In short, research on roll call voting, like that on congressional elections, has been plentiful and cumulative. Members' votes appear to reflect a complex calculus that requires them to weigh and balance competing claims—reflecting their own experiences and commitments, constituency relations, and contacts with other participants both inside and outside the legislature—under conditions of imperfect information. The sum total of such individual calculations seems to shift from issue to issue and from one time period to another.

Some Darker Corners

If we know, relatively speaking, a good deal about congressional elections and roll call voting, some equally if not more important aspects of the congressional terrain remain ripe for additional investigation. Structural features such as committees, parties, and informal processes and congressional relations with external participants, particularly executives and lobbyists, have received less attention; work on such subjects has not often moved beyond description.[8] Two fundamental factors, at least, account for this situation. First, much of what we want to know involves informal, less visible, "off-the-record" sorts of communication that members do not reveal readily and that scholars are not always able to observe. Often, we simply cannot discover who said what to whom with what effect. Second, change has been especially rampant in recent years. Reform and evolution of practice, reflecting the unrest of the 1970s, have profoundly altered congressional politics. In consequence, our knowledge of these matters remains imperfect.

Committees and Subcommittees. Since Woodrow Wilson's (1885) time, observers have recognized the centrality of congressional committees, yet most studies have focused on single committees (e.g., Fenno, 1966; and Horn, 1970 on the Appropriations Committees; Manley, 1970, on House Ways and Means; and Robinson, 1963; and Matsunaga and Chen, 1976, on House Rules). The early work (summarized in Morrow, 1969, and Goodwin, 1970) emphasized the general: committees are autonomous, expert, and successful policy makers; their decisions most often become Congress' decisions.

More recent scholarship has moved beyond these sweeping generalizations about "the congressional committee." Truly comparative studies (Price, 1972; and Fenno, 1973; see also Price, 1981) make clear that the conventional wisdom may conceal more than it reveals. Committees differ, and in predictable ways. Panel members' behavior reflects their motivations and their places in the larger congressional system. They struggle to obtain desirable committee assignments (Shepsle, 1978; Bullock, 1979); they adapt to, and contribute to, distinctive patterns of committee leadership, structure, and process including partisanship and integration (Fenno, 1973; Parker & Parker, 1979; Dodd, 1972); and they learn to cope with a host of resource management problems, especially staff (Kofmehl, 1977; Fox & Hammond, 1977; Malbin, 1980) and computer technologies (Frantzich, 1979). Overall, individual committees appear to develop a characteristic modus operandi, to which members not only conform but also contribute.

As sophisticated and insightful as these studies are, there is much to learn about committee processes, and the task has proven to be extraordinarily difficult. Change has hit committees hard. Some change is "normal," or evolutionary: congressional turnover has been high (Cooper & West, 1981) and the Republicans have captured the Senate; new committee leaders have succeeded those whom the few committee studies have examined. Indeed, leadership is a phenomenon that has largely defied analysis. Little is known about the ways leaders deal with followers: who are the leaders, the chairs or other influentials? Which way does influence run: do leaders actually steer committee rank and file, or do they conform to on-going committee influence relationships? What strategies and tactics and what modes of resource use do leaders employ in dealing with followers? What impact does leadership have on follower behavior? These and other questions require answers, and it is probably safe to assume that they will get different ones at different times, and for different committees.[9]

In addition, intentional change, or reform, has had a major effect on congressional committees. The seniority system has been modified, and much authority, formerly lodged in full committees, seems to have devolved to subcommittees (Davidson, 1981). While preliminary evidence suggests that subcommittees have become "institutionalized" (Haeberle, 1978), and relatively independent of party leaders (Deering & Smith, 1981), the broader effects of "subcommittee government" remain to be investigated. What does subcommittee leadership look like? Do the same factors that seem useful in describing full committees help explain subcommittee performance? What is the range of variation in structure, process, and behavior across the multitude of House and Senate subcommittees? Here, too, there are far more unanswered than answered queries.[10] Given the obvious importance of committees and subcommittees, there remains much to do to specify their place in congressional politics.

Political Parties. Committees, by all accounts, constitute the major decentralizing influence in the Congress; political parties, by the same token, provide such propensities for centralization as exist. The standard generalization, of course, is that parties and their formal leaders possess limited resources that permit them to bargain for, but not to command, the loyalty of their fundamentally independent members. We have clear descriptions of party organizations—offices and committees—and party resources—members' psychological commitments and leaders' bases of influence (Peabody, 1976, 1981a, 1981b; Ripley, 1969; Jones, 1970; Dodd, 1979b).

Importantly, access to party leaders, in the House at least, has permitted observers to begin to describe and assess the ways in which party officials seek to lead (Waldman, 1980; Sinclair, 1981a). Thus, Westefield (1974) indicates that leaders use the committee assignment process to accommodate rank and file members whose support they seek. Similarly, the House Democratic leadership has employed ad hoc committees (Vogler, 1981) and Speaker's Task Forces (Sinclair, 1981b) to accommodate members' goals, and thus to secure their backing for party positions. In the same vein, Dodd and Sullivan (1981) chart leaders "vote gathering" strategies, and discover that efforts to generate party cohesion vary both with the nature of the issue and the character of those partisans at whom particular appeals are aimed.

Such studies are promising but they are only beginnings in the quest to comprehend leadership strategies, tactics, and effectiveness. Since these matters involve interpersonal contacts, they raise the same general issues as efforts to understand committee leadership. In the party context, as in the committee setting, we need to know more about what leaders offer (or their ostensible followers demand), what responses their initiatives engender, and what conditions facilitate rank and file support. Since much of the leader-follower communication is likely to be tacit rather than overt, scholars will have to infer and speculate in the absence of open covenants openly achieved. Here, too, change, both intended and inadvertent, has altered the circumstances of the parties: new leaders must work with new, younger, and more independent partisans; they have new, reform-generated resources that they have tested only partially. How they operate under changed conditions, and with what results, constitutes a challenging research agenda for students of Congress.[11]

External Participants: Executives and Lobbies

The congressional forest is only part of the political topography. Much of what Congress does occurs in response to actions of external participants, most notably the president, the executive branch, and interest groups. Once again, we confront a familiar set of research issues. We know a good deal about the general contours of the political scene, but little about the precise forms of behavior that occur within the known setting. With respect to the presidency, it seems clear that the Chief Executive largely controls Congress' agenda, at least the major items (Walker, 1977). He has a broad battery of weapons, formal and informal, with which to seek victory for his preferred positions (Wayne, 1978; Edwards, 1980), but his success depends on skillful persuasion because he cannot compel independent lawmakers to accept his initiatives (Neustadt, 1980).

Interest groups operate in a similar fashion. They possess a range of resources—money, campaign aid, expertise—that they can bring to bear through direct access to legislators (Truman, 1971) or indirectly through "grass roots lobbying" in the states and districts (Ornstein & Elder, 1978). If journalistic accounts credit lobbyists with substantial influence over legislators, neither group is prepared to acknowledge that the relationships are more than cooperative and voluntary. Group representatives insist that they employ mainly "soft-sell," persuasive techniques (Milbrath, 1963; Bauer *et al.*, 1972); members of Congress claim that they can control their contacts with lobbyists without compromising their own independence (Matthews, 1960; Kingdon, 1981). Needless-to-say, however, both parties to any transactions are likely to be reticent about the exact nature of these arrangements. Moreover, group involvement in legislative politics has changed of late: changes in campaign finance, featuring the proliferation of political action committees, and the rapid rise to prominence of "single issue" groups that place a nearly exclusive emphasis on one topic such as abortion may render what little we know about group influence obsolete. Perhaps all that we can say is that interests are omnipresent but not necessarily omnipotent.

The connections between both executives and lobbyists, on the one hand, and Congress, on the other, raise the now-familiar influence problem. Knowing the general circumstances of these participants does not, in the absence of more direct and more systematic observation, permit generalization about the use of resources or their effects. On the executive branch, there is little beyond anecdote (but see, Jones, 1981a). With regard to groups, Bacheller (1977) has shown that different interests systematically vary strategies according to the type of issue and the legislative circumstances in which the matter is treated. Hayes (1981) argues that conflict is central to assessing group influence: where groups agree, they may often get their way; where they divide—where there are competing coalitions—legislators may gain considerable freedom by playing interests off against one another.[12] In fact, it is most difficult to determine the direction of influence between lobbyists and executives, on the one hand, and legislators, on the other. In most instances, we cannot easily ascertain who initiated contacts, what was exchanged in these contacts, which communicator, if any, yielded, or the net effect of such interaction over time. Untangling these complicated and reciprocal influence processes will require more sophisticated research than is presently available.

II. GETTING OUT OF THE WOODS: SOME EFFORTS AT SYNTHESIS

The partial listing of scholarly successes in identifying, describing, and analyzing specific aspects of Congress and congressional behavior does not exhaust the accomplishments of those studying the legislature. In a few areas—notably elections and roll call voting, but also committees, representation, and the "subgovernment" phenomenon—there have been valuable efforts to integrate the individual studies to produce "middle range theories." These achievements provide not only broader explanations for legislative phenomena but also explicit sets of hypotheses for further exploration.

Middle Range Theories

Elections. Research on congressional elections suggests some basic propositions: aggregate election results imply voter reactions to the president and the economy; individual choice, however, is largely independent of personal circumstance; incumbent candidates overwhelmingly win reelection, largely because they face weak challengers in their own districts. Jacobson and Kernell (1981) suggest a link, the strategic behavior of elites, that reconciles these disparate findings. The important campaign figures—the candidates and those who support, especially fund, them—*assume* that voters respond to national conditions. When those trends are favorable, attractive challengers choose to run, and attract strong support. When conditions favor the "ins," promising challengers avoid losing races and contributors seeking access or influence may donate to incumbents, who already possess all the advantages that the perquisites of office confer, virtually guaranteeing their success.

Voters face choices between pairs of candidates in their own states and districts that national economic circumstances affect through these strategic calculations of elites. Where prospects are promising, strong challengers wage strong campaigns; they emerge disproportionately in the party that economic conditions favor, but they tailor their campaigns to the constituencies in which they run. Candidate and contributor decisions "so structure the vote choice that electoral results are consonant with national level forces even if individual voting decisions are not" (Jacobson & Kernell, 1981, p. 3). Thus, though citizens choose in consequence of local conditions, the choice they confront reflects a larger economic reality, and the set of verdicts they render in the country at large translates that reality into the aggregate congressional returns. House incumbents win regularly because the circumstances that encourage strong challengers seldom appear; they occur more commonly in Senate contests, which accounts for the reduced success rates in the upper House.

Roll Call Voting. A multitude of studies suggests that members of Congress cast their roll call votes in light of their personal partisan and ideological commitments, their perceptions of constituents' sentiments, and their place in the congressional party and committee systems. They sort out competing claims on their votes under conditions of imperfect information; many matters fall outside their experience and competence. They make their choices, most studies indicate at least implicitly, through a process of "cue-taking," looking to trusted sources for guidance on subjects about which they are uncertain.

Kingdon (1981) imaginatively models this process: his interviews with House members inquired about the weights attached to cues from constituents, congressional colleagues, party and committee leaders, the executive branch, legislative staff, and the media and other information sources. He posits a "consensus mode" of decision making. Where members' "field of forces" is consistent, where there are few differences of opinion among those who provide advice, legislators simply vote for the majority position. Where consensus does not exist, representatives look first to selected informants, especially colleagues who specialize in the subject at issue, and with whom they tend to agree, i.e., who are of the same party or who are compatible ideologically. They also pay considerable heed, Kingdon finds, to their constituents, especially on visible votes on major matters. Other cues, while important periodically and on particular issues, are less significant than colleagues and constituents.[13] Note that cue-taking of this sort is entirely compatible with legislative norms such as reciprocity and members' reelection concerns.

The cue-taking perspective helps tie together other strands in the literature. Because cue-taking tends to recur and if successful be repeated, Asher and Weisberg's (1978) identification of a long-term component, a "voting history," rooted in partisanship, constituency, and ideology seems reasonable. Likewise, cue-taking patterns may vary from issue to issue (Clausen, 1973): party is important on "government management" questions; constituency on "civil liberties" (see also Miller & Stokes, 1963). Finally, enduring patterns of cue-taking explain why aggregate change in voting alignments seems related more to membership turnover than to individual opinion shifts: the newly elected bring new values and perspectives to Congress; those with longer service adhere to their standing vote decisions (Clausen & Van Horn, 1977; and Asher & Weisberg, 1978).[14]

Representation. Patterns of voting choice raise the fundamental question of representation, the connection between citizen preferences and needs on the one hand, and legislators' activities on the other. In policy terms at least, votes cast determine what the government does, which may or may not reflect what the public desires or requires. Following Miller and Stokes' (1963) empirical and Pitkin's (1967) philosophic revival of interest in this age-old topic, the rudiments of a middle range theory of representation have begun to emerge. At the most general level, representation is a reciprocal, but asymmetric, relationship between citizens and their elected lawmakers that involves several components (see Eulau & Karps, 1977). The link is reciprocal because communication, and thus influence, may begin at either end of the chain. Citizens may request (demand) that elected officials enact particular programs (policy representation) or perform specific chores such as engaging in "casework" for individuals or providing projects for the constituency (service representation). The ultimate sanction for such performance, of course, lies in the voters' ability to turn lawmakers out of office at the ensuing election. Alternatively, legislators may take the initiative, directing a variety of messages to the voters (symbolic representation) in order to structure constituents' views of them, and of the assembly itself (Fenno, 1978).

The representational link is asymmetric because any given communicative or influence effort does not automatically generate an "equal and opposite" effect. Residents of particular constituencies, and different classes of citizens within any district, may differ in the extent to which they make demands on their legislators. Members of Congress may vary widely—across issues and in terms of their electoral circumstances—in the degree to which they respond to citizen communications and to which they undertake symbolic representational activities (Cover, 1980; Johannes, 1980). Lawmakers must decide, as individuals, what representative stance to take toward their constituencies as they define them; how to allocate their limited resources among the types of representational activities; and, indeed, what commitment to make to representation generally as opposed to other claims they face.

Early work on representation adopted a policy "congruence" (or "concurrence") view that assessed the citizen-legislator link in terms of a match between preferences: legislators should act consistently with their own constituents' desires. Empirical work using this paradigm found representation to be far from perfect. Only on a few "hot" issues where citizens feel strongly and communicate clearly will legislators feel constrained to heed the "folks back home" (Miller & Stokes, 1963; Erikson, 1978; McCrone & Kuklinski, 1979). Subsequent research, however, suggests another standard. Weissberg (1978) proposes a criterion of "collective representation" that transcends congruence; "good" representation requires only that Congress enact what the nation as a whole wants. There is some evidence that it does (Backstrom, 1977; Monroe, 1979; but cf. Hurley, 1982).

These notions provide an approach to representation that moves beyond time, place, and single issues, although much empirical work remains undone. We need to know a good deal more about the conditions that induce members to listen to constituents and act accordingly. Alternatively, legislators' cultivation of constituents through symbolic representation needs clarification. The ways that members present themselves, and explain their actions, to citizens; the extent to which the former's initiatives win the latter's support and confidence; and the conditions under which they do so and the consequences, that is the policy making freedom, that flow from successful "home styles" are all central topics that deserve additional investigation.

Committee Decision Making. Recognizing the central place of committees, and more recently subcommittees, in congressional politics, scholars have charted, in general terms, the committees' contributions to a decentralized mode of legislative decision making. In a pioneering and deservedly influential book, Fenno has reoriented the study of congressional committees, stressing the differences rather than the similarities among them: ". . . [C]ommittees differ from one another. And . . . they differ systematically . . . with respect to five variables: member goals, environmental constraints, strategic premises, decision-making premises, and decisions" (1978, p. xiv). Examining six House panels, and their Senate counterparts, Fenno posits specifically that members' behavior reflects their *goals*—to seek reelection, power, or policy influence—and the particular *environmental constraints*—the chamber itself, the political party, the administration, and clientele groups—within which their committee operates. These variable antecedent conditions, in turn, shape distinctive committee *strategic premises* (or norms or decision rules) and specific *decision making processes* (partisanship, specialization, and leadership). All these committee attributes contribute to committee *decisions,* the result of panel deliberations.

Fenno's formulation has outstripped research on committees. The committees he examined (as well as those Price, 1972, studied) bear little resemblance, in composition and in performance, to their appearance a decade ago. His scheme has been applied comprehensively to few other committees (for exceptions, see LeLoup, 1979, on the House Budget Committee; and Perkins, 1980, on House Judiciary) and not at all to specific subcommittees, despite their increasing importance. In addition, there may be intra-committee variations that reflect the issues that comprise any panel's jurisdiction (see Price, 1978). Perhaps panels adopt different strategic premises or respond to different environmental actors when they deal with distinctive portions of their agendas. These and a host of similar inquiries constitute research possibilities sufficient to engage an army of congressional explorers.

The "Subgovernment" Phenomenon. Concerns about committees and the actors, executives and clienteles, in their environments merge in consideration of the subgovernment (or "cozy triangle," or "whirlpool") phenomenon. In conjunction with Lowi's (1964) distinction among distributive, regulatory, and redistributive policy arenas, scholars (Davidson, 1977; Ripley & Franklin, 1980) have identified a characteristic pattern of policy making, which appears frequently to dominate distributive (pork barrel) decision making. Within a narrow domain, cancer research funding or price supports for dairy products, for example, a cluster of participants—interested committee and subcommittee members, often with involved constituents; bureaucrats with program responsibilities; and interest group representatives—with clear stakes in particular policy results cooperate to control government decisions. Such arrangements are mutually reinforcing: they offer lawmakers an opportunity to achieve reelection, power, or policy goals; they provide executives with the possibility of defending if not enlarging their bureaucratic "turf"; and they offer outside interests the policies they seek to promote.

Where it can control conflict, keeping whatever policy opponents may exist (consumers, for example) unaware or disorganized, the subgovernment may well attain its policy objectives. Where controversy spills out into more visible and politicized arenas, where the potential "losers" perceive the costs they may be asked to pay on regulatory and especially on redistributive matters, cozy triangles are far less likely to prosper. Here, too, theory has outrun research: much of what we know is anecdotal (Ripley & Franklin, 1980) or contradictory (cf. Ferejohn, 1974; Arnold, 1979). We need, for example, clearer lines of demarcation among policy types. We need more detailed analyses of the contacts among subgovernment participants, the strategy and tactics cozy triangles employ, the conditions under which they are likely to succeed, and the results they are able to achieve. The subgovernment notion does tie together various strands of research on Congress, especially its committees, and the legislature's links to external actors, but it leaves a formidable research agenda to be addressed.[15]

Power and Influence: A Tangled Thicket

Lurking in the shadows, in both the narrower works and the efforts to synthesize them, is the thorny issue of *power,* influence, and authority—for simplicity, used interchangeably here. Much of what members do, and what Congress accomplishes, reflects the ability of some participants, inside and outside the legislature, to get others to accommodate their wishes by using methods that range from simple requests through complex bargaining to powerful pressure. Influence, of course, is reciprocal: executives and lobbyists wants legislators' support, but the latter often solicit aid from the former (Bauer *et al.*, 1972); citizens want representation, but legislators seek votes and trust from their constituents. Authority is also implicit: building credits does not require written contracts; behavior may reflect "anticipated reactions" that discourage lawmakers from engaging in actions that they do not expect to succeed.

Power, in short, is a nettlesome concept. Intuitively, we sense that some lawmakers have more authority than others, that they can win acquiescence from individual colleagues, and that they may even be able to bring about the legislative results they desire. Yet we remain uncertain about the ways, if any, that power operates in the congressional setting, and some clarification of its application seems essential (see Dahl, 1957; Riker, 1964; Oppenheim, 1978; and Baldwin, 1978). Research has identified many bases of authority, but generalizations about their use—how members employ them, under what conditions, and with what results—remain to be formulated. It will not be easy to extend the frontiers of knowledge here. There are numerous potential power-holders to consider; Congress is a complex and fragmented institution that diffuses power widely, but unequally, using formal and informal criteria.

Members are likely to dissemble, making observation of power difficult. They may act "strategically," professing positions contrary to their own in the hopes of extracting concessions for doing what they prefer to do in the first place, rather than "sincerely." If they succeed, they may claim exaggerated credit for their accomplishments; if they do not, they may be loath to reveal that they succumbed to more powerful forces.

It is far easier to call for a theory of congressional power and influence than to produce one, but the need is real. Unless and until we can learn more about authority and the exercise of it, our understanding of specific facets of congressional performance and our ability to cumulate our findings in more comprehensive explanations of legislative behavior will remain incomplete and unsatisfying.

III. THE FOREST: EMPIRICAL AND NORMATIVE VIEWS OF CONGRESS

As a practical matter, scholarly investigation proceeds in small steps, but the ultimate goal of any discipline, or subfield, is to produce as complete an understanding as possible of the territory within its domain. In the final analysis, this entails a complete mapping of the terrain. For political science generally, and legislative politics more specifically, the presence of a normative dimension complicates the cartography. We want not only to describe and explain *how* the assembly works but also to assess *how well* it performs. To achieve these goals requires some integrative frameworks that encompass the full range of performance. Students of legislative politics have in fact generated such "models," which, while rudimentary, in the long run promise to yield a vision of the "big picture," of the forest that the trees constitute. Moreover, these views point in both empirical and normative directions.

Empirical Theories

Efforts to build empirical theories of legislative politics, not surprisingly, rest on the findings and speculations that narrower explorations have generated. Thus, these attempts are most often inductive, seeking to integrate the extant literature; alternatively, they may be "horizontal," looking to adopt and adapt approaches prominent in other social sciences. In either case, theorizing about legislatures emerged from two perspectives that have guided work on legislatures. An older view is *institutional* in focus; it looks at the legislature as a collectivity that produces certain products, and performs particular functions. Its concern is less with what individual legislators do or say and more with the quality and quantity of what legislative institutions produce (policy, oversight, representation). A more recent perspective is *individual* in focus; it treats the causes and consequences of lawmakers' behavior and sees the legislature's product as, in some sense, the sum of its members' activities.[16] These vantage points have stimulated three identifiable theoretical orientations to studying legislative politics: one, organization theory, stresses institutional performance; a second, purposive theory, emphasizes individual behavior; and a third, systems-role analysis, seeks to combine institutional and individual concerns. Each has its adherents, each singles out some central issues, but none has attained wide acceptance.

Organization Theory: The Legislature as Institution. In its simplest terms, organization theory posits that formal organizations—"social units (or human groupings) deliberately constructed and reconstructed to seek specific goals" (Etzioni, 1964, p. 3)—display behavioral regularities. Because they pursue their goals within complex, uncertain settings (environments), organizations seek to adapt (structure themselves) to environmental forces to achieve their specific purposes; thus they can be analyzed in terms of their relations to external forces, their internal requisites, their internal structure and processes, the links among external and internal attributes, and their ability to attain their goals (to survive). Organization theorists seek propositions that describe organizational behavior in these terms: for example, the more heterogeneous an organization's environment, the more decentralized its decisional structures (environmental-structure link), or the greater an organization's internal need for technical skills, the greater its dependence on expert members.

A few students of legislatures, notably Cooper (1977, 1981, inter alia; see also Davidson & Oleszek, 1976; and Froman, 1968), have begun to assess Congress as a complex organization seeking to survive and meet its objectives in a highly differentiated socio-political environment. Cooper (1977) suggests that the legislature, like other organizations, must meet basic internal needs for "division of labor, integration, and motivation." That is, Congress must develop expertise, through a division of labor; coordinate, through integration, what its structural components produce separately; and influence, through motivation, its members to perform appropriately.

Congress, Cooper continues, can satisfy these internal needs only with considerable difficulty, largely because its environment imposes substantial constraints on it. For one thing, unlike hierarchical organizations, it cannot centralize its operations; member independence, rooted in electoral arrangements and institutional commitments to formal equality and democratic norms, undermines the existence of centralizing (party) authority. The potential for an effective division of labor is thereby reduced: unable to control entry, the legislature must "make do" with those who win seats, however inexpert some may be. The lack of central authority also inhibits congressional capacity for integration: independent members cannot be compelled to conform; leadership rests on persuasion not formal power. Finally, decentralization limits motivation: lacking many material rewards, leaders may be hard pressed to induce members to commit their energies effectively.

Logically, the concepts of organization theory—internal needs (expertise, motivation), environmental forces, organizational patterns (centralization-decentralization)—provide a coherent and intellectually satisfying explanation of the congressional process: for example, the legislature is a decentralized institution that acts largely through bargaining mechanisms. Moreover, organizational notions sensitize the observer to possibilities for change; as conditions alter (e.g., new environmental conditions emerge), the organization will adapt (e.g., restructure its committee or party systems). But organization theory has not, in fact, stimulated much research, and its few practitioners have focused selectively on formal structures rather than on the less readily observable organizational processes (motivation, power, interactions with environmental forces). Thus, much theoretical clarification and empirical work remains to be done, especially with respect to the links between organizational attributes and legislative outcomes.

Purposive Models: The Legislator as Individual. The organizational paradigm minimizes attention to behavior on the individual level, and those who find legislators more fascinating than legislatures have looked elsewhere for theoretical guidance. The most fully developed alternative derives from the economic theories of individual choice. Whether labelled "rational," "social choice," or, as here, "purposive," these models view individual legislators as "goal-seeking agents who choose from available strategic alternatives to further their ends" (Ferejohn & Fiorina, 1975, p. 407). Fundamentally, the purposive perspective suggests that lawmakers employ some form of cost-benefit calculation to select a course of action that will maximize their ability to reach their chosen objectives. Positive "payoffs" encourage, and negative ones deter, specific behaviors whether they are campaign strategies, votes on bills and amendments or adherence to chamber norms.

For instance, Shepsle (1978) uses a purposive model to explain freshman House members' initial requests for committee assignments. In general, members seek positions that reflect their "interests" (the value to them in achieving their goals—reelection, power, policy influence—of a particular assignment) discounted by the probability of their winning the appointment—a probability shaped by the number of committee vacancies and the extent of the competition for them. Thus individual requests flow from an "expected value calculus." Shepsle's empirical analysis of Democratic representatives' actual requests is consistent with the model; within limits, members do engage in "rational choice" (1978, ch. 4). Similarly, Weingast (1979) and Panning (1982) suggest that conformity to folkways is consistent with rational calculation under certain circumstances. Voting, on amendments (Enelow, 1981) and more generally (Fiorina, 1974, ch. 5), also appears to reflect purposive choice.

