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HERE IS a group of reports on discussion panels in fields of political science research. The panel idea was launched in 1942 by the Research Committee of the American Political Science Association. Each panel was envisaged as a representative group of scholars in a given field of political science, who might through meetings and correspondence examine and discuss the areas, needs, problems, and methods of research in their respective fields and thus formulate a strategy of research for the immediate postwar years. The reports which make up this volume are summaries of the findings of these panels.

Ten fields were chosen for examination by the panels. These varied in kind. "Some were traditional, such as State and Local Government and International Relations. . . . Three were emerging fields—chosen from the 'growing edge' of political science—Military Government, Political Communications, Political Science and War." All the fields were selected according to their breadth of common interest for members of the political science profession. The research itself was to be broadly conceived. "It was to express itself in terms of major areas rather than of desirable thesis topics, in broad outlines rather than in detail."

(Continued on back flap)

RESEARCH IN POLITICAL SCIENCE

The Work of the Panels of the
Research Committee, American
Political Science Association

Edited by

ERNEST S. GRIFFITH
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INTRODUCTION

INTRODUCTION

I

THE SECOND Research Committee in the history of the American Political Science Association was appointed by President William Anderson early in 1942.[1] It held its first meeting jointly with the Committee on Government of the Social Science Research Council, in April, 1942, at the Council's headquarters.

It was at this meeting that the panel idea was launched as the Research Committee's first and major project. A panel was visualized as a representative group of scholars in a given field of political science. These scholars were invited to undertake through meetings and correspondence to examine their field, and to formulate a strategy of research for the immediate post-war years. This would be a guide, not only to the Association's membership, but also to the Foundations and public and private research institutions. The belief of the Committee was that through such an instrument reports would emerge carrying with them a measure of prestige; and that these reports would serve intelligently to guide and stimulate much of the best work of the profession.

Four fields were selected initially, corresponding roughly to the special interests of the original committeemen—Comparative Government, Political Theory, Public Law, and the Role of the United States in World Affairs. Except for the chairman, each member of the Committee assumed responsibility for one of these panels. Within a year, the first three panels were well established and functioning with a sufficient expectation of success to justify the launching of a number of other panels shortly thereafter. Customarily the chairman of a panel became ex-officio a member of the Research Committee. This permitted

1. The first research committee functioned under the Committee on Policy in the late 20's and early 30's. The nucleus of the second committee had previously been designated by President Frederic A. Ogg in 1941.

a sharing of experience and a correlation of work. The fields selected varied in kind. Some were traditional, such as State and Local Government, and International Relations. The latter eventually replaced the abortive panel on the Role of the United States in World Affairs. Three were emerging fields—chosen from the "growing edge" of political science—Military Government, Political Communications, Political Science and War. One, Representative Government and the Legislative Process, to some extent traditional, was deemed also to belong in the category of those panels dealing with emerging problems of the first magnitude. No panel was constituted in Public Administration, because of the extensive and far-reaching work of the Social Science Research Council's Committee in that field.[2] Of the traditional major fields of political science, only those dealing with political parties[3] and public opinion and international law were somewhat neglected in the selection of panel fields, though many of the fields chosen were closely related to the three mentioned, and relevant material concerning them appears in a number of the reports. It should be borne in mind that the identification of a "field" by the Research Committee is itself necessarily somewhat arbitrary and subjective, but a genuine effort was made to identify those broad groups of our profession which possessed common subject matter interests.

To each panel was committed the same responsibility. Each was asked to formulate a strategy of research, but this research was to be broadly conceived. It was to express itself in terms of major areas rather than of desirable thesis topics, in broad outline rather than in detail. The panel was asked to indicate priorities in these areas, not only as to research in its ordinary restricted meaning, but also priorities demanding penetrating

2. The relevant portion of its report forms Chap. IV of this work.

3. Early in 1947 the Association established a Committee on Political Parties, one of the functions of which was to work out a plan of research in this field. Its Chairman, E. E. Schattschneider, Wesleyan University, writes that thus far its attempts in this direction have been largely thwarted by the difficulties of meeting. Questionnaires were circulated and the results placed at the disposal of the editor of this volume. They have been utilized by the editor in connection with his synthesis in Part II of this work.

thought and analysis. In other words, each panel was challenged thoroughly to examine its field, in the first instance to indicate those portions already substantially explored and possibly approaching sterility. Even more it was to appraise the field in terms of a creative future. What were its emerging problems; what were the areas in flux out of which changing principles of political theory might develop?

The normal panel procedure—which was by no means always followed—consisted of several more or less distinct steps. The panel chairman in consultation with the chairman of the Research Committee selected twenty or more members of the Association, recognized scholars in the panel field. These were invited to participate, initially by sending in a letter or memo on the state of the field and on research priorities therein. Those sending significant replies constituted the core of the panel, though membership remained flexible and democratic enough so that subsequent additions were usual. Any member of the Association requesting inclusion was welcomed, provided his published work had demonstrated appropriate interest and competence.

When a considerable number of replies and comments had been received from those invited, the chairman prepared an extensive memorandum for panel circulation. This memorandum contained the bulk of these comments, arranged, summarized, and synthesized. The participants were then invited to a panel meeting which, wherever possible, extended continuously over two days. The meeting was designed for discussion of the memorandum and such new ideas as might be forthcoming. In some instances one or more sessions were scheduled at the time of the Association's annual meeting. These panel meetings attempted through the inter-stimulation which comes from sustained discussion to reach agreement on the fundamentals of a panel report. Discussions were uniformly vigorous and at a high level. In most instances a younger man (often a graduate student) was named secretary, and the full flavor of the discussion was preserved in the minutes. The chairman then prepared the report which was circulated in draft form for criticism. These reports appear in final form in this volume. While every attempt has been made to have each report reflect the views of the other

panel participants, in the last analysis the chairman has necessarily assumed the responsibility for its content.

Those who examine the list of participants, contained in the prefatory note to each chapter, will recognize at once their representative character and their standing among the profession. Certainly a cross section of the best took part, chosen alike from the elder statesmen, the creative scholars of middle life, and the able younger men, to whom perhaps the panel meant most. In all, over three hundred individuals participated. Whatever may be the strength or weakness of the reports, this strength and weakness may be said to be that of the profession itself; for it is not too much to say that American political science mobilized many of its best in the venture. Even though sustained thought was not uniformly evident at all panel meetings, nevertheless, the participants were men who had given such sustained thought over the years, and they shared the results thereof with their fellow participants. It is too early to speak of the final result. This volume does attempt to incorporate the essence of the group thinking. It cannot hope to record the stimulus to the individual or the cast it may have given to his thought.

Most of the panels established temporary continuation committees of one sort or another, usually to stimulate the carrying out of certain of the panel's recommendations; but it was the prevailing opinion that the panel program in general should end with the publication of this volume—possibly to be resumed five or ten years from now when the usefulness of the present audit and appraisal had run its course.

The panels were by no means the only relevant activity of the Research Committee. The Committee as a whole, utilizing a modification of the panel method, published two reports in the *American Political Science Review*. One dealt with priorities in research in war time[4] and the other with factors favorable to or handicapping research. The latter outlined a thirty-three point program for the universities, the government, and the Association.[5]

4. June, 1943, pp. 505-514.
5. Feb., 1945, pp. 164-166.

The Social Science Research Council through its own committees has likewise made notable contributions to the guidance and stimulation of research in political science. The work of its Committee on Public Administration has already been mentioned. Its Committee on Government during the years of its existence gave fruitful attention to the development of research outlines. Customarily these consisted of a multiplicity of suggestions for specific research topics, but they were presented in a setting and with an analysis that gave perspective and meaning to what might so easily have become a mere list. If the Association's work is thought of as "strategy," these "research outlines" contained an amplitude of "tactics." In fact this division of labor was deliberately chosen at the first joint meeting of the two committees. Thanks to the work of the two committees it is doubtful if any discipline, at least in recent years, has been subjected to such a thorough audit, or has ever had presented to its scholars such a wealth of guidance for their explorations.

In the field of foreign relations, the work of the Council on Foreign Relations, auditing the teaching of international relations, deserves mention for its incidental findings on research needs.[6] The Committee on Congress of the American Political Science Association likewise made a notable contribution in its own special field, through its thoroughgoing analysis of the problems facing our national legislature.[7] The Committee on Government of the Social Science Research Council published an excellent mimeographed "Report and Recommendations" in 1944. The Association's Research Committee has published O. Douglas Weeks' exhaustive report on research in the state legislative process, originally sponsored by the Committee on Public Administration of the Social Science Research Council.[8]

The work of all these has influenced and enriched this volume in ways too numerous to mention.

6. Grayson Kirk, *The Study of International Problems*, New York, 1947.

7. Committee on Congress, *The Reorganization of Congress*, Washington, 1945.

8. O. Douglas Weeks, *Research in the American State Legislative Process*, Ann Arbor, 1947.

II

The panel method itself must stand scrutiny for its strengths and weaknesses; but before undertaking such an evaluation, it is necessary to mention the challenge that was occasionally voiced to the whole concept of "guided research." Almost every panel had at least one strong dissenter among its members. His point of view was variously expressed. Sometimes it was an unreconstructed individualism speaking—asserting that research was entirely a private affair and what a man studied was no one's business but his own. The more thoughtful among the dissenters believed—and this view was widely shared among panel members generally—that freedom made for the maximum of experiment and out of such experiment came findings of unquestioned value. Most panel members parted company with the minority when the latter held, or seemed to hold, that the kind of stimulating guidance which the panels provided in some fashion constituted coercion and endangered frontier thinking. Certainly it was the experience of many that the panels extended rather than limited horizons—and that this extension was with a wisdom both disciplined and imaginative beyond any possible paralleling through the efforts of isolated thinkers.

One must not overlook the fact that the panels as carried out in practice suffered under severe handicaps of time and money. The years were years of disruption on the college campuses, of overwork and strain in the government services, of war demands over and above the usual claims of civic duty. Responses to circular letters were often painfully slow. Panels that held more than one meeting or conference with substantial intervals intervening often were faced with a major turn-over in their personnel. The panels on state and local government and on political science and war never were able to arrange sessions apart from the all too brief discussions in conjunction with the Association's annual meetings.

Unlike the work of the committees of the Social Science Research Council, the panels of the Research Committee, as well as the Committee itself, had to depend throughout entirely upon volunteers. The work of a panel chairman was exacting, and it

ought to have been possible for such a chairman to give at least half time for a certain period or periods to the work of his panel. This was not practicable.

The Association owes a debt to the Social Science Research Council for its generous financing of the meetings of many of the panels. While the actual amount of subsidy was not great, it was vitally important. Most of the panel reports could not have been formulated without it. A special debt of gratitude is owed to William Anderson, the chairman of the Council's Committee on Government, who as a member also of the Association's Committee served as liaison between the two groups.

Panel members often paid tribute to the contribution which sustained cooperative thinking made to their own horizons. Actually a two-day panel session was a rare if not unique opportunity for the creative thinkers in a special field. It made possible the exchanging of views under unhurried circumstances. Participants could thereby subject their emerging insights to the crystallizing stimulus of kindred spirits and the equally crystallizing refinement of intellectually hostile critics. Analysis develops at a fast pace when thoughts are pooled. Thought may and doubtless usually does incubate in man's individual silences, but its intimation and articulation are accelerated and evaluation keeps step with analysis and synthesis in a situation of cooperative thought.

The final evaluation of the results of the panels must await, not only the immediate detached appraisal of the published reports, but also a long range estimate of the extent to which the course, quantity, and quality of research will ultimately have been affected thereby.

A final word is in order regarding the identification of the "important." If a panel is in essence an attempt to steer thinking and research, obviously it ought to state explicitly the criteria which led to its conclusions.

Here and there among the panel members could be found the ethical agnostic. Yet, as discussion developed, such a person seemed just as much uninhibited in expressing his views as those who declared their criteria of importance in advance. Perhaps the implicit assumption of these alleged agnostics was that whatever interested a person who had studied political science was

likely to be important; and guidance and advice were not only unnecessary but might even be harmful.

For the most part the panel members were articulate on the problem of values, and discussion centered around two or three major issues of value judgment.

A definite though mutually appreciative cleavage appeared between those who would stress the practical problems and those who favored "pure research." While there would be very nearly a consensus that both were important, such discussion certainly was of value in underscoring, on the one hand, the danger of consideration of practical problems by those who did not have their roots in the rich soil of accumulated human experience and wisdom; and, on the other hand, the danger that "pure research" would in fact be a synonym for the trivial and the irrelevant. An enormous amount of the latter might take place without any rewarding nugget of a significant and socially useful finding.

The concept of the socially useful, which in one form or another appeared most often as the supreme criterion of "important," obviously required spelling out. Separately or together it was most frequently identified with national policy and democratic vitality. Those—and they were probably a minority—who assigned top priority to research programs aiding national policy and national strength were naturally accused initially by many of their colleagues of subservience to the state, of potential chauvinism, and of too narrow a viewpoint. The rejoinder was obvious—that as of today the United States was the last, best hope of a free world, in which among other things research itself could be untrammelled. Our present problem was the practical one of survival in the face of the second major totalitarian menace in less than a decade.

This in itself suggested an ultimate affinity with what was probably the major criterion of the majority—the vitality of democracy. One panel after another sought ways and means to bring research to bear upon this supreme problem. The preoccupation of scholars perhaps even more than that of statesmen is evidently to be directed toward this end.

Moreover, a person reading the reports consecutively is struck by what is unmistakably a renaissance of values in our colleges

and universities. In the smug and ordered world of the twenties one could dally with one's hobbies or affect to rise to the Olympian heights of pure science. A troubled conscience came with the thirties, and by the forties the voice of the social scientist as well as the natural scientist more and more is echoing the cosmic or even theological cry, "What must we do to be saved?"

PART I

THE FIELDS

Chapter I

COMPARATIVE GOVERNMENT

Karl Loewenstein, Harold W. Stoke, and Taylor Cole

PREFATORY NOTE

COMPARATIVE GOVERNMENT was one of the four original regions of research which were selected for exploration by the Committee on Research in 1942. A panel on comparative government was chosen under the chairmanship of Professor Karl Loewenstein. After considerable correspondence with some twenty-five members of the profession interested in comparative government, a panel meeting was held in Washington on April 17, 1943. The results of this meeting and subsequent discussions were embodied in the "Report on the Research Panel on Comparative Government," *The American Political Science Review*, Vol. 38, June, 1944, pp. 540-49.

After a lull during the wartime period, the panel on comparative government was reconstituted under the chairmanship of President Harold W. Stoke and a panel meeting was held in Philadelphia in March, 1946, in conjunction with the first post-war meeting of the American Political Science Association. A second panel meeting was held under the chairmanship of Professor Taylor Cole in December, 1946, at the annual meeting of the Association in Cleveland. At this time the Research Committee voted to dissolve the panels, but an interim committee on comparative government was selected to act under the chairmanship of President Harold W. Stoke.

During the period between the two meetings a questionnaire was submitted by Professor Andrew Gyorgy, who had served as secretary of the panel in 1946, to members of the profession at the request of the Division of International Organization Affairs of the Department of State. The information elicited therefrom was presented in the form of a report by Professor Gyorgy to the State Department. As an outgrowth of a formal

resolution presented and adopted at the March, 1946, panel meeting and of later proposals from other sources, a Subcommittee on the Declassification of War Materials was created by the Research Committee under the chairmanship of Senator Elbert D. Thomas.

There have thus been two periods of major activity in the work of the panel on comparative government. The first period, from 1942-1944, reflected the results of war conditions, and the second period, in 1946, evidenced the impacts of the early post-war period. The fundamental questions under consideration remained basically the same during both periods, though altered conditions furnished occasions for the discussions of new problems. In addition, the post-war period has offered an opportunity to present answers to certain of the queries which had provoked comment in 1944. Because of its date of publication, which throws light on the discussions occurring in the midst of World War II, the report of 1944 is reproduced in full in the following pages. This report is followed by a briefer summary of panel activities and discussions in 1946.

Participants in the panel discussions and correspondence included the following: Colonel Herman Beukema, United States Military Academy; Arnold Brecht, New School for Social Research; Eugene P. Chase, Lafayette College; Taylor Cole, Duke University; David Fellman, University of Wisconsin; Carl J. Friedrich, Harvard University; Harold F. Gosnell, United States Bureau of the Budget; Ernest S. Griffith, Library of Congress; Andrew Gyorgy, Yale University; John N. Hazard, Columbia University; J. C. Heinberg, University of Missouri; John H. Herz, Howard University; Paul M. A. Linebarger, School of Advanced International Studies; Karl Loewenstein, Amherst College; Fritz Morstein Marx, United States Bureau of the Budget; C. A. Micaud, University of Virginia; Warner Moss, College of William and Mary; James K. Pollock, University of Michigan; Joseph R. Starr, University of Minnesota; H. Arthur Steiner, University of California in Los Angeles; Harold W. Stoke, Louisiana State University; George Taylor, University of Washington; Eric Voegelin, Louisiana State University; Frederick M. Watkins, McGill University.

A. *The Panel of 1942–1944*

I. The Role of Comparative Government

In the voluminous material accumulated by the correspondence and the panel discussion, one fact stands out clearly, i.e., that the branch of political science commonly styled comparative government has emerged from a tedious and stagnating routine and, unless this reporter is badly mistaken, is about to undergo a rejuvenation not hoped for by its most ardent devotees—this much to the surprise of those who at first had referred to comparative government as a discipline in a status of suspended animation, hardly alive under the competitive pressure of the more glamorous "international relations." In this period of total war, which has added to belligerency of a military, economic, and diplomatic character political warfare as a way to victory, the knowledge of foreign political institutions and ideologies has become of paramount importance. Comparative government has been transformed from a Cinderella-like academic discipline into a political instrumentality of the most immediate potency. It is obvious that without intimate knowledge of their political institutions and attitudes we could not hope to understand what makes our enemies strong, or to pierce their psychological armor; nor would we be able to assist them after the war in reintegration into the world community, in case this should be our task. Supercilious aloofness from other peoples' political institutions—another fact of intellectual isolationism—has definitely come to an end. If democracy aspires to become universal—and no lasting peace is feasible unless some sort of political homogeneity is established among the nations—it is essential to be informed on the political habitat of other nations, our temporary allies no less than our temporary enemies. Comparative government has ceased to be merely *l'art pour l'art*. It is forced to reorient itself in line with the technological development which is about to weld the world together into a closer union of peoples, if not of states.

Consequently, the majority of the participants in the panel agreed on two basic points: First: comparative government in the narrow sense of descriptive analysis of foreign institutions is

an anachronism. As is the case with political science at large, it has to widen its scope, to refine and redefine its methods. Second: there is no longer any single technique, neither the orthodox institutional approach nor the strict behaviorist method being sufficient *per se* to gain access to the true *Gestalt* of foreign political civilizations; methods and designs must be blended and kept in elastic touch and mutual penetration. Although some of the elder statesmen raised their warning voice that *qui trop embrace mal étreint*, that a too promiscuous or ambitious application of diversified methods would lead to dilettantism and confusion, the majority seemed agreed that the frontier posts of comparative government must be moved boldly into the precincts of neighboring and collateral disciplines—that, to use an illustration, criminal law is not under the exclusive jurisdiction of the lawyer if it helps to understand the social function of state and government. The prevailing impression among the participants was that comparative government has lost its traditional character of descriptive analysis and is about to assume the character of a "total" science if it is to serve as a conscious instrument of social engineering.

II. Methodology

If an attempt be made to summarize—or rather to paraphrase—trends and contents of what may be spoken of as the findings of the group, it would reflect a tripartite division of the discussion into problems of methodology, of substance, and finally of organization.

Fortunately, little time was lost in terminological speculations. Although it was generally felt that the term "comparative government"—denoting, in the past, a sort of herbarium of cut and dried specimens of foreign institutions—is no longer adequate to circumscribe a branch of knowledge far beyond these limits, no effort was made to find a better word for it. But it was realized that, unless the customary juxtaposition of descriptive material, country by country, was replaced by an effective functional comparison, the actual achievement would belie the very name of the discipline. Only rarely is the attempt made really to compare political institutions and functions in the

sense that a common denominator for diversified phenomena is found and deviations from the standard pattern are explained. It is not accidental that American political science has produced so few books or treatises on the state in general, while there is an abundance in Continental European literature, the reason being the lack of interest and, resultant therefrom, the insufficiency of the available factual material which would permit a fruitful comparison.

The methodological—or should one say the transcendental—interest of the group was focused on the position and function of comparative government in relation to the general culture-pattern of the state. From the stale routine of dissecting and describing foreign political institutions there emerged a blueprint of a branch of human knowledge which, "catholic" in the literal sense of the word, should become the coordinator and integrator of much that heretofore has been kept apart by narrow compartmentalization into isolated and unrelated disciplines. The *Lebensraum* of comparative government was drawn in bold and sweeping strokes, and by no stretch of the imagination could the program be called modest. It must bring into play economics and statistics, social psychology and political philosophy, cultural anthropology and intellectual history. The artificial wall separating comparative government from constitutional law proper in the limited sense of pragmatic positivism must no less be broken down than the brittle fence segregating international relations from the internal structure of government in the different countries. As an example may be noted the affinity between federalism as a problem of internal state organization and as a phenomenon of world organization. The limited institutional approach, typical of comparative government in the past, found no supporters. Research should be conducted in such a way as to make it a tool for the understanding of our political civilization as a whole rather than as an accumulation of unrelated information on individual political systems. A genuinely comparative approach should operate freely along trans-national lines. Such problems as political power, leadership (in particular, executive leadership), federalism, civil liberties—whether they have a core of sacrosanctity or are subject to sublimation—are

common to every state. Solutions arrived at in one national environment cannot fail to have a bearing on similar situations in one or several other states. The consciousness of a common destiny of the civilized world and its corollary, the abdication of intellectual isolationism, were perhaps the most surprising and gratifying aspects of the entire discussion. Understanding of our own political civilization is predicated on common experience, and, in our time, the exposition of our government cannot be properly conducted without continuous reference to the parallel situations in other countries as analyzed by comparative government.

From the methodological viewpoint, it is noteworthy that the problem of gaps played an important role in the discussions. Since comparative government stakes its claims wider than heretofore, we can no longer permit the existence of white spots on our map of the world, of areas of knowledge unexplored or neglected either in terms of political geography or out of self-restraint because certain problems are under the jurisdiction of neighboring disciplines of political science. Comparative government, conceived as a total science, must see to it that such tangible gaps are filled at the earliest opportunity, and one of functions of the group should be that of calling attention to the existence of such gaps. It was pointed out that we have little reliable information on such major areas as government and politics of the British Dominions, on Latin America (which calls for analytical monographs), on the Scandinavian countries and the Iberian peninsula—there is not a single worth-while book on the government of Franco-Spain, and even on Belgium and Holland; and how deplorably limited is our knowledge of China and Russia!

Moreover, the group realized that the *rationale* of research in this period of revolutionary revaluation of values is changing. Research which satisfies only the intellectual curiosity of the person engaged in it becomes rather pointless unless it can be utilized for the pragmatic tasks of the time and place when and where it is undertaken. Granted that the researcher, following his creative urge, is and remains the individualist par excellence. But his work is conditioned by the political environment and climate. If one may draw conclusions from the cross-section of

the profession represented in the group, American political science has become conscious of its responsibility toward other nations, which imposes upon us the obligation to make the experience of our advanced democratic civilization accessible to others as much as we may profit by theirs. Research at this juncture must free itself from national prepossessions and fit itself into a higher "strategy of research" whose general trend is not determined by the individual alone. While the group as a whole was perhaps less articulate on this point than some members may have desired, the net result was the awareness that comparative government has a definite mission to fulfill at this time and in the post-war period.

It should not be concluded from the foregoing observations that the tone and spirit of the discussion were unduly esoteric or illusionist; nor were there many exhibitions of the well-known equestrian art of riding erudite hobbies. The realistic objectives of the research effort were not lost to view. Consequently, suggestions for research were geared mainly to the requirements of post-war reconstruction. Whatever "areas" or special "topics"—the distinction being of methodological relevance—were approached, it was their bearing on the future, their potential contribution to the building of a better and saner world, which made them attractive. This projection into the future was influenced to some extent by the realization that research into current problems at this moment is badly handicapped by war difficulties in obtaining material and by the fact that a vast amount of up-to-date information is confined to the strictly confidential use of the war agencies.

III. Spheres of Research

From the discussion emerged what seems the fertile distinction between general "areas" of research, or trends in research, on the one hand, and individual "topics" of research on the other. Areas or trends may be roughly defined as wider configurations or entities of problems not confined to particularized institutions or attitudes. The approach to areas of research should no longer be sought by descriptive analysis alone, although its preparatory value is implied, but should utilize the findings and

techniques of neighboring and collateral disciplines. As an exemplification of this basic dichotomy, one should refer to such wider problems as public opinion and propaganda, within which radio, pressure groups, and censorship are contained as more specialized topics. Other illustrations for an area are: political power and leadership; civil liberties and their function within the framework of a free society; federalism in its internal and international applications; the "will of the people" (symbolic or real) as the basis of political civilization, envisaged under the alternative of conformance and consent, with its ramifications into institutions such as majority rule, suffrage, proportional representation, plebiscites; the competition between the operational controls of the state and the private lawmaking bodies. Much attention was paid to the problems of transitional government, from semi-democracy to dictatorship; from dictatorship to normalcy (what kind of normalcy?); the citizen and his state (education, civism, the role of the smallest local unit), minority protection and its institutional realization; a study of colonial administration, including German and Italian experience.

As "topics," in contrast with areas or trends, were considered problems of specialized research interest which would serve for the illustration of major trends. The mass of such suggestions was equally impressive. Those who will scan the minutes of the panel will find a rich crop for promising doctoral dissertations and hints for their own work. Many of these suggestions were brought up independently by several participants—an indication of how much they are "in the air." Only a few can be mentioned here by way of illustration: the experience with the corporative state and what should be salvaged from it; proportional representation and plebiscites (two most controversial subjects); the political implications of Catholicism and the church; how to draft a constitution; executive law-making, a subject particularly fitted for the comparative approach. All of these topics are to serve for the development of major trends or areas.

IV. Personnel

Dazzled by the variety and magnitude of such research projects, the reader may well ask: How can it be possible for the specialists in comparative government, who constitute only a minority of political scientists, to cope with a task of such dimensions? The question was constantly in our minds, and it weighed all the more heavily since for so many of the profession research is stymied by war work or restricted by teaching loads. Although it was natural for the panel to become interested in the organizational aspects of comparative government, it has frankly to be admitted that little comfort and few constructive suggestions could be offered. It was realized, implicitly and explicitly, that, at the present moment, even the most enthusiastic group of individual researchers has to bow to the exigencies of the war. A heavy competition has arisen in the work done in the field by various government agencies, especially the Department of State, the Office of Strategic Services, the Office of War Information, and the Federal Communications Commission, which, in charge of political warfare, could not fail to poach on our domain. These agencies and others have enlisted the best men of the profession. Studies are undertaken frequently by specialists with unparalleled facilities of research. Practically the entire realm of comparative government is now official business of the government. Once the material thus accumulated by the government agencies has lost its confidential character, it should be made accessible for research in political science. Here is a follow-up issue which deserves the attention of the Association and its officers.

In addition, precisely at the moment when the panel was held, the Provost Marshal's Office and the Division of Military Government began to organize, on a scale unprecedented in this country, and in any country for that matter, instruction for officers, on the Charlottesville model, in foreign political institutions. Simultaneously, an even wider program of imparting knowledge to the enlisted men of the Army Specialized Training group got under way, with comparative government a part of the curriculum. When this report reaches our readers, prob-

ably most of the teachers of comparative government in the universities and colleges will have been occupied with the subject in a grand style. Our discipline has gone to war. It is true that this instruction, by virtue of its wholly pragmatic character, has little to do with research. But it invites the comparative approach; for the pedagogical reasons, instruction of foreign political institutions cannot be conducted without constant reference to our own political experience.

This vast teaching program is based partly on the available printed material, partly on special summaries and guides elaborated for this specific purpose by the military authorities. It would be improper at this time to comment on the quality or usefulness of the material. It can safely be expected that it will be subjected to a critical review at a later time, and one need not be a prophet to forecast that evaluations will differ greatly. It is not unlikely that we shall ask whether or not comparative government was prepared for the emergency. This much can be said, that if we had had at our disposal translations or condensations of the works of foreign authors on their own countries, we should not have been compelled to use material whose main attraction was that it was written in English. The suggestion that the group should prepare a comparative study of the experience with military occupation, not only of the country but of the other states, came too late. This task has been taken out of our hands.

For purposes of this report, it may suffice to say that the group as such was unable to make constructive suggestions as to how to utilize collectively for government purposes the talent in the ranks of the profession. It seemed doubtful wisdom to contribute to the depletion of our profession by adding to the centripetal effect of Washington.

But the present emergency situation carries with it a number of desiderata for the future. The resurrected interest in comparative government coincides with the return to the campus of many students who, in an empirical way, have themselves become observers and witnesses of foreign political institutions and attitudes. The impact of this field investigation en masse on teaching and research after the war cannot be foreseen. It may

well be that our soldiers, having been rather peculiar tourists in foreign lands, will be more inclined towards intellectual isolationism than we are willing to believe now. Or, contrariwise, they may have become so imbued with the desire to know more about foreign nations which they have helped to liberate or to defeat that they will demand a synthesis of what they have observed for themselves only in scattered fragments. This would contribute further to intensification of the study of and research in comparative government.

V. Organization of Research

The most important task, however, for comparative government will obviously come in the period of transition from the emergency to what is hoped will be a normal situation in international relations. Once the liberated states turn to equipping themselves with new constitutional instruments, the discipline has a mission to fulfill in imparting our experience to other nations and in integrating scientifically their institutions into a universal pattern of civilized government. This task, visible at present only in the dimmest outlines, will call for concerted action on the part of the profession, in teamwork and planning, with individual effort subordinated to collective requirements. An organizational mechanism prepared in time and going into action when needed would be of extreme usefulness. It is perhaps not too sanguine to say that the optimism demonstrated by the panel meeting inspires hope that, after the war, scholarship, and political science most of all, may serve as a vehicle to draw the nations closer together.

While the panel meeting did not make any definite suggestions in this direction, it is proposed that within the wider framework of the Political Science Association a special subdivision or group be formed, composed of men interested in, and qualified for, comparative government. Such a group not only might place its services at the disposal of the government, which after the war will need experts, but it could at the same time be mobilized for special projects which surpass the resources and capacities of individual researchers. The need for

such an organization after the war is all the more obvious since, as can safely be expected, many of the men who during the war proudly served in the government agencies, will have returned to their classrooms and studies. It may shock the inveterate individualists of the profession to realize that here as elsewhere collective effort and coordinated planning will be superior to individual enterprise and private initiative. By no means does this imply the end of individual research. The scholar driven by his private intellectual curiosity is impervious to regimentation. Research never can function properly in a straitjacket. But what the future may bring is such a common strategy of research as will enable the individual to fit his interests into the general plan. A "pooling of subjectivities"—to borrow a felicitous expression from the panel—could result in an organization of the profession preferable to the "rugged individualism" now existing.

In order to translate the spade-work done by the panel meeting into a tangible, organizational result, it is suggested that the panel be continued as an institution, and that those members of the Association interested in a closer union among the specialists in comparative government communicate with the president of the Association, or the present writer, in order to create a nuclear frame which would serve for the pursuit of the manifold tasks connected with comparative government in the immediate future. It is most desirable that the existing panel group, whose selection was beset with various difficulties and necessarily haphazard, be effectively enlarged and permanently maintained. It is dependent on the widest possible support of the profession. Though a novelty within the dynamics of the Association, this project should not encounter insurmountable difficulties. It should be remembered that the American Bar Association has its subdivisions according to specialized interests of its members without detracting from the general professional purposes of the organization at large. Encouraged by happy experience with the tentative panel meeting, the reporter ventures to suggest that a Committee on Comparative Government be established by the American Political Science Association, and that this report be considered as an urgent and cordial invitation to

all genuinely interested in the field to enroll as members of the new group.

B. *The Panel of 1945–1946*

At the end of the War, there was a continued re-examination of the problems of methodology, substance, and organization in comparative government, though there was little that was new to add. There has been a continued recognition of the different meanings of comparative government, in which the emphasis may be placed on 1) mere description of governments abroad without any systematic comparison, 2) comparison used as a method of description, and 3) comparison used, not as a method of description, but for the solution of theoretical and practical problems. Mere description does have some limited utility, particularly under wartime conditions. There has been, however, a continued recognition of the more basic significance to political science of comparison in the third sense, that is, as a tester of theories and as a device for utilizing actual governments as laboratory specimens for the political scientists. It is this third type of comparison which was experiencing a healthy growth prior to 1939, especially under the influence of European scholars in this country. Despite expectations to the contrary, it is this type of comparison which, rooted in theory, has suffered under post-war conditions and demands. Its revival will depend in part on the contribution of the new additions to the thinned ranks of professors of comparative government who have chosen to remain in the universities.

Otherwise the considerations and deliberations of the panels in 1946 turned largely on "practical" questions. Certain of these involved 1) the present obstacles to research, 2) the utility of new teaching methods, especially in area courses, for training researchers in comparative government, 3) the evaluation of wartime collective research efforts, and 4) the usefulness of future joint research projects in comparative government.

The obstacles to research during the post-war period have been numerous. These have included the usual ones of censorship, of language, of transportation, of finance and of living conditions in foreign countries. Two additional obstacles were singled out

for special examination and attention. These were the operation of internal security regulations affecting war materials, and the difficulties in conducting field research, especially in the critical areas of the world.

The continuation of the internal security regulations during the post-war period has been necessary in many respects, but their rigid application has prevented the accessibility of much documentary information in the files of the War and Navy Departments. It is true that some steps in declassification have been taken, as in the case of military government handbooks (ASF Manuals). It is also true that these steps are being accelerated. However, many captured German documents, SHAEF and 12th Army Group Intelligence Summaries and the many materials in the Documents Library of MID have not been available for special research purposes. If encouraged, mature men now working for their doctorate under veterans' education benefits might take advantage of the opportunities which these rich materials afford with results that would be beneficial both to the researchers and to the government agencies involved.

The second limitation has been the difficult, if not insuperable barriers to research work in those special areas which are of most vital concern in current international politics. Three of these areas, Soviet Russia, China, and Germany, were subjected to special scrutiny. In addition to the other handicaps, the lack of official permission to study in Russia, the linguistic problem in China, and the lack of availability of living accommodations in Germany have impeded most individual efforts which have not been carried on under governmental auspices. Research on Soviet Russia, in particular, must be confined today largely to an examination and verification of research materials already assembled outside of the U.S.S.R. or of official documents and releases. The importance and inaccessibility of these areas suggest the desirability of maximum assistance and encouragement by the Government to students desiring to go abroad for research purposes.

Any listing of areas and topics for research will indicate altered emphases rather than new fields of inquiry during the post-war period. An enumeration of such suggested topics dealing with

the new "constitutional systems" in both Europe and the Far East will be illustrative. Under these systems, attention has been directed to the questions and problems of majority rule and leadership; of democratic minorities and undemocratic majorities; of substantive government propaganda within a democracy which is under anti-democratic attack by internal or external foes; of emergency powers and their limitation and control; of the selection of chiefs of state; of the right of dissolution and its limitation; of post-war proportional representation and electoral systems; of provisions for minimum standards of maintaining the independence of judges; of post-war parties, their ideologies, structures, and methods in comparison with those of pre-war parties; of the operation and organization of official agencies engaged in the dissemination of external propaganda and in psychological warfare activities; of the methods of planning under democratic control; of the methods of social control without nationalization; of the problems of bureaucracy during periods of reconstruction; of the administrative problems involved in departmental organization, and of regional coordination of federal administrative agencies.

The new training methods which were developed during the War under the impact of the ASTP programs and of special forms of military instruction have continued to receive wide attention and the results have produced conflicting appraisals. The conditions of instruction varied so widely, the methods utilized were so different, and the purposes of the instruction so divergent that generalization is dangerous. Certainly the wartime results fell far short of expectations, especially in the light of the indiscriminate use later made of many of the graduates. At the same time there has been an appreciation of the lasting value for the post-war period of the Foreign Area language and geography courses.

The present Foreign Area courses and programs of instruction in American universities, influenced as they have been by this wartime experience, have broadened the scope of research in comparative government and have probably endowed it permanently with a more catholic character. In several well organized and coordinated programs, political science, history,

and economics are serving as the core, but these new programs are distinctive in their use of material from many disciplines. It is generally believed that the basis for these area programs is sound and that their careful expansion will produce more well trained researchers as well as more sound research in comparative government. Furthermore, the coordinated efforts of the individuals engaged in these collective programs may result in one of the few types of group research which can be successfully carried on in the future.

The evaluation of the foreign country analysis methods used by such organizations as the Office of Strategic Services and the Foreign Economic Administration during the War has been conflicting, though there is a general recognition of the utility of these studies of wartime purposes. Certainly the materials processed by the trained groups proved of great value to the military agencies during the War. Many of these studies, including some which are the most valuable ones for political scientists today, are still classified documents. They cannot consequently be assessed as to quality except by those members of the profession who participated in their preparation.

There are conditions under which group research in comparative government may be encouraged by universities or national foundations. The restrictions which come from limited resources and conflicting pressures in universities would necessitate cooperation in the preparation and publication of certain types of studies. At the same time the completion of the various war histories which are being written may well mark the end of an epoch, for the individualistic traditions of the profession are beginning to reassert themselves. There is a prevailing belief that the quality and quantity of research in comparative government will be improved by the acceleration of the trend towards greater individual work and responsibility. The anticipation that "individual effort" must be "subordinated to collective requirements," as suggested in 1944, has hardly materialized for the post-war period to the extent suggested in 1944.

Nevertheless, there remain many types of projects which can only be undertaken in the future through some form of group cooperation. In some instances group publication of a number

of individual projects which would not merit separate publication will meet the need. In other instances the final preparation of reports will require the specialized knowledge and advanced skills of various experts in several fields. The very magnitude of some tasks will require a distribution of the physical labors. Such tasks might include a definitive analysis of the Nazi and Fascist regimes. In the light of our present knowledge from the inside, many wartime opinions and evaluations may be subjected to reexamination.

The panel on comparative government has continued to weigh the proposals made in 1944 for the formation of a permanent committee on Comparative Government or Comparative Administration. There is much which can be said for such an organization, particularly if it were adequately financed, to serve as a clearing house for the collection and publication of various types of materials. Such an organization might be the medium for bringing together periodically scholars from different countries during a period in which the United States must and should take the lead in arranging these meetings. In the past the students in this country have expected Europe to provide much of the leadership and most of the real contributions in comparative government. The French, it is true, are again assuming something of their traditional role in this regard. Nevertheless, that leadership must of necessity, if not by choice, be assumed today in the United States. Some permanent committee of the American Political Science Association could help in this task by furnishing a professional bridge between American and European and Asiatic scholars.

Chapter II

REPRESENTATIVE GOVERNMENT AND THE LEGISLATIVE PROCESS

Roland Young

PREFATORY NOTE

THE PANEL BEGAN IN 1944 when the convener sent a letter to a number of individuals, asking some general questions on what they considered to be the research needs in the field. An abstract of the answers was then circulated and comment requested. As the panel developed, certain ideas crystallized, indicating a fairly uniform agreement on the need for additional research in certain areas; also, the membership of the panel contracted and expanded, with some individuals dropping out and with other individuals drawn into the vortex of the discussion. This correspondence was supplemented by three meetings of a selected number of panel members. Two of the meetings were held in connection with meetings of the American Political Science Association and a third was held in Washington, D.C., the weekend of December 7-8, 1946.

The direction given to the panel and the permissible methods of approaching the subject were sufficiently broad to allow the panel a plenitude of discretion in what to say and how it should be said. The following report represents a summarization on the part of the convener of the various deliberations of the panel, oral and written, and it is presented in essay form in order to give unity to the proceedings.

In view of the recent studies on organization made by the Joint Committee on the Organization of Congress, by its staff director, Dr. George B. Galloway, and by the American Political Science Association's Committee on Congress, the panel did not believe it necessary to discuss this subject further at the present time.

The following were the panel members, an asterisk designat-

ing those who participated in one or more of the panel discussions: Stephen Bailey*, Wesleyan University; Donald C. Blaisdell*, Department of State; Joseph P. Chamberlain, Columbia University; Hugh MacDowall Clokie*, University of Manitoba; Marshall E. Dimock, Northwestern University; Walter F. Dodd, Chicago, Ill.; W. Y. Elliott, Harvard University; C. J. Friedrich, Harvard University; George B. Galloway*, Library of Congress; Ernest S. Griffith*, Library of Congress; John A. Fairlie, University of Illinois; Paul D. Hasbrouck, Poughkeepsie, N. Y.; Kenneth W. Heckler*, Princeton University; Edgar J. Kemler*; Avery Leiserson*, University of Chicago; Howard J. McMurray, House of Representatives; Arthur W. Macmahon*, Columbia University; C. B. Marshall*; Rodney L. Mott*, Colgate University; John A. Perkins, Budget Director, State of Michigan; Floyd M. Riddick, United States Senate Staff; Carl B. Swisher, Johns Hopkins University; Harvey Walker*, Ohio State University; B. B. Wallace*, U. S. Tariff Commission; Schuyler Wallace, Columbia University; Belle Zeller*, Brooklyn College.

I. Introduction

The report that follows is in part an analysis of the legislative and representative process and in part an identification of problems relating thereto. It represents the opinion of no one individual and perhaps does not even summarize adequately the opinion of all panel members. It does, however, represent an attempt to secure the opinion of a "representative" group on two particular, although infinitely abstract subjects.

The significance of the fact that this panel was created during the years of the war deserves some explanation. The creation of the panel was part of that broad movement for post-war planning which occurred during the war and which had its most signal results in the creation of the United Nations while the war was still in progress. In the academic field, many believed that the immediate restrictions on research imposed by the war called for the development of research programs which would be useful once peace arrived, and that the war itself had demonstrated the paucity of our knowledge of political behavior and of controlling political society peacefully. Despite the great

worthwhileness of this goal the war itself was a dual handicap in developing material for a research program. In the first place, the war made its drastic demands on time, and for panel members it was little easier to scrape up enough time to consider future research needs than it was to carry on research currently. In the second place, the panel of necessity was thinking about pre-war conditions, and it neglected to a degree the behavior of representative institutions during the war crisis and the current attempts to recreate or to establish representative governments in other parts of the world.

