



History and Context in Comparative Public Policy

DOUGLAS E. ASHFORD, EDITOR

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History and Context

in Comparative

Public Policy

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Preface

THIS BOOK is the product of a conference on comparative public policy held at the University of Pittsburgh in May 1988. If the elapsed time seems long, it is in no small part due to the challenging issues raised by the participants and the provocative papers they prepared. It will come as no surprise to those who have organized conferences that the contributors insisted on talking about their own perspectives as well as the theme of the conference, so time was needed to sift through the various messages. I hope that the richness and diversity of their contributions permeates this book, for in my view, comparative policy studies have only begun to reveal the insight and knowledge that they could provide social science. In a nutshell, that is the theme of this book.

This tribute made, our meeting was about a project initiated nearly twenty years ago. In the early 1970s, I began to think about the inability of political science to deal with an action frame of reference and, after some years examining center-local relations in Europe, I realized that most macrotheories were incapable of dealing with microlevel complexities. Out of this grew the policy and politics seminar at Cornell, made possible with the support of the U.S. Office of Education and the Cornell Center for International Studies. In the late 1970s, the Ford Foundation provided a grant to enable me and my collaborators, T. J. Pempel and Peter Katzenstein, to launch a series of books on policy making and politics in the advanced industrial societies. The series was still growing in 1990 and, with the addition of Canada, now covers seven countries. Our effort to construct a comparable framework for these books has much to do with this book.

The book series was founded on the conviction, much more controversial in the 1970s than today, that macrolevel theories need to be tested against microlevel realities. *Realities* is of course a waffle word, but we were all aware then, as now, that policy-making studies often contradict macrolevel theories about particular sectors, public-private boundaries, and center-local relations. In a word, policy was not only the product of easily compared inputs and easily measured outputs but

also the product of historical experience, institutional traditions, and political opportunity. All these suspicions put us in opposition to the behavioral dominance of political science, but they also sensitized us to an action frame of reference.

As I think this book indicates, compared to progress in most areas of political studies, the advance of policy studies over the past decade or so is truly phenomenal. There are a multitude of new theoretical perspectives, an ever expanding realm of empirical interests, and, with some dispute, some fundamental qualifications to the deductive-hypothetical model that has prevailed in American and much of European political science since the war. In the history of political science from 1950 to 1990—and quite possibly the other social sciences—the growth of policy studies will have a major place.

After nearly two decades, it is difficult to write a concise account of all the persons and agencies to whom this book is indebted. The Cornell Western Societies Program provided substantial encouragement at the early stages. The Council of European Studies supported a research group that contributed to a conference on comparative policy studies early in our work, the results of which were published in 1978. The U.S. Office of Education provided funds that enabled me and my collaborators to teach together for several years and to thrash out the format for our policy book series. There followed a grant from the Ford Foundation to recruit authors for additional books. The conference in Pittsburgh to assess the series was supported by the Council for European Studies as well as by funds from the Western European Program at the University of Pittsburgh, the Graduate School of International and Public Affairs, the University Research Center of the University of Pittsburgh, and the Program on Comparative Public Policy. No specific accounting can be made of the many scholars who attended the conference and contributed papers, but the result is indisputably the product of cooperation and common concern over many years.

For one who has always been somewhat skeptical about professional isolation from the real world, this book provides much pleasure. If the net seems too widely cast or the conceptual focus too broad, it is because of my own conviction that political science has in a sense come full circle. Having spent a generation since the war showing that it can emulate if not reproduce the objective standards of natural science, political science is now engaged in a more vital process of showing once again (there was much less doubt a century ago) that it can contribute to the common understanding.

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Introduction: Of Cases and Contexts

Douglas E. Ashford

AN INTERESTING argument can be made that comparative policy studies have made fundamental contributions to theory formation in political and other social sciences. To fully appreciate the force of such an argument, it is necessary to momentarily, possibly even definitively, suspend the conventional foundations of behavioral science and reconstruct how social scientists generally, and political scientists in particular, seek to understand behavior. An important starting point is to recall that no more than a decade ago policy analysis was understood as a relatively primitive form of investigation; that is, it had little formal theory, virtually no consensus about preferred methodologies, and, despite an enormous amount of detailed cases (possible because of the enormous amount of case studies), little idea of how to generalize about its contribution to human understanding. Put simply, in the 1960s and probably well into the 1970s policy analysis had no informing generalization to guide research. For the most part, case studies were randomly selected and randomly used, often for such perfectly good reasons as instruction about the real world. More ambitious use of cases was still derived from mainstream issues and topical concerns. In a decade or less, policy studies—and more narrowly, comparative policy studies—have come a long way. A basic aim of this book is to point out that policy studies are no longer the handmaiden of established areas of political or social inquiry, nor do they remain methodological parasites on the back of political science, sociology, or history.

Because this volume highlights the diversity of approaches to comparative policy studies and, implicitly if not explicitly, the limitations of conventional social science approaches to comparative studies, it is worth reflecting on the common question about any case study: What is it a case of? Until fairly recently, the implied question really was, How can one relate this case to some lawlike statement about social behav-

ior? Not surprisingly, *case studies* became a term of reproach among professional social scientists, since they could be related to nearly any lawlike statement. The philosophical and epistemological limitations of cases should be made explicit early in the presentation of this book. It juxtaposes contrasting frameworks of social explanation and social understanding beyond the view of standard social science, which is that social science should emulate, if not reproduce, natural science by the prudent application of lawlike generalizations (Hempel 1965; Popper 1959). Indeed, the aim of the book is to redefine the meaning of cases and to differentiate their use in different frameworks of explanation—historical, ethical, and philosophical—as well as their application to normal hypothetical-deductive forms of political reasoning. Broadly speaking, until the 1970s queries about the use of cases for comparative studies were of only one kind: How can we fit cases into the established paradigm of explanation prevailing in political science as well as the other social sciences (Eckstein 1975; Lijphart, 1971, 1975)?