Purposive models have attracted a somewhat larger following than organization theory, but inherent problems continue to limit the number of their adherents. It remains extraordinarily difficult to get an empirical, operational handle on many concepts, especially given the subjective, psychological character of individual calculation. Even direct access to, and intensive interviews with, legislators may not suffice to get measures of cost, benefit, utility, value, and maximization. Moreover, members of Congress act often under conditions of imperfect information. Calculations may vary widely among any set of lawmakers, making behavioral generalization problematic. In consequence, some applications of these models avoid data, that is they are purely deductive, or make inferences based on assumptions about individual calculations without directly observing them (Mayhew, 1974; Ferejohn, 1974; Arnold, 1979). Purposive models have unquestionably stimulated insightful thinking about legislative politics, and it is no small accomplishment to conclude that members' behavior often seems consistent with, or looks "as if" it results from, a specific cost-benefit calculus (see Moe, 1979).[17] Nonetheless, while their potential remains great, economic models to date have not won wide acceptance, perhaps because much hard empirical exploration remains to be done in order to specify precisely the form and consequences of legislators' social choices.

Systems-Role Analysis: An Integrative Alternative. A third approach to legislative politics, systems-role analysis, combines institutional and individual foci (Easton, 1965, 1979). Individuals act within structural constraints. Systems analysts see legislatures as (1) decision making structures composed of formal elements like parties and committees, operating within the mandates of written rules and informal norms, that (2) respond to communications ("inputs") originating outside the institution, from executives, judges, organized interests, and the public, (3) to produce specific results ("outputs") such as policy, oversight, and representation. Legislative activities may influence, through "feedback" processes, environmental forces to make new or revised inputs to the assembly; these may require the legislature itself to alter its structures, outputs, or both.

The role dimension of systems analysis suggests that legislators' behavior reflects a series of choices ("role orientations") that they make about their relationships to the structural features of the chamber and to the actors in its environment. For instance, lawmakers must decide how to relate to their constituents. They may choose to speak for the district itself (the "district" orientation), a larger constituency (the "nation" orientation), or a combination of the two (the "district-nation" orientation); in the same vein, they may act in conformity to constituents' expressed preferences (as "delegates"), to their own personal judgments (as "trustees"), or to one or the other as circumstances dictate (as "politicos") (see Wahlke *et al.*, 1962; Davidson, 1969). In a similar fashion, legislators assume orientations toward legislative structures and activities (Jewell & Patterson, 1977). Their choices reflect their own attitudes and the inputs they receive from external actors; the distribution of their orientations shapes the legislature's performance.

While the systems-role view suggests numerous hypotheses—for example that "delegates" will seek out and reflect citizen preferences more closely than "trustees" or "politicos"—it has generated only a modest quantity of research (see Jewell, 1970, for a review and critique). Practical problems may be responsible: legislators may be inaccessible and/or reluctant to respond candidly. Moreover, the evidence that has been unearthed is, at best, inconclusive. It seems clear that lawmakers, when asked, can and do articulate role orientations (Davidson, 1969), but the causal antecedent conditions that lead to these choices remain largely unspecified. More importantly, it is far from certain that role orientations explain representatives' behavior (Gross, 1978; Cavanagh, 1982). There is certainly no simple one-to-one relationship between orientation and behavior; given the constraints that structural and political circumstances impose, role is likely to be important only under certain, limited circumstances.[18] The challenge, of course, is to specify those conditions precisely. In short, like organizational and purposive approaches, systems-role analysis has neither generated sufficient research to justify firm conclusions about its merits nor won wide acceptance as a roadmap of the congressional forest.

Normative Notions and Congressional Reform

Political scientists feel obliged not only to describe and explain legislative politics but also to evaluate legislative performance. With respect to the former, a consensual wisdom, mostly atheoretical, has appeared, largely from among the trees: Congress is a decentralized institution that diffuses influence and that, in consequence, acts incrementally through processes of bargaining and compromise. With respect to the normative task, there is less agreement. There are models of the "good" or "better" Congress (Davidson *et al.*, 1966), but neither outside observers nor practitioners promoting reform have made much sustained use of them. Yet each vision does provide the dissatisfied with a reform agenda that, if adopted, might move Congress in the directions they desire.

The Executive Force Theory. Proponents of the executive force model (e.g., Burns, 1963), accepting the conventional view of Congress as a policy maker, are pessimistic about the legislature's capacity to govern. They stress the need to solve pressing political, economic, and social problems, and despair that Congress can contribute meaningfully to policy formulation. The executive, by contrast, is likely to be the catalyst for progress. Congress, given its members, structures, and processes, can only impede innovation: a fragmented institution, representing multiple interests, especially the rural and small town constituencies of Middle America, it is incapable of acting decisively. It is better suited to oppose than to create, to react than to invent.

As a result, if policy making is to meet the nation's needs, the president must be permitted to lead, unobstructed by a recalcitrant Congress. Reform should reduce the legislative ability to frustrate presidential policy leadership. Independent sources of power, committees and subcommittees, for instance, should be curbed. Rules of procedure that permit minorities to block action require modification. In general, the path of presidential proposals through Congress needs to be smoothed substantially. The executive supremacy view, in sum, stresses presidential power and reduces Congress' role to legitimation, perhaps modification, and review after the fact. The president proposes and the legislature disposes according to his wishes.

The Responsible Parties Model. An alternative avenue to escaping congressional obstructionism is through the use of disciplined, cohesive, "responsible" political parties. If the majority party, given its command of the legislative terrain, as the chief organizational mechanism of the assembly, marched smartly and decisively forward in rank, its policy proposals would triumph at each and every stage of the lawmaking process. If, moreover, the president commanded the party troops, they would advance his—desirable and progressive—programs without risk of rear-guard delay or defeat.

Proponents of responsible parties (e.g., American Political Science Association, 1950) promote reforms to enlist rank-and-file members of Congress in the partisan armies. In general, they would empower the respective party's national committees to manage the electoral process. With the ability to control nominations, using a legal monopoly of campaign finance, for example, the central committees could control their elected representatives. To break ranks would, in effect, end the deserter's political career; the seat would be given to a more loyal recruit. Inside Congress, the rules would be rewritten to ensure that disciplined majorities could carry more easily the legislative day. In sum, in the responsible parties view, the president proposes and his loyal partisan army disposes consistently with his marching orders. Here, too, Congress would eschew policy making, emphasizing instead legitimizing and nonpolicy activities such as constituent services and oversight.

The Literary Theory. What appear as Congress' vices in the executive force and responsible parties models are virtues for the adherents of the literary theory (Burnham, 1959). They pay homage to "constitutional tradition," to checks and balances, and to separation of powers. In their view, Congress should restrain the power-seeking executive, in both policy formulation and implementation. Policy departures should come slowly, only after careful deliberation that considers all alternatives, and only after a genuine national consensus emerges. Thus, a decentralized legislature, to which all interests have access, and that can act only cautiously, is highly desirable.

These virtues have been lost in the twentieth century, the so-called age of executives, and reform is required to restore the status quo ante. To that end, literary theorists resist all centralizing mechanisms. They prefer an election system that protects legislators' independence; they fear disciplined political parties that might run roughshod over citizen sentiments; they distrust executive leadership; and, most important, they are predisposed to congressional procedures that promote the power of individual legislators to speak freely, slow action, deliberate carefully, and oversee the administration. Overall, they want Congress to propose and dispose, to make policy, to represent citizens, to police the bureaucracy, to countervail the executive. They seek to restore Congress to what they see as its rightful, legitimate place at the very center of the political process.

The Congressional Supremacy ("Whig") Model. The literary view pushed further becomes a model that stresses to an even greater extent the centrality of Congress. Legislative supremicists see Congress as "the first branch of government" (de Grazia, 1966), the prime mover in national affairs. They favor the reforms that the literary theorists advocate as well as other changes intended, in effect, to strip the chief executive of most major bases of authority. The whig view envisages a Congress that proposes and an administration (president and bureaucracy) that disposes in strict accordance with legislative desires. A supreme Congress will both make policy explicitly, on its own terms, and oversee the implementation of that policy.

Overall, then, there are models of the desirable Congress, each of which entails its own set of structural and procedural reforms. Each view could, theoretically, provide a standard against which to evaluate specific reform proposals: does a given suggestion move the legislature, or is it likely to do so, toward a clearly stated objective? The issue of legislative fragmentation is central to such assessment. Both executive force and responsible parties proponents stress the need for governmental action and they deplore a set of decentralized institutional arrangements that inhibit solutions to national problems; in stark contrast, the literary and congressional supremacy views focus on caution and consensus and they applaud structural mechanisms that deter policy changes until deliberation leads to agreement that new departures are desirable.

Reform in Reality. These normative formulations define more or less coherent visions of what the legislature should, and might, be. Reformers, in practice, are less often moved by such comprehensiveness; they tend to be legislators who seek to alter their institution in ways that advance their own, relatively narrow causes (Jones, 1977). They have, during the 1970s, in response to a series of legislative "crises," enacted a wide variety of changes, without much conscious effort to justify them as integral parts of any far-reaching plan to create a Congress of clear design.

In fact, four broad sets of reforms were adopted; those in the House were most prominent. A series of steps, including public committee meetings and recorded committee roll call votes, opened hitherto mostly invisible congressional processes to public scrutiny; together with ethics codes and financial disclosure requirements, these moves made it easier for citizens to know what their representatives were doing, and to hold them to account for their actions. Enacting the War Powers Resolution (1973) and the Budget and Impoundment Control Act (1974) armed Congress to challenge the executive branch for military and financial policy leadership. Third, the Speaker of the House obtained new influence—over bill referral and Rules Committee membership—and the Democratic Caucus seized control over committee assignments and chairpersons. Simultaneously, however, the rank and file members moved to "democratize" the House, adopting a "subcommittee bill of rights" that, in effect, created "subcommittee government" in Congress. (On these developments, see Sundquist, 1981; Jones, 1981b; Ornstein, 1981; Rieselbach, 1977.)

Research on the effects of these changes, sketchy as it is, seems to suggest that reformers alter current congressional conditions only with great difficulty. Moreover, the changes that have occurred often seem either unanticipated or undesirable. For instance, the accountability reforms do not appear to have had appreciable impact on congressional ethics—witness Abscam and the drug and sex allegations of 1982. Nor do citizens seem more aware of their elected representatives, either in general in terms of name recognition or recall or with respect to issue positions (Mann & Wolfinger, 1980). The "sunshine" reforms, however, may have contributed to an increased rate of retirement from Congress (Cooper & West, 1981). Whether they have made the legislative process more vulnerable to interest group influence—already difficult to assess—especially that of "single-issue" organizations, as some observers assert, cannot be answered without investigations that have not yet been undertaken.

Similarly, it is perhaps too soon to judge the effects of other reform thrusts. It is certainly hard to claim conclusively that Congress has greater control over the executive than in the pre-reform period. The War Powers Act has not been tested. The Budget Act has produced mixed results in practice; as the new procedures have shaken down, the legislature seems clearly to have restricted executive impoundments, but not necessarily to have altered the contours of fiscal and monetary policy (Ippolito, 1981; Schick, 1980; Wildavsky, 1979). The safest conclusion seems to be that Congress is better equipped institutionally to challenge the president, but is by no means firmly committed to engaging in combat.

Finally, the internal reforms seem to have cut in opposite directions. On the one hand, the parties are more powerful: the Speaker has made successful if infrequent use of his new powers, but with uncertain consequences. The Rules Committee regularly supports the leadership (Oppenheimer, 1981); multiple referral may both slow the legislative process and increase the likelihood that bills will be heavily amended; ad hoc committees, when created, have been circumscribed and only partially effective (Vogler, 1981; Oppenheimer, 1980). On the other hand, committee reforms on balance seem to have enlarged congressional decentralization: full committee chairpersons are on shorter leash, but much of their power has devolved to antonomous subcommittees (Davidson, 1981). Moreover, reform impinges differentially on individual panels; some (e.g., House Agriculture) seem immune to the reformers' prescriptions; others (e.g., Ways and Means) respond to drastic surgery (Ornstein & Rohde, 1977; Rieselbach & Unekis, 1981-82). Overall, at least some careful observers (e.g., Huntington, 1973; Dodd, 1980a, 1981) seem persuaded that Congress is more fragmented, more hard pressed to formulate coherent policy or to engage in careful oversight than a decade ago. In any case, the reform movement of the 1970s has done little to gladden the hearts of the executive force or responsible parties theorists.

In reality, then, reform—and research evaluating it—reflects the Congress; it has been pragmatic, inspired by practical politics not philosophical principles.[19] Yet models with normative import do exist, as do theories of empirical consequence. Pursuing the latter should provide data for assessing the former. Once we know how Congress operates, we may feel more comfortable in prescribing for any maladies we find afflicting the institution. However underdeveloped our models, both empirical and normative, at present, they do sensitize scholars to the need to look beyond the trees to the entire forest.

IV. CONCLUSION: TRAILS BLAZED, AND FOR BLAZING

For a young subfield, legislative studies, especially work on Congress, has made remarkable progress in exploring the unknown. Scholars have blazed many trails. They have investigated, to at least a modest extent, virtually every aspect of congressional membership, structure, and performance. They have illuminated the terrain particularly with respect to congressional elections and roll call voting, and they also have begun to lay the groundwork for fuller mapping of committee and party processes and executive-legislative and interest group-lawmaker relationships. At a somewhat broader level, some integrative efforts have produced substantial enlightenment, in the form of middle range theorizing, about committees, representation, and subgovernments as well as elections and legislative voting. Understandably, grand theory has been slower to develop, but even here there are both empirical (organizational, purposive, and systems-role) and normative (executive force, responsible parties, literary, and congressional supremacy) perspectives that, while underdeveloped and too seldom used, constitute preliminary charts of the legislative territory. In short, research to date has staked out many trees, specified several stands of timber, and pointed toward some general pictures of the forest. These are unmistakably substantial and significant accomplishments.

There remain, of course, numerous challenges ahead. Future research might explore profitable areas where hard data are not so readily available. We need basic studies, for instance, of members' motives, perceptions, opinions, and even their personalities, and the ways in which such subjective factors shape legislators' behavior. We need to describe relatively unknown features of Congress more fully: staffing practices, support agencies, state delegations and other informal organizations, and the formal rules and their impact, inter alia. Most important perhaps, as noted, there is a need to deal with influence (power) processes, to chart the ways that various interactions—between executives and members of Congress, lobbyists and legislators, lawmakers and staff, representatives and constituents, and congresspersons and their colleagues—occur and the effects that these processes have on the participants' performance. These are difficult tasks, full of pitfalls, but unless we can cut through some of these tangles we will be unable to isolate and understand fully the causes and consequences of congressional behavior.

We also need to extend and apply the theorizing presently available. Fenno's useful approach to committee politics should be applied to the full range of panels. A comparable scheme for assessing political parties—encompassing the partisans and their motives, their relations to the electorate, their place in congressional party structures (e.g., the caucus), the organizations' modes of operation, and the character and effects of leadership strategies and tactics—would obviously have extraordinary value. A similar model of interest group politics would have equivalent importance. From such efforts, a theory of legislative influence might emerge; at least the centrality of interpersonal power relations in a decentralized institution argues for the effort to understand the exercise of authority in Congress.

Finally, while grand integrative theorizing may be quixotic, it remains appealing: mapping the forest is, after all, the ultimate goal. At the minimum, efforts to use, and thus to assess the utility of, the extant models should proceed. How far can organizational, purposive, and systems-role notions take us toward describing and explaining legislative politics? These models have not really been put to fair tests as integrative, heuristic, or predictive devices. Specifically, we might ask whether these, or alternative approaches, can provide a picture of the policy process. This, too, is no easy task; Congress is involved in each and every stage of policy—initiation, enactment, and implementation—though not always to the same degree. To model the entire process is, in effect, to model much of American politics. Nevertheless, some first steps seem clearly identifiable.

First, policy itself—the ultimate dependent variable—requires explicit specification. We need to move beyond issue domains (Clausen, 1973) and the classic Lowi (1964) categories (distributive, regulatory, redistributive). It has been particularly difficult to define the boundaries that distinguish among the latter. Having established what is to be explained, we can begin to look at variations in the origins of the different policy types. For instance, external actors may have greater opportunities to originate some forms of policy. Or legislators may assume differing role orientations with respect to separate policy categories. Similarly, we may eventually be able to specify more precisely distinct processes—with various outside forces and internal structures like committees and parties combining in particular ways—for given policy types. From this, it may prove possible to move on to assess more carefully the ways in which the legislature follows up on its actions; perhaps oversight varies for different policy types or processes.[20] These (and numerous other) possibilities suggest, however sketchily, a need to move forward, empirically and theoretically, to treat the total policy process.[21]

Future studies, whether narrow or broad, need to be conscious of change, evolutionary or intentional. Single propositions and general theories are at the mercy of events: international and domestic developments may raise new issues for congressional consideration; election results may alter the identities of executives who promote policy and of legislators who respond to new initiatives. Reform may alter structures, processes, and products in ways anticipated or unexpected. Scholars, in consequence, must reexamine basic generalizations; they must test their theories and models constantly to see not only whether their propositions remain accurate but also whether the variables they employ continue to constitute the full set of relevant considerations.

If the changing contours of Congress, trees and forest, can be charted carefully, then research may mesh with other developments in the legislative politics subfield. Understanding one legislature—and Congress is surely the assembly about which most is known—should encourage genuine comparative research. Indeed, complete comprehension of Congress requires comparative analyses within chambers (committees, majority and minority parties), between the House and Senate[22] (elections, norms, rules, policy product), and over time (change and reform). Accurate generalizations and useful models, partial or full, about Congress could provide hypotheses that can be tested in other legislative settings. While such propositions may be disconfirmed elsewhere—Congress may prove to be a unique assembly—testing them should point to characteristics of legislatures generally that any full theory must take into account. In the long run, we seek such a full theory, one that is truly valuable for comparative and longitudinal analysis. It is highly unlikely that theories of Congress will suggest most, or even any, of the elements of a complete model. But in conjunction with the developing research on cross-national and domestic state and local legislatures—which space considerations have made impossible to treat here—they may help advance the long-term search for a theory of legislatures.

Thus, congressional scholarship has covered considerable ground during a quarter century of renewed exploration, displaying an enormous substantive, theoretical, and methodological virtuosity in the course of these investigations. Still, there is probably only one safe conclusion to draw from this cursory and partial survey of the legislative terrain: there is no scholarly consensus, no widely preferred models or methods, about the most appropriate approach to the study of legislatures. Research has proceeded eclectically and empirically, frequently though decreasingly unencumbered by explicit theorizing, focusing both on structures and processes, in a venerable tradition of institutional analysis, and on the attributes and activities of individual lawmakers, in a more recent mode of behavioral analysis. There remains, however, enough unknown territory to engage the attention of as many scholars as care to study legislatures. We are not out of the woods yet.

FOOTNOTES

1. There were some classic clearings in the woods. See, inter alia, Wilson (1885); Brown (1922); Chiu (1928); and Follett (1896) on congressional leadership; and Herring (1929); Schattschneider (1935); and Truman (1951) on group politics. The fact remains, however, that the most significant work is of much more recent vintage.
2. Any serious effort to cover the full range of materials on legislative politics, as a subfield, within the confines of a single paper for a single panel, is doomed to fail. Thus, the focus here is on Congress, as the legislature about which most is known at present. This limited concern, needless-to-say, is in no way intended to minimize the substantial and important work on local and state legislative bodies or the innovative and integrative efforts to develop comparative perspectives on legislatures. But what we have learned about Congress should provide some direction for those interested in moving into these even less well charted areas.
3. For other efforts to assess the "state of the discipline," see Peabody (1969); Meller (1970); Eulau and Abramowitz (1972); Huitt (1976); Rohde and Shepsle (1978); and Cooper and Brady (1981). For broader, comparative views, see Polsby (1975); and Mezey (1979).
4. For instance, the congressional stability ("institutionalization") of the 1960s seems to have given way to a period of increasing flux in the 1970s. (Compare Polsby, 1968, and Cavanagh, 1980.)
5. Space considerations preclude extensive citation. Where possible, I have tried to cite studies that are basic contributions and that also summarize much of the literature. In consequence, a great number of quality studies are omitted here. For a momumental bibliography, listing more than 5500 citations, see Goehlert and Sayre (1982).
6. Recent party resurgence, especially by the Republicans, stressing central fund raising and modern campaign technologies, suggests that party influence may be greater in the 1980s than it was during the period covered in the literature.
7. Where these votes fall on the congressional voting continuum, in contrast to how accurately they reflect district opinion, is a more controversial matter. At issue, and unresolved, is whether members from "marginal" districts (defined variously)

take more moderate roll call positions than their "safe" colleagues. (See, inter alia, Fiorina, 1973, and Sullivan and Uslaner, 1978.)

8. This is not to denigrate description. Sound analysis must of course, rest on detailed understanding of "the facts." The forest, after all, *does* consist of trees.
9. For a preliminary effort to specify some of these issues, see Unekis and Rieselbach, forthcoming.
10. A similar set of problems exists with respect to committee member-committee staff interactions. We remain largely ignorant of the roles staffers play; the extent to which they influence, or are influenced by, their nominal principals; and the variations in staff-committee relationships and in the resultant panel performance across committees. (See Patterson, 1970; Price, 1971; and Salisbury & Shepsle, 1981.)
11. The place of informal norms (folkways) in congressional politics is equally cloudy. Again, interpersonal relations are central, and it is difficult to get a clear view of socialization processes, the content of the legislative "culture," and the ways in which custom and tradition impinge on member behavior. (See Matthews, 1960; Asher, 1973; Rohde *et al.*, 1974; Loomis & Fishel, 1981.)
12. For case studies that illustrate, typically or not, the problems of ascertaining groups' influence as distinct from their activities, see Gelb and Palley (1979), and Vogel and Nadel (1977).
13. Matthews and Stimson (1975) reach similar conclusions employing cue theory explicitly. The model, however, is not the full story, for as Weisberg (1978) demonstrates, as predictive devices, various analytic schemes, including cue-taking, do not move much beyond a simple "baseline model" of partisanship; projecting that members will vote with their party's majority yields almost equally accurate predictions.
14. This is not to suggest that short-term forces are irrelevant, or that individual representatives never shift their positions. Issues, such as abortion, may shift in salience. Or they may appear in new guises: civil rights seems less a "legal" than a "social" matter in the 1980s (see Deckard, 1976). Finally, lawmakers' situations may shift—a new committee assignment or leadership position, or a shift in the occupancy of the White House—and alter their perspectives on particular questions. What such change entails, of course, is a need to revise the cue-taking calculus; it does not undercut the utility of the approach.
15. For insightful efforts to broaden the treatment of subgovernments to encompass a wider variety of legislative circumstances, see Heclo (1978) on "issue networks," and Jones (1982, pp. 358-365), on "large sloppy hexagons."
16. To be sure, combining the institutional and individual perspectives has become increasingly common, but many studies remain squarely in one or the other of these two research traditions.
17. This, of course, is all that economic models claim to do. It is not necessary to stake out each tree in order to describe the forest.
18. For instance, McCrone and Kuklinski (1979) find that state legislators espousing the delegate orientation act consistently with it only when their constituents send them unambiguous messages.
19. It is, of course, true that normative judgments reflect individual values, and that political scientists have no special claim to either expertise or superiority in the realm of values. Still, congressional scholars have been loath to theorize, normatively or empirically; as a result, they have done less than they might have to pose the evaluative questions clearly and precisely. Michael Mezey and Gerald Wright (personal communications, 1982) suggest two causes of this reluctance. First, Congress is extraordinarily accessible; a superabundance of data about the legislature encourages careful but narrow empirical work (on the "trees") that

retards broader concern for theorizing (about the "forest"). Second, and probably as a consequence of the first, students of Congress come quickly to develop a great affection for the institution, and for their colleagues who research it. Thus, there is a consensus, favorable to Congress, within the scholarly community that discourages critical commentary, to say nothing of normative condemnation.

20. I have neglected oversight to a large extent, but it remains a basic, though elusive feature of the legislature's activity. For summaries of what we know to date, see Harris (1964); Ogul (1976); and Dodd and Schott (1979). Needless-to-say, there are innumerable facets of legislative-administrative relationships in need of exploration.
21. There have been some valuable beginnings here. In addition to case studies (e.g., Sundquist, 1968, and Reid, 1980), Orfield (1975) has tried to identify the conditions when Congress can exert its institutional influence most effectively. Similarly, Brady (1982, inter alia) and his collaborators have sought to specify the occasions (e.g., realigning eras) when the electoral process contributes to policy innovation in Congress. Nonetheless, these insights need to be integrated within broader theories.
22. The upper chamber has received less attention from contemporary scholars; there are fewer Senators, they seem to be less accessible, and their more heterogeneous constituencies are more difficult to categorize. The situation may be changing, however: see Baker (1980) and Foley (1980).

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7

Judicial Politics: Still a Distinctive Field*

Lawrence Baum

The decade of the 1960s was a time of upheaval in the field of judicial politics. New ideas challenged old assumptions, and new kinds of research changed the field's contours. By late in that decade one could have predicted confidently that the field would look quite different fifteen years later. Indeed, the field has changed considerably. But that change has taken forms which few people in the 1960s might have expected.

This essay is an effort to sort out what has happened to the field of judicial politics in the last fifteen years. It is not a comprehensive analysis of the research in that period. Nor is it offered as a definitive interpretation of the state of the field; it is doubtful that such an interpretation is possible.

The field of judicial politics may be defined in terms of subject matter or in terms of a set of scholars who specialize in the field. It is most useful for this essay to define the field in subject-matter terms. My definition is broad, including within it all processes that involve the courts directly. It does not include "legal" concerns to which the courts are peripheral: police as administrators, prisons, lawyers as a profession, or constitutional issues outside the context of judicial interpretations.[1] The essay will also be restricted to judicial politics in the United States. The growth of literature on courts in other nations is very welcome. But, in order to keep this essay manageable, I will exclude that literature from consideration here.[2]

In one important respect the essay's domain has been defined in terms of a set of scholars. Because my interest is in the state of judicial politics as a field of political science, the essay will focus primarily on the work of political scientists—although a good deal of attention will be given to interdisciplinary links in the study of the courts. Moreover, much of this essay will interpret the

*In developing this essay I was aided immensely by the suggestions of a number of scholars. I benefited particularly from the comments of Beverly Cook, Ada Finifter, Sheldon Goldman, Leslie Friedman Goldstein, Herbert Jacob, Charles Johnson, John Kessel, Elliot Slotnick, Susette Talarico, and especially James Farr.

state of the judicial politics field in terms of the perspectives of those political scientists who specialize in it.

The essay is divided into three parts. In the first, I will examine three important trends in the field of judicial politics over the past fifteen years. In the second, I will suggest an interpretation for those trends. The final section will evaluate the current state of knowledge in the field in light of the directions that it has taken.

In preparing this essay I benefited a good deal from other surveys of the field.[3] In addition, the essay is based in large part on a quantitative analysis of the judicial politics literature in the 1962-81 period. That analysis focused on articles in six political science journals, three law and social science journals, and doctoral dissertations.[4]

THREE RECENT TRENDS

The field of judicial politics has changed in many ways over the past fifteen years. Three types of changes seem particularly important, in part because they may have been unexpected. The growing diversity of scholarship in the field stands out most prominently, and I will give it particular attention. But two other changes also will be examined: (1) the resurgence of an interest in judicial outputs and normative questions; and (2) the establishment of new ties with other disciplines.

