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James N. Rosenau; Philip M. Burgess; Charles F. Hermann

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The Adaptation of Foreign Policy Research

A Case Study of an Anti-Case Study Project

JAMES N. ROSENAU, PHILIP M. BURGESS, and
CHARLES F. HERMANN
Ohio State University

Like everything else, foreign affairs has been buffeted by the tides of change and the swirl of conflicting currents. It has been overwhelmed by the knowledge explosion, buffeted by the implosion of financial support, challenged by those who cry for "relevance," and reduced to a sideshow status by those who have just discovered that serious inequities exist with respect to the domestic distribution of values.

Knowledge in the social sciences—including the development of varied analytical and methodological techniques—has expanded so rapidly that the individual scholar, isolated in a laboratory or library, is less and less likely to lead us to the intellectual and scientific breakthroughs and to the disciplined and multimethodological inquiries that are so essential to progress (Deutsch et al., 1971). Increasingly, social and behavioral scientists engaged in foreign policy research are finding that the problems they seek to explore exceed the talents and expertise that any one man can hope to acquire in a reasonable period of time. Hence, the emphasis on "team" research efforts and concern for inventing social mechanisms to facilitate collaborative work both within and among universities.[1]

1. We recognize that there are limitations to group problem-solving and creativity, and thus we certainly do not mean to imply that scholars can only be productive when they gather around a table. Rather, our position corresponds to one

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At the same time, resources to support programmatic team research efforts—especially those in the area of foreign affairs research—have become more scarce than ever. First, the resources available to the universities to sponsor research centers through which a "critical mass" of theoretical and methodological talent can be brought together have been drastically reduced, owing to the impact of a troubled economy and the reallocation of existing resources to meet the demands of troubled students. Consequently, the support that large-scale collaborative efforts require is very difficult to secure internally. Second, support for programmatic team research efforts in foreign affairs is difficult to secure through external funding sources, owing to the reallocation of attention and resources to "domestic" problems—a process presaged by major North American foundations in the mid-sixties and now being followed by major government sponsors of basic research in the social sciences, most notably the National Science Foundation. It is one of the ironies of an era that has devoted substantial talent and money to the study of the future, that nearly all "futurists" have identified the management of political processes in the international environment as the most critical to our survival, yet the agents of society are increasingly turning their backs on research aimed at social processes in the international arena.[2]

Just as theoretical advances, data bank expansion, and increased analytical power from the social statisticians, math modelers, and computer specialists have increased the foreign affairs analyst's capacity to depict trends, simulate both systems and processes, grasp basic social and organizational processes, and provide better information bases and policy appraisal methodologies, so have the funds necessary to exercise

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of the major findings of Deutsch et al. (1971: 456): "Teams of social scientists seem likely to be the main source of major advances during the next decade, but individual social scientists operating in the traditional 'great man' or 'lone wolf' styles will continue to be a significant, though secondary, source of new ideas."

2. Indeed, as Platt (1969) has pointed out, when we examine problems over both the short and long term, some are clearly being overstudied from the point of view of national science policy. Yet foreign area and weapons management problems remain among the most critical unresolved problems we face.

and further develop this competence become scarce. The result is that many individual researchers are unable to augment basic library research and their intuitive feel for international politics at the very time that the facilities and know-how that could give direction to their library searches and discipline to their intuition have become available. The computer has made it possible to analyze problems that heretofore had to be dismissed as unresearchable, but the finances necessary to generate and explore the data reflecting the problems are now very hard to secure. Some data banks, to be sure, have emerged, but their base is narrow and their capacity to expand falls far short of the potential need. Furthermore, even if the data banks were adequate, their full utilization would be constrained by the shortage of funds in many institutions that makes nearly impossible the sustained cooperative effort that is necessary to take full advantage of the new research technology. Moreover, data base expansion requires scholars getting together to refine the theoretical models and research designs that are necessary to guide data acquisition. Yet travel funds are skimpy and even postage for large mailings of mimeographed materials is not infrequently difficult to find.

It must be stressed that the funding dilemma described above is not likely to be temporary. As already indicated, both the federal government and the major foundations have not only drastically reduced their investment in foreign affairs research, but the decisions through which they did so appear to be based on new policies rather than momentary expedients (Black, 1970). If anything, support for foreign affairs research is likely to dwindle further, thus compelling researchers either to return to traditional forms of inquiry or to invent new ways of cooperatively pooling resources that are available.

