

The Role of Research in the Unification of a Discipline

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Large-scale complex research project designs are providing a new impetus to the elimination of sub-field boundaries within political science. Major projects are taking advantage of the methodology and technology of contemporary social research to include comparisons among institutions, across cultural boundaries, and extending through time. As a consequence, traditionally narrow field and sub-field concentrations on segments of the political process are giving way to intellectual interests that bring together hitherto separate concerns. The full potential for discipline-unifying research will, however, not be realized until there is a strengthening of the organizational infrastructures for research, a broadening of training in research design and administration, and an increase in funding for large-scale projects. The execution and subsequent intellectual exploitation of large research projects will carry additional problems that will be solved only with substantial changes in the workways of the political scientist, but those problems are greatly outweighed by the positive contribution that such research will make to the future of the discipline.

The growing capacity of American scholars to study politics and governance systematically rather than segmentally has brought us to the beginning of a new era. We should contemplate the future, understanding that political science can and should become a unified discipline. Methodological differences no longer define nor are defined by substantive interests. Many of the boundaries between traditional subfields are sustained only by the inertia of custom; the boundaries can and should disappear. The intellectual challenges of the discipline are increasingly, and appropriately, centered on complex interrelationships that are being explored in major research endeavors. And, because inexorable trends in all of the social sciences, as well as in national science policy, are fostering large-scale research that purposively brings many interrelated interests together in single projects, I believe political science should prepare for a new era in which large-scale, programmatic research activity will give a new and more coherent shape to the discipline.

The historically separate origins of the various subfields of the discipline are now less relevant than the intersections that today connect the subfields. Drawing from my own field of electoral behavior, one can ask how, in a day of presumed single-issue politics, the study of electoral behavior can be intrinsically separated from the study of bureaucratic clienteles that help shape the public policies that are reviewed by citizens engaged in retrospective, issue-oriented voting? How can judicially mandated educational policy be understood or appreciated with no regard for the politically potent demands of affected voters? How can the ebb and flow of foreign policy be

comprehended without a basic recognition of the bounds by which public outrage defines the permissible? How can the performance of an office of personnel management be appraised without regard for the electorally inspired concern for civil rights?

More generally, it seems to me there are two logical imperatives responsible for the movement away from the specialization and compartmentalization of intellectual activities that has characterized political science. One of the continuing impulses toward unification of the discipline comes from the very nature of our subject matter. Political science is unique among the social science disciplines in its preoccupation with the institutions of politics and government, their interaction, and the relationships between the institutions and the citizenry. The very concepts of political system, political process or governmental structure properly imply aggregations of activities that are by their nature interconnected and interdependent. The separateness of subfields which have focused on various dimensions and components of the political process has existed despite, and not because of, the interconnectedness of the phenomena around which subfields have been organized.

A second impetus toward disciplinary unification comes more recently from the appreciation and implementation of the basic logic of social inquiry. The development of new approaches to social research, relevant to many problems of political analysis, has diminished subfield parochialism in research and promoted research that is responsive to the interconnectedness of the phenomena we study. In the absence of a practicable mode of carrying out research on large-scale, complex in-

stitutions and widely dispersed, mass populations in laboratory settings, the systematic analysis of political phenomena has self-consciously turned to two different modes for testing those parts of political theory that have an empirical as well as a normative referent. The more familiar mode is comparative, searching across more or less similar institutions for insight into that which makes them different despite their similarities.

The second mode, with an equally long tradition, is that of diachronic analysis in which the passage of time (or the accumulation of history, if properly documented) produces change in individual institutions and in the interaction among institutions. The understanding of such change through time complements synchronic analysis of institutions across cultural space. Both modes direct attention to the context provided by functional interrelationships and thereby force a broadening of perspective and analytic grasp; description of the discrete, bounded institution ultimately must give way to an expanded scope of inquiry.

The increasing unification of our discipline as a scientific enterprise has been made possible largely because of the development of new methods of social research. In particular, the post-World War II growth of methods and techniques, often associated with the behavioral mode, has been vitally important, less because of the intellectual ferment surrounding questions of epistemology and more because those methods and techniques have increased our powers of observation. The techniques of contemporary social research have expanded the scope and refined the accuracy of our observations of the real worlds of government and politics. They have made it possible to create evidence that is needed to establish empirical regularities and to test the explanatory theories that transform those regularities from intellectual trivia into matters of important intellectual concern.