While it may seem trivial to note that the use of cases in comparative or other research design depends on the intention of the analyst, it is remarkable that other ways of using case studies have been effectively ignored. The reasons for this are not simply the tyranny of the mind imposed by a dominant form of explanation but are deeply imbedded in the various ways that nations historically related the social sciences to major national issues (Hennock 1976; Collini et al. 1983; Burrow 1981). Though not a major theme of this book, it is noteworthy that the renewed debate about the nature of social and political explanation was often ignited by studies that were essentially policy evaluations and critiques of the dominant theories of the moment (Crozier 1964; Lowi 1969; Beer 1973). Many of the works that raised new problems of social research were, among other things, complex policy studies that questioned important generalizations of conventional social science. They did so by constructing new contextual statements in order to link cases rather than relying on prevailing modes of knowledge accumulation. The authors did not necessarily dismiss familiar hypothetical-deductive models but were also sensitive to how their cases did not fit into larger lawlike assertions.

The essays in this book are intended to elaborate on how contextual meanings can be attached to comparative policy studies. The introductions to each section try to make such broad distinctions clear, but for the sake of clarity, if not analytical rigor, it may help to pose the most ambitious notion of case. A particular policy study is not simply a building block for one or more lawlike generalizations as conceived in normal social science but is simultaneously a historical narrative, a

portrayal of particular motives and intentions at work in a particular setting, an account of prevailing ethical and moral standards at work in political and social life at some moment in history, and an exercise in defining political and social reality for policy makers and the public. As these suggestions imply, a case study is amenable to many different interpretations—historical, ethical, and philosophical. But its intellectual fascination and, to some extent, its intellectual intransigence within conventional social science, arise from the ease with which a microscopic slice of reality can be manipulated. Put in more experimental terms, a case study invites the analyst to construct a context or, more simply, to disregard received wisdom when interpreting how the case might be linked to other cases. Doing so entails some form of generalization but not necessarily the conventional explanatory form of more rigorous social science. It is not entirely coincidental that one of the most respected practitioners of case study analysis and generalization, Wildavsky (1979), uses the metaphor of Diogenes in search of light to explain how his work relates to the business of government and politics.

The aim of this book is not to reduce comparative policy studies to incoherence but only to suggest that the language of normal social science does not address important issues about contextual knowledge. While it is true that many enthusiastic exponents of empirical social science expressed real doubts about the overall validity of scientific methods—Popper (1959) and Lakatos (1970) being conspicuous examples—on the whole, mainline social science was content to confine social science to the Kuhnian plateaus of learning and investigation. Cases thus became an important launchpad in the search for contextual knowledge. In this respect, it is perhaps fair to state that policy studies, in particular comparative policy studies, have had an important place in the paradigmatic explosion of social science over the past decade or more. For example, one distinguishing feature of critical theory is that the investigator is not only at liberty to explore the various meanings that might be attached to a study but is morally obligated to do so (Habermas 1973). Forester (1985) argues, for example, that a critique of the possible uses of a case study is an integral part of social research. Thus, some approaches to contextual questions not only reject the established aim of conventional social science but self-consciously demand that the investigator evaluate alternative interpretations. Less radical, but nonetheless closely associated in its intellectual demands, is interest in how policy makers themselves construct frameworks of reality or designs in making decisions (Linder and Peters 1988; Alexander 1982).

Perhaps the most elaborate attempt to rescue case studies for normal

social science is Eckstein's essay (1975) for the *Handbook of Political Science*. His elaboration of the appropriate use of crucial cases is an effort to relate case studies to conventional approaches to comparative politics and to integrate case studies within normal social science. Eckstein is not entirely happy with Verba's (1967) suggestions that cases be relegated to heuristic significance in the elaboration of serious social science, but he still sees no difficulties in limiting their use to the customary rules of behavioral inquiry. Eckstein (1975, 104) notes that "discipline configured" case studies may underestimate the problems of interpreting cases, and he expands on Verba's cautious suggestion to note the possibility of using case studies to find neglected variables to generate weak theories. His appreciation of the history of social science leads him to suggest that the elaborate cases expounded by Tocqueville and Bagehot helped generate theory. He is clearly uncomfortable with too radical an expansion of the meaning of case studies and is quick to qualify his argument about the detailed country studies that concentrated on the policy processes. Although the analytical process by which such elaborate studies of decision making are translated into comparative generalizations is not explored, he sees his own study of Norway (Eckstein 1966) as a useful example of such a plausibility probe (1975, 108-13).

Keeping in mind that in the 1960s Eckstein's interest in comparative case studies was adventurous, it is worthwhile examining his description of cases in order to see more clearly how these diverse and detailed slices of reality became, as it were, a minefield in the path of normal social science. Lest his exploration of case studies be underestimated, it is also important to recall that in the late 1960s standard behavioral research had become a virtual dogma of political science. Easton (1953) had dismissed the state as a meaningful unit of political analysis, and Almond and Coleman (1960) had launched their ambitious study that would presumably bring all political cultures within the orbit of behavioral research. As an academic product, case studies were firmly relegated to teaching devices for public administration and law schools, whose practical concerns were best distanced, if not ignored, by professional political scientists. Eckstein does not fundamentally differ from the dominant view of what constitutes social explanation, but he is sufficiently curious to give a revealing, and perhaps inadvertently a damning, account of the difference between what he calls "experimental" and "clinical" inquiries. Even a brief account will show that each of his contrasts between scientific research and detailed particularistic case studies leave some fundamental questions unanswered (1975, 81-83).

