

Overtilled and Undertilled Fields in American Politics

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Two questions about the research agendas of scholars who study American politics merit attention. First, what are the events, behaviors, and institutions that political scientists should endeavor to explain? Second, how should they allocate their collective scholarly resources among the various research tasks?

In his recent article in *Political Science Quarterly*, Nelson Polsby addresses the first of these questions.¹ His list of major transformations in contemporary American politics is insightful and comprehensive. There is no doubt that these “small and medium sized transformations” belong on scholars’ research agendas, presumably alongside the lesser variations, fluctuations, and incidents that keep them fully employed even in times of great stability.

This ever-growing research agenda, coupled with a stable supply of scholarly labor, makes it even more important to consider how best to deploy collective resources among the various chores to achieve greater understanding of American politics generally. The allocation of scholarly labor takes place in a delightfully free market; each social scientist studies whatever he or she chooses. But all must live with the consequences of everyone else’s decisions. What I know about American politics is largely determined by what others have chosen to study. Thus, although scholars can marvel at a system that gives each of them the freedom to manage their own lives, they cannot be indifferent to how others manage theirs.

¹ Nelson W. Polsby, “Contemporary Transformations of American Politics: Thoughts on the Research Agendas of Political Scientists,” *Political Science Quarterly* 96 (Winter 1981–82): 551–70.

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This article argues that scholarly manpower is poorly allocated among the many research tasks in American politics. Some plots are so overtilled that yields are diminishing and, in any event, productivity is low. Other plots have largely been abandoned, although they still offer great promise. Still others have never been well cultivated at all. Not only is labor poorly allocated, but so too are scholars' tools.

These comments about overtilled fields are not meant to be an indictment of the quality of research within these fields. In fact, I have intentionally chosen those where the achievements are demonstrable. Rather I raise two questions. First, have political scientists inadvertently allocated so much labor to these fields that the yields per scholar are abnormally low? Second, is labor now so concentrated that neighboring plots suffer shortages? These are, respectively, questions about labor productivity and opportunity costs.

The examples that follow reflect my own interests within American politics. They are drawn largely from fields where I read widely: congressional politics, national policymaking, elections, bureaucracy, interest groups, quantitative applications generally, and public-choice theory. Undoubtedly, there are similar allocational problems within and between such fields as the presidency, mass media, mass political behavior, public law, and state, local, and urban politics. On these I defer to others.

OVERCROWDED PLOTS

At least four plots seem to have attracted a surplus of scholarly labor. Research on the incumbency advantage in congressional elections is particularly crowded. A decade ago few scholars bothered to study congressional elections at all; presidential contests were the elections of choice. Congressional elections were presumed to be less interesting and their outcomes easily explainable in terms of party identification, presidential coattails, and some residual "local" factors. All this changed in the early 1970s when David Mayhew demonstrated that incumbents were becoming safer and safer, and offered a few plausible explanations for the shift.² This launched a wide-ranging search for evidence on the matter, a search that has consumed the energies of many. At least forty or fifty scholars have published or unpublished work on this question.

The results of this research have been interesting and important.³ On that there can be little doubt. But did this quest require the labor of such a large proportion of the scholars who study either Congress or elections? My judgment is that it did not. Most of the work in this area has been based on exactly the same data sets—those collected by the Center for Political Studies at Michigan, and

² David R. Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity* 6 (Spring 1974):295-317.

³ For a recent bibliography of work in this area, see Barbara Hinckley, *Congressional Elections* (Washington, D.C.: Congressional Quarterly Press, 1981), pp. 161-67.

particularly the 1978 survey that was designed explicitly to answer some puzzles about congressional elections.

A reasonable estimate is that 95 percent of the discoveries here would have been made with less than a third of the labor force. The rest made essentially redundant “discoveries.” The scholarly rewards, of course, are reserved for the nimble who get their findings into print first. The laggards contribute to the sense of *déjà vu* in the journals as they expound on their variations on an already well-known theme. Eventually someone cries out. “I am convinced,” Charles Jones has just written, “that one more article demonstrating that House incumbents tend to win reelection will induce a spontaneous primal scream among all congressional scholars across the nation.”⁴

If deciphering congressional elections were the only need, this oversupply of labor would not present a problem. The additional labor does, after all, produce some increment in knowledge, however small by comparison. Unfortunately, the oversupply on this plot diminishes the supply for neighboring fields where shortages now exist. The problem is one of opportunity costs.