Diversity. Judicial politics always has contained a considerable range of scholarly interests. In past eras, however, there seemed to be a core to the field around which most scholarly work clustered. This is no longer true.

For a considerable length of time, the core of the field was constitutional law, generally involving critical analysis of the Supreme Court's work as interpreter of the Constitution. The revolution that culminated in the 1960s shifted the core to judicial behavior, generally involving analysis of the forces that shape the Court's policy choices. In the latter half of the 1960s a large share of published research dealt with Supreme Court decision-making, and this subject was at the center of the field's concerns.

In the study of judicial behavior there was a central issue: the role of "political" factors, primarily judges' personal policy preferences, in judicial decisions. Much of the impetus for the systematic study of judicial decision-making was methodological, with students of the courts focusing on decision-making as a means to apply sophisticated quantitative methods to judicial politics. But at a theoretical level the judicial behavior movement had as its primary purpose the refutation of a mechanistic view of judicial decisions. Research on decision-making was aimed at establishing the importance of policy preferences as bases for decisional choices and ascertaining the ways in which preferences arose and influenced decisions. This research did support a non-mechanistic view of decision-making by showing the consistency with which particular judges took particular ideological positions, the significance of group dynamics on multi-member courts, and the relationship between judges' political party affiliations and their decisional tendencies (see Goldman & Jahnige, 1976, ch. 5).

Even at the height of the field's focus on judicial behavior, a good deal of research dealt with other subjects—quite aside from the continuing work in constitutional law. But most of this research shared a primary interest in the Supreme Court; certainly this was true of the developing bodies of work on the impact of court decisions ("judicial impact") and political litigation. Much of this research also reflected an effort to challenge legalistic assumptions about the judicial process. Judicial impact research questioned the assumption that Supreme Court decisions are implemented faithfully. Political litigation research implicitly challenged the view that litigation is activity undertaken by individuals for individual purposes.

Thus, as of the late 1960s the field had a core subject matter and something of a central theme. Neither has disappeared; both Supreme Court decision-making and the challenging of legalistic assumptions remain important. But the period since the 1960s has seen a great expansion of subject matter and a subtle movement away from a central theme. The field has taken with a vengeance C. Herman Pritchett's advice to "Let a hundred flowers bloom" (1968, p. 509).

Within the judicial behavior area the subjects of research have broadened. Most notably, an increasing volume of work studies courts other than the Supreme Court. Research on the federal courts of appeals, for instance, has proliferated (see Goldman, 1975; Howard, 1981). The sentencing decisions of trial judges have become a major focus for the study of judicial behavior (Gibson, 1978b; Levin, 1972; Kuklinski & Stanga, 1979).

A second expansion is in the stages and types of judicial decisions that are studied. At the appellate level research increasingly has examined decision points other than the decision on the merits (Rohde & Spaeth, 1976). A particularly large body of research now exists on the selection by appellate courts of cases to be heard. This research has demonstrated that policy preferences play a central role in this stage of court action (Provine, 1980; Ulmer, 1972, 1978; Baum, 1977). Trial court research has introduced other types of judicial decisions to the area. Predominant among them is sentencing, but other stages such as bail-setting have been studied also (Flemming, 1982).

Finally, there has been some broadening in the decisional influences that scholars study. Because of the issue that dominated the area in the 1960s, research in that era dealt primarily with policy preferences or with preference-related variables such as social backgrounds. This is somewhat less the case today. Most important, substantial research has been done on the impact of judges' external environments (Kuklinski & Stanga, 1979; Gibson, 1980). This development followed a movement away from the Supreme Court to lower courts whose environments could be compared in relation to their decisions. Much of this research has shown geographical differences in decisional patterns and suggested the importance of local political culture and public opinion in shaping judicial decisions. Of particular interest are two studies of the sentencing of selective service offenders during the Vietnam War era. The two studies differ somewhat in their findings but agree that district judges' decisions were heavily influenced by their political environments (Cook, 1977; Kritzer, 1978).

In the 1960s, as I have noted, considerable research was done in areas related to the core of the field. The most notable of these areas were political litigation and judicial impact. In both areas the anti-legalistic theme that provided a focus for early research has weakened, and this change has helped to broaden the subject matter of research.[5]

In the case of political litigation,[6] there continues to be some debate over the extent of interest group involvement in litigation (O'Connor & Epstein, 1981-82). But most scholars have assumed the importance of policy goals and group activity in certain areas of litigation rather than seeking to demonstrate it. Recent studies generally have abandoned the earlier focus on major Supreme Court cases, looking at litigation activity throughout the judiciary and sometimes linking it with interest group activity elsewhere in the political system (Sorauf, 1976; O'Connor, 1980; Olson, 1981). These studies have underlined the complexity of political litigation processes, and they have helped to dispel the idea that broad litigation campaigns are easily undertaken and coordinated.

Even more than political litigation research, early studies of the impact of judicial decisions constituted a challenge to conventional legal wisdom. The largest body of work focused on the resistance of lower courts and administrators to controversial Supreme Court decisions on issues such as school desegregation and criminal procedure. Through this research it became clear that willing implementation of judicial politics is neither automatic nor inevitable (see Wasby, 1970). Since that point was established, a steady stream of research has continued. Much of this research continues to examine the implementation of single major Supreme Court decisions (Canon, 1973), but some has moved beyond this focus. Most notably, the impact of lower courts has begun to attract research. One form of lower-court research concerns state tort doctrines; two such studies have provided evidence that doctrines promulgated by state courts influence spending on insurance and hospital room rates (Croyle, 1979; Canon & Jaros, 1979). An increasing volume of research deals with institutional relationships among courts or between courts and the other branches of government, outside the context of specific decisions. For instance, Stephen Wasby has shown the problematic character of communication from the Supreme Court to police officers (1976).

By far the most important move toward diversity in the field of judicial politics has been the growth of trial court research. Through the mid-1960s the study of judicial politics by political scientists was devoted overwhelmingly to appellate courts. The primary subject, of course, was the Supreme Court. But even when research ventured beyond the Court, it was generally to the federal courts of appeals and state supreme courts. Now all that has changed.

Significant political science interest in trial courts began on the criminal side, perhaps in part because of the growing concern with criminal justice in American society. Some of the diverse research on criminal trial courts already has been discussed. Not surprisingly, the most common areas of research have been sentencing decisions and plea bargaining.

The interest in sentencing followed naturally from the field's existing focus on judicial behavior; as noted earlier, some scholars have studied sentencing in order to probe issues in the determinants of judicial decisions. Others have joined the effort to examine normative issues such as group discrimination in sentencing (Uhlman, 1979; Spohn, Gruhl, & Welch, 1981-82). The results of that effort have been inconclusive, with disagreement among studies about the existence and extent of discrimination by race and sex.

Early social science research on plea bargaining was aimed primarily at establishing the centrality of bargaining in most criminal courts and determining the ways in which it worked. Much of the more recent work by political scientists has focused on explanations for the prevalence of bargaining. Work by Milton Heumann (1978) and Malcolm Feeley (1979, ch. 8), among others, has helped to establish that heavy court caseloads represent an incomplete explanation for plea bargaining; other motivations such as the desire to avoid uncertainty also play major—probably more important—roles. Another body of research has probed the assumption that defendants who plead guilty receive sentencing rewards, with little consensus emerging as to the validity of this assumption (Eisenstein & Jacob, 1977, ch. 10; Uhlman & Walker, 1979; Brereton & Casper, 1981-82).

Political scientists turned to the civil side of trial court activity even more recently. Marc Galanter's highly influential article on civil litigation and its outcomes (1974) is instructive. Galanter sought to demonstrate "why the 'haves' come out ahead" in court; he showed that the "haves" benefit from a series of basic structural advantages. One advantage that Galanter emphasized was the tendency for "haves" to be recurring litigants, while "have-nots" tend to use the courts infrequently. Although the article's concerns were avowedly political, its author was a law professor who drew relatively little from the work of political scientists.

At about the same time, political science research on civil litigation began to grow. Austin Sarat and Joel Grossman offered a major theoretical view of litigation decisions, discussing court usage in the context of other adjudicative institutions (1975). Craig Wanner examined the allocation of values by trial courts (1974, 1975), documenting the success of government and business organizations relative to individuals in three cities. A few studies have examined litigation patterns through analyses of court records, showing changes in the distribution of cases among areas of law over time (McIntosh, 1980-81) and urban-rural differences in litigation rates (Daniels, 1982). The interdisciplinary Civil Litigation Research Project produced a wealth of theoretical insights and empirical data (Special Issue, 1980-81). Among its important findings was the existence of a great variation among types of legal problems in the frequency of both the use of the courts and other kinds of actions to redress grievances (Miller & Sarat, 1980-81). While political scientists' contribution to the understanding of civil justice remains relatively limited, this area seems to have become an integral part of research in the field.

Trial court research has shared the anti-legalistic theme common to other bodies of research in the field. Both criminal and civil court studies have challenged assumptions about the use of formal procedures to resolve disputes and about even-handed application of the law. As in other areas of judicial politics, this theme began to fade once the defects in the traditional assumptions were established.

Outputs and Normative Issues. Aside from methodology, the study of constitutional law and the study of judicial behavior differ in two fundamental ways. The first is the distinction between a focus on the policies that courts produce and the process of making policy. The second is the distinction between an explicit focus on normative issues and an approach intended to minimize attention to those issues. Thus the shift in the core of the judicial politics field that occurred in the 1960s was from outputs to process, from normative to empirical concerns. The other types of work that emerged during the 1960s were process-oriented also, reflecting a deliberate shift in focus (see Peltason, 1955, p. 1), and to a lesser extent they shared a limited interest in normative questions. It may have appeared at the time that the field was abandoning its historic interests in the substance of the courts' work and in values.

This has not occurred. The lines between process and outputs and between empirical and normative are difficult to draw, but it is clear that research dealing with outputs and normative-centered inquiries have survived. Indeed, both have enjoyed something of a resurgence in recent years.

First of all, constitutional law has maintained a foothold in the field. Scholars continue to produce analyses and evaluations of Supreme Court doctrine from a variety of perspectives. It is particularly notable that this area has attracted new practitioners from the generation trained since the 1960s (see Baer, 1978; Binion, 1982), and it remains a staple of dissertation research. But this scholarship has not remained static. Recent work in constitutional law frequently reflects an effort to apply the perspectives and findings of judicial process research. The change that this effort has brought about is reflected quite clearly in some current constitutional law texts, which incorporate research on decision-making and the effects of decisions (Goldman, 1982; Grossman & Wells, 1980).

Analysis of judicial policy outputs has taken other forms as well, sometimes combining concerns that grow out of the constitutional law tradition with newer interests. One small but important body of research in this vein involves linguistic analysis of court decisions. The importance of this work lies largely in its partial challenge to one tenet of much of the work on judicial decision-making, the assumption that judges manipulate legal language to produce their preferred outcomes. Those who undertake linguistic analysis, in contrast, emphasize the ways in which legal language directs and constrains judges in their choices (Brigham, 1978; O'Neill, 1981).

There also have been several bodies of research that are concerned with the general roles of courts in the making of public policy. The long-standing debate about the propriety of judicial activism continues (Halpern & Lamb, 1982). That debate has acquired new life with Donald Horowitz's argument that courts are limited in their capacities to make effective policy choices in the area that he called "social policy" (1977). Several scholars have addressed that argument, analyzing the issue of courts' policy-making capacities; most disagree with Horowitz (Cavanagh & Sarat, 1980; Wasby, 1978).

On a different level, a few scholars have followed Robert Dahl (1958) in examining the relationship between Supreme Court policy and the "lawmaking majority" in the other branches (Funston, 1975; Casper, 1976; Adamany, 1973). Jonathan Casper's article was particularly important, because it made a strong case that the Court plays a more significant independent role as a policy-maker than Dahl concluded. The same kind of question has been addressed in even broader terms in research that explores the position of the courts in the political regime (Villmoare, 1982; Roelofs, 1982; Balbus, 1973).

Many of the new forms of output research, such as the work on judicial capacity, have a central normative concern. Some types of research on judicial processes also have developed a strong normative tinge. Plea bargaining research, for instance, frequently deals with the propriety of bargaining as a means of resolving cases. Another kind of normative interest is reflected in the general movement to evaluate judicial policies and procedures through empirical analysis of their actual or potential consequences. Political scientists increasingly have become involved in that movement (Stookey, 1980; Roper, 1980; Dubois, 1982); perhaps their main contribution has been to question and to probe assumptions about the beneficial effects of a variety of judicial "reforms."[7]

Thus the new directions of the 1960s have come to co-exist with earlier traditions in the field rather than obliterating them. Research concerned with outputs and with normative issues has been influenced a good deal by the newer tides, particularly in the heavier empirical component of recent work with normative interests. But that research clearly has continued to thrive.

Interdisciplinary Links. The field of judicial politics traditionally has had strong links with other scholarly disciplines. During the era in which constitutional law was the core of the field, the major links were with legal scholarship and history. The relationship with legal scholarship was particularly strong, because the doctrinal analysis in which the field specialized was very similar to the predominant form of research in the legal field.

These links weakened considerably in the 1960s. Most directly, they weakened because of the growth of process-centered interests that were not shared with law and history. This shift in subject matter also reflected a desire to turn the field toward what seemed to be the mainstream of political science and away from other disciplines. The new emphasis on judicial behavior, a subject that fitted well in the mainstream and less well into other disciplines, seemed to portend a limited importance for interdisciplinary ties in the future.

Yet during the same period a new set of links with other disciplines was in the process of formation. Political scientists began to develop ties with other social scientists and legal scholars who shared an interest in the systematic analysis of legal processes. These ties strengthened in the 1970s, and they have become impressively strong. In some respects the study of judicial politics within political science now appears to be just one loose category in social science research on law and the courts. Indeed, the discussions of judicial politics literature in this section seem artificially limited because of the exclusion of relevant work by scholars in other disciplines.

The development of a field of study that transcends disciplinary boundaries requires mechanisms for communication across those boundaries. In this instance the most important mechanism has been the Law and Society

Association, which was founded in 1964. As the Association became better established and more visible, its meetings and journal provided places for political scientists to exchange ideas and information with colleagues from other disciplines. The *Law & Society Review* has become the most important journal for social science research on legal issues, and political scientists have contributed heavily to it.

Other institutions have served similar communication functions. Among them are several interdisciplinary journals. These include *Judicature,* which has become increasingly receptive to social science research, and some newer publications such as *Law & Policy Quarterly.* At some universities networks have developed among scholars who share legal interests.

The strengthening of links with other disciplines is closely related to the development of some new interests in the judicial politics field since the late 1960s. A continued focus on judicial behavior in appellate courts would have limited interdisciplinary ties, because this is not a subject of central interest in other social sciences. When political scientists became more interested in subjects that *were* of central interest in other disciplines, particularly subjects related to civil litigation and criminal court processes, a basis for closer relationships was established.

Of course, the links with other disciplines helped to bring about these new interests. Political science research on criminal courts, for instance, owes much to work by sociologists that preceded it (Blumberg, 1967; Sudnow, 1965). Expansions of interest within judicial politics were much easier because research in other disciplines provided a base from which to work.

More generally, scholarship in other disciplines has had a profound effect on research on the courts by political scientists. The influence of individual scholars such as Abraham Blumberg (1967) and H. Laurence Ross (1970) is easy to identify. Broader currents such as anthropologists' interests in dispute settlement mechanisms also have had major impacts (see Shapiro, 1981; Mather & Yngvesson, 1980-81).

In an earlier era the work of political scientists in the general area of constitutional law could be distinguished from the work of scholars in other disciplines by its greater emphasis on political motivations and the contexts of judicial policy (see Murphy & Tanenhaus, 1972, pp. 13-17). Can a similar distinction in tone or approach be found today? I do not think that any sharp distinctions exist. The adoption by political scientists of approaches and frameworks from other disciplines has helped to bring about commonalities in research on particular topics.[8]

The sharpest distinction that exists is in subject matter. A large share of political science research in judicial politics continues to fall into areas that are of limited interest to other disciplines. Foremost among them is decision-making in appellate courts, which remains a mainstay of the field. Another area which has been much more important to political scientists than to other scholars concerns relationships among courts and between courts and other public policy-makers. Because of this divergence, the judicial politics field cannot be viewed as fully integrated into a larger interdisciplinary field of study.

A DISTINCTIVE FIELD

The three trends discussed in the preceding section involve different aspects of the judicial politics field which have developed in unexpected ways. For that reason they raise broad questions about the directions that the field has taken and the reasons for its current form. These inquiries will be the concern of this section.

It seems to me that an understanding of the judicial politics field in political science must focus on its distinctive character. To a degree, this would be true of any field in the discipline. Each field develops a degree of isolation from others and, partly as a consequence, its own traditions and approaches to its subject matter. Thus fields come to be defined not only in terms of suject matter, but also in terms of the perspectives of the people who specialize in the study of that subject matter.

Judicial politics is no exception to this general rule. Indeed, within the broader field of American politics it seems to be unusually distinctive in that it is both more isolated and closer to unique in its characteristics than are most other fields. This was not always true; early in this century judicial politics by its various names was somewhere near the center of the study of politics, because of the legalistic approach that pervaded so many fields. But gradually the rest of political science drifted away from a concern with law, thereby severing a link with the judicial politics field. With that link gone, for several decades there has been something of a gulf between judicial politics and other fields of political science.

The primary cause of that gulf, I think, is another gulf—the one that is perceived to exist between the judiciary and the rest of the political system. In part, this perception may flow from a residue of the myth that courts are "non-political"—though presumably few political scientists accept that myth. More important are the real differences between courts and most other policy-makers, including the legal context in which judicial policies are made and the partial isolation of courts from certain external influences such as interest group lobbying. It also is important that the courts operate outside the "normal flow" of the policy-making process through the legislative and executive branches.

These characteristics inevitably help to produce a view that courts are to be studied separately and in their own terms. That view is reinforced by the intimidating effects of legal language and procedure on non-specialists. As a result of both factors, fields of study that might include courts in their purview instead have fenced them off.

The avoidance of courts by political scientists who specialize in other fields seems to have weakened in the past two decades, in part because of the growing activism of American courts and a growing general interest in the judiciary. One sign of change is an apparent increase in the number of political scientists whose work is primarily in other fields but who have contributed to research on judicial politics, either alone or in collaboration with judicial specialists (e.g., Hansen, 1980; Costantini & King, 1980-81; Spohn, Gruhl, & Welch, 1981-82). But these are exceptions; in general specialists in fields other than judicial politics continue to leave the courts aside.

A good example of this neglect occurs in the relatively new field of public policy. The courts fit well into most of the major concerns of this field, such as agenda-setting and implementation, and one might expect courts to be integrated into the field's work. Indeed, a good deal of public policy scholarship does take the courts into account, sometimes in an integral way (Anderson, Brady, & Bullock, 1978; Edwards, 1980). But the public policy field as a whole gives a great deal less than proportionate attention to the courts, and frequently they are ignored altogether.

Some other fields that could include courts in their research do so to an even more limited degree than does the public policy field. The study of political participation and voting behavior is a particularly good example. Students of voting behavior seldom deal with judicial elections, though this omission stems in part from the field's national focus. Even more notably, the "modes of participation"[9] that scholars in the field study seldom include litigation (see Zemans, 1983).[10]

The avoidance of the courts by most scholars has been matched by what Theodore Becker called "a fatal attraction" which they have held for others (1970, p. 381).[11] Particularly in earlier periods, many political scientists who chose to study the courts were attracted to that area because of what seemed to be unique about the judiciary: the legal mode of reasoning in litigants' arguments and judges' decisions; the explicit addressing of normative and constitutional questions in opinions. As a result, they were predisposed to deal with the courts in terms of those characteristics rather than as political institutions analogous to legislatures and bureaucracies. Moreover, because courts operate so much in terms of the law, it seemed appropriate to retain a somewhat legalistic approach to their study even when that approach had been rejected as too limiting for other institutions.

In combination with the attitudes of political scientists outside the field, the perspectives of judicial specialists brought about an unusually severe isolation and a particularly divergent approach to the study of politics. Almost inevitably, judicial politics "developed a vocabulary and a set of methods somewhat marginal to the mainstream of political science" (Vines, 1970, p. 140). Certainly it is significant that the field could maintain a focus on constitutional law, a subject quite different from the fodder of most other fields in American politics. The general use of "public law" as a title for the field underlined its separation from other fields of study in American politics and suggested an orientation toward law rather than political science.[12]

Beginning with the work of C. Herman Pritchett in the 1940s (1941, 1948), the field began to develop in a way that brought it closer to other fields of study in American politics. The growing acceptance of what Martin Shapiro called political jurisprudence (1964, p. 15) was important, because it meant that courts increasingly were viewed as political as well as legal institutions. Meanwhile, the new focus on judicial behavior brought to the field's core a subject matter that was similar to the subjects of some other fields in American politics and brought in research methods of the type that were gaining favor in the discipline as a whole. These two developments were at their strongest in the 1960s, and by late in the decade Joel Grossman and Joseph Tanenhaus could report that the field "is now fast returning to the fold" (1969, p. 3).

In some important respects the integration process has continued. For instance, students of judicial politics increasingly have adopted perspectives and methods that are similar to those used in other fields of American politics. But in other respects the process of integration into the broader American politics field has slowed or even reversed. The three trends discussed in the preceding section represent both causes and consequences of the field's limited integration; it will be useful to take a second look at these trends from that perspective.

The growing diversity of research in the field represents a basically healthy process: the breaking down of past limits on the scope of judicial research.[13] But the sources of this diversity are noteworthy, because they suggest the continuing isolation of the field. Research on new topics has come overwhelmingly from specialists in judicial politics, not from specialists in other fields for which particular judicial processes are relevant.[14] Public policy specialists have not been responsible for research on agenda-setting in the courts. Students of state politics have not analyzed the impact of state court doctrines. When the litigation explosion helped to publicize "ordinary" (i.e., non-political) litigation as a concern, it was not students of political participation who responded.

At least one aspect of the expansion in research concerns may have increased the field's isolation from other fields in American politics. The longstanding focus of the judicial politics field on Supreme Court decisions and decision-making had the advantage that this was an area with which many scholars outside the field had some familiarity. The growing interest in courts below the Supreme Court has moved research to sectors of the judiciary with which other political scientists generally are less comfortable. For many students of American politics, for instance, the mysteries of the federal court system are exceeded only by the mysteries of the state court systems.

The resurgence of interest in policy outputs and normative issues does not distinguish judicial politics sharply from other fields of political science. After all, outputs are a focus of study in public policy analysis, probably the field of greatest growth since the late 1960s. The policy field also shares with judicial politics a concern with normative questions. And the political philosophy field, for which that concern is central, continues to thrive.

But this resurgence has represented a turning away from the subject matter and methods that are most compatible with other institutional fields of study in American politics. Systematic analysis of the decision-making process fits fairly well into the concerns of other fields. Debates over judicial capacity, relevant as they ought to be to students of government institutions, do not fit so easily into those concerns. Of course, this is the case even more with the study of constitutional law. Thus the field has in effect re-emphasized some traditional themes at the cost of reducing the potential for full integration into other institutional fields.

Why has judicial politics taken directions that move it away from scholars and scholarship in other fields of political science? Part of the answer lies in the third trend, the development of new links with other disciplines. That trend in itself represents an important reversal of the integration process. While it hardly is unusual for a political science field to build close ties with counterpart fields in other disciplines, it is striking that the judicial politics field did so at a time when its commentators were calling for integration into the discipline of political science.

The trend toward interdisciplinary cooperation also helps to explain the other trends mentioned above. For instance, interdisciplinary links brought new ideas into the judicial politics field. As I have noted, trial court research in political science owes a great deal to the groundwork of other disciplines. A similar debt exists for some new forms of normative and output-centered research, which have been spurred by the practical concerns of the legal community.

Other disciplines and interdisciplinary forums also have provided a receptive audience for newer forms of research. The role of interdisciplinary journals has been particularly important. In the 1970s and early 1980s political science journals reflected little of the expansion of the judicial politics field, concentrating instead on areas of traditional interest and those relatively close to the mainstream of American political research. But journals such as *Law & Society Review* provided a ready forum for other kinds of work.[15] This pattern of publication has had an interesting and rather important effect: political scientists are increasingly likely to be unaware of the state of the judicial politics field on the basis of their reading of the discipline's own journals.

Another part of the explanation for the field's unexpected directions lies in the continuing hold of long-standing traditions. The strength of new interdisciplinary ties in the last two decades reflects the habit of looking to relevant scholars in other disciplines as a reference group. If the membership of that reference group has changed a good deal, its importance has not. The resurgence of concern with normative issues may well reflect the early identification of the judicial politics field with such issues as they grew out of court decisions.[16] The maintenance of interest in constitutional law, in some ways the most surprising feature of the field today, can be understood in part as a product of its institutionalization in the field—not least of all in the teaching done by scholars in the field.

Tradition also affects the place of judicial politics in the discipline as a whole. Scholars in other fields exclude the courts from their concerns in part because their fields' interest long ago became defined in that way. Judicial politics is treated as a "special" field separate from the rest of American politics largely because it developed an image as special decades ago. Those ways of thinking will not break down easily, no matter how the field itself changes.[17] For that reason, if for no others, judicial politics may be fated to remain a distinctive field.

ASSESSING THE STATE OF KNOWLEDGE

Thus far I have left aside the task of examining what the judicial politics field has accomplished. In this section I offer a limited assessment of the field's progress. I do so with some hesitation. Assessment of judicial politics scholarship as a whole is particularly difficult because of its great diversity of subject matter and interests; the field's literature cannot be treated as a single integrated body of work, and any generalization about it demands numerous exceptions. Still, even a limited and tentative assessment may be useful as another way of charting the field's current position.

My concern will be the state of knowledge about judicial politics: how much we know and how well we know it.[18] The answers to these questions, of course, depend in part on what one thinks it is important to know. Theoretically-minded scholars and those with practical goals such as system reform might evaluate the field quite differently. My own assessment will focus on three types of knowledge: our ability to describe the processes of judicial politics; our accomplishments in developing explanations for those processes; and our contribution to a general understanding of political processes.

The line between description and explanation, of course, is a thin one—particularly as applied to works of scholarship. It is possible, however, to distinguish between our knowledge about "what happens" in the judicial process and our knowledge about the basis for what happens. That distinction is useful in assessing where we stand.

Description. The description of political processes is not the most glamorous task of scholarship. But its importance as a basis for explanation and evaluation, as well as for teaching, is obvious. In the judicial politics field the task of description has remained relevant; in some other fields the accurate description of basic processes was largely accomplished years ago, but that is not true of judicial politics. As of the late 1960s scholars knew little about a great deal that went on in the courts. The political science literature provided a clear picture of only a small segment of judicial politics, primarily processes involving the Supreme Court.