He first points out that in the social as in the natural sciences, experi-

mental studies are designed with a limited number of variables and in situations where the presence or absence of the variables can be empirically established. He contrasts this with the clinical situation that tries to capture the "entire individual" (1975, 81). The choice of words is significant. Consciously or not, he accepts the best advice of the leading behaviorists of the day that the most worthwhile unit of observation is the individual (Eulau 1958) and that the behavioral analysis of collective decisions is not promising. The perfection of individual behavioral studies will presumably provide more advanced methodology, allowing us to aggregate, translate, or possibly, as Kiser and Ostrom suggest (1982, 179–232), logically infer large-scale behavior from laws of individual behavior. Acquiescence to the rules of normal social science ignores the possibility that theories with less demanding rules of inquiry and radically different definitions of man might not find discourse about societies, cultures, traditions, and institutions as confusing. Though not explored in his account, in the Kuhnian sense Eckstein apparently condemns us to work on the plateaus of human knowledge. There is no mention that even a decade ago there were important arguments (largely outside political science) about the nature of explanation and discovery (Hansen 1958) that questioned whether science itself progressed by relying entirely on experimental methods.

Second, Eckstein correctly contrasts the routinized procedure of experiments with the intuitive and searching procedures of most case studies. Perhaps inadvertently, he uses the term *Verstehen* to characterize the kind of internal validity that other forms of explanation seek. There are major philosophical arguments about the difference between explanation and understanding in political science (Ball 1987; Bernstein 1976; Gunnell 1969), as well as fundamental ethical and philosophical controversies about the assumptions needed to understand human behavior (Moon 1975; Toulmin 1963; Hardin 1982). Though now largely forgotten, many of their questions were contained in the community power debate of the 1960s, but in the age of pluralist theory, the entire controversy over the substitution of method for theory (and implicitly, the limited understanding of complete reliance on routine experimental methods) was little noticed. It was of course essential to the defense of the pluralist school and to their approach to community power studies that the explanatory underpinnings of *Who Governs?* (Dahl 1961) be defended from charges that other political forces might not be as easily explained. Both Polsby (1960) and Wolfinger (1960) battled nobly on behalf of Dahl, though other Yale products raised questions. Lowi (1964), for example, suggested that organizations and leadership may independently affect city politics. Without reviewing

the entire community power literature (Hawley and Svara 1972), it is perhaps fair to say the impassioned methodological debate was at least *prima facie* evidence that the individualist assumptions of what was perhaps the most illustrious product of political science in the 1960s raised a multitude of questions about the completeness, even the impartiality, of routinized inquiry.

Third, experiments and clinical studies are contrasted in relation to their results. The former provide findings that confirm or deny hypothetical models, and therefore are presumably of no further interest in other settings and have no specific relevance to other theories, while the latter present difficult problems of interpretation. Again, without plunging into controversies over the nature of explanation, it is by no means obvious that natural science is limited to the particular explanation at hand, nor do natural scientists actually behave in this way. Precise methods permit all scientists to explore defined problems, but even in natural science there are often, as in the case of the discovery of the atom bomb, major controversies of emphasis, justification, and consequences. To be sure, rigorous methods clearly separate these issues from scientific inquiry, but even antiseptic inquiry may return to haunt science and to affect future research. Thus, interpretation may be no more than an explicit, if not rigorous, burden laid on the investigator to assess findings in more than one way, that is, in more than one setting or context. Its explicit recommendation is to assess the results in terms of their meaning to the human objects of the experiment and in terms of their probable consequences on human objects is not entirely unreasonable (Fischer 1985, 231–57). A more philosophical version of this critique is Goodin's telling comment (1982, 20) that incrementalism recommends thinking small so as not have to think at all.

Eckstein's last contrast also follows the convention that only experimental methods make persuasive claims on theoretical knowledge. Experiments accumulate in the form of generalized knowledge to reinforce theory, while the clinician's preoccupation with the unique and particular can only pursue action objectives. Even at that time, this formulation itself of routine social science was criticized by Bachrach and Baratz (1963). Similar objections are now echoed in certain versions of critical theory, in the renewed interest in deep structures (Geertz 1973), and in renewed controversies about historical explanation. The notion that social investigation inescapably has action objectives as well as pure theoretical objectives is not new to the 1980s but was advocated by many critics of behavioral studies more than twenty years ago (Mills 1956; Gouldner 1957; Berger and Luckmann 1966). Numerous studies show how hypothetical social research has real con-

sequences. One of the most fascinating examples is Burns's study (1941) of British unemployment policies, which indicates how the presumably value-free and hypothetical constructs used to assess British unemployment led policy makers to misperceive the problem and quite possibly to exaggerate its importance. The sociology of knowledge, where much of this discussion occurs, is not simply an unfortunate consequence of misconceived empirical inquiry but an elaboration of the link between research and its application. Behavior is affected by theories as well as findings. A central issue in more recent policy studies has been to understand how these linkages work and to explain the limitations of our own explanations.

