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**Final Report
of the
Workshop on the Social Bases of Stable Rule**

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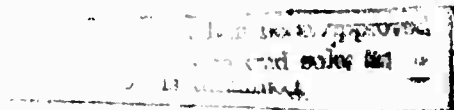
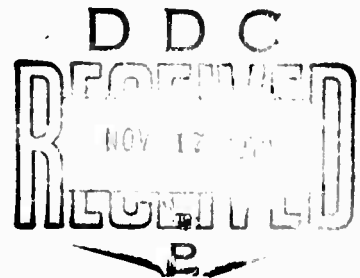
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13. ABSTRACT

The project undertook to examine the relationship between authority relations in non-governmental social units and governmental performance, on the basis of the hypothesis that levels of performance would vary directly with levels of congruence between governmental and other authority patterns. Research methods for investigating and scoring authority relations and for measuring governmental performance were developed, and applied in twelve countries. Performance data have been analyzed and scored, as have authority data in three of the twelve cases. Further analysis to arrive at an overall result is still in progress.

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KEY WORDS

LINK A

LINK B

LINK C

ROLE

WT

ROLE

WT

ROLE

WT

Governmental Performance

Authority

Congruence

Consonance

Durability

Legitimacy

Civil Order

Decisional Efficacy

Inequality

Social Distance

Participation

Responsiveness

Compliance

Permissiveness

Elite recruitment

Legitimacy

Collegiality

Introduction

This is a "final" report only in the technical sense. The work of the project is in no sense finished, and in fact is continuing, on a reduced scale, with NSF support and that of Princeton University. In addition, nine related projects by present and former graduate students at Princeton are now in the process of being carried out in the field (3) or reported on in the form of doctoral dissertations (6). An overall report of findings must await completion of their work. This report summarizes (1) the project's scientific problem, (2) the theoretical propositions developed to solve that problem, (3) the concepts and operations developed to test the propositions, (4) the organizational format of the research, and (5) the work so far accomplished.

The Problem

The project's principal problem was to account for the different performance levels of polities. What we mean by "performance" is stated in some detail below, and in greater detail in a forthcoming publication on the subject; but we are chiefly concerned with what has been termed political "civility" (the ability of polities to command allegiance or at least avoid civil strife) and the "effectiveness" of polities; i.e. their ability to arrive at and carry out policy-decisions, especially decisions relevant to strong social demands for political outputs and/or severe situational problems confronting the polity.

Although we originally were interested mainly in the performance of representative systems, we expanded that interest to include the performance of any and

all polities, by means of a set of general propositions that can be readily adapted to different types of them. However, because of problems of accessibility and certain extraneous factors (e.g. the personal preferences or language skills of those engaged in the project) more work has been done on representative systems than other types of rule. This imbalance we hope to correct in future.

Obviously the problem of the bases of political performance has been raised before in social research. Accounting for political "stability" (the durability of polities over time, without major disorders) is one of the classic concerns of political thought. Interest in the problem has been especially intense in political science since the Second World War, chiefly because of the impact on political scientists of the malfunctioning and collapse of many Western democracies before the war and the manifest inability of many new and developing nations to construct viable and efficacious systems of rule thereafter.

The great quantity of studies concerned, exclusively or partially, with our problem is an index of its importance and fascination. The chief reason for adding to that quantity was the inadequacy of the methods used and results obtained in previous work. This judgment rests on an examination of a comprehensive inventory of propositions relevant to the problem. The examination yielded a huge number of propositions, involving many different variables, which, although not all equally inadequate, were, with few exceptions, imprecisely stated, unsupported by convincing evidence or reasons (often by no evidence or reasoning at all), sometimes intrinsically unverifiable, and, if testable, readily falsified by existing evidence. Even the best propositions tended to yield rather weak theory, either in the sense of being only

indirectly relevant to the problem or in the sense of gaining wide explanatory power only through the ad hoc addition to central independent variables of many other variables, not weighted or otherwise related to the central variables, and mainly used to take care of cases contrary to the central propositions, in the familiar manner of "hypothesis-saving." Where this dubious procedure was avoided, contrary cases generally seemed to be at least as numerous as supporting ones.

