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*The New Economics of Organization**

Terry M. Moe, *Stanford University*

Over the last ten years or so, an important new approach to the study of organizations has emerged within economics. It is perhaps best characterized by three elements: a contractual perspective on organizational relationships, a theoretical focus on hierarchical control, and formal analysis via principal-agent models. This paper provides political scientists with an overview of the “new economics of organization” and explores its implications for the study of public bureaucracy.

So far, positive political theory has not contributed much to our understanding of public bureaucracy. In part this is due to the unsympathetic treatment that rational modeling and most other modes of quantitative analysis have long received from students of public administration. The other side of the coin, however, is that positive theorists have not made much of an effort to develop theories of bureaucracy. Their concerns have centered around two basic mechanisms of social choice, voting and markets, and they have devoted little systematic attention to a third mechanism that is clearly important for understanding how societies and other aggregates make collective decisions. This third and relatively unexplored mechanism is hierarchy. Movement toward a positive theory of hierarchies would fill a serious gap in the social choice literature, while at the same time making a theoretical contribution that strikes to the essence of public bureaucracy, indeed of all organizations.

In fact, significant steps toward a positive theory of hierarchies have very recently been taken—but by economists, not political scientists. In small numbers, of course, economists made contributions to the study of public bureaucracy some time ago with the pioneering works of Downs (1967), Tullock (1965), and Niskanen (1971). But this new wave of theoretical work is different. It is already a large, complex body of literature that is the focus of innovation and excitement among a growing number of economists, and it reflects an unusual degree of theoretical coherence and cumulative effort. Work in this tradition tends to receive orientation from a distinctive economic approach to the analysis of organizations, an approach perhaps best characterized by three elements: a contractual perspective on organizational relationships, a focus on hierarchical control, and formal analysis via principal-agent models. This approach has emerged from recent attempts to move beyond the neoclassical theory of the firm, which assumes away all organizational considerations, to a theory of economic organizations that can explain why firms, corporations, and other enterprises behave as they do. Propo-

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nents are enthusiastic—and not only about its promise for elaborating the theory of the firm. They are clearly intent on revolutionizing the whole of organization theory.

The last decade has been marked by a growing interest in organizations within the economics profession. . . . The science of organizations is still in its infancy, but the foundation for a powerful theory of organizations is being put into place. (Jensen, 1983)

My central purpose here is to provide political scientists with an overview of the new economics of organization. Because, with some exceptions (due almost entirely to the longstanding efforts of Mitnick, 1973, 1980), work in this tradition has given virtually no attention to public bureaucracy, it seems fair to say that most political scientists are currently unaware of these new developments or at least are unfamiliar with them. My review is designed as an introduction, stressing basic concepts and arguments without dwelling on complicating details.

Two additional purposes are pursued along the way. The first is the application of this new analytical framework to public bureaucracy, with attention to the kinds of methodological and modeling issues that arise in making the transition from economics to politics. The second is the comparison of this new tradition with an alternative perspective, also with roots in economics, that is doubtless the most popular approach to organizations among political scientists: the behavioral tradition associated with Simon (1947), March and Simon (1958), Cyert and March (1963), and Cohen, March, and Olsen (1972). Despite areas of overlap, it seems clear that these are destined to be the major competing perspectives in the analysis of public organizations, and we must begin to assess their relative strengths and weaknesses.

The Economic Theory of Organization

The neoclassical theory of the firm is not in any meaningful sense a theory of economic organization. It centers around the entrepreneur, a hypothetical individual who, by assumption, makes all decisions for the firm and is endowed with a range of idealized properties defining his knowledge, goals, computational skills, and transaction costs. Virtually all aspects of business enterprise that organization theorists find interesting and consequential—from formal structure to social context and worker psychology to bounded rationality, adaptive search, and goal conflict—are thereby assumed away. The model firm is simply a black box that produces optimal choices automatically as a function of any given environment.

Similarly, the more general theory of perfect competition is not in any meaningful sense a theory of competition. Industries are assumed populated by large numbers of firms that take prices as given and make choices without any reference to the behavior of others. The interactive, highly strategic process we ordinarily associate with competition is entirely missing, as are the organizational forms and market-structuring devices—e.g., vertical integration, tie-in sales, resale price maintenance—that firms often adopt in responding to the uncertainties,

externalities, and transaction costs inherent in actual competitive environments. As Demsetz (1982) has suggested, it is less a theory of competition than a theory of decentralization—that is, a theory of how atomized decisional units, without any mechanism of central coordination other than the free-market system of prices, can produce outcomes that are optimal for the collective. The lessons to be learned are lessons about prices and markets, not about competition.

These models are easily criticized. But this is not new or even very disturbing to mainstream supporters of neoclassical theory, since the theory was never intended to be realistic in its assumptions nor to be accurate in its micro-level implications for individuals and organizations. Its development and use by economists have generally been grounded on its value in deriving formal implications for market prices and outputs, resource allocation, equilibria, and other aggregate properties of economic systems. Assumptions about the firm and perfect competition are simply vehicles by means of which these ends are pursued (Friedman, 1953; Moe, 1979).

Moreover, these textbook components of neoclassical theory have given rise over several decades to an enormous, richly diverse body of economic theory that does address many of the obvious gaps in the simple models themselves. A great deal of work has been done on the economics of imperfect competition, addressing not only familiar system-level issues of allocation and social optimality, but also issues of strategy and competitive process—e.g., in the analysis of oligopolistic competition. Often this has involved the application of new analytical approaches, such as game theory, designed to provide insight into economic contexts involving strategic interaction. A more recent body of work is in the area of information economics, which centers on uncertainty and its implications for economic behavior and markets. Arrow (1953) and Debreu (1959) have generalized the theory of perfect competition by deriving its equilibrium properties under conditions of uncertainty. Others have investigated a range of issues that naturally take on relevance when decision-makers are less than perfectly informed—e.g., optimal search and information transfer (Stigler, 1961), optimal sharing of risks (Arrow, 1971; Wilson, 1968; Spence and Zeckhauser, 1971), rational expectations (Muth, 1961), and signaling (Spence, 1974). Currently, the economics of information is perhaps the fastest growing area of microeconomic theory (Hirschleifer and Riley, 1979).

While these developments only scratch the surface, they help to illustrate that the simple models at the core of neoclassical economics are ultimately not so simple after all—and not so easily criticized—having been generalized to yield a complex, highly varied body of theory. This is less a unified neoclassical theory than a large family of diverse theories related by shared analytical foundations, chief among them: a focus on the individual as the unit of analysis; the assumption of rational, utility-maximizing behavior; a concern for efficiency, optimality, and equilibrium; and a preference for mathematical modeling over other approaches to theory construction. Many criticisms of simple economic models are not (or should not be) criticisms of neoclassical theory *per se*, since the weaknesses and

omissions of which these models are accused can often be handled within the broader neoclassical framework. The most unsettling criticisms—from behaviorists, institutionalists, or Marxists, for instance—are those that challenge the foundations that neoclassical theories generally share, for these point to deficiencies that may call for shifts to new and perhaps markedly different theoretical approaches. Given the magnitude of the edifice they threaten, it is little wonder that these alternative views have had a difficult time attracting mainstream support or even serious attention.

Early Departures from the Neoclassical View of the Firm

Much of the economic theory of organizations has emerged in the last ten years or so and is the product of writers whose methods and theoretical orientations are largely consistent with neoclassical tradition. Nonetheless, the origins and basic themes of the new economics of organization have been shaped most fundamentally by the pioneering views of three early dissenters from the neoclassical theory of the firm: Ronald Coase, Herbert Simon, and Armen Alchian. A brief review of some of their ideas provides a useful introduction to more recent developments.

Coase was among a diverse group of economists who contended that an understanding of economic activity required systematic inquiry into the institutional context in which such activity takes place. While others emphasized legal institutions (Commons, 1934) and other structural aspects of the environment, Coase focused on the firm itself, and, in his classic article “The Nature of the Firm” raised a fundamental question: why do these organizations exist? Why do economic agents in real economic contexts tend to arrange themselves hierarchically and coordinate their decisions via central authority rather than relying upon voluntary exchange and the automatic coordination provided by the market?

His answer—that hierarchy is often more efficient—is unsurprising, but far less important than the way he goes about constructing it. He notes that real-world production processes of any complexity generally involve many transactions among owners of capital, labor, land, specialized knowledge, and other inputs, and that these transactions are costly. In a hypothetical world in which all production is carried out purely by means of market relationships, transaction costs of two types are particularly important. First, an agent interested in arranging for the production of a good must somehow learn the myriad prices of relevance to the transactions he enters into, thus suffering the costs of information gathering and evaluation. Second, he experiences costs in preparing for, negotiating, and concluding separate contractual agreements for each transaction. Rational agents will naturally seek to minimize these transaction costs. The thrust of Coase’s argument is that many such costs can often be eliminated or substantially reduced by shifting to an alternative, nonmarket arrangement that internalizes some of the agent’s transactions with factor-owners and alters his contractual arrangements with them:

For this series of contracts is substituted one . . . whereby the factor, for a certain remuneration (which may be fixed or fluctuating) agrees to obey the directions of an entrepreneur within certain limits. The essence of the contract is that it should only state the limits to the powers of the entrepreneur. Within these limits, he can therefore direct the other factors of production. (Coase, 1937, p. 391)

Thus emerges a rudimentary economic organization, the firm, centrally characterized by the authority relation and the hierarchical direction of production. The driving force behind its emergence is efficiency: economic agents arrange production within firms—they substitute authority relations for market relations—in order to reduce transaction costs and produce more efficiently. Firms only emerge when this condition is met, and expand up to the point where the cost of an additional transaction within the firm begins to exceed the cost of the same transaction in the market. In equilibrium, some transactions will therefore be internalized within firms of various kinds and sizes, and some will be left to the market.

Coase's article was, in his own words, "much cited and little used" (Coase, 1972) for more than thirty years after its publication. With the new wave of studies in the 1970s, however, it was resurrected as a major source of theoretical ideas that, perhaps more than any other, has shaped the foundations of this emerging body of work on organizations. Several of his notions have proved especially influential. (1) Economic organizations are best understood by comparing their efficiency to that of the market. (2) In the real world, which is clearly not characterized by perfect competition, perfect information, or frictionless exchange, economic activities and organizational arrangements are best understood in terms of the transaction costs inherent in any system of exchange relationships among rational individuals. (3) These relationships are contractual in nature, and the firm is best understood as founded upon a distinct kind of contractual arrangement, the authority relation. Thus, in their earliest coherent statement, we have three central components of the new economics of organization: markets vs. hierarchy, transactions costs, and the contractual nature of organizations.