II. Representative Government

The concept of representation is useful in describing certain aspects of behavior of many social groups, although the lack of uniformity of terminology makes such description less precise than might be wished. In its looser and more universal sense, there is a representation of values or interests, where, for instance, the winner of a beauty contest "represents" certain standards of beauty, or where an athletic team "represents" his institution, or where a king "represents" his people, or where the Church "represents" Christ. In all of these examples, and they could be multiplied endlessly, there is a common denominator of responsibility and of authenticity, of the smaller unit being the reflection of the larger unit or the larger idea. The panel, however, has been concerned with representation in its more sophisticated organizational forms where attempts have been made to provide institutional devices for determining representation. In considering representative government, one is constantly aware of the distinction between procedural and normative types of representation, of the determination of the component parts of representation without reference to existing procedure and of the essential elements in procedural devices which create the desired representation. A representative, for instance, may have fulfilled all legal requirements for election and, as such, is the "legal" representative of his district. Yet, from another point of view, this representative may not be considered to be "truly" representative of his district or of the best interests thereof. The repre-

sentative may be a member of a political machine which controls his voting and his political behavior and which determined whether he should be a candidate and should hold office. In using the term, "representation," there is a continuous problem of terminology, of distinguishing between representation which has a legitimate claim to power, legal or otherwise, and representation used in the sense of ends or goals to be achieved.

There are, moreover, various gradations in the totality of the representative process and various methods by which representation is determined. A relatively simple type of representation is that of the lawyer and his client; for here, ordinarily, only two people are directly involved and the scope of power "delegated" is fairly closely defined. A more complicated type of representation is the relationship between the individual and his elected governmental representative. Although the individual concerned thinks of himself as being "represented" by another individual, the representative in question thinks of himself, say, as representing a district composed of many people. The process becomes more complicated when the representative becomes a member of a legislative body such as Congress. Not only are there various types of representation within Congress, but occasionally Congress itself is represented in the formulation of certain policy. Moreover, the United States as a corporate group may be represented in some international body. The representative process, therefore, involves the relationship between an individual and his representative in any particular group and between the individual and all groups in which he is represented; it involves the representative in his relationship with the individual, with the group which he represents, and with other groups; and it also involves the complex relationship between the various representative groups.

For the sake of convenience, the problems of representative government are here classified under three major headings: the basis of representation, the relation between the represented and the representative, and the purpose of representation. In addition, there are listed at the close of this section some problems which are applicable to more than one of these major headings.

The Basis of Representation

Many problems arise in the consideration of the unit of representation, which is usually a territory or a group. In the American political milieu, the geographic basis for representation is of outstanding significance; but there are other types of representation extant, such as that of the group, and other types of geographic representation than the single-member district. In attempting to provide representation for industrial groups and various types of international agencies, the problem of the representative unit has been very difficult to resolve.

There is further need for information on the representative unit with special reference to the adequacy of that unit as a basis for administrative purposes and for supplying the requisite representation to formulate public policy. In municipal areas, to take an example, there is frequently a dichotomy between the city as a legal unit—having its own particular legalized representative process for choosing the magistrates—and the administrative needs of the broader metropolitan area. The result is often a complex pattern of special group representation, both official and unofficial, superimposed on ordinary geographic representation, which makes it possible for those areas to have a common pattern of administration for specific functions. Similarly, analyses of the relation between the political base and the administrative requirements need to be made of county and state government; and of course this problem is at the heart of the present considerations of unifying Europe.

In spite of our long history of popular elections, there is a considerable dearth of analytical information on the geographical unit of representation. This problem might be considered from the point of view of the groups most benefited and least benefited by the particular method prevailing of the determination of the unit; the resistances to redistricting; the ideal factors to be considered in creating units—such as, for instance, the degree of homogeneity which exists or which is desirable; the extent to which various national issues are reflected by problems within the unit; the tenure of elected officials; and other similar problems. There is, moreover, need for comparative studies of the

unit of representation in various associational groups, such as churches, labor unions, business groups, universities, and professional groups of all kinds; such studies might show, in some cases, that a high degree of representation is achieved by other means than periodic, multi-choice elections, a fact which would have a degree of bearing on the efforts of the government in regulating some of these groups and in relating their actions to other aspects of the economy.

It was suggested during the panel discussions that additional research on the territorial basis of representation would emphasize the weakness of party government in and outside of Congress and would indicate the need for party responsibility in the formulation of legislative policy. The experience of various European States with proportional representation might be further examined to indicate whether such representation produces effective government in a large industrialized state. Conversely, studies could be made of the effect of the single member constituency with its apparent accompaniment of the two party system.

Relation Between the Represented and the Representative

The nature of the relationship between the represented and the representative comes close to being the core of the representative process, for it treats of the fictional process by which the interests of one are entrusted to another. In our political process, representation has a legal basis, there being rules prescribed for elections, requirements for voting and so on, and the elected representative has certain prescribed legal functions which he is expected to perform. Representation, however, has many non-legal and mythological aspects, as is demonstrated, to state only one example, when a representative runs or stands for election. In the electoral process, the representative attempts to identify himself with, that is, "to represent," many particular dogmas, prejudices, irrationalities, prides, and hopes of the electorate. Even signal political issues become transformed, as it were, into specific reactions of the individual. Perhaps there is no reaction other than that the voter believing that the representative, being otherwise competent, is presumably also competent to decide

the particular issue about which the voter wishes to remain in ignorance. This process of identification with symbols and myths is not confined to the relationship between the individual and his so-called legal representative. A Congressman from another district, for instance, may behave politically in such a fashion that a particular individual may feel that he is, in truth, "represented" by that Congressman, even though he cannot support his feeling of reciprocal outlook by the concrete method of delivering a ballot for him. More than that, an individual with a relative in the service, say, may feel that he is "represented" in the Army and, at the next parade, feel a special identity with the band.

This so-called irrational factor in political behavior needs considerable study, and it calls also for the use of analytical tools developed by other social science disciplines, such as psychology, sociology, and anthropology. The subject of irrational behavior has two important correlative aspects. One is the problem of civil liberties,—the retention by the individual of certain rights which he does not or cannot delegate. The other is the problem of the mythical representation by the Leader or by the State in which, ignoring procedural requirements, the individual believes that he is completely represented by another.

• In any complex society, an individual is represented by a large number of individuals or groups; regardless of the adequacy of the representation, there are many individuals and groups who pretend to represent him and who speak in his name. The individual may be aware of his representation in various governmental units and in associational groups to which he belongs, but he may also be "represented" by special groups which speak in his name, in another of his capacities, as a consumer, stockholder, or property owner. The integration of these numerous and sometimes conflicting claims of representation can take place only within the individual himself, but there is the further problem of the institutional arrangements by which these various representative groups can adjust the conflicts between themselves and the basis on which they can be allowed to participate in the formulation of public policy.

The over-all problem of individual adjustment to divergent

interests is somewhat beyond the scope of the panel subject, but in critical cases, where there is a maladjustment of a number of people, the problem may have considerable political consequences, resulting, say, in riots. The multiplicity of groups and individuals who "represent" an individual is a problem to be resolved not only by the individual or group but by the government as well. Studies of the complicated skein of political representation, well illustrated by the long-ballot, point up the problem, but the problem is more extensive than that. The problem relates partly to the discipline in each particular group and the extent to which various individual interests are in fact represented, and partly to the need for governmental units to represent the interests of the group members or of other groups. Although some studies have been made on this general subject, it still remains a broad field which has not yet been thoroughly described or analyzed.

With regard to the competency of the electorate in the determination of policy, one might ask the question, what types of people and what groups of people are technically competent to vote and on what questions and in what situations? Although there has been vast extension of higher education in recent years, there has apparently been no systematic study of the extent to which, say, college graduates are actually better prepared to exercise the franchise intelligently than they would have been without such education. No attempt need be made to establish criteria for "correct voting"; rather, the research, by a series of tests, might be directed toward the determination of the extent to which contemporary issues of great complexity are understood. A concomitant problem is the extent to which the electorate may be expected to decide "technical questions," a problem which is discussed in greater detail below in the section on the Legislative Process.

A continuing problem of representative government is that of educating the electorate, of determining the basic knowledge which a citizen should have to carry on his responsibility as a member of many groups, of persuading the people to demand more of the men who represent them, of electing better men to perform that function, and of determining the extent to

which Congress and the Executive Office could improve the quality of public governmental information. Some members believe that further research is needed on methods of educating and stimulating public interest in national issues.

The problems inherent in representation and the necessity for the government to provide a balance between competing groups have been accentuated by the increasing importance of the organized group and the demands it makes on governmental and on other organized groups and unorganized individuals. There is need for further knowledge of the effect on representative government of the rise of the group, of the representative nature of the group, and on ways and means of relating groups to government. There is here, certainly, a cultural lag between the ideology of the age of individualism and the realities of an era in which group adjustment is the central aspect of government. Governmental problems arising from the necessity of relating groups to each other are often carried on under old rituals and without the benefit of appropriate institutions. An important factor in this adjustment is, of course, the politician, be he a representative of a political unit or an official of an associational group. A more coherent and systematic body of knowledge concerning the theory, function, and practice of the operating politician could be built up by a series of case studies which would analyze his background, financial interest, or operating political mythology. Such studies might also include psychiatric investigations and interpretations of the sub-conscious motivations of politicians. One member of the panel suggested the question of how it would be possible to remove the sense of guilt from Congressmen.

Many rigidities develop in the expression of the "will of the electorate." The process is not entirely rational, as the term is commonly used; and, as everyone knows, the various elections do not always result in concrete decisions on the major issues affecting the electorate. The electoral process raises questions concerning the control of propaganda, the utilization of symbols and of money, and the possible lag between the time when a question should be resolved and when the will of the electorate can be determined. It also raises questions concerning the efficacy

of elections, the possibility of improving their meaning, and the development of other methods for expressing the public will. There is a problem involved in providing a method for the electorate to register its opinion on separable particular issues without the delays and complexities of the initiative and referendum. It is recognized that the government might wish to secure the opinion of the electorate on occasions other than formal elections and by other means than securing the opinion of the elected representatives. The representative process is, in fact, a two-way operation, and the government may as frequently raise the issues which it wishes decided as the electorate may tell the government what it wishes done; in fact, the questions which can actually be referred to the electorate for decision are relatively few in proportion to those which must actually be determined.

The relationship between the electorate and the representative in any particular unit cannot be considered in the abstract only; one must also consider the relationship between the particular unit and other like units. It is important to examine the attitude of one group toward other groups and the extent to which there is an overall accepted prevailing mythology that a common process for the formulation of policy is acceptable. The theory of representation would seem to imply the willingness of any group to receive in the end something less than it was ostensibly organized to achieve.

The Purpose of Representation

The purpose of representation may vary from group to group, but perhaps all representation has a common psychological basis in the feeling of association, in the feeling that one "belongs" to a group; and the representative process becomes the instrument by which such a feeling is inculcated. The purpose of representation of labor on a labor-management group might be to increase productivity, an end which would be achieved through the psychological device of making labor feel that it had a greater and more important part to play in the production process. The psychological aspects of representation have not, to my knowledge, been explored, and one can think,

as an illustration, of the great psychic energy which went into the drive for woman's suffrage. The psychological elements of representation are deserving of further study, for the knowledge such research would provide should not only give new insight on such a subject as nationalism but it should also provide information on the methods by which cohesive groups may be created for the fulfillment of specific functions.

The purpose of representation in some minor associational group might be no more than to provide a limited type of cohesion and a simple administrative structure so that the group can carry on its relatively limited functions. The purpose of representation in a large organized group, such as in a State, is more difficult to define, for not only does it involve the formulation of decisions by the representatives of many units but the process itself is a part of the mythology which gives unity to a modern state.

The purpose of representation is, of course, closely allied with the ends of the State, and further research is needed to determine how the ends can be achieved through the representative process. One might advance the proposition that proportional representation, for instance, had as its main purpose the establishment of a government which more or less faithfully reflects the myriad wants of the people, with little attention given to how these various wants may be reconciled. Geographic representation, especially in single member constituencies, while reflecting geographic interests, makes it possible to create a government which is not so limited in its choices and which can consider even those groups which are not specifically nor strongly represented.

Moreover, the representative process involves a certain "balancing" of interests, and devices are continually being created to give specific representation to groups which do not receive the proper representation through the electoral system. A study of the various types of representation used by administrators, for example, would give more information about the formation of public policy, and it would aid in answering such questions as the extent to which outside groups and key Congressmen are regularly consulted on important matters, and it might point to the possible necessity for creating stronger controls in a presi-

dential cabinet. The use of presidential appointments as a method of balancing representation, and securing varied types of representation, is worthy of further research. In our multi-type of representation, there is frequently a divergence between the type of representation afforded by Congress and by the President. In the former, there is an attempt to provide a mechanical type of representation which mirrors the interests of a large number of constituent groups, whereas in the Presidency there is an attempt to provide an over-all representation where it is possible to achieve a certain balancing of values. The question of the representation of the bureaucracy also needs to be considered, and of course there are various devices extant to insure a proper representation by sections. The various attacks on the bureaucrats from time to time pose the problem of the divergence between Congressional standards of representative bureaucracy and those held by the Administration. Congress is continually supplying material on this subject; the Congressional attacks involve not only principal administrators but also limitations by categories of those who can be employed as well as prohibitions where names are specified. Such ambiguities in representation are also present elsewhere, and certainly one of the problems of representative government is the determination at what stage or level the necessary compromises between the various groups can be effected and the method by which the mystical concept of the public interest can be established.

The representative process is dynamic rather than static, and a considerable part of the political process consists in the pressing of claims for power within and among the various represented interests and the adjustment of conflict between and within represented groups. The representative process, in other words, includes not only the determination of who the representatives should be but also the methods by which those represented groups resolve their conflicts. Studies might show that in many cases the representative has little choice other than to act as the mechanical spokesman for an opinion already formulated by the group which he represents. In the national government, conflicts are frequently although not necessarily always resolved by the legislative process, although for many groups the Congressional

analogy is not applicable. A study of policy making in a selected number of cases, which would determine where the decisions are made and the influence of interest groups in selecting the focal point of decision, would have considerable significance in creating a theory of the function of Congress.

Special Problems

The use of the concept of representation is not limited by the more formalized and institutionalized usages traditionally associated with legislative representation. As has been already said there is a considerable need for further study of the representative nature of associational groups and of their relation to other groups and to the government. To an increasing extent, the Federal bureaucracy has been experimenting with various types of representation as an aid in the formulation of some specific policy. The experiments in bureaucratic representation include, most spectacularly, the farmer polls on agricultural production conducted by the Agricultural Adjustment Administration, the formulation of code authorities under the National Industrial Recovery Act, the supervision of labor-unit elections, the attempts to represent the consumer, and the multitude of examples arising in the experience of the War Production Board.

There are various types of representation which exist in international organizations, and it might be pointed out that representative principles were a major consideration in the drafting of the charter of the United Nations. Six of the eleven members of the Security Council, for instance, depend for their election upon the General Assembly which in turn represents all members of the organization. In the General Assembly, hundreds of millions of people are better represented in international organizations than ever before. In several international organizations, there is direct group representation, which is in effect a type of international syndicalism, such as in the representation given to the specialized and non-governmental organizations. From the beginning, the World Federation of Trade Unions has asked for full participation, except for the right to vote, in all meetings of the Economic and Social Council and its committees. There are in addition the special problems of repre-

sentation in the composition of the Secretariat and of the Secretary General and Assistant Secretaries General. The problem of representation in the United Nations has not been solved, and studies of the problem and the manner in which it has so far been met are needed of the Secretariat and of the various United Nations organs.

Although the question of the recognition of States is beyond the jurisdiction of the panel, it is still worth noting that the State Department has frequently utilized the concept of representative government as an instrument of foreign policy, and in particular it has specified that "free elections" be a condition for recognition. Moreover, occupation policies in Germany and in Japan are attempting to provide a representative type of government for our conquered enemies. The implications of those policies are significant, having as a premise the thought that the representative process is a necessary basis on which to build an international community. The use of representation as a concept of foreign policy would be worth while developing, especially inasmuch as the whole ideology on this problem is in such a state of chaos, ranging from non-intervention and free choice to collective intervention and administrative tutelage.

III. The Legislative Process

There are difficulties in defining the scope of the legislative process. If the legislative process is considered to be coterminous with the process of legislative assemblies, one must include other aspects of assembly action as part of the legislative process, such as, in some cases, the formation of the government. If the legislative process is to be considered a particular method of making a particular type of policy, it would be necessary to consider the operations of that process in many different types of institutions; it would further involve the differentiation between the administrative and the legislative processes and the distinction between law and policy. The panel has recognized that many institutions make law, or regulations having the effect of law, but this is not always known as the legislative process. In view of these reasons, and in view of the fact that other panels have

explored the problem of group thought, the panel has used the traditional, but limited, definition of the legislative process and has considered for the most part only problems relating to the making of law in the legislative assemblies.

Function

There is an apparent need for the re-analysis of the function of legislative bodies in the modern world, for to a considerable extent the theory under which legislative assemblies operate was evolved in a less complicated society than that in which we now live. The combination of bigness, complexity, timing, and planning, which is characteristic of modern governments, has created special problems for legislative assemblies. Complex problems must not only be resolved, and often quickly; but they must also be posed in such a fashion that the legislative assemblies can make a rational decision. Within Congress there are many divergent theories of the function of Congress, and of its proper sphere in controlling the Executive branch, and the same is true of other legislatures. The question of the function of Congress is closely related to the distribution of power among Committees, to the various attempts at institutionalizing Congressional leadership, and to errand-running for the constituents and the "sponsoring" of constituents in their appearance before departments. A theory of function should follow an intensive study of numerous legislative bodies; such a study should analyze the actual influence of legislatures on all significant questions, and should compare the theory with the actual practice of such institutions. Although the special problems facing any particular assembly vary, it would seem that there are some constants which are necessary if the legislative body is to function and is to be effective.

Legislative Concepts

Certain procedures, practices, and mythologies appear to be relatively constant in legislative assemblies, even though there may be variance in procedural details. A politician, for example, can readily acclimate himself in a municipal body, a state legislature, or in either branch of Congress, because of the similarity

of concepts and methods. One would suppose, also, that a parliamentarian from Australia or England would have little difficulty in acclimating himself in an American legislative body or that historical figures such as, say, Burke or Clay, would make competent Congressmen today. In other words, there seems to be a parliamentary mythology which is constant over time and space. Such constants would include the will of the people, the geographic decentralization in the choice of representatives, the frequency of elections, the necessity for discussion, the rights of the minority, the acceptance of rule by the majority, the operation of committees, the techniques of compromise. Similarly, legislative institutions develop distinctive "group lives," consisting of usages, customs, unwritten rules, the deference given to various members. Legislatures also develop a distinctive method of argumentation in advocating or discrediting legislation; there is a constant flow of rhetorical slogans and figures of speech which are given political meaning, and there are appeals to emotional ideas as standards of judgement and which cloak other ideas which a legislator cannot or does not wish to express. The various uses of such words as the separation of powers, states' rights, bureaucracy, and sovereignty are cases in point. There needs to be, certainly, a further analysis of legislative bodies; and many of the points mentioned previously in this paragraph should be developed and amplified. The representative process, which is essentially rational, does not completely explain the manner in which institutions operate and the rivalries which may develop between two or more institutions, each of which is in turn representative.

The Determination of Technical Questions

The determination of the issue to be posed is a constant problem besetting legislative assemblies. There is not only the general problem of what issues to present, and when and how they should be presented; there is also the more specific problem of the extent to which technical problems can be separated from problems of objectives or values. Extensive and intensive explorations of the possibility of such segregation would seem to be valuable if it were found desirable to differentiate in the in-

stitutional handling of such questions. If technical questions are to be reserved for the technically competent, one might ask what devices are available for establishing the accountability of these technically competent persons? How far, for example, can a governmental agency such as the Public Health Service actually go contrary to the organized medical profession in health regulations or policy? How far are recommendations by the technically competent in the government service dominated by the wishes of the technically competent in professional and business circles generally? What relationship does this bear to the public interest? Research of this type might shed some light upon the actual effect of legislative powers delegated to either professional associations, business corporations, labor unions, or federal officials.

Fact-Finding

Under a certain theory of legislative function it is desirable for the legislature to secure detailed information as an aid in resolving complicated political and economic questions. Whereas Parliament has practically abdicated its function as a fact-finding body, Congress has been devoting an increased share of its attention to the finding of facts and it has further developed its own staff. A broad question to be asked is the extent of the utilization of technical information by legislative bodies all over the world. With regard to Congress, there are particular questions regarding the areas in which it receives technical information, the use made of this information, and the adequacy of the present staffing in supplying the requisite information.

Relations With The Executive

The relations between chief administrators and the legislative bodies offer a continuing series of problems inasmuch as each is interested in, and to some extent dependent on, the actions of the other. Specifically, there are the problems of improving the communications between the executive and the legislative body with respect to the planning of policy, the presenting of policy questions in the consideration of the budget, the supervision of administrative policy, and the coordination of governmental functions. The relations between the departments and Congress

have, to some extent, been institutionalized, by both practice and law, the law including such subjects as reporting, consultation, and the termination of legislation. There is, moreover, a considerable body of factual material concerning the delegation of power and the use of the veto power which needs to be assembled and analyzed. The relation with the Military Establishment is a particularized problem which varies in degree from the relation between Congress and the civilian departments and agencies. There are special problems involved concerning the significance of political appointments to the service schools, the extent to which it is feasible to inform Congress of military problems and military planning, the participation of military groups in the formation of foreign policy, and the appointment of service officers to civilian positions.

It was suggested during the panel discussions that political scientists should be bolder in their approach than they have been traditionally, and that it would be appropriate for them to consider the applicability of a parliamentary system in the United States. A survey of that nature should provide additional material on the extent of the crisis in the presidential system and the possibilities of adjusting the difficulties and deadlocks arising between the executive and legislative branches. One would like to know, also, whether the presumption is justified that a parliamentary system in a large country like the United States might increase the proliferation of special interest groups. How has it been possible to achieve such a degree of energy in the presidential system and, at the same time, to escape the pathology of the presidency?

Other Relations

Other important aspects of legislative bodies include their relationship to political parties and to other political units, including, in the case of Congress, the relationship to states. The relation between, say, Congress and the political organizations of the two great parties may at times seem nebulous, and it seems true that the intensity of the relationship varies according to the issue and according to "the time." In addition to what might be termed "real" influence which the party exerts from time to

time, there is the concept of partisanship which is continuously present both in organization and in debate and which is one of the emotive words used in argumentation. The relationship between, say, Congress and the states is also somewhat nebulous, but, whatever it is, it is something quite different from that anticipated by the Founding Fathers, and it has been changed legally since the adoption of the Seventeenth Amendment. State legislatures are continually memorializing Congress on various types of subjects, but the relationship certainly goes deeper than such formalities. Likewise, the Council on State Governments has become in effect a "little Congress," which might even be looked upon as a rival in that it has developed new methods for giving representation to the forty-eight states. Sectionalism, certainly, is an important factor in influencing various questions coming before legislative bodies, and one might be interested in the determination of criteria for what could be called "acceptable" sectionalism. Such an inquiry might attempt to determine the methods used to counteract sectionalism in state legislatures, and the extent to which a staff is useful for that purpose.

Pressure Groups

The extent to which, and the methods by which, pressure groups influence legislative bodies deserve further investigation; and, for Congress, the subject should be more readily handled now that additional information is available as a result of legislation requiring the registration of lobbyists. The effect of the rise of group representation has as yet been incompletely comprehended. We know, for instance, that spokesmen for special interests cluster around the areas in which vital decisions are made, both in the legislative and administrative processes, and that special interests are extremely influential in determining what issues should be decided, how they should be decided, and in providing impetus for administrative and legislative action. There is no need to enumerate here the many studies which need to be made of the rise of the group and its impact on representative government. Studies may show the necessity for devising new institutions by which groups can be represented and new

methods for securing more adequate representation of, and within, the various groups.

Continuity

An historically-minded political scientist might be interested in studying the consistency of the process of statutory accretion. It might at first seem that politics has been a process of oscillation backward and forward, but a study might reveal that there has been an over-all consistency in the development of public policy. It would appear that if democracy is to be evolutionary, stable, and capable of dealing with modern problems, it would have to maintain with some consistency even those policies which are hammered out in the heat of controversy. Such a study might also indicate the extent to which wars and business cycles complicate or interfere with the continuity of policy.

Special Problems

A fruitful method of studying legislative assemblies might be the use of case studies relating to particular fields of policies or to particular pieces of legislation. One could, for instance, examine certain aspects of military policy or of foreign policy, determining thereby the interests which Congress had in a particular phase of the policy, the means by which Congress exercised its influence, the policy decisions made by administrators without consultation with Congress, and the questions on which Congress, or some members thereof, was informed. One could also study the genesis of some particular legislation arising out of a social need, the organization of thought to promote this legislation, the process by which the idea became a political issue of sufficient consequence for politicians to take sides, the legislative process by which the idea was enacted into law, the administrative process which interpreted and administered the law, the extent to which the original cause of the legislation was therein remedied, and the development of special interests, or perhaps even the general acceptance of the idea, which protected the legislation from being repealed.

IV. Conclusion

It cannot be gainsaid, I believe, that the successful operation of representative government is one of the crucial problems of our times, and for this reason alone the problem is worth studying. It is worth studying also because there is so much to be studied, there is so much that is not known and needs to be known. For some time, now, our attention has been called to the need for further consideration of legislative assemblies and for giving them an amount of attention somewhat commensurate with that given to the Executive Branch and the Courts. Happily, many individuals and groups are now studying legislative assemblies. But representative government goes beyond legislative assemblies, and the concept of representation is widely used as a device for the organization of other groups. In all of this great movement for increased popular participation in all groups and in the governmental process, we have only begun to realize the scope of the problem of political association, and we have neither described the phenomena completely nor created theories adequate to the needs. The panel has attempted, in a limited fashion, to outline the broad scope of the problem and to indicate some general areas or topics in which research was most needed, but it has shrunk from dogmatizing its findings or from intimating that ideas or areas not mentioned are consequently unimportant. The panel has become convinced, however, that further research in the topic of representative government and the legislative process is highly desirable, if not imperative, and it presents its findings as a modest contribution towards that end.

Chapter III

PUBLIC LAW

Carl B. Swisher

PREFATORY NOTE

SINCE THE DATE of its organization in the summer of 1942, the panel, with a somewhat fluctuating membership, has exchanged ideas through extensive correspondence. On September 9-10, 1944, it met in Washington for more detailed discussion and sharing of opinions. It met again in Philadelphia on March 29, 1946, pursuant to the program of the annual meeting of the Association. The following persons were present at one or more sessions of the conference held in Washington: William Anderson, University of Minnesota; Ben A. Arneson, Ohio Wesleyan University; Robert K. Carr, Dartmouth College; Robert E. Cushman, Cornell University; Marshall E. Dimock, Northwestern University; Oliver P. Field, University of Minnesota; Ernest S. Griffith, Library of Congress; James Hart, University of Virginia; Charles S. Hyneman, Northwestern University; Earl G. Latham, University of Minnesota; Harvey C. Mansfield, Ohio State University; Joseph McLean, Princeton University; C. Herman Pritchett, University of Chicago; Carl B. Swisher (chairman), Johns Hopkins University; and Benjamin F. Wright, Harvard University. The following additional persons have participated in correspondence: Charles A. Beard, New Milford, Conn.; Kenneth C. Cole, University of Washington; John Dickinson, Pennsylvania Railroad, Philadelphia; Charles Fairman, Stanford University; Charles G. Haines, University of California at Los Angeles; Robert J. Harris, Louisiana State University; Karl Loewenstein, Amherst College; Alpheus T. Mason, Princeton University; John M. Mathews, University of Illinois, and Spencer D. Parratt, Syracuse University. Unfortunately, no adequate record was made of the persons in attendance at the meeting in Philadelphia.

Public Law

If the wide range of opinions and attitudes is a sound criterion, there exists a high degree of individualism in the field of public law. This is the result, no doubt, of differences of initial training and experience and habit of mind, and also, perhaps, of the comparative isolation one from another which characterizes the working conditions of the representatives of the field.

The correspondence and the oral discussions already held have dealt with too many problems to make possible a report of widespread agreement. The following pages of summary, therefore, represent, not a statement of findings agreed upon, but rather statements of the trends of thought which have been presented with varying degrees of vigor in the letter and oral comments of varying numbers of members of the panel. The findings that follow are offered, not as group conclusions or official recommendations, but as suggestions for further thinking on the part of the panel and other members of the profession. Where important differences occurred, they will be noted as themselves constituting a challenge to further research and analysis.

The panel encountered initial difficulties in the way of harmonious thinking in characteristic indefiniteness and lack of agreement as to what is meant by the title of our field, namely, "Public Law." Some members use the term as if it were synonymous with constitutional law, and then speak of both public law and constitutional law as if their content were limited largely to opinions of the Supreme Court. The tendency toward an inadequate conception or an over-restricted use of basic terminology persists and seems to affect the thinking of participants, even though, when definition is attempted, every member agrees that more territory is included than is generally held in mind when "public law" is discussed. The panel members have hesitated to engage in a prolonged effort toward the definition of terminology because of awareness of the futility which often characterizes such attempts. They know that definition is often a device for substituting memorization for thought and that undue subservience to definition often leads to the frittering away of time and energy in undue preoccupation with boundary

lines. They believe that research should proceed in terms of topics and should follow the topics wherever they lead, largely without reference to the boundaries of academic fields. Most of the considered attempts at definition, therefore, have proceeded, not in terms of detailed analyses of content, but rather in terms of lists of subjects to be included or areas to be explored. One of the definitions offered reads as follows: "For purposes of this inquiry, public law should be defined by the process of inclusion and exclusion. It should comprise constitutional law, administrative law, the law of municipal corporations, and such courses in labor law and the law of the regulation of business as are finding their way into the curricula of American colleges and universities. I see no advantage in trying to formulate a theoretical definition of public law by isolating its distinguishing characteristics." Others have submitted statements varying somewhat in the lists of subjects included, but they seem to agree in large part as to the lack of advantage in trying to formulate a theoretical definition of public law by isolating its distinguishing characteristics. They would probably agree with the comment of a member that public law is coëxtensive with constitutional government, but their agreement might well turn on the fact that the comment is conveniently vague.

It was only after oral discussion was well under way that talking at cross purposes disclosed the need for closer agreement as to the use of terms or, at any rate, for more precise disclosure of individual meanings in the use of terminology. Although the panel has not reached agreement in the matter, it has struggled with the task of analysis. In the course of discussion, members of the panel have harked back to conventional definitions and have called attention to the fact that public law reaches throughout the entire realm of rules and regulations which give power to and restrict the activities of governmental agencies. In this connection they would emphasize the fact that a creative approach to public law requires analysis of the giving of power rather than exclusive preoccupation with those things which law forbids. One member stresses the fact that public law is both a method and a subject-matter field. As to method, he suggests that the defining term is "legal." As to public law as a subject-

matter field, he finds that that field is predominantly the judiciary; how firmly he would restrict the field to the judiciary is not clearly disclosed by the record. Other members insist that the judiciary is by no means the entire province of public law, but rather that the field includes the making and interpretation of rules for the guidance of the conduct of governmental agencies, wherever those rule-making and interpreting processes take place.

One member believes that the absence of clear conceptions of the basic characteristics of our field is explained in part by the fact that we seem to have lost awareness of the basic substructure of law which underlies any governmental system. We need to keep in mind the essentials of a good course in principles of jurisprudence. Explanations of the reasons for our loss of awareness of the basic substructure of law, if indeed we have lost it, are various and fragmentary. There is some suggestion that individual members have been too deeply engrossed with analyses of particular judicial decisions or lines of decisions or with particular judicial personalities to relate their sub-fields of specialization to the essential aspects of the broad field of public law. Perhaps we have been too obsessed with the rapidity of governmental change in the course of our current experience to be able to comprehend the relatively stable legal structure which is assumed to lie behind that experience. Perhaps we have been too much impressed with superficial generalizations in the field of public administration, which is characterized oftentimes by lack of respect for law and by scorn for precedent. At any rate, whatever the reasons for the current state of our thinking, a number of members of the panel are convinced that some hard thinking about the essence of our subject-matter would have great value for our profession.

Some members of the panel take the position that effective work in public law requires broad grounding in political theory and that ineffectiveness of teaching and writing in the field are often to be explained by lack of such grounding and by lack of concern about fundamentals. One member, for example, insists that mature scholars in the field must know where they stand with respect to essential values: "I think that any student of

public law who has grown up ought to make up his mind whether he values what I call democracy and how much he values it. Similarly, he should determine whether, and how much, he values (or most people value) certain other things which are very important. When he has made up his mind on these things as fully as he can, then his writing and teaching in the field of public law should show the relation of constitutional and other fundamental understandings and ways of acting to these things which he has decided he (or most people) value. If a man has not made up his mind as to what he thinks of representative assemblies as policy-making agencies, I do not see how he can write intelligently about judicial control of constitutionality. I have an idea that our neglect of the study of state constitutional law is due in large part to the fact that very few, if any, American political scientists have made up their minds as to what they think is good in the matter of dispersing political power as against concentrating it in the national government."

Another member stresses the necessity for revitalization of political theory, particularly as it relates to conceptions of liberty. He finds that the theory now in vogue in that field is barren, outdated, and pitched on an abstract and superrationalistic plane. He suggests the need for a reclassification of different liberties in terms of common sense categories. The fact that theory in any field may be a rationalization of the desires of the theorists must not be permitted to obscure the importance of theory as basic to an understanding of law.

One member suggests that the failure properly to integrate even judicial materials in terms of an adequate body of theory may be the product of over-preoccupation with the judiciary, as a result of which there has been a general failure to go beyond technically judicial conclusions. Certainly, says another member, students of public law, in addition to mastering judicial interpretation of the rules of law, should make a continuing study of the traditional quest for certainty in law as it clashes with changing conceptions of justice.

Insistence on the importance of political theory as a basis for public law is accompanied by warnings that theory, to be of value, must be "realistic"— must grow directly out of the stuff

of law and politics. Members have voiced sharp criticisms of the abstractness and mysticism, of the apparent lack of relation to actual operations of government, which characterize much of the work of German political theorists—work which during the past century has been highly influential in some quarters in the United States. Similar objectionable characteristics are found in such works as *Political Science and Comparative Constitutional Law* by John W. Burgess, *The State* by Woodrow Wilson, and the more abstract writings of W. W. Willoughby—whose work, however, has warm defenders on the Panel.

The initial steps toward organization of the panel on public law, with the avowed purpose of giving some guidance to research in the field, provoked varying reactions as to the possibility and desirability of attempting such guidance. One of the men consulted took the following position: "I've always been very skeptical—to say the least—of any ambitious scheme for blocking out needed research to be done by others, especially in the field of constitutional law. Real research there is still in its infancy—the canons of scholarship are not firmly set—we are not agreed on what is significant or how to go about our investigations. My feeling is that for a good time to come it is better for every man to do his own best on what he thinks important. It will take considerable time to make sure what sort of inquiry has lasting value." Another member asserts that the directions of his own research in years past have been determined largely by accident; he believes that the end products have been better than they would have been had he been subject to a degree of coercion in the selection of his fields of endeavor. Still another emphasizes the "divine flash" of inspiration which leads the individual researcher to projects which for him have a maximum of fruitfulness.

Without questioning the value of "divine flashes" and the desirability of preserving opportunities for yielding to their guidance, other members call attention to the fact that external guidance already plays a considerable part in determining the lines along which research will be done. The minds of graduate teachers are supposed to be repositories of long lists of potentially fruitful dissertation topics; if their lists are short and their con-

ceptions narrow, the research of students doing work under their direction is likely to be handicapped. It is pointed out that as a matter of fact the annual list of dissertations in political science shows an immense amount of repetition and triviality. It is suggested also that coercion of a sort lies in the fact that most researchers are limited, not merely by the intensity of their divine flashes, but by the availability of research materials, the opportunities for publication of studies of particular kinds, the availability of research funds for particular purposes, and various other factors. Furthermore, great advances have been made in medicine, chemistry, and physics through "directed" research financed by industry and foundations; and in economics, institutions such as Brookings and the National Bureau of Economic Research have done important work by means of directed research. Therefore, while it may be assumed that no member of the panel would be willing to have pressure put upon any member of the profession to do research which he was unwilling to do, a number of members feel that research in public law would be greatly enriched if we could have at least a fuller interchange of ideas about fruitful projects which might be undertaken. Attention has been called to research outlines which are already worked out or in progress in a number of fields, with the thought in mind that such an outline might possibly be developed for the whole field of public law.

Most members of the panel take the position that research in public law should as a rule not be narrowly legalistic in character. For the highly technical task of discovering exactly what the law is and what courts will probably decide in a particular field, law professors have superior equipment. On the other hand, political scientists have training and experience in exploring relations between law and government, both historically and in present-day operations—a task at which most lawyers are notoriously inept. The approach of the political scientist in the field of public law should be that of one interested in all the phenomena, whatever their kind, which aid in molding public law and are in turn governed by it. Successful research in public law will depend in part upon adequate training in the techniques of law itself—a field in which our equipment often leaves much to be desired—

but it depends even more upon the integration of the techniques of law with those of politics, psychology, economics, history, and other disciplines.

Members of the panel recognize that legitimate research in public law needs to be done at many levels. There is need for highly specialized investigations which can be done only by experts and which will yield results intelligible only to experts. There is need also for conventional types of factual studies to bring up to date accounts of happenings in the field and to accumulate kinds of information not previously available. Beyond the accumulation and the presentation of facts, however, there is need for that kind of research which consists largely of interpretation and synthesis. In the face of current need for significant illumination of facts, it is hardly surprising that people who fail to get that illumination from us are intolerant of what they regard as our waste of time upon logic-chopping over technical judicial decisions. Members of the panel do not insist that every member of their profession must be a skilled popularizer and persuasive advocate in order to justify his membership. They do insist, however, that the welfare both of the profession and of the thinking public requires that the profession shall include able men with capacity to interpret facts in terms of values and to make interpretations intelligible to intelligent people. They contend that the science of public law, if it is to justify itself, must be a science for the development of tentative conclusions as to what ought to be, as well as a science of organization and discovery.

All members agree that justifiable research in public law must be "useful," but they have not worked out and agreed upon criteria of usefulness. Some of them probably regard research activities principally as a means of preserving the intellectual keenness of teachers; for them, therefore, the research done should obviously be that which is most stimulating to the investigator, whatever the interest or lack of interest of students and others of the profession in the particular topic. Others feel an obligation to add to the information or understanding of the profession or of the public by their research, but still vary considerably in their judgments as to what kinds of research will achieve these ends. Some members, perhaps most of them, feel that the

approach should be predominantly historical and that our impact on government in the future should be largely in the form of clearer illumination of the past. This view is rejected by one member, however, in the following statement: "I am inclined to believe that all of the major fields of political science have had too much of the historical and analytical approach and too little of the prospective or constructive approach. . . . In my opinion, the political scientists ought to be giving a good deal of time and thought to the development of criteria for effective governmental action, as well as to ways and means of establishing the most effective governmental agencies to define and develop these criteria."

In the discussions of the substantive fields of research in public law, the judiciary has naturally received a great deal of attention. However, as stated above, members of the panel feel that political scientists ought not to deal with the work of the judiciary in the same legalistically technical fashion as that of the legal profession itself. Many great constitutional decisions are to be understood in their broader significance only after careful examination of the handling of the cases in lower courts and of the situations out of which the cases arose. Others are to be understood only in terms of periods of development which involve the interplay of innumerable factors not primarily legal in character. Others have still other facets which require illumination for adequate understanding by students of government. It should be the task of political scientists in the field of public law to do the research and provide the illumination.

Some members of the panel feel particularly strongly that research by immature scholars in pursuit of higher academic degrees should not be done at the heart of constitutional law in the hope of shedding direct light upon fundamental principles. The reason for the attitude is that the subject is too intricate for mastery in the brief period of graduate education and that the overambitious selection of a topic is likely to result in a poor product and may discourage an otherwise promising student in an important field. The immature scholar ought first to master the subject by study under guidance of instructors and by oblique approaches in independent research, such as the study of historical

backgrounds of constitutions or constitutional principles or decisions, or the evolution of administration and administrative law as parts of a constitutional structure, or biographical portrayal of judges. Many of the defective streaks which appear in our literature are the product of over-optimistic choice of research topics by young people not yet sufficiently well-trained to handle them. It is to be emphasized, however, that, in offering this warning about the selection of research topics, panel members do not desire to discourage youthful attempts at generalization about important phenomena. The early development of the capacity for disciplined generalization is vital to the intellectual health of our profession. The purpose of the warning is to promote the selection of research topics on which youthful generalization may reasonably be expected to have validity.

While it has been suggested that biographical and statistical approaches to the work of judges are the products of an era wherein belief in the stability of law is at low ebb and that the use of these approaches ought not to be overdone, the members of the panel recognize the value of such studies. Much of the value of the biographical approach lies in the fact that judges are sometimes the sources, and are at almost all times the purveyors, of ideas which find permanent place in constitutional law. While students are warned against expecting too much in the way of interpretation or prediction of judicial action on the basis of early training and association, the flow of influence has in times past been demonstrated with sufficient persuasiveness to justify the belief that this type of research ought to be continued. The study of "average" justices, either individually or *en bloc*, may be of a value comparable to the value of the study of the few who are particularly outstanding. The careers and the judicial opinions of judges of some of the state courts and of the lower federal courts may offer rich opportunities for research. As to those justices who are outstanding, and who seem to have well-integrated philosophies with respect to the subject-matter of public law, it is contended that studies of such philosophies have value, even though they do not extend to the preparation of full-length biographies.

One member of the panel suggests that the opinions of the

justices should be viewed, not merely in terms of their immediate effects in molding law, but also in terms of their broader influence upon the conduct of other agencies of government and upon the thinking of the people. He calls attention to "the strong encouragement that the Marshall opinions gave to the cause of central power both in his own time and more or less continuously ever since. I wonder, for instance, how many times his 'Let the end be legitimate' passage from the McCulloch decision has been quoted with approval on the floors of Congress in the heat of debate over controversial bills. I think we might well encourage more research along these lines, particularly the influence of Supreme Court decisions upon Congressional debate and action."