A second plot that seems overcultivated is research on the effects of economic conditions on national elections. The story here is similar. A decade ago Gerald Kramer demonstrated what politicians had long known: that voters use the ballot box to reward and punish incumbent politicians for economic performance.⁵ This prompted a spate of research, by a labor force similar in size to that for the incumbency advantage, that has explored the basic relationship in great detail, for presidential and congressional elections, at individual and aggregate levels, and cross-nationally.⁶ The latest findings are not vastly different from the initial ones, but the extensions and slight modifications have been interesting and important. The redundancies here are probably less than they are for the incumbency effect, largely because scholars have not been restricted to a single data source. But they are not trivial. A reasonable estimate is that 95 percent of the discoveries here would have been made with less than half the labor force.

Other fields that seem overcrowded include issue voting in presidential elections and roll-call voting in Congress. Each tradition has a long history, longer than that for either the incumbency advantage or economics and elections. For each the law of diminishing marginal returns from labor has long since reduced the yield from additional entries.

One cannot in any way “prove” that these four substantive areas have at-

⁴ Charles O. Jones, “New Directions in U.S. Congressional Research,” *Legislative Studies Quarterly* 6 (August 1981):458.

⁵ Gerald H. Kramer, “Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964,” *American Political Science Review* 65 (March 1971):131–43.

⁶ For citations to recent work on the subject, see James H. Kuklinski and Darrell M. West, “Economic Expectations and Voting Behavior in United States House and Senate Elections,” and D. Roderick Kiewiet, “Policy-Oriented Voting in Response to Economic Issues,” both in the *American Political Science Review* 75 (June 1981): 436–59.

tracted an oversupply of scholarly labor. It is merely an impression. It is, however, an impression shared by others, both those within these four fields and those laboring in neighboring fields. It is an impression reinforced by the flow of conference papers across my desk and by a casual examination of journal articles. A quick study of the last eight issues of four leading political science journals reveals that nearly a quarter (22 percent) of all articles on American politics focused centrally on these four narrow substantive areas. The percentages by area were: incumbency advantage (8 percent), economics and elections (6 percent), issue voting (5 percent), and roll-call voting in Congress (3 percent). By journal they were: *American Political Science Review* (33 percent), *American Journal of Political Science* (28 percent), *Journal of Politics* (19 percent), and *Political Science Quarterly* (zero).⁷ This level of activity does not suggest editorial bias. Rather it reflects well what scholars have chosen to study.

ABANDONED PLOTS

Scholars have practically abandoned at least two plots: the committee study and the case study. The study of committees was once a central feature of congressional research. Richard Fenno, John Manley, and David Price led the way with their intensive studies of House and Senate committees in the fifties and sixties.⁸ Since then, this plot has been left fallow. While committees themselves have been changing drastically, with reform of the seniority system, multiplication of subcommittees, decline of committee chairmen's power, and rise of subcommittee power, scholarship has been very quiet. No one has produced either an in-depth study of a single committee, or, more valuably, a comparative study of decision making across a collection of committees. What is known about committees in the 1970s is largely the incidental consequence of studies of reform, foreign policy, the budget process, or individual legislators.⁹ Additionally, there are a few article-length pieces about individual committees.¹⁰

⁷ The criterion for selection was simple: these are the four general journals that I receive regularly. For each I examined two complete years, beginning with the following issues: *American Political Science Review* (September 1979), *American Journal of Political Science* (November 1979), *Journal of Politics* (August 1979), and *Political Science Quarterly* (Fall 1979). I have counted only those articles that focus specifically on these four subjects.

⁸ Richard F. Fenno, Jr., *The Power of the Purse* (Boston, Mass.: Little, Brown and Co., 1966), and idem, *Congressmen in Committees* (Boston, Mass.: Little, Brown and Co., 1973); John F. Manley, *The Politics of Finance* (Boston, Mass.: Little, Brown and Co., 1970); and David E. Price, *Who Makes the Laws?* (Cambridge, Mass.: Schenkman, 1972).

