

Theory and Methodology in the Study of Comparative Public Administration

B. Guy Peters

This paper addresses methodological and theoretical problems in the study of comparative administration. This is difficult, given the varied meanings of key words such as “comparative” and “administration.” To some degree, this must be a paper about some general questions and problems in comparative social research, given that only the particular subject matter differentiates the concerns of this paper from other research in comparative government. The difference of subject matter is, however, sufficiently important that some of the intellectual roots of comparative administration must be explored. Further, as I and others (Lundquist 1985; Heady 1991) have argued elsewhere, theoretical and methodological issues are central to the failure of comparative administration to fulfill the expectations many scholars had for it during the 1960s and 1970s. That is to some extent true for comparative politics as a whole, but is particularly true for comparative administration. Fred Riggs’s optimistic forecast (1976) of comparative public administration becoming “the master science” in the field of public administration is not yet close to being realized.

INTRODUCTION: THE INTELLECTUAL ROOTS AND THEIR IMPACT ON THE STUDY OF COMPARATIVE ADMINISTRATION

The study of comparative public administration involves elements of at least two broader strands of social inquiry. First, the principal substantive focus of comparative administration is the structure and activities of public administration and public administrators. This concern raises a number of related questions about the efficiency and effectiveness of administrative systems. In this first body of literature, the variables and subjects of investigation are either at a micro level (human behavior within organizations or between clients and administrators), or are premised on shared and largely unquestioned values of a single political system.

Although administration occurs within a particular socioeconomic and cultural setting, those external values are largely irrelevant for most studies of public administration. On the other hand, most comparative public administration examines organizational structures and administrative behavior within more than one cultural and political setting. At a minimum comparative public administration involves comparison of the administrative system in one country different from that of the researcher and involves at least an implicit comparison with the researcher's own national system. This second intellectual root of comparative administration requires attention to questions of comparative research of all types, albeit with an emphasis on the comparison of institutions and the behavior of actors within institutions. Coming as it does from these two backgrounds, comparative public administration contains some elements of each and some of the intellectual problems of each. Further, blending the two traditions presents some additional intellectual problems.

Public Administration

First, from the perspective of public administration, there are important questions about the definition of administration and of bureaucracy and about where the researcher can delimit his or her concerns. For example, should we include political appointees in the United States as a part of "administration" because they occupy roles at the head of administrative organizations, or should we consider them as a part of the political executive (Hecklo 1977; Aberbach and Rockman 1983)? Should we, in fact, include ministers with the political responsibility for managing public organizations as a part of administration, or are they better dealt with as part of the "political" government? At the other end of the spectrum, do we include clients within the system and assess the impact of their characteristics on the functioning of public organizations? To what extent should private organizations performing public functions be included as a part of administrative apparatus of government (Hood and Schuppert 1988; Milward 1991)? This question becomes even more acute as numerous scholars advocate "networks" and "polycentrism" as the best approaches to public administration (Laumann and Knoke 1987; Klijn and Teisman 1991). These are all definitional questions with substantial impacts on what we can uncover when inquiring into comparative administration.

In addition to empirical, definitional questions, there is a question of where the boundary between normative and empirical questions exists in public administration. Indeed, the fundamental question is whether there is, or should be, such a boundary. To what extent should public administration be primarily concerned with the analysis and description of existing structures? Conversely, what role should the study of public administration play in the design of new government institutions (Leemans 1976; Pollitt 1984; Lane 1990; Olsen 1990), and in changing managerial behavior within existing bureaucracies? These questions exist whether administration is being studied in the United States or any other country, but become especially

acute in a comparative analysis. Public administration does have a practical and reformist side that comparative political science rarely has, at least overtly, and the important role for comparative research in policy prescription must be addressed. This is true even after the untimely demise of much of our interest in development administration (Esman 1980). Adopting a reformist and involved stance, however, quickly puts us crosswise with our more “pure,” scientific colleagues doing comparative politics and comparative sociology.

Comparative Politics

The research questions arising in comparative politics are even more vexing for the scholar. First, much of the tradition of comparative politics has been that, in practice, “comparative” means politics somewhere else, rather than an emphasis on genuine comparison; I have characterized this tendency elsewhere (1988) as the “stamps, flags and coins” approach to comparative politics. Therefore, the field has been predominantly descriptive rather than analytic. There are, of course, numerous important exceptions to that unkind generalization about the state of the field (Collier and Collier 1991). In addition, many single-country studies in comparative politics have potential theoretical importance and address theoretical issues from interesting perspectives (Power 1990). The pronounced need remains, however, to emphasize the direct comparison of political systems. This continuing need is especially damning given that comparison is the only laboratory open to most social scientists, and the generation of comparative statements appears to be the principal route to theory construction (Lijphart 1971; Dogan and Pelassy 1990). At present the comparison of administrative systems is even more primitive than the comparison of whole political systems or of other components of political systems such as elections and legislative behavior (Dalton 1988; Laundry 1989). We simply have not had either the theoretical approaches for deductive analysis (other than the ideal types mentioned below), or comparable data for inductive analysis, that might make directly comparative work in public administration readily “doable.” While the existence of usable deductive models, for example, public choice, may be questioned (Bendor 1990), it appears clear that we have not had the databases nor the agreed conceptualizations necessary for more empirical work. Further, the relative state of ignorance of even many country and area specialists about administrative systems (sometimes including their own) implies that descriptive analyses of public administration in individual countries can be of greater value than similar descriptions of parliaments or party systems.