Given these circumstances, it might prove helpful to recount how one group of some ten quantitatively inclined political scientists has sought to adapt creatively to the scientific opportunities and financial limitations presently inherent in conducting sustained foreign affairs research. Unlike other team efforts that have recently been recounted (Hilton, 1971; Singer, 1972), the participants in our project were not located on the

same campus. Thus ours has been an unusual research experience, one that had not been elaborately planned in advance and that may prove to accomplish more through its residual effects than its main product. Designating ourselves as the Inter-University Comparative Foreign Policy Project (ICFP), we have for more than five years managed to sustain a self-conscious effort to offset the noncumulative, atheoretical, case-oriented study of foreign policy conducted by isolated scholars working alone. The effort has been pursued mainly through a series of meetings devoted to the development of an integrated research design to guide extensive and continuing cross-national—that is, genuinely comparative—empirical inquiry into the foreign behavior of nations.

That which follows is not intended to make a case for the theoretical or methodological commitments that have been made by those involved in the ICFP. Those judgments will have to be made by others as they judge the research output and appraise its scientific merit and policy relevance. Rather the purpose of briefly reviewing the history of the ICFP here is to suggest ways in which cooperative research can be pursued. Bleak as the present research environment is, and difficult as collaboration among some ten scholars can be, the ICFP managed to persist, and we hope that therein lies some guidance for others. We do not maintain that ours is the only—or even the best—mechanism for sustaining a cooperative research effort. Indeed, as will be seen, lately signs have appeared that the collaboration has run its natural course and that the ICFP may some day disband. Nevertheless, we do submit that the ICFP model is one that "worked"—one that developed internal decision rules that are necessary to sustain continued interaction and one that facilitated the process of obtaining external research support. While the members of the ICFP may eventually go their separate ways, what follows is a story of gathering momentum, of collaborative research that has been able to evoke increasing support from a shrinking research subsidy pool simply—or perhaps largely—because the scientific and institutional necessity for sustained collaboration has become so widely appreciated.

Early Beginnings

The first, exploratory conference of the ICFP was convened for two full days at Rutgers University in April 1967 under the sponsorship of a textbook publisher.[3] At this meeting, the ten participants[4] decided to resist the invitation to produce a series of paperback texts on foreign policy that could be used individually or jointly. Although persuaded that the field could benefit from the availability of genuinely comparative texts, it was soon agreed that the theoretical and empirical base necessary for a qualitative step-level change from existing instructional materials did not presently exist.

Thus, the first ICFP decision involved a commitment of energies to research rather than to the preparation of a textbook. This decision was accompanied by a tentative agreement to collaborate on empirical research to be organized around a pretheoretical framework set forth by Rosenau (1966). Derived from a dichotomization of three basic parameters (physical size, economic development, and political accountability), that framework identified eight basic "genotypic" societies or "nation types" that could serve as the basis for comparison.[5] Moreover, in an initial burst of enthusiasm for comparative inquiry that was not constrained by time and

3. The generous support of the early efforts of the ICFP group by the Charles Merrill Publishing Company and its Political Science Editor, Roger Ratliff, is gratefully acknowledged.

4. In addition to the three authors of this paper, the participants in the first ICFP meeting were Professors Maurice East of the University of Kentucky, David Finlay of the University of Oregon, William Fleming of New York University, Ole Holsti of the University of British Columbia, Michael O'Leary of Syracuse University, and David Wurfel of the University of Windsor. The membership of the group subsequently varied and has included Robert D. Burrowes of New York University, John Gillespie of Indiana University, James Harf of Ohio State University, Margaret G. Hermann of Ohio State University, Gary D. Hoggard of American University, Roy E. Licklider of Rutgers University, Patrick McGowan of Syracuse University, David Moore of the U.S. Military Academy, Donald Munton of Ohio State University, Charles A. Powell of the University of Southern California, Stephen A. Salmore of Rutgers University, and Dina Zinnes of Indiana University.

5. The eight genotypic societies were labeled as follows: large-developed-open, large-undeveloped-open, large-developed-closed, large-undeveloped-closed, small-developed-open, small-undeveloped-open, small-developed-closed, and small-undeveloped-closed. The conceptual bases for deriving these eight types are outlined in Rosenau (1966) and a discussion of the problems of empirically delineating them can be found in Burgess (1970).

place, each participant initially agreed to conduct an intensive empirical investigation of the foreign policy behavior of four national societies, two representatives of one of the eight genotypes and two of another. This research strategy appeared to provide the opportunity to develop a common data base for forty societies that would allow us to make comparisons both within and between the genotypic categories, a procedure that would facilitate the testing and subsequent elaboration and modification of empirical relationships suggested by the original framework. The initial euphoria about the possibility of immediate payoff from such a strategy, however, did not endure. A number of the participants had prior and pressing commitments, and, as we tried to develop concrete research plans, it was increasingly apparent that the Rosenau framework remained in a very preliminary stage of formulation. These realizations restored reality, and the first conference ended with an agreement to reconvene at a time when the elaboration of the research design was more adequate and when the participants had discharged competing obligations and commitments.