⁹ See Norman J. Ornstein and David W. Rohde, "Shifting Forces, Changing Rules, and Political Outcomes," in *New Perspectives on the House of Representatives*, eds. Robert L. Peabody and Nelson W. Polsby, 3d ed. (Chicago, Ill.: Rand McNally, 1977); Thomas M. Franck and Edward Weisband, *Foreign Policy by Congress* (New York: Oxford University Press, 1979); Joel Havemann, *Congress and the Budget* (Bloomington: Indiana University Press, 1978), and Allen Schick, *Congress and Money* (Washington, D.C.: Urban Institute, 1980); and Bernard Asbell, *The Senate Nobody Knows* (New York: Doubleday and Co., 1978).

¹⁰ The very best is David E. Price, "Policy Making in Congressional Committees," *American Political Science Review* 72 (June 1978): 548-74.

Why should one mourn the abandonment of the committee study? Committees continue to be the place where most major policy decisions are made, subject to ratification elsewhere. They are, therefore, the place to view interest groups, bureaucrats, and presidents at work, to see legislators balancing conflicting goals and pressures, and to find leaders shaping legislation in anticipation of problems on the floor. They are the very heart of the policymaking process, as they have been since at least the time of Woodrow Wilson. Their structures and procedures may have changed, but their centrality to the legislative process remains. The discipline urgently needs new studies, not only of those committees that others explored a decade or more ago, but also of the major committees that have yet to be subjected to scholarly scrutiny.

The case study was also once a staple of congressional research. Stephen Bailey's study of the Employment Act of 1946 and Bauer, Pool, and Dexter's study of foreign trade legislation are still classics of this genre.¹¹ The purpose of the case study was to follow a bill through the legislative labyrinth, to see how it evolved, to examine how various participants worked to shape it, and to explain the final product. Its value for an understanding of Congress was similar to that of biography for an understanding of history. But political scientists have largely abandoned the writing of case studies to journalists, students, or the participants themselves.¹²

If case studies are thought of as purely descriptive accounts of the legislative process, then they clearly are not worthy of scholarly labor. Political scientists have no comparative advantage over journalists, students, or participants when it comes to narrative. But case studies can (and should) be something more. They should be analytic and designed in a way that either tests theories about legislative policymaking or contributes to their further development.

There are various approaches to the production of quality case studies. Among the most valuable are the comparative studies of how Congress has handled several policy issues during a common period. James Sundquist's analysis of the evolution of seven major issues during the 1950s and 1960s is a sterling example. So too is David Price's more structured analysis of thirteen bills in the Eighty-ninth Congress which uses a common framework to explore the roles of legislators and the executive in various legislative tasks.¹³ There have been no such comparative studies covering any period since 1966.¹⁴

¹¹ Stephen K. Bailey, *Congress Makes a Law* (New York: Columbia University Press, 1951); and Raymond A. Bauer, Ithiel de Sola Pool, and Lewis Anthony Dexter, *American Business and Public Policy* (Chicago, Ill.: Aldine Atherton, 1963).

¹² See, for example, T. R. Reid, *Congressional Odyssey* (San Francisco, Calif.: W. H. Freeman, 1980); David J. Garrow, *Protest at Selma: Martin Luther King, Jr., and the Voting Rights Act of 1965* (New Haven, Conn.: Yale University Press, 1978); and Eric Redman, *The Dance of Legislation* (New York: Simon and Schuster, 1973).

¹³ James L. Sundquist, *Politics and Policy* (Washington, D.C.: Brookings Institution, 1968); and Price, *Who Makes the Laws?*

¹⁴ There is A. James Reichley's *Conservatives in an Age of Change* (Washington, D.C.: Brookings Institution, 1981), with its case studies from the Nixon and Ford administrations. But this is largely executive-centered and concerned only with tracing the impact of ideology on policymaking.

A second approach is to follow a single policy issue through time. Here the objective is to determine how institutions change, how specific policies evolve, and how the two are interrelated. Martha Derthick's superb analysis of fifty years of social security policy sets the standard here. Robert Pastor's book on Congress and foreign economic policy during the same period provides a second example.¹⁵ There are, then, models of how to proceed, but few who have chosen to follow. The study of American politics would be much the richer if there were at least a half dozen more such as these, for example, on taxation, fiscal policy, energy, the environment, urban redevelopment, and social welfare policy.

A third approach is to examine a single policy from different analytic perspectives. John Mendeloff provides a model here with his combination of economic and political analyses of occupational safety policy.¹⁶ The former demonstrates why economists are so enamored with certain policy options; the latter explains why politicians flee those same options for more politically attractive ones. The juxtaposition of the two illustrates well why expert advice often plays a peripheral role in the legislative process.