Associated with the descriptive character of much of comparative politics is a static quality in much of the work. The existing literature is much better at describing the status quo than it is in explaining the dynamics of the political system(s) in question. While the literature on Third World countries often has a prescriptive element concerning change and “development,” little of the literature on the First World is useful for understanding changes and particularly not for advising

governments engaged in reform efforts. This is true despite the importance of continuing efforts at change and reform in most political, and especially administrative, systems (Olsen 1990; Caiden 1991; Peters 1991). We as scholars of comparative politics are faced with massive political changes, but often appear to lack the tools or the inclination to do very much to shape those changes. To be of greater utility, therefore, comparative politics, and comparative public administration, needs to be able to speak more effectively, both descriptively and prescriptively, to the problems of change.

A final question about the study of comparative politics relevant here concerns the relationship between the systemic and individual levels of analysis (Jackman 1985). Scholars often skate between ecological and individualistic fallacies and may fall into one or both. Researchers characterize whole systems and assume that individuals occupying roles within those systems behave correspondingly. On the other hand, we can characterize the behavior of individuals in political roles empirically and then assume that the encompassing systems will behave similarly. Therefore, a major challenge to comparative politics continues to be developing an ability to link the micro and the macro levels of analysis and to be able to make meaningful statements about both.

Mixing Comparative Politics and Public Administration

It should not be surprising, given the descriptions of the two fields offered above, that melding them is also difficult. One is a field (public administration) that tends to be ethnocentric, micro-level for much of its work, somewhat descriptive, but at the same time is normative and ameliorative. The other (comparative politics) is also often descriptive but presses vigorously and self-consciously toward nomothetic statements. It strives (often with limited success) not to be based solely on the experiences of industrialized democracies and to be “scientific” rather than practical or reformist in its orientation. Further, comparative politics tends to focus its attention on the macro level, and countries constitute a major unit of analysis, as well as the major (presumed) source of variance in its studies even if the data themselves are micro-level. The flowering of comparative public administration during the heyday of development administration meant that it acquired more of a practical and reformist bent, but the roots of the field in comparative politics might make it less practical.

The variety of intellectual problems facing comparative public administration has generated numerous scholarly doubts concerning the viability of the field (Aberbach and Rockman 1983; Sigelman 1976). This current skepticism follows several decades of great optimism and enthusiasm about the contributions of, and prospects for, comparative administration. I have argued elsewhere (Peters 1988) that much of the current malaise is a function of the apparent absence of accepted and easily operational dependent variables. Published work in comparative public administration rarely looks as “scientific” as that published in other areas of

comparative inquiry. The observation of the “unscientific” nature of comparative administration often was made in contrast to the apparent successes of comparative public policy studies. Interestingly, there is now substantially more skepticism about the progress of comparative policy studies, especially that work based on easily identified and utilized quantitative data such as public expenditures. The world of public policy may actually be more complex, and require substantially greater contextual and institutional knowledge (Ostrom 1991), than has been assumed by some analysts. Rather than being peculiar to comparative public administration, the malaise of comparative studies may be a very widespread phenomenon.

Although it has more company than often thought, comparative public administration appears to remain in the doldrums (Aberbach and Rockman 1983). This apparent malaise is not a function of an absence of interest, as the recent successes of some journals and other scholarship in the field clearly indicates. Further, the connections of comparative administration to several broader strands of inquiry are not entirely disadvantageous. Although some problems are shared across these fields, some strengths are also shared. There is no lack of interest and research opportunities, but a number of important theoretical, methodological, and substantive questions remain unanswered about comparative administration. These questions must be addressed if this area of inquiry is to progress. After this perhaps excessively long preamble, I will now discuss some of those questions. As well as detailing the problems, I will try to provide at least some inklings of answers that may be beneficial for the continuing work in this field.

THEORETICAL PROBLEMS FOR COMPARATIVE PUBLIC ADMINISTRATION

The first set of questions we will raise about comparative public administration are primarily theoretical. These to some extent return to the questions already discussed (Peters 1988) about the appropriate focus of inquiry for this field of study. What are we trying to explain? What is the appropriate boundary of our study, and how does it relate to other concerns within public administration and comparative politics? We will discuss these questions from a theoretical perspective here, but any choices made about the focus of inquiry will have ramifications for the methodological stances that are required. There are some basic questions about methodology implied in the selection of theoretical foci, because certain methodologies usually associated with the “scientific” thrust of the social sciences may be inappropriate for approaches concerned with more holistic and humanistic questions. In the complex world of administration, identifying independent and dependent variables may require as much faith as science, so that somewhat less precise methods and language may be useful.

What Do We Want to Know?

The most basic question is what do we want to know about comparative public administration? As Richard Rose once wrote, “First, Catch Your Dependent Variable.” As noted, most of the history of this field, as indeed of comparative politics more generally, has been descriptive. There are a number of excellent descriptions of the structure of administrative systems (Timsit 1987) and of the behavior of individuals within those systems (Suleiman 1974). If that were the focus of our work, we might terminate the paper here, for there would be little need for an extensive discussion of theory and methodology. Even if the focus were descriptive, however, we might have some implicit theoretical questions and associated methodological questions. Single country studies, even if descriptive, can have substantial theoretical importance if selected properly and motivated by appropriate questions (Lijphart 1975; George 1979). If, however, what we want to know is more theoretical and analytic, then theoretical and methodological problems become paramount.

If we assume for the time being that what we are after is more nomothetic statements about administrative life, then we must confront the substance of that desired knowledge. On the one hand, we may not want real comparative knowledge, but instead may be seeking universals about the behavior of individuals within public organizations. At the end of research that level of theoretical knowledge might still be the outcome if we are able to remove the nominal country titles from variables (or actually packages of variables) and assign to them other, more conceptual, names. In the short run, however, the questions remain about what can we learn within particular national or subnational contexts that can be used to build broader theoretical statements about administration and its relationship to the rest of policy-making and politics. A universal theory of public management will have to wait.