Nonetheless, we left New Brunswick with the belief that we had established the basis for the development of a common research design with which nearly all of us could live. We did not recognize at the time that our efforts to divide the labor, with each participant analyzing commonly defined variables in four different societies, constituted our first attempt to construct explicit decision rules to guide the collaborative research. It took repeated interactions for us to become fully aware that in addition to the further substantive definition of our task, we needed to search for procedural guidelines that would be congenial to the group and at the same time would foster substantial information exchange among scholars located at geographically dispersed locations. Indeed, one important dimension of the history of the ICFP is the continuous effort—with occasional false starts and some failures—to formulate decision rules to guide our collaboration.

Further Development

Nearly two years later, in the spring of 1969, Rosenau circulated a substantial elaboration of the original "pretheories" model. The new version proposed treating foreign policy as part of the adaptive behavior of national societies—as that part of a society's behavior which it directs toward its external environment in order to keep fluctuations in its essential social, economic, and political structures within acceptable limits—and proposed a series of operational definitions through which such behavior could be observed and analyzed.[6] Accordingly, the ICFP, minus one original member but enlarged by four new ones, gathered again at Rutgers to assess the progress that each participant had made during the two intervening years and to determine whether a point had been reached that would allow active research collaboration to begin. The second conference, lasting nearly three days, was partially supported through a research project administered from Yale by Professor Bruce Russett.

Although the second round of deliberations yielded the clear-cut commitment to move ahead with research on a collaborative basis, it also gave rise to an increasing awareness that the identification, elaboration, and measurement of variables in the revised analytic scheme was not an issue easily resolved and that the development of measurable, comparable, and meaningful units of foreign policy behavior was a conceptual and empirical problem of considerable magnitude. Indeed, by the conclusion of the second Rutgers conference, the strategy of building a data base for forty countries had lost whatever appeal it had once had. The task of mechanically applying the framework to four societies struck every participant as too constricting and lacking in opportunities for the pursuit of unexpected results. More important, the framework itself no longer seemed so readily amenable to empirical research. Discussion of the specific operational indicators that

6. Although the original draft was circulated in mimeo, it has since been published in a revised form (Rosenau, 1970).

could be derived from the analytic scheme brought forth considerable disagreement. By the end of the conference, agreement was reached on a number of independent variables that were to be used. Agreement on others was facilitated by resort to a "panel" technique that involved postconference balloting among participants.

It should be noted that experimentation with the panel method constituted another ICFP mechanism for establishing decision rules for collaborative research. After preliminary discussion of a variable or a group of variables during the conference, one member was assigned the task of developing several alternative indicators which were to be circulated to the group. Participants voted for those operational definitions they preferred and those indicators receiving a majority were to be used by everyone. Of course, each participant retained the right to use additional measures in his own research, but these would be supplementary to those constituting the common core.[7]

By comparison, however, the problems posed by the independent variables were relatively easy to resolve. The issue most constraining to subsequent research proved to be our inability to make progress in solving the problem of identifying, classifying, and operationalizing types of foreign policy behavior—the dependent variable to be subjected to inquiry. The original Rosenau framework did not specify a basic output unit and instead referred simply to "foreign policy" as the behavior that was to be observed, measured, and interpreted as responsive to the variables that differentiated the eight genotypes and to individual, governmental, societal, and systemic factors—the so-called "source variables." The little time that was reserved at the second Rutgers conference for the specification of the

7. Shortly after the results of the balloting became known, we recognized that a formal majority procedure failed to provide a workable decision rule for the identification of variables and their measurement. In some cases, the operational indicators proposed were not feasible for the countries that interested some members. Had we been together when each of us was constructing his assigned set of indicators, this problem would have emerged immediately. The balloting, however, proved to be an important exercise, providing a disciplined form of communication when we were apart. Furthermore, some of the proposals developed by members in their ballots have remained as integral components of the collective design of our work.

dependent variables did not prove sufficient for the task. The need to adjourn arrived without any conclusions on the issues, not even an agreement on a mechanism for facilitating a solution to it. While all concerned felt that considerable progress had been made, they realized that much remained to be done.

The second Rutgers conference also revealed some disagreement over Rosenau's theoretical extensions in which he suggested that foreign policy be examined as a form of the "adaptive behavior" of national societies. This suggestion necessitates viewing foreign policy acts (however classified and measured) as intervening rather than dependent variables. Moreover, changes and fluctuations in the essential structures of societies are seen as at least partially reflecting feedback from a nation's foreign behavior. Some participants viewed the suggestion as a useful way of linking foreign and domestic politics, whereas others felt that the suggestion was premature and would only confound the effort to classify and explore foreign policy behavior itself. All agreed, however, that further exploration of the utility of the adaptation model depended on conceptual elaboration and the development of measurement routines through empirical work.