Another member emphasizes the importance of the personal attitude of each individual judge toward the judicial game. By way of illustration, he refers to an analysis of the judicial work of Justice Roberts which leads to the conclusion that the Roberts approach to the judicial task was thoroughly legalistic. His preoccupation with a case was in terms of its character as a case. He was a "lawyer's lawyer." Justice Frankfurter has had an attitude toward the judicial process which has made his work hard to appraise by those who over-simplify analysis by classifying decisions largely in terms of liberalism and conservatism. Justice Harlan, in an earlier period, displayed a raw kind of realism in his approach to cases. The biographer must take into account the personal approach of his subject and must also attempt to understand more clearly than is usually understood just what a judge of a higher court does when he decides a case and what he attempts to do when he writes a judicial opinion. If it is true that the great task of the Supreme Court is that of creative preservation of the constitutional system, our judicial biographers have not yet sufficiently related the work of the judges to that task. In this connection, a panel member suggests that it is the function of the Supreme Court to spin juristic theories of politics in such a way as to act as a broker between ancestry and posterity, to adjust to change while maintaining continuity of principle.

Fields of research partly or wholly outside the judiciary have been discussed less fully. Members of the panel suggest that

modern federalism, which is peculiarly the contribution of the United States to the science of government, is worthy of major study by a competent author or authors, over and above the incidental treatments which it receives in general studies of American government. Much the same position is taken with respect to the separation of powers. Although the device was not originated on American soil, its development here has brought out unique characteristics of basic importance. In the field of the executive, attention has been called to need for further study of the development of that body of law which flows from the decree-making power of the President. The activities of the Attorney-General as a molder of law and those of other legal advisers of government are worthy of study. The process of law-making in the independent regulatory commissions and in many administrative agencies of the executive branch of the government likewise provides a fertile field of investigation. The making by the Department of Justice, under anti-trust laws and related measures, of what are virtually rules for the operation of enterprise is a significant process within the scope of our study of the all-pervading characteristics of public law.

The development of standards of administrative operation is particularly important. Concerning the Office of Price Administration, for example, there has been a legislative requirement that prices fixed shall be "fair and equitable." The process of giving content to such a phrase is a law-making process comparable to that in which the judiciary is constantly engaged. Materials are available for contemporary analysis of this and similar practices in the development of administrative law. Because of the extent to which this process is in ferment, study of it may yield even richer results than study of the work of the courts. Settled habits and lack of familiarity with the new subject-matter ought not to be permitted to stand in the way of promising research.

Again, the legislative branch of the government is also regarded as a fertile field for the study of the development and application of public law. Congress at times sits almost as a constitutional convention for the adoption of "constitutions" to create and to guide huge governmental agencies—agencies which themselves enact extensive codes of law with respect to the phe-

nomena over which their "constitutions" give them jurisdiction. All three branches of the government participate to some extent in the development of international machinery and rules of public law. The establishment of the United Nations, of UNRRA, and of the European tribunal for the trial of war criminals provides fertile fields for important research and generalization.

One member of the panel suggests that significant analogies for the study of public law are to be found in the relations of corporate organizations which in the technical sense are not states and do not exercise formal governmental powers. Agreements between giant corporations in the United States, and between corporations in the United States and others in foreign countries, have many of the characteristics of treaties between governments. They represent the stabilization of conduct by rules which closely approximate law, and this "law" emanates from organizations of such size and power that it bears a close resemblance to public law. The subject-matter becomes all the more important when the "public laws" of corporations clash with and frustrate the administration of the public laws of orthodox states. Conflicts and relationships now exist or are developing which may have far-reaching influence over the orthodox public law of the future. The study of these and other subjects not ordinarily regarded as within the immediate province of public law can do much to illuminate the characteristics of law and the place of law in the conduct of public and quasi-public organizations.

The panel has considered public law both from the point of view of the creative aspects of the subject and from that of the restrictions which law places upon the government and upon the people governed. The subject of civil liberties has come constantly to the fore. It is considered important, not merely in terms of conventional studies of judicial decisions affecting liberty, but also in terms of statutes bearing upon it, administrative decisions affecting it, and decisions concerning it which are made within private agencies such as labor unions and corporations having government contracts, agencies whose power is greater because they receive support from government. A detailed analysis of this subject is expected to appear in a research outline now being prepared by Robert E. Cushman. The ethical

problems involved in activities in the various fields of public law are the subject of constant reference. If the representatives of public law who were consulted have ever been of the impression that their branch of political science could be treated as analogous to natural science in the sense that their subject-matter could be considered without reference to values, that frame of mind has been almost completely banished. A feeling seems generally prevalent that a concern for higher welfare ought to give tone to the work of the profession.

Apart from its preoccupation with the subject-matter of public law, the panel has given thought to problems of personnel. As in many other fields, the current rate of infusion of new blood of high quality leaves much to be desired. Causes of the slow rate of the replenishment or increase are varied and not easily appraised. The flow of new personnel tends to follow dominant current interests. One member, referring to an alleged decline of interest in constitutional law, concludes pessimistically that nothing can be done about it. Political science, he says, "is altogether devoted too much to ephemeral pursuits, with the result that political scientists tend to react to a very considerable extent to the latest fads in the profession. This has been true since efficiency and economy constituted a fad in 1912 until today when war courses and courses on Latin America have become fads. After the present war, courses in international relations and organization are likely to become even more successful than they were in the twenties. Until such time, therefore, as public events bring constitutional law to the fore, any efforts to stimulate it as against present-day trends are likely to prove unsuccessful."

Certain it is that the opportunity for employment, the availability of research funds, and the lure of opportunity for the exercise of power have taken into the field of public administration men who might otherwise have sought to master the discipline of public law. The lure of the natural sciences undoubtedly draws into the nation's laboratories others whose abilities are greatly needed in a field devoted to the informed analysis and appraisal of the structure and systematic operations of government. Other men with similar potentialities are captured by the world of business. The intervention of the war drained away

temporarily, and perhaps to some extent permanently, important resources in man-power. The fact remains that, as public law has always had outstanding men among its personnel, so it still has them, and may be expected to have them in the future. The attraction of new personnel, it is true, will depend in part on the performance of present mature practitioners. We need, not the display of the characteristics of professional prima donnas, but the conscientious performance of writers and teachers who can develop insight and impart enthusiasm.

Chapter IV

PUBLIC ADMINISTRATION

William Anderson

PREFATORY NOTE

THE RESEARCH COMMITTEE of the Association did not include the field of Public Administration among its panels. In 1933 the Social Science Research Council established a Committee on Public Administration and this Committee functioned continuously and effectively for eleven years thereafter. Its final report, "Research in Public Administration," was published in 1945 and contained a notable chapter, "Recommendations for a Post-war Research Program in Public Administration," which fits in so well with the Panel Reports that we reprint it here just as it was printed then, dated though it is.

The members of the Committee on Public Administration (1933-45) were: William Anderson, University of Minnesota, 1933-45 (chairman 1939-45); George C. S. Benson, Claremont College, 1940-45; Louis Brownlow (Retired), 1933-45 (chairman, 1933-39); Adele Clark, 1935-38; John Dickinson, University of Pennsylvania, 1933-39; Harold Dodds, Princeton University, 1933-39; Rowland Egger, University of Virginia, 1938-45; George A. Graham, Princeton University, 1940-45; Luther Gulick, Columbia University, 1933-45; E. Pendleton Herring, Social Science Research Council, 1940-45; Lewis Meriam, The Brookings Institution, 1933-45; Lindsay Rogers, Columbia University, 1933-45; Harold D. Smith (deceased) 1933-38; Leonard D. White, Chicago University, 1933-45; John H. Williams, Harvard University, 1938-39; George F. Yantis, 1935-45.

The members of the Committee on Public Administration feel that they would fall short of discharging their full responsibility if they were to file this comprehensive report without making recommendations for the future. Since 1933 this committee,

through many changes in membership and in operating conditions, has kept itself close to the stream of developments in its field. Its members have had opportunities to see the needs for research in public administration as perhaps no other group in the nation. They believe that the importance of public administration cannot easily be overstated, and that the postwar years must bring increased attention to the field. Much useful research has already been done, and some of it very well done, but far more is needed. The following suggestions are put forward, therefore, with earnestness and conviction, and with a deep-seated faith that the leaders of thought and the promoters and supporters of research in the social sciences in this great nation of ours will insist that so important a field of study must continue to receive encouragement. We, therefore, recommend:

I. Implemental Proposals

1. That there be provided some continuing agency in this field with staff sufficient at least for research planning and promotion, for holding research conferences, for performing important liaison functions between social scientists, public officials, universities, and research agencies, and (under special grants and for adequate reasons) for conducting occasional research under its own auspices.

2. That arrangements be made in some way to establish a center for the public affairs aspects of the social sciences in Washington as a means for promoting the close relations that are needed between the government and its administrators, on the one hand, and the universities, research institutes, social scientists, and students of public administration, on the other. The Social Science Research Council headquarters at 726 Jackson Place have already proved to be of great value, and the Committee on Public Administration believes that even greater benefits to the government and to the social sciences could be expected from such a center as is proposed.

3. That the reporting and publication of cases in public administration, as already well begun under the committee, be continued in some form with such improvements as may be devised. It is not necessary that this be sponsored by a new Com-

mittee on Public Administration. The important thing is that this promising and well received project be encouraged to go forward.

4. That all support possible be given to bring about a continuance in the postwar period of the official records work so well established in Washington in wartime under the Bureau of the Budget, its Committee on Records of War Administration, and the many governmental agencies that created special historical or records divisions. The importance of this work for historians, political scientists, economists, and other social scientists, as well as for the government and the nation generally, hardly needs to be emphasized.

Where necessary, also, it is important to fill gaps in the records; especially in periods of great administrative changes and inventiveness, efforts should be made to promote unofficial capture-and-record projects.

5. Education for research in public administration and training for the practice thereof (which are closely related matters) should be a subject of continuing concern. The war has upset all the university graduate schools and the public administration training programs rather seriously, but it has also provided a severe testing of the young men and women trained for public administration and for social science positions in the government in the pre-war years. Perhaps the time has come for a careful appraisal of this wartime experience and for a conference on post-war improvements in public administration training programs. Closely related to these matters is the problem of the social scientists in the government service—their status, their rewards, their opportunity for rendering their best service to the nation as social scientists.

These questions must not be allowed to go unanswered. They affect too deeply not only social scientists and public administration but also the entire nation.

II. A Research Program

The spring meeting of the Committee on Public Administration in 1944 was devoted almost entirely to a discussion of research that would be needed in the years following the war. Be-

hind this discussion lay a long series of other sessions at which subjects for investigation had been raised and discussed in a preliminary way. The following suggestions are the summing up of the major topics and fields—where research is still an urgent need—that the Committee has approved for study at one time or another.

It would have been easy at this time to have selected a number of topics dealing with demobilization and reconversion and and to have placed these in the foreground of the program. The committee is not without interest in such emergency matters. In the main, however, these problems must be handled by agencies of government in the light of political and military decisions to be made in 1944 and soon thereafter. The committee's proposals contemplate a longer stretch of time even when occasional reference is made to present-day urgencies. Indeed, much of the research program herein outlined is of enduring and continuing interest through times of peace and prosperity as well as wars and depressions.

Furthermore, an attempt has been made to give real breadth and depth to the proposals. The study of public administration has been visualized as the study of government in action, government doing work and rendering public services. Thus public administration is an important phase or function of all governments at all times. Although for convenience much of the work of legislatures and the ordinary courts is left out of the field of public administration, even these bodies have administrative problems; and, in addition, their work impinges at so many places and in such important ways upon the administration that public administrators must also study these contacts and relationships.

Public administrators and students of administration are aware, also, that public administration is closely intertwined with, and dependent upon, the nation's political institutions, traditions, and ideals in a most general sense. Public administration is not something set apart from, but is an integral part of, the whole system of popular government and democratic ideals. A major practical objective of research in public administration, therefore, is to discover ways in which it can be made to serve and

to strengthen the ideals of fairness, justice, and civil equality that make up so large a part of the best American tradition. To administer is, by definition, to serve. Consequently it is necessary in all research and action in this field to keep these ideals in mind. In the following research proposals, therefore, government in accordance with law, based upon discussion and majority consent, and with an eye to promoting the general welfare while preserving and protecting the dignity of the person and the rights of individuals, is assumed as part of the background.

1. *International Organization and Administration.* Leaders in thought and public policy throughout the United Nations and in many neutral countries appear to have agreed upon the need for a firm international organization to keep the peace and to promote the welfare of all. Such an organization already exists in part among the United Nations, and a more comprehensive scheme is now being formed. The latter will include judicial organs and even a sort of international legislative authority, but we should not expect much immediate result from these. "International legislation" cannot for a long time be anything like domestic legislation.

When that time comes, there will be all the usual problems of administration in connection with enforcement of the laws. But long before that, international administration will be important. Indeed, it is so right now. Several agencies that preceded the League of Nations, several more that were parts of its organization, and a new wartime series of United Nations joint and combined boards are already at work. Acting in comformity with the wills of the nations concerned, they are engaged in administering many international agreements concerning shipping, supplies, finance, labor, agriculture, postal affairs, the conduct of the war, military government in occupied areas, international relief, health, and other matters. Some of these will probably be woven into the over-all international organization, but, until that time comes, they go on meeting in their own way all the problems of public administrative agencies. Questions of organization, personnel, budgeting and finance, interagency relations, central and field offices, relations with component governments —these and many others are but parts of their regular routine.

The studies heretofore made of the administrative aspects of international cooperation have been incomplete in their coverage and necessarily tentative in their results. Students of international organization are now agreed that studies of the administrative side of international agencies must rank high among studies in this field in the future. The Committee on Public Administration is definitely of the same mind.

2. *Federalism and Intergovernmental Relations.* The Committee on Public Administration has already done considerable exploratory work in this important area and has issued a research outline covering much of the field. With the nationalization of commerce, finance, and industry and the organization of agriculture, labor, the professions, and many other groups on a national scale, the central government was inevitably called upon to increase its controls over the economy and its services to the people. The effects upon state and local governments have been exceedingly important, although not in all cases what is popularly assumed. Many new devices for cooperation between the national and state governments and among the states have arisen and been tried. Similar experiments in the relations between state and local governments, the national and local governments, and among the local units themselves have resulted in adding to the complexity and the interest of intergovernmental relations.

Up to now the research in this field has left many important subjects largely untouched. When serious tensions have developed, as in recent years, between the states and the national government over taxes and particular functions, an up-to-date comprehension and theory of the federal system as a whole has clearly been lacking.

The administrative phases of intergovernmental relations are, of course, one of the special interests of the Committee on Public Administration. Among the specific topics involved are the handling of grants-in-aid and other intergovernmental fiscal relations; the joint use of personnel; cooperative and contractual arrangements for the performance of common services; other arrangements for division of labor among units of government; exchanges of information and personnel; the administrative

settlement of intergovernmental disputes; central standards for local administration; and central supervision over the work of local agencies. These topics suggest something of the richness of the materials and of the great changes that have been brought about in recent decades in the relations among national, state, and local governments.

Most Americans seem to accept the need for a strong central government, but at the same time they wish to preserve all the advantages of self-government in the state and local units. Whether that is the direction in which the nation is moving cannot be determined without a great deal more research in the field of American intergovernmental relations.

3. *Legislative-Executive Relations.* Administrative agencies operating under legislative authority, usually directed by, and responsible to, the executive (president, governor, mayor, or city manager) and checked in much of what they do by the courts, are necessarily much interested in the relations at the top among the three great branches of government. The people, too, have reason to be greatly concerned about these relations. The adverse effects of poor organization and friction at the top may be reflected in undesirable ways in the administration all the way down to the humblest servant of the public. Charged with the duty of putting legislative policies into effect, the administrative agencies cannot do their work properly unless both the legislature and the chief executive have done their work well and with a fair degree of harmony.

Hence arises the interest of public administrators in executive-legislative relationships. Many specific questions are involved. What is the role of the executive and of the administrative agencies in formulating policy? in advising the legislative branch and supplying it with information? in drafting legislation for enactment? How can the administrative agencies do their work properly in the making of rules to supplement and to carry out legislative enactments? How can harmonious and cooperative leadership be provided for the administration without loss of the advantages that come from criticism of each other by independent branches of government? How can stalemates be avoided? How can legislators be brought to appreciate the work of administra-

tors and to assist them in the better performance of their duties? What kinds of reporting, liaison efforts, and contacts can be worked out to this end?

4. *Relationships within the Administrative Branch.* The multiplication of administrative functions and agencies—national, state, and local—has resulted in creating numerous problems of internal relationships within the administrative branch as a whole. These are most noticeable in the national government, in the leading states, and in the large cities. (A distinction needs to be made, of course, between these intra-administrative relationships within a single government and the inter-governmental administrative relationships suggested in section 2, above.) Some examples will show what is meant.

(a) *Relations between Central, Regional, and Field Offices.* In the national administration something like 75 per cent of the officers and employees of the government are located away from Washington. Each major agency of the government has its own field organization, which consists in many cases of two levels—the regional offices, covering large areas, and the district or field offices serving the smaller areas into which the regions are divided.

The larger states have something like the same problem in their administrations, but they more commonly use a two-level instead of a three-level arrangement (i.e., they do not have regional offices between the central and district offices).

How to organize and conduct the relations between the central, regional, and field offices of the national administration has become a major issue, and one that is found in almost every important agency. What the regions and districts should be, what officers and organization they should have, what authority should be delegated to them and how, what standards should be set up at the center for the performance of the field services, how these standards should be enforced, how the channels of communication can be kept open in both directions between central and field offices—is it necessary to list more topics to show what is involved?

It is only in the last ten or fifteen years that these questions have been seriously raised. Up to now there has been altogether

too little investigation even to bring out the essential descriptive facts.

(b) *Inter-Agency Relations at the Top Level.* The proliferation of government agencies that has been necessitated by the growth of public services and the necessary specialization of activities in order to attain higher standards and efficiency has brought new problems of inter-agency relations at the top level in national and state captials. New agencies have been created and old ones have been modified and expanded of the following major types:

(i) Overhead, "staff," and "housekeeping" agencies to handle problems of budget, accounting, personnel, research, planning, and other services for all or many of the government agencies.

(ii) Ordinary operating or "line" agencies that render services directly to the people—in defense, foreign affairs, education, highways, agriculture, labor, social welfare, and many other fields.

(iii) Semi-independent government corporations and authorities (such as the Tennessee Valley Authority, housing and credit agencies) that also provide direct public services but stand somewhat outside the ordinary administrative hierarchy.

(iv) Semi-independent regulatory commissions and boards that serve as agencies of both the legislative and executive branches in broad fields of regulation, such as railroads, communications, power and utilities, labor relations, and grain, cattle, and stock exchanges.

A thoroughly acceptable set of principles for inter-agency relations at this level has never been worked out.

(c) *Intra-Agency Relations among Bureaus and Functional Activities.* Much the same questions appear at a lower level among the different bureaus, divisions, and offices in major departments. Lines of contact run in almost every direction from every unit in an agency to every other. For example, there are the relations of the personnel agency of a department to the department head, to the budget unit, to the planning unit, to the operating units, etc., and each of these in turn to every other. Up to now we have not had sufficient descriptive studies of good

quality even to lay the basis for an adequate set of principles in the field.

(d) *Relations in Particular Functional and Professional Fields.* The personnel unit in a major agency, to take one example, not only has important relations inside the department but also has relations that extend upward and outward to the civil service agency of the whole government and to the personnel advisers of the chief executive. Inside the agency its contacts extend to all appointing officers and others who handle personnel matters, including the regional and field offices. Thus if one follows through a single function like personnel he can see the agency personnel office as the center of a network of relations with others who deal with different aspects of the same function. A similar approach could be employed for considering the legal unit, budget, fiscal and accounting units, and research and planning units.

Indeed, one of the best approaches to the studies that are needed in administrative organization and procedures might be to take separately each important function or activity and trace out all the relations affecting it in every direction. Some illustrative questions might here be apropos:

What is the role of the legal staff and the various legal units in administration? Has the lawyer found his appropriate function, and, if so, what is it?

What is the relationship of research and planning units to each other in the same government, and to operations, to budgeting, and to personnel?

Where should budgeting be done—only at the center, or in regional offices, or at the field office and district level?

What are the limits of successful integration and centralization in such a function as personnel or planning? What parts of either of these functions can be best performed by a central agency for the whole government? What parts should be performed in the department, at the bureau level, and in the field?

5. *Over-All Aspects of Modern Administration.* The Report of the President's Committee on Administrative Management seven years ago led to important changes in the national admin-

istrative organization in the years before the war. Since then there have been further important developments in the administrative arrangements of both national and state governments. Post-war demobilization and reconversion will bring still other changes.

For these reasons the time may soon be at hand for new studies of over-all administrative problems, primarily in the national government, but also in the leading states and cities. The burdens upon the chief executive, the provision of his office with adequate staff aids for supervision of the administrative branch, and the means for legislative control, for example, still present problems for investigation.

In addition to these problems, the frequent attacks upon the operation of public administration as a whole, upon the merit system, and upon what is called "bureaucracy" suggest the need for an inter-disciplinary investigation of the modern civil service as a whole. It is very evident that there must be certain maladjustments, frictions, and tensions resulting from the rapid rise of what is called "the public service state." Students of history, economics, psychology, social psychology, and sociology, along with political scientists and public administrators, might conceivably contribute a great deal from their several viewpoints to such a general study.

6. *Local Government and Administration.* In this field attention needs to be given especially to the administrative problems of the great city and metropolis, to the size, adequacy, and efficiency of various types of local administrative units in both urban and rural areas, and to the relations between land planning problems and the functioning of both urban and rural units of government. Hovering over all units of local administration is the spectre of fiscal breakdown and dependency. Certainly we know too little about these matters to be able to propose a defensible program of public remedial measures.

7. *Fiscal Policy and Administration.* This is an area in which the student of public administration must accept the leadership of economists and public leaders on all matters of policy but in which public administration may have an important instrumental function. If the fiscal powers of government are indeed to be

used to achieve "full employment" and to level peaks and valleys of the business cycle, some very effective public administration is going to be necessary. The coordination of policy with administration, the arrangement of devices for quick, compensating action when upward or downward trends set in, the planning and handling of schemes of useful employment, the budgeting of funds and projects, and many other matters of like import will fall largely upon the administrator. Some cooperative studies for economists, political scientists, and students of public administration would seem to be very much needed in this field.

8. *Government and the Economic Order.* Closely related to fiscal measures is the whole gamut of relations between government and economic activity. Whether for better or for worse we know not, but everyone can see that modern governments assist, promote, standardize, regulate, and police commercial and industrial activities in countless ways. These measures are brought to a peak in times of war and great depressions, but they are always present in considerable volume. Labor and agriculture in all important aspects are brought within the purview of government along with the owners and managers. Corporations are watched with special care, while trusts and monopolies are in large part forbidden. In significant instances various governments—national, state, and local—enter into active ownership and operation of essential utilities and industries.

Scattered and piecemeal studies in this wide expanse of problems are going on all the time, but nowhere does one find a comprehensive program of such investigations. One of the difficulties is that these studies fall between several disciplines—economics, law, political science, and, in the last named field, especially public administration. A better union of efforts may be called for, but at the same time the administrative aspects of this area call for special emphasis. Relatively much more has been done on the legal and economic phases.

Administrative studies are needed to cover the organization and operation of public regulation with respect to prices, rationing, priorities, and allocations in wartime; railroad and utility regulation; the enforcement of fair trade practices; the regula-

tion of insurance, banking, industrial relations, wages and hours, professional practices and fees, commerce, weights and measures, and other similar matters.

Such studies need to be made not only from the viewpoint of the regulatory agencies but also from that of those who are regulated. This is the "grass-roots" or "worm's-eye" view that is so often neglected. In short, the community needs to be studied, and it includes those who are directly aided or regulated by administrative bodies and those more numerous members of the commuity who are benefited by such governmental activities. How does the administrator's work in some of these fields fit in with community needs, desires, attitudes, and mores? What does such administration contribute to community welfare, and to what extent do the people become aware of this? How does the administrator elicit the support and aid of various consumer, producer, and other groups for his work? How are democratic controls worked out and made to work? Is there adequate communication of ideas between the regulators, the regulated, and the beneficiaries? Examples of these and similar problems can be found in many fields of public administration where government touches the economic life of the community.

9. *Administrative Law.* Public administration must operate through law and in accordance with law. In many cases public administrators are doing work that might otherwise have to be done by legislatures or by the courts. This relative or apparent displacement of lawyers from what they consider to be their own field of work has brought sharp criticism upon the heads of administrative agencies. A number of other persons seem to believe, also, that administrative agencies have usurped domains that do not belong to them and that the growth of what is known as administrative law is endangering constitutional government. These are matters that certainly call for thorough and candid examination.

The very general interest of the law schools in this field gives assurance that the technical rules of law and current developments therein will ordinarily receive adequate study. What students of political science and public administration need to concern themselves with are such questions as the following: How

fully shall the administrative process be dominated by a legal point of view and personnel? How can the administration function if all administrative regulations and decisions must be appealable on their merits to judicial courts? Will experts in the various fields (finance, economics, business regulation, social welfare, health, etc.) be likely to protect the public interest better, equally well, or less well, than those with legal training? In short, where can the boundary lines be best drawn between the powers of the administrators and those of the courts to ensure the best service and the greatest protection to the public generally? And how can administrative rule-making and adjudication be improved and made more democratic and responsible?

10. *Civil and Military Administration.* All peoples that wish to retain popular control of government are concerned about civil-military relations. It is likely that the American people are entering an era when this issue will again be much discussed.

Meanwhile military and civilian administrators have worked hand in hand during the present war and have come to have considerable respect for each other's methods. Many lessons are to be learned from this wartime experience, perhaps on both sides, but at least by the civilians in studying military methods. Such studies made at the administrative level should throw some light upon the deeper issue of civil-military relations. Included along with other topics should be military methods of training, recruiting, personnel management, supply services, transport controls, accounting and finance, and military government.

11. *Comparative Administration*, and 12. *The History of Administrative Institutions.* These two approaches together provide a breadth and a depth to administrative studies that is greatly needed by American students. American studies in public administration have in most cases had too little historical perspective, while the number of comparative studies that are based on foreign as well as American observations are very few indeed.

The Committee on Public Administration set up a special committee for each of these approaches, and they soon reported certain important historical and comparative studies that need to be made. It is to be hoped that the work thus initiated will be continued under either these special committees or other auspices.

Chapter V

STATE AND LOCAL GOVERNMENT

William Anderson

PREFATORY NOTE

IN THE TWO SHORT SESSIONS that it conducted at the conferences of the Association in March and December, 1946, the panel on state and local government did not attempt to formulate any conclusions or recommendations. Lack of time at the meetings was not the only obstacle to a consensus. The panel had been organized only a short time before its first meeting. Although there had been some preparatory work, such as the preparation and circulation of a questionnaire and the distribution of a summary of the replies from thirty members of the panel, this was not sufficient to bring group thinking to a focus.

The meetings were both well attended. The principal participants at the March meeting were William Anderson, University of Minnesota; Charles S. Hyneman, Northwestern University; George Spicer, University of Virginia; John E. Stoner, University of Indiana; Fritz Morstein Marx, Bureau of the Budget; Orren Hormell, Bowdoin College; Lloyd M. Short, University of Minnesota; Arthur Bromage, University of Michigan; Karl Bosworth, Western Reserve University; Paul Ylvisaker, Harvard University; Ernest S. Griffith, Library of Congress.

At the December meeting, the principal additional participants were Dean William L. Bradshaw, University of Missouri; Winston W. Crouch, University of California at Los Angeles; W. Brooke Graves, Library of Congress; P. W. Wager, University of North Carolina; Lewis B. Sims, Census Bureau; Lee S. Greene, University of Tennessee; Lyndon Abbott, Tennessee Valley Authority.

I. THE SCOPE OF STATE AND LOCAL GOVERNMENT

The breadth and depth of the field constituted the major difficulty that faced the panel. Both state and local government

have had long histories in the United States. They are represented in practice by tens of thousands of separate units of government that operate under diverse conditions, have varying objectives and powers, and produce different results. If all these units, or even a large representative sample of them had been thoroughly studied there would have been less need for the panel. As it is, intensive studies in state and local government are of fairly recent origin, and there is no certainty that enough typical units of government or enough aspects of their work have been carefully studied to justify generalizations as to what needs to be studied next.

The extensiveness of the field called state and local government has led also to a considerable degree of specialization among students. For example, the tables in the *Directory of the American Political Science Association* (1945) reveal that there were then 133 members of the Association who professed a primary interest in state government without a corresponding interest in local government, and 187 others who considered local government a major interest but not state government. In addition there was an intermediate group, 173 in number, who combined the two interests. (Incidentally, these figures show a large number of American scholars to be interested in the two fields; and though many of them have little time for research there must be enough others who do have to give promise of a considerable amount of study in the fields in the years ahead.)

Even those who express a primary interest in local government, or in state government, or in the two fields combined, do not all mean the same thing. In the local field some emphasize rural government and others concentrate on urban institutions. Many concern themselves with the political side of local or state government while others study administrative and financial matters. Thus it happens that when a considerable number of specialists are drawn together from the two fields for a panel discussion that is to cover both, they do not emphasize the same things or even talk the same language. This fact undoubtedly demonstrates the need for having panel meetings. At the same time it helps to explain why a consensus was not reached after two short discussions.

The following report is presented not as a panel report, therefore, but as one individual's attempt to distill out some of the things that were said in the discussions in the light of his own thinking on the subject. He wishes that he might give credit to some of the participants in the panel for some of the ideas, but if he did so he might be putting words into their mouths, and he would be hard put to it to indicate just which person expressed a particular idea. It seems best, therefore, to let this report stand as an individual statement, and not to hold anyone else responsible for its statements. Due thanks are hereby extended to all who corresponded with the author and also to those who participated in the panels.

II. Research Priorities

In a regime of democratic or popular control over political and administrative processes, such as that in the United States, both state and local government are very close to the people. The panel discussions had not gone far before certain participants pointed out that especially in state and local government it is man himself who has to be studied,—man in the context of his activities as a state and local citizen. "Political behavior" was a phrase often used, but perhaps even that phrase is not broad or deep enough. There are attitudes, feelings, beliefs and other factors to be considered. Man as an active or relatively inactive political animal reveals his characteristics in countless ways in his state and local political life. The institutions and processes of local rule and administration are in a sense the formalized or generalized expression of man's political nature, but always there remains in the individual something over and above these things, some traits and aspects of man's nature that do not conform to but rather fight against or evade the formal institutions and procedures. Local government in operation is, therefore, never quite what it would seem to be. Both officials and lay citizens refuse to act in government as the laws and institutions seem to require. And in local government the things that men do outside of or even in opposition to the laws are very important to the operation of the local institutions themselves.

Human behavior, human tendencies to act and to react in

certain ways, are therefore very important in all local studies. If they are not mentioned again and again in the paragraphs that follow it is merely the result of a desire to avoid unnecessary repetitions and to save both time and space. How legislators, judges, administrative officials, board members, citizens, act and react within the institutions and procedures of state and local rule, is of the essence in the study of state and local government.

The discussions at the two panel meetings touched upon many interesting points and questions. The following paragraphs follow a "why," "what," and "how" outline, with some concluding sample suggestions for particular studies.

1. *Why study state and local government?* No one was inclined to question the intrinsic importance of the field. The wide range of the local public services, their recognized influences upon individual and social welfare, the amounts of money raised and spent by local governments, the extensive participation of the people in their affairs, and the high esteem in which local institutions are held, all lead to a general acceptance of the relatively high importance of the field. To these considerations the discussion added several points:

(a) The leading peoples of the modern world are engaged in a great experiment, that of national self-government and international cooperation by units of large areas and populations. National states are almost by definition large states. Certain political theorists have said or implied that such states can be governed effectively only by monarchs or dictators. Others have argued in effect that long-run popular control of national governments is possible, but only if there is a virile system of local self-government in small units within the nation, in which the citizens can learn, practice, and develop the habits and institutions needed for self-government on the larger scale. It is of deep public concern, therefore, to know how effective is the system of local self-government, and what is its contribution to self-government in the larger arenas of national and even international affairs. Such studies have yet to be made.

(b) Local government as something distinct from general or national government is a modern phenomenon. It developed along with or subsequent to the rise of popular government on

a national scale. Today it is very widespread, although in the sense of local *self*-government it is not worldwide. Because it is so widespread, so much a part of the political system in many countries, and because it comes so closely under the observation of countless students and other citizens who never get to national capitals for study and observation, local government provides excellent materials for numerous local case studies, and for comprehensive comparative surveys. The scientific study of politics cannot be built up solely from studies of national governments and international organizations. Studies of local government are indispensable to the advancement of the science of politics.

2. *What to study* in state and local government was also considered in the panel discussions. Up to now most of the studies in the field have dealt with *institutions* (constitutions and charters, forms of government, particular offices and departments), with *procedures and standards* (in civil service, finance, elections, legislation), and with *methods of popular control* (parties, pressure groups, methods of nomination and elections, initiative, referendum, and recall). No one at the panels seriously questioned the need for more such studies. It was argued, however, that to round out the body of knowledge on local government, certain other types of studies might also receive emphasis. For example:

(a) Other organizations and institutions should be studied at least to the extent that they apparently influence state and local units of government. In the modern world both national governments and international organizations clearly have their effects on local institutions. Furthermore, almost countless private and semi-public organizations either exert pressures on state and local governments, or serve them in various ways, or do both in varying degrees. Farm bureaus, veterans organizations, hospital associations, professional and vocational groups such as the American Medical Association and trade unions, cooperatives, leagues of municipalities, and associations of officials, from local to international in scope, participate more or less directly in the processes of local rule. What they are and do, and

what their influence is in local affairs, call for far more study than they have received.

(b) The people, both as individuals and as groups, also call for intensive study in any well-rounded and comprehensive investigation of local government. In a sense the institutions of local government are merely formalized or crystallized aspects of the political behavior of the people. But in addition to the formalized are all the informal, the spontaneous and unregulated phases of human behavior. Non-voting and other forms of non-participation, resistence to regulations and taxes, attempts to take unfair advantage of public services and facilities,—these and other modes of unregimented behavior appear with infinite variations among the millions of citizens affected by local government. Given a uniform law to enforce or administer, even the officials themselves respond to it, interpret it, and apply it with almost endless variations. Actual uniformity is almost never, if ever attained. Human inventiveness, and intractability seem to be almost without limit, and this fact is probably nowhere better revealed than in the operation of local government. It appears, indeed, that man-made laws have relatively less, and individual human traits and behavior relatively more influence and free play in local government than at higher levels of administration. In many respects local government as a study is close to sociology, while the attitudes and knowledge of the psychologist are also especially pertinent in this field.

When state and local governments are viewed in this way, in their setting, with national governments, international organizations, private and semi-public organizations all impinging upon them, and with the innumerable individual idiosyncracies of the people bubbling up and breaking forth in everything that they try to do, the student begins to get a more rounded and integrated view of the field. He sees how important it is to study the relations and interrelations of things, how unrealistic to take anything in the field out of its context for study by itself.

He realizes, too, that the concept of "levels of government," international, national, state, and local, calls for considerable correction. All these "levels" impinge upon the same people in

their several local communities. In each community there is a total network of officials and services, a meeting at one point of national, state, and local officials and a blending of their services and activities. Thus arises the concept of "government locally" as distinct from the older idea of a wholly separate system of "local government." This blending even to the point of integration of all public agencies and services in the local community calls for a more integrated approach on the part of the student—an approach akin to that of the cultural anthropologist who tries to see the life of a community of people as an integrated whole.

3. *How to study state and local government.* A consideration of "what to study" in the preceding paragraphs led step by step to a new viewpoint from which to make the study. This in turn leads to some modifications in the methods of studying. To attain new knowledge, new tools and new methods are needed, or at least a more extensive use of tools and methods that too few students of state and local government have used up to now.

(a) Field work, the first hand study of officials and citizens, of officers and citizens, of offices and work places, with much first-hand interviewing and observation, is obviously indispensable. Very little of what needs to be learned can be got from the library or from speculation in one's private office. The field work itself needs to be well organized, properly implemented, and directed along well-planned lines. There must be adequate controls, well-selected samples, and so on.

(b) The new tools of the psychologist and the sociologist, of the opinion surveyor and the attitude measurer, and of the statistician, need to be brought into play in appropriate places. This implies that the student of state and local government will either learn these skills himself, or that he will collaborate with scholars who know them. He will not expect by reading laws and reports to get all that that there is to learn about any important subject.

(c) Organizations will be needed to promote and finance research, because fairly large cooperative projects will be needed to supplement the work of the lone scholar and to overcome some of the disadvantages from which the latter suffers. Cooperative efforts among scholars in different places may be required for comparative studies.

(d) New bodies of comparative materials will also be needed -compilations of constitutions, laws, reports, and statistics such as are available today on only a very small scale. The Governments Division of the Bureau of the Census, and the State Law Section of the Legislative Reference Service, Library of Congress, are already active in supplying some of the demands, and so are certain associations of public officials such as the "1313" group in Chicago.

III. Examples of Studies That Might Be Made

The foregoing pages have sketched out only very briefly some of the general aspects of the field and of its requirements. To list all the desirable studies in the area would be impossible. Here are a few examples, some of which came up in the panel discussions.[1]

1. *Local and statewide studies of the nature and extent of citizen participation in state and local government,* including (a) voting in various types of elections, (b) service on local boards and committees, (c) holding of offices, both salaried and unsalaried, (d) participation in party activities and campaigns, (e) participation in improvement and reform associations. Data to be analyzed by sex, age, economic class, educational status, urban, suburban, and rural categories. Proportion of citizen time and energy devoted to public affairs as compared with church, clubs, and other activities. Relative importance attached by citizens to township, school district, village, city, county, state, and national government, as shown by (i) participation (ii) opinion surveys.

Specimen questions: Does participation in local government go with participation in state and national affairs? With activity in church, union, club? What is the evidence of carry-over from local to state and national political activity? Is local participation in fact a training for state and national leadership?

2. *Regional differences in citizen participation.* It is well known that state and local government are not the same in New England, the South, the Middle West, and the Pacific Coast

1. See also O. Douglas Weeks, *Research in the American State Legislative Process,* Ann Arbor, 1947.

States, or in Quebec and British Columbia, or in France, England, Sweden, and Australia. Presumably the people's participation is also not the same. Within the United States there is room for cooperative comparative studies on citizen participation in public affairs between regions, between urban and rural in different regions, between cities of different sizes and population composition. There are differences not only in actual participation but also in attitudes toward local government. The position of minorities—Negroes, Nisei, the foreign born—needs to be considered. The relationship between citizen participation and attitudes on the one side, and local institutions, forms of government, procedures on the other, are worthy of investigation. Foreign comparisons can come later. *Specimen questions:* To what extent is there regional uniformity, to what extent regional diversity, in systems of state and local government, and in citizen participation therein? Insofar as important diversities exist, do they strengthen or weaken the argument for local self-government as the training school for national citizenship?

3. *Tendencies toward variation in official application of uniform, general laws.* A study of individual differences in political and administrative activities. Effectiveness of in-service training, printed forms and handbooks, penalties, and other devices, in producing uniformity. Official rationalization of differences in methods of carrying out laws. Measurement of variations insofar as that is possible.

4. *The role of state legislatures, legislative members, and state administrative officers* in guiding and controlling local officials in different regions and states and in different types of communities (for example: urban, rural; large, small; multi-functional and uni-functional units). The influence of the county or city delegation in the legislature. State agency and official attitudes toward local self-government. Communications between state and local governments.

NOTE: Somewhat the same questions can be asked concerning national-local relations, and national-state relations.

5. *The processes of invention and diffusion of institutions, procedures, work-methods, among state and local governments.* How new ideas arise in this field. How they spread. Copying,

imitation; education and professional training; associations of officials and professional journals; newspapers, magazines, radio; reform associations; commercial salesmanship; and other media of transmitting new ideas in government. Local resistances to change. Modifications of institutions and procedures in the course of transmission to new places. Incompleteness of adoption of new ideas. List of important new governmental and administrative ideas of past 30 or 50 years, with tables and maps of how they have spread.

6. *The governmental agencies and services within a particular community* (county, city, village, town)—their inter-relations and their relations to the people. Comprehensive studies in the concept of "government locally." [See the Blue Earth county study and others under Council on Intergovernmental Relations.] How and to what extent are national, state, county, and other services coordinated in a particular place? Extent of and reasons for non-coordination. The "grass roots" view. The top official view.

7. *The reallocation of functions among national, state, county, and smaller units of government* in a state in the past 30 or 50 years. Measurement of centralizing and decentralizing trends. Effects thereof on governmental finances, organization, citizen interest and participation, consolidation and liquidation of local units, and other factors.

8. *The fiscal interrelations of national, state, and local governments* in a particular state or county. Amounts of revenue taken from area by each level of government, amounts spent therein by each (spent either directly or through grants-in-aid). Evidences of effect of such interrelations upon stability of local revenues, upon freedom of local officials, and other vital elements in local government. Tendency of grants-in-aid to become matters of legal right.

9. *Executive-legislative relations in a state.* Veto power, budget proposal, executive-legislative program and bill drafting, appointments. Informal means of cooperation. Legislative attitudes on executive leadership.

10. *State legislative reorganization in a particular state.* A study similar to those recently made on Congress.

11. *Administrative rule-making and adjudication in a particular state or large city*. What these powers are in law and in practice.

12. *The unionization of public employees and the effects thereof* on state and municipal merit systems. What the planners of civil service reform expected to achieve and what has resulted in practice.

CONCLUDING NOTE: As stated above, these are but a few samples of the numerous important questions that today invite intensive study in the state and local field. No attempt has been made to work out in detail how any single topic might be studied.

Chapter VI

INTERNATIONAL RELATIONS

Thorsten V. Kalijarvi

PREFATORY NOTE

UNUSUAL DIFFICULTIES forced the international relations panel to adopt a technique differing in many particulars from the other panels. Three chairmen, Walter Sharp, Phillips Bradley, Grayson Kirk, made successive attempts to operate after the fashion of the other panels but with very limited success. Some correspondence took place and one meeting was held at the time of the Association's Annual Meeting in March, 1946.