I once (Peters 1988) offered a set of four possible dependent variables—people, structures, behavior, and power—that captured some of what we would want to know. First, we need to know who is in public administration—their skills, values, and socioeconomic backgrounds. This is important not only for sociological voyeurism but also because who is there will influence what can and does happen. Also we need to know something about the structures of public organizations. Despite heroic efforts we still lack usable and comparable means of classifying the structures of government departments or of the entire populations of public organizations in a country. We also need to know what the members of the public service do, in the quotidian administration of programs and in their roles as organizational, if not partisan, politicians. Finally, we need to know something about the powers of the public service relative to other policy-making institutions and how that power is exercised in the policy process.

These are all important topics, but it is not clear that taken together they capture the essence of the administrative system of a country. Further, if we move away

from these rather simple categories, we need to ask more basic questions about administrative systems and the knowledge we need about them. Many of those questions are relational. We need to understand how administration fits with the remainder of the political system, and how it “interfaces” with the social system. The fit with the remainder of the political system goes beyond simple questions of power and goes to the match of bureaucratic elements in administration with the remainder of the system; in essence a large contingency theory question. This match is especially important in less-developed political systems but is also crucial for understanding the politics in industrialized democracies. Likewise, the issues of meshing with the social system will extend beyond administrative recruitment to consider how societal demands are processed and how decrees issued from government are processed in the social and economic system.

Level of Analysis

One of the most fundamental questions we need to ask about research in comparative public administration is at what level of analysis do we want to proceed; where will we find our dependent variable? The existing literature on public administration, or “bureaucracy,” or civil service systems, is replete with examples of macro-level research. At the most basic there is an assumption that the nominal categories of countries are meaningful and useful in explaining observed variations in administrative behavior. At a somewhat lower level, the concept of *bureaucracy* is also macro-level and is an attempt to describe a set of structural properties of administrative systems found in (Weber presumed) developed societies or perhaps in all societies (Berger 1957; Crozier 1964; Etzioni-Halevy 1983; Page 1985; Wilson 1989). This macro-level research is important and useful, provided that our purpose is to make comparative statements about countries or statements about the impacts of the structural properties of regimes.

Likewise, the public administration literature is filled with micro-level concepts and analyses that focus attention on the individual working within the public service and on his or her behavior in office. The (by now vast) work on “images” and role perceptions in public administration (Aberbach, Putnam, and Rockman 1981; Aberbach and Rockman 1983; Muramatsu and Krauss 1984; Campbell and Peters 1988; Mayntz and Derlien 1989) are all concerned with the attitudes and behaviors of individuals as they function within government. The managerial literature is also largely micro-oriented and is concerned with how best to motivate workers and gain their participation and compliance (Perry and Wise 1990). Finally, the literature on representative bureaucracy is largely oriented toward the collection of micro-level information. There is no right or wrong answer about what level of analysis at which to work, but the selection of one or another does imply something about the types of findings the research on comparative administration can produce.

Concentrating on macro-level of analysis, for example, directs attention to the connectedness of administrative institutions to other important social and political

institutions in society. Thus, when we focus on the macro level, we are concerned with accountability or responsibility (Day and Klein 1987) and the way in which administrative organizations are involved in the governance of their societies (Peters 1981; Lieberthal and Oksenberg 1988). Further, we are concerned with the extent to which administrative organizations are embedded in the social system and reflect the characteristics of that system. Thus, for example, although the data used to study representative bureaucracy are individual level, their utility is to characterize whole systems and to say something about whether the civil service reflects the social structure from which it is drawn. These research questions are all important for understanding the civil service, or the bureaucracy, and for understanding the place of those institutions in the governing system, but they are not the only important questions about the civil service.

PROBLEMS OF LINKAGE AND CROSS-LEVEL INFERENCE

There are two elements of special importance in this discussion of the levels of analysis problem in comparative political analysis. The first is that civil service systems and the individuals who work within them are linked to a number of different elements of the social system. The civil service system, as a system, shares some of the properties of the government as a whole, and that in turn shares some attributes of the surrounding society, economy, and culture. Individuals within the civil service have some portion of their behavior determined, or at least influenced, by being members of the civil service, but their behavior is also affected by the society and their personal and professional linkages with other social institutions. This portrayal of the connections of the civil service and its members should help forestall attempts toward quick overgeneralization and determinism based primarily upon its structural or even personnel characteristics.

The embeddedness of public administration in the broader social and political systems presents difficult methodological problems. Those surrounding systems are composed of a number of properties, some of which would be measured in our analyses and many of which would not. Therefore, when we find that there is a relationship between some x and some y in our analysis, we assume that is the “real” relationship that exists. It may be, however, that the relationship is spurious and the product of some as yet unmeasured z . So many z 's are tied up in any social system that, particularly when we use countries as an implicit analytic variable, there is a very high probability of making false inferences about relationships. This is, of course, a problem in any comparative research (Ragin 1987), but is perhaps greater for comparative administration because of the multiple connections with society—politics and management—and the difficulties in measuring our dependent variables. Further, the civil service like all other social institutions will have a symbolic significance within the culture that may be difficult to understand outside the culture.

The embeddedness raises the additional troublesome ramification that some philosophers of social science (Kaplan 1964) have referred to as *act meaning* versus *action meaning*. That is, actions taken within a particular social setting may have meanings that are not the same as would be imputed by observers not fully familiar with that social system. Thus, similar behaviors taken by an American and a Dutch civil servant might signal different things to other members of their organizations. The research methods needed for effective research on the civil service might therefore be more those of the “squat anthropologist” than the more conventional social science researcher. That is, we may need to do as Kaufman (1981) did and virtually live the lives of our subjects to gain greater insight into their administrative behavior.