For this reason the continued vein of descriptive scholarship in the field seems not only justifiable but necessary. Certainly research over the past fifteen years by political scientists and other scholars has expanded fundamentally what we know about judicial processes. The most dramatic expansion has concerned criminal courts, for whose processes we have advanced from a position of substantial ignorance to one of extensive knowledge. Our knowledge about other subjects such as the selection of judges and civil litigation also has grown a good deal.

Yet even today our descriptive understanding of judicial politics is far from complete. Any scholar who teaches about the courts could produce a list of important subjects on which our ignorance remains considerable. There is hardly any subject that has not been studied at all, but frequently research is so thin that it would be risky to generalize from existing findings. For instance, although there has been significant research on the "Missouri plan" for judicial selection,[19] one still would need to be quite tentative in reporting to a state legislature on how the system works in practice.

The thinness in our descriptive knowledge may be largely a legacy of the past. The formalism and resistance to empirical analysis that typified study of the courts for a long time meant that there was a relatively limited base of descriptive scholarship in some areas. For instance, serious study of plea bargaining was delayed by the assumption that cases followed formal procedures and by an absence of appellate decisions which belied that assumption (see Mayer, 1967, p. 90).

The existence of artificial boundaries to the judicial politics field in the past also had a major effect in limiting descriptive research. By focusing on Supreme Court decisions and decision-making, political scientists ensured that they would build knowledge about only a small part of judicial politics. When the boundaries fell, there was a broad expanse of new territory to cover. It has helped that scholars in other disciplines have been engaged in substantial judicial research, especially on trial courts.[20] But it has not helped that political scientists who specialize in other fields tend to avoid the courts, leaving an unusually broad domain for judicial politics specialists to cover.[21] A decade ago, Martin Shapiro warned that with certain expansions of the field "we may have a package that is just too big to handle" (1972, p. 417). With the expansions that have occurred the package certainly has become difficult to handle, and one result is that the job of describing important judicial processes is far from completed.

Explanation of Judicial Processes. The breadth of the judicial politics field is reflected in its efforts at explanation as well as in its descriptive work. Judicial politics is not a field with a single dominant dependent variable. For the past two decades the explanation of judges' decisions has stood out as a major concern of the field, but significant attention has been given to a diverse group of other issues. Inevitably, progress has varied from area to area. To provide some sense of the general picture, it will be useful to examine two rather different examples of efforts at explanation.

The first falls in the area of judicial impact research (see Canon, 1982). In this area, the primary dependent variable has been the extent to which judges and administrators implement higher-court decisions faithfully. Early research was devoted chiefly to description of this variable, in part because of the interest in documenting the existence of non-compliance with Supreme Court rulings. Explanations for findings often were offered and sometimes were insightful, but research seldom was designed with explanation as a primary concern.

Although the volume of research on the implementation process has slowed, more recent research has given a higher priority to explanation. Empirical studies increasingly undertake systematic testing of hypotheses (Tarr, 1977; Bond & Johnson, 1982). Scholars also have offered some impressive theoretical formulations (Brown & Stover, 1977; Johnson, 1979). Fragments of an explanation for implementors' behavior have developed, emphasizing policy preferences and institutional interests as sources of responses to judicial policies.

But we still cannot make very confident judgments about the forces that shape the implementation process. There is no firm consensus on what these forces are, and there is little basis for conclusions about the relative importance of those that scholars have discussed. To take one example, the evidence on reversal as a sanction producing obedience by lower-court judges remains almost entirely colloquial.

This picture is fairly typical of most phenomena that scholars in the field have sought to explain. Efforts at explanation have become more concerted and more systematic, and progress has been made in explaining what happens. But explanations generally remain fragmentary and imprecise. We certainly can say something meaningful about the basis for voters' decisions in judicial elections or about the circumstances under which political litigation succeeds, but what we can say does not add up to anything like a well-founded and coherent explanation.

A few areas of judicial research stand out because they have produced more extensive and focused efforts at explanation. The research on the causes of plea bargaining is one example, and research on decisions to litigate is at least in the process of becoming another. In these areas, the work of political scientists has been pooled with that of other scholars. As noted already, the explanation of judicial decisions has received particularly long and concerted attention specifically from political scientists, so it provides a useful second example of efforts at explanation.[22]

The literature on decision-making has produced a large quantity of empirical findings on a broad range of factors that may influence decisions. These findings often are solidly based, in part because of the cumulative character of much of this research; unlike some other areas of judicial research, later studies frequently have challenged and improved upon their earlier counterparts. Decision-making research has provided a good deal of information about several aspects of the decision process: alignments among appellate judges, especially on the Supreme Court (see Rohde & Spaeth, 1976); group processes on federal appellate courts (see Brenner, 1980; Atkins, 1973); the relationship between sentencing decisions and an array of independent variables in several jurisdictions (see Eisenstein & Jacob, 1977); the role conceptions of judges in a variety of courts (see Howard, 1981); and the relationship between background characteristics and decisions, particularly in appellate courts (see Tate, 1981). Associated with these empirical inquiries has been a body of theoretical formulations, particularly on the relationship between judges' policy attitudes and their decisions (Schubert, 1974; Rohde & Spaeth, 1976). Through this research we have gained considerable insight into the forces that shape judicial decisions.

Yet this body of research has not given us a very precise sense of what influences decisions how much under what circumstances. On every significant issue our understanding remains quite tentative and uncertain. The most central question has been about the impact of judges' preferences concerning public policy on their decisional choices. Research taking a range of approaches has established that this impact is considerable. But it is not clear just how much judges' policy preferences explain, in what ways their impact is modified by other factors, and whether their influence varies among courts or types of cases. Our certainty is no greater on other issues in this area. Explanation of judicial decisions has proceeded much further than explanation of most other judicial phenomena, but there still is a long way to go.

The limited progress of the judicial politics field in explanation must be measured against a realistic standard. Conclusive explanations of political behavior have proved to be quite elusive under the most favorable circumstances. For instance, the study of voting behavior has benefited from having primarily a single and relatively simple behavior to explain, from the ready availability of very extensive data, and from the efforts of a fairly large group of scholars. Yet fundamental uncertainties remain (see Niemi & Weisberg, 1976). Undoubtedly, most of the gap between what we would like to know about explanation of judicial phenomena and what we actually know stems from universal problems in social research rather than from characteristics of a particular field.[23]

But some special characteristics of this field do retard the development of explanation. The breadth of the field means that in most areas relatively few scholars are doing research, and the continuing need for description draws the attention of some of those scholars. As a result, the efforts devoted to explanation of particular phenomena tend to be relatively limited. Even in the study of judicial decision-making, which benefits from the largest concentration of scholars in the field, the array of decision types and courts to be studied has stretched scholarly resources a bit thin.

These problems have secondary effects as well. The gaps in our descriptive knowledge of some processes—such as implementation of decisions—complicate the task of theory-building. Further, when efforts at explanation are few in number they are likely to be scattered rather than cumulative, and this has been the general pattern in most areas of judicial research. One result is that flaws in research may not be challenged and corrected.

Another major weakness of most efforts to explain judicial phenomena is the narrowness of their research focus in terms of both what is to be explained and the factors used to explain it. This weakness may result in part from the limited base of knowledge about many judicial phenomena, in that the uncertainties that exist about a phenomenon may seem to demand a narrow focus for explanatory work. But the base of knowledge on decision-making is relatively solid, and here too the focus tends to be narrow; most research deals with single determinants of decisions in isolation from others (but see Howard, 1981; Gibson, 1978a; Cook, 1977) and with particular types of decisions in particular courts rather than with multiple decisional contexts (but see Ulmer, 1972). This pattern is puzzling as well as frustrating.

Perhaps the goal of working toward general explanations has never taken hold in the field. The strong normative interests of the judicial politics field do reflect an alternative set of goals. The anti-legalistic theme that motivated early research in so many areas may have helped to establish a pattern of narrow inquiries for illustrative purposes. In any case, the limited willingness of judicial specialists to work explicitly toward general explanations for judicial phenomena has aggravated other problems in explaining those phenomena.

In examining the limitations of what judicial research has explained it is easy to lose sight of what has been accomplished. Research in the last fifteen years has added impressively to our ability to explain judicial processes just as it has added to our descriptions of those processes. But we still have a long way to go.

Understanding Political Processes. One goal of political science research is to understand political processes as unitary phenomena. We seek to comprehend such processes as decision-making, recruitment of public officials, and political participation in broad terms, with the institutions and settings in which they occur serving as sources of variation and not as boundaries on analysis (see Gibson, 1978a, pp. 1-5). One criterion with which to measure what we have learned about politics is its contribution to that goal.

Research on judicial politics makes a particular contribution as a body of work. Because of special characteristics of the courts, many processes take unusual forms in the judicial branch. For this reason one gains a much fuller sense of the variation in processes and the sources of variation by taking the courts into account. The burgeoning of judicial research in recent years has provided a wealth of new raw materials with which to build a general understanding of politics.

This contribution of judicial research is enhanced when the processes that it studies are linked explicitly with their counterparts outside the courts. Studies of judges' role orientations do more to build general knowledge when they draw from research on other policy-makers' roles and put their findings in the context of what we know about legislators and administrators. Not so incidentally, establishing that kind of linkage strengthens judicial research in itself. Thus it is a positive sign that many students of the courts have taken this approach, and unfortunate that a great deal of judicial research continues to isolate itself from the non-judicial literature.[24]

Of course, the most direct means to build a general understanding of political processes is through research that compares processes in different institutional contexts. Where such research might compare the courts with other branches, however, it is discouraged by the gulf that separates the study of the courts from the rest of the discipline. Scholars who specialize in other fields might be expected to do little comparative research involving the courts, and indeed that has been the case (but see McMurray & Parsons, 1965).

Thus, the burden for such research falls primarily on students of judicial politics. Adding such a burden to the already long agenda of the field may seem unreasonable, particularly since scholars in the field have shown little taste for cross-institutional research even within the judiciary. It is notable, for instance, how little research compares federal and state courts (but see Canon, 1977; Grunbaum & Wenner, 1980; Haas, 1982). But, because the interest of judicial scholars in the rest of the political system tends to be stronger than the interest of other political scientists in the courts, most comparative research will have to come from that direction.

A small body of work by specialists in judicial politics that compares courts with other institutions does exist. Some of this work reports comparative empirical research, while some unifies what is known from the empirical studies of individual institutions.

A large share of this work concerns the process of making policy. Two studies have followed individuals from the legislature to the judicial branch and compared the factors that influence their behavior in the two contexts: David Danelski's study of Harold Burton in Congress and the Supreme Court (1970); and James Gibson's survey of California judges who had served as legislators (1978a). In more general examinations of the policy-making process Martin Shapiro sought to identify similarities and differences between courts and administrative agencies (1965; 1968, ch. 1), and J. Woodford

Howard compared legislative and judicial behavior in several aspects (1969). Particularly striking in Shapiro's analysis was his demonstration of a close connection between the courts' adherence to precedent and incrementalism as a general style of decision-making (see also Braybrooke & Lindblom, 1963, pp. 106-107).

Much of the research on the role of the courts as policy-makers has had a comparative element, particularly when the interaction of courts with other branches is part of the concern (Dahl, 1958; Casper, 1976; Henschen, 1983). A few studies have been particularly direct and explicit in comparing courts' roles with those of other institutions. Two empirical studies have focused on specific issues: the agendas of state senates and supreme courts (Rainey, 1975). Another study synthesized past research to compare the ability of legis-Health, Education, and Welfare in obtaining school desegregation (Giles, 1975). Another study synthesized past reseach to compare the ability of legislators and appellate courts to control the implementation of their policies (Baum, 1982). Thomas Dienes compared legislative and judicial roles more broadly in the context of a study of birth control policy (1972).

The recent interest in the capacities of courts as policy-makers already has led to some comparative examinations of capacity. In his book on judicial capacities Donald Horowitz made a brief comparative inquiry into the capacities of courts and other institutions (1977, pp. 293-298). Some commentators on Horowitz also have taken a comparative approach. In one noteworthy study Cheryl Reedy systematically compared Supreme Court and Congressional capacities in abortion policy, concluding that the Court did not display a more limited capacity to perform competently than did Congress (1982).

This body of cross-institutional research is valuable for the data and insights that it provides, but it does not add up to more than a beginning. As yet, specialists in judicial politics have shown rather limited interest in this kind of comparison. If students of the courts can increase their collective commitment to comparison, they will make a major contribution to our understanding of political processes. They also may reduce the gap between the field of judicial politics and other fields that study aspects of American politics.

CONCLUSIONS

In examining the judicial politics field I have emphasized its distinctiveness. As I have seen it, attitudes toward the courts separated judicial politics from other fields in American politics and took it in its own directions. In turn, the distinctive character that the field developed has shaped its recent history and its progress in description and explanation.

Some scholars who viewed the field two decades ago had a simple recommendation: integrate judicial politics more fully into the discipline. That recommendation has been followed to a degree, particularly in the methods of empirical research used in the field. But the integration has been incomplete and in some respects non-existent. In part this has been the result of the continuing hold of long-standing traditions in the field. But even if these traditions were overcome the integration would remain incomplete so long as the established attitudes of other political scientists toward the courts continued to exist.

It is easy to assume that the field's distinctive traits are unfortunate, particularly in light of its slow progress in amassing knowledge about the courts. But only a portion of that slowness can be explained by characteristics of the field, and some of those characteristics have to do with the size of the task it has undertaken rather than with its ways of seeking knowledge. Moreover, whatever its failings may be, the field has contributed heavily to an extraordinary growth in our understanding of judicial processes over the past fifteen years.

Furthermore, much of what makes the judicial politics field distinctive can be viewed positively rather than negatively. The development of strong interdisciplinary ties has strengthened the field's ability to build an understanding of the courts. The focus on outputs has enhanced the relevance of the field's scholarship for policy-makers. Thus, the defensiveness that may be detected in some of the field's self-analyses seems misplaced; different is not necessarily worse.

It is customary in this kind of essay to make recommendations for future work in the field, even if the impact of such recommendations is likely to be nil. My preferences for the field probably are clear by now, but I will recapitulate them briefly. Specialists in judicial politics, I think, do not need to be self-conscious about the field's place in the discipline—in part because that is something largely beyond the field's control. Researchers do, however, need to be more self-conscious about their work and how it is building toward a general understanding of courts and of politics. Given our limited scholarly resources, we ought to use them as effectively as possible.

But this does not mean that the domain of the judicial politics field should be narrowed, even though our resources have been spread thin. Ultimately we need to understand the courts in all their aspects, and a diverse field is more likely to advance toward that objective than one that is artificially narrow in its subject matter and perspectives. Moreover, the very diversity of the field helps to keep it interesting and attractive to scholars. Pritchett's advice about proliferating flowers remains appropriate, so long as we think about what kind of arrangement they make.

NOTES

1. Many specialists in judicial politics would include one or more of these concerns within the field as they define it.
2. In the foreign and comparative literature perhaps the outstanding contribution has been made by Martin Shapiro (1981). Other important works by political scientists include Ehrmann's analysis of legal cultures (1976) and the text on comparative constitutional law by Murphy and Tanenhaus (1977). Scholars in other disciplines have made a more extensive body of contributions to this work; a sampling may be found in the *Law & Society Review* over the years. The failure of the comparative literature to grow more rapidly reflects the limited scope of both the judicial politics and comparative politics fields in political science.

3. Past surveys of the field that were especially helpful include Vines (1970), Shapiro (1972), Grossman and Tanenhaus (1969), Pritchett (1968), Schubert (1972), and Murphy and Tanenhaus (1972). A new set of broad and narrow surveys appeared in 1982 through the organizing efforts of Harry Stumpf for the meetings of the Western Political Science Association and by Beverly Cook for the meetings of the American Political Science Association. See Stumpf *et al.* (1983), Provine (1982), Jacob (1982), Carter (1982), and Canon (1982).
4. I used a twenty-year period to provide a baseline for the identification of changes in the past fifteen years. The political science journals that I analyzed were *APSR, AJPS, JOP, WPQ, APQ,* and *Polity.* Of these, I gave primary attention to the first four. The law and social science journals were *Law & Society Review, Law & Policy Quarterly,* and *Judicature.* For these journals I restricted my survey to articles written by political scientists. In my analysis I borrowed ideas and some data from a survey by Thomas Hensley (1981). Missing from this survey were the "conventional" law journals, which continue to publish work by political scientists. Their exclusion probably produced an underestimate of the field's current interest in issues related to constitutional law, although those issues were found to maintain a considerable presence in the political science journals.
5. Research on the selection of judges, another long-standing interest, has taken a somewhat different and rather interesting path. It long has been understood that the selection process is "political," so in that respect there were no legalistic assumptions to challenge. But the legal community has developed a conventional wisdom that one mode of judicial selection, based on commission screening of potential nominees, elevates merit over politics. Some research since the late 1960s has aimed at testing or questioning this wisdom (Watson & Downing, 1969; Slotnick, 1982). Other political scientists have sought to demonstrate the virtues of the elective method that the legal community distrusts (Dubois, 1980). So long as the current debate over selection methods continues, it will provide a focus for research on selection processes, and something of an anti-legalistic theme probably will continue.
6. By political litigation I mean litigation brought and fought largely to further group purposes or to achieve policy goals rather than to win individual victories.
7. One good example is the work by political scientists on the "merit selection" of judges, which has questioned the view of the organized bar as to the desirability of that system. See note 5 above.
8. It does appear that political scientists are more likely to put court processes into broader political contexts by such means as linking trial court activity to local political cultures (Ryan, 1980-81; Levin, 1977). Within areas of interdisciplinary research there also are some specific foci whose study is dominated by political scientists, such as research on the relationship between plea bargaining and sentencing, and these may reflect differences in interests and perspectives that I have not identified.
9. The term is from Verba and Nie (1972), which is a striking example of a broad political participation study that does not deal with litigation.
10. It may be that the attitudes of political scientists toward the courts are reflected in their attitudes toward the field that studies them. The respondents to the survey by Somit and Tanenhaus in the early 1960s gave very low rankings to "public law" for the significance of its work (1964, p. 56). The survey was replicated in the mid-1970s; in that survey political scientists responded to all the changes that had occurred over the previous decade by dropping public law into last place among seven fields (Roettger, 1978, p. 22). These results may be interpreted in a variety of ways. But they probably stem in part from scholars' reaction to a field with which they are largely unfamiliar because of its alien subject matter.

11. Becker was referring to "the law," but I think that his observation applies to the courts as legal institutions.
12. Yet during the 1920s and 1930s there were several students of the courts, political scientists among them, whose interests and methods were remarkably "modern" (see Vines, 1970, pp. 127-129). That their work was isolated rather than a model for the field as a whole may indicate the power of the prevailing perspectives on the courts at that time.
13. The question as to why this process occurred is an interesting one. As I suggest later in this section, the field's interdisciplinary ties helped to expand its concerns. Also important was the popularity of systems analysis, which demanded a broad view of the judicial process and which called attention to areas in which little research had been done (see Goldman and Jahnige, 1971).
14. This dichotomization of scholars becomes problematical when applied to younger scholars, who may have begun to study judicial processes such as plea bargaining on the basis of broader conceptual interests but who nonetheless became identified as judicial specialists as a result. Yet it is significant that this labelling occurs, because it reflects the relegation of everything involving the courts to a single field.
15. These generalizations are based on the analysis of journals that is discussed in note 4. In political science journals from 1970 through 1981, a majority of articles dealt primarily with the Supreme Court, while fewer than 20 percent dealt with trial courts. The most popular subjects were judicial decision-making and judicial impact on other policy-makers—two topics relatively close to the mainstream—and the traditional subject of constitutional law. Meanwhile, political scientists' work in the interdisciplinary journals reflected the newer interests rather clearly, especially the interest in trial courts.

    It may be that the lack of change in the political science journals reflects the preferences of editors less than it does authors' assumptions about those preferences. The editors of the *American Journal of Political Science* provided me with the titles of judicial politics manuscripts submitted in 1979-81, and their subject matter differed little from that of articles published by *AJPS*. Moreover, most reviewers of judicial politics manuscripts are themselves specialists in the field. Thus there is little basis for concluding that the limited coverage of new areas in the discipline's journals reflects only the unwillingness of other political scientists to accept work in those areas. But I do suspect that it would have been difficult to obtain publication of articles in those areas in large numbers without the development of a new set of journals.
16. It also may be that it is difficult to study the judiciary meaningfully without attention to normative issues because such issues arise so clearly and explicitly in the work of the courts.
17. It is worth noting that in 1982 one questionnaire of the American Political Science Association to its members continued to list "American government and politics" and "public law" as separate fields, and the APSA adopted the same format for its placement newsletter in 1982. In both instances only public administration and (if defined as part of American politics) public policy received the same treatment. Not only the separate listing of the field but the continued use of a title that emphasizes the field's distinctiveness and that has lost popularity within the field (see Shapiro, 1972) is striking.
18. These questions exclude from a consideration an assessment of scholarship that is primarily normative in its goals. This exclusion seemed necessary to allow a more focused discussion, and it does not reflect a judgment that normative work is unimportant.
19. This research includes an extensive case study in Missouri (Watson and Downing,

1969) and multi-state studies of commission members (Ashman & Alfini, 1974) and retention elections (Jenkins, 1977; Carbon, 1980).

20. Other disciplines would have been even more helpful if resistance to empirical analysis had not survived in much of the work of legal scholars. For instance, articles on selection of judges are a staple of law reviews, but relatively few of these articles inquire systematically into the actual selection process.
21. The breadth of this domain merits some emphasis. Since all processes involving the courts basically are left to specialists in judicial politics, those specialists must cover more than counterpart groups in other institutional fields. For instance, the implementation and impact of statutes is not defined as part of the legislative politics field, but the implementation and impact of court decisions by default became part of the judicial politics field.
22. My thinking about this question has benefited a great deal from the analysis of the judicial behavior literature by James Gibson (1983).
23. One other comparison may be useful. The scholarly resources devoted to the explanation of organizational behavior have been enormous, but a recent critique of theory development and testing in that area concluded that "the field is not very far along" (Mohr, 1982, p. 1).
24. My survey of judicial politics articles by political scientists in the *American Political Science Review* and *Law & Society Review* over the last decade indicated that citations of non-judicial political science articles were relatively sparse, even ranking behind citations of judicial literature from other disciplines. This pattern probably is not unusual for a field, but it symbolizes a somewhat limited use of what has been learned in other fields. Judicial impact research, for instance, has drawn surprisingly little from the literature on policy implementation.

    A significant portion of judicial research, however, has utilized theoretical frameworks that also are employed to study non-judicial processes. Most prominent are organization theory, used most often in criminal court research (Nardulli, 1979), and role theory, used chiefly in the study of judicial decision-making (Howard, 1981). Use of these frameworks sometimes represents an explicit effort to link judicial and non-judicial research, and it facilitates integration of findings about different institutions.

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8

Public Policy Analysis: Some Recent Developments and Current Problems

Susan B. Hansen

As I began work on this paper in early 1982, I received a fund-raising letter from Walter F. Mondale's Committee for the Future of America. On page 1, the former vice-president stated,

> In our struggles to develop programs to give [our] principles life, we made some errors, went up some blind alleys. These missteps gave our cynical opponents the opportunity to challenge our purpose. But making mistakes in the public eye, for all the world to see, and then learning from these mistakes is what government and politics is all about.

One may question the wisdom of stating such a theory of politics in a request for funds; presumably his party's mistakes are to be preferred to those of his opponents. Nevertheless, these comments made me wonder: have we, as a discipline, learned from our mistakes? If so, are there areas of research to which we can point as evidence of the maturing of policy analysis as a field of study? If not, is there any hope that such promise will emerge in the near future, given our current interests and investments, the state of our theories, and our methodological tools?

Let me begin with a bit of history and some quantitative indicators concerning the growth of policy analysis (as I shall define it) within the discipline of political science. I will then consider three of the more promising theoretical trends in the discipline: the "political economy" approach, formal organizational theory, and new ways of measuring and conceptualizing policy failure. I will turn next to a discussion of some research strategies which appear to have engaged the attention of political scientists doing policy analysis. These include attempts to focus on "tractable" problems, analysis of im-

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plementation of evaluations, studies of policy termination, and comparative, cross-systemic research on post-industrial societies. I conclude with a brief consideration of the changing role of political scientists as policy analysts and policy makers within the political process, and of what we might learn about that role from the economics profession.

Let me make clear at the outset what I mean by policy analysis. As Salisbury (1968) has so well said, there is little in the discipline of political science which does not touch on policy because of our historical concern with public life, constitutional arrangements for decision-making, and the authoritative allocation of values. In those senses, policy analysis is centuries old. But as I shall use the term, policy analysis refers to an explicit, focused, systematic analysis of the outputs of governments and their effects on society. "Process" studies of Congressional committees, public opinion, party competition, and so forth, are not considered policy analysis unless the linkage to political outputs is made explicit. My emphasis, then, will be on efforts to measure and evaluate policy outputs, to compare policies with reference to structure and impact, and to look at the connections (direct, indirect, or reciprocal) between political processes and policy outputs.

The field of policy analysis is likely to be viewed quite differently according to one's perspective. Barnes and Dubnick (1980) list the uses of policy analysis as viewed by scientists searching for truth, professionals seeking to design better policies, political advocates seeking to justify their positions, administrators trying to implement policies effectively and efficiently, and citizens using all of these approaches to buttress arguments for their own policy preferences (p. 123). One could of course suggest other perspectives—those of politicians, consulting firms, business, the economics or legal professions, universities, or the students we teach in policy courses. This assessment will be based in part on my experiences over the past year as Chair of the policy analysis section for the 1982 APSA meetings in Denver; this experience gave me both the opportunity and the responsibility to think about what was going on in the field. My concern in this essay will be with the contribution of policy analysis to the discipline of political science and to the development of theory.

THE GROWTH OF POLICY ANALYSIS, 1967-1982

First, where have we been? Let us go back 15 years,[1] to the 1967 APSA annual meeting in Chicago. At that meeting, there was one panel on public policy, listed under the section on "American Politics, Local and National." The featured paper was by Alan Altschuler of MIT entitled "The Politics of Managing a Full Employment Economy". A perusal of the program yielded three other papers with some policy content or perspective. One was a roundtable, "Does the Hatch Act Need Revision?" Two were presented at a panel entitled "Research and Teaching on Policy Issues"; Charles O. Jones of the University of Arizona discussed "The Policy Making Approach: An Essay on

Teaching American Politics," and Michael D. Reagan of the University of California at Riverside discussed "Policy Issues: The Interaction of Substance and Process." Several papers were given in international relations and legal processes which touched on policy, but these emphasized process or normative concerns rather than policy analysis as I have defined it.

A glance at annual meeting programs over the subsequent 15 years shows that policy analysis has become an increasingly important segment of political science. 1970 was the first year in which policy was given a separate panel; 33 policy papers appeared at that meeting. In subsequent years the categories varied. At some meetings policy analysis was listed with panels on public administration or on state, local, or national government. In other years, policy was considered from the perspectives of social indicators, the impact of science on society, or specific problems such as Vietnam or the anti-ballistic missile.

