THE ROOTS OF POLITICAL ACTION:
PROGRESS AND PROBLEMS IN A CROSS-NATIONAL RESEARCH PROJECT

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The most important reason in favor of cross-national research in the social sciences applies to scientific research in any field: talent and resources are widely distributed across national boundaries. Consequently the contained development of an international community of research scholars is a vital goal: the failure to attain it will almost surely result in a relatively inefficient use of available talent and resources, and a relatively slower pace in the progress of knowledge. As a discipline reaches maturity, the major advances tend to reflect the contributions of investigators from numerous countries. The breaking of the genetic code, the recent breakthrough in demonstrating the existence and functioning of the quark, built on the work of scientists from Berkeley to Calcutta. To date, this pattern has been more evident in the natural sciences (and even in some branches of the humanities) than in the social sciences. Apart from the field of economics, examples of successful cross national collaboration in the social sciences remain rare. This fact is both a reflection of, and a reason for, the relative immaturity of the social sciences. In order to function effectively, an international community of social scientists must share (1) a common body of theory that helps identify the key problems to be solved at a given point in time; and (2) a common set of techniques and scientific standards by which the relevant hypotheses can be confirmed or disconfirmed. In both respects, the development of an international research community in the social sciences has been hindered by special circumstances. On one hand, the existence of ideological differences has sometimes retarded convergence toward a common theoretical paradigm.

That the natural sciences are not wholly immune to such problems was made evident in the case of the Lysenko affair. But the political implications of social science research tend to be more direct and obvious than those of the natural sciences, with a consequent tendency for scholars of given nations to favor given analytic approaches. The dissemination and use of a common body of techniques across national boundaries has been hindered by historical events which, in the aftermath of World War II, gave the United States a commanding lead in the development of certain techniques such a survey research. The two problems compounded each other, for to some extent the use of such techniques has been seen as implying the acceptance of a form of American cultural imperialism. Finally, it is sometimes argued that cross national social science research is unfeasible because each culture carries a unique world view: only an observer born and bred within a given society can fully

appreciate the workings of that society. Stood on its head, of course, this becomes one of the strongest arguments for collaborative cross-national research: it implies that the joint efforts of investigators from different countries are necessary in order to identify and understand what is unique to a given culture and what (if anything) different cultures hold in common. Nevertheless, these factors have all combined to retard the development of an international community of social science researchers.

This paper will report on the successes and failures of an ongoing cross national research project in which each stage of the investigation--from the basic research design to the search for funding, the fieldwork, and through the analysis and interpretation of results was carried out by a team of scholars from eight nations. Substantial difficulties were encountered, as well as certain failures, and I will try to describe them frankly and openly in the paper, in the belief that we have much to learn from our failures. On the other hand, one of the most encouraging conclusions I would draw from our experience is that cross national collaboration in empirical social science research today is indeed feasible. and highly rewarding. This project was designed to investigate the factors leading to political participation, with a focus on both conventional and unconventional forms of political action, including protest behavior. We undertook to analyze the social and ideological bases of such behavior, seeking to understand how feelings of absolute and relative deprivation can be generated by the perceived failure of government to lead up to expectations, and how these expectations themselves may vary in terms of the value priorities of given generation units. In order to do so, our basic sampling design called for interviews with representative national cross-sections of the citizens 16 years and older within each country (N=1, 200-2, 300), supplemented by a subset of parent-youth pairs from the same family, in each country (N=200-260), obtained to permit more refined analyses of intergenerational differences and continuities. The project mobilized the efforts of social scientists from eight nations. They are (in the order in which field-work was completed): Great Britain, the Federal Republic of Germany, The Netherlands, Austria, the United States, Finland, Switzerland and Italy.[1] The results seem to indicate that--at least as far as European-American interaction is

[1] The scholars involved are: Mark Abrams, Klaus Allerbeck, Samuel Barnes, Anselm Eder, Cees de Graaf, David Handley, Felix Heunks, M. Kent Jennings, Max Kaase, Barbara Farah, Henry Kerr, Hans-Dieter Klingemann, Alberto Marradi, Alan Marsh, David Metheson, Warren Miller, Pertti Pesonen, Leopold Rosenmayr, Giacomo Sani, Risto Sankiaho, Dusan Sidjanski, Charles Roig and Philip Stouthard.