In the absence of our generation's research methods, the older, traditional empirical components of political science were derived almost entirely from "process produced" information. Apart from personal memoirs and collections of anecdotes, the evidence on which the discipline's traditional research rested was created for the purposes of the participants in the political process. The study of comparative politics, for example, was largely limited to the textual examination of constitutions, legal codes, tables of administrative organization, financial reports of budgets and expenditures, and tabulations of aggregated election returns. These "hard data" were not developed to serve the purposes of the research scholar, but to facilitate the functioning of this or that institution. Even the discursive textual materials, the

treatises that articulate political philosophy or the narratives that describe political experience, were usually produced by self-interested participants whose rhetoric was intended to serve personal or political, value-laden ends rather than the needs of the scholar.

With the development of the new modes of social research, the scientist-scholar has been freed from a dependence on information that is created for the participants in the political process. We can now elicit additional information and thereby create relevant evidence for the relatively precise and very specific needs of systematic inquiry. The development of sampling theory and the methods of applying it to very large human populations, including populations of organizations, make possible an efficiency in data collection that increases the researcher's observational power by many magnitudes. The structured personal interview and the questionnaire provide vehicles for highly standardized and highly comparable descriptions of personal experiences, perceptions, attitudes and behaviors. These new methods and techniques of eliciting information, driven by hunches, curiosities, perceived anomalies of explanation, or, formal theory, and controlled by increasingly sophisticated elaborations of classical experimental design, make it possible to create new knowledge at a rate that challenges our capacities to think new thoughts and achieve new levels of understanding.

It is true that the initial application of the new methods proceeded very unevenly. Following the so-called law of "the child and the hammer," new research tools have often been used rather indiscriminately, and with too little recognition of their lack of relevance for some of the intellectual constructions that scholars have envisioned. At the same time, the invention of new tools for research has also led many of our most creative scholars into a sustained preoccupation with the assessment of existing methods and the invention of still more new methods. From one perspective, these pre-occupations with methodology have been criticized as a diversion at the expense of a continuing interest in the substantive problems which should impel and justify methodological innovation. It is undeniably true that methodological domains often acquire a life of their own, and the continued growth of these activities is still very much with us. However, very often methodological developments, particularly in domains concerned with data analysis, are stimulated by the methodologists' recognition of the inadequacies of the data available for scientific use.

It should also be noted that the use of the new tools of research has often led to the posing of new substantive questions—often questions quite remote from the immediate study of political in-

stitutions. The sheer ability to study individual behavior more directly, systematically, and comprehensively has led to an emphasis on theoretical problems confronting our understanding of individual behavior. Indeed, for a time, micro-analytic concerns threatened to supplant macro-level, institutional analysis. More recently, and more happily, our abilities to investigate at both the micro and macro levels have been recognized in the design of research that is more faithful to the real world of people and institutions. This is vitally important for the ultimate unification of the discipline because it emphasizes the relevance of empirical, social science for the central questions of both the normative and empirical theories of the discipline.

The last two decades have witnessed the production of a number of prototypic studies that, taken one by one, have added to our understanding of political phenomena and, taken collectively, demonstrate our intellectual and methodological capacity to carry out research projects of substantially greater power and broader relevance than those of earlier years. These are projects that, in scope of complexity, at least start to match the scope and complexity of the real world of politics and government. Moreover, these studies demonstrate that it is possible to do scientifically rigorous work that is sufficiently complex to be commensurate with the intricate problems we are trying to understand or solve.

For example, there is now a long catalog of studies that, by design, embrace two or more different sets of political actors participating in the patterned behavior of two or more political institutions. These studies can be illustrated by Herbert McClosky's pioneering investigation of agreements and disagreements between delegates to nominating conventions and their parties' rank-and-file constituents (McClosky, Hoffman and O'Hara, 1960). The same era produced the first study of the direct linkages between members of a national legislature and their constituencies (Stokes and Miller, 1962; Miller and Stokes, 1963), and the ensuing years have seen this basic design replicated many times over. More recently in the field of electoral behavior, studies have been designed to bring together in integrated analyses the confluence of decisions of candidates and their campaign managers (Goldenberg and Traugott, 1980), political reporters who present the campaigns through the mass media (Clarke and Evans, 1980a, 1980b; Evans and Clarke, forthcoming), and the voters who are responding to the election campaigns as presented by the media (Center for Political Studies, 1978).