In addition to the general problem of action meaning, in a political setting such as that inhabited by civil servants, there may be multiple meanings for any set of actions as the individual is engaged in multiple “games” (Tseblis 1990) that are a function of his or her multiple roles in a public bureaucracy. Even without the rationalistic logic embodied in much of this literature, these multiple and often conflicting linkages across levels and across segments of roles (Peters 1991) can be crucial for interpreting behavior within institutions. Thus, the need to contextualize administrative behavior not only within the society but even within the multiple roles and games of the average senior civil servant makes understanding outcomes of the process that much more difficult for outsiders. This will again require close observation of behavior within context, rather than the more conventional survey and descriptive analyses.

Although we need to keep our analysis at the appropriate level, it is also important to remember the interactions among levels. This is especially true when we remain cognizant of the fact that citizens and private organizations (firms or whatever), are also important components of the administrative system. Most systems models of social and political life include a feedback loop that links actions back to inputs, and for public administration the loop is usually closed through citizens. The outputs and consequences of administrative action may be individual (benefits denied, regulations not enforced, or whatever) but the cumulation of those actions may be systemic. Thus, when we attempt to measure the individual behaviors of civil servants vis à vis clients, which is certainly an important aspect of administrative behavior and administrative outputs, we may also be measuring some items that are of great consequence for the entire political system.

The second error that is important is to guard against the tendency to make improper inferences across levels of analysis. There is a common tendency in social research, and not just research about public administration, to make such unjustified inferential leaps (Robinson 1950; Retzlaff 1965). It is all too easy to assume that if the majority of individuals, or perhaps even all individuals, occupying roles in an administrative system think and behave in certain ways, the system will then behave in a similar manner. Bureaucrats may think in certain ways, but it is not always certain that *the bureaucracy* will function in that manner. Given the traditions of

comparative politics characterizing entire systems, the ecological fallacy is even more common, and researchers assume that because they can characterize the system as a system, the individuals within it will behave as they should. This is often the case, but by no means is it always the case, and deviance from the prescribed roles may be extremely important in explaining some aspects of the behavior of the system, most especially the ability to produce change.

The growth of public choice approaches to political phenomena has made the question of cross-level inferences even more important. The question of “methodological individualism” is especially evident in the work of scholars such as William Niskanen (1971) who posit that “budget maximizing bureaucrats” dominate bureaucracies and determine the outcomes of administrative decision making. There are numerous critiques and elaborations of this basic model (Jackson 1982; Blais and Dion 1991), and it holds sway over a good deal of thinking in the field. The reason that it is mentioned here is that it illustrates problems of cross-level inferences. The model assumes that micro-level motivations (budget maximization), if existant among top-level bureaucrats, define systemic properties. It appears, however, that the structural characteristics of regimes and civil service systems within which these purported maximizers operate have as much or more impact on the actual performance of the system (Peters 1991).

Equivalence—The Traveling Problem

One of the most familiar problems in comparative analysis, but still one of the most important, is the equivalence problem. It is part of the more general validity problem in social research; how can we have confidence that we are measuring what we think we are measuring? Even if we are able to validate concepts and their indicators within a single society, can we be sure that the concepts have the same meaning, or indeed any meaning at all, in different societies? To some degree this problem may be more manageable for comparative administration than for other social phenomena. The tasks assigned to public administration are somewhat more comparable than expectations about, say, legislatures in different settings. Even then, however, the nature of administrative organizations and the social meaning attached to those institutions may be widely divergent among societies, even those that appear very similar on socioeconomic and political characteristics.

One of the most obvious examples of the equivalence problem in public administration arises around the issue of “corruption” in the public service. One observer’s corruption often is another person’s conception of acceptable behavior or even of proper and obligatory behavior. There are numerous examples of scholars studying administration in the Third World and assuming that corrupt behavior was rampant and detrimental to “development.” For the participants, however, it was expected that they take care of their relations with administrative problems, and various forms of extracurricular payments were factored into the decision about the official rate of pay from government (Abueva 1966). We may argue that, after a

period of transition, these systems would function better with a more Weberian system of administration, but the claims of corruption and immorality are probably not justified in the terms of the society. It is not just in the cases of Third World countries that differing moral conceptions arise. Public servants and scholars socialized in the Anglo-American tradition of civil service “neutrality” often look askance at the manifest political involvement of civil servants in countries such as France and Germany (Mayntz and Derlien 1989).

While bureaucratic corruption is an interesting and important example of the difficulties of generating equivalent measures across cultures, it is by no means the only example, and a good deal of our investigation into comparative administration is influenced by this fundamental problem. Even terms with which we are very familiar and use without much cogitation, such as democracy, have rather different interpretations in different countries and therefore require specification if used comparatively. If we are interested in the senior civil service as a component of the total civil service, for example, it may make a substantial difference if there is a unified concept of the service, or if senior officials enter as “high flyers” with little connection to the remainder of the personnel system.

The notion of mid-range theory is applicable here. It may be that concepts are stretchable within a particular geographical setting, or within a range of functional concerns, but lose their meaning when forced to travel farther. We may want, however, theories and measures that can span a greater intellectual distance. That goal may require first doing the work necessary to generate the broader theories about public administration that are applicable and meaningful in a variety of cultural settings. We must inquire, however, whether such a goal, worthy though it may be, is really attainable within the bounds of existing resource availability and human capacity. Indeed, focusing on middle-range concerns—whether defined by geography or particular aspects of civil service systems—may be a more fruitful avenue even without consideration of resource and intellectual constraints. We may need to begin with better understanding of smaller parts of the intellectual puzzle before we are capable of understanding the larger entity. As Wildavsky argued about planning, if the theory can encompass everything then maybe it is nothing.