The flow of committee studies and case studies has slowed to a trickle in the past decade. The tragedy is that these are precisely the research projects that show best how Congress shapes policy decisions, how it competes with the executive, and how legislators respond to the diverse pressures from constituents, interest groups, and each other.

Throughout the 1950s and early 1960s, Congress struggled with civil rights, education, and Medicare, plus a host of lesser issues. Dozens of scholars labored mightily doing their committee and case studies, so that today others understand well what political forces initially produced inaction and later delivered great legislative victories that together dramatically changed the federal government's role in American society. Energy and the economy were the dominant domestic issues of the 1970s. Yet there are neither committee nor case studies to help one understand these vastly more complex issues. Policy analysts have filled library shelves with studies of what energy policies the United States should adopt; political scientists have generated little to explain actual congressional actions (and inactions) on these matters. The same is true for economic policy. Surely these two policy areas deserve more than the passing attention of a few scholars. Are the issues here not at least as important as why incumbents are a bit safer today than yesterday?

UNSETTLED PLOTS

There are various important plots that few scholars have ever tilled. Congressional nominations and primaries, for example, have attracted little attention.¹⁷

¹⁵ Martha Derthick, *Policy Making for Social Security* (Washington, D.C.: Brookings Institution, 1979); and Robert A. Pastor, *Congress and the Politics of U.S. Foreign Economic Policy, 1929-1976* (Berkeley, Calif.: University of California Press, 1980).

¹⁶ John Mendeloff, *Regulating Safety* (Cambridge, Mass.: MIT Press, 1979).

¹⁷ One still must rely on Leo M. Snowiss, "Congressional Recruitment and Representation," *American Political Science Review* 60 (September 1966): 627-39.

It is all very curious that political scientists have devoted so much to the study of citizens' final choices between the two candidates who make it to the general election and so little to the study of how the thousands of potential candidates are winnowed down to two. The irony deepens with the recent discovery by students of congressional elections that one reason incumbents are so strong is that challengers are so weak.¹⁸ Unfortunately, no one knows much about the recruitment of these challengers or why some are so much weaker than others.

Similarly, there are few scholars who have attempted to show how election outcomes affect policy outcomes. Most do believe that elections matter, but the general preoccupation with explaining electoral outcomes has not contributed to an understanding of their broader effects on policy and government performance. There is, of course, the vast literature on comparative state policymaking that purports to show how unimportant elections are for explaining policy outcomes across the states.¹⁹ But this correlational approach, which replaces political institutions with a black box, is not very illuminating. What is needed are careful studies about how elections affect the composition of legislatures and the choice of executives, how elections also influence the way politicians think about policy, and how these two in turn affect the way politicians actually make policy. David Mayhew has offered an elegant theoretical argument that makes some of the connections for Congress. Edward Tufte has provided an empirical analysis of how politicians manipulate economic policy for electoral gain.²⁰ There are also various studies of the frequency with which parties keep their preelection promises²¹ and several cross-national studies that demonstrate the policy consequences of various electoral outcomes.²² Despite the quality of some of these entries, they represent a meager investment in what should be a thriving area of research. The question of how much elections really matter is central to an understanding of democracy.

Interest groups also seem to have attracted relatively little scholarly attention given their presumed importance. Here, surprisingly, the field is theory rich and data poor. Mancur Olson and James Q. Wilson, among others, provide a variety of theoretical perspectives.²³ But there are relatively few empirical studies of interest groups as organizations or comprehensive studies of how various groups operate politically.

¹⁸ Thomas E. Mann and Raymond E. Wolfinger, "Candidates and Parties in Congressional Elections," *American Political Science Review* 74 (September 1980): 617-32.

¹⁹ For citations to this vast literature, see Edward T. Jennings, "Competition, Constituencies, and Welfare Policies in American States," *American Political Science Review* 73 (June 1979):414-29.

²⁰ David R. Mayhew, *Congress: The Electoral Connection* (New Haven, Conn.: Yale University Press, 1974); and Edward R. Tufte, *Political Control of the Economy* (Princeton, N.J.: Princeton University Press, 1978).

²¹ See, for example, Gerald R. Pomper, *Elections in America* (New York: Dodd, Mead and Co., 1968).

