

**Fifty Years of the National Election Studies:  
A Case Study in the History of “Big Social Science.”**

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Prepared for delivery at the Annual Meeting of the American Political Science Association, Atlanta, 1999.

Corrected version: 10/99.

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“Big science” conjures up images of massive centers of scientific activity furnished by the latest ultra-sensitive equipment, and scientists in lab coats flying in and out from all parts of the world. Big science, of course is massively expensive, although the \$300-million laser interferometer gravitational-wave observatory (LIGO), supported by the National Science Foundation (NSF) is, admittedly, on the high end. By contrast, the biggest big social science projects are considerably smaller, including, in the last major renewal of the “Big Three” NSF-supported social science resource projects, the roughly \$4 million each for the General Social Survey (GSS) and the National Election Studies (NES) and the approximately \$9 million for the Panel Study of Income Dynamics (PSID).<sup>1</sup>

It is no wonder that, with the economic shocks of recent years, the global appeal to “shrinking big government,” and declines in trust in major social institutions, public and private sector participants in the design of scientific institutions have been engaged in concerted study of past and possible futures of major scientific institutions, and reconsideration of their assumptions and goals.<sup>2</sup> But funding is by no means the only debate that has arisen; many important questions about the organization of science arise from this focused attention. Although the scale is dramatically different, budget questions have forced social scientists and their collaborators into similar discussions.

Big social science did not, of course, “just grow.” Its size, shape, and norms developed through processes of political negotiation; scientific debate, proposal, and revision; bureaucratic demands; and personal and collaborative creativity, trial and error. The nature of institutionalized and, eventually, partly nationalized social science has been the subject of formal exploration and debate numerous times during the course of the twentieth century, but most notably, with the creation of the Social Science Research Council (SSRC) in 1923, the debates in the late 1940s surrounding the founding of NSF in 1950, the periodic criticism of private and public funding of social science levied by members and committees of Congress beginning in the early 1950s, and periodic studies of the problem on the part of government agencies and private foundations from the late 1950s on.<sup>3</sup> These reports and transcripts are valuable documents for anyone interested in understanding the history -- and potential -- of contemporary social science. But beside these very broad, ranging debates and discussions, the assessment of specific case

studies of major research projects and programmes has much to offer also. There, we can see how the general issues hit the ground, and what happens when they do.

Growth and change in specific social science institutions, like that of other social institutions, is determined both by the impact of cultural, social, and material forces, opportunities and constraints that play upon them, and by the actions of key players. In explaining institutional development it would be too easy to fall into one of two polar traps. One is to trace a history of specific science institutions that is overdetermined by macro-level forces, ignoring the fact that social science depends on individuals developing, refining, and executing research ideas and, in many cases, seizing the opportunities provided by social forces in ways that other actors cannot or simply do not. The other is to define institutions of social science only as collectivities of individuals and their actions, thus rendering the story one of “great individuals,” agents who rise above any institutional and cultural constraint to shape the future.

This paper focuses on one particular case study of “big social science:” the National Election Studies, founded first as the particular data collection and research executed by a specific set of scholars at the University of Michigan in the late 1940s and early 1950s, and which, under the auspices of the National Science Foundation, was transformed into a “national social science resource” at the end of the 1970s and through the end of the century. This case study is worth attending to for a number of reasons. First, it was the first of the national social science resource data collection projects. It is the largest, most influential and widely used, and most hotly debated research project associated with the discipline of political science. Most importantly for analytical purposes, it reflects many key problems in the history of institutionalized social science and, given its particular history, it presents a rich opportunity for probing the joint impacts of social, institutional, and personal forces.

This case study proceeds in two steps. First, it offers a narrative history of the project from its inception in 1948 to the present time. Given its fifty year history, the narrative is necessarily very selective. It is framed by the analytical terms suggested by Pierre Bourdieu’s work on cultural production, which offers a theoretical means for understanding the different but overlapping impacts of social forces, institutions, and individuals. Second, it draws from that history a series of six dilemmas that have faced this project in particular, but which are representative of important dilemmas in “big social science” more generally. Each presents a trade-off in which there are critical costs and benefits no matter which path is

chosen; each merits serious discussion with regard to this – or any similar – “national resource” research project.

## **HISTORY**

### **How Can we Understand Individual Cases Theoretically?**

Many scholars have struggled to find theoretical and empirical means to understand the history of specific institutions as case studies of larger phenomena. To anticipate the narrative of NES a bit, it would be possible to understand the project as, in the main, a reflection of the larger forces operating in academia or its disciplines, science policy, and politics in post-war America. Each of those contribute to the story, but these major forces cannot explain why this particular project developed when, where, and in the manner it did. It surely could not pass over the signal influence of Warren E. Miller, the academic entrepreneur who not only led this project for the vast part of its history, but created or contributed to the creation of so many other related institutions (the Inter-University Consortium for Political Research, the Center for Political Studies, the Social Science History Association). At the same time, the history cannot accurately be told from a “once upon a time there lived a man...” point of view. But as numerous theorists of “agent-structure” problems have noted, parsing out the dynamic relationships, especially to serve as an adequate framework for empirical analysis, is one of the unsolved mysteries of contemporary social research.

This problem of giving full, nuanced due to agents and structures, especially in fields of “cultural production” -- where scholars have tended either to engage in “the glorification of ‘great individuals’, unique creators irreducible to any condition or conditioning” (Bourdieu 1993: 29) or in rendering the work as inevitable products of ongoing cultural norms or power structures – is one that has occupied much of sociologist Pierre Bourdieu’s work. He offers useful handles for this effort, especially in his work on arts productions:<sup>4</sup>

To understand the practices of writers and artists, and not least their products, entails understanding that they are the result of the meeting of two histories: the history of the positions they occupy and the history of their dispositions. Although position helps to shape dispositions, the latter, in so far as they are the product of independent conditions, have an existence and efficacy

of their own and can help to shape positions (1993:61).

Culture producers work not just within the larger societal field of power structures as, for example, marxist historians and some cultural studies scholars would have it, but also within specific fields of practice. Bourdieu uses the term field in a way that is akin to the way advanced scholars of academic disciplines use it: not as the denotation of stable categorical boundaries around a group of people (e.g. those in social science departments), but as a flexible and contested boundary around a set of practices based on certain norms underlying those practices. Bourdieu's method of analyzing cultural production underscores the importance of the fact that field norms and boundaries are contested within the field. Further, people engaged in cultural production within a field may be influenced by these norms and the conflicts among them, but the norms do not necessarily play important roles in their conscious intentions.

Bourdieu also specifies that norms within a field -- even in mainstream areas of the arts and, presumably, the sciences, can well be in at least partial conflict with the dominant norms of the larger society in which it operates. This claim challenges theories concluding that cultural production is ultimately merely the reflection of dominant social groups or their interests or other types of dominant social norms. For example, even if worth is generally values in money, in the arts popularity and commercial appeal is generally understood to relegate the cultural production to low status as art. The norms of a specific field do not buffer the artist from the more general norm (such as value by gain and fame), but only creates for the artist more multiple, often conflicting goals which affect the way the artist engages in his or her work. While Bourdieu's development of the notion of field revolved around his work on literature, it fits the case of social science well.

The field provides a structured distribution of positions that individuals may occupy, where positions denote, variously, those defined in relation to hierarchies and those defined in relation to differences of style and norms within the field, and thus may overlap. The positions within a field provide the range of recognized ways in individual may be integrated into it, and indicate the point at which the field can structure and give shape to those individuals' cultural production. Examples of some available positions in the field of art are "consecrated artist vs striving artist, novel vs. poetry, art for art's sake vs social art" (Johnson 1993: 16); those within social science might be positivist vs. interpretivist, survey researcher vs. experimentalist, basic research vs. applied research, survey research vs. polling. This list should suggest that a field as defined by its positions is structured but not entirely coherent and

compartmentalized; it is not just open to, but defined by the struggles and ambiguities among positions. And so, therefore, are the potential practices of the individuals within the field.

Individuals are not simply defined and structured by the systems in which they operate. Only the most staunch economic or cultural determinist could ignore the place of individual choice, strategy and, of course, creativity and inspiration in the arts and sciences. But, as Bourdieu's system reminds us, the options and strategies and even, perhaps, the range of creativity that can be produced, communicated, and received are not, in reality, unlimited or unconstrained. On the one hand, they are limited by social structures and fields in which they operate, positions with which they are framed. Tom Stoppard's (1993) play *Arcadia* illustrates this well. In 1809 the teenager Thomasina filled her notebooks with what could have been the invention of chaos theory had she had the right credentials (including, probably, the right gender and age) and lived in the late 20<sup>th</sup> century. As it was, it was perhaps poetry, but more likely nonsense. There were no other means -- positions -- by which to understand it. In the same play, the mistress of the house could set workers to the task of ripping out the classic, symmetrical 18<sup>th</sup> century gardens to replace them with carefully placed plants and a purpose-built hut and bridge designed to look old and "natural." In a style that, in the late 20<sup>th</sup> century, still has sufficient cultural positioning to make sense, this human-built design is understood as natural. As other theories argue, resources and power affect cultural production, but it is the available positions and range of field norms through which art – and science – can be realized.

Individuals' cultural production is shaped, on the other hand, by what Bourdieu calls their habitus. Habitus may be understood a set of personal dispositions and styles of thought and action, familiar in behavioral research, that are relatively stable and far from entirely self-conscious, that, together, form a kind of practical sense "that inclines agents to act and react in specific situations in a manner that is not always calculated and that is not simply a question of conscious obedience to rules. Rather, it ... generates practices and perceptions" (Johnson 1993: 5). An essential element in Bourdieu's theory, different from most behavioral approaches, is that although one's habitus is composed of predispositions which have an "independent life," as he put it, they are manifested through position taking. They gain effect through the structuring effects of positions, fields, and social institutions.

Thus, Bourdieu provides a means for trying to understand specific cases of cultural production

theoretically, facilitating integration of the individual case with others into more general analysis without lapsing into the determinism of either larger structures or individuals. Each piece of the story -- the structures and the agents -- have a history to be part of history. No analytical piece of the story is necessarily stable and unchanging. Each element can be both exogenous and endogenous in different respect.

Of course, this kind of theory does not facilitate “hypothesis testing” in any classic sense; logically, it is likely that only a system of counterfactuals can accomplish that in case studies (Hawthorne 1991, Tetlock and Belkin 1996). But if a case study is not just an unreflective narrative of events, such a theory can pose a hypothetical framework for exploring elements of the story, judging them by plausibility ill-fitting observations, and providing a means to build cases. In the case of the National Election Studies, then, this narrative emphasizes key actors, the state of behavioral political science and survey

### **A Historical Overview of the Michigan Election Studies<sup>5</sup>**

In 1948 Robert Kahn and Angus Campbell, of the brand-new Survey Research Center (SRC) at the University of Michigan, ran a small survey that was destined to become the first of a very long run of election studies. The study notes in the original codebook give a good flavor for how this enterprise was launched:

The study contains data from two nation-wide surveys conducted during October and November of 1948. The first of these surveys (s41) was not primarily concerned with the election; its major focus was on public attitudes regarding American foreign policy. However, in order to provide some measure of degree of political interest and of general political orientation, respondents were asked if they were going to vote in the 1948 presidential election and for what party they planned to vote. These two questions plus five concerning American foreign policy comprise the data from s41 included in the consortium's 1948 election study.