Simon, unlike Coase, was not primarily concerned with developing a new theory of the firm, although certainly that qualified as one of his interests. Rather, it seems fair to say that his work was motivated by two broader purposes: he sought to replace the conventional model of rational economic man with an empirically adequate theory of individual choice, and to use that theory of choice as a foundation for building a general theory of organization. His work on individual choice, moreover, was not simply a means to move toward a better understanding of organizations. As indicated by his later work (e.g., Simon, 1955, 1957, 1969; Newell and Simon, 1972), Simon has always been concerned first and foremost with modeling how people think. His departure from neoclassicism, therefore, has always been grounded on a fundamental challenge to the rational choice paradigm.

At the heart of Simon's contribution is his model of bounded rationality, first developed and applied to organizations in *Administrative Behavior* (1947). This

model recognizes that people are limited both in the information and knowledge they possess and in the computational skills they bring to bear in making choices. Thus, they cannot engage in the kind of informed optimization attributed to economic man, nor can they engage in the kind of decision making under uncertainty that information economists and game theorists have subsequently developed. Instead, boundedly rational individuals “satisfice.” This mode of choice, in turn, leads them to behave in a routine, myopic, but reasonably adaptive manner (see also March and Simon, 1958).

Simon’s theory of organization arises from this foundation. The key insight is that, just as individuals will routinize behavior if left to their own devices, so routines can also be imposed by organizational superiors, who can take steps to shape the decisional premises (information, beliefs, aspiration levels) of subordinates and provide them with the programmed responses deemed suitable for efficient pursuit of the organization’s objectives. It is this combination of bounded rationality and managerial efforts to program subordinate behavior that largely explains organizational structure. Individuals throughout the organization, precisely because they are boundedly rational, will behave in the routine, patterned ways characteristic of structured behavior; and (boundedly rational) managers, in seeking to shape and coordinate individual programs into an organized, efficient structure, impose behavioral routines via hierarchy, division of labor, communications flows, and training programs. Thus emerge the basic structural aspects of organization, all of them anchored in the inherent limitations on human decision-makers.

Simon’s work has clearly had a pervasive influence on the economics of organization, but its impacts are more subtle and less neatly categorized than those of Coase. The reason, it seems, is that Simon’s satisficing model just does not sit well with most economists in this area, who, while willing to depart from neoclassical tradition in viewing firms as organizations, are not willing to take the far more radical step of rejecting the optimization model of choice. This, in turn, naturally implies an unwillingness to accept the theoretical logic that drives Simon’s organization theory and supplies its distinctive features.

The major legacies of Simon’s early work are of a more general sort. His model of bounded rationality has been influential in arguing the empirical relevance of new elements from the psychology of decision making: e.g., memory, learning, information processing, selective attention, adaptation, socialization. Economists, given their interest in markets, generally find it easy to dismiss these sorts of factors as unnecessary complications. But organization theorists, including economists now doing work on organizations, are directly concerned with individual behavior and interaction among individuals, and they often find the psychological aspects of decision making impossible to ignore. Simon’s achievement was his early demonstration that it is possible both to take relevant aspects of human psychology into account and to employ aspects of economic methodology. Given the substantive concerns of organization theorists, this was an insight of great consequence.

He also demonstrated in detail that an organization theory can indeed be built on individualistic foundations, and thus that economic models and methods, appropriately modified, can be of real value in pursuing a better understanding of organizations, not simply individuals. Along the way, he shed dramatic new light on the rationality of routine individual behavior and organizational structure, showing them to be the product neither of pathological rule-following nor optimal managerial design, but rather a predictable reflection of the actual limitations under which human beings make decisions. Both aspects of his work—the value of economic methods for organizational analysis, the rationality of structure—are now cornerstone views of the new economics of organization.

Finally, Simon is widely cited for his concept of authority, which plays an integral role in linking bounded rationality and organizational behavior. Here he stressed that the authority relation is not characterized by command or fiat, as classical organization theorists suggest, but rather is two-way. The subordinate has a “zone of acceptance” within which he willingly allows the employer to direct his behavior. Thus, the nature of authority relation and whether or how well it works depend upon both parties to the agreement. This concept was developed in the earlier organizational work of Barnard (1938)—and, interestingly, it is virtually the same as the contractual authority relation so central to Coase’s analysis. But it is Simon who, in subsequent work over the years, has been responsible for explicating and popularizing the concept, and it is with him that the concept is normally associated.

Simon and Coase actually have a good bit more in common than the concept of authority. Because of this, their influences on the emerging economics of organization have been reinforcing in important respects. In the first place, they both reject the neoclassical tendency to treat organizations as black boxes whose internal structures and processes are uninteresting and beyond the useful scope of economics; both argue that economic outcomes cannot be understood, even in the aggregate, without an adequate theory of economic organizations, and that economic logic and methods can in fact be extended to this new area of theoretical inquiry. In the second place, both argue that the relative efficiency of hierarchy—and the explanation of organization—is due at base to limitations, imperfections, and frictions that shape individual choice in consequential ways but are omitted from the core neoclassical models. For Coase, transaction costs lead optimizing individuals to prefer hierarchy to markets. For Simon, uncertainty and limited cognitive capacity lead human decision-makers to satisfice and thus to engage in highly structured behavior. Both approaches are individualistic, anchored in economic models of choice, and characterized by the incorporation of new elements whose value is suggested by empirical observation.

While not apparent in the early work of either Coase or Simon, their theoretical traditions have come in subsequent years to share an additional component of truly fundamental importance, one deriving from Armen Alchian’s classic article “Uncertainty, Evolution, and Economic Theory” (Alchian, 1950). This is the notion of natural selection—or, more generally, the notion that the existence

and behavior of economic organizations can be understood by a translated application of Darwin's biological theory of evolution and natural selection.

Alchian is also concerned with the "removal of the unrealistic postulates" (p. 211) underpinning the theory of the firm, as well as with moving toward an understanding of firms as organizations. He rejects the assumption of profit maximization outright, arguing that there can be no definitive criterion of rational behavior under conditions of uncertainty—and that, at any rate, it is unnecessary for understanding the properties of economic organizations and economic systems. Firms survive neither because they choose according to this criterion nor because they achieve maximum profits. They survive because they make positive profits, regardless of how far they depart from what is objectively optimal, regardless of whether they are rational or irrational, brilliant or dumb.

Firms that are well suited to the particular conditions of their environment will be "selected" by the economic system for survival, while the rest will fail and disappear. Formal structures, leadership styles, and other organizational characteristics that happen to conduce to profitability in a given environment—even if the organizational participants are quite unaware of the causal connection—are thereby selected for survival, with less efficient organizational alternatives falling by the wayside. The resulting population of organizational forms is not necessarily optimal, for the system can only select from those forms that are actually tried by participants, and there is no guarantee in a world of uncertainty and bounded rationality that optimal forms will be discovered and put into operation. Nonetheless, economic natural selection favors increasingly "better" organizational forms over time, and, in the process, it corrects for the missteps and myopic choices that inevitably occur at the individual level.

In fact, the natural selection mechanism does more than this: it actually takes advantage of these lower-level blunders in upgrading the efficiency of the organizational population. To a large extent this is what economic innovation is all about. Just as mutation operates among biological organisms, so innovative behavior—whether conscious or entirely unintended—generates new organizational forms available for selection by the system. Although empirically common micro-processes like trial-and-error, imperfect imitation, and sheer chance may usually produce organizational failure, some portion of the new forms they produce will tend to be well suited to environmental conditions and perhaps far better suited than existing forms. The same is true for "rationally" designed innovations, arguably (but not necessarily) with a higher probability of success. Because of innovations, the uniformity induced by economic natural selection is dynamic rather than static, continually incorporating new forms and dropping old ones on the basis of demonstrated efficiency.

In a fundamental sense, Alchian's theory of economic organizations is different from those of Coase or Simon. He disavows an explicit model of individual choice (although it is clear he favors some sort of adaptive model), and he offers a system-level explanation of organizational emergence, structure, and survival that is largely independent of decision making at the micro level. His theory is

not individualistic. Yet it is precisely this independence of a distinct model of choice that ultimately renders it compatible with the individualistic theories of both Coase and Simon, however different they might be from one another, and that provides each of them with a new dimension of explanatory power.

This is a dimension of real importance. Whether individuals optimize under uncertainty or satisfice under the more limiting conditions of bounded rationality, they make choices about organizational structures, processes, and behaviors that may be far from objectively efficient and that may vary widely across individuals and organizations. Theoretical coherence at the micro level, therefore, may well help us to understand what is going on at that level without telling us much about aggregate properties of the system or its population of organizations. Alchian's logic of natural selection, when grafted onto either approach, provides a powerful means of deriving and integrating expectations about individuals, organizations, and systems. The result in either case is an approach that gains in scope and coherence, and that does so by remaining true to its underlying model of individual choice.

The Emerging Paradigm

The organizational views of Coase and Simon had little influence within economics for many years after their introduction. To the extent that theoretical work departed from the textbook neoclassical model by focusing on organizational concerns, it still tended to reflect the methods and broader theoretical orientations characteristic of neoclassicism as a whole.

Perhaps the best known of these elaborations challenged the assumption of profit maximization, arguing that an adequate explanation of business behavior requires attention to other, often more important goals that actual firms or their managers are observed to pursue. Emphasis has been placed upon sales (Baumol, 1959), the perquisites and discretionary resources of managers (Williamson, 1964), and rates of growth (Marris, 1964), among other things; profit often appears as a constraint, e.g., via a minimum acceptable level of profit, rather than as a value to be maximized. These efforts clearly depart from neoclassicism in ways other than their rejection of profit maximization: by their reference to managerial and other aspects of organization, for example, and by their concern for moving toward a utility function for the firm that better represents the realities of the business world. But the general approach remains neoclassical in its essentials, with the firm—whatever its objectives—still a unitary decision-maker that optimizes over all known alternatives.

Another set of elaborations comes from the economics of information. This diverse body of literature took off during the 1970s, and much of it overlaps with with the new economics of organization. Until recently, though, its contributions were less distinctively organizational and more concerned with developing analytical frameworks for the integration of uncertainty into neoclassical theory (Arrow, 1971; Stigler, 1961). There were exceptions—e.g., Marschak and Radner's (1972) work on teams, Arrow's (1964) early article on control in large organi-

zations, and the managerial application of statistical decision theory and operations analysis. And there were a number of works, less explicitly organizational in focus, that proved seminal to the organizational analysis of recent years—e.g., Wilson's (1968) work on syndicates and Spence and Zeckhauser's (1971) analysis of insurance, both of which deal with risk sharing. But, as with the literature on goals, work of direct relevance to organizations tended to proceed on neoclassical foundations.