For those interested in policy studies, there is then a sense of *déjà vu* in many of the more passionate contemporary controversies about the uses of case studies and their relation to human understanding. Whether imagined in historical, ethical, or action-based terms, the relevance of contextual knowledge to the evaluation of scientific knowledge has probably been clear for a decade or more. The theoretical debates that now encompass political science are in no small part due to the renewed search for contextual foundations to interpret scientific knowledge and to more systematically advance our understanding of unique and particular situations. In the diverse ways apparent in Eckstein's careful analysis twenty years ago, the contextual questions were never absent; they were simply not discussed. Not the least surprising effect of revived interest in contextual issues is the renewed effort to uncover how the exponents of experimental science actually thought about the matters that seem to have been so easily dismissed. Thus, it is symptomatic of the new vitality of contemporary social research that one can find, for example, essays linking Parsons's social action theory, often dismissed as a futile and reactionary effort to impose scientific rigor on all the social sciences, with Habermas's demands for moral sensitivity in social science (Clegg 1989, 129–48).

The revived interest in public policy, and in comparative policy studies, originates in two questions that the predominant pluralist theory of the 1960s, and the closely associated interest in rational decision making, could not resolve. Quite apart from the theoretical import of pluralism in the development of political theory more generally, behavioralism (as transplanted into political science after the war and over the 1950s) rejected the ideas of the state, legal order, and public administration as significant concerns of political inquiry. The new science of politics even rejected Laswell's efforts to build on the prewar work of Meriam, Gosnell, and others to produce a branch of political science focusing on problem solving. Eulau (1958, 49) perhaps the most

exacting of the behaviorists, considered the policy sciences "old public administration in a refurbished wardrobe." This sentiment was largely shared at Yale, where Dahl, Lindblom, and their disciples found pluralist behavior grounded in American ideals; indeed, in the behavioral comparative politics of Almond, it was extended to comprise all political ideals, all political cultures, and, apparently, all moments in history. There has perhaps never been such an energetic, and for a moment such a successful, effort to monopolize political thinking since the early nineteenth-century French positivist August Comte anticipated the arrival of high priests of humanity to provide universal knowledge for all societies (Bryant 1985; Hawthorn 1976).

By making normative and historical explanation irrelevant to political science, the pluralists had little interest in extracting more general meaning from policy making. Any possible structural coherence of inputs or outputs was of little intrinsic interest so long as political market forces operated freely. As Goodin points out (1982, 19) the theory contained its own circularity. If the pluralist dynamic precluded the possibility of knowing the effects of our actions before the individual entered the arena (one of their favorite words), then how would one know when an increment has taken place? In this regard, the incrementalist assumptions were essential to explain how individuals regularly arrive at the best possible policy, in much the same way that materialist assumptions elevate society in Marxism. That both were thought to be uniformly constructive suggests the high price of making ahistorical theories. There was, to be sure, a small qualification made to suggest that learning by doing might help us see more general theories or lawlike propositions underlying competitive, pluralist politics. Popper, for one, thought that causal chains might appear as incremental change took place (1959, 67). From here it was only a small step to the Caiden and Wildavsky argument (1974, 309) that thinking small was the most reliable of all strategies for policy makers and to the Landau (1974) argument that redundancy is a useful device to curtail impetuous policy makers.

For these reasons, Goodin concludes (1982, 56) that incrementalism is the dominant mode of atheoretical decision making. How to accumulate the experience contained in cases about similar problems, similar political systems, or similar situations was simply irrelevant. To contrast why differing systems found different solutions was equally unnecessary, since free political competition made such differences a natural consequence. It is perhaps worth noting that the stimulus for many of the early comparative policy studies did not come from the United States, but from Europe, and did not concern the wide array of policies

now commonly compared across countries but concentrated on different planning experiences. Not without a certain irony, the interest grew in the planning capacities (contrary to the pluralist logic) of European economies, where the various success stories appeared as complex case studies (Hayward and Watson 1975; Shonfield 1969). Without being nurtured by social science, interest revived, initially for economic and industrial policy making, in how microlevel behavior was influenced by—and in turn, exercised influence over—macrolevel behavior. From these and other studies, awareness spread that political systems had various forms of intergovernmental, interpersonal, and intersectoral preferences that contradicted the pluralist assumption that individual utilities explained collective behavior. From this point, it was a short intellectual step to suggest that more attention should be paid to how such connecting principles arose and acquired credibility and political legitimacy.

A second challenge arose as the early comparative policy studies revealed important differences in how various countries do the same thing. Closer examination of what Kingdon (1984) calls the “participants inside of government” and those outside “but not just looking” cast streams of light into the Eastonian black box. There was both a rediscovery of the bureaucracy (Armstrong 1973; Dyson 1980; Suleiman 1974) and behavioral comparisons of civil servants (Aberbach, Putnam, and Rockman 1981). The elaborate defense of mindless policy making became a statistical demonstration that budget growth was of little consequence (David, Dempster, and Wildavsky 1966). But policy studies were introduced through the back door of political science and again made respectable, in part, because they were shorn of the practical meanings displayed in public administration and, in part, because they were made compatible with behavioral science.