The second survey (s46) was undertaken in November after the presidential election had been held, and one of the objectives of the study was to gather data which would help illuminate some of the perplexities of the presidential vote. The study was designed to examine the characteristics

of Republican and Democratic voters, the importance of various national and international issues and the effects of the presidential campaigns on voting behavior. Interviewers asked respondents both closed and open-ended questions in both surveys.<sup>6</sup>

The first Michigan pre-election study consisted of a few questions; the unexpected presidential election of Harry S. Truman provoked these social scientists to delve further to find out what was going on in the minds of the voters who created this result. Perhaps more important than the content was one aspect of its methodology. Leslie Kish, one of a few scholars who came to the new research institute from the important Bureau of Agricultural Economics, brought with him his design for area probability sampling, which became standard operating procedure at Michigan, and became enshrined as the premier method of survey sampling. In the context of the well-known Literary Digest disaster in election prediction in 1936, Kish remained proud fifty years later that the little Michigan study had Truman as the winner.<sup>7</sup> On the basis of this study Campbell and Kahn wrote The People Elect a President (1952). At the time the other extant major survey study of elections was Lazarsfeld, Berelson, and Gaudet's The People's Choice (1948). Other than that, we would have to go back to Merriam and Gosnell's community study of nonvoting (1924).<sup>8</sup>

Angus Campbell was, at the same time, a member of the Social Science Research Council (SSRC) Committee on Public Behavior. A series of meetings culminated in a plan to do a major, national probability sample survey to study the 1952 election, sponsored by the SSRC, funded by the Carnegie Corporation, implemented through the University of Michigan Survey Research Center, and run by a planning committee consisting of David B. Truman (Chair), Conrad M. Arensberg, Alfred de Grazia, Oliver Garceau, V.O. Key, Avery Leiserson, and M. Brewster Smith, with the active ex officio membership of Pendleton Herring, the President of the SSRC. Given that the Michigan scholars emerged later so clearly as the center of intellectual gravity for these project, it is too easy to overlook the powerful formative role the SSRC Political Behavior Committee (1945-47, 1949-64) played through intellectual stimulation and assistance with seeking funding. In addition to the study, the SSRC sponsored a political behavior seminar held at the University of Michigan that drew in young scholars from around the country, including the future APSA presidents, Robert Lane and Heinz Eulau.

Two aspects of SSRC involvement at that stage were important. First, and most directly, this was



a distinguished interdisciplinary group of scholars, many of whom were making distinctive and varying contributions at the forefront of social science and political science theory and research. Second, and perhaps more important for linking this project into larger history of social science, the SSRC had been of signal importance to the development of American social science since its founding in 1923. Created, as it was, by the joint effort of Beardsley Rummler, the director of the Laura Spelman Rockefeller Memorial foundation and Charles E. Merriam, just past president of the American Political Science Association and chair of the important University of Chicago Department of Political Science, the SSRC stood at the center of efforts to create a vision and reality of a unified, cooperative, scientific social science that could help create the knowledge necessary to solve important social problems. Its ability to attract bright minds of social science continued, and its influence, tied to one of the powerful families of commerce, politics and philanthropy, was enormous (Fisher 1993). In 1945 it launched itself into the beginning of a new and intense debate over the state of social sciences by issuing a call for federal support of the social sciences. The SSRC remained central in the controversies that continued through the 1940s and 1950s first, with regard to whether the new National Science Foundation, founded in 1950, should include the social sciences in its mission (it didn't) and then whether a National Social Science Foundation should be founded. For scholars who came into the orbit of the SSRC, social science was not just a matter of individual scholars' research; it was a national, communal, collective endeavor. They were committed to advancing the scientific character of social science.

As for the Michigan scholars who were involved, most were not political scientists, including Campbell himself, Daniel Katz, Gerald Gurin, and Theodore Newcomb. This disciplinary configuration continued a pattern already in place in voting studies with the likes of Lazarsfeld and Berelson. The study of mass electoral behavior was a subject of cutting edge social science; it was not captured by the domain of political science. Political scientists were present too, of course. Even before the discussions regarding a potential 1952 study were underway, Campbell had brought a young graduate student from Syracuse University who had attended the SRC summer school to join ongoing discussions of political behavior and, ultimately the planning of the 1952 study: Warren E. Miller.

The 1952 study resulted in the publication of Angus Campbell, Gerald Gurin, and Warren E. Miller's The Voter Decides (1954), described by V.O. Key in the foreword as "the most impressive analysis yet made of a national election by the survey method" (Gurin and Miller 1954: 1). Key's

foreword remains a fascinating discussion of the current state and potential future of national sample survey studies. By this time Campbell and Miller were keenly interested in extending the election study series, which meant they immediately faced the problem of how to pay for them. There were not many private sources of funding for this kind of basic social science research and, while the Bureau of Agricultural Economics and a number of different defense-related agencies had sponsored survey research, there was not yet any federal agency that would take an interest in social science research per se, and certainly not this kind of non-policy work (Lyons 1969). The principals obtained internal Survey Research Center funds to mount a small study of the 1954 congressional elections, resulting in a small house-published monograph (Campbell and Hooper 1956).

By now, Ann Arbor had become the major site for intellectual activity based on survey research for a number of reasons. The formation of the Institute for Social Research and Survey Research Center in 1948 was based on a series of connections that brought in an exciting interdisciplinary group of social scientists with social psychology – itself a blossoming interdisciplinary field – at its core. These people were committed to pursuing a science of social research. Many had come from the Department of Agriculture and the wartime Office of Strategic Services (OSS), and were at the forefront of social science methodology.

The intellectual excitement of the SSRC seminars were also crucial to creating the Michigan base for the election studies. They added to the local resources both by bringing in roving bands of powerful minds. Warren Miller observed that these people came to Ann Arbor

in a broader context of a discipline that had no facilities to do survey research at all, to analyze survey data, And so this was for most of them the first experience they had with micro-analytic techniques. The counter/sorter was a marvelous machine of unfathomable complexity. .... It was clear that ... however talented and entrepreneurial they were, [they] nevertheless returned to institutions that had no infrastructure for them, no facilities, no access to any kind of technical/clerical personnel that made the collection of the data possible, or would make the analysis of the data possible (Interview, 7/20/97).

Survey research is not a game of solitaire. It must be collaborative enterprise, and it requires technical and mechanical (now electronic) resources. Finally, two other factors nested the election studies in Ann Arbor. One was the academic entrepreneurial interests and skills of Angus Campbell and Warren Miller.

Second, presumably, was a sheer force of path dependence. They organized one election study at Michigan, then another; that is where people began to expect they would happen.

The turning-point came in 1956, when the continued interest of the SSRC helped Campbell and Miller obtain funding from the Rockefeller Foundation. They, in turn, recruited two promising graduate students to join them: Philip E. Converse, who had abandoned his earlier studies in English to pursue a degree in social psychology at Michigan,<sup>9</sup> and Donald E. Stokes, pursuing a degree in political science at Yale. The resulting research reports offer the first real glimmer of the intellectual collaboration that came to be labeled the “Michigan school” of electoral research (Campbell and Miller 1957; Campbell and Stokes 1959; Stokes, Campbell, and Miller 1958). In this period they wove together their distinct research interests to formulate their influential model of the forces of partisanship, issue orientations, and candidate evaluation in electoral choice.<sup>10</sup> Crucially, this period marked the initiation of creative expansion of the research program that ultimately transformed it both into one of the most influential social science projects of the post-World-War II era.

The collaborators had already developed the assumption that they should and would seek means to continue the series of election studies for continuous monitoring and replication over subsequent elections. It quickly became clear to the participants that part of the value of the study would derive from the repetition of questions over time (Converse interview, 6/17/98). At the same time, the elaborations they attempted fundamentally altered its intellectual and methodological framework. First, they prepared for the first major panel study, even before they had secured funding for it, which ultimately included reinterviews of the 1956 sample during the 1958 congressional and 1960 presidential election studies. This panel design became a model emulated and further developed by many studies over the years. Second, Miller secured supplementary funding to interview the congressional candidates associated with a subsample of the congressional districts represented in the mass survey in 1958. These data, ultimately used for very influential works by Miller and Stokes on representation (e.g. Miller and Stokes 1963), was the first of many efforts to integrate the mass surveys of the election studies with other data, enhancing the analytical power of both. This integration of data sets has special importance for the mass election studies, because it allows the research to examine evidence “outside the voters’ heads;” an important move if electoral studies — even those taking approaches of political psychology — are to move empirically beyond the subjectivities of citizens to incorporate data that access the election in other ways.

A third development of that period was the initiation of efforts to extend the project comparatively. This was done in two ways. First, among the visitors to the SRC who learned from their interactions there and returned home with intellectual souvenirs were scholars from other countries, including, for example, Henry Valen (Norway), Georges Dupeux (France), and David Butler (Britain). Second, the principals from Ann Arbor began careers of directly engaging in collaborative cross-national research and participating in the growing international community of elections scholarship (Miller interview, 7/20/97). Miller developed professional contacts and relationships in many countries, and was instrumental in working with some who were establishing election studies in their own countries especially, in the early days, in Sweden and the Netherlands. He, with Donald Stokes, eventually attempted to expand their representation study comparatively, resulting in what he often described as “one of the best publicized unpublished books in modern political science” (Thomassen 1999).<sup>11</sup> The collaboration of Converse with Dupeux (1962) built on the 1956 study. Stokes continued to work with Butler on developing British election studies, and ultimately collaborated with him on his major intellectual legacy: Political Change in Britain (Butler and Stokes 1969).

This internationalism on the part of the early principals was only the beginning of a pattern that has lasted as long as the project has thus far. Almost every leader of the project from then on became directly involved in collaborative cross-national research even though they were trained as “Americanists.” This is partly because of the professional connections and, ultimately, prestige and leadership of the American election studies, but also, one can argue, because the norms of this very science oriented social science work, in stressing replication across time and condition (and therefore, place), was buttressed by a norm that encouraged such collaboration.

In the period from the 1956 study to that of 1958, then, the project was first and indelibly marked by the impetus to incorporate not just cross-sectional time series but actual panel data into the design; to consider the integration of mass survey with other types of data; and to encourage and investigate cross-national comparisons in an effort to generate more theoretically sophisticated models and better empirical tests of specific hypotheses about the nature of electoral behavior. In the same period the principals began to devote more intensive attention to instrumentation, for example by investigating response acquiescence (Converse interview 6/17/98). The 1960 election study completed the first (of three) multi-election panels. A series of influential articles, known primarily to elections experts rather than the much greater

readership of The American Voter (Campbell, Converse, Miller, and Stokes 1960), was the collection published as Elections and the Political Order (Campbell, Converse, Miller, and Stokes 1966). This also includes some of the representation work as well as cross-national analysis.

The SRC election studies continued in an unbroken biennial cycle through the 1960s, and reveal the crystallization of a pattern of maintaining a time series of core questions, repeated from one survey to the next, plus the incorporation of new batteries or design features geared toward the specific circumstances of the political and electoral context and scholarly and scientific developments, such as the emphasis on new theoretical approaches or methodological techniques (Table 1). The structure, personnel, and funding of the studies also changed from study to study, reflecting the varying solutions project leaders found for obtaining funding every two years for this increasingly expensive enterprise. The codebook for the 1962 study indicates that it was run by the SRC Political Behavior Program as part of the fall Omnibus Survey conducted by the SRC Economic Behavior Program. From this study until 1970 the Michigan-based principal investigators are anonymous as individuals. The 1964 study, funded by the Carnegie Corporation, included an oversample of African Americans, thus facilitating research on race politics. The 1966 study, directed by the Political Behavior Program and Walter Murphy (Princeton University) and Joseph Tanenhaus (University of Iowa), was again run through the Economic Behavior Program's regular Quarterly Survey on the Consumer Outlook. It was financed partly by an NSF grant to Murphy and Tanenhaus to study attitudes toward the Supreme Court. The 1968 presidential election study, funded by the Ford Foundation, included another oversample of African Americans, and incorporated themes and items that have since become central preoccupations of the surveys, including mass media use. In 1970 the sample included 18-20 year olds for the first time, anticipating the change in the national voting age due to occur in 1972, and again included a black oversample. The themes and substance of the studies clearly reveal the marks of 1960s and 1970s politics and political concerns.