Because of the larger problems of logistics and culture, truly comparative synchronic studies of similar political institutions are fewer in number.

Nevertheless, the study of comparative politics was clearly reshaped by the work of Gabriel Almond and Sidney Verba (1963) in their path-breaking study of civic cultures. The fact that comparative studies need not go to the extreme of crossing national lines was demonstrated early in the 1960s by the work of Wahlke, Eulau, Buchanan, and Ferguson (1957) in their interstate study of legislative systems.

Diachronic studies that exploit the passage of time are more numerous. In latter days they are better supported than ever before because of the creation and maintenance of data bases that extend through ever increasing spans of time. Here one can hope that the emergence of new communities of scholars, such as those identified with the Social Science History Association, will add interdisciplinary richness to the task of unifying our study of politics and government.

Many studies have come into being that combine all three themes of inter-institutional, cross-cultural, and longitudinal analysis. As with a number of the earlier studies, the newer studies also foretell an important aspect of the future in their presentation of the results of the active cooperation of a number of scholars as co-investigators and coauthors for each project. The most inclusive of such studies now under way is exemplified by a cross-national, intergenerational, longitudinal study of political participation in eight Western democracies, carried out by a dozen collaborating colleagues under the general leadership of Samuel H. Barnes, M. Kent Jennings and Max Kaase (Barnes, Kaase et al., 1979).

In the same spirit, with varying emphases on the interinstitutional, cross-cultural and longitudinal themes, one can note similar work under way or completed in the Verba, Nie, and Kim international study of mass-elite linkages in local government (1971), the studies of parliaments being undertaken by the Comparative Legislative Research Center at the University of Iowa (Kim et al., forthcoming; Loewenberg et al., forthcoming), the Converse and Pierce study of representation in the French National Assembly (1971, 1979; Converse, 1975), the Cotter, Bibby, Gibson, and Huckshorn study of American state political party organizations (Cotter and Bibby, 1980; Cotter et al., 1980), or the Cain, Ferejohn, and Fiorina comparative study of the American Congress and the British House of Commons (1979a, 1979b, 1980a, 1980b).

Perhaps it is no more than coincidence, but our demonstrated ability to carry out very complex research designs that produce relatively faithful mappings of the interrelated elements of the real world is occurring at the same time that new intellectual foci promise a further integration of political scientists' concerns. Both the new emphases

on policy analysis and on political economy seem to me to demand great intra-disciplinary integration. Indeed, much of the nonideological excitement in both developments comes from the new scope they bring to the work of those doing research as well as those committed to action and application. The concept of the political process as a continuing process fits very neatly the theme of studying persistent, normatively important, social or economic problems. When problem-solving policies are examined in the context of the social and the economic, as well as the political structure of a policy, the need to know more and more about more and more becomes pervasive.

Although the time seems ripe, there are a number of obstacles to overcome before political science will come of age as a relatively unified social science discipline. If disciplinary scholars are to be provided with the information and evidence that is needed for major advances in our understanding of politics and government, institutional changes within the discipline will have to occur on at least three different fronts. As things now stand, political science as a discipline cannot respond to its intellectual challenges, or take advantage of the opportunities that those challenges present, because of inadequate personnel, inappropriate institutional arrangements for meaningful research, and limited financial resources with which to carry out truly significant research.

The prime challenge presented by our new capacities and the new potential lies in the fact that their exploitation rests on individual scholars. My remaining comments center largely on problems associated with the institutionalization of complex, large-scale research, but they are pertinent only if such research is guided by the concerns of intellectually creative individuals. The significant contributions of the research that is reshaping our discipline all have their origins in single minds and the capacities of single individuals. Their unique contributions are too often lost as credit is distributed among all of those individuals and organizations required for the execution of a major research project.