Concerted attempts to bring about true paradigm shifts came in two forms: one a direct descendant of Simon, the other a direct descendant of Coase. The former was effectively initiated by Cyert and March's landmark study *A Behavioral Theory of the Firm* (1963). This book launches a direct attack on the conventional model by rejecting all its basic components—optimization, profit maximization, perfect information—in developing “an empirically relevant, process-oriented, general theory of economic decision-making by a business firm” (p. 3). Information and goals, no longer fixed by simplifying assumptions, become endogenous to dynamic decision processes internal to the organization; the forms they take and their relevance to organizational choice are matters to be explained, given the empirical context and the operation of satisficing modes of decision. Neoclassical assumptions are thus transformed into subtheories. The resulting theoretical structure is characteristic of earlier work in this tradition by Simon (1947, 1955, 1957) and March and Simon (1958), with emphasis on sequential search, adaptive learning, programmed behavior, and dynamic process. But it is perhaps best known for three of its more innovative contributions: (1) the notion that goals emerge and change over time in response to processes of coalition formation among organization members; (2) the concept of organizational slack, a resource cushion of “inefficiency” that actually performs crucial positive functions in maintaining policy coalitions and facilitating smooth organizational adaptation to environmental change; and (3) the demonstration, via a computer model of dynamic choice, that a behavioral theory can in fact be formally modeled and subjected (with great success in this case) to empirical test.

This alternative paradigm, while widely recognized, did not catch on among economists. Again, there are exceptions. The works of Winter (1964, 1971, 1975) and Nelson and Winter (1973, 1981) are important attempts to apply this paradigm, augmented by Alchian's natural selection argument, to system-level issues of markets and innovation. Also promising is Radner's (1975) formal analysis of satisficing behavior in organizational contexts. But the real influence of the behavioral paradigm has been among behavioral psychologists, political scientists, and organization theorists—who did not need to be convinced to abandon simple neoclassical models, and who were less disposed to take advantage of its characteristic economic logic and methodology.

In behavioral psychology the paradigm has been integral to empirical research on human problem solving (Newell and Simon, 1972) and individual choice (Kahneman and Tversky, 1973; Kunreuther et al., 1978), much of which has attempted to evaluate theoretical models of decision making by investigating

the actual thought processes of people in various kinds of decisional contexts. The evidence to date appears to be on the side of this alternative paradigm (see Simon, 1979).

In political science its influence is most apparent in studies of incrementalism (Braybrooke and Lindblom, 1963), budgeting (Wildavsky, 1964; Crecine, 1969; Padgett, 1980a), and policymaking (Allison, 1972; Steinbruner, 1974)—although it is often the preferred organizational approach, whatever the substantive area, when political scientists find organization theory relevant to their concerns. Recent developments are especially promising, particularly the work of Padgett (1980a, 1980b), whose stochastic models represent a major new step in formal application of the paradigm to politics.

In organization theory as a whole, work on the behavioral paradigm constitutes a small and historically not-very-influential part of the literature, dwarfed by open systems analysis and enormous numbers of correlational studies on micro (e.g., morale, leadership) and macro (e.g., structure, technology, size) organizational topics. But it appears to be taking on greater prominence, due largely to the work of March, Olsen, and Cohen, who, in a series of articles and books, have developed and applied a distinctive version of the paradigm (reflecting their “garbage can” model of organizations) that plays upon certain of its themes—especially the ambiguity (of goals as well as beliefs) and randomness inherent in processes of organizational choice (Cohen, March, and Olsen, 1972; March and Olsen, 1975; Cohen and March, 1974; March, 1978). These efforts, in combination with new sociological perspectives on the environment (Pfeffer and Salancik, 1978; Aldrich, 1979; Hannan and Freeman, 1977) and new developments in computer modeling of adaptive organizational behavior (Cohen, 1981, forthcoming [1984]; Cohen and Axelrod, forthcoming [1984]), clearly are at the frontiers of modern organization theory.

Despite the lack of enthusiasm within economics, then, the behavioral paradigm has achieved widespread recognition and is truly interdisciplinary in the theory and research it has spawned. It has exciting potential for growth within disciplines, but even more exciting is its potential for new and productive linkages across disciplines (see, e.g., March and Shapira, 1982; March, 1981). With all the work that has been carried out in the last few decades, the value of these linkages is becoming apparent to increasing numbers of social scientists.

The second paradigm, deriving from Coase, has none of these demonstrated advantages. Not at this time, anyway. The modern foundation for this competing paradigm has only recently been constructed, and analyses developing its central themes and expanding its substantive scope are clearly in their early, formative stages; indeed, works reverentially cited as classics in the area are barely ten years old, if that. In contrast to the behavioral paradigm, this one remains isolated within economics, and questions of its interdisciplinary value have only recently begun to be addressed in regard to areas, such as accounting (Jensen, 1983; Wilson, 1983), that are close to being part of the discipline itself. Virtually nothing has been done to explore its value for issues of traditional interest to

political scientists or sociologists, and whether it can rival the demonstrated value of the behavioral paradigm in these disciplines is entirely uncertain.

The new economics of organization, while shaped in important ways by Simon's ideas, is largely founded upon this second paradigm. Among its basic elements are those outlined above: the contractual nature of organizations, markets vs. hierarchies, transactions costs, the rationality of structure, individualistic explanation, and economic methods of analysis. Standard neoclassical notions—optimization, marginality, equilibrium—are often central to work in this new tradition, and writers tend to view their efforts as “complementary to, rather than a substitute for, conventional analysis” (Williamson, 1975, p. 1). In general, then, this contractual paradigm represents a less dramatic break with neoclassicism than the behavioral paradigm, and many of its adherents are concerned with seeing it integrated into the broader neoclassical framework (or seeing the latter expanded to include it). Nonetheless, its insistence that firms be viewed as organizations and that explanation of economic outcomes requires an underlying organization theory is a definite, important departure from the mainstream—so much so that it is unclear that an integration with neoclassicism as we know it is even possible.

The renaissance of the contractual paradigm is rooted in Alchian and Demsetz (1972). They argue, as Coase did, that the particular organizational (contractual) arrangement we identify with the capitalist firm is more efficient than alternative contractual arrangements occurring purely within the market, and that the existence of firms can be derived from an analysis of rational behavior. While their analysis could be couched in the general terms of transaction costs, they focus more narrowly on the “shirking-information” problem, and their claim is that the firm exists because it provides a better solution to this problem than markets do. A brief review of their argument introduces a number of important concepts.

Alchian and Demsetz note that for complex production processes there is typically a gain from cooperation. Teams of input owners can produce more in cooperation with one another than separately, and this gives them an incentive to coordinate their actions. Yet team production also suffers from a peculiar problem. Precisely because of the complex interdependence of tasks and their frequent remoteness from organizational output, the marginal products of individuals are difficult and perhaps impossible to determine; thus, in the absence of mechanisms for monitoring each individual's behavior, they cannot reward one another according to individual impacts on output. Division of the team's surplus among its members, then, must proceed according to some other rule—equal sharing, for example—that does not depend upon knowing each person's productive impact. It happens, however, that this induces a distinctive kind of reactive behavior among the members themselves: shirking.

Each individual knows that his effort has some impact on the team's reward, but that this reward is split among all members; thus, while he bears the full cost of his effort, he receives only part of what his effort produces. On the other hand,

when he shirks by reducing his effort expenditures, the savings in effort accrue only to him, and the resulting losses in team reward are borne largely by the others. A fundamental asymmetry therefore characterizes the structure of incentives, and each member will tend to find it in his own best interests to engage in some degree of shirking. Team production and rewards fall as a result, and each member may actually be worse off than if no one had shirked from the outset.

While they may realize that their collective fate is suboptimal, moreover, that will not in itself allow them to solve the problem: they are trapped in what is essentially a prisoner's dilemma. Their cooperative effort is plagued by a public goods problem (where the public good is the team reward) that promotes free-rider behavior (shirking) among members. The problem is a reflection of underlying externalities: for each individual, the fact that others benefit from his productive effort is external to his decision calculus, and he therefore chooses more leisure than is socially (for the team) desirable.

In view of this, how can member shirking be reduced? Alchian and Demsetz argue that the usual market mechanisms—e.g., allowing outsiders to bid for shirkers' places on the team—will not work, since bidders cannot know who the shirkers are and, worse, bidders would also have incentives to shirk once they join the team. On the other hand, if information could somehow be gained on the marginal products of individual members, they could agree to be rewarded on this basis, externalities would thereby be reduced, and everyone could be made better off as a result. Thus, the way to mitigate the shirking problem is to monitor the productive efforts of team members. If monitoring were perfect, each individual's marginal product could be known with certainty and shirking could be completely eliminated. But, because monitoring is costly, the best the team can do is to invest in monitoring up to the point where its marginal costs begin to outweigh the marginal benefits from reduced shirking. Some degree of residual shirking is thus both rational and to be expected.

How can monitoring be carried out most efficiently? To avoid hierarchy, members could rotate the job of monitor among them; but this sacrifices the efficiencies of specialization. A better alternative is to hire an outsider or appoint a team member to be a full-time monitor. This allows for specialization, but points to still another question: who will monitor the monitor? Because his marginal product will be unknown, he will also have an incentive to shirk and thus to monitor less efficiently than he otherwise might—which implies, in turn, that member marginal products will be measured improperly and member resources will be misallocated. Given the monitor's uniquely central role, the key to team productivity is to ensure that he has incentives to do his job efficiently. The best way to do this, Alchian and Demsetz argue, is to give him marketable title to the team's rewards and establish him as central contracting agent with all its members. He would then pay members their estimated marginal products, based on bilateral contracts between him and each member, and keep the remaining amount as personal income. This new status clearly enhances his incentives to monitor efficiently. It also gives him both the incentive and the authority to adjust pay-

ments in accordance with observed productivity and to make changes in team membership in the interests of higher team rewards. Moreover, this is an arrangement that all rational team members should favor, for it promises to make them all better off by mitigating the shirking problem. The hierarchical relationship that results is not one of fiat or dominance, but a contractual *quid pro quo* into which they all voluntarily enter in pursuit of greater gain.