Table 1 about here

The early 1970s marked other important turning points in terms of the leadership and structure of the election studies. In 1970, for the first time, the Principal Investigator was "The Center for Political Studies," founded that year under the direction of Warren Miller. Campbell, Converse, and Stokes had ceased their direct association with the Michigan Election Studies and, as continuing principal, director of

the Center for Political Studies, and founding director of the new Inter-University Consortium for Political Research, Warren Miller was the prime mover and undisputed center of the project. SRC continued to house survey operations within the Institute for Social Research, but the intellectual home of the MES was now a research center devoted to the study of politics. Although there was still some interdisciplinary involvement, and the literatures that helped shape the theoretical underpinnings were still largely social psychological, the MES was by now a much more distinctly political science project.

In this period of the late 1960s and early 1970s the Michigan Department of Political Science, and especially its program in political behavior, contributed numerous graduate students to the daily life of the Center for Political Studies (and its predecessor offices), including many employed by the election studies and other survey studies based there. As these waves of graduate students completed their dissertations and early publications using these data, they exported not just the “Michigan model” of electoral studies to the many political science departments around the country as they launched their own careers, but also the style of research, and a taste for research based on secondary analysis of archived high quality survey data sets. As such, they helped create a tremendous market for Michigan Election Studies data.<sup>12</sup>

In 1970 the principals first secured NSF funding for an election study as a whole, this initiating what came to be a long relationship. It is not possible to understand the relationship of the election studies to the National Science Foundation without understanding how tenuous was the relationship of the social sciences to NSF.<sup>13</sup> The congressional charge to NSF explicitly rejected including the social sciences in the NSF mandate; instead, the agency began with a “permissive but not mandatory” clause that was hardly encouraging. A consolidated social science program began in 1958 and a separate division was founded a year later. But in the early years of NSF involvement in social science the Foundation did not, in general, turn to influential practicing social scientists for leadership but rather, to people who had made their careers as administrators, as Pendleton Herring, president of the SSRC, complained in 1965. In FY1958 the social science budget accounted for \$750,000 of the approximately \$50 million NSF budget; in 1965 the social sciences constituted \$10 million of the \$500 million NSF budget. In all cases the budget available for political science was comparatively small even among the social sciences (Lyons 1969: 274). Members and committees of congress periodically attacked the social sciences and both private and public funding for social sciences, usually on the grounds that the social sciences were not really sciences or that they were politically suspect. Two committees in the early 1950s had declared the social sciences

too “socialistic” and anti-capitalist. Following the Project Camelot debacle in 1965, hearings on the social sciences began anew, and Senator Fred Harris (D-Okla) proposed the establishment of a National Social Sciences Foundation. Among the scholars who testified in favor was Warren Miller, who argued that only such an agency, run by social scientists, could fight for the level of funding and type of infrastructure necessary to further social science research (Lyons 1969: 293). Thus NSF funding for social science research, especially political science – which aimed at both a small pot of money and, by its nature, could be politically risky – was far from assured in the early 1970s no matter how good the proposal.

Partly because of the difficulty of obtaining the large amount of money necessary for a national survey of the scope of the Michigan election studies, the 1972 study emerged not just as a time series core with innovations, but as an omnibus study with multiple principals with different interests, including Miller and a more junior Michigan political scientist colleague, Arthur Miller, as well as Richard Brody (Stanford), Jack Dennis (Wisconsin), David Kovenock (North Carolina), and Merrill Shanks (Berkeley). The omnibus participants were interdisciplinary, but local to ISR: Frank Andrews, Angus Campbell, Ki-taek Chun, Gerald Gurin, M. Kent Jennings, John Robinson, Jay Schmiedeskamp, and Stephen Withey. Once again, the codebook describes the situation encountered by the principals and the nature of the decisions they faced:

An ambitious proposal was submitted to the best potential source of funding, the National Science Foundation. Although it was accepted, it was not supported at the level hoped for. To supplement the initial NSF grant the study staff invited the participation of several ISR colleagues in closely-related disciplines -- particularly economic behavior and social psychology. Their financial contributions made it possible for a full study to be performed, but at the same time left a very difficult choice to the study staff. The interview length could be held down to the normal hour or so, leaving the Center for Political Studies with a very small body of traditional political information in the questionnaire or the interviews could be allowed to run longer. The decision was made by the study staff to increase interview length in order to retain an amount of political information comparable with that of previous election studies.<sup>14</sup>

The linked problems of funding and scholarly integrity, discussed more fully below, had fully emerged as vexing issues. Although the 1972 study is a fascinating instrument, it is the first of an increasingly

complicated set of surveys in that it is organized into different “split half” forms, with respondents receiving a core plus a selection of the additional topics. Further problems emerged in the release of the data, with some contributors more generous than others in lifting the embargo on “their” parts of the survey. For the scholars who had become used to the idea that they could depend on the Michigan scholars to share their data in a timely fashion, this tension between the proprietary nature of the omnibus pieces and the “Michigan Election Study” as a whole created irritation both in Ann Arbor and in the larger community.

The difficulties Miller faced in cobbling together funding were not enough to dampen his persistence and entrepreneurial style.<sup>15</sup> Not only did he return to NSF for subsequent funding but, for the second time in the history of the election studies, he and his colleagues launched a major panel study, in this case incorporating the 1972, 1974, and 1976, with the presidential election studies still designed as pre-election/post-election mini-panels. Mercifully, none of these was structured as an omnibus study. The basic funding for each wave came from NSF, although in 1974 the John and Mary Markle Foundation also provided important funding. The 1974 study, led by Warren E. Miller, Arthur H. Miller, and F. Gerald Kline, and the 1976 study, led by Miller and Miller, took the same citizens through the end of the Vietnam War and the extended period known as “the sixties.” The 1974 study had a substantial focus on the mass media, including detailed information on which media sources respondents used, thus allowing researchers to incorporate information on those sources into the data. That study was guided partly by the SSRC Ad Hoc Committee on Media and Politics, including Steven Chaffee (Wisconsin), F. Gerald Kline (Michigan), Sidney Kraus (Cleveland State), and Thomas Patterson (Syracuse). The 1976 study completed the extraordinary panel.

The Michigan Election Studies had become institutionalized in many scholars’ minds as a valuable social science resource. Behavioral research and, especially, survey research, had become a dominant force in political science. Although the studies were identified as distinctly associated with the group of scholars at Michigan, they had often involved important collaboration of interdisciplinary groups of scholars, often based at different universities. Warren Miller had managed to find funding support for an unbroken line of studies, although often on a touch and go basis. But given how widely stretched he was professionally, the demands on the project, and increasing linkage of the studies with NSF combined with the continued, and perhaps increasing tensions over that funding, it was time for a change. Miller entered



into discussions with NSF program officers over the possibility of long-term funding for the election studies that would obviate the need for facing the uncertainties of continuation every two years. The ultimate result of intense negotiation was the eventual success of the 1977 proposal for “Long-Term Support for the American National Election Studies.” It transformed the project formally into a national social science resource with a wholly new structure, and guaranteed funding for 5 years (1978-1982). This was followed by a two-year grant (1982-84), two more 5-year grants (1984-1988; 1989-93) and two 4-year grants (1994-97; 1998-2001).

### **The Founding of The National Election Studies**

In the mid-1970s, the NSF program in political science was a tiny part of the social sciences at the Foundation, having been frozen for some time at about \$1 million dollars.<sup>16</sup> In light of the competition among the many subfields of political science, let alone distinct projects, any given proposal for a national election study would be in the treacherous position of asking for one-third to one-half of the disciplinary budget. The political science budget was not big enough to contain a “big science” project.

There were added difficulties relating specifically to the election studies. Warren had become a major rainmaker at ISR, and visited the NSF offices regularly. As David Leege, Political Science Program Director from 1975-76 put it, “The language around the Division ... was, ‘Here comes Miller again. Watch your wallets.’ Warren was wonderful at interpersonal relations, but it was the common problem of someone who was highly successful in gaining funding that people always suspected when Warren came around.” The reviews of the 1974 Election Study proposal were mixed, and may well have been saved by the substantial presence of panel participants from the SSRC Committee on Political Behavior. The proposal for the 1976 study faced a worse fate. Submitted by Arthur Miller and Warren Miller, it was not favorably reviewed within NSF or by the panel, including people who had long been admirers of the Michigan election studies. As program officer, Leege chose to write a candid response to Warren Miller, underscoring the faults of the proposal.

Leege and Miller, in their own ways, used this particular juncture in ways that would have major effects on both the future of the election studies, on the political science program and, indeed, on the social sciences more broadly. Leege sought a way to improve support for political science in general, and

also worried about the future of what he saw as a valuable project. He considered the models offered in the biological and physical sciences, and began to devise a plan for what he and the Foundation came to define as a “national resource in the social sciences.” He worked with Miller on ideas for transforming the project; Richard Dawson, Legee’s successor at NSF, continued the shepherding work on the Foundation side.

The logic and design of the new project is outlined in the final proposal submitted by Warren Miller in 1977.<sup>17</sup> It argues that the nature of private foundation support made sustenance of the basic investigation of the elections studies impossible, especially given the importance of continuity in the instrument. Private foundations could offer and withdraw support at will, and would likely expect to play too large a role in shaping specifics of the research. Moreover, the basic costs of executing high quality national sample surveys was prohibitive for both private and public agency support if it had to be sought on a study-by-study basis. It was especially difficult to attract support to do crucial methodological work and instrument development that was increasingly necessary as scholars learned more about -- and learned they needed to know more about -- the nature of the survey response and its implications for substantive research. Methodological research been an important focus of the project work from an early date -- after all, one of the most well-known contributions of the early gang of four, Converse’s influential 1964 article on the “Nature of belief systems in mass publics,” was about precisely this issue. A long-term grant, however, could include appropriate auxiliary research and development efforts to support a considerably less ad hoc product. Finally, Miller pointed out that timing itself had become a major issue. A scholarly survey done through face-to-face interviewing was very different from commercial polls designed to cover this week’s news. Study planning and preparation required more time than study-by-study funding procedures could usually offer. The 1977 proposal underscored that the 1976 Election Study was launched on the earliest funding notification the project had received since 1956: 18 May 1976.

What was the design of this “national social science resource?” The American National Election Studies was created with a five-year lifespan that could be renewed only upon submission of a full proposal to NSF which would be reviewed in the usual way by the appropriate program. The University of Michigan served as host for the project; the Principal Investigator (PI) and study staff were based at the Institute for Social Research. The PI, however, would be supervised by a national Board of Overseers appointed by NSF that would have staggered four-year terms. The Board would, in conjunction with the

principal investigators, develop long-range plans, review work programs and budgets, make decisions regarding priorities in the instrument, assist the principals in developing further proposals, and make an annual report to ISR. The plan also said that “as a national resource, the National Election Studies must be responsive to the wide community of users of these data, and the Board shall serve as the primary link between CPS and the National and international research communities. The Board shall identify and be responsive to the interests of scholars in the relevant research community.” Thus, the Board was required to develop regular mechanisms for soliciting suggestions and advice from the larger community and would sponsor workshops and conferences of methodological and substantive relevance to the project.<sup>18</sup>

“In recognition of the status of the election studies as a federally funded national resource the Center for Political Studies will follow a policy of early and equal release of data following each election study” (Miller 1977). The data had to be made available to any potential user – including those associated with the project – on exactly the same basis. Even study staff would have no access to the data beyond what was necessary to transform it into a usable data set before a version was released to the community. The data were to be archived with the ICPSR as soon as possible. Thus, NES data would not be proprietary in any sense; the sole mission of the project personnel was to produce data and encourage research. NSF provided no funding for analysis or research support.