The Third and Fourth Conferences

Notwithstanding the difficulties and disagreements that surfaced at the second Rutgers conference, the participants lost none of their enthusiasm for the ICFP experiment. The problems encountered seemed miniscule in comparison with the conviction that continued collaboration was leading us toward genuinely comparative analysis and explicit movement between theory and a strategy most likely to result in cumulative research. Thus, an invitation to Hermann to organize a panel at the annual meeting of the American Political Science Association (APSA) in September 1969 was seen by the group as an opportunity to sustain its momentum. At the second Rutgers

conference, in April 1969, every member of the group anticipated, rightly as it turned out, that his university would shoulder the expense of attending the APSA meetings in New York City four months later. Accordingly, a decision was made to use the panel invitation as an occasion to move the project forward and then to assess the progress through a third conference to be convened in New York immediately following the APSA meetings. Everyone agreed to prepare for the panel a research design that was derived from some aspect of the original or revised framework, one that was consistent with measurement decisions that had been reached (through mailed ballots) by the time of the New York conference. Those who were unable to develop a design would serve as discussants at the panel.

Because of the large number of papers to be considered at the panel, the participants agreed to make their papers available one month in advance of the political science meetings. The preliminary schedule of the APSA program announced that papers for this panel should be read before the session and that they could be obtained after August 1 for the cost of postage from the Center for International Studies at Princeton. Each author supplied thirty copies of his paper, but the center received more than twice that many requests. It was evident at the panel in September that at least some of the unusually large audience had indeed read the papers.

Nine research designs were presented at the APSA panel, and two days later, following the adjournment of the APSA meetings, the ICFP convened for the third time. The papers revealed considerable variability in the aspects of foreign policy that interested the members of the group (see Appendix), but nevertheless they were all consistent with the earlier agreements and the overall commitment to genuinely comparative analysis. The deliberations of the third conference were dominated by the realization that a crucial juncture had been reached, that the time had come to confront data in a more sustained and systematic way, and that further collaboration must await an initial attempt to gather and analyze some of the empirical

materials anticipated by the research designs. Indeed, aside from a spirited discussion of the need to generate data consistent with measurement decisions on the dependent variables (i.e., foreign policy behavior), the New York conference was adjourned one day early. Two days of deliberations had been planned, but by the middle of the first afternoon it was clear that further progress was dependent on the acquisition and analysis of data. Accordingly, an early adjournment was preceded by a discussion of further plans and three decisions on how to carry them out. One involved an invitation to Rosenau to organize a comparative foreign policy panel at the annual meeting of the International Studies Association (ISA) in Pittsburgh in April 1970. Since it again seemed feasible for each participant to obtain travel support from his university to attend the ISA meeting, we decided to accept the invitation and to use the occasion for the fourth ICFP conference.

A second decision made in New York was perhaps even more important to the development of the ICFP. For the first time, the group discussed and agreed upon criteria for excluding participation in its work. Convinced that theoretical and empirical progress could not be sustained without forward movement in the generation and analysis of data, we agreed that the Pittsburgh meeting would serve as a test of commitment to the ICFP, that only those who actually subjected some part of their previously developed research designs to an empirical test and reported the results to the ISA panel would be encouraged to continue as participants in the group. Although this condition regrettably resulted in the loss of two members, it proved valuable as a means for maintaining the momentum of collaboration.

At the New York meeting we made a third decision, one that, as things turned out, was the most crucial of all. Anticipating that commitments to the ICFP would deepen as a consequence of the process of gathering data to support the papers presented to the ISA panel, we concluded that progress beyond Pittsburgh required more sustained interaction than was possible in two-day conferences. Thus, assuming a successful research yield

at Pittsburgh, we decided that the tasks of revising the adaptation framework in light of the empirical findings, and of then accumulating and analyzing the data called for by the revised framework, required deliberations and resources that could not be pieced together from each participant's institutional affiliations. A minimum of four weeks of continued interaction with each other and with data seemed necessary to consolidate the empirical progress and the theoretical problems that were expected to surface in Pittsburgh. Accordingly, in New York we resolved to seek financial support for an extended session of the ICFP in the summer of 1970. Details as to the time, place, duration, and concrete research activity were to be developed at Pittsburgh in the event support was obtained. Indeed, the performance of the participants at the ISA panel loomed so crucial as a turning point that the decision to seek support for an extended session in the summer and the decision to hold such a session if support could be obtained were made independently. All concerned realized that Pittsburgh might not yield justification for continued collaborative research; thus the possibility of rejecting funds and terminating the ICFP had to be left open.