The project consequently sought to develop theoretical propositions about the conditions of differing levels of political performance that satisfy, at least substantially, the following criteria: (a) that they be precisely stated; (b) that they be parsimonious (in the sense of having broad explanatory power and involving few independent variables, if possible a single one); (c) that they be logically cogent in three senses: no indiscriminate heaping up of unrelated factors in ad hoc multivariate theories, the derivation of propositions from explicit and plausible premises, and the possibility of the deductive prediction or retrodiction of unknown from known data on either side of an equation linking independent to dependent variables; (d) that they be readily testable (i. e. falsifiable) either directly or through deduction from the propositions of testable corollaries; and (e) that they be what might be called "higher-order" propositions, capable of explaining both why propositions using other variables (particularly the better ones) have the explanatory power they possess, and do not have more.

Theoretical Propositions

Previous attempts to account for the performance of polities, especially democracies, fell, virtually without exception, into two broad categories. One set invoked aspects of structures of rule themselves as principal determinants of the performance of the structures. A second set treated polities as epiphenomena of aspects of their social environment, such as economic structure or development, social stratification, religion, education, and so on.

For various reasons, we decided to avoid locating our major independent variable under either set of factors: neither to treat polities as purely epiphenomenal nor purely self-determining. Consequently, we sought as our independent variable a factor or set of factors that would direct attention simultaneously to governmental structures and their social settings, and that would do so less nebulously than the theories of political philosophers (e. g. Montesquieu) who held that the appropriateness of forms of rule varies somehow with variations in social structures and cultures.

After much speculation and reasoning, a factor emerged that promised to satisfy this requirement, as well as the criteria of adequate theory previously sketched. This was the degree of resemblance (or "congruence") between governmental patterns of authority and the authority patterns prevailing in certain non-governmental social units, particularly those most significant in processes of political socialization and political elite recruitment (such as, in most societies, secondary and higher educational institutions). "Congruence" theory did in fact emphasize a variable that simultaneously involved governmental and non-governmental structures. Moreover, it seemed to fit a considerable volume of familiar experience, especially certain

widely familiar cases of conspicuously high or low political performance; it seemed grounded on, indeed derivable from, much well-tested psychological and sociological theory about human behavior, including the general corpus of cognitive, learning and socialization theories as well as more specific theories of anomie, strain and cognitive dissonance; and it seemed possible to use it logically to account for both the strengths and weaknesses of other theories of political performance.

Because of anticipated difficulties in testing (simple, parsimonious theories are not necessarily easy to test) the decision to evaluate congruence theory on a broad empirical scale was postponed until after the conduct of certain "plausibility probes": attempts to examine the theory's cogency in a few less familiar or unfamiliar cases that had played no role in its original formulation. These probes yielded highly encouraging results and led to a provisional decision to attempt a more thorough evaluation of congruence theory.

A further consideration convincing us of the advisability of large-scale research into the resemblances of governmental and non-governmental authority patterns was extraneous to the primary purpose of the project, but important just the same. For nearly two generations now political scientists have periodically argued that the subject-matter of political science is too narrowly defined, above all in one sense: there are, in Merriam's words, "private governments" as well as public ones. All social units, not just the structures of the state, have their patterns of authority: of direction, power, legitimacy, influence, control, etc. Political scientists, despite numerous manifestos urging the study of private governments, have largely neglected these phenomena (undoubtedly due to a fixation on the

study of national governments), and so, by and large, have sociologists (presumably because authority relations are considered the scholarly domain of political scientists). The study of "social authority patterns" is not an empty set altogether, but far more nearly so than it ought to be, especially if one counts only well-conceived and systematically conducted researches chiefly concerned with such patterns.

At the very least then, we felt that a broad, systematic inquiry into social authority patterns would begin to fill a large and fascinating void in research. The fact that such authority patterns have since become a focus of much social conflict, especially intergenerational conflict, in many societies, has made research into them seem even more fascinating and important.

