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1997 John M. Gaus Lecture

Administrative Practice, Organization Theory, and Political Philosophy: Ruminations on the *Reflections* of John M. Gaus

James G. March, *Stanford University*

The 1997 recipient of the John Gaus Award, James G. March, is Professor of Political Science and Sociology Emeritus and Jack Steele Parker Professor of International Management Emeritus of Stanford University. March was born in Cleveland, Ohio, in 1928 and is a Phi Beta Kappa graduate of the University of Wisconsin and holds graduate degrees from Yale University. He has received six honorary doctorates and is a Fellow of the National Academy of Education, the National Academy of Public Administration and the National Academy of Sciences. Among his many panel and committee memberships, he served as a Vice President of the American Political Science Association in 1983–84.

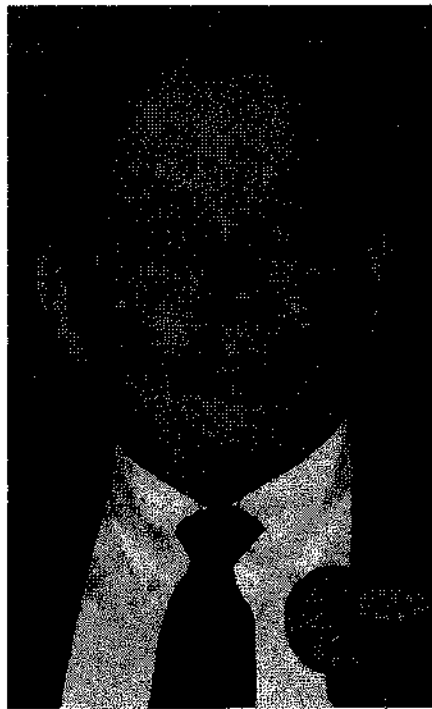
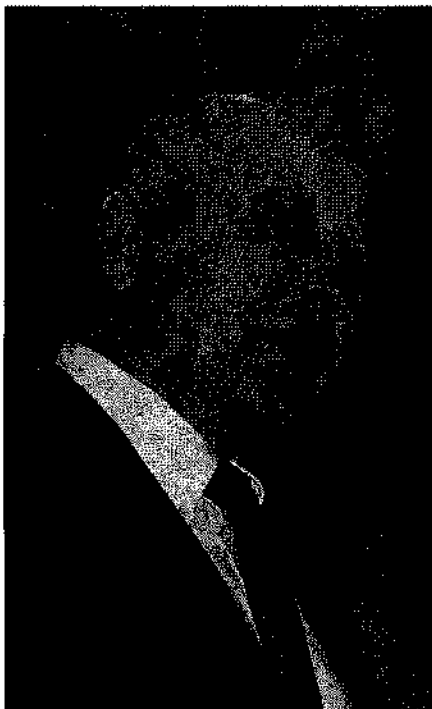
March's best publications include

Organizations (with Herbert A. Simon), *A Behavioral Theory of the Firm* (with Richard M. Cyert), *Leadership and Ambiguity: The American College President* (with Michael D. Cohen), *Rediscovering Institutions: The Organizational Basis of Politics* (with Johan P. Olsen), *A Primer on Decision Making: How Decisions Happen*, and *Democratic Governance* (with Johan P. Olsen).

Professor March has published five books of poetry and is the only recipient of the Gaus Award to have been knighted by King Harald as Knight First Class in the Royal Norwegian (Olav V) Order of Merit (1995).

Prepared by George Frederickson, Chair, 1997 John Gaus Award Committee.

When I entered the University of Wisconsin as a freshman fifty-two years ago, I was assigned a faculty advisor. That advisor was John Meriman Gaus. I suppose a more astute or better informed freshman might well have initiated a discussion of organization theory with Professor Gaus, but I did not. Nor do I recall that he felt an uncontrollable desire to talk to me about the future of public administration. I was a young squirt, and he was a middle-aged giant. By the time I returned to the university after army service, John Gaus had moved to Harvard. One brief freshman advisory meeting was my first and last contact with him. It was one of many brief encounters in life. Nevertheless, I am happy to use that minor con-



James G. March delivers the 1997 John Gaus Lecture at the 93rd APSA Annual Meeting

nection to John Gaus many years ago to claim a tenuous bit of nostalgic legitimacy on this occasion.

At the same time that he was briefly my freshman advisor, John Gaus was delivering the University of Alabama lectures that became the basis for his influential book *Reflections on Public Administration* (1947). I intend to use that book, published in 1947, as a springboard for a contemporary set of thoughts. They are a few personal ruminations of my own on John Gaus's personal reflections on public administration, a kind of second-order excursion into a Platonic cave of musings.