While the planners had to use their best judgment for design and content, which would certainly reflect their own particular expertise and interests to some degree, they were required to develop the survey in the scholarly interests of the very large and diverse community of scholars it might serve. Mechanisms to assure that this would happen included recommending new Board members who could maintain a diversity of expertise and interests within the community of scholars who used survey approaches to electoral studies;<sup>19</sup> and ensuring that people invited onto planning committees could complement the skills and interests of members drawn from the Board and not duplicate them. In addition, the Board developed a strong culture leading its member to try to represent approaches and question in the study that were important in the larger scholarly community, both by seeking feedback and counsel and by independently reviewing the current literature.

This model for the project was very different from the standard research project run by most

scholars, but, far from being a radical departure for the election studies, it was an evolutionary transformation of the model that had been developing since 1948. Certainly, it offered more scope for planning and thinking in a more long-term fashion. The notion of core time-series content had long been embedded in the operating assumptions of the project, but now, as a national social science resource, protection of the time-series became a specific obligation. Assumptions about the process of innovation changed. In proprietary studies, principal investigators are free to decide for themselves what they want to study and how, within the constraints placed by funding agencies. Although Miller and his associates already viewed the research needs of the larger community as an important influence on the study content, the new design required that mechanisms for achieving this be institutionalized. The long-term funding offered what NES something it had not previously had: support for research and development (R&D) work allowing test-driving of new instrumentation and methods for improvement or innovation in response to the changed political or scholarly context.

The new plan for the project could still be hampered by relatively small size of the NSF budget for political science. It did not, after all, make the project cheaper. As the political science program officer, David Leege had already been seeking ways to add support to the political science program. He took advantage of the interdisciplinary quality of some political science proposals, for example, those on political economy or the environment, by farming them out to other appropriate NSF divisions. Thus, a political science budget of roughly \$1 million might, in effect, be expanded by \$.7 million. Leege pursued two avenues of financial restructuring within the Foundation. First, the money from outside the political science budget that had been used for political science projects would be drawn into the political science base budget. Second, new money (amounting to about \$1 million) would be brought into the political science budget for the purpose of funding operations connected with NES. “So, in effect what we did with the panel and with the Foundation was to establish a kind of social contract, and that social contract was that the basic political science budget was already doubled in a variety of ways, and it would be tripled with the addition of the National Election Studies. So, in effect, NES was in for 33% of the money, and the rest of the discipline got a 100% increase in funding largely because of the National Election Studies coming to the program.”

At the time, Leege did not believe that the entire sum of money should be put into the political science. His question, one that was raised again within NSF in restructuring debates in the late 1990s, was

whether infrastructure funding such as the newly designed NES should be based at least partly outside of the specific programs. The point was not to make infrastructure programs invulnerable, but to avoid the inevitable jealousies and conflicts with research funding of the more usual type. “I know enough about the concept of relative deprivation that the other subfields would see that money and say voting behavior is getting more than its fair share and would start eating into that” he recalled. “I guess it should be a surprise to no one that over the time intervening that gradually that roughly 33% in the original social contract was chipped away so that by the time of the funding in the [1998-2001] go-round we’re down to about 16% of that budget. Totally predictable in terms of what we know as political scientists.”<sup>20</sup>

Major players in and around the National Science Foundation were clear that this series of decisions surrounding the creation of NES changed the conditions for social science. Leege reports that there was great excitement surrounding the NSF panel meeting scheduled to consider the project. “It was no surprise that when our panel meeting came, the word had gotten out around the Division and the Foundation that this was going to be the first effort at a national resource in the social sciences, so that room was packed.” The panel was positive, and the leadership within NSF concluded that this new design for social science research merited trying. Soon, the General Social Survey, based at the National Opinion Research Center (NORC) since 1972, and the Panel Study of Income Dynamics, based at the Michigan Survey Research Center and first fielded in 1968, also worked with the Foundation to develop multi-year proposals as “national social science resources.”<sup>21</sup>

This transformation raised difficult issues for processes within NSF. Leege had stepped beyond the usual role of an NSF program officer in being so proactive in institution creation. Some critics believed he had violated the appropriate norms of peer review. But Leege points out that that was a period of infrastructure development from within NSF, and an opportunity to solve some problems that needed solving with a new mechanism. While NSF is an institution of science, it is also a part of the federal policy bureaucracy. On reflection, Leege defines the role he took in terms of John Kingdon’s “policy entrepreneur” (Kingdon 1984).

## **Historical Overview of NES**

The basic framework established in 1977 held through the subsequent grant periods and data

collections. The Board of Overseers has worked closely with the principal investigators and, indeed, has even increased its active role over the years. Each election study was developed by a “Planning Committee” selected by the Board and consisting partly of Board members and partly of other scholars with interests and expertise especially relevant to the themes and problems of the study. Warren Miller remained Principal Investigator from 1978 through 1988, although he departed from the University of Michigan and joined the faculty of Arizona State University in 1981. In 1990 and 1992 he was joined by Donald R. Kinder and Steven J. Rosenstone, both of whom had joined the faculty of the University of Michigan. This marked the beginning of the first major full transition of leadership of the project. For the he next grant, Rosenstone moved into the lead position among the Principal Investigators, still including Warren Miller. In 1997 Rosenstone resigned from the University of Michigan faculty to take a deanship at the University of Minnesota, and Virginia Sapiro of the University of Wisconsin - Madison joined the list of principals. In the NES grant for the period 1998-2001 NES, Sapiro and Rosenstone appear as Principal Investigators. Warren Miller remained active on the project as “Emeritus Principal Investigator” until his death early in 1999. In 1999, Nancy Burns of the University of Michigan joined the project as a Co-Principal Investigator, and Donald Kinder, who had been uninvolved in the project for a couple of years, also rejoined in this capacity.

The National Election Studies ran “production studies” in presidential and congressional election years, and “Pilot Studies” in the off-years. The Pilot Studies offered one of the chief means of directly feeding contributions to the project from the larger community and, indeed, for recruitment of planning committee and Board members, and even principal investigators. For Pilot studies, the Board would develop general sets of priorities for R&D work, but then would openly solicit and review proposals from the scholarly community at large. Pilot plans were drawn largely from those proposals, and some proposers would be invited to join a planning committee. It is important to emphasize, however, that even these were never understood as “omnibus” or “proprietary” studies; much to the chagrin of many participants, the distinct pieces were never left pristine by the planning committees or Board, but were reviewed and revised extensively for quality, coherence and, above all, the likelihood that they would suggest means of improving the quality and utility of the production studies within their mission of data as a national social science resource. In each case, those involved in planning and contributing to the pilot work were held responsible for writing a report to the Board of Overseers evaluating the new material or experiments in terms of the implications for improving the quality of NES production studies. This Pilot

work then fed into production studies for successive elections. Table 2 shows the key design and content features of the Pilot Studies.

Table 2 about here

Another key part of the NES research and development efforts was the series of R&D conferences held over the years, as mandated by the original NES grant, and funded subsequently except in the 1998-2001 grant. For each, the Board of Overseers would identify a problem or theme it defined as in need of concerted work in order to improve NES studies. It would invite diverse experts to a conference both to assess the track record of NES in that area and to suggest innovations. Among the topics were congressional elections, the Gulf War, the impact of political campaigns, values and predispositions, candidate evaluation, and political cognition. Many pilot study modules emerged from these conferences.

Of course, the major work of NES remained the regular election studies. As Table 3 shows, there was great variety in design and content. During this period study planners focused especially closely on the ability to track changes during the course of the campaign and to facilitate research on campaign effects. In one of the most ambitious examples, the 1980 Election Study consisted of eight discrete data collections including a panel component, a sampling during the primary season, and a traditional pre-post election study. The 1984 “rolling cross-section” design offers another example. For some of the studies, independent random samples were released successively in order to allow researchers to use time as a variable without introducing the confounding effects of the ease of getting the interview. Technological changes have influenced the development and distribution of the data. Both CATI and computer assisted personal interviewing (CAPI) techniques have changed both the possibilities within the instrument and the processing of the data. Thus, designs became more complicated, although because of budget cuts, the 1998 Election Study returned to the simplest pre-post election format that had not been seen in years.

This period also resulted in some special study efforts. The NES conference on congressional elections resulted in the formation of a working group that transformed the content of the studies relevant to congressional elections. After further planning, the project also ran a Senate Election Study in 1988, 1990, and 1992. NES also began to attempt more collaborative links with other scholarly studies. In 1996,

for example, following the conference on the impact of the campaign, it worked with a consortium of media scholars who submitted a successful proposal to collect media data. NES also established linkages with the National Black Election Study.

A reading of the proposals and their reviews shows that many of the most major changes proposed by the principals -- and some that were elaborations of already tested features -- were rejected for funding. Among these were interviewing the pre-adult cohort of 14-17 year old in families tapped by the main survey, supplementing the survey data with in-depth interviews, content analysis of mass media, a study of Senate elections (rejected as part of the basic platform grant, but accepted eventually in a separate proposal), design features that would allow greater facility for studying the race basis of electoral politics, and continued participation in the Comparative Study of Electoral Systems after the first round. In addition, a proposal to continue and enhance the design to allow studying campaign effects and using time as a variable was rejected in the 1998-2001 round.

Table 3 about here

With the growth of the Internet and the World Wide Web, communication with the larger scholarly community and the distribution of data and other information became radically altered. Instead of relying solely on the ICPSR as a means of distribution once the days of 9-track-tape had gone, NES initiated direct distribution through its FTP site. As of 1999, all NES data were made available in this fashion, increasing direct accessibility. Moreover, in 1996 and then in 1998 NES released a CD-ROM that contained files of the data, and supporting documents, from all of the studies it had mounted thus far. The symbolic approach of the 50<sup>th</sup> anniversary of the studies, as well as the available technology (and software commissioned by NES to facilitate working with the data on the CD-ROM) led to increased focus on encouraging scholarly use of the time series data, including focusing more on developing more nuanced and interesting theories of history and change, and more appropriate techniques of analysis.

In addition to the increased focus on historical approaches, NES also substantially increased its direct involvement in comparative research. Just as Warren Miller had long been active in cross-national collaborative research networks, so, in the early 1990s, Steven Rosenstone began working with the leadership of ICORE, the International Committee for Research into Elections and Representative



Democracy to consider how to achieve enough collaboration among the many national election studies around the world to facilitate planned comparative analysis. By this point there were many active election studies projects around the world, and following the fall of the Soviet Union and the creation of other new electoral democracies, there were to be more. Could the various studies collaborate to allow scholars to use these various election studies more easily for comparative work?

At a first planning conference, in 1994, a steering committee consisting, in addition to Rosenstone, of the chair, Jacques Thomassen (University of Twente), Hans-Dieter Klingemann (Wissenschaftszentrum Berlin Fur Sozialforschung), and John Curtice (University of Strathclyde) distributed a stimulus document, ascribed primarily to Rosenstone.<sup>22</sup> It was circulated widely among election scholars around the world, and a document of responses drawn up. Following a planning meeting, the group, now titled the Comparative Study of Electoral Systems (CSES), with its Secretariat based at the offices of NES, emerged with Thomassen as its chair. CSES developed a common module that has been integrated into the regular national election studies of many countries from 1996 to 1999, including in the 1996 U.S. National Election Study. In 1999, as CSES prepared to develop a second module while encouraging analysis of the first, Thomassen retired as chair of the Steering Committee and was succeeded by W. Phillips Shively (University of Minnesota), then a member of the Board of Overseers of NES, with the Secretariat remaining in Ann Arbor. NES obtained a separate NSF grant for support of the central functions of CSES.