Thus, in the short space of thirty years, political science came full circle from a studied exclusion of policy making to an enthusiastic renewal of interest. As policy studies again became a legitimate concern of political science, a number of comparative studies in specific areas began to reveal deeply imbedded institutional and political differences in the policy process, in intergovernmental relations, and in government organization (Hecl 1974; Ashford 1982). Not only did the universality of pluralist decision making seem bankrupt, but the study of policy making began to invite theoretical attention. An important reason for the shift was that many of the conventional concepts of states, governments, and processes were contradicted by microlevel examinations. Ikenberry (1988), for example, shows that a presumably centralized and aggrandizing French bureaucracy responded more quickly and more effectively to

the oil crisis than more loosely constructed states. Kelman (1981) shows that a more concerned and more organized Sweden had more difficulty implementing occupational safety regulations than a decentralized and indifferent United States. Vogel (1986) finds that environmental regulatory policies contradicted conventional wisdom about Britain and the United States. Ashford's comparison (1986) of welfare state policy making in Britain and France shows that the prolonged and focused welfare policies of Britain failed to solve problems that France seemed to resolve with less effort. An array of economic policy-making studies were synthesized in Hall's study (1986) of the organizational and political interdependence of policy-making institutions.

Others would no doubt summarize the disciplinary revival of comparative policy studies in different ways or with different emphasis, but by the 1980s there was an unmistakable search for new concepts and theories to fill the void between detailed case studies and the abstract models of rational decision making, conveniently disinterested bureaucracies, and mindless political competition. Perhaps because the post-war behaviorists were so firmly wedded to the individual as the only appropriate focus of inquiry, political science took much longer than economics to recognize that market restrictions and imperfections imposed major qualifications on any market theory (Olson 1965).

Contextual propositions become important to policy studies and to certain other intricate patterns of interaction and exchange, such as international political economy (Krasner 1988), because one tries to generalize about numerous levels, sectors, and organizations where there is no valid reason to assume that the rules of behavior are uniformly determined. In philosophy, the resulting problem is known as the problem of commensurability (Phillips 1975), that is, the problem of how to reconcile substantially different forms of explanation or paradigmatic perspectives. Policy studies are replete with such problems. Partly because contextual arguments have been used to defend radical paradigmatic changes in social science (Habermas 1973) as well as to deny the possibility of making radical paradigmatic change (Hayek 1955), the issue has become controversial and at times inflamed with ideological difference. At the same time, except for those closely wedded to the most orthodox view of scientific explanation—the Vienna school (Carnap 1970)—there are now sufficient philosophical alternatives in structuralism, hermeneutics, interpretive philosophy, structural anthropology, and linguistics to take the question of contextuality seriously.

Contextual statements, then, are stipulations about realities in widely different situations that enable us to extend our understanding, if not explanation (Apel 1984; Dallmayr and McCarthy 1977), to cases that

escape conventional social science. As is explored more in part 1 of this volume, the use of historical explanations most clearly raises such issues, because the hiatus between levels of observation is closed to further direct observation. The translation of meanings over time always confronts the hazard of interpretation. Some areas of social explanation, most notably economics, have devised logically coherent methods to translate political and economic behavior into the same terms. Most economists use a very simple model of man, so it can be momentarily assumed within economics that rational behavior in firms, industries, and entire nations is identical. There are, of course, important reasons to doubt this is so, but a formidable body of useful economic theory has been built without calling on contextual knowledge. The justifiable notoriety of Olson's theory (1965) is based on his description of operational rules that appear to reconcile individual and collective economic behavior. Others schooled in the theory of the firm, for example, argue that the array of choices confronting complex institutions is vastly different from the simplified rules of choice that can be devised in economic organizations (March and Olsen 1975). But most postwar behavioral scientists were, and remain, highly vulnerable to radical and neo-Marxist critiques because they eschewed the analysis of collective behavior. In this sense, it is quite correct to argue that disinterest in the rules of commensurability encouraged political science to ignore collective behavior where all individual behavior could be simultaneously and accurately observed.

Context, therefore, is a conceptual device to compensate for the lack of behavioral rules and methods to compare behavior across time, space, organizations, and functions. In a word, the situations may be so different that existing behavioral methods do not apply, but at the same time there may be striking similarities and differences of behavior that we want to examine systematically. Indeed, one of the simplest forms of contextual argument in policy research is quantitative cross-national studies of decisions for which there are no clear behavioral rules to establish cross-national equivalence. British, French, and German budgeting and taxation, for example, display radically different behaviors, but the highly abstract formulations of spending decisions provided by accounting data (Cameron 1982; Hibbs 1977) can demonstrate the parameters, if not the actualities, of budget behavior.

Although they make important contextual assumptions, it is obvious that these studies are at one level of analysis, the state, and are confined to data that are intentionally standardized so as to erase major behavioral differences known to exist at the microlevel. No one, including the investigators, believes that the results of such studies say anything about the actual behavior of key political, administrative, and

business actors. Indeed, as more detailed information becomes available, as was the case with recent biographical and detailed studies of the French planning process (Ashford 1987), the macroexplanation of public spending differences as reflected in national accounting data may be totally at odds with the issues and conflicts decision makers actually experience. Still, most would agree that highly aggregated structural studies detect important government differences as did similar studies of big government (Rose 1984; Rose and Peters 1978), but the apparent simplicity of aggregate data and the familiarity of the statistical methods meant that the underlying contextual assumptions were never made entirely clear. The risk is, of course, that those less sensitive to the limitations of aggregate data would make unwarranted claims about these countries being alike or different or attribute differences to underlying behavioral causes, when in fact the causes were no more than statistical constructs.