Obviously, however, efforts to find summer research support could not await the outcome at Pittsburgh. If an extended session were to occur in the summer of 1970, those involved had to plan on it well before the April meeting of ISA. Summer plans could be changed if it proved advisable to terminate the ICFP experiment, but the task of finding support could not be postponed until after a go-ahead decision was made in Pittsburgh. Consequently, the fall of 1969 was devoted to fund-raising efforts which, happily, proved successful. Explicitly recognizing that the ICFP had already compiled a record of sustained collaboration, the University of Michigan, through its Voluntary International Cooperation Project (VIC), agreed to provide the travel and subsistence funds necessary for a four-week session, and the Inter-University Consortium for Political Research at Michigan agreed to make programming and computer resources available in the event the ICFP decided to

spend the summer in Ann Arbor. In addition, in order to ensure that the session be more than a reworking of old data, VIC provided funds for four members of ICFP to gather the "event data" that their APSA research designs required.[8]

The Pittsburgh panel exceeded all expectations. Six papers were given, and they all contained data that were consistent with the earlier designs (see Appendix), thus moving the ICFP from the abstract world of pretheory and design problems to one enriched by empirical distributions and stimulated by findings that were sometimes expected, sometimes unexpected, and sometimes confounding. Some of the data were admittedly crude, many of the findings tentative, but the ICFP had nevertheless clearly moved beyond the pretheoretical and design stage. Although the several papers covered a wide variety of issues, they all reflected the commitment of the ICFP members to a common theoretical orientation. Agreement on measurement routines proved to be more difficult to achieve at the research level, but the Pittsburgh papers clearly made it possible to envision the eventual refinement of the adaptation model through a sustained and systematic confrontation of empirical materials. Accordingly, it was decided at the meeting of the ICFP following the ISA panel that the VIC offer of support for the four-week summer session was a challenge that should be accepted. The dates were set for August 3-28 to accommodate those who had teaching commitments. The essential purpose of the occasion was articulated by the decision to follow the lead of others (Clubb, 1969; Rokkan, 1969), and call the four-week session a "data confrontation seminar" (DCS). Because the work of several members involved considerable reliance on event data, it was also decided to invite participation in the DCS by a key person in the World Event Interaction Survey (WEIS) project headed by Charles McClelland at the University of

8. It should be noted that these four members (East, Hermann, McGowan, and Salmore), convened for a day and a half in New Brunswick in February 1970. As part of the VIC-supported effort, an attempt was made to integrate, or at least coordinate, their decision rules for coding event data. While this "third" Rutgers conference did not result in the development of a common coding scheme, it did lead to a convergence around certain key categories that will subsequently facilitate partial merging of the data sets.

Southern California. The invitation went to Gary D. Hoggard and was accepted.

A discussion of the scope of the DCS led to a commitment at Pittsburgh to ensure that the collective effort of the four weeks would have a central focus and cumulative impact while at the same time allowing each member of the group to pursue his individual research interests, so long as they were constrained by the common theoretical commitments that had already been made. After some discussion, agreement was reached to spend August exploring the utility of the adaptation framework within any one of four data-based research foci, summarized as follows:

(1) *Nation types.* The empirical and theoretical implications of classifying nations according to genotypic groups defined by properties such as size, level of economic development, and political accountability.

(2) *Source or input variables.* The relative potency or impact of various *domestic factors* (e.g., bargaining among governmental units, the volatility and impact of public opinion, or the distribution of domestic political power among a nation's elite), *international factors* (considerations as different as regional political stability and a nation's vulnerability to international trade or financial fluctuations), and *individual factors* (owing to differences among national leaders and the constraints on their behavior that flow from such diverse sources as the history of the nation and the idiosyncratic needs of individual leaders) on the external behavior of nations.

(3) *Outputs measurement.* Investigation of the problems associated with examining policy outputs, activity, or behavior, including the development of competing measurement routines for guiding data collection that can be tested against each other and against the genotypic and source variables, thereby clarifying the theoretical, methodological, and empirical issues that characterize contemporary problems in scaling and classifying policy.

(4) *Component (input/output) relationships.* Some combination of any three of the foregoing foci to enable examination of the relationships that obtain among the major clusters in the adaptation framework, particularly the ability of the genotypes and the source clusters to explain variance in or to predict to policy output measures.

The final decision made at Pittsburgh concerned the locale of the DCS. Since two members of the group, Hermann and

Rosenau, had in the interim decided to move permanently to Ohio State,[9] and since Burgess was able to assure the group that the Behavioral Sciences Laboratory he headed at Ohio State would be able to arrange the necessary housing, provide programming expertise, computer facilities, and data archiving, it was no longer self-evident that Michigan should be the research site for the DCS. On the contrary, after a lengthy discussion, a number of reasons were adduced that led to a decision to hold the DCS in Columbus rather than Ann Arbor. Burgess, Hermann, and Rosenau were charged with the responsibility for organizing an agenda for the DCS. Burgess took on the additional task of directing staff efforts–ranging from making local living arrangements to providing the technical support staff and data and program archives–that were necessary to four weeks of sustained research and interaction.[10] In short, the Pittsburgh meeting of the ICFP lasted only a few hours, but in that brief period landmark decisions were reached insofar as the continued collaboration of the group was concerned.