IMPACTS ON APPLICATION OF PUBLIC ADMINISTRATION RESEARCH

In addition to its impacts upon the scientific aspects of our work, the equivalence problem also has very important impacts on applied comparative administration and the penchant of some scholars of administration for “reform-mongering” (Hirschman 1969). A great deal of the reform activity around civil service systems follows intellectual fashions (Astley 1985), or political fads, with little attention to the difficult problems of matching the reform to the particular problem and sociopolitical system within which it must be administered. This absence of concern

with cross-national learning about reform points to two glaring weaknesses in the study of comparative administration. One is the tendency to give insufficient attention to the ideational aspects of policy and administration. Governments differ in large part because the mental pictures that people (elites and ordinary citizens alike) carry around in their heads are different. Social constructionists have gained a foothold in the study of substantive policy issues (Nelson 1984; Best 1989), but have been less successful in persuading people that administrative reality is as much socially constructed as, for example, is the drug problem or child abuse. Thus, a reform that is perfectly reasonable and effective in one setting is likely to be ineffective or counterproductive if tried in another setting simply because the change is not conceptualized in the same manner.

In addition to the impacts of cross-cultural differences on learning, there are strong impacts of ideational differences across time. The administrative structures of most countries represent numerous overlays of ideas that were considered the latest concepts at one time or another, but which have since been deemed to be outmoded or even wrong. These overlays, however, do make adaptation slow and more difficult than it might otherwise be, but they must be understood if organizational change is to be successful. The principal administrative theorists adopting such a perspective are the institutionalists, guided in large part by their concept of "appropriateness" for administrative action. If administrative reform is to be the search for appropriate institutions (March and Olsen 1989), then that appropriateness must be understood within the context of the national and organizational histories of the setting in which the search takes place.

The other glaring weakness in the literature highlighted by the desire to transplant reforms is the failure to investigate adequately the conditions of policy learning, here using administrative reform as a type of policy. Some efforts have been undertaken (Rose 1990; Wolman 1992) to rectify this problem, specifically for social and economic policies, but there is as yet little attention to its implications for administrative reforms. This is true despite the frequent occurrence and significant impact of administrative change on governance. Even for more developed aspects of "policy learning," there is as yet little development of methods for analyzing the crucial attributes of social and economic reforms that may make them amenable to transplantation (Dommel 1990). This is in part because there is as yet little development of ideas about social and economic factors that lead to successful policies, nor of the attributes of policies themselves. We believe, for example, that administrative reform may be different than other policy reforms, yet we have little idea of what are the relevant variables that define that difference. Further, we are not sure if the variables defining the differences are readily manipulable. If they are not, then attempts at transplanting reforms may not be worth the effort. We are probably correct that there are significant differences between administrative reforms and other policy changes, but if this insight is to be useful we need to understand why that is true. We are faced with another problem when we try to understand what social and economic conditions are related to particular administrative systems.

This question is discussed in the anthropological literature as “Galton’s problem” and derives from the observation of similar social patterns in different cultures (Naroll 1970; Eyestone 1977; Klingman 1980). Did the observed patterns arise autonomously, or was there a diffusion of a social innovation across cultures? This problem is increasingly evident in the contemporary political and administrative world in which communications and organizations such as the OECD and the World Bank diffuse administrative innovations rapidly. While establishing the intellectual pedigree of an organizational pattern or a reform proposal is interesting, it is not so important intellectually as understanding the conditions that generate and support those patterns. Even if the identified administrative innovation is transplanted, it has been at least minimally successful if it persists.

What can that success tell us about planned transplantation of organizational patterns? We should consider the work of administrative reformers and organizational designers around the world as a natural laboratory for further planned reform efforts. Attempting to codify and integrate these findings will require better methodologies for deriving lessons from the experiments and understanding what can be transplanted successfully and what can not. That will require identifying cultural and social elements central to the success of the reform, as well as better classifications of the elements of reforms themselves. Further, we also need somewhat clearer criteria of success if this indeed to be an effective laboratory for change. At the most basic level success is the persistence of the reform, but we would also need to employ measures based more on attainment of prespecified reform goals.

GENERIC ADMINISTRATION

We commonly think of comparative administration as being cross-national comparison, but some of the same theoretical and methodological issues arise if we seek to compare across policy areas. One issue that frequently arises in the study of public administration is whether all administration is the same, or so similar that differences in policy areas need not be considered. This question often arises for administration in the public and private sectors (Allison 1986; Bozeman 1987) but is also relevant for administering different types of policy within government. The generic view that all policies are the same can be contrasted with the view that the policy problems, and associated administrative problems, of each policy area are unique and therefore must be conceptualized differently. This differentiated view has substantial appeal, given the diversity of public functions that even the most casual observer can identify. For example, Page and Goldsmith and their collaborators (1987) found that particular policies administered in different countries were more similar than different policies administered in the same country; policy rather than country was the better predictor (see also Rose 1990). The difficulty is that we do not yet possess an adequate conceptual scheme for identifying the relevant differences among policy areas for either policy-making or administrative purposes (Kellow

1988; Peters 1991).

The most common means of classifying policy areas or policy problems is functional, or the names that we see on government buildings—defense, education, environment, etc. For analytic purposes, however, the variation within each of these categories may be as great as the variation among them. In most countries (even the United States) health policy includes a mixture of direct service provision, regulation, subsidies, loans, etc. Health policies also deal with a range of target populations including the medically indigent, the aged, hospital administrators, and medical students. It is by no means clear that a policy profile and an administrative pattern effective for one policy intervention or one target population will be equally effective for others within the one policy area. What we need is a more conceptual means of dealing with policies and with their targets.