It became obvious by the summer of 1947 that there was grave danger that this volume would lack a report in what was obviously a field of transcendent importance. The editor, with the approval of the Research Committee chairman, therefore set up a small committee in Washington consisting of Francis O. Wilcox, Senate Foreign Relations Committee; William P. Maddox, State Department; Pitman B. Potter, American University; George Pettee, House Foreign Affairs Committee; Howard S. Piquet, Library of Congress; Halford Hoskins, School of Advanced International Studies; Sergius Yakobson, Library of Congress; Thorsten V. Kalijarvi, Library of Congress (chairman); and himself to consider the matter. This committee held several meetings, contributed memoranda, and invited the ideas of a large number of scholars throughout the country. Significant responses were received from the following:

James P. Baxter, Williams College; John C. Campbell, Council on Foreign Relations; Clyde Eagleton, New York University; C. J. Friedrich, Harvard University; Leland M. Goodrich, Brown University; William L. Langer, Harvard University; Emil Lengyel, New York University; Gerhart Niemeyer, Oglethorpe University; Leo Pasvolsky, The Brookings Institution;

George T. Renner, Stanford University; Walter R. Sharp, College of the City of New York; Robert G. Sproul, University of California; Robert B. Stewart, Fletcher School of Law and Diplomacy; Robert Strausz-Hupe, University of Pennsylvania; Harold M. Vinacke, University of Cincinnati; Benjamin H. Williams, Industrial College of the Armed Forces; Quincy Wright, University of Chicago.

It is a synthesis of the views of the members of the Washington committee and the responses from the outside correspondents that is incorporated in this chapter.

I. Rationale

Students of international relations in this country can in general be divided into two schools: (1) those who favor pure research, or the serving of broad, primarily humanitarian purposes, and (2) those who would conduct research in order to meet specific national needs and policies of the United States. The schools differ on the question of the degree to which research should be associated with national policy.

The objectives of the first school are epitomized in a statement to the panel as "the search for truth" and the widening of the "frontiers of knowledge without being limited by the bounds of particular national policies and programs."

The objectives of the second school are more specific and begin with the thought that significant research is socially purposeful; and that there are two dominant social purposes in international relations, namely, (a) the elimination of periodic large-scale war, and (b) the development of effective, nonviolent political institutions and processes capable of achieving security, freedom, justice, and well-being for the peoples of the world. Partly because these purposes are obtainable only in the long run and may not be achieved for some time, and partly because as Americans we believe that the American community and culture must not be impaired, the second school recognizes two supplementary social purposes, namely, (c) American power and influence should be maintained to the point at which they can adequately assure an equilibrium of power in the world, or perhaps permit the United States to participate

effectively in international organization for peace and security; and (d) that power and influence will be used to achieve the long-range objectives.

Since the two schools divide on the objectives to be sought, they divide on the priorities to be given to research in international relations during the next ten years. On the surface the so-called pure researcher would give priority to that research which would be most useful in the expansion of knowledge, since his criterion is the broadening of human knowledge about international affairs. The exponent of the second school would unhesitatingly give priority to the needs of the United States for the fullest and most accurate information possible on which to base its conduct of world affairs.

It is significant that in spite of the divergence between the two schools in theory and underlying philosophy, they are frequently brought together in the face of current conditions and the needs of the United States.

II. Scope

Research in international relations is now recognized as a highly significant, if not the most signficant, basis for the formulation of foreign policy. It is much more than a technique for the satisfying of scholarly curiosity, and possesses both national and world significance. It is one way of understanding international relations which embrace spatially the entire globe and socially all humanity. The range, variety, and complexity of the political phenomena which impinge upon international relations are broader than in any other area of political science, and also, more numerous than ever before. The need for research in this field is, therefore, greater than at any previous time in history.[1]

1. One of the consultants believes that no report on international relations should be presented at this time. He is the only one to express such a view, the others approved issuing a report now. The words of dissent read in part as follows:

> I think the field of international relations is so important and so new and so much in a state of flux at the present time that there really is something to be said for making it the subject of a separate study, the results of which might be published at a later time. Another consideration which seems to me to favor that course is

An evidence of this fact is the State Department's abandonment of the "day-to-day" basis of operation, and the substitution of a planned, coordinated, synthesized, flexible, and long-range program—to be produced by the Policy Planning Staff. While this carries with it no absolute assurance that its planning will be based on research, at least the machinery is present.

1. The challenge of contemporary research in international relations for the next decade is that of evaluating the entire subject. There is scarcely a field, old or new, general or specific, in international relations which does not require research now. Not only has the foreign policy of the United States assumed greater proportions recently, but the changes wrought by the Second World War in the social, political, and economic structure of the world have broadened and altered almost every aspect of world affairs, with the consequent demand for the re-evaluation of old and the investigation of new problems.

2. The need for broad, synthesizing surveys and analyses is great. For example, there is a present call for a general diplomatic history, written by American scholars from the American point of view, covering the period from ancient times to the Second World War. Russia has recently published a three-volume diplomatic history, treated from the Marxist point of view, two volumes of which have been translated into French, and an English version which it is rumored will soon appear.

3. Detailed, fact-finding, specific surveys and studies, for example, of the foreign policies of individual powers, great and small, are needed. Investigations into the patterns of international behavior, laying special emphasis on those which differ from our own, would be useful.

4. The forces and factors contributing to the formulation of

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the fact that the field of international relations cuts across the established departmental lines and could, I think, properly be made the subject of a study by a committee representing a number of organizations, of which the American Political Science Association would be one. I am not prepared to make a specific suggestion, but it does seem to me there is a very strong case for the cooperative project.

. . . Our initial effort might be toward establishing the nature of the contribution that we as political scientists can make.

foreign policy constitute an area still largely unexplored. Their study should include not only official documents but also such influences as geography, anthropology, demography, economics, technology, the press, and psychological forces. Much work remains to be done before there can be a thorough knowledge of the dynamics operating in foreign affairs. At the same time there is an area of interaction within the diplomatic and international organizational process which is unique in itself. The focus, constituent elements, and tools of the investigations required in the study of this area need to be explored, for this is relatively virgin territory.

III. Coordination

There is a consensus among the panel members and those consultants who answered the panel's inquiries, to the effect that coordination of research is needed; but opinions differ widely as to how much coordination should be sought. Some members of the Association fear that coordination, especially if under governmental supervision, would produce biased and distorted control over free research. They assert that governmental control of an extreme type presupposes research directed toward preconceived conclusions. Their objections extend even to governmental subsidies of research, which may mean subtle coercion and therefore the debasing of research.

One consultant says:

> It seems to me that effective research is a problem of attracting able men to the field and of obtaining financial resources to support their work. Coordination may have the effect of impairing initiative and leading to a great deal of routine and uninspired collection of data.

Set against this is the view more commonly held, to the effect that to date a great amount of research by individuals, institutions, and government agencies has been carried on with little correlation; thus it has failed to secure the maximum benefits attainable under a nationally integrated program. This view deplores the fact that quantities of material, which reputable research workers could safely and profitably use, and which is in great need of

study, lie unused in government agencies. These materials should be made available by the government immediately, as far as national security considerations will permit, and should be released under control of highly qualified scholarly organizations. It is generally feared that if the State Department has a hand in the opening and the directing of research materials, it will be tempted to control the research field—therefore the suggestion that the opening be left to the scholarly organizations. Should the State Department achieve the control, the democratic approach to research in international relations runs the risk of being subjected to political favoritism and political party-lines.

In short, while there is a general belief that coordination of research is desirable during the next ten years, opinions differ as to how it may best be achieved and how far it should go. There is special concern about the coordination of research under government auspices.

The advocates of coordination recommend a number of ways by which it might be sought. Chief among them are the following.

1. A conference of educational, research, and governmental leaders should be called to explore the feasibility of an agency to coordinate the work of such widely differing research agents as the State Department, governmental agencies, institutes of international relations, educational institutions, the Council of Foreign Relations, the Foreign Policy Association, private endowments, the World Peace Foundation, the Carnegie Corporation, private scholars, and graduate students.

2. It would be logical to consider the establishment of such an agency within the State Department, of ascribing to it among other functions those of initiating and administering a program of research coordination, calling of conferences, setting up priorities of research in terms of national policy needs, assisting in finding publishers for excellent studies, and serving as the liaison between the research and the policy formulating and administering groups. If the major interests concerned were represented on the board of control of such an agency, it could give new significance, worth, and impetus to all research efforts in international relations.

However, this suggestion raised almost unanimous protest when it was presented to the consultants. Individual panel members and consultants questioned the soundness of placing responsibility for research in international relations in the hands of any governmental agency, and specifically in the hands of the State Department. One typical comment read:

I do have some question about the desirability of having the program head up in our State Department, that is, having an agency of the State Department coordinate the program. There should undoubtedly be an office in the State Department which would coordinate research within the Department and establish priorities for research projects. Along with the establishment of such an office in the Department, there might be set up a private agency in Washington—perhaps the nucleus is already established at Brookings—which could keep in closest touch with the State Department and advise with it in determining priorities of research needs.

Another said:

. . . Of course the government may find the results of this research very useful, but any coordination, I think, should be done through voluntary action of the universities and private organizations themselves. There might well be guidance by such a body as the American Political Science Association itself to prevent haphazard choice of subjects for research projects. This would not preclude State Department requests to particular organizations to do specific jobs.

3. In its original summary of ideas the panel suggested that the American Political Science Association and related associations might profitably sponsor the establishment of an institute for the study of international relations, patterned after England's Chatham House, or the pre-Nazi Hochschule fuer Politik, which would carry on work, not only through generally practiced methods, but also through less standardized ways such as study groups. This institute, set up on the broadest bases, should perform detailed studies and gather basic data on every phase of international relations, exploring particularly the geographic, anthropological, demographic, economic, social, cultural, legal,

diplomatic, and other related international areas. The personnel should include specialists in every vital field, both geographic and functional. The first priority would be to gather information about people, institutions, languages, economics, and relations of such hitherto neglected areas as Africa, Oceania, Afghanistan, Iran, Iraq, Mongolia, Manchuria, Korea, and Indonesia.

The panel, in framing this suggestion, had in mind that, because of the European and American orientation of the thinking on international relations in the United States, some very important areas in world politics, both geographical and functional in character, had been and still were being neglected. The need for a study center of the type contemplated seemed all the clearer when some of the consultants failed to appreciate its world-wide scope, and suggested that a number of institutes and agencies already were filling this need, calling special attention to the Institute of Pacific Relations, the New York Council on Foreign Relations, Congressional committees, and other agencies, none of which exactly corresponded to the panel's idea of a continuing, all-embracing institute. One consultant indicated that a move was on foot to create a research center, comparable to that at Geneva, at New York City. As yet it has not materialized, and the panel views the need for an agency of the kind it has in mind as being both definite and immediate.

4. The panel and consultants are of the opinion that effective coordination of research would require a roster of all research personnel and facilities in the United States. It would also require a knowledge of foreign specialists, who could be called upon to perform especially difficult researches, act in a consultative capacity, or be employed in a regular research capacity. One example will suffice. If American foreign policy with regard to the Afghanistan, Pakistan, Mongolia, and neighboring areas is to be fully effective, it needs the expert advice and information of specialists abroad, and it needs to train national specialists of its own to take the places of foreign specialists as rapidly as possible.

5. In coordinating the work during the next ten years, it will be necessary to study new types of seminars and subject matter organization. For example, graduate schools should can-

vass the value, need, and possible contributions of historical geography, ideopolitics, defense politics, and technopolitics.

IV. Fundamentals and Subject Matter

The subject matter and fundamentals of world politics as well as their interrelations need to be investigated again more intensively than ever. That is especially true of the international aspects of domestic politics, of economics, of geography, anthropology, demography, technology, cultural institutions, linguistics, customs, ideas, ideals, ideologies, and psychology. The character, make-up, causes, and dynamic aspects of the political order itself need re-study. It was stressed that too much emphasis might be placed upon the novelty of international relations at the present time, but there was no difference of opinion as to the need for reinterpretation and reinvestigation.

1. Contemporary international relations are premised upon our going through a great historical transition, which is modifying the old state system into another form of political control. International relations, therefore, require a re-examination of the characteristics of the nation state. Of prime importance are studies of how states have come into existence and passed into oblivion, the secularization of the state, conditions affecting the growth of nation states, colonial systems, imperialism, national economics, factors creating world crises, and the decline of the nation state in terms of international economic dependence, the logistics of heavy industrial complexes, and the logic of the drive toward autarchy, and the modern state as a commonwealth or a political economy. It is important to explore the possibility of an organized cultural community without terminal associations, an example of which was medieval Europe. It is also important to consider the possibility of the combination of states into confederations, unions, and associations. Only a beginning has been made in this area and much fruitful exploration would seem to be needed.

2. The mainsprings of action within the sphere of international organization and politics still lie, for the most part, within the nations, or perhaps, in some instances, within the regional, population, political, or cultural areas. The clear recognition of this

fact would serve to focus a considerable part of research energy on the analysis of the forces thus operating. This would serve to indicate both (a) the forces which need to be developed, unleashed, or channelized to achieve long-range "socially purposive" objectives, and also, (b) those forces within the American political and cultural context which are significant and valuable.

The field is almost limitless and only a few frontiers can be suggested here. Parenthetically, it may be pointed out that an understanding of the dynamic forces operative in other countries is a prerequisite to the intelligent formulation of American foreign policy, strategy, and diplomatic techniques. To a very important degree, the political scientist needs the cooperation of other social scientists in his task. The contributions of the economist are traditional and well understood. Not so generally accepted are those of the sociologist, social psychologist, cultural anthropologist, and the linguistic scientist. The patterns of behavior, the ways of thinking, the systems of myths and values, the meaning of verbal symbols—all these and more shed light on actions and reactions of the government which operates within the culture. Conversely, of course, study of the actual operations of government illumines the total social context within which the government functions. This is not to suggest that a perfectly adjusted balance exists between the two, for indeed intensive research will undoubtedly reveal major maladjustments and discrepancies. But even this discovery would indicate the probability of social and political tensions arising in a future more or less distant (depending, roughly, on whether the system tends toward the democratic or the totalitarian).

Another area in which the political scientist must seek assistance is that of cultural interpenetration (in its largest sense), or the impact and influence of one culture upon another. We talk about the importance of preserving and extending American power and influence abroad as an instrumentality for the furtherance of long-range world objectives. But at the same time it has been asserted that the library-shelves are empty (except for one or two books) of studies—descriptive and historical studies—on the degree and extent to which American toilet articles, refrigerators, motion pictures, sewing machines, books, manners,

clothes, electric appliances, automobiles, values, ideas, are affecting the lives of a single national people abroad. There may be some difference of view on this last point; but there was general agreement in the panel that, lacking the basic studies, interpretations and prescriptions for action are well nigh impossible. This is not to suggest that scientific research must lend itself to the service of those who wish to prescribe action. Rather, it means that, given any social objective, the basic constituents of knowledge are not available.

More strictly within the traditional competence of the political scientist fall studies of the ways in which particular groups (or sectors or individuals) of a national community affect the formulation of national policy, and the interaction of such groups with others within the country, or in foreign countries. Small, but nonetheless integral, parts of the total international relations process may be illumined thereby.

In addition it may be well to recognize that studies of the structure of national power, and of the dynamics of national policy, cannot depend solely upon geographical, technological, economic, and other materialistic data—although no one will dispute their importance. These factors are significant in themselves, but part of their significance lies in the way in which they —along with many intangible factors—are perceived, evaluated, and projected in idea-combinations, in the minds of men. And these minds are influenced and conditioned by the cultural, national, sub-national, class, and domestic environments in which they have grown.

3. The effect of recent inventions, especially in the fields of communication and military technology have had a vast effect upon world opinion and world politics. Studies on the political effects of the airplane and the atomic bomb have been made, and opinion surveys have been devoted to these subjects. But much remains to be done and material still remains to be brought together from the point of view of political science.

4. The international society itself, as will be seen in the section under Techniques and Administration, needs further study.

5. So there is at present a need for study of public opinion in respect to nationalism and internationalism. Such a study should

consider other instances, as, for example, federations like the United States, where loyalties have moved from the smaller to the larger group, and should make use of anthropological and sociological findings on the matter. It should, however, be devoted primarily to measuring the actual trends of opinion and attitudes on this subject in various sections of the world.

V. Techniques, Strategy, and Administration

The manifestations and interactions of national foreign policies, the basic forms of diplomatic strategy, methods of developing submission, acquiescence, responsiveness, or cooperation on the part of other national governments, the means of negotiation, and of organization, and all techniques and strategy for the formulation and the conduct of foreign policy, including international organization, administration, and law, call for further study. These studies should be conceived on the broadest bases and should include such subjects as diplomacy, the resort to force, the organization of national governments for the conduct of foreign affairs, the molding of public opinion, national and international planning, world organization, and the role of treachery in world politics.

1. International institutions require study to bring out the characters of federal union, federations, confederations, and international organization, and the relationships among these. World government and international federal organization need to be distinguished further. This requires historical and contemporary facts and thorough general analysis. The United Nations Organization needs further study, emphasizing such topics as veto, limitations of the enforcement system, preservation of peace and security, limited facilities and procedures for settling disputes. There is room for much work on possible world federations including the economic, social, cultural, and socio-psychological prerequisites for their establishment and operation.

International organization is confronted by the major question of how can the power-political basis of international relations, which can give stability only through a balance of power, be developed into a constitutional basis which can assure the maintenance of law limiting at least the use of violence in world

politics? In view of the circumstances, which seem to the majority of the panel to make a restoration of stability through balance of power unlikely in our time, such a study seems of the utmost importance. The study should be directed toward the process of development, rather than toward the elaboration of any specific form of international organization or world government. It seems that it should be based upon the assumption that politics is inevitable in international affairs, but conditions may exist in which the struggle for power takes place without any legal restraint, and conditions may exist in which the methods of that struggle are limited by law.

2. The authorities and the mechanism used in the control and conduct of American foreign relations require fresh and exhaustive explorations in the light of our new position in the world. Comparative studies of systems in other countries are equally necessary, partly because of lessons of value to us which might be learned and partly because these systems of control, together with our own, constitute the principal governmental foundations of all international politics.

While there was no unanimity in the panel on the point, there were those who believed that it is not without significance that there is no up-to-date systematic treatise on the Department of State or on other foreign offices. When interested persons wish to assess the merits of the American system of separate departmental and foreign services against combined home and foreign service systems, they will look in vain for adequate studies of the subject. Here is an area in which the interests of the students of comparative government and of public administration overlap with those of the student of international relations, but its importance to the last group is primary and should not be further neglected. The illustration used here should in no wise detract from the value which would inhere in studies, country by country, of the entire complex of executive-administrative-legislative relationship in the control of foreign relations.

3. International administration is a fruitful field for further investigation. A host of topics present themselves, among them the headquarters of international agencies, the personnel of international administration, the leadership function, and the role

of the international administrator as compared with his national colleagues, and financial problems.

4. Another view, as expressed by one of the consultants, with a little different slant, is the following:

> As the field of international organization expands so as to touch nearly all activities of national governments, the problem of gearing the national legislatures, the national courts, and the national executive departments into international organization becomes more important. This is particularly true in regard to executive departments other than the Foreign Offices. It is clear that there is a conflict between coordination of government at the national level and coordination of functions at the international level. Compromises are necessary, and study of the way in which such compromises have been effected in the various national governments is important. I believe the Carnegie Endowment and other organizations have projected a study of this problem as it relates to the United States Government, but a more extensive comparative study might be in order, and the study should be oriented, not only from the national points of view, but also from the international point of view.

5. Research in international law might emphasize methods of securing or inducing agreements on points of difference among states in the matter of codification; a recanvassing of the law of war in a world where the elimination of war is being attempted, and the relations between the law and possible international police action, or its application thereto. As the individual increases in importance under international law as well as under newer international conventional legislation, his position needs further clarification. So, too, does the relation between public and private international law require review.

Attention should be paid to the important modifications of traditional concepts through recent general treaties, judicial decisions, and resolutions of international organizations. The study should be oriented to the problem of how international law can be better adapted to the present conditions of international relations.

6. Careful thought needs to be given to the differences in the attainment of national independence and greatness, including a

careful re-examination and re-evaluation of the role of violence in national and international development. Only if we understand the role of violence can we hope to conduct human social history without violence. A peaceful order can only be established when one learns how to control the international order.

VI. Formulation of Policy

While the advocates of pure research would give only partial priority to the formulation of policy, the advocates of priority of research as an aid to United States needs would give the formulation of policy full priority. It is their view that research in the fundamentals and techniques of world politics should be brought to bear upon the formulation of American foreign policy, and should be supported by studies of the subject of formulation itself. National and international objectives need to be clearly stated and understood, and maintained on a flexible level, where they can be realistically adjusted to the requirements of the world situation.

1. The place of the United States in the world today needs consideration from the point of view of raw materials, economic interdependence, changing technology (atomic energy), and imperialistic politics. One contributor says:

> I should like to see a series of joint studies by political scientists and political geographers (and perhaps military experts) covering the question of what would be the key strategic areas and sites in any probable Third World War. In such a war, there could be only three or four major combinations of nations. From these studies it should then be determined what our alternative national policies should be in order to guarantee maximum national security.

2. Of overwhelming significance at the moment is the question, who should formulate American foreign policy—the President, the State Department, the Senate, or the House, which is lately inclined to claim a share in this prerogative. This should include a study of formulae for effectively coordinating the aspirations and rivalries of American governmental agencies and institutions in the international field. Diplomacy is both an art and a craft which requires a continuing change in professional

training of people engaged in it. We need fresh studies and evaluations of the in-service training and instruction presently being given in the foreign services in a dozen foreign countries, as well as in the recently-established Foreign Service Institute administered by the Department of State in Washington. Included in this area is also the manner in which the United States formulates its foreign policy, especially in the economic field, with particular emphasis on the difficulties of arriving at a united and active foreign policy, under a system of checks and balances; and in the face of competition from fast-moving totalitarian communities. Inquiries might also be made into the alleged diplomatic ineptness of the United States in such international agencies as the United Nations, the Food and Agriculture Organization, and the United Nations Educational, Scientific, and Cultural Organization.

3. This research should include the elements and factors which make for a sound American foreign policy and the rising concern of our political parties over foreign policy and relations. We also need to examine political groups, including clerical groups, abroad—particularly so-called communist parties—which differ from nation to nation and are components of varying elements with varying objectives. It would be worth while to know something of their origins, their component elements, their relations with the USSR, their relative strength, and their international organization and ramifications. We can have no intelligent attitude toward foreign governments without such data. Such studies of parties opposed to our political parties and groupings, particularly in Europe, when taking into consideration that traditional or sentimental relationships, which always have some value, ought to lead in the end to an understanding as to where this country might look for potential friends on the one hand, or potential enemies on the other hand. They ought to have a practical bearing on long-range foreign policy.

4. We might give new attention to the ideas and forces within any nation (including those within the government itself) operating upon, or affecting, the action of the government in its foreign relations; forces cutting across national boundaries which in their interaction, or joint action, affect one or more na-

tional governments, or an international organization; the essential dynamics of a nation's power, and the ratio of national power.

VII. Methods

It will be apparent from what has been said that students of international relations of both schools object strongly to any coercion, subtle or open, in connection with research work. They object to the slanting of research in terms of conclusions. While it is recognized that individuals must be free to direct their research talents and energies according to their own interests, purposes, and preferences, it is nonetheless true that suggestion, stimulation, and direction may serve to point up—in a field of limitless extent—certain areas of high priority. If we superimpose the layer of objectives upon the elements of the political process in international relations, we can discern some of the promontories of special concern.

It is important that political scientists recognize the limitations of their own discipline, and the extent to which they must employ the concepts and techniques of all the social sciences, if they are to produce realistic, meaningful, and useful results. The specialization of the political scientist is simply a focus and a point of departure. It cannot be sufficient unto itself. Whatever may be the measure of truth in this observation as applied to other areas of political science, it is emphatically and indisputably true in regard to international relations. Quite apart from the desirability of the political scientist acquiring a reasonably solid foundation in economics, sociology, social psychology, cultural anthropology, history, and geography, it may be necessary to experiment more generally with research seminars and projects on a cooperative basis with other social scientists. Indeed, it should be equally evident that, in the investigation of an increasing number of problems with technological implications, the professional assistance of the physical, chemical, or biological scientist is required; and as Professor Northrup has demonstrated in "The Meeting of East and West," the role of the philosopher is of great potential importance. So far we have not yet found it necessary to call upon the astronomer or the archaeologist!

Corollary thoughts apply to the research methods themselves.

The value and usefulness of narrow techniques such as the strictly legalistic methods, strictly statistical methods, the historical approach, need to be re-assessed. More study needs to be given to the general usefulness in the field of international relations of the so-called characteristic method of political science, which is understood to be a synthetic technique which investigates historical facts, interprets them, describes existing phenomena, and analyzes causation, appraises with reference to proposed objectives, and gives due attention to geographic, economic, psychological, and all other social and sociological aspects of the problem. Because, when this synthetic technique is employed, exaggerated emphasis upon history, geography, economics, politics, or law, is fatal to best results, and considerable thought needs to be given to the ways by which a sound balance may be achieved and maintained. Not to be excluded is the question of the limitations of this and all other methods of research.

One of the consultants develops the point as follows:

> This, over some years, has made me conscious of what is really a shocking deficiency in the democracy which we think of as an example to be followed by all states. In a democracy, the will of the people is supposed to prevail, but in our democracy there is no way provided by which the Government can find out what the people want, or by which the people can tell the Government what they want, on any one issue.
>
> I would like to see a study made of this (of course, it is broader than international affairs, but it is fundamental in our conduct of international affairs) by a combination of political scientists, psychologists, public relations people, and especially managerial and scientific talent. Scientific devices seem to me to be necessary, and the businessman who scoffs at the antiquated governmental procedures ought to be called in to modernize them.
>
> I do not raise here a number of questions which would inevitably go along with such a study—how to inform the people, how to induce them to take the information or to think intelligently; how to get them to speak; more especially, the delimitation of the fields within which they would reserve the right to speak and the fields in which they would delegate authority to the government. If each of us had a button to punch on our easy chairs at home, to record our vote officially, I doubt if we would punch the button!

It is my conviction that communism or other isms can never be met by howling against them or by persecution or even the use of force; the only answer is to put up a better system of our own. So what I am asking is an overhauling of our democratic system of government so that it can more efficiently meet the needs of our times. It is a huge job, which the Political Science Association could undertake, but which it could not do by itself.

One of the most interesting methods discussed by a number of consultants, as well as the panel, was pool and cooperative research. One of the consultants said:

It would be well to give some attention to the possibility of "team" research in which the official, national and international, might collaborate in various ways with the independent scholar. In this connection, case studies of the handling of specific "tension" situations by different techniques might contribute illuminatingly to our understanding of the total process of adjustment. Similarly, case studies of different types of diplomatic officials and of international administrators should be encouraged. We need to learn about the actual behavior of international institutions by moving beyond the traditional descriptive and legalistic approach to the problem. Broad synthesizing surveys and analyses are, I agree, highly desirable, but before they can be done very effectively, a lot of monographic work is essential.

I should also point to the fruitful results that might ensue from faculty seminars in the larger institutions, along the lines of the recent experiment at Columbia University. In order for the teacher to supervise research that cuts across the traditional disciplines, he must be "educated" himself in the interrelations of subject matter and approach involved therein.

On the general problem of research organization and coordination, I would personally take issue with the idea of setting up within the State Department an agency with a view to facilitating clearance and the establishment of priorities. While the Department, along with other Federal agencies, should have a part in any such machinery, its locus and control, in my opinion, should be nongovernmental. On the other hand, there are real possibilities of cooperation with UNESCO for the planning and stimulation of intercountry research undertakings involving scholars of different nationality.

Another consultant points out:

As to the ways and means, I should like to stress the great need of cooperative action in this field. That type of action has produced some highly remarkable documents. I can conceive of a group of congenial and conscientious scholars devoting their sabbatical years to the creation of a document that represents not merely their isolated researches but, in addition, the fruits of their collective discussions and labors, in general.

During the Second World War, area studies, which previously had had considerable vogue in Europe where they had been tried in great detail, were tried in the United States. It has been suggested that this method of research be further explored. Said one consultant:

I should like to say a word about the "area study" approach which grew up during the war and has now been adopted in several universities. It promises much for the future, particularly in filling the great gaps in our information and understanding of certain little known parts of the world. It should also give us a better founded knowledge of some of the more familiar problems of Europe, the Far East, and Latin America. There is of course the danger of overspecialization, without sufficient coordination of these studies as between one area and another, particularly if one university or organization specializes in only one or two such areas. The job of coordination might well be done by the American Political Science Association, or by some body formed for the purpose on its initiative.

VIII. Conclusion

While students differ extensively on how the emphasis is to be placed and what the priorities are to be in research in international relations during the next ten years, they do not seem to be very far apart in the subjects selected for emphasis. Even the devotees of pure research recognize that because of the new role of the United States in world affairs the greatest emphasis in this country will be placed upon her needs. The advocates of the use of research for national purposes consider that priorities during the next ten years should be related to the formulation and administration of American foreign policy. They urge the co-

ordination of all research efforts with a view toward securing maximum results by avoiding a duplication of efforts, and by utilizing for national and international purposes all research time, personnel, equipment, and materials that are at hand.

Chapter VII

POLITICAL SCIENCE AND THE STUDY OF WAR

Bernard Brodie

PREFATORY NOTE

UNLIKE MOST of the other panels, the panel on political science and the study of war was able to hold but one meeting, and that in connection with the crowded program of the Association's annual meeting. Nevertheless, the crystallization of thinking and the importance of the points of view were such that the report of its chairman well merits inclusion in this volume.

I. THE WAR SETTING

War is not always with us, but the persisting possibility of its recurrence is a primordial fact. In this atomic age it is the dominant fact of our civilization. Nor did it require the atomic bomb to give the institution of war its sway over human fortunes. Even so favored a country as our own has over the last three decades spent a very considerable portion of its energies either fighting wars or preparing for or recovering from them. Less favored nations have suffered as a result of war the termination of their very existence as self-sustaining communities. And whatever the ultimate degree of injury or ruin, the concomitant of modern war is inevitably wholesale death, capital destruction, and the dislocation of human pursuits. It is thus a truism that war is the greatest of social problems. But from the dearth of effort which scholars have invested in the subject, it is a truism which requires restatement.

The study of war is peculiarly the province of the political scientist. The sociologist, the economist, or the psychologist cannot escape the impact of war upon the human behavior which each in his own way studies. Each may in fact carve out

for himself whole areas of research in which his special competence is applied to problems deriving from the existence of war. But responsibility for the integrated and comprehensive approach to the basic problem of war rests with the specialist in politics, especially with the specialist in that branch of politics known as international relations.

Were it not for the existence of war, international relations would hardly warrant status as an academic discipline. The ceremonial and legalistic observances which would govern the relations between states could be left to vocational schools dedicated to the training of professional diplomatists. The economic significance of national boundaries would continue to intrigue a few economists so long as there remained any artificial barriers to trade. But if international tensions could always be resolved short of violence, the substance of those tensions and the operations for resolving them would hardly be of consuming interest to most of us. It is war which makes international relations a field of study second to none in importance.

The specialists in international relations have of course been aware of the central position in their field occupied by the institution of war. They have been very busily engaged examining causes and advocating remedies. But thus far their technique has been that of the medicine man rather than the physician, for in their haste to prescribe cures they have scarcely troubled to study the disease. War is of course a conspicuous malady when it breaks out, and its symptoms—at least in the stage of military action—all too unmistakable. Since the political scientist feels confident of his ability to recognize a war when he sees one, he has felt justified in confining himself to the observance of causes and consequences, leaving to a curiously inarticulate and intellectually isolated profession exclusive concern with almost all matters relating to the conduct of war. He will acknowledge that the vast complex of operations relating to the conduct of war influences not only the consequences of war but also, by anticipation, its causes. But at least to the extent that those operations are "military" in character, he has been all too ready to dismiss the study of them as singularly esoteric and fitting to be left solely to the experts.

Such dismissal would not be so bad if the experts were more communicative—or even more expert—or if the political scientist put himself to some pains to meet them half way. But the experts do not know enough, they are not inclined to recite publicly what they know, and the usual political scientist cannot begin to understand their language even when they speak. Thus military strategy, with all its ramifications and all its impulses upon social life, is a closed book to the usual political scientist. He should then realize that at least on the level of speculative comprehension it is a not-too-familiar book to the expert from whom he expects at the critical moment to receive pertinent advice.

The above observations on the inexpertness of the experts are not to be construed as reflecting lack of esteem. This writer has known enough military officers of all ranks to know that there are among them many men of exceptional intelligence, that as a body they are quite devoted to their profession, and that by and large they show more than average readiness to sacrifice personal comfort and advantage to what they conceive to be the good of the community. It is simply that the military profession is not a scholarly calling, as its members would be the first to insist. It neither trains its recruits in scholarly methods and habits nor greatly rewards that scholarship which occasionally appears within its ranks. Various post-graduate service schools on the "war college" level provide their students with intensive training in tactical and to some extent in strategic principles. But the character of the training and especially the nature of the calling into which it is fitted conspire to make the course a mere interlude in what the student regards as the more active phases of his career. There is nothing in the armed services comparable, either in quality or purpose to the course of study which leads to a Ph. D. degree in the various graduate departments of a good university. The service schools can provide excellent field commanders, but the Clausewitz or the Mahan is an accident and is distinguished in the profession by his rarity.

It is not the obligation of the political scientist to fill the gap left by the soldier's rejection of the comtemplative life. But he must be aware of that gap, and when he finds himself—as he

often does—dealing with problems which require sound strategic analysis, he must be prepared either to develop that analysis for himself or to examine critically and intelligently the professional analyses of which he avails himself. He should not be unduly surprised or alarmed if he finds himself inadvertently contributing to the filling of the gap. He may initially feel keenly the lack of that special knowledge of weapons and their use which comprises much of the intellectual inventory of the professional soldier, but he will be astonished by how much he can acquire through discriminating reading. And he has in his research and analytical habits tools which are much more difficult to acquire and which the usual professional soldier utterly lacks.

As a matter of fact, a well-rounded political scientist is likely to possess much special knowledge not usually included in the education of the soldier but nevertheless directly pertinent to the pursuit of strategic insights. It is noteworthy that the new National War College—a four-service organization (Air, Navy, Ground Forces, and State Department) dedicated to the advanced strategic training of selected senior officers—has seen fit to devote the entire first term of its two-term course to what amounts to a survey of international relations. The teaching staff for that term was in the first two years of the College recruited from civilian universities, and apparently that procedure is to be a permanent feature of the College. Both the method of recruiting teaching personnel and the field of study represent a wholly new departure in service school training—one which perhaps indicates that in this respect the "military mind" is less backward than the academic mind. For the National War College represents an effort on the part of the military to do what the political scientist is conspicuously failing to do on his part.

But the argument for a greater attention on the part of the political scientist to military studies does not hinge primarily on his ability to contribute to strategic knowledge. The field of grand strategy, though unquestionably germane to that of international relations generally, is nevertheless distinct from it and from any organizational point of view peripheral. It should be possible for the individual political scientist to concentrate his research energies in that field without suffering loss of status

as a political scientist, but such concentration should be the province of a small minority in the profession who enjoy special equipment for the study and keen interest in pursuing it. The larger question, and the one more pertinent to the profession as a whole, is whether military studies can contribute to the intellectual stature of the political scientist as a political scientist. It is not alone a question of what he can contribute to a study of strategy but also what he can gain from it. Only under conditions where the whole profession realizes its dependence upon sound strategic insights will the individual member who possesses the requisite tools and inclination be encouraged to develop military studies as a special field.

It is worth noting that even if a real collective security system appeared to be attainable, its realization would depend upon a sound estimation of strategic realities. Certainly a plan which ignored those realities would be so transparently incapable of meeting crises as to forfeit at the outset the confidence which is the first requisite for the success of any security system. Thus, the elimination of the veto would not in itself transform the United Nations into an organization capable of keeping the peace among the super-powers. It might in fact have the opposite tendency, unless such elimination were coupled with a drastic revision of the procedures envisaged for the application of military sanctions.[1] It is because they cannot conceive as politically or physically feasible that degree of revision sufficient to provide against transgressions of super-powers that some observers have labelled the veto simply a realistic recognition of a pre-existing condition.

But whatever our predictions concerning future trends to-

1. It is significant that in the preparations for the San Francisco Conference of 1945, the American delegation busied itself scarcely at all with exploring the presumptive character of the military agreements which were to be provided for in the Charter. The few meetings of the technical experts concerned did not amount even to a superficial penetration of the problem. Equally significant is the fact that no other delegation at the Conference—including the French, who sponsored the more telling revisions of the relevant Dumbarton Oaks provisions—manifested substantially greater preparation on the issues of military sanctions.

ward international cooperation and the success of our efforts to further those trends, two facts stand out as paramount. First, in the modern world the objective which takes precedence over all others is security against war. The point is worth mentioning only because of the necessity of emphasizing that pursuit of that objective is not inevitably identical with pursuit of smoother and more intimate international cooperation, the two being especially divergent where the latter holds out little promise of significant success. We need not, however, trouble ourselves unduly with whether we mean world security or simply American security, since the two concepts are practically synonymous in a world in which a major war without American participation has become almost inconceivable.

Secondly, it is important to remember that the only significant channel available to scholars for influencing international relations is that of national policy. Because of the status of the United States among the nations, this channel happens to be an exceedingly important and effective one. And the very term "national policy" connotes something comprehensive, something made of many parts and subdivisions which, if the policy is to be sound at all, must fit together reasonably well. The relevance of this fact to our present discussion is best summarized in a comment of General Eisenhower's: it requires strength to cooperate, weakness can only beg. It follows then that one of the most important ingredients for a sound foreign policy is a national policy designed to promote our strength. This statement should be a platitude, but the public utterances of many political scientists oblige this writer to conclude that it is not yet such.

It behooves the political scientist to set about analyzing the problem of atomic-age security in a two-power world. It is a world in which the polarity of power is matched by a polarity of ideologies, and in which the ordinary barriers to human communication and sympathy, difficult enough to scale at best, are many times augmented by deliberate state policy on the part of one of the two primary powers. If the scholar insists on speaking in terms of an ideal world wholly different from that which confronts him, he is merely rejecting his problem. Nor can he conceal that rejection by avowing concern only with long-term

objectives. To the remark of Lord Keynes that in the long term we are all dead, we may now add the observation that in the short term we may all be killed. The business of devising and pursuing a sound national policy in a desperately dangerous world is something which demands all our intelligence as well as our moral strength.

II. Fields of Research

What then are the specific areas of research suggested by the above observations, areas appropriate for exploration by political scientists? Because of the dearth of work thus far done in the field, it is sufficient to mark out the areas in broadest outline. If we assume the truth of the statement that to be effective a national foreign policy must be based on strength, the fundamental question which confronts us is: What is the national strength? How can it be maximized and most efficiently organized with a minimum of interference with that complex of cherished benefits and habits which we refer to vaguely but perhaps adequately as "our way of life"? And just how high does that "minimum of interference" have to be under existing technological and political conditions? On the obverse side we must ask: What customary means of enhancing the national strength have been outmoded and thus, if they continue to be pursued, lead only to useless provocation? This fits indeed into the larger question: What are the necessary restraints, in terms of our foreign relations, upon our pursuit of maximum strength?

It is obvious at once that the pursuit of the answers to any of these questions catapults us at once into the consideration of military strategy. It is equally obvious that they are answers which the political scientist must seek if he is to justify his field as an integrated and comprehensive discipline and his own utility as an expert in matters of public policy. And in the search for those answers, the professional soldier can, by and large, furnish him but limited assistance.

The appropriateness of this general field of research for the political scientist is heightened by the fact that while a grasp of military principles is essential for its proper handling, it expands on all fronts far beyond the limits of purely military considera-

tions. The professional soldier is becoming increasingly aware that national strength implies much more than the existing military establishment. He realizes that it encompasses such issues as the soundness, progressiveness, and smooth functioning of the national economy; the health of the nation's social structure; the integrity and efficiency of the political system; and our relations with friendly states possibly disposed to align themselves with us in a crisis. What he is not equipped to probe is the true character of the *relationship* between purely military strength and those other aspects of the national strength.

The time is past when the political scientist could feel he had discharged his obligations in this general field by surveying the location of the "strategic raw materials" of the world and drawing up a balance sheet between haves and havenots. The recent war demonstrated what should have been obvious before—that raw materials are only one of many ingredients essential to the compounding of national strength, that until they have been processed into finished materials they are meaningless as well as useless for their ultimate purpose, and that the process of converting them into finished materials on an adequate level of output requires an intensive and speedy industrial mobilization. Industrial mobilization, like the general mobilization of which it forms a part, is a highly dynamic process influenced by all sorts of economic, political, and even cultural conditions which the political scientist, because of the eclectic nature of his equipment, should be well qualified to analyze.

Even more directly pertinent to political science is the study of the mobilization of man power, whether for the armed services or for industry. This field is of particular current importance in relation to questions such as peacetime conscription and universal military training. Many political scientists no doubt have, as citizens, strong feelings on these matters, but who among them are able to present a well-reasoned and informed judgment, in which costs are balanced against needs, on the case for or against universal military training? To be worth anything such a judgment would have to be based on a close scrutiny of the wealth of experience gained in World War II, integrated with a careful estimate of strategic requirements of the future,

and balanced against considerations of political feasibilities and objectives.

In an age in which major wars may be suddenly initiated and all too quickly decided, continuing peace may depend largely on our ability to gather and digest accurate intelligence concerning the intentions and capabilities of particular powers. Of the importance of this problem the military have long been aware, but in this case again the scope of the problem far exceeds the capacity of their equipment for dealing with it. In the estimation both of capabilities and especially of the intentions of foreign powers, the political scientist accustomed to rigor in the gathering and analysis of data should have a good deal to contribute. It must be emphasized that such operations are almost as important for nations which are deemed to be friendly as for those in which we fear a disposition to hostility. For example, one of the critical problems confronting our political and military policy-makers today is that of estimating the true alliance value to us of the countries of western Europe and especially of Great Britain. There are plenty of reasons other than strategic ones prompting us to assist those countries back to their feet economically, but certainly strategic considerations will condition—even if they do not govern—our conception of the degree of assistance warranted and the urgency with which this problem must be considered. And the conception of strategy entailed in the handling of such a problem must be a broad one, broad enough to comprehend the moral and psychological factors entering into a strategic estimate.