This mutual accommodation, of course, is the characteristic organizational form of the neoclassical firm, with the monitor as entrepreneur. It emerges spontaneously from the contracting behaviors of rational economic actors whenever the net value of team production (including monitoring costs) exceeds the net value of the corresponding bilateral market exchanges among independent input owners. Under such conditions, economic organization in the form of the business firm is the efficient production alternative. That is why it exists.

Alchian and Demsetz thus go well beyond Coase in demonstrating why factors overlooked by the neoclassical model in fact operate to provide a rational foundation for economic organization. Their perspective has obvious promise in application to issues of organizational management and control and, indeed, to a vast range of theoretical questions surrounding the superior-subordinate relation characteristic of all hierarchies. For their logic implies that hierarchy, monitoring, incentives to shirk, and member productivity are integrally bound up with one another. Thus, while their analysis is explicitly about firms, it rests on theoretical foundations that capture something inherent in organizational and hierarchic behavior generally. It would appear that this broad analytical scope, combined with their implicit acceptance of the optimization model, largely explains the interest and enthusiasm their article has generated. For the first time, economists had a theoretical perspective that rivaled the behavioral paradigm in organizational relevance without straying too far from neoclassical foundations.

Subsequent work on the economics of organization testifies to the generality and promise of the contractual paradigm. Substantive attention no longer focuses on the classical firm, but addresses the full array of organizational topics. A favorite is vertical integration, with attention to the conditions under which rational firms would substitute nonmarket administrative relationships for cross-firm market relationships by annexing other organizations operating at different stages of production or distribution (Williamson, 1975; Demsetz, 1982). Another is the substance of the employment relation—the internal labor market (Doeringer and Piore, 1971)—where investigation includes payment systems (e.g., salary vs. wages vs. piece rates), promotion systems (seniority vs. merit), unions, worker qualifications, on-the-job learning, employer-worker bargaining, and other topics of traditional interest (Spence, 1975).

The one that has stirred the most excitement is the separation of ownership and control. A traditional topic of concern and controversy among economists (Berle and Means, 1932), it has now taken on special interest—for, in shifting the Alchian-Demsetz logic to the upper reaches of organization, new light is shed on the rationality of governing structures generally. In the case of the corporation,

attention centers on whether its characteristic form of organization is efficient relative to market and nonmarket alternatives. The corporate form allows for decisional specialization and unrestricted risk sharing, both conducive to efficiency in large-scale enterprise, while, at the same time, important mechanisms are available for mitigating the shirking problems inherent in the stockholder-manager relation. (1) The unrestricted sale and ownership of stock means that stockholders can pull their investment out whenever management decisions fail to yield profits that compare favorably to those of other corporations. (2) Corporations whose potential for profit goes unrealized due to managerial shirking are prime targets for takeovers by other organizational management teams. (3) Stockholders generally delegate most control and monitoring functions to small boards of directors, which have far better information and resources for mitigating shirking problems. Under a variety of economic conditions, therefore, the modern corporation emerges as an optimal organizational form. It allows funds from large numbers of unrelated, decisionally uninvolved investors to be productively employed by specialized, skilled decision-makers, while the shirking problems that otherwise threaten productive efficiency are substantially mitigated (see Fama, 1980; Fama and Jensen, 1983a; Jensen and Meckling, 1976).

By extension, this same logic can be applied to other types of organizations, for the stockholder-manager relation is but a special case of the more general relation between those who have a "right" to control an organization (ordinarily, residual risk-bearers) and those decision-makers (managers) who in fact make most of the important organizational decisions. As contributors to the literature are well aware, current analyses of corporate governance are important not simply for what they tell us about corporations, but for what they imply for governance in a wide variety of organizations (Fama and Jensen, 1983a, 1983b).

Work in this tradition also reflects a shift in theoretical focus from Alchian and Demsetz, due in part to criticism of basic points of their argument: that production processes are not truly inseparable, that the causes of shirking are more general than their analysis of teams implies, and that factors other than shirking are also central to the rationality of organization. Probably the most comprehensive alternative to the Alchian-Demsetz logic is developed by Williamson in *Markets and Hierarchies* (1975), where an explicit attempt is made to integrate major components from the contractual and behavioral paradigms. Williamson argues that the relative efficiency of organization arises from the joining of uncertainty and small numbers (environmental factors) with opportunism and bounded rationality (human factors). Uncertainty combines with bounded rationality to limit reliance on long-term contracting—which would require specification of complex future contingencies—and to encourage the substitution of internal organization, where uncertainty can be absorbed through adaptation, learning, sequential choice, and related mechanisms in the Simon tradition. Similarly, small numbers combine with opportunism to limit reliance on frequent short-term contracting. If an economic agent relies on the market in contracting and recontracting for the myriad services needed for production, service-suppliers

gain specialized knowledge and skills through their performance of these tasks and become far better qualified than others in the market. In subsequent contracting situations, then, large-numbers exchange tends to be transformed into small-numbers exchange in which the moderating effects of market competition are largely absent and, due to opportunism, service-suppliers will use "information impactedness" to their own advantage in exacting concessions. The way to get around this is to avoid engaging in frequent short-term contracting, producing instead by means of an organizational arrangement. Given appropriate reward and monitoring structures, transaction costs can be reduced substantially.

Yet Williamson's framework is not by any means standard. To the extent that this literature has a common theoretical emphasis, it is instead on more general aspects of the contractual relation: incomplete information, especially when asymmetrically distributed across the contracting parties, and conflict of interest. Together, these offer a more general explanation of the shirking-information problem that is not tied to teams or production inseparabilities. They also provide a more powerful framework for the analysis of hierarchical relationships, one with clear links to information economics, game theory, and other areas of the discipline.

Two concepts at the heart of this framework are adverse selection and moral hazard. Both emerged from early applied work on insurance and were incorporated into the modern work on information and organization in recognition of their much broader theoretical significance. Adverse selection derives from unobservability of the information, beliefs, and values on which the decisions of others are based. Consider an example from the employment relation, in which an employer seeks applicants for a marketing research position requiring independent, creative work. While he would like to attract highly qualified and motivated individuals, he cannot know any given applicant's true intelligence, aptitude, or work habits. What he can do is to proceed on the basis of rough indicators, like education or job background, thus declaring his willingness to pay a certain price for individuals who are nominally qualified according to these indicators; the price is in effect a statistical average, reflecting both the estimated implications of the indicators for productivity and the estimated variation in productivity across all individuals who qualify. The individual evaluating this price, on the other hand, does know his own qualifications and work habits. If he is in fact highly intelligent, creative, and motivated, he will tend to find that the employer's proxy-based price understates his true economic value; while, if he is in fact quite lacking in all these desirable traits but still meets the formal proxy requirements, he will tend to find that the price overstates his true economic value. The latter type of individual is thus likely to view the job as an opportunity, while the former is likely to look elsewhere—especially for "better" jobs whose proxy categories are either more finely measured or simply pitched at a higher level. In addition to all this, individuals who happen to place high value on leisure are likely to find this job particularly appealing, because supervision is minimal and productivity is difficult to measure; high-productivity individuals, in contrast, will find

the measurement problem a negative factor, since they want their true productivity to be observed and rewarded.

Because the employer cannot in general know these things about applicants (i.e., each applicant's true "type"), whereas the applicants themselves clearly do, his recruitment effort will tend to suffer from adverse selection: he will attract a disproportionate number of low-quality applicants. Moreover, even though he and the best of these applicants share a common interest—he wants to hire the best, the best wants to be hired—this may not ease his problem, for the asymmetry remains. He cannot know for sure which applicant is truly the best, and the best applicant cannot credibly claim that he is in fact superbly qualified because all individuals have incentives to make the same sorts of claims in order to get the job.

Moral hazard arises from the unobservability of actual behavior in the ex post contracting situation—here, after an applicant has been hired. The employer cannot know for sure to what extent the individual is productive and instead must ordinarily rely upon proxies—e.g., quality of reports, timeliness, diligence. The individual then has an incentive to redirect his efforts toward the proxy measures (a phenomenon called goal displacement in the sociology of organizations) rather than the abstract goals implicit in the employment contract; and he also has an incentive to substitute leisure for productive effort, since the unobservability of his marginal product allows him to achieve these benefits at low cost (the expected cost of being detected). Shirking behavior, therefore, is an aspect of moral hazard, with the incentive to shirk deriving from underlying information asymmetries.

Moral hazard and adverse selection are general problems whose potential is inherent in all contracting and hierarchical relationships. As theoretical concepts, they are particularly valuable for understanding situations in which one party seeks to control the behavior of another, or, more generally, to achieve certain outcomes (such as profits) by relying on and structuring the behavior of various other actors. These, of course, are the essence of organizational analysis, whether the substance has to do with decentralization, division of labor, formal rules, structure, communication, or ownership vs. control: all are reflections of efforts to control the productive efforts of organization members—and all, because of information asymmetries, are shaped by moral hazard and adverse selection.

Consider what happens, for instance, when organizations decentralize. Tasks and authority are delegated to lower-level units in the expectation that they will use their specialized knowledge and productive capacities to contribute toward organizational ends; but the inevitable information asymmetries create incentive problems. An upper-level unit soliciting policy inputs from lower-level units will pay the price of adverse selection, since only the lower-level units know what information their inputs are based upon, and they can use that to their own advantage; similarly, a superior unit trying to increase a subordinate unit's productive efficiency will have to grapple with the problem of moral hazard, since the observability of productive behavior is asymmetrically distributed in the latter's favor. To take another example, consider the relationship between stockhold-

ers and managers. This is a variation on the same theme: stockholders want to control managers in the interests of profits, but managers tend to possess far better information both about actual organizational behavior (including their own) and about the technical and cognitive grounds on which proposals are formulated and adopted—leading, once again, to moral hazard and adverse selection problems for stockholder control efforts.

Across these and other organizational areas, therefore, the economic analysis of organizations tends to center on questions having to do with the incidence and content of information asymmetry, why the asymmetry exists, what it implies for contractual outcomes, and how the asymmetry or its consequences can be mitigated. Theoretical inquiry into these questions takes diverse forms, but one analytical framework is so eminently well suited to the task that it has become the dominant framework for the formal analysis of hierarchy: the principal-agent model. This model was initially developed to investigate more general questions of incomplete information and risk sharing (Ross, 1973; Spence and Zeckhauser, 1971), not to spur the development of organization theory, and it has become a major analytical tool in the general literature on information economics. But its relevance for explicitly organizational analysis was quickly recognized, and applications to the central issues of organizational theory are growing (Jensen, 1983).