The choice of Gaus's *Reflections* as a text is, of course, partly a bow to his name in the context of a lecture in his honor. But it is also a modest attempt to reframe some elements of contemporary debates in public administration in a way that he might recognize. Those debates have emphasized a set of issues associated with the increased use of the term *management* in discussions of public agencies (Ostrom 1989; Lynn and Wladavsky 1990; Boyne 1996). I think that emphasis can be overdone.

I have an affection for the pleasantly masculine connotations of "management." I have been a professor of management for much of my career and have spent more than 30 years on the faculties of business schools that have tried to develop and propagate modern analytic tools for effective management. I think various forms of decision analysis, contracts, and markets can be used to improve public, as well as private, bureaucracies. And I have spent much of my life trying to develop economic theories of choice and the firm.

Nevertheless, I do not think the major research questions confronting public administration are associated very directly with such issues. And I will claim John Gaus's authority for such a belief.

The Reflections of John Gaus

John Gaus's Alabama lectures and the book that resulted from them were crafted in a particular time by a scholar with a particular history. He spoke and wrote from the per-

1998 John Gaus Award Committee Seeks Nominations

The 1998 John Gaus Award Committee is currently seeking nominations for the 1998 prize. The award is presented annually to honor a lifetime of exemplary scholarship in the joint tradition of political science and public administration and, more generally, recognizes and encourages scholarship in public administration. The recipient will deliver the John Gaus Lecture at the 1998 Annual Meeting in Boston.

Nominations should be directed to the committee chair, Lois Wise at Indiana University, SPEA 453, Bloomington, IN 47405.

spective of a man who had been born shortly before the nineteenth century ended. He had matured through the period of the First World War, the 1920s, the Great Depression, and the Second World War. It was a period of enormous change in the role and size of government, much of which Gaus had seen at close hand and most of which he viewed as benign. It was a period in which public administration had flourished, both as a practice and as an area of study. It was a period of great technological change, particularly in the means of transportation, communication, and destruction. And it was a period that, in retrospect at least, seems to have been deeply (and perhaps, from a contemporary perspective, overly) optimistic about the possibilities for making governance both more democratic and more intelligent.¹

Gaus's comments were addressed primarily to people anticipating a career in public administration. In 1947, most Western democratic regimes were about to embark on major expansions of governmental involvement in their social and economic systems and a consequent major expansion of their public bureaucracies. Gaus saw a post-war environment for public institutions characterized by complexity, volatility, and especially interest multiplicity. Unlike many social democratic commentators of his day, however, he was an advocate neither of corporatism nor of interest competition as a basis for administration.² Rather, reflecting a tradition that extended back at least to the Greek philosophers, Gaus saw democracy as served best by public officials and institutions that exhibited the highest professional competence yet subordi-

nated their own private interests to the general good of the community.³

Although the primary audience for the book and lectures consisted of career public administrators, Gaus had a clear secondary audience in mind, academic researchers in public administration. He tried to clarify the tasks that would face public administration researchers in the last half of the twentieth century. He saw research in public administration as an instrument of democratic intelligence, involving disciplined investigations organized around three fundamental scholarly commitments.

The first commitment was to understanding the detailed context of administrative practice. Gaus saw studies of administration as rich historical and contextual investigations of concrete activities in specific settings. His study (with Leon Wolcott in 1940) of the Department of Agriculture was an example. It was an elegantly detailed examination of the development and institutional texture of an administrative agency.

The second commitment was to a study of public administration that was intimately linked with organization theory. Gaus viewed research on public administration as drawing from and contributing to more general knowledge about organizations.⁴ For example, his 1930 report to the Social Science Research Council described the study of public administration as a natural site for examining the most fundamental features of modern social life, thus inextricably linked with the major disciplines of scholarship.

The third commitment was to maintaining a close connection be-

tween the study of public administration and democratic political philosophy. Gaus saw the problems of administrative intelligence as intertwined with the problems of securing democracy. He emphasized the ways in which the compelling questions of public administration were also questions of political philosophy, seeing clearly that vital issues of democratic theory lurked within many mundane aspects of administrative procedure.

An Old Tradition

These three commitments—to understanding administrative practice, to contributing to organization theory, and to illuminating political philosophy—are enduring themes in Gaus's writings and an important part of his *Reflections*. In this respect, he was entirely in step with the field of public administration of his time. Studies of public administration in the 1940s and 1950s were, for the most part, studies of the administrative institutions and policy problems of a particular political system. They typically emphasized the historical and contextual details of a specific set of policies and practices.

This focus on the concrete specifics of particular activities was, and is, one of the great strengths of the study of public administration. It has kept the field from engaging in orgies of abstractions and has served as a welcome reminder that much of what research can contribute to an understanding of political life lies in the details of idiosyncratic histories and institutions. As Gaus recognized, an indispensable part of the scholarly integrity of political science lies in its emphasis on the profound importance of historical and institutional detail for comprehending political and administrative processes.