But throughout this time period, the number of panels and papers grew, both in absolute terms and as a proportion of all conference papers. The magnitude of that growth may be indicated by the number of papers given in 1981 and 1982. I arrived at the number simply by counting those papers that had "policy" or the name of a policy (regulation, OEO, health care, etc.) in their titles, or those that indicated in their abstracts that they offered a systematic analysis of government outputs in a substantive policy area. I also included discussions of the theory or methodology of such studies. I excluded papers on judicial process or foreign policy which appeared to stress issues of law, process, or the internal workings of bureaucracy, but included those oriented toward policy outputs and effects. These distinctions are of course highly arbitrary, but since they have been applied consistently in 1967, 1970, and 1981-82, they afford us a basis of comparison that I trust will not tread too much upon the turf of other subfields.

In 1981, I counted 86 papers on policy out of a total of 432, or 20 percent. Seventeen of these appeared to be comparative cross-nationally. A total of 29 panels were oriented toward policy (substance, theory, or methodology). In 1982, 140 papers on policy analysis were given at the Denver meeting, at 36 official panels. This constituted 31 percent of the total of 459 papers. I should also note the growth of interest in comparative (cross-national) public policy: a total of 38 papers at the Denver meeting, or 27 percent of the total number of policy papers. The first section devoted entirely to comparative public policy appeared in 1979, with six panels, although in earlier years one or more individual panels was devoted to comparative policy analysis, in addition to diverse individual papers given under other panel headings. And I have not even mentioned the many policy-related papers given under the rubric of "Courtesy Listing of Unaffiliated Groups" such as the Conference Group on Political Economy, the Caucus for a New Political Science, the Disaster Policy Group, the Women's Caucus, etc.

Policy analysis has grown by a number of other indicators as well, as a glance at figures supplied by the Policy Studies Organization on membership, journals, publications, and conference papers will show.[2] In quantitative terms, policy analysis has had a major impact on the discipline, at least as indicated by papers given at annual meetings. Let this be a warning to anyone so bold as to assume the responsibility for setting up policy panels in upcoming years: I had nearly one hundred proposals to evaluate, and suspect the number will increase in the future. I wholeheartedly agree with Aaron Wildavsky (1979) that it is "impossible to keep up."

The substance of the papers given at the 1982 APSA meetings may provide some idea of the current interests of persons involved in policy analysis. By my count, about half of the policy papers were concerned primarily with substantive issues: welfare, school desegregation, tax policy, natural and man-made disasters, environmental policy, budgeting, and international agricultural policy. Several in the policy analysis section focused on new approaches, such as policy design, policy failure and deimplementation, risk and uncertainty, and productivity and public policy. A few were concerned with measurement and the methodology for assessing and comparing policies. One panel on the "limits of the economic paradigm" was explicitly concerned with metatheoretical issues, although many of the substantive and methodological papers also raised questions about the models and assumptions we use.

Both qualitatively and quantitatively the changes in the relationship between political analysis and the discipline of political science since 1967 are considerable. Whether these changes offer evidence of disciplinary progress or maturity is an issue to which I shall return.

THREE PROMISING THEORETICAL TRENDS

"Political Economy": A Macrotheoretical Approach

Let me now turn to a survey of three emerging trends in policy analysis which show promise of making major theoretical contributions to the field of political science. Fifteen years ago, political scientists interested in policy were likely to focus on substantive issues, often using case studies of policy design, execution, and effects. But the ideas of David Easton (1965) and two empirical studies published in 1966 dramatically changed the field. Each posed broad questions about the relationships between the economic development or capacity of a political system and its policy outputs. The first of these was Thomas R. Dye's *Politics, Economics, and the Public,* easily the most widely cited and the most frequently criticized work in the growing field of policy. The second was Gabriel Almond and G. Bingham Powell's *Comparative Politics: A Developmental Approach.* The conclusions of these books differed considerably, as did their methods, data bases, and time periods. Dye's cross-sectional analysis of the American states found little relationship between political factors and policy outputs once economic and demographic factors were considered. But Almond and Powell's historical and cross-national analysis suggested a strong relationship between political development and political capacity in several areas (extraction, regulation, distribution); in their analysis political and economic development were clearly linked.

Each of these books raised questions about the relationships among policies, and between politics and economics, that have engaged the attention of considerable numbers of researchers since. What was clear from these two seminal works, and the discussions they provoked, was that David Easton's model of the political system had empirical validity; political processes and outputs depended on their environment, international as well as national, as was shown by the increasing interest in the domestic constraints on foreign policy. But if both politics and economics were important for policy, what exactly was the magnitude of the relationship and the direction of causality? Given the current state of each academic discipline, neither economics nor political science has the tools, theoretical or methodological, to disentangle these complex and reciprocal relationships.

Roger Benjamin (1982) stated the problem well:

> It is time to reconsider basic relationships between politics and economics in the context of the breakdown of the distinction between domestic and comparative/international politics. This breakdown is especially important with respect to relationships between post-industrial and industrial states. . . . It is necessary to attack first-order questions such as how one should *conceptualize* the boundaries between economics and politics. (p. 75) (Emphasis added.)

Since the 1960s, there have been several attempts at such new conceptualization under the rubric of political economy. This approach to policy analysis has attracted an increasing amount of attention, as indicated by frequency of use of this term in books, journal articles, and conference papers. A large number of panels, many well-attended and including persons closely identified with the Policy Studies Organization, have been presented at APSA annual meetings since 1977 under the auspices of the Conference Group on Political Economy in Advanced Industrial Societies, chaired by Stephen Elkin.

But what is political economy? Unfortunately for our students—and people who attempt to write papers on the "state of the subfield"—the term has almost as many definitions as practitioners. Basically, however, all approaches examine the interrelationships between the political and economic systems. One orientation—widely used by Marxist scholars but by no means limited to them—considers the impact of economic factors on political power, structure, and processes. Studies of dependency theory, multinational corporations, corporatism, political development, and the distribution of wealth, all build on this orientation.

In the American and Western European contexts, another focus of political economy has been on the positive role of the nation-state. Under pluralism, the state is usually seen as a neutral umpire that enforces the rules of the game; it is in itself simply the vector of organized interests in the society. A political economy approach would focus more on what Easton termed "withinputs": policy preferences and political entrepreneurship by persons within government, seeking to maintain or increase their own power and to define social needs to suit their own interests. Examples include: Frolich, Oppenheimer, and Young's (1971) theory of political leadership; Hugh Heclo's (1979) account of issue networks in the executive establishment; Niskanen (1971) on bureaucracy; and Morris Fiorina's *Congress and the Washington Establishment* (1977). All view the state as a self-perpetuating system having an independent impact on society and the economy (see also discussion by Lindblom, 1982).

A major issue for policy practitioners as well as scholars concerns the ability of governments to control the economy. Do politicians in democracies manipulate macroeconomic policy in order to further their own goals, such as reelection (Tufte, 1978; Hibbs, 1977)? Or are secular trends in productivity, population, and resource shortages so compelling that politicians can make only minor adjustments in economic policy (Benjamin, 1980)? Or is the economy so complex that efforts to manage it—whether for political purposes such as reelection, or economic goals such as growth or price stability—are likely to fail and may even make things worse (Shultz & Dam, 1977; Buchanan & Wagner, 1977)? Or can governments bring about economic growth and increased revenues by doing less: cutting taxes, reducing deficits and spending, and returning to the gold standard, as proponents of supply-side economics claim? (See Fink, 1982, for an excellent collection of essays debating supply-side economics.) One can find a considerable literature backing each point of view; aspects of the debate hinge on the data bases, methodologies, and causal models used (see Beck, 1982, and Kramer, 1983, for recent summaries, and Barry, forthcoming, for a critique).

Although macroeconomic change remains problematic, political scientists have long been interested in the capacity of parties and politicians to make marginal policy changes to benefit their supporters or particular regions or interest groups. Morehouse (1981) finds that state political factors such as party coherence and powers of the governor are linked to a range of policies benefiting the "have-nots." Welch and Karnig (1981) show that black representation has affected the level and distribution of city services. Boneparth (1982) documents areas in which women's political activity has led to policies benefiting women. Rundquist's *Political Benefits* (1978) gives some theoretical coherence to such research and includes numerous examples. The differential regional impact of Reagan's economic policies offers convincing evidence of the potential for politicians to affect the distribution of economic costs and benefits, but their failure to date to reduce unemployment suggests the limitations of political influence on the economy (Palmer & Sawhill, 1982).

Clever research design can go a long way toward delineating more precisely the boundaries of political and economic choice. In their study of state planning agency response to federal LEAA grants, Gray and Williams (1980) essentially hold state socioeconomic characteristics constant since their policy was totally funded by the federal government. They thus focus on the political and organizational dimensions of state compliance with LEAA goals. Winters and Reidenberg (1983) investigate the presence of an electoral budgetary cycle by contrasting changes in spending in constituency-oriented agencies as opposed to agencies supplying more public and indivisible goods. The former exhibit far larger election-year increases. Clearly, however, further research is needed to specify more precisely the macro- and microeconomic dimensions of political decision-making.

The whole political-economy orientation can be criticized as simply a new intellectual fad. It has been accused of being cynical rather than normative, of stressing strategy over advocacy, and of becoming so technical and abstract in its methodology that any policy payoffs are increasingly remote. My purpose here is neither to resolve such arguments nor to attempt further review of this voluminous literature. I would simply suggest that the intellectual controversy generated has been good for policy analysis (and political science) in a variety of ways. One is the increasing willingness to challenge economics and the economic paradigm, whose rational models have too often been based on highly simplified assumptions which ignore the operations of political institutions. From the standpoint of "covering law" principles, rational models are not only invalid as scientific theories (Moe, 1979, p. 240), but can do serious damage when applied to policy. Few political scientists today could read Joseph Pechman's 1975 article on economic policy-making in the *Handbook of Political Science* without some sense of embarrassment—which he may well share—over his optimism about the possibilities of economic management. Not only have political scientists begun to pay more attention to economics, but those in the field of institutional economics have even ventured to learn something about political science (Schotter, 1980; Samuels, 1979). Our historical strength in the analysis of political institutions and processes may become increasingly relevant to the debate over the future of the economics of advanced industrial societies as well as the Third World.

Another implication of the interest in political economy is a growing recognition of the interdependence of policies. Changes in one area, based on the best available knowledge and noblest of intentions, may cause serious dislocations elsewhere (for many good examples see Haveman & Margolis, 1982). As Wildavsky, Heclo, Jones (1975), and many others have argued, policies create their own problems, and much of our collective energy must be devoted to putting out fires caused by previous policy choices.

Recognition of policy interdependence has had two consequences. On the one hand, we see discussion of government "overload" (Rose & Peters, 1978), of the adverse consequences of trying to do too much with limited resources. On the other hand, we see increasingly sophisticated attempts to model such interdependence. One example is Roger Benjamin's (1982) thoughtful essay on the product cycle and the need for new theories applicable to political and economic changes which have occurred in post-industrial societies. Another is Mancur Olson's analysis of the impact of interest group power on government growth and economic decline (forthcoming). A third is Sabatier and Mazmanian's (1980) outline of a theory of implementation which considers the secondary and tertiary effects of policies in political as well as economic terms. Existing theory and methodology may indeed be insufficient to deal with the sorts of policy issues we now face, but the gap between performance and expectations has spurred the development of new approaches in politics as well as academia.

Formal Theories of Organization

A second potential area for theoretical advance is that of formal models of the operation of complex organizations. The term political economy has been applied to these as well, but in these studies the level of analysis shifts to discrete organizations rather than political systems. Theories of the behavior of bureaucracies or Congress are based on assumptions about the goals and resources of the actors involved. Studies of the political economy of regulation, for example, consider regulative policy in terms of interactions among government commissions, regulated industries, and politicians or interest groups which endeavor to represent the public interest. The policy outcome is a function of the goals and strategies of each sector as well as economic constraints such as the costs of organization, information, and factor scarcity (Mitnick, 1980). Other prominent examples include Niskanen (1971) on bureaucracy and Anthony Downs (1957) and his critics and elaborators on economic theories of democracy.

Over the past decade, a vast new literature has developed examining the consequences of systems of social choice for policy outcomes. Riker (1980) argues that "prudence in research directs the science of politics toward the investigation of empirical regularities in institutions" (p. 432). The emphasis has shifted considerably from the methodological individualism of an earlier era of behavioralism, when social outcomes were explained on the basis of choices, tastes and values of individual voters or legislators. Plott (1976) cogently sums up the results of two decades of debate: the concept of social choice involves an "illegitimate transfer of the properties of an individual to the properties of a collection of individuals" (p. 525).

Instead, in organizational theory, the emphasis is on the consequences of specific structures and decision rules (majority rule, decentralization, interorganizational communication) on policy outputs. Tastes and preferences are still important, but they are analyzed as a consequence as well as a cause of particular political structures. If preferences and outcomes diverge, of course, a system or subsystem is likely to be unstable and changes in either preferences or institutions may be attempted to bring it back into equilibrium. As Shepsle (1979) has shown, in an example particularly relevant to the U.S. Congress, a stable set of decision-making patterns can evolve from routines based on committee decentralization and rules concerning jurisdiction and amendments, even if individual motives and preferences conflict.

This is not the place to attempt a summary of this literature, nor am I qualified to do so (for an excellent overview which integrates older and newer theories of organizations see Scott, 1981). Over the past decade, however, formal organizational theory has informed many empirically-oriented policy issues. Hanf and Scharpf (1978) discuss "limits to coordination and central control" with a useful theoretical introduction and examples of intergovernmental policy-making in the U.S. and Europe. Gray and Williams (1980) use state implementation of LEAA grants to test propositions concerning the conditions of influence of federal agencies over state agencies. Ostrom *et al.* (1978) and Parks and Ostrom (1981) have considered the administrative effectiveness of alternative institutional arrangements. Pommerehne and Schneider (1983) have explored the consequences of various voting rules and institutions for representation of voters' preferences. Hansen (1983) has explored some of the structural and institutional reasons why tax policy outcomes diverge so far from taxpayers' preferences, using examples drawn from the recent "tax revolt" as well as from historical federal and state tax innovations.

Many of these studies could also be described under more traditional categories of public administration, implementation, and representation. I mention them here in the context of formal organizational theory because they all illustrate some of the best features of such theory: core assumptions are stated precisely, alternative predictions are subject to rigorous empirical testing, and cumulative modifications are made in the underlying theories. The seminal works of Anthony Downs (1957) and William Niskanen (1971) have contributed greatly to this field. Even though many of the original predictions derived from their work were later challenged, modifications and extensions of their original theories on voting, budget size, and bureaucratic behavior have led to theoretical advances.

As Ashford (1983) notes, "because the ideas of complexity and interdependence figure so heavily in systemic-level theories of the modern state, the insights provided by organizational theory are especially valuable" (p. 26) in providing operational, intermediate-level generalizations more easily subject to empirical validation than either macro- or micro-theories. Comparative policy studies, whether cross-national or across states and cities, have too often neglected policy implementation and the study of bureaucracy. Such an emphasis is crucial particularly in an era when many policies are enacted nationally but implemented locally, and when they involve interaction among many actors and organizations (Gray & Williams, 1980, p. 3). Jones' *Clean Air* (1975) provides excellent illustrations of these points although it would not be classified as formal organizational theory.

Changing Conceptions of Policy Failure

A third area of major theoretical interest involves new ways of conceptualizing and measuring policy failure. This concept has undergone considerable redefinition over the past 15 years. Ranney in his seminal 1968 article suggested that policy analysts attempt comparisons of "good" and "bad" policies—such as the Marshall Plan and the Bay of Pigs invasion—to help develop criteria for evaluating policy processes and contents. This proved to be a difficult task. Wildavsky notes on page one of *Speaking Truth to Power* the "difficulty of finding programs that work well." All of us had our intuitive notions—often based on prejudice or ideology—of program successes and failures; some, like the Bay of Pigs, were self-evident. When Alice Rivlin asked in 1971, 'How do we know what works?" the answer she implied was that we did not. Many programs, particularly Great Society social programs, were never subject to systematic evaluation according to the canons of social science. If evaluations were done, they were often *post hoc* and lacking in experimental controls. Without some baseline measurement, some agreement on what was being measured, and planning for evaluation as part of program design, indications of progress or failure could be based only on political expedience and partisan considerations.

The problem is threefold.[3] The first is that of data and measurement. At one time we may have been optimistic about the prospects for social or economic engineering, but we have by now discovered that the evidence on large and complicated issues of public policy is frequently ambiguous or missing altogether. In the 1960s these data problems seemed capable of amelioration if not of solution. Requirements and funding for evaluation studies appeared more frequently in legislation; the EPA's mandates for environmental impact statements constitute one example. Organized groups such as the Social Science Research Council urged the development of social indicators, and publication of the bicentennial *Historical Statistics of the United States* by the Census Bureau spurred the search for historical measures of social change to use as benchmarks for program evaluation. Large-scale experiments were conducted, most notably those on guaranteed incomes (Palmer & Pechman, 1978).

But masses of expensive data, though necessary, were not sufficient to solve issues of policy evaluation. Scientists need to worry about operationalization and the underlying theoretical assumptions of our design and measurement techniques. Such subtleties are not always easy to communicate to practitioners. Miller (1981) discussed the poor fit between our mathematical models and our standards of policy judgment. Measures of efficiency (particularly in economics and management science) have been developed to a far higher degree than measures of equity or responsiveness. Her work developed alternative mathematical formulations for use in evaluating educational reform. In this same vein, Smeeding (1982) and Paglin (1980) have considered different ways of measuring poverty based on alternative interpretations of in-kind transfers. Researchers at the University of Wisconsin's Institute for Research on Poverty have suggested a variety of ways to interpret statistics on trends in income equality in the U.S. (for summary and review of the literature see Taussig & Danziger, 1976). Taussig and Danziger (1976) argue that the omission of all forms of non-money income from the census and other time series "invalidates all empirical statements about trends in inequality since World War II" (p. 21).

The second problem is that of values. Standards for judging policy failure have changed considerably over the past 15 years. In the 1960s, during a period of economic prosperity and governmental growth, standards tended to be based on "demand-side" ideals of policy equity and of broad-based participation in the process of decision-making. In the 1970s, emphasis shifted to "supply-side" elements of control, efficiency, and effectiveness within budgetary constraints. In the earlier period, market failure was a sufficient reason for policy intervention. By the 1980s, judgments of the failures of many public spending and regulatory programs led to demands for a return to market methods of allocation based on competition and price mechanisms. Wildavsky (1979) suggests that we may have been "doing better and feeling worse" with respect to many policy areas; "we need to ask if present standards for judging policies are appropriate" (p. 5). If public policies are to be judged against our own overblown expectations or the promises of politicians, they may well fall short.

The private sector has been rediscovered as an alternative solution to various social and technological problems and as a benchmark against which to measure public programs. In the 1960s, since few measures of productivity were available for the public sector, productivity was not considered as a part of either policy design or implementation. Since then, however, efforts have been made to develop such measures (Ostrom, 1975; Bahl & Burkhead, 1977; Nagel, 1983; see also the new journal *Public Productivity Review*). Although ideology still weighs heavily, assessment of policy alternatives in at least some areas (notably urban politics) has become an empirical question rather than an ideological one. In her 1982 *APSA* paper, Hughes compared proprietary and non-profit provision of nursing care and found advantages and disadvantages for each. The analytical task is to formulate problems so that implicit values can be turned into explicit criteria for gauging the likely effects of alternative policies (May, 1981).

Politics is not the only alternative to a free market; a broad range of government and independent corporations and regulatory agencies must be considered. Before 1970, these received little attention outside of the literature on pressure-group politics and regulatory agency "capture." A review essay (Thomas, 1976) noted only a handful of studies by political scientists on the politics of economic regulation. Sharkansky (1979) describes what he terms an intellectual crisis in political science because of "the profession's myopic preoccupation with formal institutions of the state" and its behavioral and individualistic perspective. But this may be changing, as evidenced by his own work on public/private institutional hybrids, Lindblom's *Politics and Markets* (1977), and the rapidly growing regulation literature by political scientists as well as economists (see Mead, 1977; Mitnick, 1980; and Stone, 1982, for excellent summaries of political and economic approaches to regulation).

A revival of interest in market solutions also poses problems for evaluating policy failure. First of all, many social scientists lack much experience with businesses, and may confuse the rational, goal-directed, hierarchical textbook corporation with the real thing. In fact, large corporations face the same problems of information and control as do bureaucracies in the public or non-profit sectors. Second, given international economic trends, businesses can no longer predict markets or factor prices and thus they cannot operate efficiently (Muller, 1980). As Michael Reagan argues, private-sector or free-market solutions to the energy crisis pose more serious costs in both the long and short run than do government regulations, even though the latter are far from perfect; several externalities (particularly the unequal distribution of rising energy costs) have no apparent market solution. Imperfect markets dependent upon OPEC are not necessarily preferable to imperfect political regulation.

Third, despite the many problems that political scientists and economists have found with regulation, deregulation is not necessarily the solution. It is already proving disastrous for the airlines (Thayer, 1977) and adverse impacts on trucking, occupational hazards, and banking are to be anticipated. The problem is one of better management and information; whether public, private or mixed approaches are preferable must be decided for each industry under specific market situations. And we know little as yet about the deregulation process (for preliminary assessments see Mitnick, 1980; and Brewer & DeLeon, 1983).

Despite the progress made in social indicator measurement, statistical techniques, and mathematical modeling, evaluations remain difficult to evaluate because of our many and conflicting notions as to what failure is (Ingram & Mann, 1980). If a program has a negative cost-benefit ratio, or if a policy's stated objectives are not met, judgments of failure may be fairly straightforward. But what if the program's objectives were ambiguous or unattainable in the first place? Is the program we are evaluating better or worse than doing nothing, better or worse than relying on the private sector, better or worse than other programmatic alternatives? Systematic evaluation alone can seldom answer such questions, particularly in response to short-term policy issues such as when to cut losses and terminate a program whose outlook for success is ambiguous.

The evidence needed to support decisions is all too frequently amassed after the fact. Neither public nor private econometric models of the U.S. economy included estimates of supply-side pricing and incentive effects until after Reagan's election and the implementation of his economic policy (Roberts, 1982). Political power conveys not only the authority to make decisions, but the ability to influence the gathering of data and the funding of research on which future decisions will be made. As social scientists, we deceive ourselves if we think we can conduct value-free evaluation research. Research trends over the past ten years suggest that the deployment of knowledge remains highly partisan. The gap may be widening between policy analysts who are expected to evaluate policies and social scientists conscious of the ambiguities of determining policy failure. But as measures and models improve and as evaluative criteria are made more explicit, autopsies of political failure should contribute to better theories of the boundaries of politics.

Certainly a new humility, an awareness of the limitations of policy, has affected many of us. I note recent titles such as *The Futility of Family Policy* (Steiner, 1981), *The Ungovernable City* (Yates, 1977), *The Collapse of Welfare Reform* (Leman, 1981), "The Impotence of Sex Policy" (Wasby, 1980), and *City Limits* (Peterson, 1981). This is a far cry from the positivist orientations of the 1960s. The political implications of this "strategic retreat from objectives" are obvious. Less obvious may be the impact on our students. At the end of my most recent graduate seminar on public policy at the University of Pittsburgh one student told me that, from her perspective, the course had been a detailed study of Murphy's Law and that she was too discouraged to continue the study of policy further.

Several interim strategies are possible. One strategy—to be discussed below—is to define more carefully the scope of problems to which policy analysis is applicable: to find more "tractable" problems to keep policy analysts busy and politicians happy. Another is to take advantage of the research opportunity to study policy termination and the organizational responses to failure, as Ingram and Mann (1980, p. 16) suggest; given current trends, this could keep political scientists busy for quite some time.

The failures of policy are not necessarily those of policy analysis, but both reflect to some degree our limited knowledge. They can lead us to formulate our problems in different ways, redefine our theories, and improve our methodologies. In Karl Popper's words, "it is through the falsification of our suppositions that we actually get in touch with reality. It is the discovery and elimination of our errors which alone constitute that 'positive' experience which we gain from reality" (cited in Wildavsky, 1979, p. 59).

THE PRESENT: RESEARCH STRATEGIES

The policy payoff, if any, from such emerging theoretical developments may not be apparent for some time. This time lag may lengthen further if available funds continue to flow to contract research on short-range solutions to small-scale problems. In the interim, however, policy analysts within political science have turned to other strategies which appear to offer more immediate gains in terms of their applicability to pressing social issues. These include: (1) efforts to focus on "tractable" problems which can be solved, thus conserving both our own and the society's resources; (2) emphasis on implementation and knowledge utilization; (3) studies of policy termination; and (4) a growing interest in comparative policy analysis. Each of these strategies has been pursued for good reasons; each offers at least some potential for improving policy outputs and policy analysis. However, each also has serious pitfalls for theoretical development.

First of all, what is a "tractable" problem, and how do we distinguish one from others which may be less tractable? To some extent this judgment must be *post hoc* rather than predictive. If certain policies worked well in the past, whatever mix of processes and resources used then should work again on similar problems, *ceteris paribus*. But this poses obvious difficulties if we find few successful paradigms to follow or if conditions have changed. Nevertheless, attempts have been made. One is Richard Nelson's insightful and widely cited essay *The Moon and the Ghetto* (1977). In this work Nelson moves far beyond the obvious differences between messy "social" problems (race, education, welfare) and structured technological issues such as air pollution or landing a man on the moon. A problem, he notes, is more manageable if we can define it precisely, can agree on its causes, and if its causes are factors reasonably under our control within the constraints posed by time and money. Thus some social issues (e.g. implementation of the 1965 Voting Rights Act) have proved manageable, while some technological problems (e.g. nuclear and hazardous waste disposal) are currently beyond our grasp. In the same vein Sabatier and Mazmanian (1980, p. 542) suggest an index of problem "tractability" based on: (a) the availability of valid technical theory, (b) the diversity of target group population, (c) the target group size, and (d) the extent of behavioral change required.

But problem tractability may be more an indicator of the state of our theories and methods than a factor inherent in problems themselves. Thus a few brave researchers have attempted to discover structures in messy problems. Simon (1974) considered the "structure of ill-structured problems." Rockman (1981) was able to model the behavior of "regulars" and "irregulars" in the State Department and the National Security Council, and to use this model to evaluate proposals for improving American foreign policy. Another promising effort is Cohen, March, and Olsen's (1974) "Garbage Can Model of Organizational Choice" in which they attempt to structure the conditions of choice faced by organized anarchies. An organized anarchy—their example is a university—is characterized by inconsistent preferences, choices looking for problems, solutions looking for issues to which they might be an answer, and decision-makers looking for work. From such unpromising assumptions they proceeded to model the behavior of large and small universities under different economic conditions using a matrix of choice situations solved with the help of 324 simultaneous equations. Many of us may be encouraged and intrigued by their results if we are not discouraged by the mathematics. If the behavior of universities and the State Department can be modelled successfully, perhaps other political problems are not altogether beyond hope (see also Sproull *et al.*, 1977).