Political scientists have long since adopted as peculiarly their own the field of propaganda, which during the recent war was significantly labelled "psychological warfare." However, it is extremely doubtful whether research in this field has begun to keep up either with the new materials made available or the needs of the time. There are those who insist vehemently that the Soviet Union is already conducting unrestrained warfare against the United States on the psychological warfare level, and that we are losing all the battles by forfeit. Whether or not such a view exaggerates the facts or distorts their implication should be answered by political scientists who are appropriately specialized,

and if their considered view is such as to support the one just cited they should be able to offer some operational guidance to whatever efforts are set in train to remedy the situation.

On the side of restraints, it is necessary to have the answers to such questions as whether the bases in the Pacific desired by our armed forces are really important enough for strategic reasons to warrant our demanding them. Would we be significantly compromising our moral position before the world in making such demands? Similiarly, is the petroleum of the Middle East important enough for us strategically to warrant our government's intervening in that area beyond the degree which would be indicated by purely economic consideration? In the past political scientists have in the main been disposed to discount the the allegations of the military and to emphasize the provocative results which might be expected from satisfying their demands. That has been conspicuously true of the whole question of required levels of armament or movements toward disarmament. Certainly that is a legitimate and necessary function insofar as the exercise of it is based on objective and reasoned analysis. But have political scientists even prepared themselves adequately for that kind of analysis?

Moreover the politically self-sufficient nation-state is passing; the "solar system" of the great power and its satellites is with us as one of the great facts of the world order. The political scientist can play a major role in analysing what such a development means to military strategy. How firm or how brittle is such a cluster or group of nations? Does the additional allegiance or affiliation of a weak nation add to the strength or weakness of the group? The military implications of these and other questions require the approach of the political scientist as well as the military man.

In the fields of domestic politics and of public administration, political scientists have not begun to direct at questions concerning the national defense and the size and character of the military establishment a degree of attention commensurate with that bestowed upon other functions of government. The sheer size of the military budget today, let alone the importance of its purpose, would certainly warrant their major concentration on questions

relating to the processing of that budget and the expenditure of the monies allocated. Is the budget too large or too little for the needs of the time? Is the huge sum of money being spent on national defense being spent *efficiently* for that purpose? Is it desirable that Congress continue to control military appropriations on an item by item basis or should the departments concerned be voted lump sums to use according to their discretion? Are our methods of recruiting and training officer personnel appropriate to the needs of a democracy and to the objective of maximizing the professional efficiency of that personnel? To what extent and in what manner do considerations of local politics influence military appropriations? In what other ways is our governmental mechanism deficient in providing for the national defense? Should and can military appropriations be adjusted to various economic objectives of fiscal policy, such as that of ironing out the business cycle or otherwise minimizing unemployment? Professor Pendleton Herring in his pioneer work of some years ago, *The Impact of War,* pointed the way to a rich field of research, but unfortunately his lead was not followed by any significant number of scholars.

The atomic bomb obtrudes itself upon, if it does not dominate, all questions of military policy and the relationship of that policy to our foreign affairs. It figures in every one of the questions posed above, and clearly accentuates the urgency of answering them. In addition it poses gigantic special problems. There is the problem of achieving effective international control of this horrid weapon, the use of which in future war would almost certainly mean the end of our civilization. This problem by no means rules out that of adjusting to it politically and militarily in the event that effective control proves impossible of achievement. Can a democracy like ours take the steps essential for that adjustment? What revisions in our political and governmental processes are necessary to give our democracy survival value in a world in which hostile totalitarian states possess atomic bombs? The colossal magnitude of this question has apparently numbed political scientists into unawareness of its existence. But is it not the basic question of our time? The contributions thus far of the political science profession to the whole fund

of questions created by the successful release of atomic energy have been absurdly and pathetically meagre.

Why has this been so? Undoubtedly it is in large measure due to the political scientist's limitations in equipment. In the case of the atomic bomb especially, he was confounded by the explanations of the physicists, which served only to remind him that he had small mathematics and less physics. He felt frustrated too by the barrier of secrecy and the knowledge that some simple fact which was being kept from him might stultify all his conclusions. In the case of military matters generally, he felt the want of a knowledge which was not even taught in the schools. Almost every field of human endeavor, certainly anything which could by the most liberal construction be labelled a science, was represented in courses at some university or other *except* military strategy. The so-called "military science" training included under R.O.T.C. programs was obviously concerned with banalities which had absolutely nothing to do with the philosophy of a Clausewitz or a Mahan.

That is a large part of the explanation, but it is not all of it. The recent war introduced many political scientists in a most intimate way to matters related to the conduct of war. Many of them were obliged to absorb both the knowledge and the folklore which distinguishes the professional soldier. But there is as yet little evidence that this experience, intensive as it may have been, is being exploited in their research by scholars returning to civilian life. They are anxious to get back to the kinds of preoccupations which absorbed them before the unpleasant interruption. Clearly there is another reason, one which springs from a sense of moral values, from a conviction that institutions of learning exist to develop and impart techniques which will improve and multiply the means for a better life—not to increase knowledge of how to destroy those means and life itself.

This conviction is an old one among scientists. Niccolo Tartaglia, founder of the science of ballistics, kept to himself the result of his experiments in 1531 because he had decided that "it was a thing blameworthy, shameful, and barbarous, worthy of severe punishment before God and man, to wish to bring to perfection an art damageable to one's neighbor and destructive

to the human race," and that to concern oneself with such matters was "a grave sin and shipwreck of the soul." When the Turks threatened an invasion of Italy, however, Tartaglia changed his mind. "Today," he wrote in the preface to his work, "in the sight of the ferocious wolf preparing to set in our flock, and of our pastors united for the common defense, it does not seem to me any longer proper to hold these things hid, and I have resolved to publish them so that all should be in better state either to attack the common enemy or to defend themselves against him."

The social sciences have in large measure contributed to that modern sophistication which saves us from the childish kind of distinction between "we" and "they" to which Tartaglia succumbed. It is a great advance to be able to speak of a possible opponent in terms more sober than "ferocious wolf." It is an advance not only intellectually but practically as well, for the understanding of the potential opponent opens many new avenues for resolving tensions by friendly negotiations. But if we permit ourselves to feel that our understanding, however much it be perfected, will enable us to resolve all tensions between us in that manner, we are not merely deceiving ourselves; we are denying the elementary facts of life. It is imperative that quarrels between us and the Soviet Union continue to be settled pacifically; but as aids to such settlement we shall have to rely on much more than the friendly word or gesture.

Chapter VIII

MILITARY GOVERNMENT AND OVERSEAS ADMINISTRATION

Carl J. Friedrich and Arthur A. Maass

PREFATORY NOTE

At the Philadelphia Meeting of the American Political Science Association in May, 1946, a small group of scholars, most of whom had recently participated in military government activities in the various theatres of operations or at Washington, met for dinner at the invitation and under the chairmanship of C. J. Friedrich in order to discuss informally the problems of future research and training in the fields of military government and overseas administration.

The response at this dinner meeting was so enthusiastic that the chairman suggested to the Research Committee of the American Political Science Association that it establish a panel on research in military government. Such a panel was authorized in September, 1946.

The first formal meeting of the panel was held in Cleveland in December, 1946, in connection with the annual meeting of the Association. Participants were: C. J. Friedrich, Harvard University (chairman); A. A. Maass, Harvard University (secretary); J. T. Caldwell, Vanderbilt University; J. Q. Dealey, Jr., War Department; Joseph Dunner, Grinnell College; H. T. Goss, War Department; H. J. Heneman, United States Department of State; R. M. Kempner, Lansdowne, Pa.; M. M. Knappen, Michigan State College; E. G. Lewis, University of Texas; R. E. Long, University of Vermont; J. B. Mason, Oberlin College; Franz Neumann, Columbia University; R. G. Neumann, University of Wisconsin; Wallace Parks, Bureau of the Budget; W. D. Stout, University of Kentucky; P. H. Taylor, Syracuse University; W. W. White, Western Reserve University; Quincy Wright, University of Chicago; Harold Zink, Depauw Univer-

sity. At this meeting the panel discussed a wide range of topics related to research in military government, adopted a resolution concerning accessability of research materials, and agreed upon the necessity for a further meeting in the near future.

The second formal meeting of the panel, and the meeting at which this report was adopted, was held at Williamstown, Massachusetts, June 13 and 14, 1947. The chairman, C. J. Friedrich, was in Germany on a military government mission at the time and H. J. Heneman acted for him. The list of participants follows: Comdr. Sydney Connor, Joint Chiefs of Staff; Lashley G. Harvey, Boston University; Harlow J. Heneman, State Department (acting chairman); Lt. Col. Chester L. C. Johnson, United States Military Academy, West Point; Marshall M. Knappen, Michigan State College; Arthur A. Maass, Harvard University (secretary); John Brown Mason, Oberlin College; Donald S. Macdonald, Massachusetts Institute of Technology; F. E. Ropshaw, Civil Affairs Division, War Department; Robert H. Slover, Harvard University (associate secretary); Philip H. Taylor, Syracuse University.

I. Broadening Scope of Panel

It is significant that the panel voted to change its title from "research panel in military government" to "research panel in military government and overseas administration." The longer title was intended to cover, not only the research problems of military government in the literal sense as that term is used by the armed forces, but also the related research problems in the fields of civil affairs, colonial administration, dependency administration, trusteeship administration, and the administration of occupied areas. With the proposed transfer of responsibility for control of occupied areas from the military to civil departments or authorities, the broader frame of reference appears desirable. Further, these problems are so closely interrelated that a survey of the research potentialities of the one without a consideration of the others would be unreal.

In this connection, the panel appreciates that research in any of the more important problems in military government and overseas administration cannot be confined to political science.

Anthropology, political economy, sociology, and other related social sciences must contribute jointly with political science to the conduct of research in military government. The coordination of such joint research indicates the need for a permanent coordinating agency, and recommendations to this effect are contained in Section VI of this report.

II. Evaluation of Existing Plans for Research

The panel has surveyed known plans for research in military government and overseas administration. This survey has included (1) plans for the preparation of books, articles, or dissertations by individual scholars; (2) plans for the conduct of institution and joint research; (3) plans for the conduct of Government sponsored research; and (4) the compilation of a list of unpublished studies.

The survey has revealed a surprisingly small amount of research underway. The panel attributes this primarily to the general inaccessibility of research materials, a subject discussed in more detail in Section IV of this report. Other causes are lack of research funds and lack of publication media, both discussed in Section V of this report. The panel finds no evidence of lack of interest or lack of available scholars qualified to conduct research in military government and overseas administration.

Furthermore, in connection with such research as is at present being undertaken, the panel is aware of the need for coordinated planning to reduce duplication of effort and to insure a more economic use of available research materials. Such planning indicates the necessity for a permanent coordinating agency, and recommendations to this effect are contained in Section VI of this report.

III. Urgent Topics of Research

Military government will be a central feature of government for some time to come. It profoundly affects internal and external policy of the great powers. It has been intimately linked to war, and it is concretely linked to the peace. The issues which military government raises are among the least explored of problems of modern government. They must be faced, and

faced now. They confront the public, and they confront all those who, whether inside or outside the military, are charged with preparing men for participation in military government duties in Europe and in Asia. Military government is becoming a part of the regular training of officers of the United States armed forces, as indeed it should be. The importance of the field, hitherto almost completely neglected by American scholars, is slowly becoming recognized.

Although there has now evolved a considerable interest in military government on the part of some scholars, there is still lack of completed research on any important topics in the field. Works such as Harold Zink's *American Military Government in Germany*, and Hajo Holborn's *Military Government* constitute helpful beginnings. But these authors recognize the urgency of more extensive research.

The striking lack of attention given to the problems of military government in the past by social scientists generally raises the problem: why should this have been so? Why should military government have been allowed merely to occupy a small niche in the field of international law? On the whole, the answer lies in the fact that military government, from a strictly practical standpoint, is transitional, temporary, and related to the emergency of post-war readjustment. Modern political science, like other social sciences, has shown a general disinclination to concern itself with the problems of war, especially in the United States. War being deemed bad, an aberration due to "mistakes" and "guilty sinful behavior," was evidently felt to be no very proper interest of scientific inquiry. Yet, the United States, of all countries, has had an extraordinary experience in military government at the end of its greatest crisis during the nineteenth century, the Civil War. Until fairly recently there have been few searching studies of the history of military government in the period of "reconstruction." Among these Paul H. Buck's *Road to Reunion* is especially noteworthy. The first chapter is a remarkably lucid statement of the problems which confront a democracy which tries to bring a defeated enemy "back into the fold."

Military government experience goes a long way to show

that force can, to some extent, be a significant factor in shaping and changing social institutions and traditions. It is possible, as revolutionaries have always claimed, to impose from the outside a new order and have it take root. If the new order helps to solve vital problems confronting a community, then military government, when dedicated to the task of "reforming" a people, is in effect a revolution from without. This conception of the "constitution" as an educational device was very popular with the Greeks, especially Plato. It runs strictly counter to modern (conservative) ideas of organic growth. From this standpoint it appears "mechanistic." At the same time the ardor concerning democracy has obscured the vision. Military government may be conceived in terms of "reform" by Napoleon, Hitler, and Stalin no less than by the North in the South, or by the United States in the Philippines, or by the Allies in Germany and Japan. Even more than democracy other ideological systems are inclined to employ the force of arms to revolutionize a given society. The striking analogies, which here present themselves, are often obscured by the fact that the whole subject is engulfed in propagandistic distortions resulting from the belief in one of these ideological systems. It is important for political scientists in a democracy not to allow themselves to be misled by their convictions that democracy is valuable and gainsay the use of force which military government always entails. Indeed, this is especially important when military government is used to extend the usages and contributions of constitutional democracy, because constitutional democracy encounters peculiar difficulties in this field. It is believed to be undemocratic to use force. Actually, of course, no democracy could survive for a single day, if it did not "enforce" its laws. But even if the problem is stated more accurately as "force approved by the majority of those over whom it is exercised," theoretical and practical issues of great complexity remain. To be sure, the present enterprise of the military occupation of Germany and Japan may exceed in vastness of scope anything hitherto undertaken; for the military occupation of France by the armies of the German Empire after 1871 was devoid of ideological significance; a military dictatorship of aggressive de-

sign had been routed, but the French people themselves were greatly divided and Bismarck did not make any serious effort to promote the re-establishment of a monarchy. What further enhances the unprecedented novelty and intrinsic interest of the military governments of the present time is the fact that they are joint enterprises of several powers which attach quite radically divergent meanings to "democracy" and "democratization." This has made the related process of "denazification" a prime object of controversy and disagreement. The revolutionizing of German society is following different patterns in the different zones.

All this and many other items, such as de-industrialization, and economic reconstruction, constitution-making by military fiat, constitutional reforms and re-education, to name but a few, show that military government is a subject with wide ramifications. All of the conventional institutions of political science have some real interest in the results of research in military government. Political theory and comparative government, international law and relations, public administration and party politics, they all are involved. In order to illustrate these wide relationships, and at the same time serve as a sample of what needs to be done, a breakdown of research topics follows:

1. *The importance of civil affairs/military government policy as a phase of foreign policy.* This topic needs exploration, both in terms of historical experience where it would be a matter of showing how military occupations have figured in the development of foreign policy, and as the subject of contemporary analysis for the present and future obligations in this field explored in the light of the emerging patterns of our foreign policy.

2. *The importance of civil affairs/military government in attaining military objectives.* This subject is especially significant in relation to the historical struggle just passed, and it may receive inadequate attention from writers primarily concerned with military operations. It is often easy to lose sight of the importance of military government while the conflict is actually raging, but it is during that very period that so much groundwork is laid that spells success or failure after the guns cease.

3. *The conflicting interests of security and democracy in determining occupation policies.* While security and the spread of

democracy might be considered the primary interest of the United States in its prosecution of occupation policies, other interests also need careful analysis—such as the securing of commercial advantages, the safe-guarding of American property, and the complex problems of the balance of power.

4. *Civil affairs/military government patterns of organization both at Washington and at the various governmental and administrative levels in occupied territories.* The most perplexing problem here is, of course, that of the relationship of the military to the civil authority, including such specific issues as how to combine the emphasis on status in the Army with the lack of such in civilian organizations.

5. *Analysis of civil affairs/military government operations at the various governmental and administrative levels in occupied territories.* The field of possible investigation in this area is vast, since, after all, military government embraces all phases of modern government. In fact, military government under contemporary conditions is government under planning.

6. *Comparative studies of the American experience in military government and overseas administration and the experiences of other nations.* Some extremely interesting studies could be made under this heading, not only by contrasting the democratic occupation with the methods used by the totalitarian but also by utilizing the extensive knowledge we now have of Nazi and fascist methods for a comparison with the Soviet occupation.

7. *The legal basis and problems of military government and overseas administration.* A veritable revolution is in process in this field of international law. Many of the ramifications and implications are as yet dimly perceived.

8. *Personnel policies in military government and overseas administration.* Our knowledge of what type of personnel is required for military government administration overseas under democratic conditions is as yet woefully inadequate. At present policies of this nature are largely of the trial and error kind—and mostly of the latter.

9. *Training for military government and overseas administration.* One of the most depressing aspects of American military government operations is to be found in this field of training.

The remarkable initiative developed by the War Department during the war has borne very little fruit in the postwar period, and the whole training aspect of personnel has been allowed to go by default. How to adjust wartime activity in this field to peacetime conditions is a topic of great urgency.

10. *Civilian supply and logistics in occupied areas.* This field is at present the subject of much study on the part of government agencies. Critical evaluation of such *ad hoc* efforts is a task to which American writers have made no significant contributions.

11. *Relationships between occupying powers and cooperating indigenous governments.* Military government has been said to be a species of dependency governments. It bears close resemblance to colonial government and the charge of imperialism is frequently leveled at it. Rapidly accumulating experience in this field is of primary importance for comparative government.

12. *Relationships between powers in combined control or occupation of a single area.* Penetrating studies based on field experience with the Allied Control Council in Germany and with similar bodies elsewhere are now being undertaken by some political scientists. They deserve all the support that can be secured for them.

13. *Functional studies such as in the fields of economic reconstruction, refugees, displaced persons, and public health* (for example, venereal disease control). This field is closely linked to what has been said under (5) but it names some functions peculiar to the period immediately after a great war which deserve special attention and research.

14. *Interrelationships among military government units, tactical troops, civilian administration, and civilian populations.* The most urgent issue here is that of the protection of civil liberties of the people of the occupied territory. The gradual progress of military government in bringing troops under control is a vital aspect of democratic control and practice.

15. *Appraisal of methods of reorientation for democracy, educational reform, and public opinion studies.* Many programs and well intentioned plans have been set forth in this field, and attempts have been made to carry out some of these in the face of

persistent sharp criticism. It is vital that the success and failure of this entire range of activity be continually studied and appraised by students of political science and government as well as by educators.

16. *Comparative aspects of military government; the peacetime administration of liberated, occupied, and dependent areas; and trusteeship administration.* This broad field relates to what has been said under (11). The possibility of trusteeship as a transitional phase needs much further analysis than it has hitherto received.

It is obvious from the above that any of these topics might become the basis of study of scholars representing several of the social sciences and it is hoped that such will be the case.

IV. Access to Research Materials

Problems of classification and availability of materials at War, Navy, and State Departments.

The panel has long realized that the principal obstacle to the conduct of scholarly research in military government and overseas administration is the general inaccessability of research materials. In this connection the panel adopted a resolution at the Cleveland meeting requesting the War Department to make certain types of classified material available to qualified scholars and to declassify such materials relating to military government as are not of current operative importance. The Assistant Secretary of War, Mr. Howard Petersen, in reply to the resolution, stated that (1) certain unclassified basic reports are now being distributed in limited quantity to university libraries, (2) it is his hope that military government materials will be given a high priority for downgrading, (3) the Director, Intelligence Division, has been asked to advise of the security factors which would have to be considered in establishing a preferential category for qualified scholars.

With this information at hand, and based on their own experiences and those of their colleagues and students in gaining access to research materials in Washington, the panel has examined three related aspects of the problem.

First, the availability of unclassified materials. The Civil Affairs

Division of the War Department has turned over to the Library of Congress copies of all unclassified research materials. The Library has indexed these and issues a periodic bibliography which is available to all libraries. The Library of Congress will microfilm these documents at cost for interested scholars.

Where the Civil Affairs Division has sufficient copies of unclassified materials, they distribute them to a list of interested public and university libraries.

The War Department (Research and Analysis Branch, Civil Affairs Division), Navy Department (Office of Naval History and Office of Island Governments), and the Joint Chiefs of Staff (Historical Section) stand ready to assist scholars who contact them concerning the availability of unclassified materials. However, any contacts with these organizations would be facilitated if the scholars were certified by a central professional agency, and recommendations to this effect are contained in Section VI of this report.

Second, downgrading of classified materials. Unclassified research materials now available are not sufficient for the conduct of any quantity of scholarly research. Most of the important background materials, materials which in many cases are known to members of the panel and to thousands of others who have worked with them in the various theatres of operations, are still classified and not available for research use. Many official histories of military government operations have been prepared in the field and at Washington. As far as is known, none of these histories has yet been published or otherwise made available.

To date this panel has had no success in speeding up the downgrading of any quantity of materials at the War Department nor in obtaining clearance for any of the official histories. The War Department, however, has indicated it will cooperate with scholars in this field in establishing priorities for the reexamination of classified materials with a view to downgrading. A coordinating professional agency to work with the War Department on behalf of all scholars in the field would be highly desirable, and recommendations to this effect are contained in Section VI of this report.

Third, making classified materials available to qualified schol-

ars. The panel has noted that qualified scholars in certain fields have gained access to classified materials under special arrangements with the War and Navy Departments. The panel had hoped to obtain similar privileges for qualified scholars in the field of military government and overseas administration, but all efforts in this regard to date have failed. The panel recommends continued negotiation with the service departments to this end. Such negotiations could best be conducted through a coordinating professional agency, and recommendations to this effect are contained in Section VI of this report.

Collection and availability of basic research materials outside of Washington.

So far this report has discussed largely the availability of research materials at Washington. But if any quantity of research is to be conducted in the field of military government and overseas administration, materials must be made available throughout the country. There are few scholars who can afford long sojourns in the nation's capital. The Library of Congress will microfilm any materials in its collection, but scholars are hesitant to use this expensive process until they have examined the materials to ascertain definitely their value.

As noted above, the Civil Affairs Division, War Department, does distribute to a list of interested public and university libraries copies of unclassified materials which it possesses in sufficient quantity to make a complete distribution. Where, however, the CAD possesses some copies for distribution but not a sufficient number to supply the entire list, the copies are sent to the Library of Congress, and no distribution is made to libraries outside of Washington. The panel recommends that the CAD continue its present distribution to the full list where sufficient copies are available but, in addition, that it designate some twelve to fifteen depository libraries to which will be sent *all* available materials, including those in short supply. These depository libraries should be located with reference to the concentration of scholars interested in the field and to the existing collections and facilities of the libraries. The panel suggests the following: Harvard University, Yale University, Columbia University,

Philadelphia Public Library, University of Virginia, University of Texas, Syracuse University, University of Michigan, St. Louis Public Library, University of Chicago, University of Minnesota, Denver Public Library, University of Washington, Stanford University (Hoover Library), University of California at Los Angeles.

Field research.

The panel has noted the almost complete lack of field research in military government and overseas administration. This is due in part to the difficulties encountered by scholars in gaining admittance to occupied areas for sufficiently long periods of time. The panel feels that scholars representing the American Political Science Association and related organizations in the social sciences should receive equivalent treatment in occupied areas to those representing religious and press organizations, for example. To this end efforts should be made, through the War Department, the commanding generals of the theatres of operations, and the State Department, to relax restrictive regulations and to encourage scholarly research by providing rations and other supplies at cost to scholars in occupied areas. These efforts can best be coordinated through a central professional agency, and recommendations to this effect are contained in Section VI of this report. When such arrangements have been negotiated, it is felt that the various research foundations will be in a position to approve grants for field research in military government and overseas administration.

V. Methods of Conducting and Publishing Research

One of the most important factors in the conduct of research today is time. Thousands of Army and Navy personnel have recently participated in military government operations and thousands of military and civilian personnel are today participating in various types of overseas administration. A large number of these personnel are in academic life; the vast majority are not. Be they teacher, student, engineer, or politician, however, many of these people have valuable experiences to record based on first hand knowledge, and they are anxious to do so provided

the facilities are available. The immediate need is for getting as much material as possible in writing before memories dim. More careful and scholarly evaluation can proceed at a more leisurely rate.

Apart from availability of research materials, the most serious obstacle to conduct of such research is lack of media for publication. The panel has surveyed the present media and finds serious limitations. With respect to articles, in addition to some few articles that have appeared in popular magazines, the *American Political Science Review, Public Administration Review, Public Opinion Quarterly*, and other journals in the social sciences have carried accounts of military government and overseas administration; but these journals are not in a position to accept any large number of contributions in this field at the present time. The panel recommends that scholars make more use of the military and navy journals, a number of which have already carried articles on military government. The panel further recommends that negotiations be conducted with the newly formed Military Government Association (an organization of former military government officers headed by General Hilldring, Assistant Secretary of State) concerning the nature of its prospective journal. Such a journal might be designed to provide an outlet for scholarly articles.

With respect to contributions of greater length than those normally accepted by the journals, the situation is more serious. Book publishers will not absorb any great quantity of work in the immediate future; and time is important in this new field. The panel has considered the possibility of establishing a new series of monographs and/or case studies. The monographs might be reproduced cheaply, possibly through the special publication arrangements established between the APSA Research Committee and Edwards Brothers, Ann Arbor. The case studies might be organized into a series similar to the studies on public administration issued by the Committee on Public Administration of the Social Science Research Council.

Either or both of these arrangements would require some initial financial aid and a central professional agency to edit and publish the series. With respect to funds, the panel has suggested

that the Social Science Research Council, the Military Government Association, and other agencies be contacted. Recommendations with respect to establishing a central agency are contained in Section VI of this report.

In addition to the funds required for new media for publication, the conduct of many research projects in military government and overseas administration, particularly those involving field research, is dependent upon the availability of research funds. The panel is of the opinion that once the main obstacles to research have been met—access to research materials, gaining entrance into occupied areas, provision of publication media, and general coordination of research efforts—several of the large research foundations will look with favor upon research projects in this field.

VI. Military Government Clearing House

Throughout this report the panel has emphasized the need for a continuing coordination of research efforts in the field of military government and overseas administration. To meet this need the panel recommends the establishment of a Military Government Clearing House, with the following duties:

1. Generally coordinate research.
2. Maintain current file of all scholars, organizations, and universities interested in military government and overseas administration.
3. Maintain current file of all research projects proposed, underway, and completed.
4. Maintain current bibliographies.
5. Give advice to scholars designed to reduce duplication of effort and insure a more economic use of available research materials.
6. Coordinate work of political scientists with that of other social scientists. Help arrange for joint undertakings.
7. Maintain contacts with all cooperating organizations, such as Military Government Association.
8. Certify qualified scholars to the State, War, and Navy Departments and to the Joint Chiefs of Staff.

9. Work with the State, War and Navy Departments in speeding up downgrading of classified materials.

10. Negotiate with the State, War, and Navy Departments to obtain special arrangements whereby qualified scholars may gain access to classified materials.

11. Negotiate with War Department, commanding generals of theatres of operations, and State Department to obtain satisfactory conditions for the conduct of research in occupied areas by qualified scholars.

12. Contact Social Science Research Council, Military Government Association, and other organizations to obtain funds for financing a new series of monographs and/or case studies.

13. Edit and publish the new series of monographs and/or case studies.

14. Seek new research funds and endorse individual requests for such funds.

15. Cooperate with the military services with regard to their research and training programs.

16. Lend assistance to War Department in its civil affairs affiliation (reserve) program and in the development of correspondence studies for reserve officers, and to Navy Department when and if it establishes a reserve program for military government.

The panel recommends and requests that the American Political Science Association seek to obtain financial support for such a clearing house from the Social Science Research Council or from some other organization. The staff of the clearing house should include a full or part-time director, secretarial services, and funds for a limited reproduction and distribution of circular material.

The panel feels that it can not overemphasize the importance of establishing such a clearing house. In contrast to the fields covered by other research panels of the Association, military government and overseas administration is a new field which requires organized and coordinated exploitation. Such direction would be furnished by a Military Government Clearing House. There are no well established lines of research, no well established media for publication of research results, no firm foundations

of research on which to build. These ought to be established and, of equal importance, they ought to be established forthwith. The raw materials for this product should be utilized as a matter of urgent priority before any of them are lost. Unfortunately, some—those relating to military government in the combat stage —are already being lost or neglected.

Chapter IX

POLITICAL COMMUNICATIONS

Harwood L. Childs

PREFATORY NOTE

DURING THE SUMMER OF 1944, Professor Harwood L. Childs of Princeton University, while on leave in Washington with the Office of War Information, undertook to organize a research panel on political communications and initiate consideration of a research program. The first meeting of the panel did not occur, however, until the Philadelphia meeting of the American Political Science Association in March, 1946. At this time it was possible to report the results of a survey of the opinions of specialists in the field concerning research needs and priorities. The work of the panel was concluded at a special meeting in Princeton, August 17, 1946, on which occasion a few of those who had displayed most interest in it discussed the preparation of a final report. Those who participated in the Princeton meeting were: Archibald Crossley, Princeton, N. J.; Harwood L. Childs, Princeton University; Herman C. Beyle, Syracuse University; Julian Woodward; James McCamy, University of Wisconsin; Jesse McKnight; Ruth Inglis, Commission on Freedom of the Press; and Joseph R. McLean, Princeton University. Messrs. Wallace Irwin and Howard Ludden attended in the capacity of secretaries.

I. PREPARATORY EXPLORATION

We begin the report of the political communications panel by quoting the following letter from its chairman:

Princeton University
February 21, 1946

DEAR————

Several members of the American Political Science Association's research panel on Political Communications have submitted their views regarding research priorities in the field. On the basis of

these suggestions I have attempted to list below the broad areas which seem most pressingly in need of study at the present time.

1. Place, time, and media studies of the *effect* of communications upon the political attitudes of selected publics.
2. Place, time, and media studies of the *use* of communication channels by government agencies and pressure groups, domestic and foreign.
3. Studies of barriers to the free *flow* of communications.
4. Studies of the *content* of political communications, with special emphasis on the degree to which content is "balanced."
5. Further studies of the relationship between ownership and *control* of communication agencies to other aspects—use, flow, content, "balance," etc.

In preparation for the first meeting of the panel on Friday morning, March 29, at the annual meeting of the Association in the Bellevue-Stratford Hotel, Philadelphia, I am now canvassing other panel members to obtain their opinion of these preliminary suggestions.

1. In general, what do you think of the suggestions? Do you agree that the areas of research listed should come first in any program of political communications research?
2. Are there any important areas which have been omitted from the list? If so, what are they?
3. How should the areas listed be ranked in the order of their importance?
4. Within each broad area what specific research problems should be given high priorities?

I hope that you will be able to attend and participate in the forthcoming meeting. In any case may we have your views regarding research priorities for inclusion in our report to the Association.

Sincerely,

HARWOOD L. CHILDS

Almost without exception those who replied agreed that the five areas listed were the important ones, and that no important areas had been omitted. In a few instances, however, topics were mentioned which did not at first seem to fall into the five-fold classification. For example, Dr. Kris of the New School for Social Research stated:

A discussion of procedures in education should be included. School books in history create predispositions which later become obstacles to understanding of balanced presentation.

Dr. Warfel, then of the State Department, observed:

I do not spot any omissions from the areas of "political" communications. I would remind you to keep for secondary consideration the fact that every communication between nations ultimately is political. All economic and cultural activity potentially may be political even if it is designedly altruistic. A collection of art shipped from X to Y may be meaningless politically; the canvasses are the purest abstract art imaginable. Yet the presence of the art in Y and the resultant fanfare (if any) leads to a more or less favorable attitude toward the mental and intellectual stature of X. Especially interesting is the way in which favorable biographies and histories, written by people of other nations, are being disseminated by Russia in Latin America. The purpose doubtless is to gain the fruits of understanding through cultural devices; yet the end can be deemed purely political in its ultimate analysis. United States materials sent abroad doubtless serve the same purposes, although there may be a distinction made between those which are sent by philanthropic organizations and those sent by the government. Yet one remembers that Iraq is friendly to the United States because of the 500 Iraqui graduates of the American University at Beirut.

Mr. Bernays, public relations counsel, made the following points:

From the standpoint of real research, isn't it perfectly true that one of the most fascinating fields of exploration might be the relationships of the expert technician in public relations or communications to his client? It seems to me that if one could analyze the frustrations and blocks that exist between the political leader and his adviser a lot of points you raise in your #1 to #5 would be eliminated as necessary for research. In other words I think that the advice political leaders get from their experts is much better than what they actually do.

I have no doubt, for instance, that a man like Pauley received very good public relations advice relative to the effect of communications, relative to the flow of communications, and relative to the content that would bear upon his particular problem. From the standpoint

of the political scene and the consequences of his actions, it would be much more interesting to know what actually motivated what he did, despite what he may have been advised to do, than to treat the situation that actually results. From the standpoint of political reality the effects of any overt situation as broad as the one he was associated with do not have to be researched very much.

I think we are all pretty well convinced that communications do have an effect. We know within limits if communications are sufficiently black or white what that effect will be. What we don't know and what it seems to me would really be of great value is to attempt to find out just what is preventing the soundest results due to human elements as between expert techniques and the political leader. Here is a subject for research that it seems to me is socially much more useful than digging into the attempt to break down effects, uses, barriers, and content after the beans have been spilled.

Mr. Bernays was not alone in thinking that attempts to survey the whole field and break it down into its component research areas would be needlessly pedantic. Professor Lynd of Columbia was emphatic in his impatience with this kind of approach. He wrote:

There is real danger that the big problems will get smothered under a welter of neutral empiricism. I wish your panel could put first on its agenda the following:

Assumptions

1. There is at present a heavy class-wise bias in the ownership of communication agencies and in the control of the content of public communications; and this tendency is increasing.
2. This class-biasing of the raw material from which the people of democracy must try to "make up their minds" was not intended as part of the role of communication in a democracy, and it is factually the central threat to a democracy among those factors with which experts in the field of communication work.
3. This class-bias in public communication is not an accident, but rather reflects the central contradiction within democratic society—i.e., the contradiction between political democracy and private capitalism.
4. The resolution of this conflict is today democracy's No. 1 problem.

Questions

What do experts in the field of political communication recommend as specific steps to make the content of public communication in the United States democratically representative of all relevant sides of public issues?

I know this is a $64 question, and I don't know the answer. But I believe this is the master question from which social scientists today should proceed in the formulation of their research problems. Specifically:

1. What is the bias in the present presentation of labor news? What do you experts propose as a remedy for this? The answer is *not*: "Labor should set up its own press." The answer is much more complicated than that.

2. What do experts in political communications propose that democracy should do about the situation revealed in the lead-article in *In Fact* for February 25, 1946?

3. I believe democracy in our time is in very serious trouble. We can all go over Niagara in a rowboat "counting content" and doing other tidy things. Why not state the $64 question and ask squarely: "What do we as a body of experts in Political Communications recommend, and what research is needed to implement our line of recommendation?

4. I don't believe an industrial sociology will ever be entirely classless. That's a dream. My point is this: Living in a class-stratified society that wants to move toward more and more democracy, we research people should ask:

(a) At what specific points is our present class control of communication increasingly and at what points decreasingly damaging to democracy? (And I mean: really document that in terms of specific processes and specific segments of the society.)

(b) What changes suggest themselves, and what further research do we need to clarify and test the relative workabilities of these several changes?

A few of those who replied to the February 21 letter stressed the interrelated nature of the five broad areas listed and pointed out that attempts to segregate areas within the field were necessarily somewhat arbitrary and artificial. Professor Doob of Yale wrote:

A great deal of work has been done on all five of the topics you mention but little effort has been made to relate the five to one another. Numbers 1, 2, and 4, for example, of course represent arbitrary, pragmatic divisions which obviously are interrelated.

Dr. Gosnell of the United States Bureau of the Budget observed:

A study might cut across the five broad areas which you have suggested. Thus, a study of international broadcasting might cover the use, effect, and control of this medium.

Miss Inglis of the Commission on Freedom of the Press stated:

The broad areas for study which you have outlined seem to me to be the important ones. I would agree that the problem of defining and measuring the effects of mass communications should come first. It should include the effect upon political behavior (voting) as well as upon attitudes. Of course, an adequate effect study would necessarily include a content analysis and what you have outlined under your number 2. Your outline seems to omit any study of audiences, which is also of prime importance. Your numbers 3 and 5 might well be combined. It is difficult to separate the various parts of the communications process. In analyzing a communication situation, ideally one would want to investigate all aspects, including control, content, audience, and effects.

When all replies to the third question in the letter of February 21,—How should the areas listed be ranked in the order of their importance?—were summarized it appeared that the panel as a whole would place area 5 ahead of area 4,—that is, give precedence to studies of control rather than studies of content. Mention should be made, however, of a few departures from the typical pattern. Although the overwhelming majority gave first place to effect studies, Dr. McCamy then of the Department of Agriculture and the late Mr. Field of the National Opinion Research Center gave highest priority to studies of barriers to the free flow of communications. Dr. McCamy wrote:

My own inclination is to place number 3, i.e. barriers to the free flow of communications, and number 5, i.e. ownership and control, at the head of the list (these two seem to be so closely related that they are almost synonomous if I understand your terms), and 1, 2, 4, as following and of equal importance.

In fact both Dr. McCamy and Mr. Field gave number 5 at least second rank. Dr. Leo Rosten went even further, and was the only one to insist that number 5 be placed first, although Professor Lynd's pronouncement (see above) may well belong here. Dr. Rosten wrote:

> Suggest studies of interlocking control and policy, i.e. magazine and radio ownership. We need late studies of changes in ownership, i.e. magazine and radio ownership. We need late studies of changes in ownership, i.e. number of papers, radio stations, etc., owned by how many? Decline of diversified ownership? Increase in power of which?

The disposition of practically all the members of the group was to leave content studies in fourth or fifth place. As indicated above there is a slight preference for ranking control studies above content studies. The upshot of the matter is that the panel as a whole would give first rank to effect studies, substantially equal but secondary place to use and flow studies, leaving farther down the scale studies of the control and content of communications. It is abundantly clear that in the opinion of the vast majority of students of political communications the most urgent research problems at the moment relate to the effects of communications via press, radio, and motion pictures upon the opinions and attitudes of the publics and audiences served.

The response to question 4 on the letter of February 21,—Within each broad area what specific research problems should be given high priorities?—elicited an avalanche of suggestions of varied scope and method. On the basis of these replies the following summary is presented, not because it is in any sense a complete list, but because it furnishes illustrations of types of projects that might well be studied in each of the five broad areas. The attempt to list and classify research problems in any area is often perplexing. Such a listing may suggest that the problems are mutually exclusive, which is seldom the case. The imaginative scholar will almost certainly perceive in the case of such lists opportunities for combining problems, and the list below is no exception. In fact there are frequent occasions when

a total political communications situation may be the subject of investigation, calling for answers to a list of questions far more inclusive than those listed. It is quite impossible to anticipate all questions that may arise during the course of a study. One question usually leads to another. All that this panel felt that it could do was to raise various questions which would serve to precipitate further study. Mature scholars usually proceed in their research under their own steam, and seldom follow obediently the guidance gratuitously offered by research program makers. But lists such as the following may stimulate scholarly thinking along new lines. If so it will have served its purpose.

Suggested research projects in Political Communications

A. Place, time, and media studies of the *effect* of communications upon the political attitudes of selected publics:
   1. A study of the audience for specific types of media in selected foreign countries.
   2. A study of the exposure of opinion leaders in selected foreign countries to different types of media.
   3. A study of audience interest in important public questions.
   4. A study of areas of ignorance and misunderstanding in the United States, with special reference to the political consequences.
   5. An analytical study of the American public with special emphasis upon the extent to which various groups of adult Americans participate in political discussions and decisions respecting the major fields of political action.
   6. Comparative studies of the effectiveness of presentation techniques with various population groups.
   7. Comparative studies of the techniques of enlisting the support of strategically-placed leaders.
   8. A study of the specific effects of particular communications such as the promotion of factionalism, tolerance, etc.
   9. A study of the role of specialized media: i.e. trade journals, official communications (dispatches and instructions to official and unofficial representatives abroad, communiques, off the record statements by public officials to editors, official publications).

10. Studies of the effect of communications on voting behavior as well as political attitudes.
11. A study of the relative influence of communications and specific environmental conditions such as: crisis situations, isolation, population density, culture, social institutions, ownership of communication agencies, etc.
12. A study of the lasting effects of specific types of communications: i.e., Nazi and Japanese propaganda.
13. A study of the effectiveness of United States psychological warfare during World War II.
14. A study of the relationship between public acceptance and political behavior. (Does the public act as well as it knows?)
15. A study of the impact of polling data on Congress and on the administrative offices of government.
16. Further studies of the technique of measuring effect.
17. A study of the balance between the localizing and the standardizing tendencies of mass communications.

B. Place, time, and media studies of the *use* of communication channels by government agencies and pressure groups, domestic and foreign:

1. By foreign interests in United States.
2. By selected government agencies in the United States such as the State Department, state and local governments, legislatures, etc.
3. By government agencies abroad, especially in Great Britain, Russia, China, and France.
4. By political leaders in the United States and abroad with special reference to the reasons why they fail to use or follow the advice of experts.
5. By professional public relations counsel.
6. By political parties in the United States.
7. By the United Nations Organization and other international organizations.
8. By economic organizations, especially by business and labor groups during periods of industrial unrest and strikes.
9. By selected special purpose organizations, especially those

concerned with promoting international good will and understanding.