The point must be made, and is here recognized, that the scope of large-scale research does not limit, but enhances the importance of the individual scholar. In each subfield the creativity of numerous individual scholars could be identified as the ultimate source of our new knowledge. And it is not the direct participation of these individuals in large-scale social research projects that is crucial; but without their guiding insights and their synthesizing wisdom, our new methodological and organizational capacities would not have remade those portions of the discipline to which their work has contributed. The impact of the research of the future will likewise depend at least as

much on the ability of individual scholars to utilize and exploit our new research capacities as it will on the continued and expanded existence of the institutions which provide them.

Although political science training relevant to large-scale social inquiries is infinitely better today than it was two decades ago, it still has major deficiencies. First of all, too few political scientists are trained in the tasks of designing research and collecting data. Whether to take advantage of those new data that have been produced, or to overcome the limitations in those data, our training in empirical studies has emphasized data analysis and data manipulation rather than the generation of new data. Too few political scientists have first-hand experience in designing research that will faithfully reflect the real political world. Numerical theory, and data analysis, will always be vital, but their exploitation must be complemented by expertise in research design and in the conduct of complex, multipurpose data collections rooted in familiarity with politics in action.

There are also too few political scientists who are professionally trained for multilingual, cross-cultural research. Although there has been a certain logic to treating research methods and statistics as research tools that are equivalent to the command of a second or third language, the equivalency is relevant only in those cases where one finds within a single language or cultural area the historical record or the inter-institutional variation that makes rigorous research possible. And, of course, for many inter-institutional comparisons the other institutions are necessarily found in other national settings. I am thoroughly persuaded that the comparative method lies at the heart of our discipline and the power of the comparative method can be fully exploited only by those who command the linguistic and other cultural understandings necessary to specify the real equivalencies in institutions across cultural boundaries. The study of empirical research methods is crucial, but it is no substitute for the training needed to study institutions in cultures other than one's own.

Perhaps the greatest deficiency in the ranks of today's research scholars is the absence of the appetite and the imagination needed to launch and then carry out large-scale research. Coming out of an era in which complex research projects could be mounted only upon rare occasions, we have taken a nettle for a rose and have emphasized the exploitation of data collected by others. As all can appreciate, I am personally persuaded that there is indeed much to be said for the data archive movement (ICPSR, 1979) and fuller use of data which it has made possible. I suspect, however, that the ability to do meaningful research with data collected by others has seduced many scholars into

“making do,” into molding their research interests to those tasks which are more easily accomplished, and thereby forestalling the commitment that produces the extraordinary effort that is required to launch the most relevant and most needed new research efforts.

Today, unhappily, limitations on the capacity of individual political scientists are matched or exceeded by limitations in the institutional settings needed for the conduct of significant new research. A second obstacle exists in the fact that the discipline is still served almost exclusively by academic institutions that are admirably suited for the conduct of library research or the secondary analysis of archival data, but not at all equipped to support the generation of significant new evidence appropriate to the testing of important social-political theories.

Despite repeated efforts over the past two decades to establish large social science centers to be the equivalent of the astronomer's observatories or the physicist's accelerators, no new centers of first magnitude have come into being. Indeed, many smaller local institutions have withered and disappeared. Given the magnitude of our research needs, and the importance of the social, political, and economic concerns they would ultimately serve, it is hard to understand why the National Opinion Research Center at Chicago and the Institute for Social Research at Michigan continue to stand as relatively unique institutions in a nation as large and as rich as ours. Past efforts of leaders in the social science community to develop and implement policies that would see these two venerable institutions joined by others of similar capacity have failed almost completely. Both the BASS Report (National Academy of Sciences, 1969) and the Simon Report (National Research Council, 1976) which followed it recognized the fact that the institutional infrastructure for social science needed massive expansion if our claims to intellectual or social relevance were to be redeemed. Both reports have been ignored by those who shape national science policy as well as by those who control access to the financial resources that would be needed for such a change.

In like manner, there has not been an adequate response to the need for increased social science research capacities at the university level. Some collective efforts, such as represented by the Inter-University Consortium for Political and Social Research, have persisted with minimal commitment of resources by the participating universities. Occasional departments of political science, blessed with unusually energetic and tenacious leadership, have developed data laboratories. Significantly, however, these and other limited tales of success across the nation pertain almost entirely to facilities for the secondary analysis of data

collected by others for other purposes. Although it is no longer extraordinary to find departmental facilities for training in data analysis, the number of departments served by anything more than the most rudimentary, ad hoc capacity for the design and conduct of new research has not increased. To my knowledge, no new major academic resource for conducting original research and generating new data has come into being within the past 10 years.