As Wildavsky (1979) notes, "the age of decision is over; the era of implementation is passing; the time to modify objectives has come" (p. 43). There is both temptation and opportunity in concentrating on manageable problems rather than raising expectations about problems that no one yet knows how to solve. But if we focus on the most tractable and well-defined problems—assuming we can find some—it is easy to lose sight of the complex relationships among issues. As MacRae (1976) has suggested, one argument for policy analysis is the need to surmount disciplinary boundaries, to deal with problems that have no fixed constituency, or that are caused in part by an entrenched or rigid one (regulation frustrated by the operation of iron triangles, for example (p. 290). Well-developed issue networks (Heclo, 1979) may engage in turf battles in their efforts to find problems to which their particular expertise is a solution. Problems are defined politically so as to aid agency and budgetary growth, not necessarily to generate solutions (as Nelson, 1978, found for child abuse).

Even worse, we may end up concentrating scarce resources on less pressing issues. Cohen and Lee (1979) have devised an index of natural and man-made hazards, based on the degree of risk (damages, lives lost) and their probability of occurrence. Their purpose in doing so was to recommend a focusing of societal resources on the greater hazards. But Johnson (1981) found that, for the period from 1957 to 1978, Congressional effort (bills introduced and laws passed) was greater for "old" hazards with which Congress was familiar and had dealt successfully in the past, than for newer issues, however urgent. One suspects the same relationships would hold for appropriations as well. It is understandable that a society would concentrate its efforts on manageable problems that we know how to handle at fairly low cost; Congressional desires for "credit claiming" are only one reason for such behavior. But the long-run implications are rather chilling, since several large potential hazards (nuclear reactor accidents, dam failure, hazardous wastes) have only very recently attracted much attention. The fastest growing domestic policy area, health care, appears to be beyond anyone's control despite the many efforts to limit health costs now being discussed (Marmor & Christianson, 1982).

In short, a focus on "tractable" problems assumes that we know more than we do. Ingram and Mann (1980) suggest no simple answers: "there are few hard and fast rules about what makes for successful implementation, and what works in one setting often fails in another" (p. 25). Without clear standards and mechanisms for ensuring compliance, success or failure may be impossible to assess, as O'Brien (1980) found in his study of the mutually contradictory Freedom of Information and Privacy Acts. And, as Pressman and Wildavsky (1973) noted in their Oakland study, the process of implementing policy is so difficult that we should be favorably surprised when policies have any favorable accomplishments. Even minor improvements in efficiency in some areas of management may end up saving millions because of the absolute size of government expenditures. But Quade (1972) would exclude such operations-research problems from the purview of policy analysis altogether and confine that term to the more difficult—and interesting—issues of tradeoffs among programs, resource allocation, program evaluation, and budgeting (p. 16).

One might, in fact, question whether tractability is a viable structuring principle or simply a rationale for reducing the scope of government, or cutting particular programs. As Ingram and Mann note (1980), "politicians and political systems are expected to solve problems, not ignore them. Policy failure . . . may be evidence of a political system that responds to problems, even if knowledge may be limited and appropriate policy tools unavailable" (p. 3). Retrospective voting for Reagan over Carter in 1980 suggests that in that election popular preferences leaned toward efficiency rather than effort or equity as values by which to judge candidates, but that balance could shift in 1984, as it has in the past.

When governments are faced with budgetary cutbacks, are the less "tractable" programs the first ones to go? Not necessarily. A RAND corporation study (Lipson & Lavin, 1980) of cutbacks in California cities after passage of Proposition 13 found that traditional services, those with the most entrenched bureaucracies, and those which could be financed by user fees instead of taxes, were least likely to be cut. Clark and Ferguson (forthcoming) note considerable differences among Republican, Democratic, and populist city governments in their responses to fiscal stress. Again, politics rather than program "tractability" appeared to be the major factor. Given these patterns, it remains to be seen whether policy analysts' judgments of "tractability" will have much programmatic effect.

The second research strategy I detect is a focus on knowledge utilization. This approach basically seeks answers to the question that frustrated policy analysts have too often had to ask: "Why is our good advice so seldom followed?" Earlier in the 1970s the problem of implementation received considerable scholarly attention, particularly after Pressman and Wildavsky's (1973) study of how grand designs in Washington could be frustrated in the hinterlands (in their case, Oakland, CA). Around that same time, evaluation studies became big business, both for governments and for universities and contract-research facilities. Budgetary constraints in recent years have been a strong impetus for cost-benefit analyses to find the programs most efficient per dollar spent. The problem, however, is to combine the studies of implementation and evaluation—to put knowledge into practice.

The focus on implementation has put the skills of political scientists to good use, since their graduate training emphasizes the knowledge of government processes and organization which the scientists, lawyers, engineers, or economists who design policy often lack. The institutional decision-making approach continues to be well represented in both political science and policy analysis (Rourke, 1976; Ripley & Franklin, 1980; Weiss & Barton, 1979; Derthick, 1980). The focus is on organizational processes, jurisdictional rivalries, cognitive limits, and bounded rationality. "Muddling through" remains a viable organizing principle for policy-makers as well as analysts, although organizational theorists, as discussed above, have attempted to model these processes using a rational choice perspective on complex organizations. One could also mention here recent emphasis on the quantitative study of agency decisions, represented by the work of Beck (1982) and Moe (1982).

During the 1970s social scientists learned more about how to do evaluation; texts appeared; courses were taught; experiments designed. The need for evaluation had been recognized, and the "how-to's" provided in terms of data bases, computer facilities, and increasing methodological sophistication. The U.S. government financed large-scale experiments, spending over $10 million to evaluate the effects of various income maintenance alternatives on work and leisure.[4] Many fine studies were done, often with counterintuitive results; the growth of evaluation has certainly enriched the social sciences in terms of knowledge, theory-building, and methodology—to say nothing of the personal economic benefits accruing to researchers. But somehow this growth in evaluation was not able to help us tell success from failure, or to discover "tractable" problems.

Studies of implementation tell us that, first of all, organizations actively distrust evaluation efforts that pose threats to their personnel, budgets, or missions; they have devised numerous strategies to prevent or bias evaluations efforts (McLaughlin, 1975). There is a growing suspicion that the more sophisticated our tools for analysis and evaluation, the less likely we will be to find successful programs. Persons generally favoring social change have long suspected that, in Salamon's (1976) words, policy evaluations are "handmaidens of conservative powers that be" because they neglect the time bias against potentially promising programs that pose an immediate threat to the status quo (p. 282; see also Berk & Rossi, 1976). Social-service agencies perceive short-term evaluation as a threat which diverts scarce resources from clients' needs (Marcus, 1981). Finding that bilingual education is not helpful to minorities, or that fiscal equalization of school-district funding will not solve school problems (Bish, 1976), is unlikely to be greeted with much enthusiasm by the proponents of these programs. Conservatives also distrust evaluations that may work against them; as Conway (1982) notes, the Reagan administration has decided to eliminate a number of statistical series altogether, thus depriving potential critics of ammunition. The Congressional Budget Office and its director, Alice Rivlin, have come under attack by Congressional Republicans for providing budget analyses considerably less optimistic than those of the Reagan administration.

Second, evaluations (positive or negative) can have little effect if they are not communicated to persons in a position to act on them. Donna Shalala, formerly Assistant Secretary for Research at HUD, noted in a Spring 1980 address at the University of Michigan that the Department had commissioned hundreds of studies of housing and urban policy, but had never devised ways to communicate these results either within the organization or to outside scholars. The sheer mass of available information may preclude its use or analysis. Thousands of studies are indexed every year by the Congressional Information Service and the National Technical Information Service, but they are seldom subject to peer review and the data on which they are based may not be available to scholars for reanalysis. There is little evidence that they are used by policy makers.

The best policy analysis information can be lost or defeated by organizations, and social scientists have shown increasing interest in such organizational processes (Brewer, 1973; Bulmer, 1982; Sproull & Larkey, 1983; Feldman & March, 1981; Rich, 1981; Weiss, 1977; Meltsner, 1976; and papers presented at the 1982 Denver meeting by Ellwood, MacRae, Kelley, and Dunn). Wildavsky noted the dilemma of the tradeoffs in organizations between error recognition and error correction. Small errors are easy to correct, but may be difficult to detect until they have caused large-scale problems; major failures are highly visible, but by the time they are discerned they may be difficult to remedy (1979, p. 16 ff.). It might be more useful if evaluations could pick up small problems early, but it is also more difficult.

Third, "knowing what works" implies communicating results to decisionmakers in a fashion that would give them leverage for dealing with social problems. MacRae stressed the importance of "actionable alternatives," a distinction between things we can and cannot control (1976, p. 284). A sophisticated, multi-dimensional evaluation might well find that the problem at hand is due to forces beyond the control of the organizations being evaluated. For example, the cause of many urban problems lies in Washington, state capitals, or corporate board rooms where local governments have little leverage (Peterson, 1981). The tools that educators can manipulate—classroom size, teacher salaries, new curriculae—are not those that affect the learning rates of culturally and economically deprived children (Comfort, 1980). Organizations can redefine their goals or clientele in efforts to justify their continued existence, but the basic problems may survive untouched if the solutions are beyond their control, are rooted in other political cultures, or would involve coercive methods that cannot be tolerated in a democratic society.

Fourth, evaluations would have more impact if they considered long- as well as short-term effects, indirect, secondary or tertiary implications of programs, the impact of changing circumstances, tie-ins with other policies, and changing expectations. Given the political obstacles to doing even limited evaluations of single programs, this is a tall order. John Grumm (1975) described the methodological difficulty well as that of one independent variable (the program) and a very large, perhaps infinite, number of dependent variables, or outcomes; it is hard to know when to stop. Evaluations usually require results in a finite time period with limited funding, but politicians, and voters, have to make decisions before all the results are in. Evaluations, however sophisticated, may become increasingly irrelevant to policy unless solutions can be found to the problems political science and public administration have raised about implementing them (for some suggestions, see Nagel, 1980, *Improving Policy Analysis;* and Wildavsky, 1979, on the "self-evaluating organization").

A major challenge, however, is to include implementation strategy as part of policy design and evaluation—the "missing link" as described by Hargrove (1975). Even if legislative efforts are made to clarify policy objectives, provide financial resources, and include incentives for compliance and oversight provisions, no statutory design can determine "the number of veto-clearance points, the formal access of various actors to the implementation process, [or] the probable policy predispositions of implementation officials" (Sabatier & Mazmanian, 1980, p. 540). A "fixer" or "political entrepreneur" who is committed to a program and has the time and other resources to devote to it, may be far more instrumental to its success than either formal evaluation or statutory effort at legislation and oversight (Lewis, 1980). Successful implementation can too easily become a matter of luck or personality—elements hard to control and of little use in theory or prediction.

Interest in implementation and evaluation may have peaked just as the focus of most governments switched to de-implementation and policy termination. According to ACIR (1982), local government spending reached a maximum in 1974, state spending in 1976, and federal domestic spending in 1978 (all figures in constant dollars). In the era of implementation, analysts had the opportunity to examine what bureaucracies did when they were faced with unobtainable goals: redefine those goals, find new clients, use old clients for new purposes, etc. As Comfort (1980) notes of educational bureaucracies, "since the original objectives set for the Elementary and Secondary Education Act were probably unobtainable, implementers had no alternative but to redefine objectives to meet what could be achieved" (1980, p. 19). But in the new era, hundreds of programs have had their budgets and personnel reduced drastically or have been eliminated altogether. Cameron's (1978) discussion of the deinstitutionalization of California state hospitals found that "potential consequences were not considered until they were experienced" (p. 322). He described a massive policy shift based less on evaluation results than on changes in the prevailing myths or ideologies concerning mental health, on the allure of budget cuts, and on political considerations such as greater job opportunities for professionals in community mental health. No systematic appraisal of the costs and benefits of alternative policies took place, and release of long-term patients was not so much "implemented" as simply allowed to proceed without planning for alternatives. Cameron foresees "disastrous" results if his example suggests a general model for policy termination, but Brewer and DeLeon (1983) describe some successful terminations in their new text. More models may be expected with more research experience.

A decade or so ago the "agenda-building" perspective attracted scholarly attention to the processes by which social problems move from the private to the public sectors, and externalities produced by collective goods came to be defined as government responsibilities. We as yet know little about the reverse process of privatization. Deimplementation—and its cousins deregulation and deinstitutionalization—may well become a major new focus for policy analysis; this trend is indicated by several recent articles and a number of papers at the 1982 APSA meeting on cutbacks resulting from Reagan administration policies.

Another research strategy could be to examine whether certain characteristics of agencies (organization, political support) help predict their ability to survive in the current era. One might anticipate that the first programs to be eliminated would be those with explicit redistributive aims that were the most difficult to enact in the first place. The Reagan administration has been far harder on means-tested social programs (Food Stamps, disability, etc.) than on other non-means-tested programs with primarily middle-class constituencies such as Social Security and veterans benefits (Palmer & Sawhill, 1982). After a generation of government growth, many programs clearly need to be terminated; no country could afford all the defense or social programs which public bureaucracies and private interest groups have supported. But as DeLeon notes (1978, p. 293), termination strategies which are not carefully formulated and implemented can undermine their purpose and create more problems than they resolve. Deregulation, for example, is no panacea. Breyer (1982) argues that regulatory failure may result, not simply from problems of implementation, but from poor choice of regulatory tools. He identifies six specific types of market failure and the appropriate range of regulatory approaches for each type.

I will briefly mention one last example of interim research strategies: comparative policy analysis (for a fuller assessment, see Donald Hancock's essay on comparative policy elsewhere in this volume; also Ashford, 1983). Comparative policy research is hardly new; researchers in state and local governments have been doing it for years. But cross-national comparisons usually offer a greater range of policy variation, and political scientists are more sensitive to the importance of political system variation than, say, economists. Wildavsky may be right that some of our problems have no solutions, but other political systems may do better than the U.S. at posing and implementing alternatives and at policy succession. On the other hand, if post-industrial societies are facing similar difficulties (rising welfare costs, declining productivity), this may suggest something about the range of alternatives open to politicians and perhaps lead us to either lowered expectations or searches for common solutions (Benjamin, 1980). For example, U.S. manufacturing industries have advocated trade restrictions and tariff barriers to solve their problems, but if other producers follow suit, the outcomes could be harmful to all (Walters, 1982).

In terms of theory-building, comparison can put our judgments about policy processes and outcomes in better perspective. In terms of concrete assistance to policy makers, however, comparative research on policy may not be of much help. Neither policies nor institutions may be directly comparable. There seem to be few powerful conceptual tools that cross cultural boundaries. One exception is Caiden and Wildavsky's *Planning and Budgeting in Poor Countries* (1974). Uncertainty and resource scarcity produce similar budget strategies in a variety of political settings. Conversely, very different institutional arrangements may produce similar policy outcomes; differences may appear in the consumption or the legitimization of policies rather than in their production (see Lundqvist, 1980, on air pollution; Kelman, 1981, on regulation; or Leman, 1980, on welfare).

The most serious problem, however, is that the major variables accounting for different policy outcomes may be due to historical or constitutional factors not readily manipulable by policy makers. The U.S. is unlikely to import responsible party government or corporatist systems of government/business economic planning on the Japanese or Italian models (Vogel, 1979; Holland, 1972; Schmitter & Lehmbruch, 1979). Yet recognition of such limitations on the range of choices may be of some use in directing policy efforts to problems whose amelioration may depend on policy levers that we can employ.

CONCLUSION

One issue that engaged the attention of political scientists early in the development of policy analysis continues to spark intellectual controversy. That is the issue of the ability of political scientists to make recommendations or take policy stands based on their disciplinary qualifications rather than as individuals or interested citizens. Ranney (1968) and Lowi (1979) suggest that political scientists do have special skills and perspectives to offer based on our traditional interests in public law, political theory, and the operation of institutions. But the problem is twofold. First, such knowledge does not constitute a tight analytic system on the model of economics; our theories are not developed to the stage where they permit predictions or concrete advice to policy makers. Second—and probably a result of the first—political scientists are seen as lacking legitimacy, as not having any special knowledge that is not shared by reasonably well-informed citizens, politicians, or bureaucrats. Both Ranney and Lowi caution against trying to "beat the subject-matter experts at their own game by becoming especially skilled hydrologists or welfare economists or astronautical engineers or whatever" (Ranney, 1968, p. 18), lest we become laughingstocks to those who are truly knowledgeable.

The controversy still rages, but behavior has changed, despite such warnings. Increasing numbers of political scientists are becoming policy experts. Joint degrees in political science and other areas are not uncommon; graduate programs in public policy often require or recommend extensive course work in subject areas. Even academic job descriptions now request political scientists with substantive knowledge in particular policy areas, and some departments may become as specialized in policy fields as large economics departments have been. One cannot write on subjects such as air pollution, earthquakes, nuclear waste disposal, urban land values, or taxation without acquiring considerable practical knowledge in these areas and often the academic or field-work credentials to substantiate that knowledge.

Perhaps because of this trend, political scientists appear to be more willing to offer specific policy recommendations concerning all phases of the policy-making process, following the recommendations of Easton (1969) for action and relevance. It is not that their credentials in specific policy fields give them the standing to compete with other experts—although some in our field may have become sufficiently knowledgeable to do just that. Rather, substantive knowledge of a policy area is required in order to gain access to decision-makers, attentive publics, or the research funds to enable one to do policy research at all. Once having gained both expertise and access, policy analysts can and have made recommendations in terms of political processes and social values as well as technological alternatives concerning specific policies and programs. Examples include Henry Kissinger and Jeane Kirkpatrick on foreign policy, Aaron Wildavsky (1980) on the balanced-budget amendment, Alexander George (1972) on multiple advocacy in foreign-policy decision-making, Allen Schick (1981) on the budgetary process, and Ted Lowi himself on the writing of laws for Congress.

Is this advice likely to be any good, or have any effect? That question cannot be answered *a priori,* but must be based on the actual experiences of political scientists and policy analysts in the field. Ballard, Brosz, and Parker (1980) suggest that social researchers can play a variety of roles (substantive expert, information processor, disciplinary scholar, change agent) depending on their own proclivities and the situations in which they find themselves. They may be effective in some roles more than others, in some situations more than others. Some research has already been done on the use, and misuse, of social science research and the recommendations of policy analysts; the evidence to date is mixed (see Weiss, 1977, for some positive examples; Szanton (1981), Brewer (1973), and Aaron (1978) for more negative views). But I find little analysis of the substance or effects of policy advice tendered by political scientists *qua* political scientists.

One symposium issue of the *Policy Studies Review* dealt with anthropologists in public policy. I was struck by a thoughtful article by Michael Agar (1981), "Strangers in a Strange Land: Anthropologists in Agency Settings." He gives a discouraging account of his own experiences of "culture shock" and the differences between agency and academic perspectives on policy research. But he nevertheless recommends that such involvement continue and that graduate training be changed to change the field itself. He concludes that "to move into non-academic settings will stimulate and enrich anthropology from basic premises to specific techniques." Will this be true for political scientists as policy analysts? I suggest that we do not yet know the answer to that question, but that it would be well worth finding out. One alternative, as suggested by Erwin C. Hargrove (*PS,* Spring 1982) is the development of closer professional ties with our students and colleagues who have entered non-academic employment. A symposium in the *Policy Studies Journal* or *Review* on the experiences of political scientists as policy experts or advocates might consider the short- as well as the long-term implications of their experiences for policy outcomes and for the discipline.

But we must be prepared to make mistakes, to have our cherished assumptions and theories questioned, to have our expert advice ignored, and even—like Dennis Palumbo—to be sued. We can also expect critiques such as Michael Nelson's (1979). He argues, using a graphic illustration based on CETA evaluations, that academic policy analysis, whether based on methodological erudition or advice given in terms policy-makers can understand and accept, is of little help in dealing with policy problems. Yet the outcomes of greater political involvement need not be entirely discouraging. Knorr (1980) finds evidence of mutual accommodation between policy analysts and policy-makers, rather than sycophancy by social scientists and domination—or rejection of experts' recommendations altogether—by political elites. The important point, according to Jones (1976), is that policy analysis is only one input, only one aspect of the political process. He strongly urges that that voice be heard, quoting John Gaus (1976): "for any achievement of gains, however slight, toward a reasonable solution of the problems of government contributes to a renewal of confidence in reason itself and a strengthening of nerve" (p. 286).

Many have argued against the involvement by political scientists, as political scientists, in applied policy analysis because we have not developed a workable model of the world, an all-encompassing theory that would permit prediction of political outcomes. As should be clear from this survey of the state of the field, the critics are probably right. We cannot tell success from failure in any scientific sense, nor can we tell policy-makers how to make such determinations. Our *post hoc* evaluations seldom have the theoretical base to permit predictions. The world is a far more complex place than we had thought; traditional and disciplinary distinctions between American and cross-national, domestic and international politics, cannot deal with policy decisions in an interdependent world. The political economy and organizational approaches suggest a movement toward theoretical integration, but we are only at the beginning, and the policy payoffs of such integrative theories are as yet far off.

In the interim, policy analysts in our profession have turned to a variety of short-term research strategies. Each of those I have summarized here has some potential for contributing to knowledge—and keeping policy analysts employed—but all pose problems for policy as well as theory development (Portis & Davis, 1982). The choice between training substantive experts and experts on processes and procedures is real. Perhaps the only area in which political science has a firm toehold on substance may be foreign policy. In other respects we are at a great disadvantage compared with economics. In addition, it may be much harder to understand institutional and decision-making processes than it is to understand policy outputs. Designing a proper decision system is like constitution making; one needs to make it effective for the moment but also flexible enough to accommodate new developments, including new information and values. Political scientists are much better as social critics than as developers of normative or explanatory policy theory.

As Charles Lindblom has said, however, "policy aims at suppressing vice even if virtue cannot be attained." Despite the variety of approaches and differences in methodological sophistication, policy analysts in political science are almost unanimous in their agreement on one principle: the inadequacy of economic theory and methods. Policy analysis has too long been regarded, by too many in this profession, as an instrumental orientation with emphasis on efficiency in an economic sense. The tools of economic analysis are powerful and can be extremely helpful for certain classes of problems; most public policy texts draw heavily on economic theory and methods. But how empirically useful are models whose behavioral assumptions are increasingly open to question and that pay little attention to real-world political institutions? Economics has little to say about the legitimacy or administrative effectiveness of alternative organizational structures (Mead, 1977), yet these are inextricably linked to goal attainment, as implementation analysis demonstrates. Our unstated behavioralistic or ideological assumptions may be just as unrealistic as those explicitly formulated in political economy or formal organizational theory (Boynton, 1982). But political scientists can at least pose questions about administrative effectiveness or organizational structures even if we cannot claim to have all the answers.

An irony of the present condition is that the dominance of economics in policy analysis and in policy-making in Washington coincides with considerable self-searching going on in that profession about failures to deal with macroeconomic policy, corporate behavior, consumer choice, energy, and other pressing social issues (Bell & Kristol, 1981; see also Eyestone, 1978, for a perceptive review of recent books by economists critical of their own field). A major contribution that political scientists could make to the policy process might be to communicate to decision-makers what is wrong with the economic approach, aided by our growing policy expertise, our understanding, however imperfect, of political institutions, and the profession's collective memory of government decisions that represented good economics but bad public policy. Most political scientists can easily think of examples (see Nagel, 1980, for several good ones), but because of our disciplinary self-doubts we have been unable or unwilling to share these with a wider audience.[5]

I must conclude by again stressing my debt to Karl Popper—and Walter Mondale. We may not know what works, but we are beginning to get some notions as to what does *not* work in the real world of politics. Perhaps it is time to convey some of this hard-earned humility to economists and decision-makers.

NOTES

1. This starting point is not intended to imply that political scientists' interest in policy analysis dates only from 1967. See Ranney (1968) for a capsule history of policy perspectives in the discipline.
2. According to the *Personnel Directory* of the Policy Studies Association, 68% of its 1100 members were political scientists (1979 data).
3. Special thanks to Bert Rockman for ideas for this section.
4. The income-tax experiments raise fascinating questions about the design of large-scale public policy experiments, their results, and their political implications. See Rossi and Lyall (1976) for a trenchant critique. As Steiner (1981, pp. 106-110) describes, the impact of tax payments on work effort was minimal, but one wholly anticipated result was increasing family instability. So many divorces and family breakdowns occurred among subsidized families—although the precise causal link was never specified—that it became politically impossible to implement the income maintenance experiment on a larger scale.
5. It is interesting to compare economists' and political scientists' writings on their duties as policy advisers or policymakers. I have been unable to discover any economist since Edwin Nourse, the first chairman of the Council of Economic Advisers, who questioned the right, duty, and obligation of their profession to participate in the making of public policy. As Pechman (1975) notes, economists argue among themselves in strongly partisan terms and have difficulties communicating their models to policymakers.

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9

Federalism: The Challenge of Conflicting Theories and Contemporary Practice

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FEDERALISM: ITS THEORETICAL STATUS*

Federalism represents America's greatest contribution to the science of government. It is, as Sheldon Wolin (1964, p. vii) observes, "an innovation in Western political theory and practice" that has been widely copied. Indeed, over the past four decades something of a "federalist revolution" has swept the globe, embracing a substantial portion of the world's population under systems based, at least in part, on this key invention of the Founders (Elazar, 1981, p. 5).

In its native territory, however, the subject has come on hard times. Troubled by ambiguity and inconsistency, as well as by an inability to marshall evidence to support key assertions, the theory of federalism has fallen into disrepair. Intergovernmental relations, its principal heir, has not yet proven to be an adequate substitute. That, at least, is the conclusion of many political scientists in the field. Hence, the clarification or reformulation of federal theory appears to be an urgent task.

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*The views presented here are those of the authors and should not be attributed to the members or staff of the Advisory Commission on Intergovernmental Relations. However, some of the content is drawn from Advisory Commission on Intergovernmental Relations, *The Condition of Contemporary Federalism: Conflicting Theories and Collapsing Constraints* (Washington, D.C.: U.S. Government Printing Office, 1981), Chapter 1.

The Traditional View

Perhaps because the concept of federalism was so influential and so deeply rooted in the structure of American government, the fundamentals of federal theory long remained remarkably constant. Federalism, as traditionally understood, meant "dual" federalism: a system for dividing functions between the state and national governments that left each considerable autonomy within its own areas of jurisdiction. This was the concept implicit in the Constitution as elaborated in the *Federalist* (14, pp. 82, 83). It later found clear judicial sanction, as when the Supreme Court (*Abelman v. Booth,* 1859) declared:

> The powers of the general government, and of the state, although both exist and are exercised within the same territorial limits, are yet separate and independent of each other, within their respective spheres.