What was distinctive about research on public administration immediately after the Second World War, however, was the way this emphasis on the micro-structure of administrative practice associated with specific policies in particular places was combined with attention to Gaus's other two concerns—organization theory and political philosophy.

Scholars such as Robert A. Dahl, Pendleton Herring, Charles S. Hyneman, Herbert Kaufman, Charles E. Lindblom, Norton E. Long, Philip Selznick, Herbert A. Simon, David B. Truman, Aaron Wildavsky, and James Q. Wilson drew from research on public institutions to make major contributions to our understandings both of organizations and of democracy.

In a recent book on organization theory, Jeffrey Pfeffer, not himself a political scientist, comments that "many of the most prominent early figures in organization science came from political science. . . . Many of the earliest studies of organizations were studies of public bureaucracies, so there were many connections between public administration . . . and organizational behavior" (1997, 12–13). Pfeffer is right. Ideas of bounded rationality, pluralistic competition, muddling through, cooptation, resource dependence, garbage can decision processes, and loose coupling all emerged from studies of organizations in the public sector. Each has become part of the standard repertoire of students of organizations.

At the same time, students of public administration struggled with and elaborated discussions of such central issues in democratic theory as the role of representativeness in bureaucracy, concepts of accountability, conceptions of the relation between public duty and personal incentives, and the relation between expertise and politics. They tried to identify the conditions for effective use of markets, bureaucracies, and politics in pursuing the common good and reconciling conflicts of interest. For example, one of the more influential books of the period was Dahl and Lindblom's 1953 treatise on systems of governance, a book that anticipated subsequent interest in governance and transaction costs (see, for example, Williamson 1996). In short, research in public administration contributed importantly to more general theories of organizations and democracy.

A Tradition Lost

In the years since the 1960s, theories of organizations and democratic

political philosophy have continued to flower, building on those earlier contributions. With respect to theories of organizations, ideas of bounded rationality and conflict have become the basis of modern economic theories of organization (Milgrom and Roberts 1992), including ideas of agency (Levinthal 1988), information asymmetries (Kreps 1990), and transaction costs (Williamson 1989, 1996; Teece 1987), as well as the basis of more behavioral theories of organizational decision making (Shapira 1996; March 1994a, 1996). Ideas of organizational adaptation have been elaborated into models and empirical studies of organizational survival (Nelson and Winter 1982; Carroll 1988; Hannan and Freeman 1989), organizational learning (March 1991; Cohen and Sproull 1996), and the spread of innovations (White 1975; Brown 1981; Pennings and Buitendam 1987). The significance of rules and institutions in shaping social systems has been rediscovered (North 1990; March and Olsen 1989, 1995), along with the intricate relations among talk, symbols, and action (Pfeffer 1981; Brunsson 1989) and the complexities of reform (Brunsson and Olsen 1993; Olsen and Peters 1995). Richly detailed studies of organizational problem solving, history, and reaction to technology have led to new conceptions of the processes by which organizations change (Barley 1986; Baum and Singh 1994; Garud, et al. 1997; Tyre and von Hippel 1997) and the ways in which institutions are created and become taken for granted (Meyer and Rowan 1977; DiMaggio and Powell 1983; Haveman and Rao 1997). Careful collections of data combined with analytical models and the power of modern computing have led to new insights into the non-hierarchical networks of organizations (Marsden 1982; Burt 1983, 1987) and the inter-organizational diffusion of knowledge and practice (Egeberg 1980; Håkansson 1987; Cohen and Levinthal 1990). And studies of the stories and cultural myths of organizations (Martin 1992; Czarniawska 1997), the significance of gender in the construction of organizations and organization theory (Mills and Tancred 1992), and the ways organiza-

tions make sense of their experiences (Weick 1995) have clarified the role of organizations in the construction of meaning and their dependence on it.