The principal-agent model is an analytic expression of the agency relationship, in which one party, the principal, considers entering into a contractual agreement with another, the agent, in the expectation that the agent will subsequently choose actions that produce outcomes desired by the principal. Examples of agency relationships are legion: lawyer-client, doctor-patient, broker-investor, politician-citizen, and, most generally, employee-employer. As these examples tend to suggest, a principal may seek out an agent for various reasons. Often he may lack specialized knowledge or legal certification that the agent possesses, and sometimes the size or complexity of the task simply requires coordinated action by persons other than himself. But given some motivation for relying on an agent, the principal's decision problem is far more involved than simply locating a qualified person—for there is no guarantee that the agent, once hired, will in fact choose to pursue the principal's best interests or to do so efficiently. The agent has his own interests at heart, and is induced to pursue the principal's objectives only to the extent that the incentive structure imposed in their contract renders such behavior advantageous.

The essence of the principal's problem is the design of just such an incentive structure. The difficulty, of course, is that information about the agent's actions and the inputs on which they are based is not only imperfect but skewed in favor of the agent, yielding adverse selection and moral hazard problems that must somehow be mitigated. The design of an efficient incentive structure is thus bound up with the development of monitoring systems as well as mechanisms for inducing the agent to reveal as much of his privately held information as possible. The principal must weave these interrelated components into a contractual framework

that, in mitigating the informational asymmetries and structuring rewards, prompts the agent to behave as the principal himself would under whatever conditions might prevail.

The logic of the principal-agent model, therefore, immediately leads us to the theoretical issues at the heart of the contractual paradigm: issues of hierarchical control in the context of information asymmetry and conflict of interest. It is a natural framework for the economic analysis of organizations of all kinds, and adherents are enthusiastic about its promise.

The problem of inducing an "agent" to behave as if he were maximizing the "principal's" welfare is quite general. It exists in all organizations and in all cooperative efforts. . . . The development of theories to explain the form which agency costs take . . . and how and why they are born will lead to a rich theory of organizations which is now lacking in economics and the social sciences generally. (Jensen and Meckling, 1976, p. 309)

The advantage of a simple analytical framework is that organizational issues can be cast in a clear, rigorous manner that allows for the application of conventional economic methods. A corresponding disadvantage, however, is that such a framework sometimes encourages highly complex mathematical treatment of trivial problems; form tends to triumph over substance, and analytical concerns tend to take on lives of their own that have little to do with the explanation of empirical phenomena.

The principal-agent literature reflects both these positive and negative forces. It has clearly enhanced our understanding of hierarchical relationships and represents a major advance beyond the usual sociological methods of organizational analysis. In part this is due to important theoretical conclusions—regarding, e.g., incentive mechanisms for revealing an agent's "type" (Myerson, 1979), the role of signaling and screening devices (Spence, 1974), conditions for the optimality of alternative reward systems (Shavell, 1979; Holstrom, 1979) and monitoring and accounting systems (Baiman, 1982), among others. But it is also due to its demonstrated value in clarifying what the relevant aspects of hierarchical relationships are. It cuts through the inherent complexity of organizational relationships by identifying distinct aspects of individuals and their environments that are most worthy of investigation, and it integrates these elements into a logically coherent whole.

On the negative side, however, much of the current literature focuses on matters of little substantive interest; "authors are led to assume the problem away or to define sterile 'toy' problems that are mathematically tractable" (Jensen, 1983, p. 333). Inherent substantive complexity is magnified rather than simplified by many of these efforts, and analytical progress often has little to do with arriving at better explanations of truly interesting types of organizational behavior. The real danger, critics would argue, is that the economics of organization will go the way of mainstream neoclassicism, with analytics and methods dominating substance. This would be the ultimate irony—for this new area began, after all, as an effort to put realism and substantive relevance back into microeconomic theory.

Yet there are impressive counterbalancing influences. Central figures in the economics of organization—Simon, Williamson, Alchian, Demsetz, Jensen, and others—are strongly committed to the development of analytical frameworks firmly anchored in the substance of organizations. The behavioral paradigm itself is another important factor; its emphasis on adaptation, learning, search, and other empirical aspects of dynamic organizational processes are forceful reminders that static, dyadic (as most are) principal-agent models assume away much of what a comprehensive theory of organizational behavior must ultimately account for. And finally, there is the imposing reality of organizations: hierarchic systems of interdependent participants whose joint, time-dependent behavior appears to be well beyond the capacity of simple rational models to explain, at least in most respects. The disappointing performance of *N*-person game theory, after much early excitement, is but a case in point.

In all likelihood, the contractual paradigm will continue to dominate economic approaches to organization, owing to its linkages to neoclassicism. But it will also continue to include diverse approaches, some far more concerned than others with the empirical richness of organization. The principal-agent model, ideally suited to the analysis of hierarchical relationships, is understandably the major means of formal modeling at present and should become well established as an important tool of organizational analysis. Given the countervailing influences, however, and given the broader tasks of organization theory, the current fervor surrounding the principal-agent model will likely give way to a more eclectic methodology within which that model plays a less pronounced but integral role.

Applications to Public Bureaucracy

That this framework might be an important tool for political and social analysis more generally was recognized from the beginning by Mitnick (1973, 1980). But Mitnick's arguments attracted little attention, and to this day there are very few applications of organizational economics to public bureaucracy, or to any area of politics for that matter. New work along these lines is only now emerging (Weingast, 1983; Kalt, 1981; Kalt and Zupan, forthcoming [1984]), a spillover effect of its demonstrated analytical power and growing popularity within economics. Conditions are ripe, however, and it is only a matter of time before politics becomes the contractual paradigm's new frontier.

My purpose in this section is to suggest some of the basic elements that come into play when this framework is applied to public bureaucracy—and to emphasize that the transition from economics to politics is by no means straightforward. The discussion is usefully structured around three questions to which the contractual paradigm clearly must assign central importance. (1) Why do public bureaus exist, as opposed to alternative arrangements for the provision of public services? (2) How can bureaucratic superiors control bureaucratic subordinates? (3) How can politicians, as principals, control their bureaucratic agents?

Why Does Public Bureaucracy Exist?

The new economics of organization engages in a familiar kind of reasoning associated with the social contract theorists in political theory. Reference is to a state of nature comprised of autonomous decision-makers, and questions concern the kinds of social agreements at which these individuals would voluntarily arrive. In the economic analogy, the state of nature is characterized by a free market populated by economic agents. As Williamson (1981, p. 1547) puts it, "in the beginning, so to speak, there were markets." Rational behavior then leads to various sorts of contractual arrangements, some of which assume well known productive forms: the firm, the modern corporation, etc. These forms emerge because, given transaction costs, monitoring costs, and other obstacles to cooperative exchange, rational actors find them more efficient than markets or alternative organizational arrangements.

Suppose we go along with this line of inquiry and explore its implications for the existence of public bureaucracy. Could it be, as the conventional logic seems to imply, that public organizations also exist because they are more efficient than the alternatives? For starters, we can only recognize that the state-of-nature argument is more than a little strained for public bureaus. Williamson's premise must be turned on its head: in the beginning, there typically were no markets for public sector services. Economists have long pointed to the supply of public goods as a major justification for the existence of government and its power to tax—but the problem that public goods pose, of course, is the failure of markets. Citizens have inadequate incentives to reveal their true demand and to contribute accordingly, and potential suppliers have inadequate incentives to produce. A solution is for the government to act as the citizens' agent: it arranges for the optimal supply of the public good and taxes each individual according to the benefits he receives. Yet how does it arrange in the absence of an available market? There are two basic possibilities: it can create its own bureaucracy, or it can engage in marketlike transactions by contracting with private actors who promise to provide services at a stipulated price (there is no prevailing market price) to be paid by the government. The latter option will require that producers move into new areas of activity or that new organizations be created, perhaps with governmental assistance.

The contractual paradigm suggests, among other things, that government is more likely to prefer the private contracting method when contractor "types" (reputations, expertise, honesty) are well known, service outputs are easily measured, and negotiations are not plagued by the small-numbers problem. Knowledge of contractor type is necessary to minimize problems of adverse selection, whereby the government is in danger of attracting inefficient or unreliable candidates; measurability and therefore monitoring are important if the government is to minimize moral hazard—clearly a distinct possibility with profit-maximizing producers who can keep whatever is not paid out to the government, and who may even be reimbursed for unnecessary expenditures and actions that were never taken; and small numbers points to a situation in which the government is ulti-

mately boxed into negotiation with one or a few contractors, who, especially after some initial period of performance, have knowledge and skills so specialized that they become “irreplaceable” and are in a position to reap near-monopoly gains through hard bargaining.

A standard example of a public service well suited for private supply is garbage collection. Refuse companies are relatively inexpensive to operate, allowing the government to employ several at once in different areas of a city or to shift from one to another (or threaten to). This element of potential competition provides information over time on company “types,” and it undermines the threat of small-numbers bargaining. Moreover, because performance is easily observed and evaluated, shirking can be controlled reasonably well without substantial monitoring costs. For all these reasons a city government may find it quite efficient to contract out for refuse collection services.

When these conditions are not met, however, the government may find it more efficient to internalize contracting relationships by setting up its own bureaucracy. Consider the provision of police services. Adverse selection is a severe problem here, since the job of police officer is unfortunately likely to appeal to many people more interested in power and physical combat (and, in the old days, graft) than “keeping the peace”; efficient performance requires candidates with sensitive judgment and well-developed interpersonal skills, qualities that are difficult to observe at the hiring stage—particularly if someone else is doing the hiring. Behavioral outputs are also difficult to observe, in part because it is unclear precisely what the operative goals of a police department ought to be and how their achievement can be measured. While a contract may easily stipulate proxy criteria (e.g., number of hours of police patrol), specifying “amicable race relations” or “effective crime deterrence” is far more problematic, as is performance evaluation on those bases—leading to serious moral hazard problems. Finally, should a town decide to use the marketlike mechanism of contracting for police services with a larger municipality or county, it faces a small-numbers bind: the sole supplier can use its monopoly position to inflate price and reduce quality. All of these are arguments for establishing a public bureaucracy for the provision of police services. Adverse selection can be controlled via personnel departments or, better, by use of police academies that screen, train, and evaluate police recruits. Behavioral outputs, while still difficult to observe, can be monitored far more closely by insiders; and, if desired, special mechanisms can be adopted—civilian review boards, internal affairs divisions—to introduce different types or still greater degrees of output surveillance. Thus the asymmetries that promote moral hazard can be attacked more directly. And the small-numbers problem is reduced by shifting from an outside sole supplier to insiders whose jobs, pay, promotion, and careers are to some extent subject to hierarchical control, and whose incentives and opportunities to bargain as internal monopolists are shaped accordingly.