Although dual federalism lost its constitutional status during the New Deal era (Corwin, 1950), the underlying philosophical concept remained. In the 1950s, Wheare (1951) still defined federalism as "the method of dividing powers so that the general and regional governments are each, within a sphere, coordinate and independent" (p. 11). Two decades later, Martin Diamond (1976) continued to maintain that:

> . . . the American system is federal to the extent that governing functions are kept out of the center and remain constitutionally with the states, just as they would in a traditional federation. This division of functions between nation and states was always understood, at least in principle, as having been settled by the Constitution. To quote *Federalist* 39, . . . the jurisdiction of the proposed central government "extends to certain enumerated objects only, and leaves to the several States a residual and inviolable sovereignty over all other objects." (p. 189)

Theory in Disrepair

Over time, the practice of American government has increasingly departed from this idealized pattern. Especially after the New Deal and the Second World War, Washington was transformed from a sleepy backwater into the nerve center of a massive welfare state. The relative independence and autonomy of the different levels of government envisioned under dual federalism were replaced with "cooperative federalism" by the 1960s and extraordinary intergovernmental interdependence during the 1970s (Beer, 1973; Scheiber, 1978; Walker, 1981). Despite the development of new, more flexible forms of assistance—revenue sharing and consolidated block grants—federal aid programs continued to mount in number, functions, recipient jurisdictions, and dollar amounts, and were augmented by new regulatory "mandates" and a steadily increasing number of preemptive statutes.

Federal theory has never adjusted adequately to these changing circumstances. To be viable, a political theory should be *descriptive* of key facts; give rise to useful *generalizations;* and include a *normative component* that offers useful policy guidance (Mayo, 1960, p. 11). Accordingly, a theory of federalism should keep abreast of changing empirical realities and, from a normative perspective, should justify the existence of two independently constituted levels of government; offer guidance on the appropriate allocation of functions between levels; specify the areas and character of shared responsibilities; and indicate how power may be balanced between levels in a manner that preserves and sustains the federal arrangement.

Contemporary theories of federalism are often faulted on all of these grounds (Wright, 1978, pp. 16-20). Although recent scholarly efforts to keep pace with policy developments have produced many excellent descriptive accounts of intergovernmental practices, critics stress difficulties in generalizing from the results of program studies and in developing well grounded policy recommendations. From a normative standpoint, the highly prescriptive theory of dual federalism has been abandoned without a satisfactory replacement.

A considerable number of scholars of federalism—with distinctive theoretical commitments of their own—have decried the current status of inquiry. Daniel Elazar (1981), who remains the field's best recognized expert, believes that

> . . . there is a crisis in American thinking about federalism. Even a casual perusal of the literature on the subject of the past decade, explaining, justifying, or advocating particular courses of government action in the United States in the name of federalism, would indicate that this is so. (p. 15)

Patrick Riley (1973, p. 90) has agreed that "the theory of federalism is in a state of confusion," while historian Harry Scheiber (1980, p. 664) has declared it to be in "considerable disarray." William Riker (1969, p. 145) suggests that the belief that federalism makes any difference has resulted in much misdirected scholarly effort, including his own. And, after a comprehensive historical review of federalist thought, Rufus Davis (1978) glumly concluded that:

> . . . there has rarely been a time in the history of the subject when it has been in a more depressing and uncertain condition than it is now. And this is not because we know less about the facts of federal life; on the contrary, there has never been a time when so much has been known about the subject. . . . Only the more we have come to know about it, the less satisfying and less reputable has become almost the whole legacy of our federal theory. (p. 205)

The Shift to Intergovernmental Relations

After the outpouring of new legislative enactments during the "Great Society" era, many scholars openly proclaimed "the final burial . . . of traditional doctrines of American federalism" (Sundquist, 1969, p. 6). "Federalism—old style—is dead," Michael Reagan (1972, p. 3) concurred, naming intergovernmental relations as its successor. Federalism, he charged, was a static legal concept stressing the constitutional division of authority and functions between the national government and the states. Intergovernmental relations, agrees Deil Wright (1978), is a dynamic, political and pragmatic concept, emphasizing the actual administrative relationships between the levels of government in the day-to-day performance of shared responsibilities.

This new approach fit well with the discipline's growing empiricism and the turn away from institutional and normative problems. Students of intergovernmental relations made questions of administration, politics, and finance the chief foci of inquiry. Building upon the work of earlier students (Anderson, 1956, 1960; Graves, 1964), they examined closely the development and impact of new social programs, especially those involving the federal government in urban affairs.

There is no doubt that these studies have enormously deepened our understanding of the operation of major grants-in-aid (Sundquist, 1969; Derthick, 1972; Pressman & Wildavsky, 1973; Nathan, Manvel, & Calkins, 1975; Larkey, 1978) and clarified the contours of the aid system as a whole (Wright, 1968; Mushkin & Cotton, 1969; ACIR, 1978; Anton, Cawley, & Kramer, 1980). At the same time—partly, perhaps, because of the inherent difficulties in generalizing from case material—the theoretical contributions have been relatively meager. Sheldon Edner (1976) has declared intergovernmental relations to be a "virtual wasteland" from a theoretical standpoint:

> Much of the literature has been confined either to political rhetoric and speculative analysis or a fragmented, piece-by-piece look at some of the elements of intergovernmental relations—i.e., specific programs, . . . administrative and professional contact between levels, . . . and most thoroughly on the fiscal aspects of the system. Little, if any, work has been done to try and trace the policy implications of the results of these studies. (pp. 150-151)

To Van Horn and Van Meter (1976) most writers on intergovernmental relations

> . . . are less interested in prescribing the proper allocation of responsibilities than with charting existing relationships where the assignment of responsibilities is shared by various governmental units. The primary task of these works has been to provide a description of the intergovernmental system with emphasis on such concerns as the distribution of power, the sources of leverage held by each governmental jurisdiction, and the consequences of administrative centralization and decentralization.
>
> While this body of literature has alerted us to a number of important considerations, it has failed to provide an analytic framework promoting either tests of the significance of particular variable clusters . . . or the policy implications of a particular administrative arrangement. . . . Since most of these works lack a coherent theoretical perspective, the results of the analysis lack generality, and they tell us little about how public policy is implemented in the intergovernmental system. (p. 41)

Catherine Lovell (1979) summarizes the current "conceptual crisis" well in stating that

> We have slipped away from the search for models of federalism and into the more satisfactory, albeit as yet less rigorous, discussion of IGR. . . . The transi-

tion in theoretical focus from "federalism" to "IGR" has not answered many fundamental questions. (pp. 11-12)

CONFLICTING ASSESSMENTS OF AMERICAN PRACTICE

One good indication of the present confusion is provided by scholarly disagreements over the effect of new intergovernmental grant and regulatory programs on the balance of power between the states and the national government. On the one hand, there are those who view the recent growth of new federal programs with considerable alarm, believing that centralizing forces have compromised essential virtues of American federalism. In the judgment of Stephen Schechter (1982)

> Over the past fifteen years the United States has crossed the fault line from a federal system to a decentralized national system. . . . When it comes time to make policy, all eyes look to Washington, and federalism is viewed as one among many cross-pressures rather than as a pathway through them. When it comes time to implement policy, federalism is transformed into a managerial model in which the states and localities are cast in the roles of middle and lower echelons of management that cannot be trusted to follow orders without being paid off and reined in. The political idea of states as polities and localities as communities has all but disappeared. (p. 61)

Similarly, Daniel Elazar (1980) has answered his own question, "is the federal system still there?," by stressing the shift in policy determination from the states to Washington:

> [W]e have moved to a system in which it is taken as axiomatic that the federal government shall initiate policies and programs, shall determine their character, shall delegate their administration to the states and localities according to terms that it alone determines, and shall provide for whatever intervention on the part of its administrative agencies as it deems necessary to secure compliance with those terms. . . . Not only has the Constitutional theory of federalism been replaced by a half-baked theory of decentralization, but it is a vulgar and, at times, vicious theory as well. . . . (pp. 84-85, 86)

Theodore Lowi (1978) concurs, arguing that the development of new, more coercive regulatory and redistributive programs at the national level merits a change in nomenclature from the "United States" to the "United State" (p. 15). What he terms the "Second Republic" has a new constitutional foundation, rooted in the belief that "There ought to be a national presence in every aspect of American lives. National power is no longer a necessary evil; it is a positive virtue" (1979, p. xi). Finally, Martin Landau (1973)—while finding some resurgence of the federal principle in the myriad organizational entities at the local level—has concluded that

> . . . the nationalization or centralization of authority has all but stripped the states of their independence. If there are now two separate systems operating in distinct but parallel channels, the states are not one of them. (pp. 191-192)

In pointed contrast, other scholars believe the reports of federalism's demise—as Mark Twain said about his own obituary—are greatly exaggerated. To Donald H. Haider (1981),

> . . . federalism . . . is very much alive, though a bit battered from the past twenty years' experience. . . . Although complex and multifaceted, federalism is still susceptible to experimentation, flexibility, and change. (p. 30)

Drawing upon his monitoring studies of general revenue sharing and the community development and CETA block grants (see Nathan, 1982), Richard Nathan argues that much of the concern about intergovernmental relations is rooted in mythology, rather than facts. For example, he maintains that

> . . . local democracy in the United States is really quite healthy; the trend of the past decade toward broader and less conditional federal grants has aided and abetted localism as a basic value of our political system. . . . The odd man out in all of this lately has been the states, the all important middle-man of the federal system. However, the states have basically good prospects down the road (1981a, p. 535; see also Nathan, 1981b; and Anton, 1980, p. 74)

Catherine Lovell (1979) shares a similar view, suggesting that only "on the surface" does the intergovernmental system appear to be tilting to Washington. Counterbalancing the fiscal advantages of the national government, she suggests, are new political pressures:

> [T]he last decade has seen the failures of so much centralization and has brought intensified demands for devolution and decentralization. . . . New breeds of liberals have joined hands with traditional conservatives in Congress to enact general revenue and block grant forms which devolve responsibility and expand state and local flexibility in decisions about spending. . . . Local governments are alive and well and becoming more aggressive. (pp. 9-10)

These sharply conflicting assessments illustrate the limitations of current theories of federalism and intergovernmental relations and demonstrate the diversity of views that presently mark the field. Leading authorities cannot agree whether American federalism is "very much alive" or recently expired, having crossed the line to a unitary state. Such dissension has both empirical and normative roots. In part, the current "complexity of the American public sector has itself been a major obstacle to clear understanding" (Anton, Cawley, & Kramer, 1980, pp. 9-10) and, as Lovell argues (1979, p. 6), "intergovernmental processes have been modified . . . more rapidly than we have been able to understand them." Yet, earlier developments in both theory and governmental practice contributed equally to this confusion. The balance of this essay will examine both dimensions, tracing the dissolution of federal theory as it has developed over time. Three schools of federal thought, each rooted in classical defenses of federalism, are examined, with particular attention to the potential guidance each provides on current federalism issues. Finally, the paper identifies some promising directions for future research.

FEDERALISM AND DEMOCRACY

From the late eighteenth century to the present, two basic political claims have been advanced on behalf of federalism. One maintains that federalism provides the most advantageous governmental arrangement for reconciling the competing political advantages of large and small republics. The second emphasizes its role in preventing concentrations of governmental power and in promoting public access to government decision-making.

While powerful arguments were developed very early along these lines, there have been few recent advances in federal theory. Normatively, these political arguments do lend support for the concept of dividing governmental authority, but they provide little practical guidance for designing a workable federal system. Empirically, evaluations of the links between democratic values and federalism have produced mixed or inconclusive results. Moreover, democratic theories of federalism have lost much of their original descriptive value as intergovernmental administrative arrangements have proliferated and American politics has been nationalized during the twentieth century.

Governmental Size and Democracy

An essential element in the political theory of federalism stems from the view that genuinely democratic government can flourish only in a small political entity. This notion has exerted a powerful influence on American political thought from colonial times to the present. It arises ultimately from the physical constraints on direct democracy: a true democracy cannot exceed a size that will accommodate a decision-making assembly of its citizens. Even allowing for representation, many believe the association between democratic vigor and smallness to be strong. In a small community, citizens are thought to be more familiar with both issues and leaders. They may perceive a greater stake in governmental affairs and have a greater sense of political efficacy. Both motivationally and logistically, then, smallness often is thought to contribute to democracy by enhancing citizen participation in and control over government.

Initially, such reasoning drew heavily upon the writings of Montesquieu (1748/1961), who wrote that:

> It is natural for a republic to have only a small territory; otherwise it cannot long subsist. In an extensive republic there are men of large fortunes, and consequently of less moderation . . . the public good is sacrificed to a thousand private views; it is subordinate to exceptions, and depends on accidents. In a small one, the interest of the public is more obvious, better understood, and more within the reach of every citizen. (pp. 395-396)

In America, this theory found its most influential spokesman in Thomas Jefferson—although similar views were held by all the "anti-federalist" opponents of the Federal Constitution. Jefferson urged that American government be founded on a series of local "wards," so that "any citizen can attend, when called upon, and act in person" (1816). In the New England states, he observed, such wards or townships "are the vital principle of their governments, and have proved themselves the wisest invention ever designed by the wit of man for the perfect exercise of self-government, and for its preservation."

Despite the passage of time, this view has continued to exert a powerful and lasting effect on American political thought and rhetoric. Today, it is often expressed as the belief that local government is "closer to the people," but the concept itself has changed very little. Thus, the following statement by Richard Nixon (1971) might well have been spoken by a Jeffersonian Democrat:

> [T]he further away government is from people, the stronger government becomes and the weaker people become. . . . [L]ocal government is the government closest to the *people* and it is most responsive to the individual *person;* it is peoples' government in a far more intimate way than the government in Washington can ever be. (pp. 53, 59)

Carried to its logical conclusion, however, this argument favors confederation, not federalism, and was advanced by the antifederalists for this reason. It therefore raises all of the impracticalities and administrative ills of confederation identified and attacked in the *Federalist*. But there also is contained in the *Federalist* a deeper, philosophical challenge to the notion that small units of government are particularly suited to nourish democratic government. In his famous argument in *Federalist* 10, Madison turned the small republic theory on its head, arguing that small states actually are more, not less, susceptible than large ones to domination by a narrow faction:

> The smaller the society, the fewer probably will be the distinct parties and interests composing it; the fewer the distinct parties and interests, the more frequently will a majority be found of the same party; and the smaller the number of individuals composing a majority, and the smaller the compass within which they are placed, the more easily will they concert and execute their plans of oppression. (p. 60)

The greater diversity of a larger state mitigates this tendency and promotes freer, more representative government:

> Extend the sphere, and you take in a greater variety of parties and interests; you make it less probable that a majority of the whole will have a common motive to invade the rights of other citizens; or if such a common motive exists, it will be more difficult for all who feel it to discover their own strength, and to act in unison with each other. . . . [I]n the extent and proper structure of the Union, therefore, we behold a republican remedy for the diseases most incident to republican government. (p. 61)

A variety of modern scholars have come to share Madison's assessment of the relative disadvantages of local government. Grant McConnell (1966) argued that "the effect of a small constituency is to enhance the power of local elites. . . . Decentralization . . . does not make for democracy. . . . It creates conditions hostile to democracy" (pp. 109, 114). Likewise, Morton Grodzins (1963, pp. 9, 10) maintained the claims that local government is closer to the people are simply "meaningless."

Such reasoning might ultimately imply a unified national state, although it was developed in the *Federalist* in support of the proposed Constitution. Nevertheless, there has been a continuing and perhaps inevitable tension between the two schools of thought. For most of American history, the conventions of localistic democracy held sway among the populace-at-large (Croly, 1965; Beer, 1973). Not until the 1930s was the federal government widely perceived to be more democratic than subnational units of government. At this time, a series of developments unforeseen in the eighteenth century—the creation of a modern party system, welfare state, and a system of progressive federal taxation—contributed to the belief that a distant national government could become more representative of and responsive to a majority of its citizens.

However, the tensions between the competing theories subsumed under federalism did not diminish over time, nor have they been resolved by empirical evidence on the relative political merits of different levels of government. Though the record is sparse, it tends to yield mixed results. On the one hand, the national arena seems to possess advantages for organized electoral participation. As a rule, party competition appears to be stronger at the national level (Leach, 1970, p. 124). Moreover, the nationalization of the media means that political information is increasingly focused on the national government. Studies of governmental salience have found that people are more aware of the federal government (Jennings & Zeigler, 1970). Other studies show that more people can identify their national elected officials than their state and local officials (Reeves & Glendening, 1976), and electoral statistics demonstrate that voter participation is higher in national elections (Verba & Nie, 1972, p. 31; Dahl & Tufte, 1973, p. 57).

On the other hand, modern studies of public attitudes reveal that people believe local government is more understandable to them, and they feel more capable of affecting a governmental policy at the local level (Dahl & Tufte, 1973, pp. 53-61). With the apparent dissolution of our political parties, this sense of efficacy may become an increasingly important source of democratic stability. In addition, survey research indicates that people have more confidence in state and local government performance ("Opinion Roundup," 1982a, p. 36, 1982b, p. 28).

These varied results are generally supportive of the federal principle, with its efforts to utilize the merits of each level of government. But this is, at best, a very general concept. It offers little guidance for the actual assignment of governmental functions or for coping with the exigencies of governing. Within a balanced federalism, one might prefer a somewhat greater concentration of programs or authority at one level or another, but unless there is a clear link between citizen participation and representation in a certain function at a given level of government, democratic assertions about governmental size provide little basis for determining where individual programs should be placed or how they should be shared. This perspective does suggest, however, that a governmental system should be simple enough to facilitate citizen understanding and have clear channels of accountability.

Federalism and Pluralism

While challenging the small republic theory of democracy in *Federalist* 10, Madison suggested a second link between federalism and democratic government. Federalism, he argued, was one expression of the constitutional system of divided powers intended to safeguard individual liberty. As Madison argued in *Federalist* 51:

> In the compound republic of America, the power surrendered by the people is first divided between two distinct governments, and then the portion allotted to each subdivided among distinct and separate departments. Hence a double security arises to the rights of the people. The different governments will control each other, at the same time that each will be controlled by itself. (p. 339)

Thus, he provided a pluralistic defense for federalism based, not on the democratic merits of different levels of government, but on the constitutional protections afforded by the territorial division of governmental authority.

This justification of federalism has attracted many modern adherents (Elazar, 1968, p. 354), and, over time, it has been strengthened and elaborated by additional pluralist arguments. It is maintained, for example, that the multiple power centers and access points in a federal system may enhance governmental responsiveness and improve the process of representation (Truman, 1971, p. 507; and Beer, 1978, p. 15). In the study of comparative government, federalism has been viewed as a device for protecting and insulating territorial minorities in such ethnically divided countries as Canada, Switzerland, Yugoslavia, and Nigeria.

To be sure, most empirical research has failed to demonstrate any systematic relationship between a federal system and the protection of individual and minority rights. As William Riker (1964; see also Neuman, 1955, p. 54) has pointed out:

> Local self government and personal freedom both coexist with a highly centralized unitary government in Great Britain and the Vargas dictatorship in Brazil managed to coexist with federalism. (p. 140)

In the U.S., he stressed, federalism facilitated the oppression of blacks, first as slaves and later as a depressed caste (1964, p. 152). Similarly, Eric Nordlinger (1972, p. 31) found that "federalism may actually contribute to . . . the failure of conflict regulation [in] some deeply divided societies." Nonetheless, it is evident that federalism has assisted in avoiding undue concentrations of political power in the American context, and it probably contributes to protecting individual liberty when it is part of a broader fabric of constitutional representation and restraint. Most importantly, perhaps, the pluralist argument for federalism is one of the few theories that clearly justifies genuine federalism, with its autonomous and unalterable subunits, rather than simply administrative or political decentralization.

Federal Theory and Political Change

Possibly the most serious problem facing both political theories of federalism is that neither has been systematically adapted to account for dramatic changes in American governmental structure and operation over time. Moreover, in comparative politics, where scholars first attempted to modernize political theories of federalism, they launched a chain of reasoning that threatened to trivialize the subject altogether. Starting in the 1950s, a number of political scientists began to view federalism in behavioral terms, as a political "process" or "bargain" that was frequently an unstable phase of political development (MacMahon, 1955; Riker, 1964; Friedrich, 1968). As the often vague concept of the federal process was stretched to encompass "infinite variety" (Earle, 1968), however, one prominent scholar was led to conclude that the concept was simply meaningless and without value (Riker, 1975).

In the American context, one example of the growing disjunction between democratic theory and governmental practice is evident in Madison's argument for an extended republic in *Federalist* 10, which was premised on the difficulties of enacting legislation at the national level (Leach, 1970, p. 57). In *Federalist* 46, moreover, he argued that state interests would be abundantly represented in the Congress.

Until the 1960s, such assessments appeared to be substantiated by political scholarship replete with references to "veto groups," "deadlock" and the "obstacle course" in Congress. For its part, the political party system was described as the bulwark of state and local interests in the United States (Grodzins, 1966, p. 254). In the post-1960 era, such generalizations seem hopelessly anachronistic (King, 1978). A decentralized and entrepreneurial Congress has been transformed from a legislative burial ground into a principal source of federal program initiatives (Orfield, 1975; Price, 1972; ACIR, 1981a). Interest groups are now widely viewed as the clients and supplicants of new federal legislation. The decentralized party system has decomposed, replaced in part by an active national media and by nationally-based interest groups. Yet the implications that dramatic federal growth and the nationalization of politics hold for the political theory of federalism have barely been explored.

Similarly, federal theory has yet to come to grips with problems of political accountability once "cooperative" federalism becomes the rule instead of the exception. Established theories have explored the relative advantages of concentrating government decisionmaking at the national or local level, as well as the advantages of utilizing both levels in a federal structure and dividing governmental functions between them. But as more and more activities are shared—often among three or more levels of government—through an increasingly complex array of grant and regulatory programs, political accountability becomes increasingly diffused. Citizen and even scholarly understanding of the actual workings of government is eroded, and responsibility for program performance cannot be fixed. Thus, the links between public preferences, citizen involvement, and policy outcomes that federalism was designed to enhance have become attenuated, with little guidance for redressing the situation provided by static theories of federalism.

THE POLITICAL ECONOMY OF FEDERALISM

A second normative approach to the study of federalism applies the methodology of economic analysis to political behavior in a federal system. This approach, often referred to as fiscal federalism or rational choice theory, shares many of the concerns of the more traditional democratic theories of federalism. Both tend to define political behavior in simple majoritarian terms. Moreover, both are concerned with the influence of governmental structure on the relationships between citizen preferences and public policy. On this basis, rational choice theory argues convincingly that federalism matters—that the territorial organization of government affects the content of policy.

Although its origins have been traced back to the *Federalist* (Ostrom, 1971), rational choice theory has developed most fully only in recent years. It has made several notable contributions to the study of federalism, such as providing a general framework for assigning governmental functions. Ultimately, however, rational choice theory suffers many of the flaws of traditional democratic theory in failing to account adequately for many features of modern intergovernmental relations.

Government Structure and Public Preferences

The underlying justification for federalism in rational choice theory derives from the distribution of public preferences for various public goods. Given regional variations in the demand for a particular public good, government production of the good can more closely match the preferences of more individuals through a decentralized system of government. Each jurisdiction then can respond to local preferences precisely, rather than having the central government produce a uniform level of the collective good (Oates, 1972). This efficiency can be enhanced through the market-like conditions of residential mobility, allowing like-minded individuals to migrate to jurisdictions offering a "market basket" of public goods corresponding to their own preferences.

A basis for allocating governmental functions also can be found in the argument that welfare is maximized when individual preferences match jurisdictional boundaries. Mancur Olson calls this concept the "principle of fiscal equivalence." He suggests (1969) that inefficiencies will occur when:

> (1) the collective good reaches beyond the boundaries of the government that provides it; [or] (2) the collective good reaches only a part of the constituency that provides it. (p. 482)

Either case results in economic externalities (or "internalities") that may encourage suboptimal production of collective goods. When benefits from a local function extend to nonpaying residents outside a jurisdiction, incentives exist to support the program at levels below its total social value. On the other hand, if a portion of the costs flow to individuals on the outside, the jurisdiction may overproduce the good because it is not bearing the total costs of the program. Similar effects result from "internalities," wherein a public good reaches only a geographic subset of a jurisdiction's population.

Based upon these propositions, most economists assign the tasks of economic stabilization and income redistribution to the central government, due to the national scope of their impact and the constraints on decentralized responses to them (Oates, 1972, pp. 4-11, 31-33; see also Musgrave, 1959; Ladd & Doolittle, 1982). Other governmental functions are then assigned to various levels of government according to the geographical range of their benefits. On the basis of this principle, economist George Break developed a classification of 20 public services useful in making functional assignments. For example, police and fire protection were classified as local functions; pollution control and mass transit were described as regional; and education and income maintenance were placed in the federal category (Break, 1967, p. 69).

Carried to an extreme, however, optimizing the concurrence between individual service preferences and governmental jurisdictions would promote a vast array of special servicing authorities (Ostrom, 1973) and ultimately "a separate governmental institution for every collective good with a unique boundary" (Olson, 1969, p. 483; Tullock, 1976). To avoid this in cases where externalities are of only modest size or accurate functional boundaries are difficult to draw, rational choice theorists recommend the use of grants from surrounding or higher jurisdictions to compensate a provider government for its production of external benefits (Olson, 1969, pp. 485, 486; Break, 1967). Without such assistance, the producing jurisdiction would have economic incentives to underproduce the collective good involved.

Evaluating Economic Theory

This line of reasoning suggests that the territorial organization of governmental activity can have a significant and systematic effect on policy outputs and on governmental responsiveness to citizen preferences. In a period marked by behavioralist skepticism about the effect of government structure on behavior, this contribution alone is significant. In addition, rational choice theory establishes an explanation and justification for the growth of intergovernmental grants, and it helps to direct the assignment of functions.

Yet, there are a series of unresolved problems confronting rational choice theory. At the most general level, economic theory does not justify federalism but political decentralization (Oates, 1972, pp. 17, 18). The fixed jurisdictional boundaries of component states in a federal system are inconsistent with the economic objective of continuous jurisdictional adjustments to changing patterns of externalities (Beer, 1977, p. 27).

Moreover, the promise of rational choice as a tool for directing functional allocations has been largely unfulfilled to date. In many cases, the externalities resulting from governmental policies simply cannot be measured with any accuracy (Oates, 1977, p. 6; McKean, 1966, p. 54). This problem is exacerbated when various important but unquantifiable factors are included in the calculation of functional assignments. For example, the gains promised by fiscal equivalence may be eroded by countervailing advantages present in a simpler system of fewer, consolidated jurisdictions, such as administrative coordination and economies of scale. In addition, increased governmental fragmentation in a rational choice setting may impose sizable information costs even on the population of rational citizens inhabiting the world of public choice. Because such factors rarely can be measured, rational choice theory may lose much of its normative power when confronting the real world.