The funding of the DCS clearly made the ICFP something of a Cinderella at a time when resources for foreign policy research were and continue to be otherwise scarce. For that reason it is important to emphasize the meager support that sustained this collaborative effort during its first three years. In addition to whatever funds individual members secured from their universities (primarily for attending professional meetings), the ICFP as an entity received a total of $8,400 from three different sources across 36 months. Recalling that the group consisted of ten individuals (actually there were more at some points), the project received the equivalent of $280 per person per year. Those funds primarily sponsored the conferences and four pilot

9. Although the convergence of three members of the original group on the same campus occurred for a variety of personal as well as professional reasons, it is not far-fetched to conclude that the prior experience of collaboration in the ICFP was one of the reasons.

10. In addition to the financial support provided by VIC and data acquired through the Inter-University Consortium for Political Research at the University of Michigan, the costs of the DCS were also subvented by three groups at the Ohio State University–namely, the Mershon Center for Education in National Security, the Behavioral Sciences Laboratory, and the Polimetrics Laboratory in the Department of Political Science. The support from all these sources is gratefully acknowledged.

data collection efforts. Because the intellectual and group development of the ICFP took time, vastly greater financial support from its inception probably would not have eliminated certain aspects of this developmental process. We have little doubt, however, that, under more favorable funding conditions, our progress would have been more rapid than it actually was. For example, problems inherent in measurement decisions that were made could have been detected more quickly (such as those that evolved from the mail balloting); talented people who felt that they could not tolerate the slow and uncertain pace of the ICFP might not have been lost; and some of the data problems that were recognized as early as the second year of the project might have been tackled far more vigorously.

The Data Confrontation Seminar

This is not the place for a day-to-day account of the DCS. The purpose of this report is to record how a group of some ten scholars managed to sustain a collaborative and cumulative research effort in a period of dwindling support for the study of international affairs. Thus it is sufficient to confine ourselves here to a brief review of the accomplishments and failures of the DCS, noting the main problems that arose and the consequences of the intensive four-week experience for the continuation of ICFP research.

The DCS began with a self-conscious effort to delimit the extent and scope of the interaction among the participants. Formal discussion meetings of the entire group were scheduled for four mornings each week, but it was agreed at the outset that such meetings should last no more than two hours, that they would begin and end promptly, and that they would be immediately adjourned if none of the participants had a substantive problem to present to the group. The goal here was to maximize fruitful colleague confrontation while at the same time allowing ample time for each participant to confront data. It was felt—correctly, as things turned out—that collective

deliberations could not be productive unless all concerned had time for private study and informal interaction. Thus, in the early days of the DCS, the formal sessions were devoted to presentations and discussions of the research designs and data the participants expected to confront, while during the last week they were devoted to reports on the outcomes of data explorations. In the interim, the days were filled with seemingly endless contests with the computer, frequent interchanges with programmers, spontaneous informal meetings on particular methodological and substantive problems, and never-ending dialogues that extended into the car pool, the lunch hour, the evenings, and even the tennis court. If it cannot be said that the DCS participants lived, breathed, and slept comparative foreign policy for four weeks, that is only because most of them decided to extend the experiences into a fifth week and remained in Columbus a few extra days.

No less salient in retrospect is the range of substantive concerns around which the sometimes frantic activity of the DCS was organized. Not only did the designs presented at the outset cover a wide variety of phenomena and range across all four of the research foci noted above, but the deliberations were also enriched by a number of new formulations generated during the process of daily interaction. The "master file" of the DCS is filled with memos proposing reconceptualization of variables, measurement proposals, outlines of models tracing newly identified causal linkages, and commentaries on the appropriate methodology for the solution of particular research problems. Among other things, for example, the discussions led one participant to frame a mathematical model of adaptation and stimulated another to circulate a lengthy analysis of alternative schemes for classifying nations.

Yet problems abounded. Colleague confrontations were far more numerous than data confrontations. Notwithstanding a creative and hard-working staff of programmers and research assistants, the DCS was nearly half over before the data necessary to explore the research designs began to become available. One on-line data set previously compiled at the

Behavioral Sciences Laboratory (Burgess, 1970) did make it possible from the outset to check out certain kinds of empirical "realities" when disputes over them arose in discussion, but the many data sets thought to be relevant to test the propositions advanced in the research designs were not so readily accessible. They had been archived earlier by the Behavioral Sciences Laboratory, but the participants in the DCS had not anticipated their empirical needs sufficiently to permit the prior preparation of programs that would allow for analyses across data sets. Valuable time in the early days of the DCS thus had to be spent merging data sets and regrouping variables drawn from different data.