From the point of view of understanding the history of the election studies as a case study of big social science, as compared with simply tracing the history of the project for its own sake, the periods of the Michigan Election Studies and National Election Studies offer some similar points of interest and some that are different. As suggested above, although the transformation to a “national social science resource” placed many responsibilities and constraints on the project that are not typical of the usual research project (as well as the opportunities provided by the four- and five-year grants and the funding for auxiliary functions), given the norms according to which the project had already been operating, the shift was not radical. Miller himself strongly held the vision of collaborative, unified science, which is why he was also the central figure in creating the Inter-university Consortium for Political Research as a means to share data and was devoted to organizations promoting these values.<sup>23</sup> A large network of scholars became involved early on and came to integrate the data into their own work, partly because of the

institutional framework provided by the SSRC. The culture of the Institute for Social Research also supported this kind of development as did its basic infrastructure; it became the home to a number of large and continuing projects over the years including, among others, the Panel Study of Income Dynamics, Monitoring the Future, the Michigan Socialization Study, and the Detroit Area Studies, and it was also the home of the ICP(S)R. The unique characteristics of the Michigan Election Studies also provided an opportunity for NSF to transform its method of dealing with social science.

Nevertheless, the election studies project was transformed into a more institutionalized entity bound by contract with NSF and through that, the larger community. Although the Michigan Election Studies were subject to the constraints of the foundations on which they had depended, had developed its own working culture, and the scholarly community already had developed its own expectations of the studies and depended on their fulfilling those expectations, these demands became more formalized in the post-1977 period. Most particularly, it was now a publicly funded institution with a formal mission of representation and responsiveness, in a context in which scholars with an awareness of the project, and even those who depended upon it, understood the potential zero-sum tradeoff between the resources expended for NES and those expended for other projects.

For this reason, and for the purpose of drawing conclusions about the significance of “big social science” projects, rather than continuing the chronological tracing of the project, the remainder of this paper is devoted to exploring a series of dilemmas that arise structurally from the situation of NES, that underlie a significant portion of decision-making within and about the project, and that are likely to be at issue, at least to some degree, for any major social science project depending on a model like that created for NES.

## **DILEMMAS**

The boundary between a proprietary study and a more public social science resource is not clear. Indeed it would probably be best to pose a continuum between the two. At one end would be the small, purpose-built study designed to test a very discrete set of hypotheses, constructed by the researcher(s) who will do the analysis and publication of the results. Even at this end of the continuum, however, changes in social science norms make the fully proprietary study less “private” than it once was. The

norms in political science, for example, have moved significantly toward the responsibility of researchers to facilitate replication of their work by other scholars (“Verification/replication” 1995). At the other end of the continuum is a project, such as NES, which is founded for the specific purpose of providing the data needed to a non-bounded public (consisting, in fact, of scholars of the present and future) with no provision whatsoever for providing any other resources or opportunities for research to the principals.<sup>24</sup> In between, there are many projects that were purpose built, but which offer such rich mines of data and which were made available for use by other scholars either because of the public-regardingness of the principals, or because of the growing norm (and NSF requirement) that data be archived, that they have become public resources for the larger scholarly community in their own right, as was the case with the early Michigan Election Studies.

Not surprisingly, there are trade-offs and dilemmas to face in making choices along this continuum. Although occasionally, in academic politics (as in any other kind) the choices among these values can be viewed by interested parties as choices between good and evil, more often than not they are matters of simultaneous goals that happen to be in mutual conflict. As argued above, the norms of particular fields and institutions are often not coherent and closely integrated, and the varying positions that make up the field define the range of tension and struggle within that field. The weight of the choices that particular actors and institutions make within the field of possible positions redefine the field itself, and become the mechanism for historical change. The remainder of this paper focuses on six such dilemmas, most of which were present in the Michigan Election Studies, and all of which define the persistent issues faced by the National Election Studies: data generation versus data analysis, data versus research, time series versus innovation, the problems of R&D work, auxiliary functions, and institutionalization.

### **Data Generation versus Data Analysis**

The generation of nationally-representative sample survey data has always been expensive because of its labor intensity and geographic coverage, especially in the case of face-to-face interviewing. Financing data collection is a critical problem in this field. The cost of data collection is so high that it has been conventionally difficult for survey researchers to obtain funding for their own research time. Data analysis has never been covered in NES grants, and was not covered in MES grants. Before the days of

personal computers and current forms of statistical software, this was in fact more difficult for active researchers using survey data than it is now, given how massively labor intensive were the data management and analysis processes using counter-sorters and then card-based mainframe computers. For a team engaged in provision of time-series data, the demands of the cycles of funding, design, field work, and data management (as well as auxiliary services discussed below), mean that lack of support for analysis reduces the possibility that those involved in data generation can then use those data for analysis and research.

Philip Converse reports that at a crucial point in the 1950s, the principals involved in the election studies had one good opportunity for working on a manuscript, which resulted in the most famous “data report” of them all: The American Voter. But even the, he said, Angus Campbell was dubious about whether the team should try to write a book, because they would have insufficient time, given the lack of financial support for that time. As Converse said, “I always had a sense that that was absolutely signal in the success of the report of the 1956 study, if you want to call it that: is we had a lot of time to write on it and four of us clearly ... rapid writers....And I just think that was so central in that experience....” He contrasted that with the more usual situation that held, in which “analysis moneys were always underfinanced in these grants.” In other “studies I was involved in, this underfinancing of the analysis, and the writing, and the intellectual payoff of the whole thing has always struck me as pathetic. And people ... just have to be writing for their next grants, and they’re using their six months for wrapping up a huge study, and so on.” Following the transformation of the project into a national social science resource, a further trade-off further drove a wedge between data generation and analysis: Given that the principals could no longer embargo the data for any period of time, the pressures on them further inhibited their ability to do large, major pieces of scholarly research and report writing. For a project that is expected to be scholarly, and held to the standards (including by its own principals) of creating theoretical and substantive break-throughs, this creates great difficulties that must be solved.

This trade-off between data collection and analysis creates the need for certain choices in the development of a project. A data collection team is not the same as a research team; participation in such a venture would attract different kinds of people with different skills and interest. For scholars to remain interested, and for a project to attract high quality scholars, at least it has to leave room for research, if not actually sponsor it. Further, lack of support for analysis and writing creates incentives to search for ever

more data collection support, potentially distancing the organization from scholarly research endeavors. As Converse noted with respect to the emerging self-identity of the Institute for Social Research in the 1950s, “ISR prided itself on not being NORC, where the difference was that ... NORC did contract research, and they sat and talked with the people who wanted the questionnaire done and then they’d do the questionnaire up pretty and professional, and they’d collect the data, and they’d whip the data out the door. And [the data] would be analyzed by the sponsor.”

One solution is for a research center that incorporates a project like the election studies to engage in increased commercial research in order to support the more scholarly products. Once the Center for Political Studies (CPS) was founded as a separate research center in ISR, and the election studies became a centerpiece in CPS’s portfolio rather than remaining in the Survey Research Center, which has the capacity to allow more commercial contracts to subvent the research work of SRC scholars, this option was closed for support of NES. CPS was purely a research center,<sup>25</sup> with no facilities for survey field work or other such data generation functions and given that unlike SRC, almost all of its professional staff are professors, they have virtually no motivation to engage in research-related activities that would not be part of the process of generating scholarly work. NES has commissioned field work for all production studies and most R&D studies from SRC, but the bureaucratic organization of ISR distinctly separates the various research centers, and even makes them competitors with each other.

The interim election studies strategy, represented in the 1972 Election Study, began to recreate the project as an “omnibus,” in which the burden of seeking funding for data collection is distributed among different investigators who bring in their own support to attach discrete pieces to a common core. This created a very mixed outcome that ultimately did not suit the vision of Miller and other principals. An omnibus study has a very different construction and mission from a survey that is constructed as a coherent piece despite the fact that the election studies have become and contain many subthemes and modules. In an omnibus the designers’ visions of the integrity of any individual piece must take some precedence over the whole. In unitary<sup>26</sup> studies, the interaction among elements of the study — a crucial question from the point of view of data quality -- and the ultimate needs of the research community at large ultimately take precedence over specific pieces and interests. In omnibus studies, the embargoes and related decisions of the individual researchers affect the data as they are made available to a wider community for research.

In the NES era, an emergent strategy has involved expanding the role of the Board of Overseers collectively and individually to participate in design and planning, broadly speaking. This commitment has drawn many Board members into contributing an impressive amount of voluntary time and energy into research and development analysis which often cannot be transformed into scholarly publications in the kinds of prestigious journals in which these scholars generally publish. Thus, sharing the burden has become the norm, but this still requires that active, productive scholars contribute time they might have spent doing their own research.

### **Project Productivity and Contributions: Data versus Research**

The conventional product scholars use to analyze the quality of a research project is the research report: the book(s) and article(s) that integrate the ideas, theories, data, methods, findings, and conclusions. This is what usually constitutes the “contribution” of a research project. Thus, reviews of the project rightly ask:<sup>27</sup> What are the scholarly, theoretical, methodological contributions of the project now? How does it advance the frontiers of electoral and related research? Where are equivalents for today’s NES of The American Voter? Scholarly projects in which data generation is a core part of the mission are generally evaluated by the primary data analysis such as Campbell, Converse, Miller, and Stokes’ (1960) The American Voter; Jennings and Niemi’s (1974) Political Character of Adolescence, Huckfeldt and Sprague’s (1995) Citizens, Politics, and Social Communication, or Verba, Schlozman and Brady’s (1995) Voice and Equality, not by the secondary analysis, which is icing on the analytical cake. Over the years the principal investigators have occasionally published widely-noticed books and articles using the election study data. Miller and Levitan (1974) used a long run of the time-series data, and Miller and Shanks (1996), using the 1992 Election Study, was published after Miller had retired from the day-to-day running of the project. Steven Rosenstone, with John Mark Hansen (1993) published his work on participation while he was actively running NES.

But the situation is different for a project aimed at providing data for a large community of scholars and which itself, cannot in any obvious sense produce a scholarly project report such as those named above. In the case of NES, for example, while many members of the Board of Overseers have produced some -- sometimes considerable — scholarly contributions in the forms of books and articles based on NES data, these are not widely understood to be scholarly contributions “of NES” because, in

the strict sense, they are not.<sup>28</sup> The logic of a national social science resource with this structure should lead evaluators to look to a different place -- indeed, two different places -- to assess the quality of the product. The first is to what we might normally think of as secondary analysis: the collectivity of all published research that relied on project data in whole or in part, regardless of the relationship of the researcher to the project. If a national social science resource data collection project is fulfilling its mission, it should serve the purposes of a large community of scholars who find in the data the inspiration and resources for good scholarly work. And given that we are talking about a lengthy time series of data, that collectivity of research output is very much, increasingly, an open-ended entity.

Thus, normal methods of scholarly judgment result in a disjuncture between the mission and imperatives of the project as it is established, and the criteria often applied to it in evaluations by members of the research community. Moreover, by its nature, then the published research contributions of a national social science resource data collection cannot have the coherence and singularity of the corpus produced by a research team; much of the work, after all, is produced to contest the conclusions of other scholars using the same data.