10. A comprehensive or representative survey of the sweep and scope of promotional activity within a definite area, such as the United States, during a designated period.
11. A study of the costs of using varying types of media.
12. Further comparative studies of communication strategies and techniques.
13. Survey studies of the objectives of communications use.
14. The role of communications in relation to other promotional techniques.
15. A study of the extent to which the use of communications is based upon and follows communications research.
16. A study of the competition for and barriers to the free use of communications.
17. Studies of the personal traits of users.
18. A study of the possible use of polls for a sort of by-election procedure—testing public sentiment between elections.
19. An annual study and survey of pressure group activity.

C. Studies of barriers to the free *flow* of communications:

1. Quantitative studies of the speed, direction, volume, duration, and differentials in the flow of communications, with particular reference to the international scene.
2. Studies of the effect of various factors upon the flow of communications such as the source of the communication, editorial policies of channel owners, public tastes and desires.
3. Studies of the flow of specific ideas with special reference to the reasons why certain ideas flow more rapidly and spread more widely than others.
4. A study of the sequence of communications flow with special reference to the role of opinion leaders and other intermediaries between producer and consumer.
5. Comparative, composite, and discrete studies of barriers to the free flow of communications such as:
   (a) Official and unofficial censorship.

(b) Costs—capital investment, rates, etc.
(c) Limitations of time and space.
(d) Language—reading difficulties, semantic obstacles.
(e) Illiteracy.
(f) Hidden barriers—import taxes on books, control of raw film stock, lack of standardization in radio sets.
(g) Policies of news gathering agencies.
(h) Lack of know-how and skill in use.
(i) The profit system.
(j) Official secrecy and the doctrine of 'privileged communications.'
(k) Patent laws.
(l) Isolation and inaccessibility of facilities such as libraries.

6. Studies of techniques for measuring the flow of communications.
7. Studies of methods for obstructing the flow of bellicose communications, anti-democratic propaganda, etc.

D. Studies of the relationship between ownership and *control* of communication agencies to other aspects,—use, flow, content, balance, etc.:

1. Studies of inter-locking control and policy as, for example, in the case of radio and magazines.
2. Studies of intra-government control of communications output.
3. Studies of recent changes in media ownership.
4. Psychological studies of owner attitudes and predispositions.
5. Studies of the effect of owner interests, affiliations, traits, and beliefs on content.
6. Studies to ascertain the points at which communications control endangers democracy.

E. Studies of the *content* of political communications, with special emphasis on the degree to which content is balanced.

1. A study of the concept "balance" with a view to formulating an acceptable theory of its nature.

2. An empirical study of "unbalance" with special reference to techniques of distortion, slanting, and deception.
3. A study of communications coverage on particular topics, with some consideration of the relation between actual and potential coverage.
4. Comparative studies of the treatment given by selected media to important subjects of public discussion.
5. A study of the laws of libel and slander with special reference to their actual effects on newspaper content.
6. A study of the relation between "freedom of press" in the United States and the willingness of talented citizens to hold public office.
7. Studies of individual departments of newspapers and other media such as the editorial, financial, sports, etc., pages and their influence on each other.
8. Studies of the treatment of government agencies, policies, activities, and personnel.
9. Correlation studies of the relation between editorial opinion and public opinion.
10. Studies of the editorial policies of selected media.
11. Studies of "news slanting" as a basis for inoculating citizens against the practice.

No matter what aspect of a general problem receives first attention there are always a number of persistent questions which arise, such as who, what, when, where, how, and why? One need only select one of the specific topics listed above and raise these questions regarding it to realize how soon even the most minute topic expands into a research project of mammoth proportions. Nearly every topic listed may profitably be subjected to place, time, and media approaches as well as the who, what, when, etc., attack. Moreover, nearly every item listed may be used to test methodological theories—statistical, quantitative, ecological, analytical, philosophical, historical, among others. Almost every study of audiences lends itself to breakdowns in terms of human traits and personal characteristics much more extensive than the familiar age, sex, income, and residence treatment. Again, relationships are very important. Regardless of the

subject chosen for study, interest may soon focus on the relationships between it and other phenomena. Since it would extend needlessly the list of possible research topics to suggest in each case all the who, what, and why questions; to cite all the potential breakdowns; and to identify all relationships which might be investigated, the panel simply lists some of the principal points of reference mentioned by various members.

II. Analysis of the Field

Taking as its point of departure the list of specific projects presented above, a small group meeting at Princeton undertook first to discover any significant omissions and then to discuss the possibility of consolidating and ranking projects within each of the five broad areas. The informal, day-long discussions revealed general agreement on certain points, disagreement on others, and underlined certain problems probably common to any attempt to plan research in social science.

As was to be expected on the basis of previous correspondence, everyone in the group was agreed that the five broad areas into which the field of political communications research had been divided were inclusive and satisfactory. The group felt, however, that any division of the general field was certain to be somewhat artificial and arbitrary, and that the interrelationships of the five broad areas should be constantly stressed. There was also general agreement that the list of specific research topics was adequate. No list could be complete but the group believed that the list as presented was representative of worthwhile undertakings which should be encouraged.

The discussions at the Princeton meeting did reveal, however, some real differences of opinion which are significant and pertinent to the formation of a research program in the political communications field. The suggestion that the panel undertake to rank in their order of importance the various specific projects in each of the five broad areas prompted a lively discussion of the problem of differentiating important from unimportant research projects. What criteria should be used? Important for what and for whom? Some members of the group felt that it would be performing a real service by ranking the various projects in what

the individual members of the group considered the order of their importance. Others were insistent, however, that the results, indicating merely the personal opinion of the members, would have no value. The majority felt that in defining and ranking the broad areas of research they had gone as far as it was feasible to go in trying to set up priorities, and that a listing of illustrative projects in each area was sufficient.

The 100 or more persons who expressed in one way or another some interest in the work of the panel displayed several different approaches to the problem of working out a research program. There was first of all what may be labelled the comprehensive, systematic approach,—an attempt to delimit the over-all scope of the political communications field, to break it down into a comprehensive list of sectors, and then to spell out all the possible phases of each sector which might be studied. This approach characterized the philosopher and the perfectionist. It proved rewarding in many ways, but the task of establishing such an overall framework, which would be generally acceptable, often proved a formidable one. The architects of the building often never quite succeeded in finishing it so that researchers could move in and begin work.

Many members of the panel adopted what might be called the interesting question approach. Without trying to be systematic or comprehensive the member would merely pop a question or topic which he considered interesting. "How interesting it would be," he opined, "to find out how many emotionally overtoned words appeared in last Sunday's *New York Times*."

Some panel members, however, use what may be cited as the big problem approach. They perceive in our contemporary political, social, and economic life certain problems or evils demanding immediate solution or remedy. The religious leader focusses attention on Godlessness; the economist on monopoly or prices; the educator on ignorance; the Communist on the class struggle. Out of these several dominant attitudes emerges a world outlook which is often as individual as it is exclusive and sometimes fanatical. After all, what is a "big problem" goes back ultimately to one's personal philosophy. There is seldom widespread agreement as to what the "big problems" are. Yet there

is something appealing and basic about this approach in contrast, for example, to the "interesting problems" approach. It generates a high degree of singleness of purpose, and a consequent drive. It frequently offers the hope that research based on it can and will be translated into tangible results.

The discussion of the scope of the panel's program was the occasion for a consideration of the matter of research levels. One member of the panel stressed the point that, although all research was concerned with finding answers to questions, the questions themselves varied greatly in scope and philosophical import. In other words there were various levels of research endeavor which should be distinguished. There is a vast difference between the questions asked by the philosopher—what is truth, justice, the good life?—and those of a more lowly origin—what is the temperature, the price of chewing gum, the tastes of moviegoers? Should a classification of research projects be attempted on some such basis as scope, philosophical import, complexity, intelligence required to implement, or some other basis that would distinguish significant levels of endeavor? If various levels of this nature were defined, however, which would be the more important to exploit? Should political communications research be directed along the "higher level" of complex, philosophical, almost impossible to answer enquiries, or along "lower levels" of simple and often trivial types of pure fact gathering? Obviously it would be necessary to adjust the task to the mental capacities of the investigator and to his facilities for investigation. Researchers are often confronted with the dilemma; is it preferable to try to find the answers to questions on the higher levels and run the risk of failure, or restrict one's efforts to the readily answerable. Altogether too much research in the field of political communications is lamentably on the lower levels, particularly in that sector called content analysis. The industry displayed in counting words and symbols is matched by the dearth of penetrating analysis and insight.

The Princeton meeting also gave extended consideration to the aims and purposes of communications research. It was felt that the relative importance of different items in the research program depended to a large extent on the definition of purpose.

Was the purpose merely to add to the sum total of human knowledge without reference to anything more specific; merely to discover "potential building blocks to be left on the shelf of knowledge for possible further use"? Was the primary aim that of assisting young scholars to find theses topics? Or was it to assist agencies of government in their policy making; business in its profit making; or foundations in the distribution of grants? The group agreed that all these purposes might well be served. As might be expected from the professional nature of the Princeton group many members felt very strongly that the importance of a topic for research purposes depended largely upon the extent to which such research would throw light on government problems.

The members of the Princeton group were generally agreed that their deliberations might well be of value to scholars, old and young, as well as to foundations, government agencies, and private institutions. There was no disposition to focus attention exclusively on one group. Although the immediate and pressing problems of government in the communications field loomed large in the thinking of panel members, there was no disposition to neglect the cause of pure research, or the needs of business and other private groups. Although some members thought that attempts to formulate elaborate research programs for others to execute seldom met with the desired response, except perhaps at the level of undergraduate and graduate students, there was no desire to restrict the program outlined to one which the panel itself would execute. Nor was there a general conviction that panel members should try to implement, at this stage, any specific research projects. In the opinion of most members the primary task of the panel should be merely to indicate the broad areas within the general field of political communications research which were most pressingly in need of scholarly attention.

The members of the panel devoted very little time to the problem of defining precisely the meaning of the term, political communications. There was a disposition on the part of most to restrict the term to agencies of mass impression, and to communications having a political significance. No objections were raised, however, to including other forms or media of communication if

desired. Studies, for example, of the two-way flow of communications between government and citizen might well take into account the totality of communication channels and symbols involved. The starting point in the panel's thinking, however, were the great instruments of mass communication—press, radio, and motion pictures.

Panel members as previously indicated had little difficulty in defining the broad areas of political communications research, in ranking these areas according to their relative importance, and in presenting illustrative examples of the types of research that might profitably be undertaken in each area. They were reluctant, however, to go further and indicate the relative importance of these specific projects for the reasons given above. A poll of panel members generally had shown that there was very little agreement as to which of the specific projects in each general area were most important. The discussion of the problem evoked a number of suggested criteria for appraising the relative importance of research problems, usually subjective in nature. Among them were: (1) the number of persons affected by the problem; (2) the purpose served by the enquiry, whether social or special interest; (3) the skills, intellectual competence, and philosophical insight required to execute the project; (4) scope of the project; (5) immediacy of the problem; (6) extent to which it occupies a key position with reference to other problems.

In conclusion, mention should be made of still another approach to research in the field of political communications which, in the opinion of several panel members, had merit. Reference is made to what may be called the functional approach. As stated by one member, "Unless one can arrive at some clear definition and conception of the proper functions of communication agencies, some fairly precise idea of the ends to be served by these agencies, attempts to construct a significant research program would founder in a fog of confused imagination."

Few students of political communications have attempted to develop a systematic theory regarding the functions of such communications in general, or the functions of particular communications in a democracy. To be sure there are a few explicit observations on the matter, and in many studies assumptions re-

garding the functions of communication agencies are implicit. In order to focus attention on this aspect of communications research one member presented the following list of propositions to describe the functions of communication agencies:

1. Communication agencies, especially mass communication agencies, should assume full responsibility for the effects of the communications they carry.
2. Mass communication agencies should maintain a neutral position with respect to those they serve.
3. It is not the responsibility of communication agencies to decide what should be communicated, but to give the public what it wants in information, opinion, and entertainment.
4. Communication agencies should promote the unrestricted flow of communications, and the maximum competition in the spread of ideas.
5. Communication agencies should promote rather than obstruct the raising of moral standards in the community.
6. Communication agencies should promote goodwill and understanding at home and abroad.
7. At the present juncture in world history communication agencies have an obligation to further the success of the United Nations.
8. Communication agencies have a primary responsibility for keeping the people of a country such as the United States fully informed about their government and its activities.
9. One of the primary functions of mass communication agencies in the United States is to help raise the level of the people's capacity to think straight and to reach decisions on public questions rationally rather than emotionally.
10. Communication agencies should promote democracy, tolerance, fair treatment of minorities, respect for law, and the fundamental principles of the Bill of Rights.

The above statement of functions represented only the viewpoint of a few members of the panel and was not subjected to extensive consideration. Regardless of the validity of the statement, it calls attention to the importance from the point of view of communications research of defining the functions of mass

communications. Once this is done, a most important project of research becomes clear, i.e., finding out the extent to which communication agencies are performing their functions adequately, and in cases in which they are not doing a satisfactory job to find the remedy.

Chapter X

POLITICAL THEORY

Francis G. Wilson, Benjamin F. Wright,
Ernest S. Griffith, Eric Voegelin

PREFATORY NOTE

THE POLITICAL THEORY PANEL met in Washington, D.C., during November, 1943. Extensive correspondence preceded this meeting. The panel presented its findings in symposium fashion in the *American Political Science Review*, August, 1944. This symposium is reprinted here, with only minor changes.

Subsequent meetings of the panel were held in connection with annual meetings of the Association.

Participants in the panel correspondence and discussion which resulted in the chapter that follows were:

William Anderson, University of Minnesota; Charles A. Beard, New Milford, Conn.; Everett S. Brown, University of Michigan; Francis Coker, Yale University; Kenneth C. Cole, University of Washington; Thomas I. Cook, University of Washington; Guy Howard Dodge, Brown University; Peter F. Drucker, Bennington College; W. Y. Elliott, Harvard University; Carl J. Friedrich, Harvard University; R. G. Gettell, University of California; Walter F. Gouch, Johns Hopkins University; Ernest S. Griffith, Library of Congress; Waldemar Gurian, Notre Dame University; John H. Hallowell, Duke University; R. C. Hartnett, S. J., Detroit University; Earl Latham, Amherst College; Arnaud B. Leavelle, Stanford University; Max Lerner, New York City; John D. Lewis, Oberlin College; H. M. McDonald, University of Texas; Charles H. McIlwain, Harvard University; R. M. MacIver, Columbia University; Charles E. Merriam, University of Chicago; Paul A. Palmer, Kenyon College; Wilfrid Parsons, S.J., Catholic University of America; J. Roland Pennock, Swarthmore College; C. B. Robson, University of North Carolina;

Carlton C. Rodee, University of Southern California; George H. Sabine, Cornell University; Mulford Q. Sibley, University of Illinois; T. V. Smith, Syracuse University; Carl B. Swisher, Johns Hopkins University; Eric Voegelin, Louisiana State University; Rene deVisme Williamson, University of Tennessee; Benjamin F. Wright, Harvard University; Francis G. Wilson (chairman), University of Illinois; Charles R. Nixon (secretary), Cornell University.

I. The Political Theory Panel
Francis G. Wilson

Broad differences as well as agreements on essential principles characterized the membership of the political theory panel. One would expect this. Since political theory is in part a philosophical consideration of the field of politics, these differences and agreements are, we think, more explicit than in other areas of our discipline. It is, indeed, one of the duties of the political theorist to point out and analyze the principles upon which further discussion is organized. Teaching and investigation in political theory must and does concern itself with metaphysical principles embedded in the work of those who study primarily political institutions and processes. In any case, a political theorist is such in measure because of his insistence on the central character of principles in the study of political science.

But granting that the study of principles commonly used throughout political science is the primary teaching and research field of the theorist, we must recognize the deep cleavage among theorists in the area of primary ideas. Such differences concern the arguments that have gone on for centuries among the philosophically literate. But each generation of social thinkers must come to these questions with insights freshened by continuous historical and research experience. The discussions within the political theory panel make all this abundantly clear, but the panel would like also for all political scientists to share its concern.

The ultimate issue may be stated in several ways. It may be that one's work in political science proceeds from the metaphysical principles accepted by the investigator as to the nature of man and society, and of the relation of man and society to the uni-

versal order, or to God. One group of theorists contends that the great political thinkers have dealt clearly with these issues, while another group would say that metaphysics is little more than a name given to logical thought. Thus some would urge that the essential philosophical position of the student must determine in the end both the kind of investigations undertaken and the character of the results. Not only must the individual study his own framework of thought, but considerable research may be done in showing how conclusions or institutions are correlated with the metaphysical starting point. Those who favor more metaphysical concern in the social sciences would argue that much of the thinness of social science arises from the attempt to by-pass the philosophical issues latent in the examination of social questions. In other words, much of the so-called detachment of social scientists is a product of philosophical ineptitude. If it is said, for example, that political science is the study of power, can one in good and scholarly conscience avoid the issue of what power is for, and why it makes a difference whether one group or another has in fact the control of the state? It is certainly one theory of politics to say that a descriptive study of power is the full content of political science, but most political theorists accept the burden of formulating a theory of political ethics. Such a theory, in turn, provides a set of criteria for judging the exercise of power and for saying whether it is better to have one group or another directing the vast control over individuals that is within the grasp of the modern state. Yet it can be said that in defense of democracy against totalitarianism during the last twenty years there has been a constant tendency to evade the ultimately inescapable moral issues that such political systems present. One may well cite the twisting uncertainties of the discussion during this recent period of the basis upon which rights may be claimed by an individual citizen.

Another facet of this problem is the clash between those who would favor broadly a "theological" approach to politics and those who would accept the now traditional "positivistic," scientific, or liberal technique of social study. One group of theorists would say that a political theorist must be a reasonably good theologian, not only because so much of the history of

social discussion has been written in the theological framework, but also because the theological approach clarifies fundamental issues of the nature of man, society, and the universal order. These members offered varying ideas as to what constituted an essentially theological approach to political theory. Their general view was variously challenged by others who held to the traditional approaches of idealist and rationalist liberalism.

However, neither the theological nor the empirical theorist will deny the importance of the study of ethics, values, or principles in politics. Indeed, one of the major tasks of the theorist is to study political ethics, that is, to formulate and criticize values and principles. It is one thing to say that ethics and principles exist, and it is another to say that valid principles or a valid system of ethics can be attained. It appears that most of the panel will agree that valid social and political principles may be attained. Thus the panel for the most part would agree that we can get beyond affective motives in thought to the validity of thought. This proposition is obviously central in the method and purposes of the study of political theory. It is a criticism of that scientific method which rests content after the operations of thought have been described, or which attempts to invalidate an argument by showing that the proponents of an idea are either opportunistic or neurotic. Such a statement is not a criticism of clinical work in the field of politics; it is an assertion that there is more in politics than simply clinical observation.

Some theorists have stressed, in our deliberation, the place of value-free discussion in political science. There was no disposition to deny that value-free inquiry may take place, and one member insisted, for instance, that when one discusses the conditions necessary for the existence of society, i.e., Aristotle's emphasis on political stability, values are not involved. Such conditions for the existence of society are precedent to the realization of any values that may be attained in society. Here is certainly a principle to guide research, but agreement that any particular discussion is "value-free" is not likely to be reached. Much of the same type of argument concerns the issue of recurrence in historical behavior. What does one get when it is shown that in human behavior certain actions tend to recur? It hardly

needs to be pointed out that much of the Italian tradition in the study of politics from Machiavelli to the present day operates on the principle of value-free interpretations.

Some contended that the issue in research and teaching discussed by the panel could be described as a choice between the Middle Ages and the nineteenth century. This argument arises from the belief that the political interpretations of the late medieval period, notably the thirteenth century, offer the sharpest possible contrast with the nineteenth century. The thirteenth century is viewed, in this line of thought, as the culmination of a long development in Christian, or theological and ethical political ideas. In contrast, the nineteenth century was the period in which the full force of the implications of secular and scientific thought as applied to politics was felt. It was in the nineteenth century, therefore, that rationalist liberalism, which attached itself strongly to the ideas of science and progress, came to be offered as a complete alternative to the more ancient moral tradition of the West. The principle of a continuity between sound medieval ideas and modern democratic thought accepts, likewise, the proposition that, though Protestantism broke from Catholicism on many doctrinal points, there is a deep and underlying continuity in the appreciation of man as a rational and moral creature. Others challenged both the suggestion that political theorists are limited to that sort of choice and the assertion that there is so sharp a contrast between medieval and nineteenth-century approaches to political theory. Thus, more specifically, if we argue that there is a moral order in the universe, i.e., natural law, and that out of this order the natural rights of individuals emerge, we cannot but regard the nineteenth century as the rejection of the foundation of legitimate government. The democratic tradition, for example, is divided, in this view, between those who would argue that the minority must always bow before the omnipotent majority and those who would see democracy in the self-limitation of the majority and the guarantee of individual rights. The French Revolution, it is argued, has resulted in a perversion of the medieval and Anglo-American tradition of democracy. Such a discussion naturally focuses the research that political scientists may undertake on the history of

democracy in the West. The historian of constitutionalism, who combines history, law, and philosophy in his work, may well come to the conclusion that the Anglo-American insistence on a bill of rights and the protection of the minority is the only basis on which the defense of majority rule in other respects is tenable. He may likewise argue that bills of rights emerged slowly from medieval experience and theory, and that modern continental liberalism has often failed because it has rejected one-half of the democratic tradition itself. We must ask: What is legitimate political democracy?

A further division of thought related to the fundamentals of political theory research concerning the "philosophy of history." Some theorists contend that we need in America a consciousness of the meaning of our history; we need an examination of the American philosophy of history. None of the panel would deny that the individual researcher must know the principles he uses for the interpretation of history, but a difference of opinion exists as to whether it is proper to state it, for ethical purposes at least, in terms of a philosophy of history. The philosophy of history, which suggests process and inevitability in the pattern of historical events, is thus balanced against the notion that history is made freely by freely willing men. Ends and means are thus chosen voluntarily, and though the results of such free action never quite conform to the ends accepted in the beginning, such results do conform in part. These freely chosen ends and similarly chosen means give history the meaning that it has, and history is thus in large part the expression of the aspiration of men. It has been argued that a philosophy of history has usually relied on factors outside of human choice, and in the extreme even to an exclusion of any significant human choice in the course of history. If there is to be meaning in political principle, it must involve choice and results flowing from that choice. In other words, the important factor in social history is not the objective conditions of life, however forceful they may be, but the selection of ends and the selection of means for realizing ends. To some of the panel, it was clear that research in political thought has been governed to some extent by assumptions that relate either to the philosophy of history idea, or, in

contrast, to the free choice of ends and means, though often they have been confused. It is the system of inevitabilities in a researcher's mind that reveals him most clearly. Is progress inevitable? Is war unavoidable? Was our political system, or any other, always embryonic in history? Or, is progress the result of conscious and intelligent purpose? May we eliminate war by taking thought? Does democracy exist because people decided it should be?

One way of summarizing the discussion of basic issues is to say that the political tradition of the West must be subjected to close scrutiny in political theory research. The present-day interpretations of democracy, for example, grow out of differences of opinion as to the traditional roots of democratic government. Significant work has been done in recent years which throws light on the continuities of history. While it is obvious that students of political theory will applaud such work, they will likewise insist that more must be done. Whether the postwar reconstruction of Europe is involved, or the relation of Western society to the East, understanding must be based on the intellectual and institutional tradition of these areas. Both a philosophy of history and the issue of ends and means run deep into the tradition of which the student is heir. Americans, of course, must interpret the background of their own political life. They must not only trace specific American ideas and institutions since 1776, but they must put these same ideas and institutions into their pre-independence history, and they must see all of this in the context of modern political culture as a whole. There is little room for a simple-minded provincialism in the study of political theory, whether American or otherwise, and such an effort to surmount the inevitable narrowness of specialized research will necessarily involve a consideration of Western tradition, and its relation to other cultures. The panel seemed to be agreed that the future organization, for example, of democracy or constitutional government on a broad scale must rest on the recognition of well-understood traditional diversity, yet with agreement on primary political values. One might call to witness the current efforts to establish through the United Nations an international recognition of human rights.

The political theory panel agreed generally that there should be usable definitions of political terms. It was agreed that one of the functions of the theorists is to define the concepts of political science. The panel agreed that there is unfortunate confusion on the use of the common and necessary conceptions of the discipline. Some members of the panel thought that there might be, on the analogy of contemporary work in the field of law, a restatement of political science. Others thought of the possibility of a dictionary of political science, though some believed that such would not differ widely from the *Encyclopaedia of the Social Sciences*. The examination of this subject, however, led to the question of the relation of concepts and institutions. The development of modern constitutional theory, the structure of modern ideologies, and a large number of common American political conceptions might be studied in relation to institutions. What relationship is there between concepts, or interpretations of concepts, and the political arrangements or institutions which have been set up? Likewise, concepts have varied in different philosophical systems, and there is a large field for investigation in the relations of systems of thought, political concepts, and specific institutions. Institutional arrangements in relation to values obviously is another version of the end-means versus the philosophy of history problem. For the political theorist must consider principles or values, and he must view them as having arisen from institutional patterns on the one hand; and, on the other hand, he must regard institutions as directed, controlled, or developed from the force of values themselves. The crucial question is, however, whether either institutions or values arise from the free determinations of rational thought. Yet on many issues the range of useful descriptive research for the analyst of ideas is sufficiently wide. Fiscal policy and the money issue in American politics, to mention only one such question, should give political theorists ample opportunity to make a contribution to political science.

The use of concepts or ideas presents another issue which has perhaps as much to do with teaching as with research. Most of the panel would agree that we should study the ancients because they are really modern, because they have something to say to the present generation in the solution of its problems. Plato,

Aristotle, Augustine, Cicero, and scores of others must be studied because they tell us something about ourselves, because they make us conscious of the content of our own minds. In other words, because of historical continuity, or because of the essential nature of moral man or the moral universe (or even Satanic man and the Satanic phase of the universe), we must regard the great political thinkers of the past as timeless—at least in part. To show wherein these thinkers are timeless, and wherein they are not, involves the metaphysics we adopt; but it also involves research in political theory for every generation. Modern scholarship continues its labors, and we can almost see a year-by-year change in the appreciation of the past.

Certain members of the panel, however, were careful to insist that there is danger in work of this character. We may take our modern concepts and re-interpret the past simply in terms of what we think today. We may, therefore, as in some works on the Middle Ages, get a fictitious continuity of concepts and theory. We must be sure we understand the relation between concepts and institutions at the time they were developed. It is especially true, according to some, that the study of medieval ideas has been falsified by projecting modern conceptions into the past; the state of medieval times has been discussed as if it were the state of the eighteenth and nineteenth centuries.

The considerations mentioned above suggest that we need today both an integrated vision of society and a more particular examination of the lesser movements in political thought. The political theorist, so most of the panel believed, must interpret institutions and values, in both the past and the present, on both the large and the small canvas. Questions of world organization, its history or its evolution, its changing spirit and its values may command his attention. But the architectonic force of political theory must express itself in the study of the small, and the seemingly insignificant. The small dissentient group in the past may have left its imprint on the latter course of ideas, and so the contemporary group that rejects the major assumptions of, for example, industrial society may have its value for the future. That the political theorist has a duty in this respect has been repeatedly stressed, and in particular the theorist should

analyze other phases of political science for their contribution to the larger end of a comprehensive picture of value and principle in modern politics. Some would say that such an understanding of our times will not come from the method of the positivists; only a theology or a metaphysics of politics can supply it. Certain members of the panel believe that along with a much greater emphasis on religious thought there should be closer attention to the political utopia as a means of criticism and integration. The political thinker should even undertake the writing of utopias. But no theorist would say that the careful collection and organization of the facts of modern society can be eliminated. The issue arises over the place of such necessary labor in the total scheme of scholarly enterprise.

The integrated view must be related to a variety of views. Smaller or less significant movements in political thinking need, in the opinion of the panel, to be studied carefully. Many movements need monographic studies; the ideas of church groups, of labor organizations, the evolution of the ideas of long-established journals, and those who favor the agrarian or cooperative movements should be studied for their interpretation of political values and principles. In other words, there should be a frontal attack by political theorists and their graduate students on tracing the emergence of interpretations and values in American political society.

In a more specific vein, the political theory panel has concerned itself with the lack of suitable texts of great thinkers, for both teaching and research. The teaching of political theory is dependent on suitable texts in the American and European fields, to say nothing of the problem presented by Oriental and Near Eastern political thought. There is already a committee of the American Political Science Association working on the publication of usable volumes from the writings of distinguished American political thinkers. The panel decided that a small committee should be appointed to draw up a list of the most needed texts from the European field. It is to be noted particularly that changing interpretations of history have brought to light what scholars of previous generations did not do. The older writers neglected by the eighteenth and nineteenth centuries will

get their due, let us hope, from those who are students of political philosophy in the twentieth century. The changing but continuous character of social questions makes writers of the past rise and decline in their importance to contemporary life. For example, a number of the panel felt that texts from the ninth and sixteenth centuries were especially needed at the present time.

After careful study in the political theory panel, the committee[1] has brought together a report which indicates the more important and immediate tasks in the publication of European sources. A meeting of the committee was held in Washington at the time of the annual meeting of the Association in January, 1944. A report on the replies received to the committee's questionnaire was considered and actions were taken which may be summarized as follows: (1) The committee tentatively decided to confine its activities to works published before the middle of the nineteenth century. (2) The committee decided that among single treatises the most important for our purposes are: (a) a translation of Bodin, probably abridged; (b) a translation of Marsiglio in condensed or abridged form; (c) a popular edition of the Nugent translation of Montesquieu. (3) The committee also looked with favor upon the proposal to reprint Sidney's *Discourses* and Harrington's *Oceana*, but it felt these to be of distinctly secondary importance as compared to the editions previously mentioned. (4) Under the general heading of "Selections," the committee favored publication of the following: (a) a volume of selections from Bentham's works; (b) selections from medieval political theorists; (c) selections from Occam, Wyclif, Luther, Calvin, and others, appropriate to a volume on the Reformation; (d) a similar volume on the Counter-Reformation; (e) possibly a volume on the theory of absolute monarchy in the sixteenth and seventeenth centuries; (f) selections from writings on the theory of toleration.

As a result of the work of the committee, it is clear that there is marked interest in improving the available teaching materials

1. The Committee on European Texts is composed of Paul A. Palmer, Kenyon College; Father Wilfrid Parsons, Catholic University of America; Eric Voegelin, Louisiana State University; and J. Roland Pennock, Swarthmore College (chairman).

in political theory, and there is also a notable concentration of interest upon a few basic publications such as a translation of Bodin, a popular edition of Montesquieu, a volume of selections from Bentham, and a volume of selections from medieval political theorists. It is to be hoped that the Association and the Research Committee will be able to bring about the publications of these needed volumes.

Part of the work of the meeting of the political theory panel was a short joint discussion with representatives of the Special Committee on Civil Liberty of the Social Science Research Council's Committee on Government. This discussion emphasized the inadequacy of an approach limited to constitutional law; the problem of civil liberty runs back to the fundamentals of man and society. The defender of civil liberty must examine the principles on which the rights of an individual may be based, and he must defend an order in which it is valid that individuals have civil liberty. Thus civil liberty should be regarded as a special application of many of the problems so far discussed in this report. A number of specific subjects for research were, however, brought before the panel and the representatives of the Special Committee on Civil Liberty. It was suggested that political theorists might examine the conditions of society and government under which men have civil liberty; that the history of the theory of civil liberty might be examined with profit; and that the Christian and other elements in civil rights should be studied, with some attention to the differences between Protestant and Catholic thought. In addition, examination should be made of the changing content of civil liberty, the relationship of property rights and other civil rights, the effect of industrialism on civil liberty, the supremacy of the civil over the military authorities, the effect of war on civil liberty, the civil rights of members of the armed forces, and the freedom of the press under conditions of press monopoly. These suggestions must be regarded, of course, as illustrative of the general problem, and not as an exhaustive statement of needed research in the relations of political theory and civil liberty.

II. Research in American Political Theory

Benjamin F. Wright

It is not difficult to list a considerable number of subjects on which further research is needed. The simple fact is that almost none of the great subjects has been adequately dealt with, even for this generation, and the same can be said for most of the lesser ones. The difficulty lies not so much in an enumeration of worth-while subjects as in consideration of the methods of attack to be employed by the researcher, and even more in the problem (the difficulties of which can be no more than suggested in this article) of the equipment of the researcher.

Modern federalism is peculiarly the contribution of the United States. When Madison and Hamilton were defending the proposed constitution against its critics, one of the major arguments which they had to meet was the contention that the Fathers had created, not a federal system, but a national or consolidated one. So far as recorded history went, the argument of the Anti-Federalists was correct. And when Madison came, in the thirty-ninth *Federalist*, to analyze the nature of the proposed constitution, he freely admitted that the new system was not strictly federal within the traditional meaning of that term. Rather, it was a system partly national and partly federal. But from the point of view of later times it was a federal system, because the term "federal" has come to be attached to the distribution of powers between central government and states which was worked out by the Convention in the summer of 1787. Many other countries have taken this work as a model upon which to build, although none has imitated it with complete fidelity to detail. We have, then, in this country the arch-type of modern federalism, and it might be reasonable to suppose that we should find here a considerable body of literature discussing the history and theory of such a system. There is, in fact, surprisingly little literature on the theory, or even the history, of American federalism, although a good many books and articles dealing with certain of the problems growing out of the federal distribution of powers have appeared. The subject cries for further analysis, and no single study will close off the field for those students who

are interested in the theoretical problems of federal government.

Somewhat the same thing can be said about the separation of powers. True it is that the Founders acknowledged their indebtedness to Montesquieu, to Locke, to Blackstone, to Harrington, and to others who had written on this subject. But again it is apparent that the system worked out in this country bears but slight resemblance to that described by any of the preceding philosophers or jurists. There have been excellent discussions of several aspects of this general problem, e.g., the recent studies of presidential powers and the numerous writings on judicial review, but the general problem of the separation of powers is still lacking an interpreter. Nor is there any history of the theory of the separation of powers in America.

The principle of representative government was certainly not originated in 1787, nor first discussed in the *Federalist*. It is nevertheless true that there is room for a very considerable amount of further analysis of the assumptions underlying representative government as applied in this country and as debated in many constitutional conventions and in a variety of writings. The vastly increased complexity of modern government is a truism with which everyone agrees. It is not apparent that our theory of representation has been clearly analyzed with a view to the consideration of the relative functions of representatives and voters in modern society. It is ridiculous to suppose that Burke or John Stuart Mill said the last word on this subject, and it is unsound to assume that theories applicable to other countries are applicable here, at least without some modification.

The whole cluster of questions which gather about the problem of individualism offers many opportunities for further discussion, and not only such traditional topics as the relation of the individual to various political communities, or of the position of the individual in industrial society, but also the status and political relationships of the individual as a member of various non-political groups. The subject has, to be sure, been attacked in a piecemeal fashion by several scholars, but I should suppose that no one of them has thought that his own contribution was inclusive or, perhaps, even final within the limited range of his objectives. At the present day, the problem of minority rights,

and particularly of the civil rights of minorities, seems of particular importance. Several scholars are at work upon some aspects of this subject. Whether any will deal with the general over-all theoretical issues involved, I do not know.

Scholars dealing with American political thought have been inclined, like most of those about whom they were writing, to steer away from many of the relatively abstract subjects. It is easy to understand why this has been the case; the nature of their materials has inclined them away from the major terms. I venture to suggest that we have made a mistake in avoiding such conceptions as justice and such great problems as the principles of political obligation. Even though the more significant American writings rarely do more than mention them, if they do that, there are assumptions in those writings which need further analysis, and those assumptions should be discussed in terms of the classical concepts of political thought. There are great possibilities for scholars who have the patience and the capacity to probe into the vast area of the implicit in American political thought. Such discussions would, I believe, serve to throw a great deal of light upon the nature of American political thought. We have had too little basic analysis.

There are opportunities for further study dealing with the influence, or lack of influence, of various English and continental movements of thought in the United States. This has recently been illustrated by articles on Benthamism,[2] and on German idealism.[3] These were, of course, major movements in England and Germany; in this country their influence was relatively slight, and the very slightness of that impact is itself a fact which helps to illuminate the nature and development of American thought. There have been many studies of socialism in America, and especially of Marxist socialism, but all, or nearly all, of them, at least so far as my observation goes, have dealt with the economic aspects of such movements. To be sure, the impact of socialism has been greater upon economic than upon political

2. Paul A. Palmer, "Benthamism in England and America," *American Political Science Review*, Vol. 25, pp. 855-871 (Oct., 1941).

3. Thomas I. Cook and Arnaud B. Levelle, "German Idealism and American Theories of the Democratic Community," *Journal of Politics*, Vol. 5, pp. 213-236 (Aug., 1943).

thought in this country, but again there is an opportunity for a consideration of the non-influence of Marxism upon American political thinking.

And if there is opportunity for a variety of studies dealing with the impact of the European thought in America, there is at least as great an opportunity for studies of comparisons or contrasts between European and American developments. There are undoubtedly many areas in which the native doctrinal developments have been both vigorous and important, and where the possibility of comparative study of political ideas has been largely neglected. It has been a serious mistake to write about American political thought as though, after the Revolution or the adoption of the Constitution, it had a being which was not only independent of, but not even comparable with, the various developments of political theory in the remainder of the Western World. There may be similar opportunities for a study of comparative political ideas based upon the writings of the Orient, but of this possibility I cannot speak with any authority.

A committee of the American Political Science Association has been working on a plan with the University of North Carolina Press, which originally proposed the scheme, for the publication of a Library of American Political Thought. The plan is that a series of volumes should be published to cover the major American political writings. Each volume would have a special editor, and each would ordinarily be devoted to a single writer. Since few of the major political thinkers in America have written books, it is now extremely difficult for the student and teacher, and particularly for those who do not have a large library close at hand, to consult most of the writings which are of primary importance for their subject. A collection of from fifteen to thirty volumes of this kind, each of substantial size, would go far toward making a large part of the raw material of American political thought readily available. It is unnecessary to emphasize either the importance or the difficulty of selecting from the writings of most of the political thinkers. It will be equally important for the editor of each volume to write an introduction which not only will give the setting of the theorist's work, but also analyze that writing and offer an estimate of its significance.

It is to be hoped that the funds to finance this important project can soon be raised.

While I have high hopes for this project, I think that it should also be pointed out that, no matter how successful it may be, it will leave a great wealth of material unconsidered. There are many sources which cannot possibly be compressed within the covers of such volumes as these. Let me give two illustrations.

Many of those who have worked in the field of American political thought have had occasion to make some use of state constitutional convention debates. I think it unlikely that these rich mines will be worked out within the forseeable future. On most of the major issues that were discussed during the nineteenth century, and for some which are being debated in the twentieth century, they furnish source materials which are as important as they are difficult to get at. But the difficulty of working such mines, and the sheer mass of low grade ore that must be sifted through in order to find the occasional pay dirt, can be no justification for failure to make use of them.

There are many movements which have scarcely been studied at all from the point of view of political theory. Everyone is familiar with at least the general outlines of the slavery arguments, although even this material has by no means been exhausted, but relatively few have done much work in the many lesser reform movements of the same period, even though it must be evident by now that the effect of these humanitarian crusades was materially to alter the conception of democracy in the United States. There have been studies of the political ideas and agitations of labor groups in recent periods; there has been much less attention to the political thought of the labor organizations before the Civil War; and there are numerous other groups not so easily classified which have at least attempted to affect the course of political action and political thought in the United States.

I hope that I have been able to make it clear that, in my opinion at least, there are great opportunities, that the field is open to the talented. It seems to me that more needs to be said about the way in which these various research tasks are to be carried out. We still suffer from the blight of the descriptive textbook.

Too many scholars have assumed that they had fulfilled their mission when they wrote summaries of the ideas expressed by the various political thinkers and then put these summaries together in a volume which sometimes resembles a football program containing "the names and numbers of all the players." It may be assumed that accurate summary and description has its place. We do need to know something about the players in the game. But such descriptive listing is at best scarcely more than a beginning. If the study of American political thought is to have the importance either for students or scholars, or for the entire course of political thinking, in America that it deserves, we certainly cannot stop with description or with the cozy task of compilation. The charge of thinness has sometimes been brought against writings on American political theory. It is not a charge lightly to be ignored.

It is a severe, but, I am afraid, a just criticism of the writings of political scientists dealing with this subject-matter that the most stimulating and, taking it all in all, the most valuable history of American thought was written by a professor of English. Parrington's book has many defects, but instead of resting content with pointing them out, we would be much better advised to take both his limitations and his merits into account in our own work. Where, for example, he had almost no understanding of the constitutional tradition and of the history of constitutional institutions in America, where he largely disregarded economic history, where he was frequently neglectful of the many currents which went to make up the climate of opinion in the various periods with which he was dealing, we can, it is to be hoped, improve upon his lack of thoroughness. If his treatment of John Adams is misleading, his discussion of the Federal Convention both inadequate and misleading, and his few pages on the *Federalist* almost a travesty, that is not a sufficient justification for relapsing into dullness. It remains true, in my judgment, that Parrington did give an over-all picture of the character of American political thought which has vitality and meaning. He had a point of view, one that he frankly stated in his Foreword, and he viewed the entire course of American political and literary thought from that location. His point of view today may seem somewhat naïve,

but it had the tremendous merit of being an attitude which was firmly grounded in the thinking of his period. He was a child of, and a vigorous spokesman for, the Progressive movement of the early part of this century. It does not need to be stressed that this movement has been one of the most influential in American thought since the establishment of the Republic. Had he written a purely descriptive (I am afraid this is what too many students mean by "objective") book, it would have been as quickly forgotten as have the many other histories of American literature which are frequently more accurate and invariably less important.

Perhaps I might be excused for taking one additional illustration from the writings of a man who was not a political scientist. I should suppose that many would agree that America's greatest historian was Frederick J. Turner. Turner's interpretation of American history has profoundly influenced, not only the thinking of historians, of political scientists, and of economists, but also the thinking of publicists and statesmen. It is, and always was, a partial and one-sided interpretation. It is of less value today than it was a generation ago, but it still contains, and will continue to contain, elements of importance for any student of American life and thought. Had Turner been content to write monographs, or had he, like so many of his unimaginative followers, been satisfied to repeat the words of the wise men who came before him, he would have been a figure of the second magnitude. Being a man of imaginative power, as well as of great scholarly ability, he left an impression upon American thought which has been equalled by no other historical writer. I am not arguing that every student or teacher of political theory should instantaneously attempt to write essays in the manner of Turner, much less that they should proceed imitatively along the paths that he marked out. I do mean that he furnished an example of the possibilities for the interpretation of the past which few others have had the courage or the ability even to attempt to imitate.