²² Douglas Hibbs, Jr., "Political Parties and Macroeconomic Policy," *American Political Science Review* 71 (December 1977): 1467-87; and David R. Cameron, "The Expansion of the Public Economy," *ibid.* 72 (December 1978): 1243-61.

²³ Mancur Olson, Jr., *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965); and James Q. Wilson, *Political Organizations* (New York: Basic Books, 1973).

Finally, relatively little is known about how politicians perceive their world and how they reason about politics and policy. Actually, social scientists may know more about how little children see their simple worlds than about how legislators, presidents, or bureaucrats see their vastly more complex worlds. In congressional studies there are but two studies that emphasize politicians' perceptions: John Kingdon's interviews with members of Congress immediately after they had made difficult voting decisions, and Richard Fenno's intensive study of congressmen in their districts.²⁴ Both are invaluable. Given that institutions are, in part, products of the people within them, however, scholars should do more. Students of Congress need, for example, theoretically informed studies about how legislators think about economics, energy, the environment, and other such issues on which they make judgments daily—not merely how they react to pressure on these issues, but how they think in the absence of overpowering pressures.

THE NATURE OF SOCIAL SCIENCE

Not everyone would agree that there is an oversupply of labor in some fields and an undersupply in others. The argument is sometimes made that science advances best when labor is heavily concentrated on a few crucial research tasks because this maximizes discussion, interaction, and the probabilities of innovation. Then, when these research tasks are accomplished, scholarly labor can move on (in mass) to solve the next problem. This notion of science as a series of sequential problems is very powerful in the natural sciences. Modern advances in biochemistry, for example, show exactly these patterns.²⁵

Although natural science thrives on this model, social scientists should be wary about adopting it for themselves. The work of natural scientists is simplified by the knowledge that the basic building blocks they study—elements, particles, molecules, chemical reactions—do not themselves change overnight. Deferring the examination of some protein for a few years, while everyone solves some other problem, does not diminish the probability of eventually understanding that protein. Social scientists do not enjoy this same freedom. They have no basic building blocks that remain constant over time; the very political and social institutions they study are in constant flux. Imagine how distorted the view of American politics would be if political scientists proceeded sequentially, studying interest groups in the 1940s, congressional elections in the 1950s, congressional committees in the 1960s, presidential influence in the 1970s, and economic policymaking in the 1980s.

The lesson is simple: the study of political science incurs substantial costs

²⁴ John W. Kingdon, *Congressmen's Voting Decisions* (New York: Harper & Row, 1973); and Richard F. Fenno, Jr., *Home Style: House Members in Their Districts* (Boston, Mass.: Little, Brown and Co., 1978).

²⁵ See Horace F. Judson, *The Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979).

when scholarly labor is poorly allocated. The absence of much research on congressional elections throughout the 1960s has hindered recent research efforts on the incumbency advantage. Political scientists know a great deal about elections in the 1950s and 1970s, but very little about them in the 1960s—exactly the period during which incumbents developed their safer seats. Soon scholars will pay the price for their inattention to congressional committees in the 1970s. None of these lost opportunities can ever be reclaimed. Researchers cannot return to the 1960s to interview voters in congressional elections or to the 1970s to interview committee members. The best scholars can do is hope that they do not repeat their mistakes.

The Choice of Tools

Scholars bring to their work different collections of tools. Some are more adept at statistical analysis, others at interviewing elites. Some know instinctively the art of writing questions for survey research, others excel at finding patterns in mounds of data. Some construct theory inductively, others deductively. All scholars profit from the diversity of tools brought to bear on their subjects.

Regrettably, some tools are poorly deployed. Statistical analyses, for example, are largely concentrated in a few subfields. They are used to study voting in every setting: within legislatures, committees, courts, party conventions, primaries, general elections, and international assemblies. They are used to explore the attitudes and opinions of children, voters, and elites of every variety. They are used to study government spending at every level—local, state, and national.

Statistical tools are used only sporadically in other subfields of American politics. Few studies of bureaucratic decision making, for example, rely on them, yet the setting is near perfect. Bureaucrats make decisions repetitively on a host of similar items, all in an environment structured by law, hierarchy, and often stable relations with legislators and interest groups. These lush gardens are potentially more productive for statistical analysis than are other areas where such tools are routinely applied. But few have even tried.²⁶

The same is true for studies of the independent regulatory commissions. Their unique combination of executive, legislative, and judicial functions, their large case loads of similar disputes, and their careful documentation and record keeping make for a paradise of ripe quantitative data. The best studies here, however, continue to rely on interviews—*asking* officials how they decide rather than carefully modeling the decision-making process.²⁷

²⁶ See R. Douglas Arnold, *Congress and the Bureaucracy: A Theory of Influence* (New Haven, Conn.: Yale University Press, 1979).