Similarly, the study of political philosophy has flourished in the last 20 to 30 years, stimulated in part by feminist critiques of democratic theory and practice (Brown 1988; Pateman 1989; Bock and James 1992). There are new debates over the political meaning of community (Mansbridge 1990) and the concept of representation, particularly as it involves the provision of voice to those not present, for example, the unborn, women, citizens of other jurisdictions, and the weak (Pitkin 1967, 1981; Pateman 1970). Political philosophy has also been deeply affected by the political and intellectual triumphs of rationality and market mechanisms of collective action. Rational exchange ideas of the nature of politics and governance have been elaborated in considerable detail, both as a basis for predicting political outcomes and institutions and as a political philosophy (Taylor 1975; Riker 1982; North 1990; Shepsle 1990; Viroli 1992). They have also been challenged, particularly from philosophical and behavioral perspectives that place greater emphasis on institutions, identities, and rules as the foundations of political life (Moe 1990; Apter 1991; March and Olsen 1989). Within such perspectives, there are key debates over the contributions of social structure, values, and institutions to sustaining democracy (Putnam, et al. 1992), over the processes by which identities and rules are created (March and Olsen 1995), over the role of discourse and public argumentation (Habermas 1981, 1992; Rawls 1971, 1987, 1988; Yack 1985; Dryzek 1990), over the extent to which preferences and interests should be treated as exogenous to politics (Sandel 1982, 1984), in particular over the nature and source of civic identity (Wolin 1989; MacIntyre 1984) and the construction of the public official (Thompson 1980, 1987).⁵

These elaborations of organization theory and democratic theory define, I think, an impressive story, perhaps not of progress—for they have, along the way, raised questions

about the concept of progress—but certainly of productivity and excitement. The period since the 1960s has been one of considerable development in studies of organizations and democratic political philosophy.

In retrospect, however, there has been one conspicuous difference between this period of intellectual ferment and the twenty years immediately after the Second World War. The excitements in studies of organizations and in theories of democracy over the past few decades have occurred in substantial isolation from American research on public administration. American students of public administration have continued to produce research on the concrete details of specific programs and agencies, but their contributions to the elaboration of organization theory and to debates in democratic political philosophy have been notably fewer.

One indicator of this feature of recent research is that studies of public administration have become infrequent visitors to the major journals of political science, economics, and organization studies or to the citation lists of leading contributors to those fields. The most important journal in organization studies in the United States, the *Administrative Science Quarterly*, was founded some forty years ago. Students of public administration were heavily involved in its founding and in its early issues. In the past 20 years, however, it has published only an occasional paper dealing with public sector organizations. Those occasional papers were rarely written by political scientists and almost never gave any attention to issues of democratic theory. The journal has been relatively frequently cited by modern students of public administration, but contributors to the *Administrative Science Quarterly* have almost never cited recent research on public administration as a source of ideas. A similar pattern characterizes the past 20 years with respect to the *American Political Science Review*. Papers in public administration journals relatively frequently cited papers from the *Review*, but the converse was not true.

The involvement of American students of public administration in the elaboration of organization theory

and democratic political philosophy declined notably after the 1960s. The change was not because organization theory and political philosophy retreated from attention to more “applied” work. Rich dependence on applied studies was characteristic of new developments in both. For example, recent research on organizations is filled with new theoretical and philosophical ideas developed through active involvement of students of relatively applied domains, such as accounting, decision analysis, strategic management, and gender.

The reality of the last few decades has been that the philosophers of democracy and theorists of organization have paid very little attention to studies of public administrative practice. Students of public administration have paid a modest amount of attention to organization theory and political philosophy, but their work has been, for the most part, derivative rather than contributory. Game theoretic analyses of asymmetric information and problems of agency have come, for the most part, not from students of public sector problems of control but from students of similar problems in the private sector. Understandings of how organizations adapt to changing environments have come, for the most part, not from students of public sector institutions but from students of organizational ecology and learning in the private sector. Key conceptions of decision making in organizations have come, for the most part, not from students of public sector decisions but from students of decisions in the private sector. Ideas about the roles played by networks, diffusion, and interorganizational linkages in the spread of ideas and practices have come, for the most part, not from studies of the public sector but from studies of mergers and acquisitions and knowledge transfer in the private sector and from sociological studies of institutionalization. Interest in postmodern perspectives for public administration (for example, Fox and Miller 1994) does not seem to have generated contributions from public administration researchers to the development of contemporary ideas in democratic theory (Goodin and Pettit 1997) or organization theory (Hatch 1997).

In a similar fashion, developments in philosophies of governance based on conceptions of voluntary rational exchange and on conceptions of rule-based institutions have occurred outside of research on public administration for the most part. Discussions of the democratic philosophies of accountability, representation, gender, and civic discourse have arisen only to a minor degree from considerations of the context of public administration. Ideas of community, civic identity, political empathy, social capital, and political trust have been elaborated with only minor attention to the public administrative life for which they are fundamental. And modern treatments of the ancient problems of power and powerlessness in political systems come more from observations of political elections, legislation, and political leadership than from observations of public administration.