Lowi's classificatory scheme for policies is a major attempt at developing such a conceptual device for public policies. That work has spawned a huge corpus of literature in political science (Kjellberg 1977; Peters, Doughtie, and McCulloch 1977), but also appears severely flawed. First, as with the nominal classification of policies, there may be as much variance within as among the cells of the typology (Spitzer 1987; Kellow 1988). Further, it is not clear if the variables used to classify policies, especially the proximity to coercion, are really the most effective variables of this purpose. Coercion has been used as a means of classifying the instruments of government (Phidd and Doern 1978; Woodside 1986), but even there the concept appears to miss the subtle differences existing among policies and the many alternative means of achieving policy goals. Thus, we believe that policy studies, and the associated administrative science, should look further for schemes to classify policy.

Based upon the above description, and my own earlier work (Linder and Peters 1984, 1991), I would argue that using nominal titles of policy problems, or Lowi's (or Wilson's) classifications as a labelling device, is unlikely to be productive in comparative administration. Rather, a better strategy may be to look at the instruments that government uses to reach policy goals as the basis for a more useful classificatory scheme of administration. We argue that for policy studies per se, alternative and more problem-focused schemes would be necessary to link with instrument choices, but for *administrative* purposes instruments constitute an acceptable basis for classification. If we consider that governments attempt to influence their economies and societies through one or another instrument, then these become the central content of administration.

As well as capturing the content of policy, there is substantial variance in the manner in which instruments are administered. At one end of the spectrum some government programs, albeit a declining number, are provided directly to clients, and government is responsible for their staffing and implementation. At the other end of the dimension, a number of public programs (e.g., tax expenditures) are provided indirectly and rely upon private organizations or citizens themselves to take up the benefit offered. Whereas the first category of programs is administratively

intensive and requires complex hierarchies of service providers, the latter type of program requires relatively little direct administration but a great deal of monitoring and review. Between the two extremes are public programs provided in whole or in part by the private sector. These require some direct administration, as well as a means of monitoring private compliance with public policy intentions. Thus, the selection of one or another instrument will tell us a great deal about the policy preferences of governments, as well as something about the administrative needs of those governments. This may be a place to start when looking at classificatory schemes for policies that can assist in understanding administrative differences.

RELATIONAL ISSUES

The role of instruments is perhaps especially important since many of the truly significant questions about comparative public administration are boundary and relational questions. That is, what are the boundaries of the administrative system, especially the public administrative system, and what are its relationships with other significant actors? As the state increasingly becomes “hollow” and dependent upon the private sector, or other governments, to implement its policies and programs, then the absence of clear definitions of the state are all the more evident. Further, within governments themselves issues of accountability and the role of the “political” institutions in controlling the more privatized and decentralized state abound. This is perhaps especially evident in the United Kingdom where “Next Steps” appears to have altered long-standing traditions of ministerial responsibility for policy and administration. Similar reforms in New Zealand (Scott, Bushnell, and Salle 1990) have even more greatly changed relationships between the political masters and the instrumentalities of policy implementation. A focus on the instruments of governing would provide a common focus for analysis, regardless of what type of organization is actually wielding the tools.

One danger of any conference concentrating on the comparison of civil service systems alone is that these components of governance do become isolated from other important components of the process, and the relational aspects are not sufficiently attended to. We risk doing a disservice to our own understanding of the civil service and of other components of comparative politics if we disaggregate the institutions of government too much. This concern with linkages may need to extend even beyond the formal institutions of government as the experience of corporatist states and corporatist political theory has demonstrated. Clearly, there is something to be gained by a particular focus on the civil service, but we also need something of the big picture of the total system of politics and government.

As with so many of the questions in comparative research, for example, grand theory versus mid-range theory, there is a problem of balance. How wide does our theoretical and empirical net need to be to capture enough of the total systems within which administration is embedded, and how particular do we need to be to focus

sufficient expertise on the topic? These are difficult questions to answer definitively. Again, we need an iterative approach, using some research designs that are more anthropological as well as some more positivistic ones. The former may help us frame better questions and better measures, while the latter may enable us to provide at least preliminary answers to those questions.

APPROACHES TO METHODOLOGY

We have discussed a number of methodological points, but at this point there is a need to raise some final points about the way in which to build knowledge in this area of inquiry. As we have been discussing the field, we have been implicitly contrasting it with the more “scientific” parts of the social sciences with their emphasis on indicator construction, measurement, and statistical relationships. We raised a number of doubts about the ability to reach that level of scientific development in this area. Therefore, this section will discuss briefly some of the alternative methods that may provide much useful information but which need not conform to all the requirements that our colleagues in voting behavior or even judicial behavior might impose on themselves. Further, we will make some arguments that such levels of “development” might really be counterproductive.

Ideal Types

If we move from the idea of building classificatory schemes inductively, we can go back to the roots of administrative science and use the method of ideal types as the means of understanding differences among real-world administrative systems. By moving back to our roots I obviously refer to the importance of Weber’s ideal type model for the development of bureaucratic theory. The method of ideal types has the virtue of providing a standard against which real world systems can be compared. Even if the “model” is rather ethnocentric (as Weber’s certainly is), the comparison is meaningful. We can clearly tell that country X has an administrative system that does not conform to Weber’s idea of rationality. The question then becomes why, and what difference does it make? The danger, of course, is that the ideal type analysis is converted into a different type of ideal, with the assumption that the Western conceptions of “good” administration become normative standards rather than empirical referents.