In many cases, however, bureaucracy and contracting out are not dramatically different organizational arrangements in practice. This is because contract-

ing typically involves substantial elements of hierarchy and monitoring. Most federal contracting, for instance, takes place in the area of defense; and the Department of Defense is continuously involved in ratifying and directing the decisions of defense contractors, observing their productive efforts, and evaluating outputs. Such is the involvement, in fact, that critics claim defense contractors are effectively an integral part of the Department's bureaucracy (Melman, 1970). This is a familiar example of "close" government-contractor relationships, but it is hardly unusual—and is precisely the kind of thing the contractual paradigm would lead us to expect. All principal-agent relationships contain within them characteristic "agency problems," resting on information asymmetries and conflict of interest, and it is generally rational for the principal to mitigate these problems through hierarchical controls and monitoring. Depending on the nature and severity of the problems, this may entail setting up a new public organization, delegating tasks to a private contractor with virtually no strings attached, or any "mixed" type of contractual arrangement between the two poles. It is not a dichotomous choice, but a matter of balance.

This perspective on public bureaucracy helps to structure our thinking about the relative efficiency of alternative organizational arrangements. Yet we must remember that the contractual paradigm has been developed with reference to private organizations, particularly business firms, and that some of its most fundamental components must be modified if its application to public organizations is to be meaningful and instructive. Barring this, straightforward application of the paradigm could be seriously misleading. Consider the following points of departure.

1. Politicians are the decision-makers with authority to determine whether public bureaus will be established to deliver governmental services, and, in general, how these bureaus will be structured. They play the entrepreneurial role. Yet politicians are not primarily motivated by productive efficiency or the public interest in making such decisions. Most obviously, electoral considerations prompt concern for constituency service, pleasing interest groups, rewarding contributors, avoiding conflict, taking symbolic stands, and claiming credit for popular outcomes (Mayhew, 1974; Fiorina, 1974, 1977). This means that, when politicians choose between public bureaucracy and contracting out, they are generally not choosing on efficiency grounds—nor are they, in the conventional sense, seeking an optimally balanced set of hierarchical controls and monitoring mechanisms in designing the details of the contractual arrangement. A contractor may be chosen because he is a major contributor to a subcommittee chairman's campaign; a bureau may be created because it opens opportunities for patronage; and the control structure may have less to do with the direction of policy than the funneling of expenditures to legislative districts. We would therefore be quite wrong to conclude that public bureaucracy exists "because it is efficient" without qualifying what we mean by "efficient." It exists and takes peculiar structural forms because it conduces to the well-being of critically located politicians. It may be an efficient organizational arrangement from their standpoint, but not necessarily

from anyone else's—unless, of course, there is something like a hidden hand operating to guarantee that, in pursuing their own self-interest, these politicians are unintentionally maximizing social welfare as a by-product.

2. Unlike the private sector, where the argument is at least plausible, there is no hidden hand to guarantee optimality for the public sector. On the demand side, common criticisms have to do with the ways in which electoral and political institutions distort the underlying distribution of citizen preferences, promoting fragmentation, parochialism, and cooptation by special interests rather than concern for the general social welfare (see, e.g., Lowi, 1969; McConnell, 1966; Schattschneider, 1960; Shepsle and Weingast, 1981). These familiar problems are compounded by a fundamental flaw on the supply side: service suppliers in the public sector are not subject to forces of natural selection based on economic efficiency. Under a market system, producers who fail to operate efficiently tend to be weeded out; they are underbid in product markets, cannot attract funds in capital markets, and ultimately cannot survive. The assertion that firms and corporations are in fact more efficient than production alternatives is rendered far more plausible by this natural selection mechanism for eliminating the inefficient. But such a mechanism clearly does not operate for public bureaus. Bureaus survive by securing political support—from congressmen in committees, the institutionalized presidency, interest groups—sufficient to veto life-threatening legislation by enemies; and they expand the scope of their activities by building on this base of political support. Natural selection clearly has something to do with a bureau's "political efficiency." Yet this criterion for selection is not only consistent with productive inefficiency, it often tends to cause it. Moreover, this is a weak mechanism for actually weeding bureaus out of the population, for, particularly in the American separation of powers system, the minimum level of support is rather easily achieved; bureaus can be quite bad at garnering political support and nonetheless survive. Newer, "better" organizational forms may emerge in the public sector, but they generally do not replace the older ones; they simply make an undisciplined organizational population even more diverse.

In short, the contractual paradigm can only provide a meaningful answer to the question "Why do public bureaus exist?" with considerable translation. As a general framework it offers a refreshing way of thinking about the emergence, survival, and relative efficiency of public organizations, and clearly has the capacity to generate a range of new ideas and directions for inquiry. The study of public bureaucracy could certainly use this kind of invigoration. But even a brief discussion makes plain that the translation from economics to politics will not be a simple matter. It will require a good deal of theoretical innovation, backed by a recognition that the models and conclusions of organizational economics may have to be modified substantially.

How Can Bureaucratic Superiors Control Their Subordinates?

Hierarchical control is facilitated by a number of interrelated mechanisms, among the most important of which is the "residual." In Alchian and Demsetz's

analysis of teams, the entrepreneur is given marketable title to the residual in order to induce him to monitor team members efficiently; he has maximal interest in properly measuring member productivity—and in increasing it through hiring, firing, and reorganization—because the outcomes of member efforts accrue to him alone. Similarly, the more general principal-agent models of hierarchical control have shown that, under a range of conditions, the principal's optimal incentive structure for the agent is one in which the latter receives some share of the residual in payment for his efforts, thus giving him a direct stake in the outcome (e.g., Shavell, 1979).

For public bureaucracy, however, there is no residual in the ordinary sense of the term. The typical bureau receives a budget from governmental superiors and spends all of it supplying services to a nonpaying clientele. Regardless of the agency's performance or how it changes over time, the results are not reflected in an economic surplus accruing to bureau heads, and this major incentive for the efficient monitoring of employee behavior fails to operate. Incentive plans that give employees a share of the "profit" in partial payment for their effort (e.g., in locating welfare fraud or cost overruns) are also ruled out. The absence of a residual and its implications for internal efficiency, therefore, would seem to be factors that immediately set public bureaucracy apart from private organizations and shape our expectations about their relative performance.

But is there any quantity that might play essentially the same role as the residual? The obvious candidate is slack, the difference between the true minimum cost of service provision and what the bureau actually spends (the budget). It has been argued that bureau heads are indeed motivated by slack, for, just as Cyert and March (1963) pointed out for the firm, it represents a cushion of resources available for the leader's personal consumption or for "payoffs" (perquisites, leisure, new equipment) to allies for their policy support or subordinates for their compliance (Migue and Balanger, 1974; Niskanen, 1975). Like the economic residual, then, it clearly can be used for purposes of internal bureaucratic control. It also gives the bureau head an incentive to monitor member behavior and to encourage efficient work effort, because he is then in a position to capture the difference between the budget and true cost; this is how he can act to increase his storehouse of slack resources, and thus his very capacity for control.

The crucial difference is that slack is only available to the extent that the bureau as a whole operates inefficiently by producing at greater than minimum cost, with budget exceeding the true costs of production. The greater this inefficiency, the greater the slack. When slack is a control mechanism, then, efficiency and inefficiency curiously coexist. In fact, they feed on one another.

Consider the case of a subordinate who chooses on his own to shirk. In doing so, he is soaking up slack resources that could be captured and ultimately disbursed by the bureau head. The latter therefore has an incentive to employ monitoring and other methods to eliminate "illicit" shirking, and this in turn encourages efficiency. Yet the bureau as a whole remains inefficient, for the slack has simply been transferred from the former shirker to the bureau head, who has

no intention of turning it back to his political superiors (by accepting a smaller budget) and who is now in a position to apply it toward bureaucratic control—e.g., by permitting a compliant subordinate to engage in “legitimate” shirking. Efficiency makes inefficiency possible, and inefficiency is traded for control.

In short, slack is not a functional substitute for the economic residual. If it does in fact motivate bureaucrats—and it probably does, to some extent—then it emerges as a mechanism with its own distinctive consequences for bureaucratic efficiency and control, consequences which again underline the differences between public and private organizations.

There are still other important differences when we consider motivational structure more generally. It is only reasonable to suggest that pecuniary gain—salaries, profits, fringe benefits—is far more salient a motivator for top-level managers in the private sector than for top-level bureaucrats in the public sector. Political theories of bureaucratic behavior have tended, quite plausibly, to focus on budgets, slack, policy, career opportunities, and security as central motivators, and they have recognized that the salience of specific motivators varies across bureaucratic officials and with the nature of the appointment (Downs, 1967). Political appointees, most obviously, are more likely to be concerned with policy and its consistency with the current administration than career officials are. Personal income is of course an important consideration for all bureaucrats, but financial rewards and opportunities are much more limited and rigidly structured in government, and they are much less contingent upon individual productive effort or bureaucratic outcomes.

What does this expanded set of motivators suggest for internal bureaucratic control? Above all, it implies that different “types” of bureaucrats will exercise control toward different ends, depending on which motivational elements happen to be salient for their “type.” There is no reason to think that political appointees will exercise control in the same way as career officials, nor that budget-maximizers will exercise control in the same ways as those motivated by policy or slack. The emphasis on efficiency, the kind and degree of monitoring, the manipulation of incentives—all will depend on precisely what a bureaucratic superior wants from his subordinates. Because this may be very different from the objectives of the typical managerial superior in a business firm, the hierarchical control mechanisms surrounding public management will tend to be correspondingly different from those in private management.

They will also be different because public managers, whatever their personal goals, must operate within an organizational context whose structure and objectives are in important respects imposed by outside actors. The Civil Service system, for instance, imposes rules for hiring, firing, pay and fringes, promotion, and the processing of grievances. These elements of structure condition the kinds of individuals attracted to bureaucratic jobs (the adverse selection problem) and their opportunities and incentives to shirk once hired (the moral hazard problem). Although difficult to demonstrate empirically, it is often argued that these impacts are the opposite of what productive efficiency would require: they tend to attract

and retain individuals who are of lesser quality, overly concerned with security, and not disposed to innovate, and they tend to enlarge the opportunities for shirking while minimizing the rewards of productive effort. Not coincidentally, they also place severe constraints on the ability of public managers to do what the contractual paradigm implies they ought to be doing in the interests of hierarchical control: screening and selecting appropriate personnel, weeding out the inappropriate, and designing incentive structures that conduce to maximal compliance.