The hypotheses investigated in the project, and the rationales for them, were outlined in Harry Eckstein, "Authority Relations and Governmental Performance," Comparative Political Studies, Vol. 2 (3), October 1969, 269-326. We only summarize them here.

The central purpose of the project was to test the following hypothesis:

Hypothesis I: High performance by a government requires congruence between its authority pattern and the authority patterns of other (specified) social units in the society.

The intended implications of this hypothesis, which, as is evident, states a necessary but not sufficient relation between its x-and y-variables, were elaborated in three subsidiary hypotheses:

Hypothesis 1.1: If governmental performance is above a specified threshold,

congruence will also (in all cases)
be above a specified threshold.

Hypothesis 1.2: Above a specified threshold, con-
gruence increases monotonically
as a function of governmental per-
formance.

Hypothesis 1.3: For all cases, the correlation
between performance and con-
gruence will be high.

Since hypothesis I states a necessary but not sufficient relationship between its variables, it does not preclude the consideration of other independent variables that may account for performance below the threshold specified in hypothesis 1.1 or for the overall correlation in hypothesis 1.3 being less than unity. As a result of early researches and discussions we in fact decided to add, primarily for these purposes, a set of propositions that stress a different aspect of social and governmental authority patterns: the internal "consonance" of their elements. In essence, while the idea of congruence stresses the fit, agreement, or harmony among the overall authority patterns of different social units, that of consonance concerns the fit or harmony among component elements of authority relations in a single unit, authority patterns being multidimensional phenomena. The main (but not only) reason for adding consonance to congruence theory was that dissonance can trigger some of the same psychological processes as incongruence, and thus have similar consequences. Hence:

Hypothesis 2: High performance requires con-
sonance among the elements of the
authority pattern of a social unit.

This hypothesis was elaborated in three subsidiary propositions, hypotheses 2.1 - 2.3, worked similarly to 1.1 - 1.3. It should be noted that hypothesis 2 adds a value to hypothesis 1 also in that it says something about the performance of any social unit whatever, not just governments or other "inclusive" social units.

Since hypotheses 1 and 2 relate two different x-variables to the same y-variable, it was necessary to specify their joint impact on the y-variable and on one another. For this purpose we distinguished two types of cases: convergent ones, in which congruence and consonance both are high or low (above or below specified thresholds) and divergent ones, in which one is high and the other low.

For the convergent cases, strict corollaries are deducible from hypotheses 1 and 2:

Corollary 1: Above a threshold equal to the highest specified for hypotheses 1.1, 1.2, 2.1, and 2.2, congruence and consonance will both be high and increase monotonically as a function of performance.

Corollary 2: If both congruence and consonance are low, performance will be low.

To state propositions about the far more interesting divergent cases, however, we deemed it advisable to weight the two x-variables relative to one another, rather than treating them as of equal significance. On various grounds, we considered congruence to be a more powerful variable than consonance and consequently derived the following propositions for the divergent cases:

- Hypothesis 3.1: The overall correlation between dissonance and low performance will be lower in the presence of congruence.
- Hypothesis 3.2: The overall correlation between consonance and high performance will be much lower in the presence of incongruence.
- Hypothesis 3.3: In sets of divergent cases, average performance will be closer to the levels expectable from congruence alone than those expectable from consonance alone.

Concepts and Operations: Authority Patterns

Investigating the project's hypotheses systematically required certain tools of inquiry. (1) We needed a set of concepts for describing variations in authority patterns, and these had to be applicable to authority relations in all kinds of social units. (2) We needed operational guidelines for research into authority relations, so that data relevant to the concepts could be found in a highly standardized manner. Although an advantage of the project was that it only required intracultural comparisons among different social units, this still presupposed standardized data-gathering procedures, and we at least hoped to be able to make informative cross-cultural comparisons on levels more specific than the notions of congruence and consonance themselves. (3) We also needed quantitative measures of the data: procedures for

scaling specific observations or responses to interviews and questionnaires, methods of aggregating observations and responses in regard to particular aspects of authority, and overall measures of congruence or consonance.