I very much hope that what I have been saying will not seem to be a justification for the free and easy expression of prejudice,

or that I am not suggesting that scholars concerned with American political thought are relieved of the necessity of hard and intensive research. Quite to the contrary, my criticism would be that we have usually published before we had done nearly enough hard work. There has, for one thing, been an insufficient utilization of the materials commonly thought of as belonging to other disciplines, particularly economic history, constitutional history, and sociology. I know of no general rules which seem to be applicable here except that I should think it a defensible proposition that almost any subject in the general area with which I have dealt above needs to be considered in the light of materials drawn from several fields of study. We have all heard much about the breaking down of departmental lines. I wonder whether this cannot be done most fruitfully in terms of particular topics and research problems.

All subjects need to be considered in the light of their times. In the study of American political thought, the climate of opinion in which nearly any problem lives and has its being almost invariably includes the institutional background as well as the intellectual history. The fact that virtually all American political writings that have counted have proceeded out of controversy, that none of great stature has been a detached, systematic, philosophical study (Calhoun's *Disquisition* was both significant and systematic, but it was certainly not detached), makes it even more important that materials drawn from political, constitutional, and economic history be considered along with the ideas that are there being expressed.

A book which illustrates, in one fashion, the sort of writing that I have in mind is A. V. Dicey's *Law and Opinion in England During the Nineteenth Century*. That work, in large measure because of the breadth of its coverage, because it ignores ordinary professional boundaries, and because it deals with ideas in their setting, has a lasting value and a pertinence to the problems of a century with which it does not deal far in excess of most of the books written about current problems. Somewhat the same kind of treatment is found in two quite different articles published within recent years in this REVIEW. Professor F. W.

Coker's "American Traditions Concerning Liberty and Property"[4] and Professor A. T. Mason's "The Conservative World of Mr. Justice Sutherland, 1883-1910,"[5] bring to bear upon a particular problem a very considerable variety of factual and theoretical data. The result is, in each case, far more enlightening than it would have been had the author merely summarized a particular book or document. I certainly do not mean to imply that there have been no other and not less worthy examples of a method characterized by breadth of research combined with a discriminating selection of relevant materials. But this paper is not offered as a bibliography. It is a statement of possibilities and needs in the field of American political thought, and the few citations here included are merely illustrative of the rich opportunities which the subject-matter affords.

What I have been saying has also been strongly influenced by my conviction that the greatest need in this country today is a statement of objectives in terms of ideas and ideals. We lack any clear conception of what we are fighting for, or what goals we should seek to attain, even in this country, after the war.

It may be assumed that scholars who are concerned with the study and teaching of political theory cannot themselves supply all of the needed answers. It does not follow that they are justified in dodging the attempt to aid in this all-important enterprise. The work of the student of political theory must, in part, be a work of selection and interpretation. I do not mean to argue that we should substitute prejudice for objectivity. Objectivity, properly used, is necessary to scholarship; abused, it leads to sterility. The scholar must attempt to discover all of the facts that are pertinent to his problem. He should never neglect materials or data which are not in line with his own inclinations. It does not follow that he should not begin his work with an hypothesis, much less that he should have no conclusions, or that he should not feel free, indeed obliged, to express them. We have been too inclined to hide timidly behind the excuse of objectivity.

Interpretations of the history of political thought and analysis

4. *American Political Science Review*, Vol. 30, pp. 1-23 (Feb., 1936).
5. *Ibid.*, Vol. 32, pp. 443-477 (June, 1938).

of current winds of doctrine can alike make great contributions to the future course of American democracy. Here lie an opportunity and a challenge which we cannot shirk and yet fulfill our obligations as scholars. So long as research and teaching remain free, we need have no fears of the old bogeymen, propaganda and indoctrination. The only test of political truth in a free country is that stated by Mr. Justice Holmes in his great dissenting opinion in the Abrams case: "the power of the thought to get itself accepted in the competition of the market." A truly free market includes the study and the classroom. Scholars also are citizens, as are their students. It has been said that "to teach is to affirm." It would doubtless be unwise, probably even dangerous to the best interests of scholarship and of teaching, to apply the maxim literally and without qualification. It is just as harmful to the best interests of both of these to neglect the important truth which it contains.

III. Concepts and Institutions as Fields for Research

Ernest S. Griffith

Research in political theory hitherto has been largely synonymous with searches for the origin, growth, and decline in ideas, principles, and doctrines. Sometimes such research has concerned itself with individual theories; sometimes with periods. Less frequently, it has explored the basic concepts which underlie all theory. Yet it is with these concepts in their various aspects that researchers in theory must inevitably come to grips if they are to obtain precision in their findings. For example, principles to govern the relations between church and state, between business and government, between law and opinion, rest upon precise definitions of the concepts involved. The principles may indeed be found to be corollaries of such definitions. So also in the more elaborate analyses of democracy, or political processes, or the structure of the good society, or the several elements in an ideal constitution, concepts precisely used are basic.

It will be noted that the adjective used is "precise," rather that "correct." There is a reason for this. More frequently than not, it will be discovered that the creative political theorist has imparted new meanings and new insights to many, if not most,

of the admittedly major concepts of political science—meanings and insights which are frequently derivatives of changes in political institutions observed for the first time by the theorist in question. Now concepts of this sort are and can be "correct" only in relation to a given situation in time and place; or, alternatively, in their relationship to the writer's own utopia. In this latter instance, given the writer's set of absolutes or premises, the concepts may indeed be spoken of at least as subjectively correct. For example, many of the absolute concepts in Catholic political theory are obviously of this subjective nature. This statement does not preclude the possibility of their being objectively correct also, although whether they are so or not must find its demonstration in the controversial fields of epistemology and metaphysics.

Unfortunately, most political theorists have thought of their explorations as searches for the correct concepts out of which could be erected absolute principles, dogmatically stated. Therein they have largely gone astray, for they have seldom been aware of the fact that most, and perhaps all, of the concepts they use have been, as has been suggested, derivatives of the institutions of their experience—and these institutions have been products of varied circumstances at a given time and place. Not to underrate the value of research in the origin and development of a particular political theory or principle, it may be suggested that similar research in the history of the several concepts which go to make up all theories and principles is at least equally important. Concepts such as "justice," "sovereignty," "democracy," are obvious candidates for such research, and many others will suggest themselves. It scarcely need be pointed out that such an exploration should lead the scholar to considerations of those subtle changes which take place when, not merely the institutions from which the concepts are derivatives, but also the language in which the concepts are expressed, has changed. Studies of this type—best done thus far in connection with interpretations in English of the terms of ancient Greece—are obviously basic also in the understanding of the Latin medieval thinkers, to mention one group only. In the contemporary scene, I well remember a remark of Thomas Mann to the effect that the concept of

"power" carried quite a different meaning in German (both the language and the culture) than in English. In the former, it was an end; in the latter, a function.

Here and there a political theorist has recognized explicitly the obligation of his theory and his concepts to the institutions of his own time, and has claimed for this theory and these concepts only that degree of validity which comes from a correct analysis of the particular culture pattern of his experience.[6] This suggests another series of fruitful researches, some of which have already been undertaken, but more of which are obviously needed. These researches would center around the contribution to the theorist's finished product of the institutions he observed and experienced as well as of the books he read and the teachers to whom he listened. Such research would pay particular attention to *changes* in these institutions, which are then reflected in changes in the operative meaning of the concepts used to describe them. An approach of this type would lend greater precision to our understanding of even the greatest of political thinkers. If these explorations can probe more deeply and include the total cultural context within which the political institutions operate and a part of which they are, so much the better.

This is not to say that there are no constants among the concepts, that is, concepts whose meaning is or can be independent of time, place, and cultural setting. Yet it is doubtful if concepts of this sort can be found among those deemed the peculiar property of the political scientist. Down through history, our preoccupation as a profession has been with the state, and the state is an institution whose attributes of necessity change with the changing culture of which it is an integral part. If there are to be constants among the conceptual tools of the political scientist, they must be borrowed tools from the social psychologist, the sociologist, the cultural anthropologist, and (possibly) the historian and the geographer. These sister social sciences, unlike our own discipline, at least purport to deal ultimately with basic or universal characteristics of man as an individual and in his

6. This is the approach used in the author's *Modern Government in Action* (New York, 1942), which lays no claim to permanent truth. Cf. especially pp. 10, 26, 36.

social and environmental relationships. In the case of the first three disciplines, many concepts are independent of both time and place; in the case of the geographer, some concepts are independent of time; in the case of the historian, independent of place in so far as a philosophy of history is sought. The change in their meaning from thinker to thinker is an effort to approach nearer an admittedly existing absolute, whereas the concepts of a derivative science like politics in so far as they are peculiar to politics are cultural traits of the period.

It is at this point that the serious researcher in political theory is invited to use these concepts of these other social sciences in order to illuminate political phenomena—whether of the past or the present. Research armed with such tools can promote better understanding of all the great thinkers. For example, a great book remains to be written on the fashion in which theory as to the nature of man has determined the political theory of many, and perhaps all, of the greatest political theorists; and a still greater book can then be written upon how the contemporary insights and findings of social psychology and cultural anthropology sift the permanent from the ephemeral in their political theory. Hobbes, for example, can best be attacked on the basis of the incompleteness, and even the falsity, of his view as to the nature of man—but the tools for the attack are the concepts of the social psychologist, and not the exclusive, or even the primary, property of the political theorist.

Environmental factors, in giving a cast to governments and to the theorists' dealing with governments, are likewise constants among the battery of concepts which the political theorists must and should borrow, in this case from the geographer. These are naturally concepts associated with landscape—islands, the sea, valleys, mountains. They open up a series of researches which can and should have results in political theory of considerably greater validity than the para-science of *Geopolitik*, which attempts to bend them to serve a preconceived end. Some of the major concepts of the sociologist—assimilation and conflict, for example—furnish *foci* of research for the political theorist as he examines their manifestations within the operations of the state. As a matter of fact, power—the one constant among the con-

cepts which the political scientist of late has claimed as peculiarly his own—is essentially sociological in that it is an attribute of organization and human relations wherever found, and not of the state alone.

A further promising avenue for research and precise thinking has to do with the various conceptual *systems* used by political theorists—not only in the past but among our contemporaries. Confusions in classification and fuzzy thinking generally are often traceable to error at this point. It has already been suggested that most that has proved ephemeral in the writings of theorists of the past is probably attributable to the mutability of the institutions which influenced their thinking and the mutability of the larger culture pattern of which these institutions were parts. Still another, and by no means negligible, part of their unsoundness is traceable to a failure either to adopt an internally consistent conceptual system, or, alternatively, to indicate the shifts from one system to another.

A conceptual system must ultimately be derived from classification of a particular attribute of events. "One classifies on the basis of attributes. Material things, for example, are sorted by such things as color, weight, uses, form, substance. Our problem in social science is accordingly to discover attributes of our own peculiar phenomena—attributes which are at one and the same time distinguishable and significant. If we reduce the phenomena of social science to its integers or simplest forms, these phenomena may be spoken of as 'events'—that is to say, situations or changes of any sort in the relationships between human beings.

"Just as with material objects, each event in the field of human relations may be thought of as likewise made up of a number of attributes. [The word 'attribute' as used includes 'aspect.'] A consistent basis of classification of events must, accordingly, confine itself to different manifestations of a single attribute or type of attributes, which attribute occurs in all the phenomena thus classified. [In certain instances, the presence or absence of a specific attribute may furnish a basis.]"[7]

In history, for example, it is the time attribute of events, in

7. Ernest S. Griffith, "Classification of Social Science Phenomena," *Journal of Social Philosophy*, Vol. 6, p. 231 (Apr., 1941).

social psychology, the inborn drive, that furnishes the distinctive conceptual systems of these disciplines. At least six such attributes can be discovered which create as many distinct conceptual systems in political theory.[8] These systems arise from considering (1) events pertaining to the state as an institution (the "orthodox school"); (2) events exhibiting the power relationship (Merriam); (3) events which are derivatives of an inborn social instinct or drive—a conceptual system especially favored by the Greeks; (4) those portions of events traceable to an alleged inborn desire for dominance; (5) "To many of the idealists among political theorists, the 'state' is defined in terms such as 'the operative criticism of all institutions' (Bosanquet), while law is abstractly thought of as the incorporation of the community values. Such approaches to political science (or theory) derive from the frame of reference of 'facts and meanings' ";[9] (6) events classified so as to isolate the social processes of securing adherence to norms and administering groups (Leon Marshall). Doubtless there are other systems which will yield to research, but at least the foregoing are already identifiable as constituting conceptual systems which the political scientist may claim as his own. The fact that so many differing orbits of our subject-matter are implicit in these different systems accounts in large measure for the confusion in defining, not only the scope of our discipline, but even its core. Number 6, for example, which is unadulterated sociology as well as illuminating political science, carries the researcher far afield to organized activity wherever found.

Along quite a different line there would seem to be a happy hunting ground for the researcher in exploring the converse relationship between philosophies and institutions. Usually research in political theory seeks to trace the origin of theories in the experience of observation of institutions by their exponents. Far less research has been done to discover the normal constitutional expressions of the great schools of thought—liberalism, socialism, communism, fascism, Catholicism. In connection with this area of potential research, attention should be called to a note-

8. *Ibid.*, p. 241 f.
9. *Ibid.*, p. 242.

worthy similar study by Fritz Karl Mann in which the fiscal systems expressive of these various philosophies were made the subject of investigation.[10]

Closely related to this area of research would be inquiries designed to ascertain how far the breakdown of a constitutional system arises from a gulf between the values behind it and the technology within which the institutional expression of these values must operate. The cultural compatibility of various political institutions and devices with the prevailing philosophies or value systems of the people living under them offers an alternative approach.

Finally, there is urgent need for inquiry into the applicability or appropriateness of *any* of the commonly used conceptual systems in analyzing contemporary political phenomena. If, as has been suggested, the concepts of political science under most conceptual systems are definitely derivatives of institutions in themselves ephemeral and are not constants, then during or following major changes in these institutions we should be on the alert for new or revised groups of interrelated concepts, or even a new conceptual system altogether. Many, if not most, of the concepts with which we are familiar, even at the present day, were in fact merely frequently observed relationships or processes in the state as an institution in former times and under differing circumstances. Such concepts are separation of powers, legislature, executive, judiciary, federalism—to mention a few of the most obvious. If we direct our attention especially to the most frequently used trilogy—legislature, executive, judiciary—we note changes so profound as to render not only their present meaning, but also even their usefulness, suspect. If perchance this trilogy should be a product only of a liberalistic culture, and associated with or appropriate only to such a culture, then the manifestations of organization, administration, and power in all sectors of contemporary society, and especially in government, ought to find as their by-product new ways of talking about government, that is, new concepts in political theory. In another connection, the author has explored this hypothesis more fully, and has suggested that the new political relationships or processes

10. Mann, *Steuerpolitische Ideale* (Jena, 1937).

which are basic to the emerging governance of a technological and organizational age are at least six: value adoption, leadership, popular influence and control, planning, adjustment, and research.[11] In some such pattern lies the true architecture of political theory of the immediate future.

May we summarize? Research in political theory can and should be directed to many ends. Chiefly it has concerned itself with individual theorists, with schools of thought, and with periods. Without at all underrating the need for further research of these types, a plea is in order for more research at another, and probably more fundamental, level—that concerned with the concepts basic to all political thought. Such research will do much to render more luminous the meanings of even the greatest of the political theorists. A contribution to our science surely fully as great will then be the quests suggested in this article—the quests for the origin, development, changing meanings, and contemporary suitability of these concepts themselves.

IV. Political Theory and the Pattern of General History

Eric Voegelin

I

In speaking of the general history of political ideas, we have in mind the field that is represented with distinction by such treatises as that of Dunning, or McIlwain, or Sabine, or Cook. The problems of this field and their further development are of specific importance for American scholars. For, while the monographic literature on the various phases of this history is international in scope, the general history as such is almost an American monopoly from its beginnings. When Dunning published the first volume of his *History of Political Theory* in 1901, Janet's *Histoire de la science politique dans ses rapports avec la morale* was the only noteworthy competitor. Janet's *Histoire*, however, as Dunning rightly remarked, stresses primarily the development of ethical doctrine; and that was precisely the field which Dunning wished to avoid in order to isolate clearly the development of political, as distinguished from ethical, theory. A science,

11. E. S. Griffith, *Modern Government in Action* (New York, 1942).

however, that may be said to have begun with Dunning's work is a young science; and, as happens with young sciences, a field is staked out, while the details are far from being settled. They are even less settled today than at the time of Dunning, because the last two generations have witnessed a prodigious enlargement of our knowledge of historical materials and, at the same time, there has occurred a serious revision of our views concerning the structure of history. It will be advisable, therefore, to begin with a few remarks on the ways in which the development of historical science has affected the more special problems of a general history of political ideas.

The historians of political ideas have followed, on the whole, the "straight-line" pattern of history, according to which the history of mankind moves in continuity through the ancient, modern, and medieval phases. The idea that human history moves along a straight line is by origin a theological conception, deriving its strength from the Christian belief that mankind moves through a sequence of meaningful phases according to a providential plan of salvation. The pattern was established by the early Christian philosophy of history, in the period from the *Letters* of St. Paul to the *Civitas Dei.* Its empirical usefulness in the post-medieval period was largely dependent on the chance that the medieval spiritual and intellectual contraction of the historical horizon to the Western World would be continued; the belief in the straight line could be maintained as long as the independent parallel histories of non-Western mankind were simply overlooked and the pre-classic civilizations were practically unknown.

By naming the conditions for the maintenance of the linear pattern, we have indicated the sources of its disturbance. The disturbances are connected with the successive breaches made in the closed medieval horizon. The first, and hitherto most important, irruption of new materials—that of classic antiquity—was digested with comparative ease. The straight-line pattern was simply shifted from sacred history in the Augustinian sense to a new profane history. In the practice of writing history, this meant that the Israelitic history as the Western pre-history was relegated to a second plane and replaced by Hellenic his-

tory, and that, furthermore, the Middle Ages slipped into the category of the "Dark Ages." But the ease was comparative only. It is forgotten today that not all humanists of the fifteenth and sixteenth centuries swallowed without resistance Graeco-Roman antiquity as the linear pre-history of the Renaissance. The contemporary Near Eastern events were sufficiently impressive to suggests a construction of profane history on the pattern of parallel streams. We have letters of Poggio in which he shows himself sick and tired of the glory that was Greece and Rome and ranks the military and political achievements of Tamerlane higher than those of Caesar; and Louis LeRoy understood the conquests of Tamerlane as the decisive event which opened the new period of the Renaissance politically and civilizationally. The impression of Asiatic politics, as the model of what politics might be on a grand scale, was considerable at the time; but this tendency to give meaning to the structure of Western history by orienting it toward the parallel Asiatic events was superseded by the shift of the center of politics to the Atlantic, with the discovery of the sea-routes and of America. The linear pattern remained afterwards fairly intact to the time of Hegel.

The generation after Hegel had to grapple again with the problem of parallel, non-Western developments. The first decisive document of the new situation is Bruno Bauer's treatise on *Russland und das Germanentum*, in 1853; the rise of Russia began to influence our picture of European history as one of several parallel streams. The revisionary movement was accelerated by the gradually increasing knowledge of the Near Eastern preclassic and of the Far Eastern civilizations. The integration of the new knowledge in a work that gained wide popularity, in Spengler's *Decline of the West*, did not meet with complete approval, because Spengler, setting aside the dilettantisms in detail, was so eager to demonstrate the plurality of civilizational histories that he overreached the mark and neglected the fact that some of these civilizations are not isolated from each other, but are related by the transmission of a considerable civilizational heritage. The linear pattern had to be qualified by insight into the internal cyclical structure of civilizational histories, but it still was empirically applicable to a stream of meaning running

in continuity through Graeco-Roman antiquity and the Middle Ages into the modern West; and that stream of meaning was revealed as originating even farther back in the pre-classic Near Eastern civilizations. The revised pattern of history is today used in Toynbee's *Study of History*, of which six volumes have been published.

In what manner do these changes in the pattern of political history affect the history of political ideas? The answer will depend on our definition of the political ideas of which we intend to write a history, and of their relation to the political environment in which they grow. A first answer to these questions could be the assumption that the history of political ideas does not show an internal structure of meaning at all, and that, as a consequence, the historian can do nothing but record ideas concerning political problems in their chronological order. The result would not be a history, if by history we understand the unfolding of a pattern of meaning in time, but a chronological encyclopedia. This assumption has hardly ever been made on principle, but in practice we sometimes find an approximation to it when a historian's desire to be complete becomes stronger than his power to organize a structure of meaning.

Of greater practical importance has been the assumption that only highly integrated systems of thought, like the Platonic, the Aristotelian, the Thomistic, are of real historical significance because only in instances of this type do political thinkers approach a treatment of their topic that can be called scientific. We can make the degree of scientific achievement the standard by which to measure the relevance of a system of thought. If we make this assumption, the pattern of political history would have little bearing on the pattern of a history of political ideas. A tendency in this direction is discernible in Janet's *Histoire*. It originated in a study comparing the moral and political philosophy of Plato and Aristotle with that of modern publicists. The result was the conception of a "true" system of political thought, holding the middle between Platonic moral absolutism and Machiavellian amoral, political technicism. The true middle is represented by the ideas of the Declaration of the Rights of Man and the Citizen, which considers it the function of the

state to provide the framework in which man can develop his moral destiny as a free agent. The history of politics since 1500 shows progress in the desired direction. A consistent application of this principle would have required the elimination of all materials which cannot be integrated in a line of progress toward the ideal aim. But such consistency is not to be found in Janet any more than in other historians who make similar assumptions of an absolute standard of political thought. Janet plods conscientiously through history from ancient times to the French Revolution, giving thereby to his work rather the touch of the chronological encyclopedia. The encyclopedic character is particularly marked because the part on classic antiquity, which opens the treatise proper, is preceded by a "preliminary chapter" on China and India, for no other good reason than that the Chinese and Hindus exist; an integration of the Far Eastern body of thought into a general pattern of history is not attempted.

The work of Dunning marks a tremendous advance of methodological consciousness over Janet. Dunning uses the term "political theory" in order to distinguish his principle of selection from Janet's "political science." "Political theory" is every scrap of idea, whether integrated into a scientific system or not, which tends to explain the origin, nature, and scope of the authority of rulership. This wide definition could cover the ideas concerning authority among primitive tribes as well as those produced in the vast stretches of history between the great systematic thinkers. Dunning restricts, however, this liberal definition by confining the meaning of "political theory" more narrowly to those theories which presuppose the idea of a "state," as distinguished from the family and the clan. By this restriction he is enabled to eliminate from the field the ideas of primitives, while retaining all non-systematized theories which deal with the phenomenon of political authority, as for instance, mentioned specifically, the theories of Burke and American political theory. The substitution of "theory" for "science" has, moreover, the advantage of breaking with the prejudice "that formal political science is more a cause than a result of objective political history." A theory is important, not because of the scientific insight which it embodies, but because it is in touch with "the

current of institutional development." Hence, the historian of theory will have to depart on occasion entirely from the literary expressions of theory and to interpret the theoretical content of institutions themselves if no other source is available. The history of theory thus is subordinated for its pattern to the structure of political history—with an exception, however, which we have to discuss presently. The pattern of history will be decisive, therefore, for the historians who follow the principles inaugurated by Dunning.

The weak point of Dunning's *History* is the Middle Ages. The weakness is due to Dunning's conception of "progress" in political history. By progress in politics, he understands the differentiation of an autonomous sphere of politics, and the disentanglement of political conceptions proper from ethical, theological, legal, and other contexts. Such differentiation was achieved by the Greeks (in Dunning's opinion); and it was again achieved in modern history. Political theory is largely an account of this progress; when such progress ceases, the history of political theory ceases. On these grounds, Dunning can eliminate Far Eastern theory from the field, which does little damage to the project of a history of Western political theory because the connections, if any, are thin; but he also eliminates the Near Eastern pre-classic history, which does considerable damage, because a good deal of Western political thought is deeply rooted in the Mesopotamian, Persian, and Israelitic pre-history. The worst, however, was that he eliminated the Middle Ages. His assumption compelled him to declare: "The Middle Age was unpolitic." Medieval theory is political only in so far as it is concerned with the separation of Church and State. "Medieval political philosophy is in fact exhausted when it has propounded a theory as to the relation of secular to ecclesiastical authority." This sweeping statement that the Middle Age was devoid of political history, except in so far as the State was separated from the Church, was hardly tenable in 1901, when it was made; it certainly is not acceptable today. The definition of politics has to be revised in such a manner that we can deal adequately, not only with the later phases of civilizations which show the differentiation of spheres, considered progressive by Dunning, but also with the

equally important early phase of a civilizational cycle, in which the temporal power, as in the Middle Ages, is considered one order in the embracing mystical body of Western Christian mankind. The elimination as irrelevant of a phase of history which is in direct and broad continuity with our own, because its structure of political ideas differs from ours, cannot be justified by any standards of scientific method.

The weakness of Dunning's *History* has been corrected, on principle, by Professor Sabine's *History of Political Theory*, published in 1937. Professor Sabine adopts Dunning's principle that political theory is a function of politics and that, therefore, the pattern of a history of theory has to follow the pattern of political history. He does not adopt, however, the principle of "progress." "The substitution of the belief that there is a determinate order of evolution or historical progress for the belief in rational self-evidence displaced an unverifiable idea with one still less verifiable." Neither the conception of an ethically right order, like Janet's, is admissible as a standard of selection from this position, nor an arbitrarily restrictive conception of politics like Dunning's. The historian has to follow with impartial loyalty the structure of theory as it reveals itself in history, whether it reflects the problems of a differentiated sphere of politics, or whether it reflects an undifferentiated complex of community order including "morals, economics, government, religion, and law." Applying these principles to the historical materials, Professor Sabine has organized them into three great parts: the Theory of the City-State; the Theory of the Universal Community (from Alexander to the end of the Middle Ages); and the Theory of the National State. With the elaboration of this methodological position, the problem of principles has come to a rest. The structure of a history of political theory is unconditionally subordinated to the structure of political history. From the acceptance of this principle follow the problems which today are the main concern of the historian of political theory: he has to be clear, first of all, about the pattern of history which he wishes to adopt as the basis for the organization of his materials (Toynbee's pattern, or perhaps another one); and, secondly, he is faced by the never-ending concrete task of classi-

fying and adequately integrating the richly flowing stream of new materials.

II

If we accept the principle elaborated in the preceding section, a number of problems will arise from the necessity of harmonizing the history of theory with political history. The following remarks will suggest some points at which such harmony is not yet achieved. The pattern of history presupposed in these suggestions is principally determined by Toynbee's *Study of History* and by the studies contained in the *Cambridge Ancient* and *Cambridge Medieval Histories*. The reader should be warned again, however, not to mistake these modest, casual remarks for a presumptuous list of desiderata for a general history of theory; they simply draw attention to the more or less obvious fact that we possess on the one hand a wealth of monographic studies on special phases of political theory, that we possess on the other hand a knowledge of political history far surpassing the knowledge of a generation ago, and that the historian of political theory, as a consequence, has the fascinating opportunity of trying his hand at bringing the two complexes of knowledge together. This is hardly a feat to be achieved by one man at one stroke; the cooperative efforts of a great number of scholars will be necessary to produce an adequate solution. But we can at least approach the task by pointing to some problems which are typical of the wide field that is opening. The following enumeration of such problems, in chronological order, should not be taken for anything but a list of examples.

(1) Our knowledge of the Near Eastern pre-classic civilizations and of the Hellenistic period is now sufficiently advanced to make it clear that a history of theory can no longer legitimately begin with Hellenic theory. It is duly recognized that the theory of the *polis* ends with Aristotle, and that with Alexander and the Stoa a new type of theory makes its appearance. But there is still some hesitation to recognize the continuity of the imperial Hellenistic period with non-Hellenic Near Eastern imperial history. Such recognition would entail a resolute break with the linear pattern of history and the con-

struction of the Hellenistic period as an amalgamation of the Near Eastern and Hellenic streams of history. The Mesopotamian, Persian, and Egyptian theory would have to be accepted as a body of thought on an equal footing with the Hellenic, and it would have to be treated with equal thoroughness. The break with the linear pattern of history, however, is not the only reason that would cause some hesitation. The recognition of ancient Near Eastern history would also require a break with the widely accepted conception of political theory as a theory concerned with the explanation of governmental authority. The problems of governmental authority dominate the scene only in those phases of civilization in which the political communities themselves are established and taken for granted. In the initial phases of civilizational cycles, the problems of community-substance, of its creation, its delimitation, and its articulation, are of equal importance with the problems of source and scope of governmental authority; and the same is true for the periods of political crisis, as for instance the present, when the problems of spiritual disintegration and regeneration, and of the community-creating political myth, come to the fore. The adaptation of the history of political theory to the process of politics would require a well elaborated theory of the ideas concerned with the mythical creation of communities, and of the far-reaching theological ramifications of these ideas. The task is formidable, but not hopeless. We are well equipped today with easily accessible sources, like the translations of Babylonian, Assyrian, and Egyptian sources, published by the Oriental Institute of the University of Chicago; and we possess a wealth of monographic literature, within which should be mentioned, as of particular usefulness to the historian of theory, the great study on the Ancient Orient by Alfred Jeremias.

(2) The formulation of a concept of political theory that will permit us to subsume the phenomena of rising communities, as well as those of the established ones, is perhaps the most important general task. It is indispensable for a more realistic treatment even of systems of thought which seem to be well understood by now, like the late Hellenic. Again, the problem as such is fully recognized. We know that Plato marks, not the beginning,

but the end, of Hellenic theory; and we know that his political philosophy is a theory not of the *polis*, but of the lethal crisis of the *polis*. Nevertheless, the understanding of Plato's philosophy as an attempt at a spiritual reform of Hellas and as an attempt to create a new community-substance leaves much to be desired, because the theoretical apparatus that would be necessary for a thoroughgoing analysis of this question is insufficiently developed. Help can be found again in the monographic literature, particularly in the works of Friedemann and Hildebrandt which accentuate this aspect of Platonic theory.

(3) The treatment of the Roman imperial period will have to undergo a complete revision. The later volumes of the *Cambridge Ancient History*, published in the thirties, have for the first time put at the disposition of the scholar who is not a specialist in Roman history a digest of the otherwise almost inaccessible materials for a history of Roman imperial theory. In addition, there should be mentioned the great monograph on the principate by von Premerstein, which gives new insights into the sacramental coherence of the early Empire through the oath to the *princeps*.

(4) The appearance of Christianity raises again the question of the conceptual apparatus for its treatment. The cautious evasion of religious problems and of the creation of the mystical body of Christ is untenable. If we dodge the question of the pneuma of Christ and of its function as the substance of the Christian community, nothing is left of Christianity but the reception of Stoic ethical and legal theory, a few remarks concerning the recognition of temporal authority, and the hierarchy of functions. The substance has disappeared. As a consequence, the struggle between Christianity and the counter-religion of paganism in the late Roman Empire becomes quite as unintelligible as the community problem of the Middle Ages. It will not do to eliminate from the field of political theory the theory of the community within which the structural political problems arise by classifying it as religious. Precisely the so-called non-political ideas, as for instance the eschatological sentiments and ideas, are the great source of political fermentation and revolution throughout Western history to this day.

(5) Great difficulties have to be overcome in the treatment of the Middle Ages. Let us first isolate one of the more manageable problems, that of the migration period. Dunning was still of the opinion that we did not know anything about the political theory of the Teutonic tribes which were the active nucleus in the formation of the Western Empire and of the later national states. The assertion was hardly true in Dunning's own time; it is still less true today. We have the histories of the migration period (Jordanis, Isidorus, Paulus Diaconus, etc.) and a wealth of other sources, as well as a rich modern monographic literature. As a result, we can draw today a solid picture of the Teutonic political ideas which have entered as an integral part into the conception of Western kingship. The gap which mars most histories of medieval political theory, as for instance that of Carlyle, can be closed.

(6) The great problem, however, is a satisfactory organization of medieval materials beginning with the ninth century. We have today two major treatises on the subject, the *History of Mediaeval Political Theory in the West* by the Carlyles and the *Sacrum Imperium* by Dempf. The two works complement each other admirably; and thanks to them we can see today at least the outlines of a problem and its solution that were rather dark even twenty years ago. The *History* of the Carlyles is an indispensable, encyclopedic mine of information on a vast body of materials; but it is incomplete in so far as the principles on which it is built did not permit the authors to include the body of literature which concerns the vicissitudes of the medieval community-idea. The most serious gap is probably the omission of the Joachitic and Franciscan spiritual literature, which marks the beginning of the ideas of a Third Realm and of possible new mystical bodies replacing the mystical body of Christ. Dempf's *Sacrum Imperium* stresses precisely these aspects of medieval problems, but it is incomplete as a medieval history because it concentrates on the fate of the *sacrum imperium* and, therefore, cannot give sufficient attention to the growth of the institutions and ideas which resulted in the formation of the national states. Both treatises fail to include the body of literature connected with the sectarian movements. These movements constitute one of the im-

portant "parallel" streams of history; it merges with the main Western stream in the Reformation and gives to post-medieval politics one of those supposedly "modern" touches due to the elevation to the main level of civilizational development of political habits and thoughts which in the Middle Ages remained sub-institutional. For these gaps we possess, however, a rich monographic literature, amidst which should be mentioned two great American contributions: the studies on medieval institutions by Professor McIlwain and the studies on mystical religion by Rufus M. Jones.

This enumeration of examples has not touched upon the great complexes of the Byzantine, Islamic, and Jewish medieval "parallel" histories and their contacts with Western history; nor has it touched upon the problems of harmonization between the histories of theory and of politics which arise for the modern period. But it is hoped that the list has brought out the methodological principle which must guide us in the formulation of our task. The field for research is wide open; there is no lack of problems, only a lack of strength to deal with them all at once.

PART II

SCOPE AND METHODS

Chapter XI

THE METHODS AND PROBLEMS OF RESEARCH

Ernest S. Griffith

POLITICAL SCIENCE must ultimately stand or fall on the soundness and comprehensiveness of its methodological base.

It was not the primary purpose of the panels to analyze this methodological base, but both the discussions and the reports inevitably gave considerable attention to the organization, materials, and methods of research. Implicit also were certain conclusions with regard to the training of researchers. Though no concerted directive was given, it is remarkable to note certain common threads of inclusion and omission in these matters.

I. ORGANIZATION

The reluctance to leave research to the whim of the individual scholar led a number of the panels to recommend some permanent organization in their respective fields, in part designed to implement the panel's program of research priorities. These recommendations began at the level of an exceedingly modest secretariat, perhaps only the half time of a single person. The secretariat's functions would be to keep those interested in the field meeting periodically (probably at the annual meetings of the Association), to discover and record research in progress and inform the interested parties, and to serve as liaison with the government in ascertaining the latter's needs and in opening its facilities to researchers. More ambitious were the recommendations for "institutes" which, while performing all the foregoing functions, would at the same time themselves subsidize or perform research. Two pending developments give a measure of reality to these hopes. One is the emergence of UNESCO, which—for example, in the field of comparative government—might eventually sponsor such an institute on an international

scale. In the second place, the United States Government itself seems certain sooner or later (and sooner rather than later) to extend the existing practice of contracting for research in the natural sciences to the field of the social sciences as well, with international relations, military government, and political communications among the fields most likely so to be aided. Already such institutes have appeared at certain of our universities. This university affiliation seems rather more likely to be followed than the creation of independently organized centers—in spite of the fact that the latter find it somewhat easier to obtain the cooperation of other centers of research (especially in universities) than does an institute attached to a single university. Conceivably the American Political Science Association might make a modest beginning by itself having a full time paid secretariat, one of whose functions might be the stimulation of research. In any event it is hoped that the momentum of the panels will not be without ultimate influence in these regards.

In one of its earlier reports, the Research Committee called attention to the growing importance of group research.[1] This figured also in the discussions of a number of the panels. While we all have paid and shall continue to pay tribute to the achievements of an individual[2] working on his own, there still remains in many quarters a stubborn scepticism as to the value of collective effort. So, without at all minimizing the importance of the work of the isolated scholar or failing to note the extent to which such a scholar is today assisted materially by advice or other aid from his colleagues, most panels held that something more was obviously needed. The reasons cannot be too often stated.

In the first place, the tempo of social change has been enormously accelerated. One can no longer take a "time exposure" of many, if indeed of any, of the contemporary political problems. To "capture and record" the workings of military government in Germany is utterly beyond the capacity of the individual scholar; it requires group effort of the first order. Luckily the writing of the official history of the war, including the home and industrial front, has been a group research project. A by-product

1. *American Political Science Review*, Feb. 1945, pp. 154f, 157f.
2. *Ibid.*, pp. 151-153.

of this has been the training that scores of social scientists have now had in such collaborative scholarship.

The fact that textbooks in comparative and American government are steadily being forced toward annual editions, or at least annual supplements, is further evidence of the aforesaid tempo. Take as an example of the increasing weakness of isolated research the problem faced by a hypothetical person who decides to prepare a treatise on political parties on the Continent of Europe. If such a study is to be fundamental, he must first master the social and economic structure of these various nations. But by the time he has finished with this preliminary task, the boundaries have changed, populations have migrated, and a major economic revolution may have taken place. Yet such a treatise is of very great importance; equally for political science in general and for our foreign policy. As a practical matter, only group research can do the job.

Of a similar nature are problems in local and state government which require simultaneous and coordinated study in a number of different localities for maximum usefulness. Moreover, it was the belief of a number of panel members that of at least equal importance with the aforementioned factors of space, time, and magnitude, were the creative possibilities attendant upon group collaboration. The inter-stimulation and mutual disciplining of ideas that accompanied the deliberations of a panel would be multiplied many times in the continuous partnership of a number of scholars studying a common problem. Examples exist already in volumes such as *The Absolute Weapon*, a Yale Institute Study, and several of the products of the Brookings Institution—notably *America's Capacity to Produce*. When the full story of the Office of Strategic Services is written, there will be similar examples revealed; for example, the planning of the North Africa campaign.

One final value of group research should be noted, that of integrating the scholarship and points of view of representatives of other social sciences with our own. Group research of this type will inevitably emerge from the various centers of "area studies" springing up in both universities and government; but problems other than those associated with a given area lend them-

selves also to this type of group research. Notable opportunities exist for collaboration with the psychologists, cultural anthropologists, and sociologists in the field of political communications. The possibilities of developing a democratic corporate state require the joint exploration of the political scientist and the economist as a bare minimum. These and other examples indicate that a new dimension must be added to research.

For better or worse the government seems destined to play an increasingly important role in social science research. The National Science Foundation seems certain to be an accomplished fact in the near future, and the door for the inclusion of the social sciences has been left open. Subsidies and contracts from this and other governmental sources seem likely to center in fields such as public finance, public administration, comparative government, and international relations at first; but as confidence is gained and as needs show themselves, the orbit of governmental interest will inevitably widen.

Whether government should or could guide research apart from a subsidy program and apart from its own self-administered projects is more problematical. For all the thought, effort, and advice that have gone into attempts to convert government needs and requests into private research, the results have been meagre indeed. Government as such is not likely to be articulate as to long-range objectives, and its most keenly felt needs are apt to be short-range and urgent and not likely to await the more leisurely pace of university research. The average agency prefers to do the job itself whenever possible, rather than to farm it out. Theoretically, the possibility exists of government at no expense to itself commanding the service of a number of our scholars, who ask only that their research be significant and actually used. This possibility awaits a formula not yet found for its translation into reality. It obviously involves a cooperative attitude on the part of the institution with which the scholar is connected.

There are dangers as well as advantages in all this governmental activity. The world has witnessed the bending of scholarship to the national will too much to greet lightly the entry even of a democratic government such as our own into a dominant position in research in the social sciences. This is especially true if govern-

ment enjoins secrecy. It is therefore all the more important that private universities and foundations shall remain free, adequately financed, and alert to explore, not only the areas for which government research is inherently unsuited, but also or even more those areas in which the subtle force of public policy leads, however unconsciously, to distortion in selection of materials as well as of findings. A special role may be played by the state universities in the field of research into various aspects of our federal system, through their decentralized insights; for an independent check is surely needed to the research that pours out of so many of our national government bureaus—the tenor of the findings of which so often is the transfer of more and more functions to the nation. Here is the peculiar role of the political scientist in a state university—not of course to slant his findings in the direction of the states but to be certain that the whole picture appears. His task is likewise steadfastly to explore ways and means of fostering what remains of state and local vitality.

Inevitably government in the future will devote vast sums to subsidizing and itself carrying on research. We must welcome this development with all the possibilities it holds; but those of us who are associated with government in such a capacity have a peculiar responsibility to remain loyal to the higher loyalty, which is truth, the science of our profession.

II. Materials

In the course of the panel discussions, enough attention was given to difficulties attendant upon research materials to warrant at least brief mention at this point of the principal obstacles and gaps in this regard.

In the first place, the enormous quantity of materials—even if a person confines himself to the English language—is nothing short of staggering. The only possible answer seems to be in the shape of bibliographical controls more adequate than those now in existence. One suggestion, frequently heard, is that the Library of Congress issue periodic, annotated, and classified bibliographies of its accessions. To be really valuable, these should include worthwhile articles in periodicals and pamphlets and documents as well as books. Some such project could be

undertaken relatively inexpensively, if done for the social sciences as a whole—and might conceivably be financed by the National Science Foundation or as the United States Government's contribution (matching that of other governments) under some cultural agreement concluded under UNESCO auspices. There seems little likelihood that the old *Social Science Abstracts* could be revived on any self-supporting basis; yet such a project as the one suggested might well have sufficient value to government itself to warrant financing with public funds. Favorable opinion must first crystallize.

Barriers of language and text exist. The classic graduate school requirement of French and German has resulted in acute bottlenecks of research requiring Russian, Chinese, Japanese—to mention only the most serious needs. The sentiment was occasionally expressed that the value of a foreign language to the scholar was now much more in the opening up of research materials than it was in enabling him to read standard "texts." If this be true, the social usefulness to the profession as a whole of diversifying the language accomplishments among our scholars is obvious.