A good deal has been achieved in public administration through the careful analysis of Weber’s work and the comparison of real-world experiences with the ideal-type model he created. Page (1992), for example, used Weber’s ideal types to explicate a number of important comparative differences among European political systems. Likewise, Torstendahl (1991) utilized Weber’s ideas somewhat less precisely as an instrument to examine administrative change in Scandinavia. Earlier, Berger (1957) used the Weberian ideal to understand the emergent

bureaucracy of Egypt. No author went into their exercise expecting to find a perfect Weberian system, and indeed no one should have, but seeing the deviations in different countries and in different time periods does help explain administrative development. This is especially true given the developmental logic that undergirded much of Weber's model (Mommsen and Osterhammer 1987).

Weber's model has been the intellectual gold standard against which real-world bureaucracies have been compared most often, but there are other options that may be more meaningful for political scientists seeking to understand the role of public administration in the governing process. Ferrell Heady, for example, demonstrates the use of a number of models of civil service systems for comparative purposes. Further, the model of the *administrative state* offers one view of the tasks that bureaucracy must perform in government and can be used to identify the manner in which those tasks are performed (Waldo 1948). As with Weber, there is something of a developmental perspective embedded within this framework, with the assumption that not only will "modern" administrations perform these tasks, but they will perform them in certain ways. Even if this is regarded as excessively ethnocentric, the "model" does provide a basis for comparison. Much the same is true of our own (Peters 1981, 1991) ideas about "bureaucratic government" as a means of examining the role of the civil service in governance. The model is derived from the experience of Western democracies, but has fewer factors peculiar to those systems. In fact, given that many Third World countries have bureaucracies that are strong relative to the political institutions of their governments, a notion of bureaucratic government may be particularly useful in those settings. Also applicable would be the growing body of work on steering, whether taken from a more centralist position (Linder and Peters 1984) or a more decentralized position (Kickert 1991).

The Theoretical Case Study

We warned above of the dangers of concentrating attention on the single case and assuming that the one country is either so particular that no others need be compared, or is so general that all others are like it. American researchers, for example, tend to do the former for other countries and the latter for the United States. The advocates of case study methodology appear to say that this strategy of "comparative" research is not desirable in many settings, and opponents do tend to outnumber the advocates. The difficulties encountered with case studies can, however, be ameliorated by the use of theoretically driven case studies (Walton 1973; George 1979; Agranoff and Radin 1991). Such studies attempt to use the same methodology and the same research questions in a number of settings and then later induce generalities from the findings (George 1979). Again, this research design cannot be applied unless there is already some theoretical and conceptual guidance; the researcher must know in advance what questions to ask (Yin 1984). On the other hand, this approach can be useful for either theory "testing" or theory elaboration

after an initial stage of deduction or simply cogitation has been completed.

A variant of the theoretical case study is the “comparable” case study (Lijphart 1975). The idea is very similar to the theoretical case but rather than necessarily being so specifically informed by theory, and concerned with the elaboration of a particular theory as the product of the exercise, the comparable case study strategy is more concerned with comparison per se. Of course, comparison can be argued to be the principal tool for theory development for the social sciences (Smelser 1976), but this is really a matter of emphasis. The idea is to build theory by looking at a number of comparable cases and extracting generalizations from that research.

The theoretical case study is a useful and relatively cheap way of generating more directly comparable research in comparative politics, but also has pitfalls. One is that the “instrument” in a series of case studies will be different and is a source of error (Campbell and Stanley 1966). Each researcher will be an expert in his/her area and therefore will bring to the research situation vested interests and pre-conceptions about what the findings should be. If that is the case, then there is a strong probability of bias in the findings. We can guess about the directions of the biases, but attempting to counteract them or adjust for them may simply add another type of bias. The alternate strategy of using nonexperts has some appeal, but has obvious countervailing disadvantages. Again, the problem for comparative research is how best to balance breadth and depth. How can we marshal sufficient expertise for each case without making the research outcomes just another set of incomparable studies of different political or administrative systems? The iterative approach between more descriptive studies by country experts and more theoretical studies by functional experts would appear to be one way around this methodological conundrum.

The “comparable case study” strategy has some special pitfalls. The most obvious is that we are placed in the difficulty of having to define “comparable” (see Sartori 1970; DeFelice 1980). On how many dimensions must cases be similar before they are close enough? Must the researcher assume that he or she knows in advance that the cases are comparable? If not, there is a need for (expensive) sampling of cases to have a sufficient number that are truly comparable. This statement of the problem also would return us to the familiar Przeworski-Teune (1970) territory and the need to ask ourselves whether “most different” or “most similar” cases are the best for comparison. Comparable in the usual sense of the term means similar, but comparable may also mean that the cases should maximize variance on some (presumed) independent variable so that we find differences on the (presumed) dependent variable. Given the relatively low level of scientific development (especially of usable indicators) in much of comparative administration, we may well want to adopt strategies that do maximize observed variance.

False Scientism?

A final point here is whether the expectations raised in the preceding portions of the paper are not inappropriate, and whether we should aspire to less sweeping

developments of theory and methodology. There have been a number of arguments advanced that public administration is not amenable to the quantitative techniques associated with the social sciences and is better understood phenomenologically (Denhardt 1981; Hummel 1987). At a less extreme position, it can be argued that the emphasis placed on quantitative methodologies and indicators is misplaced, and we should place greater emphasis on qualitative methodologies (Miles 1979) and on methods (“meta-analysis”) that permit cumulation of case study materials. On the other hand, it might be argued that this is much too easy an admission of defeat and that we should merely push ahead searching for full scientific development of this field rather than retreat so quickly.