Once the full array of data became available for analysis, moreover, an even more severe problem arose. Although eighteen data sets had been collected and catalogued (Munton and Youra, 1970), it turned out that they fell far short of being appropriate to the theoretical needs of the conferees. All concerned managed to adjust their research designs to make them consistent with the data on hand, but there could be no escaping the inadequacy of the data available to international relations scholars relative to the adaptation model in general and the interests of the participants in particular. This is one reason that colleague confrontation occurred for more often than data confrontation. Indeed, much of the colleague confrontation consisted of presentations of propositions or findings which, in turn, evoked criticism that either the available data were inappropriate for testing the propositions or that the data used were insufficient to support the findings. For a brief moment one colleague even got so caught up in confronting his theory with data that he had to be reminded that the data he used to measure his independent variable reflected events that occurred after those he used for his dependent variable. It required colleague confrontation to identify the unhappy fact that there was no way in which any of the existing data sets could be used to test his model.

Perhaps it seems surprising that an appreciation of the inadequacy of presently available data constitutes one major outcome of the DCS. It could be argued that this is obvious and

should have been recognized at the outset. To a certain extent, of course, some sense of the discrepancy between theory and data did exist prior to the DCS. The effort to generate new event data arising out of the "third" Rutgers Conference (see note 8) manifested a dissatisfaction with the limitations inherent in existing data. For the most part, however, it must be conceded that a full appreciation of the gap between theory and data developed *during* rather than before the DCS. It would seem that the frustration of ten colleagues collectively trying to explore phenomena had to be experienced at first hand before clarity emerged as to the extent of the data gaps in the field.

The frustration with respect to data had a number of sources. As the participants knew before the DCS began, little data exist on foreign policy actions, except for the WEIS data and that which the participants had begun to collect for themselves. Nor are there cross-national data collections on the perceptions or attitudes of policy makers. These known constraints loomed even larger as the seminar continued. Moreover, the merging of variables from different data sets was often obstructed by the lack of comparability among country code numbers and in the time period for which each data set had been collected. Finally, everyone in the DCS came to recognize the need for genuinely longitudinal data in order to trace the sources of foreign policy and then the consequences—or feedback—of these same policies on the processes of societal adaptation. Even the few available data sets that at first blush appeared to be longitudinal turned out to be discontinuous, with large time intervals between the points for which the data were gathered.

Thus, in retrospect, it is *very* clear that many more data sets, spanning long and identical periods of time, and involving perceptions as well as actions and attributes, need to be developed if the questions that evoke interest in the comparative study of foreign policy are ever to be answered. This is not simply a call for more data. It is rather a plea for specific types of data, the need for which grew out of a comparative approach to empirically defined foreign policy problems that members of the ICFP tediously and painfully hammered out over an extended period of time.

Subsequent Developments

The DCS proved to be the high point of the ICFP. Thereafter, the various members began to go their separate ways and to become increasingly tardy in fulfilling their ICFP commitments. While it might be concluded that the ensuing record of dilatoriness stemmed from too much colleague confrontation at the DCS, a more accurate explanation would seem to be that extensive collaboration across several universities had been carried as far as it could go, given the lack of adequate data and theory that could serve as stimuli to sustained interaction.

At the time, of course, no one knew that the DCS was at a turning point. Although dismayed by the enormity of the research tasks uncovered by the DCS experience, the participants in the ICFP left Columbus determined to carry on. All agreed that much had been gained through the extensive colleague confrontation and the newly won appreciation of the limits of presently available theory and data. Accordingly, in the last session of the DCS, commitments were made to work on the problem of generating more appropriate data while simultaneously agreeing that the time had come for the ICFP to get feedback and appraisal from a wider range of colleagues. From the outset, it had been recognized that the ultimate test of this venture in cross-university collaboration lay, not in the ability to sustain collaboration, but in the quality of the intellectual and scientific outcomes that result from collaboration. Thus, at the concluding session of the DCS, an invitation to Burgess to organize a session in conjunction with the 1971 Annual Meeting of the ISA in San Juan was accepted and viewed as an occasion for compelling completion of the work begun in the DCS. At the same time, it was also decided to bring together in book form all the papers, both methodological and substantive, that had emerged from continuous collaboration and that were to reach final fruition at San Juan.

Although the San Juan session of the ICFP occurred some six months after the close of the DCS, it fell short of successful completion of the collaborative endeavor. Several new papers

were presented and non-ICFP members participated in critiquing them, but at the same time some in the group, each for his own good reasons, did not finish their papers. Hence, postponement–plus renewed commitment stemming from a sense of group obligation–was the order of the day in San Juan. All concerned resolved not only to complete and revise their own papers in time for the September 1971 meeting of the APSA in Chicago, but also to provide a detailed critique of one other paper. In this way, each participant would be assured of feedback that would enable him to revise his effort for a final time, and the group would be assured that in Chicago the makings of a collaborative volume would be available.