There were and are important exceptions, of course, and the story would not be complete without them.⁶ It may be instructive to observe, however, that a disproportionate number of the exceptions are not American. Consider, for example, the work done by Michel Crozier and Jean-Claude Thoenig and their associates in France, Renate Mayntz and Fritz Scharpf and their associates in Germany, Johan P. Olsen, Nils Brunsson, and Barbara Czarniawska and their associates in Scandinavia, and John Halligan and Robert Goodin and their associates in Australia. Without abandoning a commitment to the fine contextual detail of political life, these scholars have made observations about public organizations that have illuminated not only specific political institutions in specific countries but also theories of organizations and theories of democracy. In their review of dissertation research in public administration, Adams and White (1994) made a similar observation (albeit qualified by sampling problems) about the greater theoretical relevance of non-American dissertation research in public administration.

As one example that illustrates the contrast between American and non-American approaches during this period, consider Norwegian research on public administration. Tom Chris-

tensen and Per Læg Reid (1997) have recently reviewed that research. They identified 215 books, book chapters, or journal articles published by Norwegian students of public administration in the period from 1954 to 1997, about 60% of them published in the past ten years.⁷ An equivalent per capita productivity rate in the United States would have resulted in about 7,500 publications by American students of public administration over the past ten years. The Norwegian productivity is impressive, but what is even more distinctive is the extent to which Norwegian studies of public administration are embedded in and contribute to broader concerns of both organization theory and democratic political theory. Under the leadership first of Knut Dahl Jacobson (1960), and subsequently of Johan P. Olsen (1983), the Norwegian research establishment has consistently produced studies of public administration that tie administrative practice, organization theory, and political philosophy together.⁸

My primary intent is, however, not to hold out non-American studies of public administration as necessary models for American studies. There is ample room in scholarship for the variety produced by national and regional differences, and American research on public administration continues to be a primary contributor to our knowledge. Nevertheless, I cannot help noting that the predominant intellectual course that came to typify public administration research in the United States after the 1960s was one that deviated not only from the course followed in some other countries but also from an earlier American path (represented particularly by public administration and political science scholars of the 1935–65 period). Partly because of signs of new energy in public administration research, I think we are now in a position to rediscover, and return to, our earlier course.

Rediscovering a Lost Tradition

The earlier tradition of public administration was one that assumed active interaction between studies of

the institutional and contextual realities of organizational life, on the one hand, and the development of theoretical ideas, on the other. Issues of administrative practice and issues of organization theory and political philosophy were seen as intertwined, and intertwined in a way that led to contributions from public administration to organization theory and democratic political philosophy as well as the other way around. The recent separation of concrete studies of administrative practice from broader intellectual concerns not only has made American research on public administration less compelling; it also has made organization theory and political philosophy less well-grounded. It is this latter failing that is of particular concern. Without a linkage to ideas arising from the empirical realities of administrative practice, both organization theory and political philosophy tend to become excessively entranced with elegance.

We know more about bounded rationality and the scarcity of attention as theoretical problems than we do about how organizations cope with them. We know more about abstract agents dealing with abstract principals than we do about real bureaucrats dealing with real politicians. We know more about incremental hill climbing on an imagined surface using formally specified decision rules (or rules that learn) than we do about problem identification, problem solving, and change in the messy world of real organizations. We know more about the formal properties of learning from experience than we do about the ways organizations evaluate, record, and retrieve the lessons of history. We know more about the effects of local positive feedback in adaptive models than we do about the ways in which real people in real organizations respond to the development of individual or collective competence. We know more about the transfer of abstract knowledge through abstract networks than we do about the fine detail of the exchange of information in real networks. And we know more about the possibilities for collective confusion in the development of social meaning than we do about

the evolution of real understandings in real organizations.

This disparity between theoretical ideas and empirical knowledge about the fundamental micro-processes of organizations embedded in a social and historical context has led recently to calls from organization theorists for greater attention to the details of administrative practice. These calls for greater empirical and contextual reality are particularly characteristic of recent themes associated with three related, but somewhat different, "institutional" theories. The first is "the new institutional economics," the effort to rationalize observed administrative forms and practices as solutions to hypothetical metagames by which the rules regulating competition among rational actors are established (North 1990; Shepsle 1990). The second is the "new institutional political science" that focuses on organizational rules, identities, and resources as endogenously-molded basics of organizational behavior (Wildavsky 1987; March and Olsen 1989, 1995, 1996). The third is the "new institutional sociology" that emphasizes the ways in which organizational survival involves satisfying the "taken for granted" presumptions of a population of organizations (Powell and DiMaggio 1991; Scott 1995) or culture.

All three "institutional" approaches are strikingly in need of ideas drawn from empirical studies. They have gone about as far as they can with their existing ideas. In particular, they need to identify the rules and institutions that endure; they need to clarify the ways in which complicated mosaics of rules and identities are converted into actions in particular situations; they need to understand the detailed micro-processes by which such rules are created, maintained, changed, and eliminated. Most of all, they need some new ideas about the processes they are modeling.