And Civil Service is only the beginning. The Administrative Procedure Act circumscribes the structures and processes of administrative decision making, removing crucial components of organizational design and control from the realm of choice. The Freedom of Information Act requires that agency files be open to the public upon demand, implicitly encouraging bureaucrats to constrain their internal communications and storage of information. Agency-specific statutes dictate bureaucratic goals, impose internal structures, require reorganization, set resource levels, and determine types and numbers of personnel. The list could easily go on.

In principle these sorts of constraints are smoothly handled by the contractual paradigm; indeed, its perspective on hierarchical control tells us what contextual elements are likely to be constraining. Yet actual analyses in the contractual tradition have not emphasized this aspect of the control problem. Alchian and Demsetz's entrepreneur has virtually a free hand in designing incentive structures and monitoring systems, as does the principal in most principal-agent models. This is reasonable under the circumstances; theoretical development is still in its early stages, and it is necessary to understand the full dimensions of the optimization problem before shifting attention to specific constraints that vary with substantive context. Nevertheless, there are good reasons for thinking that bureaucratic control is much different for the public sector than the private sector, and that a straightforward application of contractual theories and their implications is likely to be very misleading. Again, serious efforts at translating and extending these theories are required—including, in this case, attention to the motivations of public bureaucrats and the highly constraining conditions under which they seek to exercise control. Public bureaucrats do not want the same things as private bureaucrats, and they tend to have far less flexibility and fewer resources for pursuing their objectives through the compliance of subordinates. Assuming these points of comparison are roughly correct, they must somehow be reflected in contractual theories if we are to arrive at a better understanding of public bureaucracy.

How Can Politicians Control Bureaucrats?

Democratic politics is easily viewed in principal-agent terms. Citizens are principals, politicians are their agents. Politicians are principals, bureaucrats are their agents. Bureaucratic superiors are principals, bureaucratic subordinates are their agents. The whole of politics is therefore structured by a chain of principal-agent relationships, from citizen to politician to bureaucratic superior to bureau-

cratic subordinate and on down the hierarchy of government to the lowest-level bureaucrats who actually deliver services directly to citizens. Aside from the ultimate principal and the ultimate agent, each actor in the hierarchy occupies a dual role in which he serves both as principal and as agent. The formal apparatus and deductive power of the principal-agent model are applicable to each of these hierarchical stages of government, and might usefully be employed in investigating even the most basic questions of democratic control and performance.

Our concern at this point is with the second stage, in which politicians are principals and bureaucrats their agents. The model offers a simple reinterpretation of the traditional problem of administrative accountability: what institutional mechanisms allow elected officials to hold bureaucrats accountable for their administrative decisions? The reinterpretation, however, leads to a nontraditional slant on the subject. The principal-agent model focuses on information asymmetry and, in particular, on information available to bureaucrats—about their true “types” (honesty, personal goals, policy positions) and their true performance—that politicians do not automatically possess and often can only acquire with much imprecision and expense. It then encourages us to inquire into the monitoring devices and incentive structures—aspects of institutional design—that mitigate the asymmetry and thus minimize the problems of adverse selection and moral hazard that will otherwise cause bureaucrats to depart from their political directives.

The existing economic literature on principal-agent relationships is even now directly helpful, since the politicians’ control problem is but a special case of more general control problems that have already been analyzed in detail. In fact, the problem of political control is analogous in many respects to the economic problem of the separation of ownership and control, with politicians attempting to control bureaucrats rather than stockholders trying to control managers. Yet, as we have seen, this work can take us only so far. A good bit of translation and elaboration are ultimately required if they are to provide real insight into the substance of political control. Among other things, principal-agent models must eventually find a way of dealing with the following sorts of considerations.

1. As noted, the empirical diversity and ambiguity of individual goals presents a real modeling problem, since there is no simple quantity like income or profit that can reasonably be thought to motivate either bureaucrats or politicians. Simplifying assumptions can and must be adopted, e.g., that bureaucrats are budget- or slack-maximizers or that politicians maximize the probability of reelection, but there is likely to be some dispute as to which are empirically appropriate—and, clearly, the ones we choose make quite a difference for our theoretical “understanding” of political control. A few general points about motivation, however, seem evident. First, because politicians are not centrally concerned with economic efficiency, they will tend not to hold bureaucrats accountable on efficiency grounds; in fact, politicians will find many kinds of inefficiency quite valuable—e.g., those that involve patronage or expenditures in the home district. Second, politicians are not interested in everything bureaus do.

Only some aspects bear on politicians' reelection chances or policy interests, and these are the focus of political control; the rest they will ignore or perhaps use as bargaining chips with bureaucrats and other politicians. This is one foundation of bureaucratic discretion. Third, politicians are not necessarily motivated to ensure that bureaucrats faithfully pursue their statutory mandates; the mandate is a collective expression of programmatic purpose, and individual politicians may or may not find its pursuit relevant to their own self-interests. What the bureau is "supposed" to be doing and what politicians are asking it to do, therefore, may often be two quite different things.

2. Whatever politicians might be trying to accomplish in controlling bureaucrats, they generally cannot count on the economic residual as the stockholders of a corporation would in motivating their managers. They might, however, be able to count on slack. Particularly given that politicians care little about efficiency, this may in fact be a major tool of political control; for it is something that politicians can purposely provide and that bureaucrats can subsequently convert into a range of other valued quantities. Yet it is also plagued by an information asymmetry: bureaucrats are in a position to know what the level of slack is, but politicians are not (even with extensive and costly monitoring). Other things being equal, this puts politicians at something of a disadvantage, and may encourage reliance upon less demanding mechanisms, e.g., simple budget or staff levels (without estimates of true minimum costs), new programs, selective monitoring, or media attention.

3. Stockholders, like politicians, are burdened by similar information asymmetries in their efforts to control corporate managers, but they are also blessed by certain external mechanisms that substitute remarkably well for direct knowledge—stock market evaluation of the company's profitability, labor market evaluation of a manager's economic value, the threat of takeovers. These mechanisms are clearly not present for public bureaus, making the monitoring job of politicians far more difficult than it would otherwise be. There are, on the other hand, political mechanisms that may serve as partial substitutes. One is the reputation of bureaucrats. Over time, politicians are able to observe bureaucratic behavior and, for many of the more important actors, arrive at tacit agreement as to their honesty, competence, ideology, innovativeness, and other qualities of relevance. This is less concrete than a market evaluation of economic worth, and it may be of little use in evaluating many political appointees, but it is certainly a central tool in the task of political monitoring. Another important tool is the "decibel meter" (Weingast, 1983; McCubbins and Schwartz, 1984), which refers to the feedback politicians receive from constituency groups affected by bureaucratic performance. By complaining about service delivery or demanding services of different kinds, groups provide politicians with critical inputs that are directly relevant both to monitoring the bureaucracy and furthering their reelection goals. As these examples suggest, then, political mechanisms available to politicians are quite different from the economic mechanisms available to stockholders, and they are probably less effective in generating control—but they are characteristic fea-

tures of politics that fit comfortably within a principal-agent framework, if only we recognize their relevance.

4. Politicians operate under heavy constraints in their efforts to exercise control over bureaucrats. Most aspects of hiring, firing, and promotion are structured by formal career systems such as Civil Service and are manipulated only with difficulty. The president is in the best position in these regards, since he has authority to hire (with senatorial confirmation) and fire a few thousand political appointees; and presidents have often made these choices on the basis of political philosophy or loyalty. But, even here, it is well known that appointments are also frequently made on grounds (e.g., favors to allies) that are not conducive to subsequent control, and firing is often unlikely in view of its negative reception by the appointee's political supporters. Similarly, aspects of organization that politicians might wish to manipulate—formal goals, structure, decision procedures—are constrained by legal statutes, and, in the absence of costly and time-consuming new legislative efforts, are effectively beyond their reach in many respects. What is most interesting about these constraints, however, is not simply that they are so confining, but that they are imposed by the politicians upon themselves. Clearly politicians have chosen—presumably in the rational pursuit of their own goals—to structure the formal context of their agency relationship with bureaucrats in such a way that the prospects for control are actually reduced. A simple principal-agent model can only find this kind of behavior paradoxical indeed.

5. This prompts recognition of a complicating but quite essential fact of politics: any given bureau is likely to have multiple principals. At the federal level it will be subject to direct oversight by the president (through the OMB and the departments) and several congressional committees, with each of the latter bodies comprised of many politicians. Whether or not we consider every politician a principal in his own right, the context of control is clearly a very complicated one. Each principal is effectively in competition with the others in his efforts to exert control, while the bureau, on the receiving end of all this, finds it must contend with uncoordinated and often conflicting demands, requirements, and incentives. Multiple-principal arrangements are not unusual in organizations, since subordinate units often take orders from more than one superior unit (accounting, personnel); but an effort is ordinarily made by organizational leaders to impose order and consistency. In democratic politics, however, party competition tends to promote multiple-principal arrangements that are competitive rather than cooperative, as none of the parties wants the others to have unfettered control over the bureaucracy. And in a separation of powers system, competitive multiple-principal arrangements are actually built into the system by design. The president struggles with Congress over control of the bureaucracy, and, within Congress, House committees struggle with Senate committees. This is the way administrative accountability is supposed to work in a separation of powers system: it is literally true that no one is in charge. Bureaus are “partial agents” of various governmental principals, without being under the complete authority of any one

in particular, and without any common understanding of how authority is legitimately divided among the competing principals. Once we move away from the simple principal-agent model by recognizing that American politics is, by its nature, a context of competitive principals, it is hardly paradoxical that politicians impose constraints "on themselves." In fact, politicians impose constraints on one another in a competitive effort to see to it that their own interests are protected from the intrusions of politician-opponents. This is rational for individual politicians and groups of politicians, but the net result is that politicians in general have a more difficult time controlling the bureaucracy. This can only tend to strengthen the foundations of bureaucratic autonomy.

6. Political control is also undermined by multiple-agent arrangements. The most obvious of these arises because each principal has various bureaus to oversee; but this complication may not be of great significance, since principals in fact have their own specialized hierarchies for monitoring each bureau and, in many cases, no coordination is even desired. Another dimension of multiple-agent arrangements appears far more consequential: political control of a given bureau requires not only that politicians induce bureaucratic superiors to comply with their wishes, but that bureaucratic superiors are subsequently able to induce bureaucratic subordinates to comply as well. We have already seen that this second step is highly problematic for a variety of reasons. Thus, principal-agent models that focus on relations between politicians and bureaucratic superiors will be missing a necessary dimension of bureaucratic compliance, and models that focus on politicians and "bureaus" will tend to merge two quite different kinds of agency relationships without attention to their distinctive contributions to bureaucratic outcomes.