Second, quality must be judged partly by investigating the instruments systematically, both individually and in the context of the time series, for hints as to their potential utility for research. The logic of the mission has led to the practice of the principals in generating the data of reviewing each question, each piece, each element of the design for potential uses, and for aspects that could eventually leverage any other given element into larger and different analytical uses. Of course a daunting problem, given this distinction between data generation and analysis, is that the time series mission means that the analysis that forms part of the primary mission of the project may be designed years after the data are generated. Some of the dissertations that may be written in the next few years that explore shifts away from the New Deal electoral coalition over time, or changes in the impact of race politics on voting behavior, or the changing relationship of political trust to electoral involvement, will surely analyze data generated before their authors were born.

### **Time Series versus Innovation**

The major mission of NES is to provide the scholarly survey of record of the American

electorate. The core of this mission is continuity of the time series. Because of increasing difficulties caused by the competition between repeated measures and new ones for scarce space, and because of confusion that had emerged in the research community over the definition of “core,” the Board of Overseers refined and made their approach more explicit in the early 1990s. It emphasized that there were core concepts, not core questions. That is, pursuing the logic of hypothetical-deductive approaches to research, the Board clarified that the questions incorporated into the study stood as indicators of concepts important to theoretical work in electoral studies.

A difficult series of problem and trade-offs emerges from commitment to time-series work in a scholarly study, including especially issues over innovations needed to protect the core and innovations to respond to changed context:

*Innovations to protect the core:* Although the Board of Overseers has defined “core” in terms of concepts rather than questions, its policy and practice lean heavily toward preserving the wording of indicators, for obvious reasons. The Board authorizes changes in the instrument used for core concepts under two circumstances. The first occurs when changes in the political environment alter the link between a question and its intended underlying concept. For example, changes in international politics — such as the demise of the Soviet Union and its satellite nations — periodically change the range of questions that can sensibly be ask to tap defense policy hawkishness. Questions about race politics have been an important feature of NES studies since the 1950s because of the importance of race politics to American electoral politics, but some of the questions in the 1950s studies could not be asked now, and the specifics of what race questions should be asked has shifted somewhat since the 1960s. Some authorized changes appear quite small, but may have important effects on the responses. For example, where the surveys once asked about responses to the “Women’s Liberation Movement,” the wording was eventually changed to “women’s movement.” As long as the primary definition of core is conceptual, whether to maintain or change wording will almost always be controversial.

The second reason to change the indicators for core concepts is more purely methodological. Social scientists have learned a lot about the nature of the survey response and the effects of different techniques in asking and framing questions over time. Thus, at times, it becomes so clear that there are better means of asking particular questions that the Board decides those changes must be made.



In both of these cases change is made in order to stay the same. Nevertheless, these changes are made in the judgment of some scholars on behalf of a wide community with quite diverse scholarly interests and levels of technical knowledge. Researchers using the data often become frustrated and angry when they find that questions they use in time series have changed, even when explanations are offered in the codebook. For many of these scholars, the proper definition of “core” revolves around questions. Other scholars become frustrated when question wording remains the same despite what they see as overwhelming evidence, for example, that the measure could be recalibrated to make it more precise or stable. These differences of view necessarily create tension between the principals (who have often, among themselves, gone through lengthy and difficult arguments to settle on a course of action) and the research community, and struggles among principals to achieve the best balance.

*Innovations to respond to changed context:* Concepts, and therefore related questions, stay in the repeat time series only if, in the scholarly judgment of the principals, informed by communications from the larger research community, they are likely to have remained (or will soon return) to prominence in helping to explain the major phenomena NES is designed to study.<sup>29</sup> The balancing of demands of a diverse community over what should be repeated and what should not is often difficult.

The original mission defined for NES by NSF in 1977, and the judgment of the Board of Overseers since then, is that in order to fulfill its mission NES cannot restrict itself to the time series alone. First, purely in terms of explaining any given election, short-term and particular forces require that scholars be able to account for these in their analysis. Second, new theories and approaches developed by scholars over time require that these be included in the data to make them useful for engaging in research at the frontiers of knowledge. If the time series were left pristine, the theories and concepts of electoral research would have to be left frozen in time – and picking which time was the golden age of electoral theory would be the key question. All current “core” content, of course, was once new to the study; to be less facile about it, in fact a relatively small proportion of what most users of the survey now see as “core” appeared on the early surveys. Much of it was developed later, often through the Pilot Studies of the 1970s and 1980s.

Third, in fact the boundaries of the field of “electoral research” are neither formally set nor defined the same way by all scholars in the field. Some scholars define the appropriate boundaries for

NES narrowly, as predicting voting behavior or electoral outcomes (two different things, in fact). For them, NES studies appear to stray too much into the distinct field of public opinion. For many other researchers, the studies must allow investigation of the dynamics of key elements of the electoral picture, such as political ideology, orientations toward certain key issues such as race politics, the nature of the group basis of public opinion, and the like.

Of course, all of the debates over what constitutes core and what innovations are necessary are matters of contested scholarly judgment. In the context of a project designed to be reflective of a large scholarly community rather than a particular set of principal investigators, there can be no settled consensus on exactly what choices to make. Presumably, there would be full agreement throughout the scholarly community only if all of the important theoretical, conceptual, and substantive questions in the field had been answered, in which case there would be no need for the data collections. These differences are regularly reflected in the responses to Board calls for suggestions and ideas and reactions to proposed core carryover it issues before every study. These responses usually include (with varying degrees of politeness) defenses of previously repeated content that is being considered for hiatus; defenses of previously used content that was never understood as part of the core, but which, the writers argue, should now be so incorporated; and arguments for specific new content without which, the writers usually claim, the study cannot be adequate for current research needs. For a practical example of the relationship among community demands, time constraints, and the conflict between innovation and time series, in preparation for the 1998 Election Study, scheduled for 60 minutes of interview time, the Board of Overseers memo to the research community proposing the list of “core” items to be included generated many responses and considerable heat, sometimes suggesting that the principals had no understanding of electoral analysis. The amount of “core” and repeated items alone that would have to be kept to satisfy the expressed community responses would have run close to 90 minutes of interview time.

As if the problems were not difficult enough, “core” and “innovation” does not define the full set of potential content. NES has a very large inventory of content that has been used before, sometimes multiple times, but which have not been designated as part of the “core” content, even if it might become analytically useful from time to time. Also, measurement is not just a matter of what questions are asked, but the many design features that combine to make a survey an analytically useful instrument. Design features that respond, in the first instance, to analytical needs (e.g., measuring change over the electoral

period) or resource constraints (e.g. choosing between telephone and face-to-face interviewing) have many implications for content and time constraints.

The major problem for balancing continuity and innovation is the increasingly tight space available to accomplish all these ends. NES surveys are very long by conventional standards -- too long, according to some scholars' judgments. For a long time the principals aimed at 70 minutes per session. Time pressure has built both from substantial demands to maintain a strong time-series of items, and from demands that each study contain adequate content to deal with changes in the intellectual field, changes in the political field, and hypotheses about short-term forces which, after all, are hypothetical until the results come in. Additionally, budget cuts in the 1998-2001 grant led to a decision to reduce the length of the instrument to 60 minutes. Indeed, these cuts were premised partly on the views of many members of the research community that the sole mission of the National Election Studies should be "maintaining the time series," apparently interpreted by many as a stable set of questions asked from one study to the next.

Other critics suggest a contrary problem with the trade-offs between the time series and innovation. The combined effects of the designation as a national social science resource and the mission to preserve the time series is a conservatizing force leaving, in some scholars' view, too little room for innovation. Change must always be carefully justified. Although virtually all reviewers of the project over the years support the idea of preserving the time series, given the substantial resources devoted to NES, many critics find the weight of repeated measures stifling of innovation in this particular field, and in political science more generally. For many scholars the variety of new material in each study is a major part of what keeps it analytically interesting and of continuing value.<sup>30</sup>

### **Instrument Development vs. "The Survey:" The Role of R&D**

In most of the natural sciences, instrument development is a function separate from data generation, usually performed by different people. They are certainly related, and must refer to each other, but the people who build the machinery through which the scientists look, listen, or otherwise sense, and record, are not, usually, the same as the scientists who use the instruments for research. In contrast, the line between instrument development and "running the survey" is often not distinguishable in the field of social science survey research.

The pressures for data quality and efficient use of financial resources in “big science” surveys, however, create a logic in which a clearer separation of instrument development and “running the survey” is the norm. The Board of Overseers of the National Election Studies has taken a conservative policy with respect to the process by which new questions appear on the production studies. Its view is that even in a 70 minute survey, it would be wrong to send out trial balloons in the regular election studies that might fail (as so many trial balloons do in survey research), given that the data sets are public goods that will be archived as the surveys of record. Moreover, pretesting in the conventional sense is not an adequate mechanism for instrument development and refinement in a national social science resource of this type. First, there is not time in a regular election study’s production process to do instrument development work, especially if the process must involve broad community participation. Second, for high quality testing of new instrumentation, a larger and more representative sample is required to provide the scope and power necessary to the task. Third, the requirement that no principal do analysis of any NES data before it is made available publicly would inhibit the ability to analyze pretest data in the course of a usual pretest process. Finally, the mission of NES to provide top-of-the-line quality means that appropriate testing of instrument innovation must be more elaborate than is often the case with typical, smaller-scale surveys. NES instrument trials typically involve setting up competitive testing among multiple potential versions, and incorporating enough other questions in the test mechanism to allow vigorous investigation of the behavior of the proposed innovations. Thus, following the practice established in the first NES grant, NES has run the long series of “Pilot Studies” described above, devoted mostly to testing design and content features for possible inclusion in upcoming surveys. This is one of the most direct and influential ways in which a variety of members of the scholarly community to participate in the survey development process.

The research and development needs of the project, and the choice of the Pilot Studies as the mechanism creates a number of dilemmas for the execution of the project. First, the conservative tendencies with respect to the production studies can be frustrating for principals and users alike, for example, when new issues, for which there may be no inventory questions, emerge during the course of a campaign. Second, the strong norm of the project, that it is a public good, and that the collaborative process helps guard its character as a public good, can be frustrating for contributors to the Pilot Study process who are not fully socialized into these norms. For those who contribute research ideas, or even participate in the planning process, abandoning the standard feelings of personal ownership of specific ideas, and having them altered by the collective discussions (especially if one is nevertheless expected to

issue an analytical report to the Board on the basis of the now-much-changed module) can be frustrating. This can create problems for participation in these efforts.

One of the most important problems is that the Pilot efforts have met with grave skepticism on the part of the research community and, in the context of budget issues, some reviewers urge that they not be funded. Indeed, research and development work was almost entirely eliminated, for the first time, from the 1998-2001 budget. There are a number of reasons for the antipathy to the Pilot Studies. One is that the larger community of scholars has probably not been very aware of the Pilot Studies and the impact they have had on production studies. This lack of awareness has two further effects. First, although they have served their purposes for R&D within the project -- a large amount of content in the election studies since the 1980s was developed through the Pilot Studies -- they are underutilized by the larger research community. They are a rich source of data, both in and of themselves and as panel waves. The second effect of the lack of awareness is that when other scholars do encounter them, they appear to be small "stand alone" surveys, quite distinct from the NES mission of studying electoral behavior. It would not be immediately clear from the codebooks what is the relationship of some of the content and experiments to development of production studies; this would require reading the Pilot Study reports and technical papers that are eventually written.<sup>31</sup>

But finally, part of the skepticism is derived from differing notions of the appropriate process for funding and executing research and development work if it is to be done. Within the structures developed under the original grant and its first three follow-ups, research and development was seen as an integral part of the period in which the project was supported. The research cycle, as it worked from the late 1970s to the late 1990s, was that in the course of doing the production studies, the Board of Overseers, in consultation with the principal investigators, study staff, and other members of the research community, would develop some priorities for problems that needed to be solved through research and development work. In some cases, this would result in organization of research and development conferences to draw together a wide spectrum of scholars with expertise in the area under consideration; in other cases the Board would move directly to a call for proposals to the wider community both to solicit proposals to pursue the prioritized problems and to take the opportunity to suggest other foci for R&D work. The planning, execution, and analysis of the data would then take place before the planning process for the next production study was seriously underway. This is a lot to accomplish in the biennium between

studies, and could not occur, especially with widespread community involvement in the development process, if it were also necessary to submit a grant proposal for consideration by a funding agency.