As a result, students of rules in all three of the institutional traditions are increasingly eager for empirical work on the actual ways in which rules change, knowledge spreads, identities are fulfilled, and organizations survive. This includes research on such things as the microprocesses

of survival in populations of organizations (Carroll and Hannan 1995), the development of new institutions of governance (Olsen 1996), the dynamics of rules (Andersen 1988; Zhou 1993), the interpretations of technologies by the users of it (Barley 1986), the spread of institutional fashion (Czarniawska and Sevón 1996), and the microlevel nuances of administrative reform (Olsen and Peters 1995).

Because of consciousness among theorists of organizations and democracy that contemporary theoretical ideas need a more profound grounding in the details of practice, we are, I think, entering a period in which research on public administration, particularly comparative administration (Bekke et al. 1996), can play a more significant theoretical role than it has in the past few decades (Wamsley, et al. 1990; Hood 1995; Golembiewski 1996; Rhodes 1996). It is a period in which we can try to take seriously what everyone knows about administration—that organizational actions are located in particular historical, political, and social contexts and that the fine details of organizational structure and procedures, as well as of the sequencing of events in partially interacting streams of history and the networks of linkages among organizations, often make a difference. And it is a period in which we can revitalize the connection between administrative research and political philosophy with a new eye to the administrative and political contexts of fundamental issues in democratic theory.

For such a transformation to occur, however, more students of organizations and democratic theory will have to read more of the research on public administration. There is a limit to what can usefully be said theoretically about organizations or democracy without consciousness of empirical realities in administrative contexts; and the habitual indifference of some organization theorists and some democratic political philosophers to studies of those contexts is self-destructive.

At the same time, research on public administration probably needs to be more effectively oriented to key theoretical and philosophical

problems for which it is relevant. To some extent, this means revisiting old issues of public administration from the perspective of a modern sensibility. For example, one such issue involves the relation between democratic demands for equality and disparities in the possession of knowledge. John Gaus was deeply concerned, both in the *Reflections* and in other works, with the democratic uses of expertise. He, as well as others, saw questions about the role of expertise as embedded in specific administrative contexts, in more general organization theory, and in classical democratic theory. For the most part, he framed the issue of expertise as one of using knowledge while protecting democracy from the disparities of information and power that it introduced.

Time and the erosion of confidence in knowledge, expertise, and shared interests have changed our conception of the relations among public officials, experts, and citizens. The neutrality of expert knowledge has been challenged repeatedly in discussions of medical knowledge, engineering knowledge, and social science knowledge, indeed virtually all varieties of claims to differentiated competence. The basis and meaning of expertise have been transformed by a combination of political pressures and intellectual deconstructions of knowledge. Deviant scientists have claimed equal standing with established scientists; voices representing religious faiths and traditional communities have claimed equal standing with scholars and professionals over a wide range of judgments. Not only have those claims often garnered substantial support among politicians and their constituents, they have also been buttressed by a trend toward greater attention to the role of social construction in establishing truth, and thus to the problematic nature, even illegitimacy, of special claims to knowledge.

As a result, any discussion of the role of bureaucratic intelligence in contemporary democratic government must begin by recognizing that the position of experts and expertise is less secure than it was in Gaus's world. As various forms of constructivist conceptions and ideologies

have gained intellectual approval, the test of knowledge has shifted away from *ex ante* evaluation by certified experts. It has shifted toward the demonstration of capabilities in broader markets, more open public forums, or other forms of interpersonal, inter organizational, or international competitions for approval. An expert has become not so much someone who knows something as someone who wins approval, who gains and retains customers, who sells books or services. In a political setting, in particular, an expert has become one who secures certification in political discourse. Knowledge has become defined as a belief shared within a political community. In a way that might have surprised John Gaus, expertise has been, in this sense at least, significantly democratized—or at least marketized.

An obvious problem is that much of our thinking about experts in public administration assumes that establishing and exercising expertise requires something other than securing popular approval. According to traditional views, having expertise involves comprehension of the way things work. It is exhibited through an ability to articulate fundamental principles that are demonstrable and significant. It is confirmed by recognition within a community of experts exercising exacting professional standards developed within traditions of competence. Knowledge, in that sense, may often be useful to winning public approval, but its validation is seen as distinct from general approbation. Winning public approval often involves position, energy, power, *chutzpah*, style, and luck at least as much as it involves knowledge. So, when expertise is redefined in terms of being successful in a wider public, rather than within a community of experts, the nature of the relation between democracy and expertise is altered quite substantially. The issues are ancient in political philosophy and connected to quarrels over the possibility of political judgment (Ruderman 1997) and to discussions of the proper intertwining of expertise and lay judgment (Nelkin 1979; Marcus and Hanson 1993).