The barriers facing any attempt to build “science” in comparative public administration are formidable. We are (usually) denied the experimental method as a means of establishing causation between change in the external environment and administrative behavior, although internal changes within organizations can be treated as experiments. Even with statistical analysis, we have more variables than cases, especially given that using country as an implicit control variable packages together a huge number of variables—some of which are not identified or even imagined (Frendreis 1983; Jackman 1985). Such models are at best indeterminate statistically so that the usual canons of social scientific research do not apply well. Therefore, we often are in the position of *illustrating* theoretical arguments with comparative examples (Smelser 1976) rather than really being able to test theoretical arguments systematically using comparative data. The illustration may well illuminate the theory being considered, but it cannot be said to “test” the theory in the usual sense of that term.

At an even more basic level, our understanding of public administration and its milieu may not be sufficiently well developed to distinguish independent and dependent variables so that a statistical “test” could even be conceived properly. Above, I advised that we should catch our dependent variable first, but that may beg the question of how one knows where the factors fall in a presumed causal sequence. For example, do recruitment patterns for the civil service replicate the social structure, or do they help to create or perpetuate social patterns? Are certain “less-developed” patterns of administrative behavior a function of lower levels of economic development or are they causes of that low level of development? We could make arguments both ways in the above examples and in many others where we would want to establish causation. It may be that rather than seeking the precision and parsimony sought by science, we should instead search for thicker and more useful descriptive statements about systems with the hope first of some descriptive generalization and then perhaps *science*.

DO WE WANT AVERAGE PERFORMANCE?

It could be argued that the purpose of comparative administration research is not to generate generalizations about public administration that will push back the

frontiers of social science. Rather the purposes are more ameliorative and reform oriented; we need to know what works. If that is indeed the case, then scientific generalizations are not the appropriate target for analysis. Instead, finding exceptional performance is the goal. Miller (1984) made this point about the study of public policy, and much the same can be said about administration. That is, we may want to identify civil service reforms, or continuing administrative arrangements, that have been unusually successful rather than identifying modal patterns or even stable patterns among factors in administration. If we can do this, then the possibilities of offering useful advice are enhanced. This research pattern would not be well tolerated by the more “scientifically” oriented scholars’ departments, but it is still a viable and meaningful approach to research in comparative civil service systems.

Just as we want to identify patterns of exceptional performance if the goal of the inquiry is to improve practice, we must also be concerned about which variables are manipulable. Research in comparative policy studies often has identified economic development as the best predictor of public spending for education, health, etc. The lesson then is to get rich, but that is not particularly useful advice for developing countries that were trying to do that anyway. The point is that if research is to be useful for policymakers, they must be given strings to pull that will produce results rather than be told that some remote factor *X* is the root cause of the problem. Again, good social science may not always be particularly good policy or administrative science.

CONCLUSION

This paper has plowed a good deal of ground, much of which will be very familiar territory to most readers. Indeed, most of the issues that confound students of comparative public administration in 1994 are the same issues that have plagued us for decades and that have plagued students of comparative politics in general for that same length of time. That continuity does not make the questions and problems any less important. Also, it does not mean that the problems raised are necessarily insuperable. What it does mean is that there is no quick technological fix for most of our research questions, nor any methodological medicine that will cure all our ills. That form of easy salvation does not exist in this social scientific vale of tears.

It appears the real hope for deliverance will come not from quick fixes but from an interaction between theory and data. Social scientists have a history of denigrating “barefoot empiricism,” and often that has been justified. On the one hand, there is much to be said for actually having data that speak to a real issue even if the data are not as neatly packaged for theoretical purposes as we might like. On the other hand, more empirically minded colleagues tend to devalue the work of theorists as being excessively disconnected from the reality of the world or as being so broad as to be useless in practice. Some of those complaints have been valid, but have assigned perhaps too little value to the generation of meaningful generalizations that

can guide future empirical work. Clearly the need is to connect the two strands of thought and work. Further, that connection may need to come initially at a rather low level of generalization—at the level of mid-range theory. We noted above the possibilities of developing such theories, organized around geography, instruments, or substantive administrative issues. There is no shortage of things to be done, and this volume and the associated collaborative research represents an excellent way in which to start the enterprise.

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Public Administration Theory is the amalgamation of history, organizational theory, social theory, political theory and related studies focused on the meanings, structures and functions of public service in all its forms. It often recounts major historical foundations for the study of bureaucracy as well as epistemological issues associated with public service as a profession and as an academic field. Issues constituting comparative Public Administration (CPA) is a matter of polemics. A school of thought traced the subject to a conference held at Princeton University in September 1952 at the instance of Public Administration Clearing House. A sub-committee to develop "criteria of relevance" with the underlisted terms of reference was inaugurated to: - Identify major research needs and priorities; - Suggest ways and means of stimulating research in CPA; - Review and assess the existing state of knowledge in the field of CPA; - Prepare a general research guide/design for such studies. The study of CPA thus: - That all administrative systems are guided by hierarchy in the ordering of relationships.

6 Theory and Methodology in the Study of Comparative Public Administration

B. Guy Peters. This paper addresses methodological and theoretical problems in the study of comparative administration. This is difficult, given the varied meanings of key words such as comparative and administration. The flowering of comparative public administration during the heyday of development administration meant that it acquired more of a practical and reformist bent, but the roots of the field in comparative politics might make it less practical. The variety of intellectual problems facing comparative public administration has generated numerous scholarly doubts concerning the viability of the field (Aberbach and Rockman 1983; Sigelman 1976). The study of comparative public administration has gained momentum in recent years because of the fact that the emerging nations of Asia and Africa are trying hard to develop their economy and political system. The simple objective is to meet the growing needs of people. The Great Depression of the thirties of the last century drastically changed the entire administrative system of USA. The comparative public administration became highly popular in the high tide of cold war period. The top administrators of White House and other offices of Washington thought that the public administration must be made appropriate to meet the challenge posed by Cold War. Finally, the Comparative Administration Group emphasised the building up of theory.