In contrast, given the expense of the project as a whole, many members of the research community believe that the Pilot Studies have not yielded enough return for the investment, that because the main mission is continuation of the time series it does not need extensive R&D work, and that, in any case, if there is a specific case to be made for particular investigations, the requirements of objective, peer-reviewed science mean that proposals for those purposes should be submitted independently.

### **Coherence and Theory versus Representation**

NES has a mission to provide the research community with data that can stand as social science resources. This requires development of coherent theory from which indicator construction and design will flow. NES is also supposed to be representative of the relevant research community, the enforcement mechanism for which is the collective response of the research community to its efforts, and especially responses by NSF reviewers and panelists. It should be clear by now from various problems raised above, that these twin goals, and especially the question of representation, are part of the crux of the matter in many key decisions involving NES. Because the survey instrument is long and complex, and guards against grouping questions together in a way that would produce response biases, it is probably difficult to detect, at a casual glance, how much theoretical and substantive discussion goes into justifying each piece for inclusion.

But the major, underlying problem might best be expressed in terms familiar to political scientists: What constitutes representation? Who, exactly, is being represented? If the Board of Overseers is responsible for making sure that the project does represent the needs of the relevant social science, should its role be that of a trustee or a delegate?

As in many representative bodies, the Board of Overseers as a collectivity tries to fulfill both representation roles in some measure, while different members of the Board over time vary in which is more prominent in their decision styles. There can be no doubt that the element of the delegate is strong;

the Board frets when it cannot respond positively to all the demands placed on it. NES surveys regularly contain content that is of no scholarly interest to the particular members of the Board and other principals, but which serve a program of research that is ongoing in the wider research community.

At the same time, the Board ultimately must make choices, and it must make choices for a research community in which members vary widely in analytical interests and skills, in the amount of attention they have paid to the project, what it does, and how it operates, and – a point made above – a community in which not all the members are yet alive. Moreover, while there is a group of active scholars who monitor the survey for its content relevant to their particular research interests, the Board of Overseers, like other representative bodies, is not supposed to be responding only to this kind of constituent. Members are suggested and selected for Board membership not because they are good vote-getters, but because they are scholars who can help fill out the scholarly interests and expertise needed to fulfill to produce a good survey. Nevertheless, as a collectivity, they are very aware of the political issues of representation.

A review of the evaluations solicited by NSF of the project and its proposals, other statements within the research community, and communications with project leaders reveal, unsurprisingly, that members of the larger research community differ among themselves as to how well NES has achieved this scholarly representation. Many people are aware of the efforts project leaders have made to obtain and respond to diverse scholarly views, and believe that project leaders have, by and large been responsive to the research community within the boundaries of the basic mission. Others cite areas that have not received enough emphasis, areas that have received too much emphasis, and some observers have detected undue influence of project leaders' agendas in some of the areas NES covers. that are covered by NES. Some have fundamental disagreements with some of the important balances and choices NES project leaders have made. Significantly, project reviews also reveal a substantial vein within collective opinion that sees NES as too responsive to varying views, resulting in studies that lose coherence, and thus scientific value because they are designed in hopes that they can be "all things to all scholars." There is no reason to believe that there is truly an equilibrium point at which a project designed with the missions of NES can an objectively "optimal" balance. Rather, as Bourdieu would suggest, the project must resolve for itself the differences of simultaneous positions within the field, even while it keeps an eye on the next set of NSF reviewers.

## **The Functions of a National Social Science Resource: “The Survey” versus Auxiliary Functions**

A national social science resource designed to provide data to a large community of scholars cannot simply generate data. NES doesn't do this, nor does GSS or PSID. Indeed, long before NSF formalized this “national social science resource,” the Michigan Election Studies faced what almost appeared to be a natural evolution toward functions well beyond generating data, analyzing them, and writing them up. In the 1950s, when the Michigan project leaders worked with SSRC to develop political behavior seminars at Michigan, a growing circle of scholars became interested in analyzing the data for their own purposes by obtaining copies of the data to take home. Many, of course, developed suggestions of content they believed would be important to include. In this case, the principals, notably Warren Miller, believed that working with this larger community of scholars was important. As Converse reported,

... a group of young behavioral scientists [came] to Ann Arbor in the summer of either 1953 or 1954 to work on the 1952 data, as it turned out, a very illustrious list. The names are reconstructable. ...Heinz Eulau, and Bob Lane, and so on ... they were all young unknowns at that time, and they had a ball and they got very converted....But ...already, before 1956 even came, here's this set of people who are already interested in getting copies of the data for their own uses, and as well as a broadening circle of their colleagues who understood this was possible, and naturally wanted to get them. And we were spending a hell of a lot of time with this, even the minute the 1956 data were cleaned .... [It] was Warren's view that it was important to service these people....

Very early in the history of the election studies, Miller and his colleagues became involved in assisting other scholars to use the data, and doing the tasks necessary to provide their data and appropriate documentation to other scholars. Converse offers many examples of the multiple tasks involved in working with the growing community of scholars interested in data analysis. Such collegiality benefits the wider research community, and enhances the influence of one's research efforts, but it is costly in terms of personal research productivity. Given the expanded public role of the project in the NES era, including the demands of overseeing its multiple roles and in working with the wider research community and occasionally other agents, such as social science policy actors, a scholar would be foolish to become involved as a principal who was not propelled by the norms of collegiality.

The project is a public good, and expectations from the larger research community with respect to



the provision of a considerable number of services — and in a timely and efficient manner — are vast. The logic of the “social science national resource” designation is that scholars in the field expect service: not just the availability of good data, but data that respond to their needs, data delivered soon enough after elections to be able to deliver papers at the right conventions, assistance with understanding the structure of the data, occasional advice on analysis, special access requests for those who need data that are embargoed for purposes of protecting confidentiality, not to mention access to the planning process. As the data have become more complex, they require more intensive documentation. NES data, like those from the other major surveys, are used extensively in teaching, which creates an overlapping, but different constituency for services. The electronic era has provided an improved means of extending these and other services. The NES web site, which itself takes considerable resources if it is to be a useful gateway to the project and its data for people with varying interests and skills, fulfills many of these roles, but cannot fulfill all of them. Beside the demands placed directly by the user community, part of the impact of the need for public responsiveness is that the staff and project leadership operates on the basis of an institutional norm promoting the creation of services for the larger community. Again, in terms of evaluations of NES as a project, especially in terms of critiques of its expenditure of resources, it is likely that most people take account of one main function — the generation of the production studies — and are at best peripherally aware of the other functions that go along with that central mission.

The point is that a “national social science resource” develops demands for auxiliary services other than mere data collection, and those services tend to be expected, but overlooked by members of the research community in terms of the resources they require. And, the demands for these other functions are rising rather than falling. Moreover, this mission has been internationalized to a considerable degree by the existence of ICORE, the Comparative Study of Electoral Systems, and other, often less formal organizations within the international community of elections scholars. NES, as the grandparent project, and one of the largest (although some other national election studies have now outstripped NES in the types and amounts of data they are funded to generate, as well as by the fact that some, such as the British Election Study, are funded for analysis), could not stay aloof from the international community. But that participation, especially in the forms expected of the U.S. participants as part of one of the richest nations, places significant resource burdens on the project participants.

## **CONCLUSION: THE INSTITUTIONALIZATION OF BIG SOCIAL SCIENCE**

The story of big science in the natural sciences has been told many times. It grew primarily out of war and defense efforts, and was guided much by the logic of “national needs” in defense and technology. The end of the Cold War, budget woes, and revitalization of a widespread emphasis on (if not reality of) small government have caused a rethinking of needs and the trade-offs with respect to big science budgets. The history of the social sciences can and has also been told in terms of relationships to national needs; many of the funding agencies, like many social scientists themselves, have explicitly seen their role as trying to shepherd the development of knowledge for the sake of addressing pressing problems of human societies. Of course few serious historians of either natural or social sciences seriously entertain the idea that science is merely a matter of competition of ideas. In this case, I have examined a case study of a particular “big science” project to explore the process of institutionalization and problems of this form of social science. I have used a framework that is designed to balance consideration of multiple agents and overlapping and nested structures.

When the first election studies were run at Michigan in the late 1940s and early 1950s, the principals were not just engaging in research that interested them; they had dreams of the creation of a unified social science that could respond to social needs. Nevertheless, in an important sense the early election studies were simply the research projects of scholars with aspirations, much as many others were. These studies happened to be embedded within a brand-new research institute, indeed run by some of the founders of that institute, who shared this vision of creating a particular kind of social science. The studies, and the associates of those studies, gained moral and financial support from the SSRC and other foundations also involved in attempting to set an agenda for a professionalized, unified social science. The methodology that served as the new specialty of much of the Institute for Social Research and certainly, for the election studies, was in its formative years as a tool for high-powered social science. These early days were, for all concerned, days of field and institution creation, a condition that offered great scope for the major players on that local scene to have widespread influence on the field more generally, even while their own work was in part shaped by the larger context.

The transformation of the project that took place in the late 1970s, and the maturation of all of the institutions and fields in which the project operates generates a very different context and set of positions the project occupies. Some of these configurations of simultaneous conflicting positions result in the sort of persistent dilemmas outlined above. But also, given the maturation of the multiple institutions in which

the project is placed – the National Science Foundation, the national government within which it operates, the University of Michigan, the Institute for Social Research, the Center for Political Studies, a highly professionalized set of social science disciplines, and its own bureaucratic structure, these dilemmas are likely to appear more rather than less intractable.

The National Science Foundation created a set of “national social science resource” projects with multiple year funding, but, at least until the end of the 1990s, when it designed a program for support of “infrastructure projects in the social sciences,” it never developed a set of norms or procedures that could comprehend the differences between these projects and the more usual research proposal. NSF-commissioned reviews of the project reveal the struggle of scholars attempting to perform a standard peer review of this project, along with others. This sort of project is more open-ended and complex; there are many elements of infrastructural support needed by an organization running a major data collection project, such as the auxiliary services discussed earlier, that are not normal parts of a research proposal in this field of the social sciences. Most difficult of all, there were increasing conflicts over the nature of the tasks that should be basic to the project, and those that should be considered elaborations requiring separate proposals. The research and development aspects of the project and data collection other than repeating time series questions became the particular focus of these debates.

From the point of view of project principals and the original designers of NES in the late 1970s, research and development work and innovation were part of the basic design; flexibility in design and content were crucial. The proposals for R&D work in particular tend to leave most of the planning and specification for after the grant is received for two reasons. First, unlike in an extended grant period for, for example, experimental drug trials, in which the methods must be planned in all details for the entire grant period, an extended period for survey studies of an ongoing historical social phenomenon, such as elections, requires flexibility in instrumentation. But second, the design chosen for the “national social science resource” projects require widespread community input into developmental processes. The NES process used for 20 years, of organizing R&D conferences followed by calls for proposals implies significant delegation of authority for disposition of research funding to the project. It is exactly this delegation, so uncharacteristic of the usual review process, that has caused special problems for reviewers and panelists in trying to apply normal peer review standards to the National Election Studies. In the face of the continuing relatively small size of the Political Science Program (a fact which also must

be taken into account) and the demands it faced, these were the major activities cut from the budget or scaled back dramatically in the final grant of the 20<sup>th</sup> century. As a result, the project leaders were ultimately forced back into the position of the original leaders of the Michigan Election Studies: They have turned to multiple supplementary proposals to support the research activities those involved in the field of electoral research have grown used to, including sustaining data collections that can support innovative research. How long the resulting structure of the job of leading the project can attract active research scholars is unclear.