concerned--collaborative cross national survey research has come of age. Though the initial proposal for this research originated from the United States, somewhere along the way the center of gravity shifted from North America to Europe. The basic research design was developed in a series of international conferences held in Brussels (April 1971), Geneva (June 1971), London (December 1971) and Ann Arbor (February 1972). Pilot studies were then carried out and the results discussed at Mannheim (April 1972), Cologne (September 1972) and Montreal (August 1973), where a core questionnaire was agreed upon. Fieldwork took place from late 1973 through 1975, in the respective countries. Cross-national analyses were planned at conferences in Bellagio (May, 1974), and in a three-week Data Confrontation seminar at Mannheim (July-August 1975) where a combined cross-national data set was prepared containing our basic scales, indices and other constructed variables. Key variables, including the Political Action scales constituting the dependent variables for our first volume, were primarily developed by European participants.

This five-nation data set will be made available to the social science community at large in January, 1978, through the Zentralarchiv für Empirische Sozialforschung in Cologne. Preliminary working papers were discussed and criticized, and plans for further analyses adopted, at meetings in Oisterwijk, The Netherlands, and Edinburgh (both in August, 1976). Finally, the first draft of an initial cross-national volume was completed in August, 1977, utilizing data from the first five nations to complete fieldwork. Preparation is underway for two additional volumes based on data from all eight nations. These two volumes will focus on Political Socialization effects; and on Social Structure and Political Cleavages. In addition, a number of articles and a book-length single nation study have been published; and several additional articles and books are being prepared.

Most of these conferences were supported by funds raised by the investigators from the host country, drawing on local sources.[2] Similarly the fieldwork and the analyses carried out in each nation were supported by funds raised locally by the investigators in the given country.

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2. The exceptions were the conferences at Montreal and Edinburgh, which took place in the context of IPSA Congresses; and the meeting at Bellagio, which was supported in part by the Rockefeller Foundation. At the initial meeting in local expenses were covered by the European Community; apart from this, each participant paid his own way.

In view of the substantial costs involved and the increasing difficulty in funding research from American sources, this broad cross-national sharing of research support represents a significant achievement. Mobilizing the efforts of social scientists from a number of different countries to work on a common set of questions, using common techniques, may be of even greater significance. For I believe that this project has made an incremental but perceptible contribution to the evolution of an international community of social scientists. It could not have taken place if there were not already a certain measure of consensus concerning what are theoretically important questions to ask, and the appropriate means to obtain an answer. But it has also brought gains in both areas. It has even led to a surprising degree of convergence in the interpretation of results, between scholars who sometimes seemed to be in diametric disagreement on key questions. The manuscript that has just been completed, under the editorship of Barnes and Kaase, reflects this convergence, I believe. And it also reflects the relative balance of intellectual input: well over half the pages were written by the various European investigators.

The process of convergence took time. As is evident from the chronology just sketched out, more than six years elapsed between the initial discussion of basic research design and the completion of the first cross-national book length manuscript; it is expected that nearly two more years will elapse before completion of the two additional eight-nation volumes. It seems entirely possible that a study financed and executed by scholars from a single center might have been completed more quickly. It is uncertain whether they would have obtained the stimulation resulting from intense debate (and, sometimes, quiet discussions) between scholars with different backgrounds and perspectives, as well as an intimate knowledge of several different social systems. And it seems highly unlikely that it would have brought about anything like the multilateral exchange of techniques and ideas that took place in this project. For while there was general agreement that analysis of the roots of conventional and unconventional political action constituted an important and significant focus of research, there was considerable disagreement concerning various hypotheses about what led to it--

disagreement that was debated in the meetings of this group and, in some cases, led to publications representing the differing interpretations.[3] But there was consensus concerning appropriate ways to test the respective hypotheses-- and in this respect, it seems clear, European survey research has come of age. In discussing how to operationalize and measure the variables that concerned us, there was a two-way flow of technical expertise. I must concede that I learned a great deal from my European colleagues, not only about the dynamics of their own societies, but about recent advances in survey research techniques. I suspect that the same would be said by the other American investigators.