As a consequence of the limited number of sites where new research projects can be initiated, there has been virtually no training base for research careers. The post-doctoral research assignment is virtually unknown in our discipline. Because of the same poverty of institutional resources, there is no base for the training of research administrators.

The third obstacle to the realization of the promise of a unified, rigorously scientific discipline that is moved and shaped by new research lies in the ubiquitous problem of funding. Whether cause or effect, the level of funding now available for basic political science research is totally inadequate. It is not adequate to maintain a high level of secondary research effort, to say nothing of supporting a portfolio of significant new data-collecting enterprises.

When one examines university support for research, the prevailing norm for the social sciences is still appropriate, at best, for individual efforts modeled on classical library research. Apparently, it is generally considered adequate to pay for a bit of released time for a scholar which, when added to uncompensated evenings and weekends, is sufficient to permit the writing of articles or a book. A serious attempt to collect a significant body of data as new evidence can be mounted only if the researcher seeks funding elsewhere or devotes months, if not years, of individual effort to data collecting. Funding to permit the gathering of evidence which will make possible the subsequent serious intellectual endeavor is extremely limited.

When one turns to the private foundations, the generally low level of support available to political scientists is exacerbated by the maintenance of the philanthropic “do-good” tradition. Those foundations that are interested in significant political problems are most likely to support the writing of a solution-promoting book. Only on rare occasions can a private foundation be persuaded to support a research project that does not promise amelioration but, instead, has data collection and theory testing as significant components. In many instances, of course, foundation resources are limited, and a significant social science research program would quickly spend the staff out of its job. For whatever reason, the private foundations seem wedded to a policy of providing “seed

money" to the social scientist, while delegating to unspecified "other" sources the responsibility for providing "harvest" money that would transform planning and thought into research and new thought.

With neither the universities nor the private foundations willing to supply funding for large-scale research, the quest for money usually turns to some agency within the federal government. Limitations on funding levels there also remain disastrously low within such sources of basic research support as the National Science Foundation. When one moves beyond NSF, the philanthropic "seed money" theme of the private foundations is replaced by the need for "cost-effective" investment in problem-solving research supported by government agencies. Mission-oriented agencies within the federal government do indeed support large quantities of research done by social scientists, even by political scientists. However, the emphasis on application is pervasive. It is seldom possible, from the researcher's perspective, to put first things first, and first conduct the appropriate basic research. The problem is doubly severe because there is such widespread pressure, greater now than before the social and political revolutions of the last decade, to demonstrate that one's work is immediately relevant to solving some part of the nation's problems.

Political research is perhaps also uniquely vulnerable when it seeks funding from government agencies that are, in turn, dependent on the political process for their very existence. In part this is true because political self-interest often dictates the response to proposals for research programs, and the worlds of the politically sensitive and the social scientist are poorly knit together in the resulting conflict of interests. We are also perhaps uniquely vulnerable in that those involved in the chain of decisions that would produce support for our research are often the very actors whom we would study. Arguments that we need more knowledge are often dismissed on the grounds that we are simply outsiders trying to find out what insiders already know through personal experience.

The reduction or elimination of the obstacles presently posed by inadequacies of personnel, institutional structure, and funding could bring political science into a new age of intellectual ferment and maturity as a discipline. The excitement of such a prospect can scarcely be underestimated, and yet the realization of such a development would bring with it new problems that are only dimly apparent at this stage of disciplinary immaturity. Many of the problems would result from the fact that, for the foreseeable future, large-scale research will remain multipurpose research. Several correlates of multipurpose re-

search will have to be accommodated by the workways and the norms of the discipline.