Developing these tools involved considerable labors. Although we made much progress in regard to them--enough to permit useful field research--a good deal of the work remains to be done and will continue to be a major concern in future.

The work was difficult intrinsically, but also because the whole area of authority relations was conceptually and operationally almost uncharted when our work began. Concepts for describing authority patterns had been developed mainly for the study of states and consequently were not fully applicable to all kind of social units. Moreover, they were, by and large, gross and rather ill-defined (as well as highly value-laden) global concepts, summarizing, usually only implicitly, a large number of dimensions of authority relations. At worst, they consisted of nebulously defined dichotomous distinctions, like that between "democratic" and "authoritarian" patterns. At best, they consisted of conceptual equipment like the widely-used distinction, developed by Lewin, Lippitt, and White, between three types of overall authority patterns (the "laissez-faire," the "participatory," and the "directive") varying on a small number of implicit dimensions (director's influence, subordinates' feelings of freedom, frequency of joint discussions, frequency of personal contact with directors, and subordinates' influence on the directors), or the formally similar distinction, developed by Dahl and Lindblom, between "hierarchic," "polyarchic," and "bargaining" patterns. The existing literature did provide some useful conceptual materials, but in the main we had to develop our conceptual and operational tools ourselves. We were,

to speak with some liberty, in a position comparable to chemists working without a previously developed periodic table or structural linguists equipped with only a very primitive scheme of phonemes.

Our first task, after appropriately defining and delimiting our subject matter, was to devise an "analytic scheme" for the description of authority patterns. "Analysis" in this case is used in the literal Greek sense of the term: the breaking down of complex phenomena into their basic components or elements. This was done by specifying dimensions on which authority patterns vary and how they may vary on the dimensions. Initially, the analytic scheme was not very systematically constructed: it was based on existing literature about authority relations, hunches about possible improvements, logical reflections on the implications of the definition of our subject matter, and some special research into concrete authority relations. The first outline of the scheme was subjected to intensively critical group discussions and revised as a result of these discussions. It was then applied in a kind of "simulated field research": specifically, we tried to see how readily the scheme could be used to conceptually chart the universes of authority presented in certain novels as well as sociological and anthropological case-studies. This led to further revisions, the results of which led to a document on the empirical study of authority relations to be used in fieldwork.

The dimensions of authority discussed in that document include the following, in outline:

1. Superordinate-subordinate relations:

- 1.1 Inequality dimensions

- 1.1.a. Distance

- 1.1.b. Department

1.2 Influence dimensions

1.2.a. Compliance

1.2.b. Permissiveness (or Directiveness)

1.2.c. Responsiveness

1.2.d. Participation

1.3 Proximity

1.4 Nature of Directives

(Now dropped, or subsumed to other dimensions.)

2. The Structure of Superordination:

2.1 Stratiformity

2.2 Intricacy

(2.1 and 2.2 are now collapsed in a single

dimension: conformation.)

2.3 Monocratism (or collegiality)

2.4 Headship

(Now dropped.)

2.5 Concordance

3. Superordinate Recruitment

4. Type of Legitimacy Beliefs

After developing a decent first approximation to the sort of analytic scheme ultimately wanted, our next task was to prepare the scheme for application in actual field research by devising a set of "operational manuals" for research into each dimension. These manuals are presently being collated in a large monograph on the empirical study of authority relations.

The operational manuals provide (a) working definitions of the dimensions and rules governing variations on them, (b) suggested sources of pertinent data (which differ among the dimensions and, overall, are deliberately varied and eclectic), (c) general questions for interview and questionnaire schedules, where applicable, and (d) preliminary instructions for scaling data. In actual field research the items had to be adapted to particular cultures and social units; the majority, however, proved serviceable widely enough to permit cross-cultural comparisons.

Concepts and Operations: Governmental Performance

To relate our independent to our dependent variable, similar conceptual and operational work had to be done on the notion of governmental performance. The criteria used to gauge governmental performance in existing work suffer, by and large, from one or another of three flaws that we have tried to avoid in our own work.