Probably a corollary to this is the translation of more of the significant foreign works into English. The time saved would be very great; and, as the number of political scientists multiplies, this becomes more practicable as a commercial venture. Similar need was expressed for new (or first) editions in English of some of the great political classics.

Security factors allegedly stand in the way of access to the richest material collected or created during the war and the period of military occupation. Our finest scholars in the fields of comparative government, international relations, military government, and war strategy find themselves more or less completely frustrated by the delays and barriers set by the Army, Navy, and State Departments in declassification. Yet it is these very scholars whose studies would be most valuable to our nation in the years immediately ahead.

Finally, we face the iron curtain in that part of the world which we most need to understand. Censorship is bad enough, but complete exclusion is fatal to genuine understanding. Here

we can only hope for a happier day, when once again scholarship can be truly international.

III. Methods

Inevitably the panels gave attention to method, though the Research Committee gave them no directive in this regard. Occasionally the attention was casual or perfunctory, perhaps regarding methodology as belonging to the realm of the "taken for granted." In such instances a panel contented itself with a listing of all the methods, traditional and otherwise, in the battery of the political scientist, benevolently bestowing upon each the epithet "important."

Other panels were able to take the time to make a more discriminating evaluation, exhibiting insights the cogency of which cannot be dismissed lightly. In recording and evaluating these insights, it would be hazardous in most instances to claim any formal agreement upon them on the part of panel members generally. Not enough time or thought was devoted to their consideration to warrant such an assumption. Consequently one is forced to a very considerable extent to speculate as to a probable collective verdict. Nevertheless it is worth making the attempt.

By a probably unwarranted over-simplification, one may characterize at least four methods of political science as being traditional in the sense that they were the ones most frequently, perhaps even exclusively, used until the last two or three decades. These are the historical, the legal, the descriptive, and the comparative. It is appropriate to consider each of these in turn.

The complete omission from the discussions of any real emphasis upon the historical approach—save only in the instance of political theory and to a lesser degree in public law—probably has some significance. Perhaps this omission is accounted for by the belief that the historical method has been so much used in the past that diminishing returns have set in. Conceivably its neglect is a sign of superficiality among those leaders of the profession who make up the panels. Much nearer to the truth is probably the observation that contemporary phenomena are actually so often different in kind as well as in degree that historical parallels are deceptive tools in analysis. Emphasis

has certainly shifted from similarity to differentiation; from precedents to uniqueness. In any event the emerging impacts of international cleavages upon nationality, of the tempo of scientific achievement upon the sluggishness of constitutional development, of psychological insistences upon legal formalisms—all suggest a new world for which reliance upon the "lessons of history" is of doubtful value and may be positively dangerous.)

Any narrowly legalistic approach would fare but little better. The public law panel in its report reveals how far the profession has progressed in this regard. It would, as a rule, turn its back upon the type of case approach as conceived by the traditional lawyer, and would integrate its techniques with those of politics, psychology, economics, history, and other disciplines.

Perhaps it is the very failure of a large sector of political scientists to realize how far the thinking as to public law has actually progressed that has been responsible for the almost total absence of emphasis upon the legal approach in the other panels. The concept of "political behavior" had so permeated (or infected?) panel after panel that the tools and methods of law seemed to be regarded as irrelevant and unfruitful.

The attitude of the groups toward the descriptive approach ranged from a decent respect to contempt. Perhaps the best verdict is the usual one—that it is an essential preliminary to all other approaches and that any analysis not preceded by painstaking, conscientious, inductive fact-gathering must be accepted with extreme caution. Among these frontier thinkers (and many if not most of the panel members were frontier thinkers) there was probably present the feeling of a certain boredom with pedestrian fact-gathering as an end in itself—as something which might well be delegated to junior colleagues or graduate students.

Of the four traditional approaches, the emphasis placed upon the comparative was by all odds the strongest. Explicitly, as well as implicitly, analysis of similar phenomena or problems or processes in varied settings was regarded as basic to political science and political theory—if a distinction can be drawn between the two, which is doubtful. The comparative study of the impact of large scale industry upon the governance of all modern indus-

trialized states was regarded as more rewarding and more important than the study of the historical evolution of a particular constitution. Similarly also for studies of federalism, of leadership, of legislative-executive relations, in fact of all the major political problems, the comparative method was deemed indispensable. Here again what was meant was no mere series of descriptions in or of a number of nations; but penetrating thought applied to the identification of universal trends or general principles.

In the light of the major stir created in the decade of the twenties by the so-called scientific method, the relative absence of any systematic attempt on the part of a panel to articulate the present status of this method calls for comment. If one identifies the term "scientific" in some peculiar fashion with the terms "quantitative" or "statistical," it would probably be fair to say that our profession has found the field for the use of this method to be considerably narrower and disappointingly less significant than the high hopes originally entertained for it. Whether this is an inherent limitation in dealing with political phenomena, or because no real attempt has been made to evolve such scientific measurements is of course arguable.

Yet in a larger sense, the panel discussions and reports bore rich witness to the fact that, not only was the attempt to introduce the scientific attitude not lost, but on the contrary as attitude or approach it is now so woven into the profession's thought pattern as to have passed into the realm of the "taken for granted." Virtually without exception there was an intellectual humility characterizing the panel discussions, a studied and consistent effort to keep the consideration of the great issues and problems of the present free from any emotional bias or distortion. The search for objectivity has many pitfalls along the way. It is refreshing to find how well our frontier thinkers have avoided them.

One approach akin to or a part of the scientific is that of the field study. Enough of the panels singled this out for special mention to make it safe to characterize its emphasis as very much alive. To rub its nose in reality is a useful exercise for the academic profession almost any time, and the field study is this kind of breed. One may remark in passing that the migration of

so many scholars into government during the war seems likely further to add to the appreciation of field work as a research method high in general esteem.

In their totality the discussions on methodology laid the major emphasis, not upon any of the traditional approaches to political science, but upon the insights obtainable through utilization of the content, concepts, and methods of the other social sciences. While the reports without exception stressed these insights, it is well to record the warning to beware of superficiality in this regard which was voiced at some of the meetings. Up to a point this warning represented the conviction on the part of some, not only that the central problems of political science should continue to be the preoccupation of the political scientist, but also that he could make much his best contribution by a thorough mastery of his own tools rather than by dabbling with those of others with which he must necessarily have but a nodding acquaintance. If the other social sciences could contribute toward the solution of our problems—and this was not seriously challenged—such a contribution would be better made by collaborating with the scholars in these other fields rather than by the political scientist spreading himself methodologically thin.

It would therefore seem fair to say that basically there was no quarrel as to the usefulness of these insights and methods, but only a fear of one person's attempting too much.

What then are these insights?

The political scientist and the economist are analysts of institutions. They are also analysts of social processes. Both institutions and processes are emanations of human behavior, the former no less than the latter. To isolate the political man or to postulate an economic man—each in a vacuum—may be useful in the analysis of a particular factor, but both are at their best deceptive exercises. The political man is caught in an economic mesh; the economic man is unpredictable in periods of heightened political tension. Both alike are pulled and even dominated by motives and loyalties associated with religion and family and successful social adjustment.

To what conclusions does this lead?

Surely one conclusion is the renewed realization that all social

sciences are studies of the mind of man in action and hence are ultimately but branches of psychology. Another conclusion is that only an apprehension of the totality of human behavior in a given situation, a given problem, a given institution, a given process can yield accuracy in analysis, especially when that analysis is directed toward suggesting remedial measures, or predicting social trends. Tough problems lying at the heart of the workings of democracy, of international relations, of large scale administration simply do not and by their very nature cannot yield to an unaided political science.

The tools of the psychologist, especially the social psychologist, have won a measure of favor among political scientists, especially among the students of public opinion and international relations. In spite of instances of superficial and inaccurate use and in spite of areas of disagreement and admitted gaps in the knowledge of psychology itself, enough that has been truly illuminating has been written to predict and urge further application of psychological tools and concepts. In the field of applied political and social psychology, the dictatorships have initially been far ahead of the democracies. The disturbing fact is that probably they will inherently remain so, because it is permitted to the power-hungry demagogue or dictator to use devices which those of us who retain faith in and govern our action by the major premises of democracy and its underlying ethic would scorn to use. It is, however, at least permitted to us to study these methods and devices, and expose them.

Of the other social sciences, probably the most useful at the present time—and at the same time the least used—is cultural anthropology or cultural analysis applied to the contemporary scene. The anthropologists themselves are largely to blame for such limited use, for they suffer from a virtual preoccupation with primitive societies and a lack of courage to apply their tools to the advanced and complex civilizations of the contemporary industrial order. If one takes a work like Leighton's *The Governing of Men*, the insights and understanding of political problems arising from the tools of cultural analysis become evident. Here there is brought to bear upon the analysis of administration of a War Relocation Center the total cultural experience and

background of the relocated Americans of Japanese ancestry. The "principles of public administration" seem feeble in comparison with these insights from a sister discipline.

Sociology in the sense of the description and analysis of social processes is scarcely less useful as a tool or approach.

What the panels, at least some of them, were attempting to say was that many, perhaps all, of our fields and problems of political science can best or only be understood by political scientists literate in that cluster of closely related disciplines called social psychology—cultural analysis—sociology. Many would confidently predict that this is to be the next major development in our profession. Problems as widely diverse as the functioning of Congress and the fall of Czecho-Slovakia will not otherwise be skillfully and accurately explored.

We need less argument or demonstration of the value to the political scientist of an understanding of the economic order. The growth in prestige of the concept of "political economy" is surface evidence thereof. Deeper evidence lies in the preoccupation of government with economic problems and operations and the impact of these problems and operations on governmental functioning and structure. The panels underscored all this.

Closely related to all the foregoing are the contributions made by the geography of resouces and the geography of location. Obviously and especially useful to the political scientist studying international relations and war, they are surely worth exploring also by students of federalism, regionalism, and local government.

Finally, and of another character altogether, is the notable plea made in certain of the panels that the political scientist also become a philosopher. This in no sense is confined to the Neo-Thomists. Rather has the rhetorical question "Knowledge—for What?" brought reluctant attention to the need for an apprehension of what is meant by concepts like "value" and "meaning," philosophically and ethically speaking. Philosophic thinking, philosophic insights, may be the only types to which a problem, such as the reasons for the spread of communism, will ultimately yield.

What the panels have to say on methodology adds up to this:

that political phenomena require analysis from *all* aspects to be accurately and completely understandable. The political scientist must view them not only through the lenses of history and law to which he has become accustomed or through such descriptive and comparative lenses which he might regard peculiarly or primarily his own, even when these are reinforced by quantitative tools and field observations. He must also use the lens of the psychologist, the anthropologist, the sociologist, the economist, the geographer, the philosopher. If it be argued that acquisition of skill in the use of all these methods and, even more, the depth of reflective thought necessary for the subsequent synthesis are beyond him, one can only reply that there may be no other way to such a synthesis. Rare will he be who reaches it, but the man who does so will be among the rulers in the kingdom of knowledge.

For the rest of us, it will be enough for many if we realize our limitations and do not claim over much for our research and conclusions. Yet more is demanded of all of us, and that more is that we continue to widen our horizons, and one by one attain those skills and insights hitherto largely reserved to scholars in other fields. As these additional skills and insights are ours, we can more completely approach the exploration of the problems peculiarly our own.

IV. Training for Research

Before concluding this discussion, it would seem to be appropriate to call attention to two or three obvious implications of the foregoing for training and graduate study.

Patently the graduate school curriculum could profit by any device which would make operatively evident the social science base upon which political science has erected its superstructure. The way is wide open for experiment in this regard. A survey course in the social sciences at the graduate level is one possibility, in spite of the wide variants in previous preparation among the students likely to participate. An expansion or enrichment of the frequently offered course in the methods of political science is another and more probable development. Interdepartmental seminars dealing with some major problems are likewise useful, if

carefully developed. A succession of short courses in the several approaches might be formulated. What is obviously needed is a greater realization on the part of our graduate faculties that the amassing of facts can as readily be performed after graduate training as during, but that methodology requires a certain contagion and practice until it becomes a habit of mind; and that this is obtained with difficulty or belatedly, if at all, apart from the guidance of the mature and stimulating teacher during the formative graduate school years.

The expansion of three other occasionally used devices of research organization or method seems also inherent in the panel reports. These are areal or regional studies, training in group research, and the use of internships for field observation and experience. It is not that all, or in fact any, of these need be part of the training of all graduate students. It is rather that, relative to the more orthodox graduate programs, they are far less developed or frequent than their already evident contributions to frontier research warrant.

Entirely on his own responsibility, the writer in conclusion would like to raise the question as to whether what will ultimately be the outcome may not be the rise of the *social scientist* as such—one who transcends in his preparation, his outlook, his research, and his creative thought, as well as in his name, the contemporary classifications of knowledge. The possibility, for example, exists that a man like Arnold Toynbee may take his place at the side of Aristotle and Aquinas as the contemporary incarnation of this great and lofty concept. Surely we are not pleading for superficiality nor are we arrogant enough to claim that contemporary human relations in all their aspects can ever be fully mastered by any human being. We do, however, raise the question whether somewhere, some day soon, some university or institution may have the courage wholeheartedly to explore this idea of a "social scientist" in all its major implications—to see what the consequences thereof would be to the outlook of its faculty, to the training offered its graduate students, and to the product of its research.

Chapter XII

THE FRONTIERS OF POLITICAL SCIENCE

Ernest S. Griffith

I. Introductory

A BLUEPRINT FOR THE immediate future of political science emerges from consideration of the panels as a whole. A correct and bold synthesis of these panel reports furnishes a kind of collective judgment of the profession as to the frontiers of its thinking, and hence its probable course for the next decade. It is an exciting picture.

A word of caution is necessary as to a possible limitation inherent in the original selection of the topics of the panels. Attention has already been called to the omission of focused consideration on certain fields, such as political parties and public opinion. Probably other similar gaps will suggest themselves to the reader. While every attempt has been made to "edit out" this type of distortion in what follows, nevertheless a summary is necessarily limited by the material summarized.

In this over-all view, attention is directed chiefly to three questions.

First, which common denominators appear in the fields of interest of more than one panel? In other words, are there certain areas earmarked for exploration which more than one panel has deemed of vital importance? If such there be, then these fields are likely to prove among the ones in the front rank of deserved attention. This is something deeper than merely a counting of the votes of those holding them important. It is rather that they are areas which inherently are meaningful to, or illumine, a wide span of other problems and fields.

Secondly, what are the declining or relatively unprofitable areas? Either by explicit rejection or implicit ignoring are there areas which panel findings would suggest that the scholar who has ambitions to make his research creative and significant would do well to avoid?

Thirdly, what are the fundamentally dynamic areas today, which make up the "growing edge" of political science? Some of these will be found to constitute a rediscovery of forgotten truth; others will be along the borderline between political science and other disciplines; others will reflect crises in contemporary governance; and a final group will derive their importance—especially to Americans—as corollaries to America's present role in world affairs. This fourfold grouping, somewhat artificial though it is, still does seem to be the one that emerges from the panels themselves.

Hence this concluding chapter will not so much attempt to recapitulate or summarize the details of the various panel reports, as it will to rise above these details and group in a framework of meaning the consequences of the inclusion or exclusion of various areas of recommended research.

II. The Place of the Traditional Fields

Certain traditional fields appear to be less emphasized than before. Probably the preoccupation of the past decade or two with its dizzy succession of crises has given to political scientists as it has to many others a sense of urgency. These crises bid fair to continue. Constitutional law, international law, and even jurisprudence for the time being would appear to many, if not to most of the profession, to hold a low priority, for there was scant mention of them even in most of the panels in which their emphasis might have been anticipated. Yet as the chairman of the public law panel has well put it:

> The need for "action and action now" has distracted popular attention and the attention of many political scientists from the need for maintenance of the rule of law, if government in terms of our traditions is to be maintained. However, in spite of distracting pressures the practitioners of public law are individually struggling with the debris of social disorder in an attempt to rediscover and reestablish basic patterns. They meet the same frustrations which are met by the contemporary Supreme Court, which periodically splits and splinters in all directions in its attempts to redefine the course of constitutional development. While as of the moment they are not inclined to boast of their success, they have no doubt as to the neces-

sity for achievement of that for which they struggle. The field, far from losing its importance, has by the thrust of circumstance achieved an importance never previously attained.[1]

As regards jurisprudence in general, there were voiced pleas for research and thought which would again make political scientists aware of the role of law in society, that laws could empower as well as restrict, and that the hard core of law was the stuff of which political science was made. Significantly it was felt that research and thought in public law must for the present at least be largely by individuals rather than groups, because its principles themselves are now so much in a state of transition that groups can scarcely find a basis for agreement. Moreover, research in public law must not be narrowly legalistic; it must be grounded in political theory and at the same time skillfully integrated with history, psychology, economics and other disciplines. When it is so grounded and linked, it retains its historic importance and merits a renaissance of prestige.

"When history, law, and philosophy are combined, there is a powerful reality in this approach. Deep in the history of law and political practice are found values that lead straight to the defense of human freedom." So writes one of the panel members in protest against contemporary neglect of legal research.

It is possible that the international relations panel through its title may have conveyed to some of its participants the implication of an intentional exclusion of international law from its orbit of consideration. Consequently the almost total absence of references to this field should not be pressed too closely. Moreover, there is obviously great interest in the subject in many of the law schools. Yet there is also the possibility that the great cosmic forces let loose in the world have so far destroyed international law or so far shrunk the interest in it or dwarfed its significance, that not many political scientists would select it in the near future as a field of research to which any high priority should be assigned. Yet ultimately it must again come into its own, and some at least of our number should engage in the effort to think through its future outline.

1. Letter, Carl B. Swisher to Ernest S. Griffith.

An exception to the occultation of the legal galaxy must certainly be made of administrative law; but this is better treated in another connection.[2]

There was a time when textbooks, courses, and even doctoral dissertations and post-doctoral monographs were preponderantly treatments of governmental structure. National government, state government, municipal government, comparative government, international organization, legislatures, administrative organization, federalism—in fact the descriptions of any and all political institutions—were the sum and substance of most of the teaching and research in political science. In the light of a momentum and a tradition as great as this, obviously the panels must inevitably have given some attention to problems of structure under these various headings.

Yet a close and continuous reading of the reports leaves one with the feeling that this is largely lip service. Perhaps research in structure is largely a completed task; perhaps we are indeed witnessing the evanescence of constitutional inventiveness and the formalization of administrative patterns. Post-war constitutions have been singularly lacking in new and fresh signs of adaptation to the major tasks of government in the contemporary world. By way of illustration, the constitution maker seems to have lost his interest in a possible democratic corporatism with the demise of authoritarian corporatism, and in spite of an initial presumption of its adaptability to the newer structure of society.

The major shift of interest in these matters has been from description of structure to analysis of its consequences. The pragmatic note in comparative government has grown stronger at the same time that constitutional experimentation seems to suffer from arrested development. We are in a period of soul searching as to the things that are, with the shape of things to come not yet evident.

Only in legislative-executive relations and in federalism and the various devices to establish sound inter-level relationships can major areas for exploration of structure really be identified. In the case of federalism, interest in the organizing of a supra-

2. Cf. p. 228 below.

national government and interest in the preservation of the vitality of state and local government coincide in setting a problem having certain vital common elements.

This is not to say that all of the traditional is similarly moribund. In at least two areas, truths or doctrines which were scorned or largely forgotten as recently as the first decade or two of the present century find themselves the subject of renewed interest. These are the doctrine of natural rights and the concept of community.

Curiously, both owe this new sense of vitality and significance to the rise of totalitarianism. The dusting off of natural rights is one of democracy's inevitable answers to their challenge and denial. The idea of community as superior to state serves equally well as the theoretical basis for a communist world and a free social order whose goal is human welfare.

Not so long ago the basic theory of natural rights was at a discount even in the house of its presumed friends. The sociologists of the positivist persuasion had, to their own way of thinking, demolished its theological underpinning. Even some political scientists considered it enough to observe and describe, and gave little attention to the need for defending it. Liberty, or liberties, assumed the comfortable status of useful culture traits, somewhat superciliously characterized as among the mores of our particular generation.

The awakening has been rude. We are confronted with a contemporary world in which so-called useful culture traits such as liberty are gleefully overthrown when some allegedly better objective—such as security or class triumph or national conquest—is held out. The hollow way in which so many of the Czechs, presumably the most liberty-loving people of Central or Eastern Europe, have cynically or in helpless fashion voted Communist or surrendered to force and are now cheering their enslavers, itself shows how thin the veneer-like loyalty to liberty really is when it has no theological conviction back of it.

Hence in those panels in which the subject matter was appropriate, this challenge was put. How can natural rights become once again a matter of profound and unshakable conviction? In the mood of Thomas Jefferson, the question is once again posed,

"Can a theological basis be found whereby the bulk of mankind can share the conviction that 'man is endowed by his Creator with certain unalienable rights'?" Perhaps there is no more important field for political scientists to explore. And, if one scorns the theological, can science provide an alternative base? What would carry equal appeal?

As an echo of the Middle Ages there comes again the concept of "community" as a living thing. It is becoming increasingly difficult to defend "state" as a separable element in the great society. A de-differentiation or fusion of the state and other institutions is taking place, which is far from being confined to the totalitarian nations. This conceptual and institutional de-differentiation or fusion is hastened by a realization of the meaninglessness of thinking which treats social phenomena as isolated. The political scientist who would understand power, its locus and its exercise, must first understand the nature of society, that is, the nature of community as consisting of the interlocking relationships of the individuals who make it up. Questions, such as the necessary identification of democracy with a pluralistic community, and totalitarianism with a unified one, suggest themselves not only as among the most profound and difficult, but also as among the very few of first importance in the contemporary scene. It may be to Aquinas and not to Rousseau that one must look for light.

III. Borderline Fields

It is natural to turn from the general observation of the blurring of the borders or the indefinite extension of the body politic or the "state," to other evidences of the fertility and importance of research along the borderlines between political science and the other social sciences. We have already called attention to the importance of such cross-fertilization in the matter of methodology. It exercises a similar role in the matter of content.

Political behavior has largely replaced legal structure as the cardinal point of emphasis among political scientists. It would seem then that the social psychologist and the sociologist are challenging the lawyer in thus achieving the center of interest

of our political thinkers and researchers. A striking example is found in the recommendations of the state and local government panel. Here the central preoccupation is in the way in which human beings operate local structure and institutions and not in the cataloging or description of institutions. Of the same type is the strong recommendation that the effect of centralization on attitudes is probably more worthy of exploration than is the formal distribution of powers. So also at the other pole, the mainsprings of national policy, the motives and techniques of parties, leaders, and dictators, loom as more important for research than the precedents of international law or even the formal arrangements of international organization. It is not that for one moment the political scientist abdicates in these fields to the sociologist or psychologist; it is rather that he recognizes them as borderline fields which neither he nor the sociologist can fully understand unless each draws also upon the insights and tools of the other.

There is to be noted an interesting parallel with recent historical scholarship at this point. Just as meaning or a structure of meaning has replaced chronology or mere narrative as the summum bonum of the historian; so analysis of political behavior or cultural analysis has the prestige formerly assigned to formal description and law among the political scientists. Usages of a constitution, its modification under the impact of political parties, seem more worthy of exploration and study than its framework or its case study. With full awareness of the danger that this trend will lead to loose and superficial thinking, it is fair to state that at its best research with this newer approach will yield a realism and depth of analysis denied to the old. It will lead alike to a more profound diagnosis of current problems and a more accurate prediction of future trends.

For such an approach will start with motivation. The political psychologist may help to furnish the necessary insights to trace the mainsprings of political behavior among individuals. With full recognition of the many uncertainties and disputed points—even fundamental points—among the psychologists themselves, nevertheless one recognizes convergences upon principles and findings, if not full agreement. Let us be frank at

this point. Analysis of the political behavior of individuals by political scientists has at times uncritically accepted certain dogmas or assumptions of psycho-analysis, at the very time that the psychologists themselves were viewing these assumptions with increasing scepticism. Yet this is not the same as saying that such analyses have not been of value methodologically speaking, or that our profession should eschew further attempts at such border-line exploration until the psychologists have made up their minds. The fact is that research in and understanding of leadership and followership, of the preludes to dictatorship, of the power struggles within a community or group and between communities and groups involve political problems of the type that simply cannot be intelligently explored short of considering the psychological nuances of the individuals involved.

Nor are they understandable or explorable without a similar mastery of the sociology of group behavior. Particularly fruitful is such exploration in the stubborn area of international relations in which the conflict of cultures is superimposed upon economic and political rivalries; and national and international loyalties are in conflict. Other fruitful areas of exploration along this particular borderline between political science and sociology range from heterogeneity as a factor in municipal politics to the sociological factors evidenced in the workings of separation of powers or bicameralism.

Political behavior as a subject of research reaches perhaps its ultimate dimension in the exploration of government in its social context. The impact upon government of economic, social, religious, geographic forces and factors furnishes a whole series of rewarding explorations which in their turn will render luminous the entire political scene. Government never has and never can operate in a legal vacuum, unpenetrated by unfolding forces from other segments of a culture. In the study of such interaction lies one of the most important among the many priorities for future research.

Among the fruitful borderline areas lies also the challenging field of political communications. The panel report devoted to this subject leaves little to be said at this point save only to underscore the fact that here also is a striking example of the emer-

gence of new and important problems and insights, once the political scientist is encouraged to join forces with others in the social science field.

In the relationship of government and economic life, one point of view and several areas have seemed to be high lighted in the panel discussions. The point of view notes the reemergence of political economy as a dominant concept in much of our contemporary thinking. The point of view of laissez faire, which postulated politics and economics as whirling in self-contained orbits, is dead, buried, and damned. From isolated government intervention in particular situations virtually every major industrialized state has moved into a national planning in which it is becoming as increasingly futile as it is difficult to attempt—even as an exercise in logic—to separate government and business. Rather must thought be more and more directed toward the subtle as well as the overt ways in which this fusion of the erstwhile separate or at least separable aspects of man's social activity has once again become such an integral part of the community's central objective as to constitute more properly a single field of study.

For the concept of political economy implies objectives, and these objectives seek their realization alike in structure and function. They become most evidently dominant in the international impact of the nationally planned economy, and wreak havoc upon the theoretical values of free trade. They send the smaller states scuttling to cover through association with one of the two or three or four giant powers. All this is fairly familiar in its broad outline, but it requires rigorous exploration of its ramifications and consequences.

We are less aware of the changes effected by the on-coming political economy and its objectives upon our internal domestic institutions. Most pervasive is its role in developing the process of adjustment between conflicting power groups. Whatever the ritual, be it the regulatory commission, price administration, or the corporate state (to mention only a few), the process itself is increasingly clear. Once a political economy determines upon its objectives, it follows as a corollary that the various elements in national life must be brought in line with these objec-

tives. In what used to be known as the economic world, this involves, not primarily the passing of laws, but continuous governmental intervention, in any or all of the relations between economic groups. The self-adjusting economy is agnostic as to ends, and the myriad consumer choices guarantee neither national strength, nor the conservation of vital resources, nor the good life. That the clue to both national strength and the good life is found in political economy is the universal verdict of the modern state. It is for the political scientist to become political economist and explore this trend in all its magnitude and intricacy.

Approached in this fashion, the study of administrative law takes on new meaning. It becomes an affirmative, positive tool and not a narrow inhibiting procedure. It contains possibilities for safeguarding human rights, even though the facilitating of state action, not its limiting, seems its destiny. Whether one sympathizes with or deprecates this trend is beside the point. The nub of the really important research seems to be found in the apprehending of administrative law as primarily a tool in the adjustment process, with the latter society's chief activity in realizing its national objectives. Nor should the imaginative scholar stop at this point. Whether it is to be one world or two which is to transcend national lines; within these greater political economies, adjustment on a vaster, grander scale will necessarily be sought, and will take all the wisdom of the politico-economic man to make it effective.

IV. Crises in Contemporary Culture

The recommendations of the panels contain another group of major problems or areas to be explored, which have at least this much in common—they involve crises in contemporary governance. In understanding them in their collectivity, the concept of "culture lag" or "cultural disharmony" is especially useful. Each of these crises may be thought of as the by-product in the governmental sphere of a major change in some other sector of world culture. Each evidences the way in which the deeply rooted institutions serviceable to an earlier age are shaken to their foundations and warped and distorted by the emergence of

a new and great development not previously adjusted to or anticipated. The researcher to deal intelligently with the situation thereby created must first understand the social force or forces that produced it, and then and then only will his findings be imaginatively fruitful.

Of this nature is the whole series of problems created by the rise of the group, especially the economic group, to its place as the dominant reality in the political scene. Institutions created on the assumptions of individualism are strained or shattered thereby.

For the group is here to stay, and it is for statesmen to find ways to assimilate it in the new political economy. It is here, because technology with its specialization of function has decreed it; and the laws of sociology have made it, at least in times of peace, the most articulate and the most powerful of political realities. The social pluralism created by its proliferation is basic in political analysis. In local, national, and international politics its impact forces one to study it; for it is the very texture of politics itself.

From the study of its impact on politics to the search for its actual and potential effect on structure is a natural step. The politics of group pressures leave their impact upon legislation; and agriculture and labor and many lesser groups find "representation" in the departments and agencies of government created by this legislation. The subsequent struggle within government, between the powerful dispersiveness with its conflicting objectives (which is the by-product of the origin of these agencies) and the over-all coordination or planning attempted in the name of the public interest, is one of the most important as it is one of the most difficult and obscure problems of contemporary governance. The former is the institutional evidence of culture lag; the latter is the attempt at adaptation or search for a new cultural synthesis.

It is at this point that group representation in an economic parliament and the ramifications of a corporate state may assume an importance in future statecraft far greater than the very limited success hitherto attending them. Certainly the political scientist who lives on the frontiers of his field will

readily recognize that the problem of adjustment or integration of groups (to which these devices are a response) lies very close to the heart of still another problem—the possibility of the simultaneous existence of an integrated economy and a democratic state. He will study carefully the experience of corporate and pseudo-corporate states, such as Italy, Portugal, Germany, Austria, to unravel the origins of the autocracies therein expressed, and to see whether corporatism might in fact be a democratic device as well—the regulation or administration of at least some of the industries of a nation's economy by representatives of the groups with a stake therein. For the group, which hitherto has had its impact on governmental function largely expressed in terms of an administered or protected scarcity, might well be harnessed in conjunction with other groups as an instrument for adjustment in the light of the objectives of the political economy as a whole. In any event the elimination of the cultural disharmony of the unassimilable and discordant group in the bodies politic and economic (or in the emerging political economy) stands out as among the really great areas of research to which the panels would give priority.

That national and also metropolitan governments have grown faster than the ability to manage them has created a second great culture lag or disharmony. Here is a favorite area for research on the part of the students of public administration. Most of the resultant problems are organizational and procedural. Traditional problems, such as span of control and line and staff relations, are tremendously magnified; and the solutions of acute problems peculiar to big government, such as interdepartmental liaison and integration and central-field service relations, are still very much in the realm of theory. Other serious problems center around the fiscal underpinning, around crisis government, around the difficulties of enforcing responsibility or accountability, not only to the administrative head, but even more to the presumed policy makers in the legislature. Then, too, as the bureaucracy grows in size, so it grows in political power, and acquires vested interests in its own perpetuation, prestige, and expansion.

In so far as the legislative branch asserts its control, and yet

is not itself well informed or relatively free from the impact of the gusts of a mercurial public opinion, the dilemma at times seems to be presented as though it is between representative government and effective government. The causes of such a dilemma, if dilemma it be, the ways of its resolution, the relationships of it to sheer bigness or extension of governmental functions, these assume high priority in research and penetrating thought. How far is there a continuity of statutory accretion that makes this problem less serious than it seems?

Nor are these two problems of the non-assimilability of the group and the control of the bureaucracy the only ones which indicate the possibility of serious obsolescence in traditional democratic institutions. To quote: "Liberalism [the traditional democratic underpinning] postulated free trade; it found incredible trade barriers. Liberalism postulated the rule of reason; it found emotionalism rampant. Liberalism postulated the individual as the motivating unit of political action; it found the individual already an economic anachronism and the group the most all-pervading of phenomena."[3] So the assumptions of the creative political-economic thought of the nineteenth century one by one were whittled away; and the liberal democratic constitutions find themselves profoundly shaken or changed or in search of new assumptions to buttress the old. We are not sure what will be the eventual outcome. For example, it has yet to be proved that the quintessence of political liberalism, proportional representation, and its corollary, the multi-party system, are capable of producing a workable government in a large, industrial state. Security, once the objective and apparently the product of democracy, now is assigned by many the role of democracy's seducer par excellence. This is all the more true when democracy's enemies use all the tricks of misrepresentation and propaganda to deck security in a fashion which conceals the potential hussy within her. Steady and clear thinking, careful research combining the resources of the economist and psychologist with those of the political scientist, are needed to unravel the relationship of security, real and fancied, to modernized democratic institutions.

3. E. S. Griffith, *The Impasse of Democracy* (New York, 1939), p. 16

Moreover, many of the old ways of citizen participation, especially those associated with local self-government, show signs of decline. To find substitutes more adaptable to modern nationalism or internationalism is the last but not the least of the crisis problems in contemporary institutional democracy. To define democracy as the right of the individual to participate in decisions of importance to him is to set an objective. The ways and means of realizing this objective, or even the possibility of so realizing it, are the province of the researcher. The magnitude and span of the problems faced by contemporary government and the intricate specialization needed for their solution suggest no easy answer.

In the adaptability of the democratic state to contemporary needs, the role of the political party is crucial. Highest priority in this matter falls to a reconsideration of the nature and functions of American major parties:—the selection and management of the personnel of government versus the formulation of policy; the role of parties in the conciliation of local and special interests; parties versus pressure groups. Ways of popular control of party machinery, the role of party in legislative-executive relations, and the actual locus of power in party control—all are relevant and all are important to understand.

The autonomous nation-state seems to have become an economic anachronism; it bids fair to invite likewise a military catastrophe in its lag behind the realities of the economic, ideological, and scientific world. The gravitation of the weaker states toward two or three great "solar systems," whose suns are the remaining "great powers," suggests the need, either for disciplined thinking as to the nature of partial inter-continental federalisms or of ways and means short of federalism of effectively institutionalizing the "blocs" that are developing.

Last but not least we are urged by certain of the panels to consider the profound implications of the varying rates of progress in science and social control. Closely related to this and even more serious is the lag or gulf between the dynamics of social control and the necessary or requisite spiritual motivations.

Inventiveness marches with varying pace. The nation has no weapons for defense against the atomic bomb; and inter-

national relations writhe under the strain. Atomic power opens up vistas of a land and world of plenty, and the corresponding technocracy or the corollary in the realm of industrial engineering is so far outside the climate of contemporary opinion as to invite ridicule rather than resistance. If we are to realize the potential good of nuclear physics, we must as a world meet world responsibilities.

If we include psychology among the sciences, the inventions of opinion control apparently have shot well beyond the discovery of educational devices which might emancipate the mass of men from the danger of their exploitation. Nor have the righteous found really effective means for the social control of applied psychology. Here, too, then, is a major cultural disharmony which reflects itself in terms of a crisis in contemporary governance.

Somewhere near the heart of democratic survival lies the question of the separability in man's thinking of technical means and social end. The former is or may be the province of the specialist; the latter may be democratically determined. If means and end should prove to be thus separable, or to the degree that they so prove, the political scientist may possibly find the clue to distinguishing the role of the bureaucrat from that of the legislator. The responsibility of the technically competent, the participation of the amateur, alike are bound up in the adaptation of the political process to these inherent distinctions. The translation of our thinking from these abstractions to their concrete meaning in particular situations is identifiable as a major area of research.

In concluding this section we would speak also of the imperative reenthronement of values. If the political scientist as scientist recoils a bit from the obvious danger of his own values distorting his research, perhaps he will nevertheless accept the importance of research and thought as to the role that values play in the political scene. We are all of us pragmatists enough to see that certain values, toughly held, produce dictatorships; others produce an industrial oligarchy; still others disintegrate a state through class conflict; yet others, by placing the development of individual personality as an ultimate end, create or pro-

pel political and economic institutions designed better to accomplish such an end. There is a profound belief on the part of the Communists that the wave of the future is theirs. The psychological vacuum in the weakened democracies of Europe and Latin America and even in our own democratic society and state implies the lack of a comparable faith to move mountains. It is this that justifies the investigation of the implied hypothesis with which this paragraph began—the imperative reenthronement of values.

V. The World Position of the United States

It is not surprising that a whole group of recommended research priorities cluster around the world position of the United States. That reluctant imperialism of the spirit which by and large characterizes our people does not find the political scientists similarly inhibited. With all our heart as well as our mind we political scientists apparently wish our nation to rise to its responsibilities, and we see for ourselves a major role in the hard thinking that must accompany such a world leadership. Many of us believe that our democratic institutions, and even more our democratic ideals, are transferable or exportable; and we would do our share in the important but difficult analysis of ways and means to attain such an end. It is not to detract therefrom to enter a note of caution that too easy or too enthusiastic acceptance of this doctrine of transferability may defeat its own end.

The report of the panel on the government of occupied areas accepts such responsibility without question. Even under the most favorable conditions the imposition of new institutions is not an easy task; and chaos of conquest and the presence of an alien culture create appalling additional hazards. Yet the critical role to be played in the future by virile peoples like the Germans and Japanese shows the very high priority which is correctly assigned to political scientists in such military government. Nor would this panel have stopped at this point had the time of its meetings been extended. More than conquered peoples now look to us. Greece, China, and to some degree most of the ERP

nations, find themselves in the highly delicate situation of at least needing to take account of the American viewpoint in their domestic affairs and institutions. We on our part paradoxically find that our very reluctance to impose our will is one of the greatest assets in making this will felt. Yet false steps can do enormous damage. Hence it is fair to place coordinate with research in the methods of governing occupied areas the methods of advising or steering or co-operating with other areas and peoples whose more or less fragile political economy needs, not only our money and military support, but even more the contagion of the best in our democratic spirit and our experience in successful governance.

American political scientists have tacitly assumed their function to be the analysis of peace-time problems and institutions. War came into our horizon as something to be prevented, and our study thereof was dominated in the international relations field by investigations of ways and means of its prevention. A minor note, which was regarded as sophisticated by those in sympathy with it and as cynical by those who claimed to be more idealistic, was the power-political approach in this field.

The panel on the political scientist and war does not ask that these earlier approaches be abandoned. It does, however, claim that rewarding results will follow, if at least some of our political scientists will conduct their research on the assumption that war, not peace, may be normal for some years to come. This is particularly true, if they view the intervals between the fighting as war fought with other methods. An alternative assumption would be that the behavior of states is very largely influenced by the expectation or fear that war may recur. Such assumptions highlight many areas of research hitherto neglected or ignored. Relations between the civilians and the military, the organizational aspects of war-time political economy, political elements in over-all strategy as well as in detailed tactics, these will serve by way of illustration from among the many suggestions made in the panel report.

Moreover the political scientist is exceptionally well qualified to deal with the whole concept of "national strength." The na-

ture of total war, including its preparation, transcends not only the military factors, but the economic as well. It includes the confidence men feel in their governmental institutions, the efficiency and integrity with which they function. It includes the ways and means of enlisting and sustaining allies. Even more it moves in the whole area of psychology and the things of the spirit. Unification of motive, élan, a sense of destiny, these and other factors come at least in part within the orbit of our appropriate research. Finally, this panel obviously believes that the political scientist is peculiarly fitted to see and appraise the total aspects, the synthesis of all these elements—military, economic, governmental, social, spiritual—which together constitute the strength of a nation or a way of life, when locked in a life and death struggle with a rival nation or system. That a substantial group of our scholars believes that such a struggle is even now upon us carries with it, not only the obligation to find ways and means of avoiding it, but equally the obligation to work out ways and means of winning it, if avoidance proves impossible.

To spell out in detail the research needed in connection with our foreign policy was the task of the international relations panel, and there is no need at this point to do more than to underscore once again how much attention was directed to motives and forces, how little to international law and even to organization.

As a nation we must formulate and understand our national objectives. No longer can we play by ear and improvise or falter, if we would survive. For what do we stand and why? At what point would we fight? What other nations have policies similar to our own? Are these policies firm or fragile? What indeed are the mainsprings of the policies of other nations—the less known nations as well as the powerful? Is there or can there be an area of agreement or even tolerance between the Soviet Union and ourselves?

Today, more than any time in the recent past, transnational forces must be reckoned with as well as national. World Communism is the first great instance; but world Catholicism and world socialism are both forces of great power. Some would

add international capitalism, but the scepticism with which this would be greeted by others suggests that relatively little is really known concerning it. This is all the more reason for its study along with the other forces mentioned.

No study of our world position, or of international relations, would be complete that did not include within its scope the formulation and examination of proposals for world government. Probably most men would regard this as the ultimate objective, "the common destiny of the civilized world." Past experience with the League of Nations, present experience with the United Nations, all the stubborn obstacles and resistances which man has experienced—these all merit the most thorough exploration and analysis which scholars can give. Careful studies of federalism in operation, and of the British Commonwealth of Nations, should provide fruitful insights into this far greater and more difficult objective. Certainly one of the most rewarding approaches would be to concentrate on developmental or evolutionary factors. What aspects or activities within or outside of the United Nations possess the inherent capacity for growth? We hear much of the "larger loyalties," but know little of the technique of developing or evoking them. Is regionalism a step toward or away from "one world"? With nationalism shaken, we need to know far more than we do of the emerging alternatives.

For better or worse the world's destiny and our own are intertwined as never before. "How?" and "To what end?" are questions whose answers not only lie in the future; but the way of whose answering will itself affect the future.

VI. Conclusion

We would add one word in conclusion: The frontiers of political science are exciting frontiers. They need more for their exploration than the pedestrian mind. These frontiers are not for the intellectually timid nor the mentally stagnant. It is the creative thinker who must give meaning to the painstaking research that, while indispensable, is still not enough. Hence the panels throughout seem to have been right in this regard.

Never did they confine themselves merely to a listing of topics or areas for research. Always they have sought to nourish a new political theory, whose architecture is modern, whose dynamics are broadly social, whose breadth starting from the political man reaches out into all society.