The set of issues associated with expertise is only one cluster of con-

cerns that can mobilize studies of public sector organizations that can make contributions to our understandings of fundamental issues in organization theory and political philosophy. I would not presume to make a definitive specification of those issues, but I will list a few others that seem to me of considerable importance and in clear need of illumination through studies of practice.

First, the problem of *endogenous interests*. Most standard versions of democratic theory presume that a political system should respond to the preferences and identities of citizens. Similarly, most standard versions of organization theory presume that preferences and identities of actors shape organizational actions. Many versions of organization theory, as well as many versions of theories of democracy, assume that those preferences and identities are given exogenously. It seems clear, however, that they are also molded endogenously, that processes of democracy and organizational life create identities and preferences at the same time as they respond to them. From this point of view, a classical issue of democratic theory—the existence of a “common interest”—is less a question of whether such an interest arises spontaneously than it is whether such an interest can be constructed. The endogeneity of interests poses serious potential problems for many theories of democracy and organization, but those problems have been examined largely in isolation from detailed understandings of how administrative organizations actually work.

Second, the problem of the *inefficiencies of history*. Many theories of democracy and organization assume that political organizations adapt to the distributions of interests and resources among participants. Except for lags in adjustment, organizational forms and practices are assumed to come to match requirements defined by interests and their resources. When interests and resources change, the forms and practices are assumed to come to match the new situation. This conception, sometimes called historical efficiency, suggests that administrative practices and organizational forms can be predicted reliably from a knowledge of

current interests and the distribution of resources among them.

There are ample reasons for questioning such a conception. There are symptoms that interests and the distribution of resources coevolve with the institutions they affect. There is no easy way of assuring that the adaptive processes by which organizational forms and practices track environments have only one unique equilibrium, thus no way of assuming that the match between the current environment and the organization is independent of previous matches. These problems are well known in abstract discussions of adaptation, but there is much less in the way of detailed empirical studies of the precise ways in which historical adjustments in organizational rules occur.

Third, the problem of *imitation and diffusion* of organizational forms and practices. Recent research on networks has helped to identify the social and information structures by means of which ideas and practices are diffused, but the ways those networks form, change, and are used are little understood. More generally, the majority of studies of diffusion adopt two convenient assumptions of elementary epidemiology that are known to be violated in diffusion of organizational technologies and practices (Mahajan 1985). The first is the assumption of “reproductive reliability,” the idea that an object of diffusion is reproduced precisely when it spreads from one place to another. Most instances of social diffusion involve clear elements of transformation in the process of diffusion (Czarniawska and Joerges 1996). The second assumption is “network exogeneity,” the idea that the networks through which diffusion occurs are unaffected by the process of diffusion. Most studies of diffusion among and within organizations suggest that networks are affected by use and by success or failure in diffusion. The theoretical apparatus necessary to consider these complications is less of a problem (although it is not trivial) than is an adequately detailed understanding of what actually happens as practices spread from one agency to another.

Fourth, the problem of *trade-offs across time*. Organizations make de-

isions under conditions in which the costs and benefits of alternatives are distributed across time. For example, the gains to the exploitation of existing capabilities and to low risk alternatives are, on average, realized earlier than are the gains to exploration of possible new capabilities and high risk alternatives. As a result, organizations have difficulty engaging in exploration or risky alternatives, and the pursuit of short-run efficiency undermines long-run adaptiveness (Levinthal and March 1993; March 1994b). This is particularly the case in political organizations where turnover usually assures that those who make decisions will be different from those who incur their costs. This problem is a difficult one for theories of choice and theories of democracy, but it is made particularly difficult because the ways in which actual administrative agencies deal with costs and benefits that extend over time is not well-understood.

These are not new problems. Quite the contrary. They are well-known to the theoretical and philosophical literature, and recent work on them has clarified their importance to organization theory and to theories of democracy. But that recent work has, on the whole, been less diligent in exploring the empirical realities of each of these in the context of administrative practice and using those observations to develop new theoretical ideas. How, in fact, do interests evolve in the course of administrative practice? What, in fact, are the effects of history on contemporary administrative practice and how do they determine local equilibria? How, in fact, does knowledge spread and transform in an administrative system? How, in fact, does an administrative system deal with conflict between long run and short run concerns? And what are the implications for theoretical concepts of such features of administrative practice?

Answering such questions is, or should be, high on the priority list for students of public administration. And the empirical opportunities seem extraordinarily good. The world is currently running numerous grand experiments in the development of public administrative order. In particular, the European Union is

an idea in search of a workable administrative structure. Hong Kong is a colonial city-state seeking to find an administrative niche within a larger nation. South Africa is a colonial, white administrative structure transforming itself to serve a new black-dominated regime. The Internet is an anarchy developing an organization. Networks of associations among public agencies are elaborating around the world, forming new kinds of multinational administrative structures that may be transforming the national character of public administration. It is hard to imagine a richer opportunity for students of public administration to study such theoretically vital things as the ways interests develop, history meanders, practices spread, and institutions balance long-run and short-run perspectives. The next few decades will provide an unusually good set of occasions for studies of public organizations to contribute to our understanding of the ways in which organizations develop, stabilize, and change.