(a) Where precisely stated and readily measured, they tend to produce inane results, precisely because they were chosen for the sake of intrinsic precision and easy measurability. An example is Lipset's classification of any democracy as "stable" if it has existed since World War I and not had, over the most recent 25-year span, an antidemocratic movement receiving more than twenty percent of the popular vote; all others are classified as unstable. This implies that all democracies founded since World War I are unstable, even if they have never faltered, and that all democracies which have, over a recent generation, reduced to little or nothing a once large antidemocratic vote are unstable--among many similarly absurd conclusions.

(b) Less dubious criteria generally suffer from the opposite flaw: lack of precision and measurability. An example of this is J. H. Spiro's position that "successful" constitutional governments must be "stable" (i. e. durable), "adaptable" (i. e. capable of adjusting to their environments), "efficient" (i. e. able to act so as to solve their problems), and "effective" (i. e. acceptable to the citizenry). These criteria are verbally defined, but no ways of gauging their levels are specified, and even the verbal definitions are rather ambiguous. Except for the first criterion Spiro's standards obviously leave much scope for arbitrary judgments and are in fact only impressionistically applied by Spiro himself.

(c) A third problem is that political performance is often evaluated in terms of substantive values or goals, regardless of the values or goals of polities themselves. In one case the evaluative standard may be "democracy," in others economic development or the creation of an overriding national identity. This is perfectly acceptable, provided that one is frankly interested only in the values concerned, not in political performance in general, and is willing to concede that low performance may be due simply to lack of commitment to a value. It is also acceptable if one knows the substantive commitments of a polity in advance of evaluation, but this is never easy and sometimes scarcely possible, particularly if absolute intensities and relative priorities among values must be scaled. It would be acceptable as well if one could posit a general welfare function applicable to all societies, but all attempts to do so have foundered. Moreover, attainment of many substantive values may be hindered by unalterable environmental circumstances rather than shortcomings in political processes.

Our problem, therefore, was, first, to find a set of criteria that could be used to evaluate the performance of polities regardless of their substantive values--some set categorical political imperatives; and, second, to define them precisely and find ways to measure them accurately. Monographs on the first subject and reporting initial findings using the measures for eleven polities during two ten-year periods have been completed and are scheduled for early publication in the Sage Professional Papers in Comparative Politics. We used five imperatives as general criteria of performance:

- (a) Permeation: The extent to which a polity's directives can be carried out throughout the society to which they apply and the extent to which a polity is able to extract from a society resources needed to pursue its objectives.
- (b) Strife-avoidance: The extent to which polities are able to minimize violence among their members.
- (c) Legitimacy: The extent to which polities evoke positive commitments (as well as merely avoiding violence against political superordinates).
- (d) Efficacy: The extent to which a polity is able to arrive at policy decisions (directives) per se (i. e. avoid immobilisme), particularly decisions responsive to social demands on the polity or urgent environmental threats to it.
- (e) Durability: A polity's ability to persist through time. This we take to be governed by performance on the previous four dimensions, independently measurable and, in extreme operational straits, a useful but imperfect unique measure of overall performance.

The selection of the performance dimensions resulted from much reflection and discussion. However, the basic rationale behind each criterion can be sufficiently grasped through a simple mental operation appropriate to any position for which the support of logic is claimed: imagining whether the contrary of what is being asserted is equally tenable.

Project Organization: The Graduate Student Workshop

The organizational format of research followed in the project was designed to solve or reduce certain ubiquitous problems in comparative (i. e. cross-national) political study, perhaps in all cross-cultural research, which underlie some of the more serious weaknesses in contemporary comparative politics.

The most obvious difficulty in carrying out cross-national research is that it usually requires skills and resources individual scholars never possess. If, for example, one set out to test the congruence hypothesis at age thirty in only ten case-studies, allowing for each case a mere year for fieldwork and another for data-processing and writing, plus one or two years for project design and a summary presentation of the work, one would need academic leave during two out of every three years of one's working life, have no substantial time for keeping up with his field, and have to master at least four or five languages; even so, one is likely to have only a superficial knowledge, historical and cultural, of each case and to treat only a very limited number of social units in a rather small sample of societies. Some cross-cultural undertakings are less arduous, but others even more demanding.