The process of convergence was facilitated by a long series of international conferences where we came to grasp each other's ideas so that, even when disagreeing, we were not talking past each other. These conferences were supplemented by a rather voluminous exchange of working papers; and by a series of bilateral exchanges. Klingemann, Marsh, Kaase and Handley each visited Ann Arbor for periods ranging from three months to one year; Barnes spent six months in London, Inglehart spent several months in Geneva; and there were numerous relatively brief bilateral exchanges involving the various European groups. These exchanges took place in pursuit of shared theoretical interests.

One of the key decisions made in this project was to base the division of labor, in analyzing results, on analytic topic rather than on a country-by-country basis. Accordingly, the investigators participated in working groups that cut across nationalities and were focused on such topics as the measurement of: ideological levels, types of political action, value priorities, intergenerational socialization effects, relative deprivation effects and the role of social class. Although fieldwork in each country was supervised by the respective national groups, data from both the pilot studies and the

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3 See Alan Marsh, "The 'Silent Revolution,' Value Priorities and the Quality of Life in Britain," American Political Science Review 69, 2 (March, 1975), 21-30; and Inglehart, "Values, Objective Needs and Subjective Satisfaction Among Western Publics," Comparative Political Studies 9, 4 (January, 1977) 429-458; and Marsh, Protest and Political Consciousness (Beverly Hills: Sage, 1977).

main body of surveys were distributed to each center and analyzed cross-nationally.

Through these bilateral and multilateral exchanges, a large measure of convergence took place concerning both techniques and interpretations. There was plenty of debate during the entire course of the project but as data became available with which to test various propositions, it became possible to move a good deal of the way toward consensus as, I believe, the volume edited by Barnes and Kaase will make evident. Though such debate takes time, it was probably of great value to the investigation for it forces one to sharpen and specify one's hypotheses. The testing of alternative hypotheses about value change, for example, led to the development of indicators and scales that were seen as appropriate ways to measure given concepts, permitting hypothesis-testing which, in turn, led to refinement of the original hypotheses and also to further exploration, such as investigating the relationship between values and subjective satisfaction.

In sum, apart from whatever value our substantive findings may have, this investigation has, I think, made progress toward the development of an international community of social scientists sharing a common culture in terms of theory and practice. Needless to say, this was a process that was underway long before this project was launched and that still has a long way to go.

At this point it seems appropriate to comment on one of the most important failures that took place in the course of this project, for it helps put the topic of convergence into perspective. For both theoretical and substantive reasons we hoped to include France among the countries in which this investigation would take place. Some leading French social scientists were contacted and took part in the first several meetings of

the group. After the Mannheim meeting in 1972, they dropped out on grounds that they were not able to obtain funding for their part of the project. Lack of funding is certainly a compelling reason for not participating, and it may be that this was the dominant consideration. Informal conversations with our French colleagues suggest, however, that it was also, in part, a question of motivation: their interest in the project was not strong enough to induce them to continue seeking funds in the face of initial negative indications from funding sources (which are almost exclusively governmental, in the case of France). Furthermore, we were unable to interest other French social scientists in the project. In itself there is nothing particularly surprising about this situation: perhaps the project was not inherently significant or well designed. On the other hand, this investigations did seem interesting and promising to social scientists from most neighboring countries, and it may be that part of the problem was specifically French. One possible factor is linguistic. At the meetings our French colleagues attended, the French participants usually, though not always, spoke French and the other participants usually, though not always, spoke English. Virtually everyone was able to understand both languages with little difficulty, and it was emphasized that both languages could be used. I have attended conferences sponsored by the European Community, the OECD and the IPSA where such arrangements functioned quite adequately. Nevertheless, there was some tendency for the group to consist of a Francophone minority and an English-speaking majority (most of whom spoke English as a second language). It was not simply a question of speaking the same language literally but also, to some extent, of sharing a common social science culture. I have the impression that French social scientists tend to limit themselves

to the literature available in French to a far greater extent than German, Scandanavian, Dutch or Italian social scientists limit themselves to the literature in their own native languages. American social scientists are probably a good deal worse on this score than the French--but the failing costs them less, simply because a very large proportion of the social science literature is originally published in English. It would be premature to speak of an international community of social scientists as already existing, full-blown. It may be emerging, but its attainment will require--and deserves--greater efforts to bridge barriers not only of language, but more subtle ones of scientific culture. There are no easy solutions but I think our group might have done more to integrate the Francophone participants into the project.