²⁷ See, for example, James Q. Wilson, ed., *The Politics of Regulation* (New York: Basic Books, 1980); and Paul J. Quirk, *Industry Influence in Federal Regulatory Agencies* (Princeton, N.J.: Princeton University Press, 1981). One study that does perform statistical analyses of such decisions is Richard P. Barke, "Political and Economic Determinants of ICC Decisions" (Paper presented at the Annual Meeting of the Midwest Political Science Association, Cincinnati, Ohio, April 1981).

Case studies could also benefit from a small dose of statistical analysis. Invariably one must explain the outcome of some crucial legislative vote along the way, separating out the effects of party, ideology, and constituency. Statistical analysis would do this nicely. Most scholars use more impressionistic means, highlighting how champions of various ideological causes or special interests voted, or sifting through debate for clues. Thus, there is the curious spectacle of a large literature on roll-call voting, all done with elaborate scaling of multiple votes taken out of context, and a much smaller case-study literature, where context is preserved but with only primitive methods to unlock the secrets of crucial votes.

Theory Construction

Social scientists must not be mere chroniclers of political events. They must seek to generalize and to construct theories of political institutions and behavior. This need not transform all social scientists into political theorists. But it does imply that there should be a constant dialogue between empirical research and theory construction. Empiricists need to design their research either to test theory or to contribute to its further development. Theorists need to create testable propositions about interesting political phenomena and to watch carefully empiricists' discoveries. Such dialogues do not always take place.

Public-choice theory, for example, often seems to have a life of its own. The approach, with its emphasis on modeling the behavior of rational, goal-seeking individuals in political institutions, offers great promise for understanding politics. Unfortunately, the past two decades of research in this tradition have produced only modest dividends. The problem stems from a lack of communications between practitioners of this craft and empiricists who actually know the institutions that theorists seek to model. Forcing such a dialogue is no easy task. One recommendation is for public-choice theorists to sacrifice small portions of elegance and abstraction in order to tailor their theories to produce testable propositions about questions that empiricists find intrinsically interesting. A second is for more scholars to bring theory construction and testing together in the same enterprise, so that the needs of one are quickly addressed by the strengths of the other. Some of the most innovative research has actually come from such close collaboration.²⁸

THE SUPPLY AND DEMAND FOR SCHOLARLY LABOR

How can one account for the maldistribution of labor across the various research tasks in American politics? At one level the answer is easy. Since in-

²⁸ See Kenneth A. Shepsle, *The Giant Jigsaw Puzzle: Democratic Committee Assignments in the Modern House* (Chicago, Ill.: University of Chicago Press, 1978); and Morris P. Fiorina, *Retrospective Voting in American National Elections* (New Haven, Conn.: Yale University Press, 1981), for two excellent examples.

dividuals are free to study what they like, there must be a poor fit between the preferences of individual scholars and the collective needs for a balanced research agenda. That merely redefines the problem, however; it is necessary to account for those preferences. Here I argue that these preferences are, in part, a product of the peculiar incentive system that has developed in political science.

Scholars generally study what interests them. Their interests, of course, are not constants; they shift in response to the findings of others. When scholars make new discoveries, expose contradictions in the literature, or establish paradoxes, they create opportunities for others to move in and debunk the discoveries, explain the contradictions, or solve the apparent paradoxes. This is surely what happened with the incumbency advantage, economics and elections, and issue voting. Overcrowded turf results when too many heed the call for further research. As individuals, their responses are highly rational; only the collective results are not.

Research projects can just as easily go out of fashion. Once the prefix “just” becomes attached to any form of scholarship, the game is over. No serious scholar wants to be known as the producer of “just another case study.” The label is highly appropriate for purely descriptive case studies, and the effects laudatory. Unfortunately, the same label drives people away from doing the theoretically interesting case studies.