The results are evident in the state of the field. Some practitioners simply opt for expertise in only one society or culture area. Many evolve frameworks and

propositions that others are invited to put to work, which rarely happens; the field consequently is overpopulated with still-born theoretical embryos. Some base research mainly on readily available aggregate statistics, however variously and unreliably compiled, however dubiously they indicate what is wished to be known, and however much that implies ignoring crucial problems that available aggregate statistics do not allow to be handled. Others put their trust in "data banks," over deposits in which they have little or no direct control. Still others engage in "secondary analysis" of other people's work, which generally is spotty from the point of view of one's own, always highly variable in quality, and always requires a lot of chancey reinterpretation.

It is widely believed that comparative politics is in a parlous state because its practitioners lack a "paradigm" or methodological sophistication. A simpler and less disrespectful explanation is that a proper format for performing cross-national research has not been devised or widely used.

Cross-national research, especially if it requires the generation of new data, clearly calls for collective research of some kind in place of isolated, self-dependent study. A good deal of such research has recently been carried out, but in ways that pose problems of their own.

One device used for group research in politics is the committee (standing or ad hoc) of fairly like-minded senior scholars, on the lines of the SSRC's Comparative Politics Committee. This has obvious advantages over purely individual work, but also manifest shortcomings. (a) It is expensive. (b) The potential sponsoring organizations are few. (c) The participants being scattered, they can rarely work together continuously over appreciable periods. (d) The participants

being senior men, they will rarely have highly similar interests and views or accept close project direction, and coordinating their work will involve much wasteful wrangling. (e) The participants will rarely wish to engage in the more boring and mundane aspects of field study. In consequence, their group researches are themselves likely to yield mainly untested frameworks and loosely integrated essay-collections. The present "Smaller European Democracies" project, organized by Dahl and Rokkan, may prove an exception, but to date experience supports this conclusion.

An alternative avoiding this difficulty is cross-national research designed by a single individual or team of close collaborators and actually carried out by hired research assistants and/or survey organizations in different societies, in the manner of the Civic Culture project. This is better, but still flawed in important ways. (a) The project directors are rarely in a position to develop a design well-adapted to all of the societies studied, unless the number is very small. (b) Research employees are rarely of high quality and research organizations are generally limited in range of research capabilities; moreover their commitment to a project is generally low. (c) The project generally yields none of the knowledge-in-depth and theoretical spin-off that qualified scholars acquire and produce in personal field study. (d) Only highly routinized data, e. g. the sort yielded by closed-ended questionnaires, are obtainable; other potentially rich data-sources, especially those calling for nimble, imaginative interpretation (from raw observation to the sifting of relevant documentary materials) are perforce neglected.

How then could the advantages of individual study, especially by country experts, be wed to those of group research, and the disadvantages of both be

minimized? The solution, abstractly, must be some sort of close-knit team of qualified scholars, each taking responsibility for a segment of a common, detailed project design (perhaps assisted by directly controlled research employees) and each subject to workable central direction or coordination. The graduate student workshop format used in the project, was our practical solution of the problem, and had the potential of reducing as well a number of problems in political science teaching, not least that of teaching and research being done at cross-purposes. The following sketches summarily its nature and rationale, as we originally conceived it.

(1) A graduate student workshop operates, in broad outlines, as follows:

A relatively broad subject (like the present project) on the frontiers of knowledge in a field is chosen. Under the close direction of one or more faculty members a limited and carefully selected number of graduate students work on the subject from different angles and by looking at different cases, but using a common framework of study (problems, concepts, hypotheses) formulated by the workshop directors, to some extent in collaboration with the students, so that their products constitute a coherent, cumulative body of knowledge. Unlike seminar work, participation in a workshop is not confined to an academic term but a continuing relationship among the members. A typical student's participation takes the following form:

- a. The student enters the workshop after having had one year of graduate work, and spends a term being instructed in the literature on the subject and the operations of the workshop. This period of instruction leads to a paper setting forth and defending a project design within the broader field of the workshop.