In the first place, the risks are greater. Complex research poses new problems from the beginning to the end of the research process. In planning and designing a large study, in carrying out the data collection for the study, and in the analysis of the evidence amassed by the study, there will be opportunities for delay, disappointment, and disaster of a greater magnitude than are now associated with research projects of more familiar scope. Under any circumstance, those who make their living by engaging in large-scale research are aware of the high risks involved. The most exciting research tests significant theories. Since, over the years, it has seemed virtually impossible to disconfirm any important piece of social theory with a single test, it has not been possible to publish null findings. Confirmation and extension are the hallmark of "good work," and the risk of devoting substantial time and energy to produce "nonpublishable" results has dissuaded many from attempting new and more venturesome enterprises and has turned them to less threatening avenues of intellectual activity.

In carrying out a complex research design, there is often a very real possibility that the researcher will not even get to the state of data analysis. Because of our lack of experience in research administration, the sheer management problems involved in large-scale studies offer new opportunities for failure. The large and complex study is not only extremely expensive, but also whenever such a study is innovative in any important aspect of design, the researcher's ability to estimate the ultimate cost is limited. In general, the financial costs of planning with sufficient care, executing with sufficient control, and analyzing to a satisfactory level of closure are very hard to estimate without a background of experience. Individual scholars, at least those with tenure, who confront unexpected delays and expenses in the conduct of personal research can often shift personal timetables and accommodate themselves to the unexpected. A massive research project that runs out of money before the data are collected cannot always be suspended until new resources are found. When time and money are consumed on a very large scale, and where anything short of completion leaves one no further ahead than if one had not started at all, the potential cost, whether out-of-pocket cost or opportunity cost, comes very high.

Quite apart from the intellectual and logistic problems of organizing large-scale research, another set of problems results from the fact that the magnitude of multipurpose research usually demands the cooperative participation of multiple investigators. If the number of scholars respon-

sible for the final intellectual product increases interdependence, and the unification of knowledge, it also poses a further challenge. Unexpected interruptions and delays in the participation of any one investigator have immediate and very human implications for the others. It is often difficult to sustain a steady state of cooperation among multiple investigators whose fates are joined together by their project. Moreover, familiar problems found in smaller if not lesser enterprises are exaggerated in the large project. These originate in the initial division of labor and they continue with efforts to reflect that division in an ultimate division of rewards. At the present time, multiple authorship of articles and books is seen by many as more of a curse than a blessing. Particularly where junior authors are concerned, the question of the value to be attached to a third authorship (particularly out of alphabetical sequence) is threatening at best.

Among senior figures, serious problems can develop as the division of labor may distinguish the intellectual leaders from the chief administrators. In the instance of the national election studies conducted by the Center for Political Studies, for example, it is not clear (and will probably not be clear for some years to come) whether the principal investigator should be the intellectual first-among-equals as a scientist, primarily an expert in the politics of multipurpose research that is intended to serve many intellectual constituencies, or should be primarily an administrator who, as chief executive officer, translates the legislative wishes of advisors and other principal participants into research operations that, when carried out by the research staff, remain faithful to the conceptual superstructure by which others have defined the intellectual destiny of the project.

Whatever the internal division of labor and allocation of rewards, all of those for whom large-scale research is relevant must be prepared for a third change in our workways, a change in the time expectations associated with the completion of significant research. Given proper training, and a year or two for careful thought, a more traditional scholar may well hope to complete a book in three or four years. The number of person-years needed to produce the final results of a complex research project may increase this figure by a number of magnitudes. Junior personnel, in particular, may find the time lag in publication highly disadvantageous for their own career needs. Of course, it is also true that the continued maturation of our theoretical interests may add its own force toward encouraging the publication of the journal article rather than the extended monograph or book. In any event, our disciplinary reward system based on the timely publication of books may have to change, as it has already

changed for economics and psychology.

At this remove, none of the problems that would follow from an increased disciplinary role for very large research efforts seems as formidable as the obstacles that stand in the way of that increase. The subsequent problems will complicate the lives of those directly engaged in research, and it will take some major changes in our workways and our professional norms to resolve these and related problems. Nevertheless, the costs of this kind of problem solving would appear to be insignificant compared to the benefits that would accrue to the entire discipline if it were instituted.

Political science has the intellectual capacity and the methodological and technical competency to make a massive contribution to the welfare of the nation while evolving into a conceptually coherent scholarly enterprise. Knowing many of the colleagues who will be engaged in creating a new era for our discipline, I am quietly confident, and I think realistically confident, that to dream the appropriate dream is not to dream the impossible dream.

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