Partly because of such difficulties, the policy prescriptions of rational choice theorists often vary, especially between more traditional public finance economists and a newer school of "public choice" economists. The former often utilize the theory to justify increased use of federal grants (Break, 1967) and greater program centralization. As Alan Campbell (1975) observed: "To minimize the flow of externalities, a greater number of activities must be assigned to large jurisdictions—to metropolitanwide ones, to states, and frequently to the national government" (p. 36). In contrast, the public choice school has favored highly decentralized policies. Its advocates (Bish, 1971; Ostrom, 1961) look favorably on a great diversity of small local governments that can establish market-like conditions in public services, as each jurisdiction competes for resident taxpayers through its offering of services and its rate of efficiency and taxation. Public choice economists have also expressed strong opposition to governmental centralization in other forms. Ostrom (1974), Niskanen (1971), and Buchanan (1977), for example, have all stressed the administrative and representational weaknesses of large, centralized bureaucracies.

Ultimately, both schools resemble political theories of federalism in their failure to adequately address current patterns of federal politics and intergovernmental relations. Economic analysts generally assume drastically oversimplified models of political behavior. Thus, the median voter model of representative government often is used to link government behavior to externalities (Olson, 1969, p. 482), despite ample evidence that actual electoral behavior is poorly described by the model (Stokes, 1963). More recent attempts by public choice theorists to explain government growth on the basis of budget maximizing bureaucrats (Niskanen, 1971) also lack a strong empirical footing (Derthick, 1975; ACIR, 1981a, p. 20).

The relationships between rational choice theory and current intergovernmental practice are also inadequate. Although the theory does attempt to account for intergovernmental transfers, grants are often viewed as marginal adjustments to the system. Economic theories of federalism still assume that the normal pattern of governmental adjustment to externalities is for spillovers to be internalized within relatively autonomous jurisdictions. Moreover, close observers of the intergovernmental system note that grants often do not fit the economic model. Charles Schultze (1974) maintains that "most categorical grants for social programs are not based on this [spillover] rationale" (p. 182). Like others (Monypenny, 1960; Mushkin, 1960), he suggests that existing programs are products of quite different historical and political factors. Similarly, economic norms about the proper design of grants conflict sharply with administrative experience derived from operating programs (Beam, 1980). In short, the powerful normative thrust of rational choice theory, like traditional democratic theory, requires more empirical verification and descriptive validity in order to contribute more fully to policy issues, both now and in the future.

ADMINISTRATIVE PERSPECTIVES ON FEDERALISM

Another early rationale for federalism centered on administrative considerations. The Founders believed that the system of divided authority established by the Constitution would assure better management of public affairs than had occurred under the preceding confederation or would be possible under a wholly unitary state. This remained the dominant administrative premise for at least 140 years. Although political scientists and public administrators developed new theories of cooperative federalism to describe and justify the expansion of intergovernmental programs from the New Deal to the mid-1960s, many recent commentators stress the obstacles to effective management created by a federal division of authority and the extensive use of intergovernmental techniques. However, despite some promising recent suggestions, no widely accepted alternate theory has yet been devised.

The Premise of Functional Separation

As MacMahon (1972, p. 29) observes, the United States pioneered the principle of direct national administration in a federal system. In *Federalist* 23, Hamilton stressed that one of the principal failings of the Articles of Confederation was the dependence of the national government upon the states in such crucial areas as defense and finance. The national government, he argued, needed to be authorized to execute independently whatever functions were entrusted to it. Under the new Constitution, he contended,

> . . . the essential point . . . will be to discriminate the OBJECTS . . . which shall appertain to the different provinces or departments of power; allowing to each the most ample authority for fulfilling the objects committed to its charge. Shall the Union be constituted the guardian of the common safety? Are fleets and armies and revenues necessary to this purpose? The government of the Union must be empowered to pass all laws, and to make all regulations which have relation to them. The same must be the case, in respect to commerce, and to every other matter to which its jurisdiction is permitted to extend. . . . Not to confer in each case a degree of power, commensurate to the end, would be to violate the most obvious rules of prudence and propriety, and improvidently to trust the great interests of the nation to hands, which are disabled from managing them with vigour and success. (p. 144)

A century later, John Stuart Mill (1861/1958) endorsed this theory as well:

> Within the limits of its attributions, [the Congress] makes laws which are obeyed by every citizen individually, executes them through its own officers, and enforces them by its own tribunals. This is the only principle which has been found, or which is ever likely to produce an effective federal government. (p. 323)

On the other hand, the Founders—and many others since—also stressed the impossibility of effectively managing a large nation from a single center. Jefferson (1816/1939) bolstered his democratic arguments for federalism with a claim that a unitary system would invite maladministration:

> Our country is too large to have all its affairs directed by a single government. Public servants at such a distance . . . must . . . be unable to administer and overlook all the details necessary for the good government of the citizenry, and the same circumstance, by rendering detection impossible to their constituents, will invite the public agents to corruption, plunder, and waste. (p. 30)

Madison (*Federalist* 14) offered a parallel view. Indeed, he contended,

> [I]t would not be difficult to show that if [the states] were abolished the general government would be compelled, by the principle of self-preservation, to reinstate them in their proper jurisdiction. (p. 102)

Thus, important administrative arguments were offered in support of dual federalism, and early practice largely conformed to these dictates. Nearly all major functions of the time (the administration of justice, road building, care of the indigent, the chartering of businesses) fell primarily to local and state governments. The federal establishment of the early 1800s was "small almost beyond modern imagination," with a "Congress larger than its administrative apparatus" (Young, 1966, pp. 28, 31). Yet when needs arose within its own areas of competence, the federal government could and often did act independently to carry out its responsibilities (Walker, 1981, p. 51; MacMahon, 1972; Beer, 1982, p. 17).

Although Hamilton's early emphasis on an energetic executive set out a philosophy of administration that served the nation well, and was in many respects far in advance of its time, professional attention to the subject was almost entirely lacking during most of the nineteenth century (White, 1955, pp. 10, 14-15). Recognition of the managerial function as one requiring specialized expertise and technique did not occur until 1890 and thereafter. When it began, it began

> . . . in the cities, especially the big ones, not in theories of sovereignty or the state or separation of powers. The cities were where most government was, where most action was, where most problems were, where the services of public administrators could most demonstrably be made more effective, more honest, and less costly. (Mosher, 1975, p. 8)

New administrative doctrines, developed chiefly by municipal governments, spread to the states and to Washington. The focus of attention was on prescriptions that remain the heart of administrative orthodoxy—a strong chief executive with budgetary and appointive powers, hierarchical organization by function, and the separation of administration from politics—at *each* level: national, state, and local. Functional autonomy of these governments, as prescribed by dual federalism, was assumed, since this accorded well with predominant practice. Indeed, Lovell (1979, p. 15) notes that many administrative theorists—unwisely in her view—still continue to assume that most functions should be performed by one particular level of government.

In another respect, however, the newly emerging theory of public administration was inconsistent with the traditional understanding of federalism. Administrative doctrine requires the presence of a formal hierarchy, whereas federalism is incompatible with such a form of organization. Eventually, this hierarchical model was transferred from administrative relationships *within* each government to those *between* them, though the authoritative relations which make hierarchical structuring possible were absent in this setting. From the standpoint of conventional theory, as Ostrom (1974, p. 35) has charged, "Large jurisdictions are preferred to small. Centralized solutions are preferred to the disaggregation of authority among diverse decision structures." To be sure, operational considerations may require an element of decentralization in the execution of policies established at the top. But even decentralization may imply hierarchical relations, "a pyramid of governments with gradations of power flowing down from the top" (Elazar, 1976, p. 13).

The Rationale for Cooperation

Morton Grodzins (1966) and Daniel Elazar (1962) in particular have stressed that the actual allocation of functions in the American system never corresponded entirely with the dictates of dual federalism. For example, even before the Constitution was ratified, the national government offered land grants for public schools.

During the nineteenth century, the number of land and cash grants slowly mounted, and most of the features of the modern formula grant—with its plans, matching requirements, audits, and so forth—were in place by the 1920s (ACIR, 1978, pp. 16-17). Yet, no special place was provided for these programs in administrative theory. After all, the Hamiltonian vision of independent governmental levels, separately financed and administered, still applied in most fields. Even as late as 1927, state governments received only about 2% of their revenues through the 15 grant programs then operating (Walker, 1981, p. 62).

This changed during the New Deal. The enactment of permanent and temporary grant-in-aid programs generated outlays that, in 1939, were more than 15 times the 1933 total, and touched a host of new functional fields. Supreme Court decisions in that period gave the grant technique a firmer constitutional footing and eliminated long-standing barriers on the scope of Congressional action.

The range of new intergovernmental activity began to attract attention from some scholars (Key, 1937; Clark, 1938). Those who examined the administrative implications of grants-in-aid often found much to applaud in what seemed to be a practical device for shared responsibility and cooperative effort. An early study stressed its managerial advantages:

> The grant system builds on and utilizes existing institutions to cope with national problems. Under it the states are welded into national machinery of sorts and the establishment of costly, parallel, direct federal services is made un-

necessary. A virtue of no mean importance is that the administrators in actual charge of operations remain amenable to local control. In that way the supposed formality, the regularity, and the cold-blooded efficiency of a national hierarchy are avoided. (Key, 1937, p. 383)

Although intergovernmental mechanisms were generally regarded as exceptional by the public administration community (White, 1955, p. 138), and sometimes ignored entirely (Pfiffner & Sherwood, 1960), this positive view prevailed. The Commission on Intergovernmental Relations, which undertook the first comprehensive official assessment, regarded the administrative problems associated with the grant system as comparatively minor inconveniences, more matters of detail than basic design (1955, pp. 140-142). Indeed, the strength of federalism increasingly was said to lie in its capacity for collaborative, rather than separate, action. As Grodzins (1966) declared:

> The grant programs have supplied a cooperative method for achieving results that might never have been achieved if the grant technique had not been developed. . . . It made possible the allocation of responsibilities between the levels of government according to criteria of administrative and fiscal efficiency. These criteria can be simply stated: the national government assumed partial responsibility for supplying funds and primary responsibility for establishing minimum standards of service, because the national government possessed superior fiscal resources and was concerned with the general welfare of the residents of all states. The states (and their political subdivisions) assumed primary responsibility for administration, because they were in the better position to interpret and meet local needs. (p. 62)

Contemporary Crosscurrents

Through the 1950s, the model of "cooperative federalism" provided an excellent description of federal-state relations. Grant programs were manageable in number and concentrated chiefly in areas in which federal and state officials shared common goals and administrative perspectives. Beginning in the mid-1960s, however, both public officials and management experts began to recognize that this doctrine offered inadequate guidance on many emerging management issues. These concerns reflected the proliferation of categorical aids—more new grants were authorized between 1964-66 than in all preceeding years—as well as a broadening of recipient jurisdictions and changes in program requirements (ACIR, 1967, pp. 150-184).

In the wake of the Great Society, many state and local officials complained of administrative confusion and red tape resulting from the array of new programs (Haider, 1974, pp. 52-62). Such analysts as Sundquist (1968) noted that, while basic policy decisions had been made in favor of an expanded federal role, new administrative strategies were now required:

> Many of the new goals that have been proclaimed by the national government have to be achieved through the initiative and the administrative expertise of other governments, state and local, that legally are independent and political-

> ly may be even hostile. The transformation of the federal system seems to have been accepted, but the mechanisms that will make it work have yet to be perfected. . . . If and when a new "Hoover commission" is created, these are the questions that need attention. (p. 536)

Sundquist's critical assessment was confirmed by many later evaluation and implementation studies. Pressman and Wildavsky (1973), in particular, noted the difficulty of translating intentions into results through the multiplicity of actors and long string of decision points typical of most intergovernmental programs.

Yet, for the most part, such calls for reform as first emerged did not challenge the fundamental premises of the system of shared responsibility. Rather, the stress was upon devices for improved program coordination (at the community, regional, and national levels) and for the consolidation of separate categorical programs into broader block grants. This latter strategy had earlier been proposed by the First Hoover Commission (1949), which in a short discussion of federal-state relationships had condemned the growing fragmentation, overlap, and duplication among aid programs in a manner parallel to its criticism of haphazard executive branch organization. Although the Hoover Commission's proposals for grant consolidation met with little success in Congress, leading some to question the theory upon which they were based (Mushkin, 1960), block grants became a major instrument of reform under the Nixon Administration's 'New Federalism" (Conlan, 1981). Throughout, however, the key issue was viewed as simply one of the appropriate degree of administrative centralization or decentralization, not the reallocation of functions between autonomous governmental levels.

Many implementation researchers contend, however, that federalism poses very serious obstacles to the effective operation of centrally designed programs. They also stress that potential administrative pitfalls need to be taken into consideration during the policymaking process and generally favor simple, direct techniques for the delivery of public services over complex grants-in-aid. Yet, as Elmore (1979) has commented, "implementation research is long on description and short on prescription" (p. 601). Certainly it provides no clear or consistent guidance on questions concerning the proper allocation of functions and powers among governmental levels or for managing intergovernmental programs. Many studies confine their findings and recommendations to the operation of a specific aid program (Dommel, *et al.*, 1982; Mirengoff & Rindler, 1978; Nathan, Manvel, & Calkins, 1975). Those that do offer broader conclusions draw upon many different organizational models (Elmore, 1978) and often have conflicting policy views. Thus some writers believe that past failures of federal program initiatives resulted from a division of authority that

> . . . made it hard for federal policy makers to know what must be done to achieve their objectives locally, and for federal administrators to bring federal resources . . . effectively to bear on local settings. The same characteristics largely account also for the federal tendency to set unrealizable objectives. (Derthick, 1972, p. 93)

At the same time, others attribute the weaknesses of program performance to

> . . . the federal system—with its dispersion of power and control—[that] not only permits but encourages the evasion and dilution of federal reform, making it nearly impossible for the federal administrator to impose program priorities; those not diluted by Congressional intervention, can be ignored during state and local implementation. (Murphy, 1971, p. 60)

The first diagnosis seems to point toward devolution, the second toward increased national control.

In very recent years, a few public administrators have begun to assert that the issues posed by the management of federal-state-local relationships may necessitate a reexamination of the basic premises of administrative theory. Frederick Mosher (1980) notes that:

> In decades gone by, most of what the federal government was responsible for and expended money for it did by itself through its own personnel and facilities. Consequently, much of the doctrine and lore of federal management . . . was based on the premise that its efficiency rested on the effective supervision and direction of its own operations. (p. 541)

In fact, he adds, less than 20% of the federal budget now is directed to domestic activities that the federal government actually performs itself. Changes in both the *content* and *means* of program operation suggest the need, Mosher argues, for a "truly new public administration."

Another writer, Lester M. Salamon (1981), concurs with this general thesis, noting that the federal government is now heavily dependent upon "third parties," including state and local governments, for the accomplishment of its objectives. These changes may well have rendered many of the traditional preoccupations of public administration obsolete. The key to developing a more useful theory of public management, Salamon believes, might be to focus research on developing a systematic body of knowledge about the alternative "tools" of public policy, including various forms of grants-in-aid, insurance programs, loan guarantees, and so forth. From the standpoint of contemporary intergovernmental relations, special attention should be paid to the host of new regulatory devices affecting state and local governments (Beam, 1981). In contrast to the older "economic regulation," much of the "new social regulation" (Lilly & Miller, 1977) is directed at or implemented by these jurisdictions. This change—like the earlier development of grants—has important fiscal, political, and legal implications (Dubnick & Gitelson, 1981; Lovell, *et al.,* 1979; Muller & Fix, 1980).

Such innovative efforts to guide the research agenda warrant consideration. At the same time, however, they are a clear admission that an adequate theory of administration in a modern federal system does not exist presently. Public administrators have consistently failed to recognize the full implications of an expanding array of intergovernmental programs for governmental management at every level. While the traditional normative theory of separated responsibilities was set aside long ago, largely because it failed to accurately describe actual practice, no new approach has yet been developed to take its place.

CONCLUSION

This paper has outlined the theoretical divergence and empirical deficiencies of three traditional schools of federal thought. The record is disquieting, for it suggests that contemporary research provides inadequate guidance concerning the nation's most important governmental invention and the resolution of some of its most pressing political issues.

This is not to agree, however, that the concept of federalism is outmoded —or that it is, as some writers have argued, simply a temporary way station on the developmental path toward a single national community and a unitary national government. Ironically, just as federalism was being abandoned by many specialists, the influence of federalism and intergovernmental relations was belatedly discovered in other fields of political science. Comparative studies disclosed differences in the character of intergovernmental relations in federal and unitary states (Rose, 1982, pp. 161-165). The federal context of city politics was stressed increasingly in urban studies (Yates, 1979; Peterson, 1981). The role of federal grants in encouraging a new pattern of independent Congressional behavior was dissected in legislative research (Mayhew, 1974; Fiorina, 1979; Arnold, 1979), and new conceptions of bureaucratic politics and public administration were built upon intergovernmental foundations (Salamon, 1981; Mosher, 1980).

Even more importantly, federalism remains a living political reality, changed in form but embodying history and popular attachment (Hamilton, 1978). In policy circles, federalism is constantly being reborn in both pragmatic and philosophical attempts to restructure American government and improve its performance. In pursuit of very different objectives, issues of federalism have formed the centerpiece of domestic policy under four of the last five Presidents. Many post-industrial nations are currently grappling with proposals for devolution and the United States is no exception.

Indeed, if the idea of federalism did not now exist, it might have to be reinvented. Recent American experience validates Woodrow Wilson's (1908) observation that "the question of the relation of the States to the federal government is the cardinal question of our constitutional system"—one that cannot be permanently resolved because "every successive stage of our political and economic development gives it a new aspect, makes it a new question" (p. 173). Just as the rigid legal compartmentalization of national and state responsibilities was properly set aside to meet the exigencies of the Great Depression, theories of cooperative federalism used to explain and guide the formulation of policy during the late 1950s and early 1960s may now require similar revision. After all, their ready acceptance occurred in an era of widespread prosperity, rapid economic growth, growing public sector revenues, and high public confidence, and reflected the comparatively limited intergovernmental system that then prevailed. Despite cold war tensions, America's leadership in much of the free world was then unchallenged. In contrast, the contemporary period has been one of economic stagflation and resource shortages at all governmental levels, rising public alienation, and a very large and complex system of intergovernmental programs. The U.S. faces increasing economic competition from abroad, a declining status in global affairs, and an increasing threat of nuclear catastrophe. The states, on the other hand, are in far better institutional shape to assume an active domestic policy role than they were 15 years ago (Elazar, 1974; Sharkansky, 1978; ACIR, 1982; Reeves, 1983). All of these factors suggest that questions about the allocation of governmental responsibilities will figure prominently on the political agenda of the 1980s and 1990s, as they have during the past two decades.

Partly because of such developments, some scholars now suggest that American federalism may be in need of fundamental legal and constitutional reforms. For instance, Robert B. Hawkins, Jr. (1982; see also Elazar, 1980; Buchanan, 1975; and Wildavsky, 1980) comments that

> . . . the Founding Fathers, if they were around today, would certainly look at today's problems as problems of constitutional design. For example, the inability of Congress to control spending would be seen as a constitutional defect requiring a constitutional change. (p. 251)

Certainly, the widespread perception that key constitutional issues have all been resolved in favor of national authority overlooks the lively debate that has occurred within the judicial branch over the past decade. During the 1970s a considerable number of federal regulatory statutes were challenged by state and local governments (or other interested parties) and the judicial branch has reemerged as an important arena for intergovernmental deliberation (Cappalli, 1979; Horowitz, 1977; Howard, 1982; Kaden, 1979; Frug, 1978; Cole, 1982).

Although both legal and emerging policy issues deserve scholarly attention, we would suggest that the most critical task may be theoretical in nature: the consolidation and revision of the three schools of federal thought outlined above in light of current empirical observations. Each has something useful to contribute to the rejuvenation of federal theory, but none is adequate to the task alone. Because each perspective tends to correct, at least in part, for deficiencies in the others, there is a pressing need for a greater degree of theoretical integration. Some of the most interesting and potentially valuable works in recent years have drawn upon two perspectives—for example, studies in public choice merge politics and economics and implementation blends politics and administration—but very few have attempted to combine all three (but see Peterson, 1981).

Democratic theories, as the foregoing account makes clear, provide the most compelling rationale for maintaining a legally rigid system of divided authority under federalism, as opposed to a unitary system with some measure of administrative decentralization, which can be justified on a number of other grounds. This aspect of "checks and balances," like the separation of executive, legislative, and judicial authority, remains a useful hedge against excessive concentrations of power. Although such considerations may seem remote from the day-to-day concerns of programs and policymaking episodes during the Vietnam War and Watergate suggest that the Founders were wise in providing some institutional protection against potential abuses. At the same time, the experience of other nations demonstrates that federalism alone is by no means an adequate safeguard of individual liberties.

Although Madison's theory of the extended republic retains a certain credibility in the area of individual rights and liberties, which in recent times seem to have been most consistently advanced by the national government, it has become increasingly less relevant to many issues of positive governmental action. Madison believed that the sheer size and diversity of larger jurisdictions would tend to prevent their domination by organized interests in opposition to the majority will. This position had a certain plausibility during the 1950s and early 1960s, when the obstacles to action at the national level were so severe that scholars warned against the "deadlock of democracy" (Burns, 1963).

More recent experience, however, has seemed to turn Madison's forecast on its head. It is at the national level that both scholars and other political observers now find evidence of rampant factionalism and hasty action (King, 1978; Fiorina, 1977; Huntington, 1975). Charges are rife that the federal government is biased toward excessive spending and regulation, hypersensitive to the demands of special interest lobbies, or dominated by self-serving bureaucrats and politicians. The state-centered political party system—which once was regarded as the backbone of federalism—has largely withered away (Sorauf, 1976; Crotty & Jacobson, 1980). At the same time—partly because of the success of national reapportionment and civil rights policies—state governments and politics are far more representative of their constituencies than they were two decades ago.

From the standpoint of political representation, then, a federal system has opposing virtues and liabilities, each of which may be differently revealed under differing circumstances. On the one hand, as pluralists stress, multiple decisionmaking centers do improve access to government and offer alternative channels for the redress of grievances (Beer, 1978). At different times, and in different fields, the national government or the states have been the principal contributors of policy innovations. But, recent experience suggests that too much overlap in responsibility among the governmental levels can obscure channels of accountability and reduce popular control over policy decisions and outcomes.

Democratic theories of federalism provide little guidance, moreover, for determining where functions should be fixed, or which should be shared. Rather, there is a tendency to abstain entirely from normative judgments in support of whatever outcomes are ratified by the mechanisms of popular and legislative consent.

In contrast, political economic theories provide the most compelling framework for addressing questions concerning rationales for governmental action and the assignment of functions within a federal system. First, many of the problems addressed by contemporary governments are explicitly economic in character, and many of the others are at least partially susceptible to economic analysis (Amacher, Tollison, & Willett, 1976). Second, decisions about governmental services cannot be separated from questions about the availability of necessary fiscal resources: federalism has an essential revenue component as well as a servicing dimension (Musgrave, 1959; Nathan, 1975). Third, and most importantly from the perspective of this review, economic analysis offers the strongest policy prescriptions on the allocation of responsibilities among national, state, regional, and local institutions. While most political scientists have turned away from this issue, many political economists still regard it as the basic question to be resolved by a theory of federalism (Breton & Scott, 1978, p. 41).

On the other hand, this paradigm offers almost an embarrassment of riches, as evidenced by the competing prescriptions derived from the public choice and more traditional public finance perspectives. Furthermore, policy proposals that appear desirable from the standpoint of economic efficiency often turn out to be politically or administratively infeasible. Economic policy prescriptions need to be much more carefully validated if their potential power is to be realized. For example, some students of fiscal federalism have recognized that certain intergovernmental programs involve goal conflicts between governmental levels, leading to behavior quite different from that hypothesized by economic theory alone (Break, 1980; see also Ingram, 1977, and Porter, 1973).

There is, then, a pressing need for a more thorough understanding of the administration of intergovernmental programs and of the overall managerial implications of an extensively intergovernmentalized domestic policy system. Yet, neither of the two major administrative perspectives on intergovernmental relations—that is, theories of cooperative federalism and the implementation studies—provide a sound basis for generalization and policy prescription. As Peterson and Wong (1982) comment,

> . . . the new implementation theory is as undifferentiated as the old marble-cake theory. Where the optimistic view of Grodzins and his students found few areas of government activity where conjoint activity could not be undertaken, implementation theory reaches almost exactly the opposite conclusions. (p. 5)

What is required, as they argue, is a more differentiated theory of federalism. Research should attempt to identify the determinants of relatively successful and relatively unsuccessful outcomes.

One potentially useful focus involves the nature of program goals. For Elazar (1981), the major difference between the programs of the 1960s and those of an earlier period is that the latter were directed toward shared objectives, while the former were aimed at purposes established in Washington. The resulting tension has led to programmatic failures and steadily increasing federal coercion in an effort to secure compliance. These observations are consistent with some case study findings, which stress the dependence of federal program results on the state acceptance of national objectives (Edner, 1976) or the degree of local leadership and commitment (Greenwood, Mann, & McLaughlin, 1975). Peterson and Wong (1982) attempt to elaborate by suggesting that intergovernmental relationships will be more complex and conflictual where assistance programs are redistributive in intent. In contrast, federal and state objectives are likely to be shared in developmental programs, which are aimed at enhancing recipient jurisdiction's economic position. On this basis, they suggest that redistribution be a federal prerogative, while a good deal of developmental activity may be left to the states and localities.

A second promising strategy is more concerned with means than ends. The crucial point, as noted above, is a fuller recognition that the national government now depends very heavily upon state and local governments and a variety of other "third parties" for the execution of its domestic initiatives. For this reason, many administrative problems cannot be eliminated through such traditional panaceas as departmental reorganization, stronger executive leadership, or improved staffing. In many cases, what is needed may be more thoughtful program design. As Kirlin (1978) comments:

> Alternative policies usually exist for the pursuit of any policy objective. Thus, one can seek to reduce environmental pollution through regulation employing standards, enforced by administratively or court-determined sanctions, or a policy employing taxes upon effluents can be developed. As another example, the goal of reducing poverty can be pursued alternatively by policies employing cash transfers, manpower training, or attempts to redistribute political power. . . . Most importantly for present purposes, policy strategies constrain the choice of programs, thus largely determining implementation processes. And implementation processes, the relationships among governmental units and with clientele, structure the intergovernmental system. (p. 11)

The emergent literature on policy tools, instruments, and strategies has the potential of offering guidance on the appropriate use of a variety of specific subsidy and regulatory techniques.

From both perspectives, problems of management are viewed as integral with basic policy choices. The need to link these two areas, long separated by the hoary politics-administration dichotomy, is the clearest lesson emerging from the new implementation research. From the standpoint of federal theory, administrative perspectives should be tied to an updated understanding of democratic values and contemporary political processes.

Here, as in other areas, new scholarly efforts could help to articulate a theory of federalism that is both descriptive of and prescriptive for the present intergovernmental system. Although the heritage of dual federalism was set aside principally on pragmatic grounds, pragmatism now suggests that contemporary theories are inadequate from both normative and empirical standpoints. A new functional theory, merging democratic, economic, and administrative elements, is needed to provide the conceptual means of narrowing the gap between intergovernmental practice and the traditional principles of federalism.

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