- b. During the second term the student participates more informally in the workshop, refining his project design in close collaboration with the workshop directors, and reading and discussing the work of other workshop members. By the end of the year, he has a very detailed project outline that is highly congruent with others' researches.
- c. In the following year, if the project directors think the student can make a useful contribution to the group, the student does field research on his project with funds provided by the workshop. In doing his research, the student continues to be closely supervised in order to help overcome unfamiliar research problems and assure that his work continues to be relevant to the workshop. The workshop director might join him briefly in the field, if the expense of doing so is not too great. Or the student might return to the university in mid-research for consultation and reporting, both to the workshop director and new workshop members, who would thereby observe the process of research in progress and learn to anticipate problems. There is also constant reporting in correspondence.
- d. Having done his main research, the student spends a year writing up his findings, again with constant supervision. These normally constitute his doctoral dissertation, but some of the year could also be devoted to articles or other writing for publication. As the writing proceeds, reports on it are periodically given in the workshop and discussed there. This helps the student, and helps instruct new members of the workshop.

The workshop director's (or directors') functions are:

- a. To devise, and if necessary revise, the overall workshop project.
- b. To select students for membership in the workshop at all stages.
- c. To closely direct and participate in their work.
- d. To teach new members and preside over workshop discussions.
- e. To do administrative work connected with the workshop.
- f. To do research on the workshop project himself, and publish in the area, sometimes as a collaborator with other workshop members.
- g. To write a summary volume on the overall findings of the workshop.

Workshops have a tangible personality. That is, there is a special workshop room or rooms, for reading, discussions and informal meetings, a library of basic materials and workshop papers, and desks for students' work. A secretary-typist is usually available, and the workshop provides specially needed facilities, such as means for recording discussions and multigraph machines. Research assistance can also be provided.

The number of students is hard to specify, but to permit close direction (verging on collaboration) and corporate esprit, should probably involve an intake of only about three or four per annum. This adds up to about ten students on the premises and in the field during any single year.

(2) The workshop is designed to satisfy at once a considerable number of ends--
one of its principal advantages. Summarily, these are:

- a. Advanced teaching on the graduate level and the improvement of such teaching where it is now weakest: after "residence." Training and supervision are continued to the advanced research stage where currently students get only a minimum of help and training, if any at all.
- b. The production of coherent, cumulative published knowledge in difficult problem areas, especially when wide ranges of comparable data are required, by the gradual buildup of data banks and the concerted testing of theories in fields where work by isolated scholars is not likely to be closely related.
- c. The relatively economical production of data and theories, since graduate students are currently much underemployed, especially in the sense of their work being scattered and too little directed.
- d. The spreading out of drudgery in doing the routine work involved in any large-scale project, especially in data-gathering, which in fact only rarely gets done.
- e. The combining of general theoretical concerns with detailed research in special countries and/or areas, overcoming the most vexing problem of how to integrate "comparative" and "configurative" study by country experts.

(3) Much of the case for workshops is apparent from its various purposes.

The following considerations should, however, be particularly stressed:

- a. At present, graduate training stops at too superficial a level and rarely includes close guidance in creative research, which ought to be its sine qua non. Seminars too often tend to be glorified survey courses or discussions of only very loosely related student papers. Rarely do they introduce students to the unexplored frontiers of his field, or provide intimate intellectual contact with senior scholars, or permit the student to see a senior scholar at his work. This is the main teaching case for workshops.
- b. Many important problems, especially on the frontiers of disciplines, are beyond the time or powers of single scholars, and unlikely to be handled in a coherent and cumulative manner if scholars are separated by institutional and departmental affiliations. As in the natural sciences, such problems are best handled by groups of co-workers in which responsibilities are carefully allocated and integrated.
- c. Graduate student time in the social sciences is now wastefully employed in many cases. Most graduate students now get much financial support for what are mainly scattered, non-cumulative and often untutored dissertations. Hence, on a cost-benefit basis, workshops are likely to yield better results than isolated researches by senior men and especially by degree candidates.