A second familiar use of contextual concepts is to link microlevel studies to institutional comparisons. The minimalist behavioral definition of institutions, the authoritative use of power, intentionally avoids differentiating institutional behavior from any other kind of behavior. March and Olsen (1983, 1984) identify more precise distinctions that might be used in a modern theory of institutions (there were numerous theories about democratic institutions in the nineteenth century), but there are as yet few policy-based hypotheses about democratic systems. Few would argue that conventional behavioral studies of party, parliamentary, and administrative behavior can be conflated to provide a more general theory about institutions as such. What does happen, of course, is that behavioral findings treat aspects of institutional behavior. Few such studies make any claim on our understanding of the content of policy making, leaving us with what Kingdon (1984, 122) calls primeval soup.

Another version of what might be called quasi-institutional theorizing from case materials is to use well-developed theories, as Hall (1989) does with Keynesian economic theory, to assess institutionalized economic behavior in complex situations. The qualification *quasi* must be inserted: first, there are obviously enormously important economic institutions that Keynesian theory leaves untouched as unrelated to economic macrotheory; second, much of Keynes's influence was felt after his death, so there are the major problems of interpreting his concepts in practice; and third, important historical differences are introduced by the timing and situations when policy makers translate Keynesian principles into public policies. These are empirical questions, to which we can bring our best historical efforts, but they are all interpretive

problems of how economic institutions are defined using an analytical terminology not designed for institutional generalizations. In translating institutional behavior through abstract systems of social thought, we are at the mercy of the translators. This causes immense problems of the sociology of knowledge, most apparent perhaps in the lively debate over whether Keynes should be considered an apologist or a critic of modern capitalism (Hall 1989; Meltzer 1989; Clarke 1989; Parsons 1985; Booth 1983).

There are no clear rules of commensurability to link the macrotheory and microlevel behavior, so the contextual assumption, that Keynesian thinking characterizes most democratic economic policy-making machinery for a given (and disputed) period, is essential. This is one of the intriguing instances in social inquiry where heat may be more important than light. The dispute discloses the importance of interpretation as a prerequisite to linking macroevents and micro-events. The problem is whether the hypothetical assertion of causal relations (Keynesian macroeconomics) in its application to actual circumstances produces a particular interpretation of capitalism or only of capitalism superimposed on Keynesian ideas. For this reason, contextual arguments can be accused of circularity. The contextual stipulation (Keynesian theory) cannot be totally excluded from the alleged causes of change.

Another contextual approach to the study of institutions (Ashford 1978) is to forgo the potential reification of interpreting events with disciplinary theories by using independently established institutional generalizations appropriate to a given country. There is a strategy (Anderson 1975), if not a principle, behind a procedure that sifts through social and political history of institutions and, in normative terms, the country's early choices of how to institutionalize democratic governance (Nairn 1988; Bouvier 1986; Rudelle 1986; Harden and Lewis 1986). Of course, the same problem of interpretation returns in different interpretations of historical influences and thematic consistencies but in more explicit ways than in abstract social science models. Both involve important judgments about intention and history.

The most clear-cut case, and perhaps the most persuasive one, was in Britain, where constitutional, judicial, administrative, partisan, and electoral institutions appear so well suited to adversarial politics (Finer 1975). A strong case can be made that, since the war, British policy questions have been governed by adversarial assumptions first developed in the seventeenth century (Ashford 1981). More detailed case studies of center-local politics (Ashford 1982) and social welfare (Ashford 1986) provide consistent evidence of adversarial behavior at

the microlevel. This is not a functionalist argument in the sense that British jurists from Coke to Dicey set out to make but only a microlevel observation that the agencies and officials of British government have remained highly susceptible to adversarial influence for centuries. The historical and philosophical reasons are not necessarily based on policy needs or even on conscious processes of articulating British democracy (Pocock 1975) but are contextual realities that can be regularly confirmed at many levels, across many sectors, and in various kinds of institutionalized behavior within Britain. This approach to institutionalization is similar to that suggested by Krasner (1988) for transnational behavior.

The method has the virtue of arriving at institutional explanations that are unmistakably germane to the country in question, though of course subject to the charge of circularity that also arises when a macrosocial or macroeconomic theory is superimposed on microlevel experience. Depending on how one thinks theories are made, causal links may seem clearer when a macrotheory approach is applied to cases. But explicit links to institutional principles are equally available from speeches, debates, and policy materials (Hennock 1976). Explanation rests on the consistency of decision-making patterns, reinforced and often explicitly reasserted in microlevel policy making. The microlevel argument is not about some general feature of social or policy choice but is grounded in institutional, administrative, and legal conditions of exercising authority.

For these reasons, recent writing on corporatism, clientelism, networks, and linkages during the 1970s was a radical departure from behavioral requirements of the 1950s and opened the way for more ambitious studies of comparative public policy. The search for a logical explanation of how interest intermediation worked almost inevitably led back to microlevel questions concerning public policy. As these terms suggest, the first efforts were limited to the conventional social science assumptions; that is, that microlevel explanations were analogous to macrolevel explanations. Behavior that did not follow the same logic or observe the same rules was simply ignored. To return to the Keynesian illustration used above, the macroassumption was that actors in budget offices, investment banks, and credit agencies could be subsumed under an abstractly defined law of aggregate demand. In a sense, this is correct, but in terms of actual institutional behavior, thousands of agencies made decisions affecting the Keynesian model. The point is not that the hypothetical parameters of the Keynesian model are wrong but that the aggregate effect is both contextual and objective. The uniform objective effect may or may not reflect similar behavior in

all the microlevel agencies. Thus, intermediation within economic policy making, for example, is not simply tracing mirror images at lower levels of decision making (or within the stated hypothetical reasons) that assume microlevel consistency with abstract theory.