NES must also be understood in the context of national politics, which affects it through the impact of national politics on the National Science Foundation.<sup>32</sup> The federal government took a long and complicated path toward supporting the social sciences, especially when it came to talking about basic support for social sciences as such rather than purpose-built programs within federal departments such as Agriculture or Defense. Even with the move toward supporting “basic” research, public policy justifications for support social science remains very tied to the applied purposes of solving social and economic problems, and the suspicion runs high and wide in many political circles that social science, far from being a “science,” is merely a reflection of ideology and politics. From time to time in the last half of the twentieth century politicians, including both members of Congress and the executive branch, took aim at federal support of social sciences. Sometimes this has happened through attacks on specific projects, as occurred in the course of Senator William Proxmire’s (D-WI) “Golden Fleece” awards, one senator’s critique of the NES Senate Election Study in the late 1980s, or some Congressmembers’ attacks on a study of candidate recruitment in the late 1990s (Maisel and Stone 1998). Sometimes, as in the case of the Reagan administration hopes of slashing the social science budget in 1982, or Rep. Robert Walker’s (R-Penn) attempts in the 1990s, the assault has taken more widespread aim. In both these cases the social science community gained valuable assistance from Republican and conservative pollsters, who were able to lobby with their fellow partisans about their interests in preserving support for these areas. NES thus faces two political problems. It is a social science project focusing on electoral politics. Moreover, although it contains a lot of material relevant to public policy, the applied policy relevance is not obvious.

The local home of the National Election studies is much changed from the earliest days of the project. No longer a medium for an idealistic vision, run by relatively young scholars in rapidly changing fields using new methodologies, it is now a venerable bureaucratic organization containing a set of distinct,

individual research centers heavily focused on the need to bring in funding to continue their existence. Funding, of course, was always a problem for this research organization depending completely on soft money, but it is now an institution that trades heavily on its experience, its status and professionalism, and its past. NES must also be understood for its position within that set of nested institutions. For the Center for Political Studies (CPS), it has been one of its largest sources of income and a source of prestige, although one with considerable financial and energy costs attached. It is, in contrast, a relatively small contract for the Survey Research Center, although one that carries prestige externally. A number of tensions surround the position of NES as a designated national social science resource, with a national constituency and Board of Overseers, hosted by one particular institution, where, in addition, the project is subject in substantial key ways to two distinct suborganizations: CPS, where it is housed, and SRC, which is commissioned to do the field work and thus, is the major site of the expenditure of resources.

The position of “host” for a national social science resource project is a difficult one, and creates its own ambiguities and conflicts. The research project is, at the same time, of the host institutional and not of the host institution. It is supervised, at the same time, by a national Board of Overseers, the larger set of “constituents,” and the bureaucratic leadership of the host institution. The organizational complexity of a large-scale, time series project, including the demands for continuity, institutional memory, and auxiliary services, mean the hosting function is not a metaphorical research guest house, especially not after 50 years of operation. At the same time, Board of Overseers and other associates who are not based at the host institution, as well as the national constituency for the project, is very sensitive to ways in which the host institution can treat the project as proprietary. In the case of NES, reviewers and others from outside the University of Michigan tend to view that institution’s relationship to the project with wariness, especially because of widespread views that research costs are high there.<sup>33</sup> But the point is that there are institutional reasons that go beyond the specifics of a particular host that make that position difficult.

Most of this paper has been devoted to exploring the issues that stem from the development of the particular project organization that characterizes NES. The object has been to understand this particular project as a case study in big social science rather than tracing its history in and of itself. This analysis suggests that, for a variety of reasons, the structure of big social science is structurally unstable. In the world of academic politics, the problems are often blamed on parochial politics, resource jealousies, and personality clashes. No doubt, these usually factor in. But social science is institutionalized, and can

be analyzed as such.

## ENDNOTES

Beside the sources listed in the Endnotes and References, the “data” used here were also generated by my observation of NES as a participant, particularly, as a contributor to the 1991 Pilot Study, as a member of the NES Board of Overseers (1992-1997), and as co-Principal (1997) and then Principal Investigator (1997- ) of the project. Views and interpretations expressed here are my own and not those of the National Science Foundation, the NES Board of Overseers, other Principal Investigators or staff of NES, or the Center for Political Studies. (Indeed, some of the views would -- at minimum -- annoy some of them.) More information on the current state of NES can be obtained from the NES website (<http://www.umich.edu/~nes>), which also contains information on how to contact the principals.

The more I review the history of the project, the more my attempt to understand it is dedicated in gratitude to Warren E. Miller. I would also like to thank Phil Converse and Dave Leege for taking the time to offer their observations on the project.

1. It clearly could not be a physicist who entitled an article commenting on his experiences on an NSF panel, “Gee! I’ve never spent \$5.5 million before” (Dalton and Siverson 1998).
2. To see the global scale and context, consider a comparison of the coincident experiences in the United States (Mervis 1997), Britain (Williams 1996), and Japan (Normile 1997).
3. For useful sources on the history of these studies and debates, see Lyons 1969, Klausner and Lidz 1986, Fisher 1993, and King 1998.
4. Although Bourdieu has studied the academy, I focus especially on his work on the arts (1993), which is interestingly isomorphic with the sciences for our purposes in terms of the layered roles of the creative person, the institution, and larger social forces.
5. I will distinguish here between the Michigan Election Studies (MES) and the National Election Studies (NES) to differentiate between the pre-1977 period and that following 1977, when NSF funded NES as a “national social science resource.”
6. Angus Campbell and Robert L. Kahn, “The 1948 Minor Election Study” file “INT48” in Sapiro, Rosenstone, and Miller 1998.
7. Oral presentation at the Annual Meeting of the Social Science History Association, November 1998.

8. Campbell, Converse, Miller, and Stokes (1960:15) claim that the first nation-wide sample survey pre-post-election study was conducted by the National Opinion Research Center in 1944. They traced only one Ph.D. dissertation and one master's thesis that had drawn on those data.

9. Converse claims that Miller was especially interested in him because of his background in English because Miller thought good writing skills were lacking among social scientists. Moreover, Converse says Miller urged him to avoid political science courses, but to continue in social psychology, which would be more useful for this area of investigation.

10. Although it is common to talk about "the Michigan School" and run the names of The American Voter authors together, these scholars had distinct research interests and styles, and contributed different pieces to the survey instruments and publications. The point must not be lost in assessing collaborative social science. Converse's contributions, for example, revolved around group influence.

11. In the 1970s graduate students would purloin copies of the draft chapters of the planned political representation book out of respect for their quality and fears that it might not appear in time for one's preliminary examination preparation.

12. This factor of graduate student influence eventually decreased, not necessarily because of lower interest in this kind of research among Michigan graduate students in political science but for financial reasons. The Graduate Employees Organization, founded in 1974, eventuated in contracts for research assistants that made them too expensive to include in large numbers on grant proposals because of the need to cover tuition costs. When, eventually, the real dollar value of grants to the National Election Studies (and other projects) began to shrink, reducing the number of graduate research assistants drastically became one of the standard methods of reducing the cost of operations.

13. I have drawn this history of NSF and the social sciences largely from Lyons 1969 and King 1998.

14. Warren Miller, Arthur Miller, Richard Brody, Jack Dennis, David Kovenock, and Merrill Shanks, "The CPS 1972 American National Election Study," file "INT72.CBK," in Sapiro, Rosenstone, and Miller 1998.

15. In 1972 Miller had simultaneously obtained and held funding for another major collaborative study, the 1972 Convention Delegate Study, the first in a series of five, the last of which he did with Richard Herrera in 1992.

16. Much of the following narrative about NSF and the NSF side of the NES story is drawn from an oral history with David C. Leege (July 1999), who was the program officer for Political Science at NSF during part of this time. All quotations of Leege are drawn from this oral history unless otherwise specified.

17. The initial proposal for long-term funding is a fascinating document, rewarding reading for students of the history of election studies.

18. Funding for Board meetings was reduced in later grants; per diem support was eliminated, and, in the 1998-2001 grant, funding for conferences was eliminated.

19. One of the most notable points of difference on the Board was the generally accepted idea that it should remain well-representative of two overlapping but distinct groups of scholars: the "elections



people,” whose primary interest in the project was specifically focused on understanding elections and electoral behavior per se, and the “political psychologists” (which actually also included political sociologists), whose primary concerns fell more broadly within the field of public opinion, political cognition, and political participation. While these constituencies overlap considerably, they can certainly tend toward different priorities in tight spaces. In addition, the Board has regularly included members whose research is not primarily survey based, and some whose research interests focus substantially outside of the field of political behavior per se.

20. NES budgets eventually were reduced over time in real dollars; the original grant for the years 1978-1982) grant averaged about \$579,000 per year; the 1997 grant (for the years 1998-2001) averaged \$490,000 per year in 1977 dollars.

21. For reflections on the history of PSID, see Duncan 1999.

22. The Planning Document and many other documents of the project are available at the CSES web site, <http://www.umich.edu/~nes/cses>.

23. At the memorial service for Miller in Ann Arbor in April, 1999, a colleagues who knew him well in the early 1950s commented that they recalled him at that time stating that his major goal was to transform political science into a science of this sort.

24. Indeed, to the contrary, a case can easily be made that all the active principals engage in significantly less research than they otherwise would because of their time and energy commitments to this project.

25. An exception to this statement: CPS included ICPSR from CPS’s founding until 1998.

26. I will use the term “unitary” as a short-hand to contrast with “omnibus”.

27. Discussion of reviews of the project is based on a reading of the anonymous reviews commissioned by NSF in the 1980s and 1990s and filed in the NES office, and other publicly available documents, such as published critiques of NES.

28. For a listing of the publications drawn from the election studies over the years, see The NES Bibliography at the NES website, <<http://www.umich.edu/~nes/resources/biblio/bibintro.htm>>.

29. Here it is crucial to point out that although NES must, to some limited degree, track responses to certain kinds of questions over time, the major purpose of the project is analysis of electoral behavior, electoral response, and public opinion and participation in the context of elections, not the tracking of public opinion, which is much more appropriately done by other means and especially, the vast resources of commercial polling data.

30. One prolific political scientist who often relies on NES data (but who has never been associated directly with the project other than attending an R&D conference) described the process of encountering each new study as “like opening up a wrapped Christmas present.” (Personal communication.) This may display unusual affection for social science data.

31. These are all available at the NES website.

32. Although Congress did, in fact, launch an investigation of private foundation support of the social sciences as part of the general climate of anti-communism in the early 1950s. See Lyons 1969:278.

33. The University of Michigan has relatively high overhead rates for federal grants, for example. The Chronicle of Higher Education reports the following overhead charge rates for federal grants for the top 12 public institutions of higher education in order of their total federal awards for research and development:

Washington: 48.5, Michigan: 52.5, UC-San Diego: 51.0, UCLA: 49.0, Wisconsin: 44.0, Minnesota: 47.0, UC-San Francisco: 47.0, Pennsylvania: 59.0, Pitt: 48.0, Colorado: 44.6, North Carolina: 44.5, Penn State: 41.7. (“Overhead Rates at top 100 universities in federal research-and-development awards 1998.”)

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