In taking stock of this project's shortcomings, I believe that another crucial difficulty arose from a pervasive tendency for technical and administrative problems to drive intellectual questions off the agenda. The former are relatively concrete, specific, easily identified. The latter can rarely be solved here and now, and usually got postponed. To some extent, this tendency was mitigated by delays in funding, which provided more time than originally anticipated for debate and reflection on our research design. But the tendency was present at all stages, including our Data Confrontation Seminar in 1975. Jennings and Farah have already provided an excellent account of this crucial event in the project, so I will only discuss it briefly.[4] The data confrontation seminar at Mannheim was intended to provide an opportunity for (1) constructing and validating

[4] See M. Kent Jennings and Barbara Farah, "Continuities in Comparative Research Strategies: The Mannheim Data Confrontation Seminar," Social Science Information 16, 2: 231-249.

scales and indices of our key variables; and (2) carrying out a first wave of analyses and discussing their results. In fact, the first week of this seminar was completely absorbed by a crash program of recoding the given national data sets to conform to a standardized format that had been agreed upon in advance--but which the respective national groups had failed to comply with in various ways. Most of the remaining time was taken up in creating and validating our constructed variables--a task that was, quite properly, a joint effort. An amazing amount of work was accomplished by a group of eighteen people working ten or twelve hour days almost continuously for three weeks; I agree with Jennings and Farah that the data confrontation seminar was a success; indeed I would recommend it as an essential part of any future collaborative cross-national project. The fact remains that its full potential was not realized; a few days were available for joint analysis and discussion of results, but we had considerably less time for cross-national feedback than we had planned on.

One reason for the imperfect success of this seminar was inadequate coordination of the respective national groups. To some extent this problem was inherent in the structure of the project: in keeping with our policy of full equality between all national groups, there was no central headquarters. In the various conferences, the group from the host country made arrangements and sent out communications concerning that conference. But in preparation for the DCS, a good deal of routine recoding and formatting was to be done by the group responsible for fieldwork in each country. Insufficient coordination at this phase cost us heavily when we reached the DCS. Similarly there were some minor inconsistencies in questionnaire wording from one country to another. It would probably be advisable, in future projects of this kind, to accept (and fund) a somewhat greater degree of centralization.

Another problem mentioned by Jennings and Farah is that in cross-national collaborative research, those who have done extensive research on a given topic may carry excessive influence in discussions of how to approach that topic: they have thought it through, their arguments are well rehearsed, they have data at their fingertips and they may intimidate other members of the group from expressing their own views. I would agree with this argument--for the imput of a variety of ideas and the stimulus of opposing perspectives was unquestionably one of the greatest assets this project had. At the same time, I would point out that the opposite danger also exists: on scientific questions, the majority is not necessarily right. The process used to decide what went into the questionnaire in this project was to allow lengthy debate but ultimately to take a vote. I can recall cases where we adopted formulations that I was convinced were not optimal; but it is difficult to conceive of an alternative decision process that would be workable. Together with the advantages of many contending viewpoints goes an increased possibility that one's own preferences will not be adopted. On the whole, I would say that the benefits clearly outweighed the drawbacks.

Finally, in drawing up the list of this project's shortcomings, I must mention one serious inequity. Each national group sought funding independently, and experienced delays of various lengths. Although Swiss, Finnish and Italian colleagues contributed to the research design and participated in the pilot tests and questionnaire formulation, they are not included among the authors of the first cross-national volume. For in order to avoid excessive delays in a project that has already taken several years, it was agreed that only those groups that had a complete machine-readable dataset ready in time for the data confrontation seminar at Mannheim would be

included in the first cross-national volume. The DCS was considered sufficiently crucial as to justify establishing a firm cutoff date. Due to delays in obtaining funding, our Swiss, Finnish and Italian colleagues were not able to meet this deadeline. Members of these groups participated in the DCS, nevertheless, aiding substantially in the tasks of recoding, index construction and preliminary analyses. The good grace with which they accepted the cutoff date and continued to work on this investigation testifies to what degree it had come to be viewed as a joint undertaking, of interest to us all. I hope their efforts will be recognized as fully in the second and third volumes resulting from this project as they deserve to be in the first.