John Gaus was an optimist about scholarship. He thought research was important and that research on administrative practice could contribute important insights to organization theory and political philosophy. I think we have lost that vision a bit over the years, that we ought to restore it, and that the conditions exist for doing so in ways that will move research in public administration to its classical positions as a vital part of research on organizations and democratic political philosophy. It is, to be sure, a romantic and optimistic conception in an era of skepticism and cynicism; but romantic and optimistic conceptions need not be justified by their reality, only by their necessity and their beauty.

Notes

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1. Note, however, that Gaus was sensitive to the post-Second World War disillusionment of citizens with bureaucratic bloat and self-

serving officials. "It is believed that the bureaucracies are too swollen, that the attitude of the bureaucrat is too smug and too concentrated on security of tenure and automatic increases and promotions. The recruitment has been from too limited a portion of the population; staff and auxiliary services have been recognized as necessary, but in establishing them we have multiplied steps to be taken and forms to be filled, and have become skeptical of the resulting jargon and patter. . . . [It is believed] . . . that any public operation is wasteful, and that complex organizations are full of shelters for the mediocre or the intriguer" (Gaus 1947, 148).

2. For example, he wrote: "I would not like to live under a building code administered by a joint board of contractors and building trade officials; or have my public medical services, or admission to professions or crafts, exclusively controlled by doctors or other specialists. To be sure, I do not like my building code administered, either, by a corrupt spoilsman; but in the long run it is easier to oust him, than to oust from the minds of people the idea that life is a series of special bits of knowledge and interest, and that there is no corporate responsibility for evaluating and deciding concerning the government of the community" (Gaus 1947, 53-54).

3. He wrote: "It is the present duty of the administrator to stand between the expert on the one hand and the lay citizen on the other as interpreter and adjuster, and this, too, in a society in which the special interest that seeks to prevent such direction and control can win the attention of the public through new and subtle forms of appeal. The research that will lay bare and focus attention upon this relationship, which will interpret achievements and reveal the nature of failures is, in a democracy, not merely a matter of personal curiosity, but an obligation of the government, the public servant, the civic association, and the university" (Gaus 1947, 143).

4. In this regard, see for example the two Gaus chapters entitled "A Theory of Organization in Public Administration" and "American Society and Public Administration" in Gaus, White, and Dimock (1936). See also Gaus (1930). This latter report is an extensive compilation of the history and current (as of 1930) status of public administration in the United States. After observing that much of the research on public administration is routine and uninformative and fails to develop the larger and more theoretical aspects of the material, Gaus called for closer ties to historical studies of administration, to studies of industrial organization, and to legal scholarship, citing the examples of people such as John R. Commons, Walter Dorn, Mary Parker Follette, Walter Gellhorn, Howard Odum, Frederick Jackson Turner, and Jessie Turner. For example, the report noted that "the study of financial procedure has implications for theories of 'social control' and the exploration of administrative corruption may profitably bring us to an inquiry into the bases of social psychology" (Gaus 1930, 138).

5. It may be worth noting that John Gaus was unambiguous in seeing the construction of a public official as vital to public administration. He wrote: "We must go further and encourage that kind of professional and per-

sonal attitude toward the job that will operate from within the individual person who is the public official, and will help to fuse his private satisfactions with his public duties" (Gaus 1947, 123; see also, Gaus 1929).

6. Among American students of public administration and public policy, consider for example, John Kingdon's work on attention (Kingdon 1984), John Padgett's work on the development of Florentine political/administrative structure under the Medici (Padgett and Ansell 1993), Guy Peters' work on the politics of bureaucracy (Peters 1984), and Judith Tendler's work on development administration (Tendler 1975, 1997). For an example of a persistent voice for the political philosophy of public administration, consider Norton E. Long (1996).

7. The research varied from single case studies to large survey studies. They were heavily empirical. The publications were predominantly in Norwegian, but about 20% appeared in English. The count does not include graduate dissertations.

8. Note that the differences between American practice and that of public administration scholars in Europe are not simply differences in professional orientation and intellectual leadership. For example, the Norwegian political and educational system has been willing to devote considerably more resources to research on administration than the American system has, and the Norwegian organization of research and teaching on administration has treated that field more like an academic discipline, and less like a training program or a consulting service, than has the American system (Christensen and Lægrevid 1997).

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