While hardly exhaustive, this discussion at least begins to suggest the kinds of factors that become relevant when principal-agent models are applied to questions of political control. With this as background, it is useful to close the discussion by taking a brief look at two intriguing perspectives now current in the public bureaucracy literature. Both proceed on rational choice foundations in investigating the relationship between bureaucrats and legislators. One, associated with Niskanen (1971, 1975), argues that this relationship is dominated by bureaucrats, while the other, associated with Weingast (Weingast, 1983, Weingast and Moran, 1983), argues just the opposite. How do their arguments square with the political logic of principal-agent analysis?

Niskanen's theory is a pioneering effort to explain the rational bases of bureaucratic power and budgetary outcomes. Very briefly, he contends that bureaucrats have close to a monopoly over information about supply costs, which they use along with information about legislative "demand" to select budget-output packages that are presented to legislators as take-it-or-leave-it proposals. This combination of informational monopoly and agenda control allows bureaucrats so to constrain legislators' options that they "have no choice" but to ratify what the bureaucrats want. Since bureaucrats generally value larger budgets (Nis-

kanen, 1971) and perhaps slack (Niskanen, 1975), the overall tendency is toward budget and output levels that far exceed the social optimum.

Although Niskanen's theory was first developed before the principal-agent model, he focuses on information asymmetry and its implications for political control. Moreover, one of his partial solutions to the big-government problem is to reward bureaucrats in proportion to the residual they turn back to the legislature, again introducing a control mechanism central to the principal-agent literature. Yet there are fundamental differences between the two. Niskanen views the situation as one of bilateral monopoly in which the bureau happens to hold all the cards, whereas, empirically, Congress is the bureau's superior in the hierarchy of governmental authority—it is the principal (or, in practice, its committees are) and the bureau its agent. As a principal, Congress therefore has the authority to issue orders, adopt incentive systems, control the bureaucratic agenda, and otherwise structure the principal-agent relationship as it sees fit (Miller and Moe, 1983a). Also, it can and generally should devise monitoring mechanisms for gathering information about bureaucratic "types" and productive behavior (Bretton and Wintrobe, 1975). The principal-agent model, by viewing the relationship as hierarchical and emphasizing the distinctively different positions of superiors and subordinates, naturally points to a whole array of resources and options available to the legislature in its pivotal role as principal. This does not mean it will succeed in dominating bureaucrats. We know the latter have resources of their own, cost information among them; and, as Niskanen has noted, the legislature may be subverted by high-demand committees that act in league with bureaus by seeking larger budgets. But it does mean that legislatures may be far better off than Niskanen suggests, due to their special status as governmental superiors and all that that implies for the dynamics of control. It also means that important elements of a broader understanding of political control—the goals of politicians, the constraints under which they must operate, competition among principals (including the president), and other factors discussed above—tend to be de-emphasized or ignored as part of the general inattention to issues of hierarchy.

Weingast, in contrast to Niskanen, explicitly employs a principal-agent framework in asserting that Congress successfully engineers bureaucratic compliance. He notes that a conventional interpretation of the evidence points to the opposite conclusion, for Congress in fact does a very poor job of directly monitoring bureaucratic behavior. Hearings are sporadic and superficial, congressmen are often ill-informed or uninterested, and committees are overwhelmed by the sheer numbers and complexity of the bureaucratic decisions they are charged with overseeing. It is not surprising, then, that the literature tends to portray Congress as having little systematic influence over the bureaucracy. Weingast contends, however, that Congress has relied upon less visible control mechanisms that substitute quite effectively for monitoring. (1) Members of Congress assess bureau performance by means of their constituency "decibel meters," the most direct measure of the impact of bureaucratic performance on reelection. They do not

need to monitor performance closely or in detail. (2) Bureaus judged to be doing a good job by this standard are rewarded (with budgets and programs); those judged to be doing a bad job are sanctioned. (3) Congress controls appointments to bureaucratic positions, thus influencing the “types” of the individuals with which they deal. (4) Bureaucrats know that their success in achieving both career goals and agency goals depends on satisfying congressional interests. These mechanisms operate unobtrusively to enable Congress to control the bureaucracy. Hearings and other public acts of control are only necessary in those rare instances when bureaus exceed their implicit limits—so it makes sense that a Congress exercising smooth, effective control over bureaus will appear to be engaging in only sporadic, inept oversight.

Weingast’s argument is a provocative challenge to conventional wisdom on congressional influence. It encourages a broader perspective in which bureaucratic behavior is understood in institutional context, and it directs attention to the more subtle web of incentives and relationships that condition political control. Despite these useful insights, however, a good deal of work is called for in developing the model. Its premises are highly debatable—one could easily argue, for instance, that Congress does not control appointments and that bureaucratic incentives are not nearly so conducive to congressional influence. Its conclusions, moreover, are essentially asserted and then documented by reference to selected empirical examples. They are not formally or logically derived, and there is no clear basis for believing that the control mechanisms to which he points are as powerful as he claims. Noting the relevance of the “decibel meter” is one thing—demonstrating that it is indeed an efficient mechanism of congressional control is quite another, as is demonstrating the claim that direct monitoring is unnecessary. At present, there is nothing in the principal-agent literature to justify these conclusions about Congress. And, as our discussion of the political context suggests, there are plenty of considerations that point in other directions. What about the president, competition among committees, and, in general, the multiple-principal setting in which all bureaucratic control takes place? There are also matters of bureaucratic motivation, constraints on legislators, difficulties that bureaucratic superiors have in controlling subordinates—and, not least, the information asymmetries that the “decibel meter” cannot mitigate.

Finally, there is the question of what we mean by “control.” If congressmen do not care about many and perhaps most bureaucratic policy decisions, how important is it that Congress controls bureaucrats on certain other decisions? By this interpretation, congressional control can easily coincide with substantial bureaucratic discretion, or perhaps systematic presidential influence over the general directions of bureaucratic policy. Moreover, are we primarily interested in control relationships within a set legal framework, or are we primarily interested in whether changes in the laws bring about changes in bureaucratic behavior? Weingast’s evidence tends to bear on the latter issue—but no one seriously doubts that new laws have an impact on what bureaucrats do. The more important theoretical question, it seems, is the former one of whether Congress (or presidents) can

influence bureaucrats through oversight, appointments, budgets, etc., and thus without passing new laws. Until these and other issues of relevance can be evaluated systematically, leading to an explicit model of control from which conclusions can be logically derived, the position that Congress controls the bureaucracy would appear to be more provocative than compelling.

At this point in the development of bureaucratic theory, however, provocative positions are doubtless just what is needed to stimulate innovative work on public organizations. Niskanen claims that the bureaucracy dominates Congress, Weingast claims that Congress controls the bureaucracy, and both appear to have staked out untenably extreme positions. Yet their arguments clearly do have something new and important to tell us about the determinants of political control and the foundations of bureaucratic and congressional power. Bureaucrats do have crucial informational advantages and certain abilities to engage in agenda control; Congress's outwardly poor performance in monitoring the bureaucracy may indeed hide an effective reliance upon unobtrusive mechanisms. These are the sorts of contributions that theoretical progress is built upon. The task for the future is to incorporate these insights into more comprehensive models, perhaps principal-agent models, that provide broader perspectives on political control.

Conclusion

The basic purpose of this paper has been to provide political scientists with an overview of a new body of literature, the economics of organization, and to offer a few suggestions about how it might usefully be applied in the study of public bureaucracy. My hope is that this introductory treatment will help to impose simplicity and coherence on a growing, complex body of research, and that it may serve as a useful starting point for those who wish to pursue these ideas further.

Two simple themes deserve emphasis in conclusion. The first is that the new economics of organization is extraordinarily promising. For decades, the study of public bureaucracy has been one of the most underdeveloped areas in all political science. The work of Simon, March, and others in the behavioral tradition has to this point been the most promising source of creativity and theoretical progress; but it has yet to generate the amounts and kinds of theoretical work its proponents had hoped for and probably would have predicted many years back. The economics of organization may turn out to be different. It sheds interesting new light on bureaucratic behavior by focusing on hierarchical control—an elegant, beautifully suitable focus that captures the essence of organizational relationships and offers a coherent framework for integrating both the bureaucratic and the political dimensions of administrative performance. Largely because it maintains the optimizing model of choice, moreover, it offers virtually the full range of powerful analytical methods characteristic of neoclassical economics, and it overlaps in useful and important ways with better-developed areas of economics, information economics in particular. For these reasons, among others, many students of poli-

tics are likely to find the contractual paradigm an especially attractive line of inquiry.

Yet this new approach also has definite drawbacks that, in this early period of novelty and excitement, threaten to go unnoticed. While hierarchical control is in some sense a narrow focus, it still proves to be a very complex one that simple models may severely distort rather than clarify. The politics of control tends to involve multiple principals and agents, competition and cooperation, diverse individual goals, salient constraints on choice, and a number of other features anchored in the institutional context. For analytical purposes, there is a legitimate inclination to assume away as many of these complications as possible—but it may well be that we cannot explain behavior to our satisfaction, or even roughly predict it, without taking these sorts of things systematically into account. In the short run, this suggests that simple models threaten to supply us with theoretical “insights” that are actually quite incorrect; given the difficulty of conducting empirical research on such topics, these “insights” could easily gain acceptance and serve to mislead us for years to come. In the long run, the interplay of theory and data could well produce far more complex theories of dynamic political processes and relationships, theories much more representative of political reality than current models—and much more complicated than modelers would like. In a world of multiple actors, multiple time periods, and strategic behavior, this is surely a reasonable expectation.

This brings us back to the behavioral paradigm. The major objection to work in this tradition has long been that its models of dynamic process and adaptive decision making are too complicated to be useful. Yet, while part of the excitement surrounding the contractual paradigm is that it avoids all this, we have seen that it may ultimately lead to models every bit as complex once it runs the gamut of empirical testing and modification. Indeed, these models may look very much like those employed all along in the behavioral tradition. As the excitement surrounding the contractual paradigm begins to catch on among political scientists, this sobering point of contrast is worth remembering. The contractual paradigm is likely to contribute more to our understanding of public bureaucracy, particularly in the dangerous short run, if the behavioral paradigm and its lessons are kept firmly in mind.

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