These methodological problems lead to the third strategy this book is concerned with: Is it possible to devise general, possibly even lawlike, statements about microlevel policy making that might then be compared, confirmed, or denied at higher levels of institutionalized behavior? The distinction is an important one for policy studies. The aggregate-data solution, discussed above, essentially confines generalization to questions where information makes the characterization of behavior on two or more levels uncontroversial. What is left out is of secondary concern, even though these studies can reasonably claim their findings apply uniformly to all levels of behavior. Whether it explains similar or different behavior at all levels is not demonstrated. The abstract model solution is an advance insofar as it suggests that the hypothetical general law of behavior can be directly measured and is equally applicable across units, levels, and sectors. The relevant behavior is made explicit rather than implicit. But the guiding principles or rules of behavior at lower levels are still assumed to be consistent with the general theory. The third step, like earlier arguments about the independent effects of policy, is to show that features of microlevel behavior to some extent constitute, if they do not formally explain, general institutional features of the state.

The third group of comparative suggestions arises from the critique of pluralism, the search for better explanations of interest mediation, and the continued pursuit of organizational models of decision making under uncertainty. To be sure, concentrating on the microlevel in order to capture the policy-making context has certain disadvantages. For one thing, the sweeping generalizations about massive social change or pervasive economic forces (capitalist or socialist) must be forgone, at least until better ways are found to understand how officials work. For many, the sacrifice of familiar political and social concepts about class politics, electoral behavior, partisan behavior, and well-known macrosocial theories about modernization, mobilization, and protest (Ashford 1991) is asking too much. Second, at least initially, such generalizations are likely to be valid only for given institutional settings and therefore most useful for intrasystemic comparison. Again, the possibility that there are multiple forms of rationality distinguishable in the rules, procedures, and traditions of institutions contradicts the common assumption in economic and social theory that individuals are consistently maximizers and optimizers. In fact, in complex situations persons may

not be consistent at all, and their inconsistencies may only become understandable in the context of institutional settings.

Third, such investigations emphasize how officials actually communicate, so there is an implicit radical implication that policy making involves advocacy. Such institutional contextual theories as policy style (Richardson 1982), organizational garbage cans (Cohen, March, and Olsen 1972; Dryzek 1983), or policy design (Anderson 1977; Alexander 1982; Linder and Peters 1988) also make few if any presuppositions about the resources, tactics, or channels through which policy makers operate. There are no agreed upon independent variables but an array of possible independent variables among which decision makers choose. They select, according to their best judgment, how to obtain a desired outcome—and, in this broad sense, are rational. But the rules of being rational, or the evidence of external influence, may not identify an outcome in a more general sense. Essentially, decision makers are advocates; perhaps they are not impassioned Habermasian communicators or driven ideologues, but, nonetheless, their perceptions and behavior are governed by the best solution within given situations. Perhaps the best characterization has been provided by Sabatier (1988), who discusses “policy-oriented belief systems.”

Interest in these more subjective internal characteristics of policy making also means that the functional division of labor in policy studies, the subject of the Furniss chapter, no longer works. The policy and politics book series from which this conference took its inspiration makes a similar assumption: that is, there is no reason to presume that any given policy arena necessarily takes precedence in explaining institutions and policy-making behavior. This does not mean that there are no compositions or ordering of the content of policy making but only that ordering features are not likely to be discovered by conventional hypothetical-deductive models of policy making. Policy makers work within restraints, but they are not the abstract political, economic, and social constraints common to social science theories, nor the obviously pertinent empirical questions of how general features of government relate to the amount of spending, nor the even less understood connection between abstract theories of economics, sociology, or (insofar as any comparably reliable theories exist) political science and microlevel behavior. If there is any finding of policy studies that seems incontrovertible, it is that policy makers rarely follow models, especially social science models.

The shared characteristic of these more subjective concepts of policy-making behavior is their concern to see the policy process in the eye of the beholder. To be sure, policy styles are situated within particu-

lar governing traditions and institutional frameworks, which are well known to policy makers; the garbage can does not receive information, options, and choices in a wholly random manner; and the policy design never emerges from a completely blank mind. As Farr argues in chapter 7 and Skillen in chapter 9, there are situational factors at work, as well as moral foundations at all policy-making levels. Rooted in the sociology of knowledge, shared techniques, social and economic concepts, and technological experience form part of the decisional setting for policy makers, as discussed by Wittrock and Wagner in chapter 10. At this point, the aim is only to show that the analytical choices for the comparative study of policy making are by no means limited to behavioral and quantitative techniques subsumed by political science.

As mentioned early in this chapter, the breadth of this book is intentional and will, we hope, enlarge the range of controversy and interest in comparative policy studies. The first part is in some respects the most adventurous in linking historical explanation to policy studies. History shares with policy studies the problem of evaluating intentions, and longitudinal studies of public policies by historians seem to have difficulty taking their appropriate place in enlarging the dimensions and depth of comparative policy studies. Analytical essays have been mentioned, but essentially they are illustrations of underlying issues of policy analysis that comparison might clarify. In an inversion of the usual sequence of more specific to more general, the final part of the book deals with the discipline itself, as it sifts through its findings and theories, hoping to see how, if at all, some of the traditional concerns of political science might provide guidance for new perspectives on comparative policy studies.