- d. The teaching and research functions in the social sciences are now insufficiently combined and workshops can bring them together. Research funds for faculty members rarely have any direct return in instruction, the student's graduate work rarely any direct research results. Furthermore, many of the best teachers and most productive scholars now particularly get research support that subtracts from their availability as teachers; in some universities, indeed, the best men do the least instructing. To improve the quality and increase the quantity of training, they should be made more useful in the instruction process, especially in view of the now envisaged acute shortage of university instructors. But it would be unfortunate if this were done at the direct expense of research. The workshop thus is a useful device for combining what is now largely separated, so that neither research nor instruction will be promoted at the other's expense.

While the early operation of the workshop turned up some unanticipated problems, these have been minor and manageable, and did not impugn its essential research (or teaching) rationale: to combine cross-national study with field study in depth by country experts commanding relevant language skills, historical knowledge, and cultural empathy.

The Project's History

(1) Prior to the recruitment of the first cohort of project members, the basic design of the project was developed by the Workshop Director, Harry Eckstein. This was subsequently much added to and modified, and is still, for reasons previously mentioned, in some respects incomplete.

(2) The recruitment of the first cohort of project members produced twelve applicants out of sixteen pre-generals students in comparative politics at the university; this dispelled a major uncertainty about the project: whether political science students, unused to group research, would be willing to do their dissertation work under the constraints of such research. With ARPA's agreement a number somewhat larger than that originally budgeted for (seven) was admitted to the project, on the assumption that drop-outs (which did not occur) or subsequently reduced recruitment would permit us to stay within envisaged personnel limits. Two additional students chose to join the project without financial support, and have in fact done their research along its lines.

The first cohort of students was trained in the project during 1966-67, contributed to its overall design, and developed designs for their own fieldwork. During 1967-68, they carried out the fieldwork in the following countries:

- Sweden
- Holland
- The Philippines
- Tunisia
- Spain
- Germany
- France
- Colombia (not directly financed)
- Mexico (not directly financed)

A mid-year conference of project members was held in December 1967. All project members returned from the field by December 1968, and they have since been engaged in processing their data and in writing their dissertations, two of which were completed in 1969.

Most of the projects worked out according to plan. However, one project (Spain) had to be dropped because of a combination of local access problems and personal difficulties by the researcher; one not financed by us (Colombia) had to be

done hurriedly and inadequately because the student, a Colombian supported by the Institute of International Education, had to work to a very constricted schedule; and fieldwork was only recently completed in the other unsupported project (Mexico) because of delay resulting from a motor accident. In addition, the work of two members was somewhat delayed (but not impaired), in one case because of difficulties with a draft board, in the other by illness.

(3) A second (smaller) group of members entered the project in 1967 and did fieldwork during 1968-69 in French-Canada and Italy, with a third member postponing fieldwork (in Yugoslavia) until 1970-71. (Projects that were dropped, for the reason previously mentioned, included research in Japan and Uganda.) During 1969-70 a group of three additional students prepared for fieldwork: one in English-speaking Canada (now completed) and two in Brazil (still in progress). Funds for the Yugoslav and Brazilian projects, both of which are obviously sensitive politically, were allocated by Princeton.

(4) Finally, using local research assistance, we compiled a considerable amount of data on the performance of politics and surveyed and summarized much of the existing literature dealing to any significant extent with non-governmental authority patterns. We produced a monograph on one part of the latter work as well: a survey of research on authority in industrial establishments, the best work having been done in that area.

(5) The project output to date thus includes: one journal publication, two forthcoming monograph publications, five technical reports, two completed doctoral dissertations, six dissertations in progress, and three or four further dissertations in the research stage.

Work on the projects by both directors and students continues and a "final" report, in the true sense, will emerge, in all probability, during 1971-72, as envisaged in our original application to ARPA. We cannot yet clearly envisage the result, but it is most likely to be a step in continuing, and somewhat redefining, work, rather than a conclusive theoretical product--although a large advance toward understanding the conditions of governmental performance and ways to gauge its level with precision, hence also to intelligent policy-making requiring assessment of the actual and probable future performance of governments and strategies for raising its level.

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