By the time of the Chicago meeting, one further member had circulated a paper and only a few critiques had been prepared. All the participants, however, reported their papers were in progress and the commitment to the volume of essays was reaffirmed. Accordingly, the group selected three of its members to serve as an editorial board that would decide the merit of papers submitted by November 1, 1971. Although this deadline also proved unmanageable for a couple of the participants, by the time of the Dallas meeting of the ISA in March 1972, the editorial board was able to report to the eighth session of the ICFP that the papers for the volume had been selected (see Appendix) and that, collectively, they added up to a work that would provide a significant record of the intellectual efforts of the ICFP.

The slowness of the group to complete their joint publication effort may have been symptomatic of the declining cohesion of the ICFP as a collaborative research effort. Other signs of perhaps greater long-run significance emerged to suggest that the group had developed as far as it could in its present form. In contrast to earlier stages of the collaboration, the preparation of the articles for the volume did not lead any of the contributors to urge the group to consider new areas of joint endeavor. Moreover, the resulting research products cannot be described as collectively constituting a major breakthrough in the theoretical paradigm that had been the substantive focus of the

undertaking. The resulting volume seems likely to possess a greater coherence than many edited volumes, but it also appears destined to lack integration around a tightly defined problem in comparative foreign policy. Finally, signs of the limits of collaboration emerged in the resistance of some contributors to respond to the critiques that their colleagues provided.

Conclusion

Although it is difficult to determine at this writing whether the Dallas session of the ICFP will be its last, certainly the initial impetus that brought it into being will have been spent.[11] The volume is completed and finding a publisher for it is all that remains to be done.[12] In the end, of course, this volume should be viewed as the prime yardstick against which to measure the ICFP. Whatever contribution the volume may make, however, it should not be seen as the only measure of the ICFP's efforts. There are a number of residual effects of the collaboration that can already be discerned. One is that virtually all the participants have carried back their collective experience to their own campuses and are presently conducting research and training students, undergraduates as well as graduate students, in ways that they could not have employed without participation in the ICFP. Another is that several members maintained interuniversity collaboration and are involved in a mini-ICFP that should eventually yield a vast new data base with which to compare the several genotypes in a variety of issue-areas. Still another is that participation in the ICFP by scholars from two nearby universities (Indiana and Ohio State) led directly to a series of informal exchanges between a larger number of political

11. Subsequent to the preparation of this account, support for a three day, "where-do-we-go-from-here" conference of ICFP members was procured, with the generous help of Professor Ross Berkes, from the School of Politics and International Relations, University of Southern California, so it may well be that the initial inputs of the ICFP will be replaced by a "second wind" that takes it off in new and innovative directions.

12. This has since been accomplished: *Comparing Foreign Policies: Theories, Findings, Methods* is scheduled to appear in late 1973 or early 1974 under the imprint of Sage Publications.

scientists in the two schools. A fourth consequence is that several members of the ICFP have assumed leadership in the creation of a Section on Comparative Foreign policy within the ISA. A fifth consequence is that three ICFP members collaborated in the preparation of a booklet of materials on comparative foreign policy for use in college courses. Whether all these activities might have ensued without the ICFP is difficult to judge, but it does not appear unreasonable to assume that the existence of the ICFP reinforced and encouraged their development. Viewed in terms of such spinoffs as these, it might even be argued that the ICFP faltered toward the end because of its own success, that members had difficulty meeting their original commitments to the group because the collaboration led them to identify more clearly their individual research interests in the field.

So, unlike many case studies, ours cannot reach a clear-cut set of conclusions. That a group of like-minded colleagues can adapt their common research interests to a geographically scattered and financially barren environment over a five-year period has been demonstrated by the ICFP, and we would argue that such a demonstration is of some use in the present world of converging concerns and scarce resources. That the ICFP has been successful by scientific standards, however, remains to be demonstrated. We are confident that it will prove to be the source of a useful body of knowledge (as well as the focus of a case study), but we are aware that thus far only evidence for the latter is available. All of the participants have had their talents, vision, and understanding enlarged by the ICFP—and so have a number of graduate students in several universities who have been involved in one way or another—but it remains to be seen whether the benefits will accrue more directly to the larger community of researchers in the foreign policy field.

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APPENDIX

(This appendix, along with the second and sixth entries in the list of references, contains all the papers written through 1971 under the auspices of the Inter-University Comparative Foreign Policy Project. Those items marked with [a] were presented at the American Political Science Association meetings in September 1969. Items marked with [b] were presented at the International Studies Association meetings in April 1970. Items marked with [c] are scheduled to appear in the published volume of ICFP papers. This list includes several papers prepared by students associated with members of the ICFP and includes two doctoral dissertations. A copy of any unpublished item in this bibliography can be obtained for the cost of photocopying by writing to ICFP, Behavioral Sciences Laboratory, 404 B 17th Avenue, Ohio State university, Columbus, Ohio 43210).

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