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I

IN SEARCH OF TIME

2

Historical Context and Policy Studies

Douglas E. Ashford

IN CONSIDERING the relation of historical knowledge to the analysis of public policy, it is instructive to begin by recalling how, if at all, the social science disciplines have treated history. Broadly speaking, the behavioral revolution meant that in political science and economics the presumption of rational self-interest made historical generalization and historical knowledge superfluous. There were important exceptions, such as Hirschmann (1977), Heilbroner (1963), and Gerschenkron (1943), but they were products of a European intellectual tradition where economic and political history had never been so energetically excluded from respectable social science. As time passed, economics departments had less and less use for history, even though one can still occasionally find a department with a bemused economic historian who survives more or less on collegial tolerance. Political science never embraced an impressive body of historical writing such as Commons and Beard bestowed on American economic history. The tedious necessities of large data banks not only limited study time to the available quantitative records but oversimplified the comparison of historical continuities and historical change. Only a bit more than a decade ago, I attended a conference where an analyst of French communal budgets, laboring to extend a data bank to 1871, was mystified until someone told him of the Franco-Prussian War.

Anthropology and sociology were by no means unaffected by the behavioral revolution, but their intellectual antecedents and subject matter meant that historiographic, if not genuine historical, controversies were never totally dismissed. Dealing with artifacts, remnants, and incomplete records, anthropologists and archeologists have always had to reconstruct the past with imagination as well as zeal. Though less visible now than a generation ago, anthropology and archeology has always had one foot planted in classical, humanistic learning. The rela-

tion is somewhat different in sociology, but the founders of the discipline were accomplished historians, and the discipline grew from a late nineteenth-century fascination with the "laws" of history. As Abrams writes, "In some fundamental respects the two disciplines are trying to do the same thing and are employing the same logic of explanation to do so" (1982, ix). As a result, sociology never suffered from the exclusion of history. During the 1970s, the connection between sociological analysis and history thrived, while it was virtually excluded from political science (Anderson 1974; Stinchcombe 1978; Stedman Jones 1976; Thompson 1978). The debate concerning state and society revived many traditional controversies about social history (Skocpol 1984).

This brief excursion into the history and place of longitudinal analysis in the disciplines helps us understand why, in some respects, restoring a time dimension to political science—and by implication, to policy studies—has been delayed and often resisted. In the behavioral revolution of the 1950s and 1960s, neither the remnants of historical scholarship preserved by the history of political thought nor the policy foundations of constitutional law were able to press their natural links to policy studies. Political theorists were for the most part disinclined and even disinterested, though the core problems of uncertainty, obligation, and choice that permeate official decision making are the stuff of political thought. Indeed, most of the giant figures in debates over democratic theory—Hobbes, Locke, Bentham, and Mill, to name a few—frequently gave policy advice, drafted legislation and constitutions, and were actively engaged in the policy debates of the day (Cowling 1967; Harrison 1965; Heyck 1982; Finer 1972). Legal and constitutional scholars were in some ways less harshly treated, in part because such intense issues as the civil rights movement, Vietnam, and the cold war preserved interest in the foundations of political justice. Even so, neither problem solving in the Laswellian mode nor the practical application of behavioral methods to policy issues for opinion measurement provided the grounds for the systematic theoretical integration of policy studies into politics nor for their subsequent intellectual legitimacy within the discipline.

As noted in chapter 1, behavioral politics helped revive comparative politics but did so with highly abstract theories of modernization, communication, and perception that reinforced behavioral presuppositions (Lerner 1958; Rostow 1963; Almond and Verba 1964). These studies showed little interest in internal structural relationships, such as center-periphery conflicts, or confounding social relationships, such as clientelism or corporatism. By definition, a clearly articulated macro-micro foundation, and therefore concern with contextual questions, was

not only unnecessary in the pursuit of empirical knowledge but actually threatened behavioral constructs by exposing the limitations of behavioral social science. For this reason, such enthusiastic advocates of behavioral social science as Almond, Easton, and Dahl simply banished such lumpy ideas as *the state*, *sovereignty*, and *social class* from the vocabulary of politics. It is interesting to note that one of the most prominent early political sociologists attached to this group, Lipset, initially performed a detailed historical and structural study of the origins of Canadian socialism (1950) but later unreservedly subscribed to the behavioral model (1960), which erased time and situation as elements of human understanding. All of these dispositions were of course antithetical to the development of historically sensitive political analysis and, for reasons illustrated by the essays in part 1 of this book, erected formidable barriers between political and historical scholarship.

Perhaps the most succinct way to illustrate how historical research might contribute to comparative policy studies, both substantively and methodologically, is to examine historical writing whose relation to policy making is unmistakable. Biographies are perhaps the most important, because underscoring how uniquely placed figures manipulate authority, perceive policy choices, and use political institutions through time directly challenges behavioral assumptions. Biographies are among the richest sources of detailed, microlevel information about policy making, but they are understandably among the most confusing sources for those working with macrolevel theory. First, their interpretation rests heavily on subjective, motivational factors, which more rigorous social science, above all behavioral political science, tries to eliminate. Second, the relation of major actors to any situation is always problematical, depending on such highly variable circumstances as tactical advantage, partisan opportunity, and even chance encounters. Third, macropolitical studies often stipulate a uniformity context in order to claim equivalence in explaining how policy makers attach priorities to problems, weigh information, and manipulate the institutionalized levers of power. In sum, biographies are unique statements of famous persons and, except for the occasional moralizing and commentary, make no claim to universal or lawlike knowledge.

Against these conceptual and descriptive hazards one must also weigh the richness and persuasiveness of the various kinds of information that biographies contain at the microlevel. First, biographies help establish and clarify the institutional continuities and political decision making in a way that behavioral analysis cannot. The famous British controversy about individualism and collectivism discussed in chapter 3 by Kumar, for example, not only links political ideas to Brit-