Next are the effects of subsidies. Some forms of scholarship receive large automatic subsidies that defray most research costs. Others receive little or nothing. Economists have long argued that subsidized (and hence underpriced) goods will be overconsumed, while unsubsidized (and relatively overpriced) goods will be shunned. Economists will no doubt be pleased to learn that political scientists behave as their theories predict! Subsidized forms of scholarship are relatively overcrowded; unsubsidized forms are abandoned or sparsely populated.

The subsidies and their effects are most obvious for research on elections. Survey research is, in the first instance, very expensive, and someone must pay the bill. Once paid, however, it becomes very inexpensive to duplicate the data files and distribute them to interested scholars. Many universities pay for the automatic acquisition of such data through the Inter-University Consortium and then provide free computer time for analyzing them. These are part of the overhead costs for a modern university. From the point of view of the individual scholar, then, doing research on elections is costless. Complete and automatic subsidization eliminates the need for either grants or the investment of personal resources. Small wonder that so many scholars are doing research on the incumbency advantage, economics and elections, or issue voting. It is free.

Compare this with the direct costs of doing case studies, committee studies, or research on interest groups. All require that one travel to Washington and maintain oneself for extended periods of observation and interviewing. Few universities subsidize such costs completely, and most provide nothing. Ordinarily one must raise such funds from foundations or personal sources. The decline of these studies, given the constraints, is really no surprise at all.

The contrast is most striking in the literature on roll-call voting in Congress. The literature is vast, consisting mostly of statistical analyses of collections of roll-call votes. Typically, scholars attempt to measure the impact of party, ideology, constituency, and the like, and to infer how legislators must have made their decisions. The size of the literature is to be expected, given that the data are collected by Congressional Quarterly, distributed by the Inter-University Consortium, and analyzed on university computers—all three services provided free at most universities. John Kingdon's study stands alone, both for its innovative methodology (interviewing members of Congress immediately after they made difficult voting decisions) and the quality of its execution.²⁹ The advantage of this approach is that the researcher can see exactly how each legislator perceived both the issues and the positions of various interested parties, rather than having to infer all this from fuzzy voting patterns. The cost of collecting such data, both in time and money, explains the absence of companion studies.

It is somewhat more difficult to explain the concentration of statistical analyses in only three subfields: voting, opinion research, and government spending. One begins with the fact that these were the first areas where statistical methods were used. It may simply have been chance. Once established, however, such patterns can affect how future scholars are recruited. Those with quantitative aptitude gravitate to fields where their skills are seemingly in demand and avoid such traditional fields as public administration where the new techniques seem inappropriate. Moreover, there is the subsidy argument. Quantitative data for these three fields are readily available at low cost, whereas identifying, collecting, and organizing data from bureaucratic files is costly and risky. Third, many scholars appear to believe that multivariate analysis requires an interval (or at best, ordinal) dependent variable. Thus, one cannot analyze a single roll-call vote (as in a case study), but rather must scale many votes to produce an appropriate dependent variable. Similarly, one cannot analyze simple dichotomous bureaucratic decisions, but rather must either combine many such decisions or restrict the analysis to governmental expenditures that occur naturally as interval data. This is false. Probit analysis and other techniques provide all the multivariate tools required to handle these cases, much as regression does for interval data.

REMEDIES

No simple remedies for the misallocation of scholarly manpower appear on the horizon. Eliminating the subsidies for certain fields is *not* the answer. There can be no guarantee that killing off that goose will produce golden eggs elsewhere. Moreover these have been extraordinarily productive sectors, and I have no wish to end that productivity. My aim is to entice surplus labor from those fields, not to close them down.

²⁹ Kingdon, *Congressmen's Voting Decisions*.

The simplest remedy is to raise consciousness about the misallocation and hope for the best. Fortunately, the profession rewards those who stake their claims in new productive territory. Thus, the incentives are there for scholars to reclaim abandoned plots or settle on new ones, once they see the gold they have been overlooking.

But there is still the matter of resources. Gathering new types of quantitative data or conducting intensive interviews in Washington are expensive propositions. Without adequate funds no reallocation of labor can take place, no matter how much one may hope. Presumably, that means scholars also must raise consciousness among those who dispense scarce resources—universities, foundations, and the Washington-based research institutes. What is needed is a large welcome mat to encourage proposals from abandoned or unsettled plots, not merely a grudging acceptance of outstanding unsolicited proposals. The competition, after all, is plenty of free data on magnetic tape that scholars can analyze without ever applying for a grant, spending a dime, or leaving the comfort of the university.