The Handbook of Organizational Economics
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Edited by
ROBERT GIBBONS and JOHN ROBERTS

PRINCETON UNIVERSITY PRESS
Princeton and Oxford
1. Introduction

For most of the past century, the study of public bureaucracy was a theoretical backwater in political science. Public administration, its home field until recently, was heavily focused on good management and the nuts and bolts of government operation, and these concerns tended to drive out theoretical thinking. Moreover, unlike the more advanced fields of political science—legislatures, elections, public opinion—bureaucracy did not provide scholars with raw materials that were readily quantified. Legislators vote, for instance, and bureaucrats do not. This single fact had much to do with putting the study of legislatures on the fast track and that of bureaucracy on the slow track.

Theory was also impeded by a conceptual blind spot. Early studies rightly saw public agencies as strategic players in the political process, seeking out relationships with legislative committees and interest groups, nurturing their own constituencies, and otherwise engaging in bureaucratic politics (e.g., Long 1949). But scholars failed to see that the underlying organization of bureaucracy is thoroughly political too—for organization affects performance, and powerful actors have incentives to design and shape it to their own advantage. In the scholarship of the time, bureaucratic behavior was understood in political terms. But bureaucratic organization was not.

This nonpolitical approach to organization had deep roots. During the early decades of the twentieth century, reactions against political corruption and party machines led scholars to see bureaucracy as the savior of good government and as properly nonpolitical. They embraced the “separation of politics from administration”: the notion that, although policy is inevitably a product of politics, it should be implemented by impartial experts in the bureaucracy, which in turn should be organized for effective performance (Knott and Miller 1987).

As scholarship became more scientific, and thus more concerned with explaining the actual features of public agencies, this nonpolitical take on bureaucracy was not overcome. In fact, it was reinforced by the emerging organization theories of the era—which took their orientations from sociology, social psychology, and psychology and offered only nonpolitical explanations for why bureaucracies became organized as they did (Perrow 1986). Even the groundbreaking
contributions of Herbert Simon and James March, both political scientists, did nothing to make
the theory more political. Simon and March developed an elaborate theoretical tradition—the
dominant perspective on organization among political scientists from the 1950s through the
1970s—that explained formal organization by reference to the cognitive limitations of human
beings, and it had almost nothing to say about how public organizations arise out of politics
(Simon 1947; March and Simon 1957).

The turning point came during the 1970s, when economists began developing a new set of
theories and analytical tools—transaction cost economics, agency theory, theories of repeated
games—for explaining economic organization. Fairly quickly, political scientists began applying
the same theories and tools to the study of government organization, and the long-stagnant
theory of public bureaucracy was soon transformed (Moe 1984; Williamson 1985; Milgrom
and Roberts 1992).

At the heart of the new theory is a unifying focus on the problems that politicians face when
delegating to bureaucrats and on the control mechanisms they can employ in trying to ensure
that bureaucrats faithfully implement public policy. Some of these control mechanisms operate
ex post, with the authorities monitoring and reacting to what bureaucrats do in the performance
of their jobs. But much of the emphasis has been on controls that operate ex ante, with the au-
thorities taking action through politics to strategically organize the bureaucracy in such a way
that bureaucrats are constrained to pursue the right policies. A theory of political control and
delegation, therefore, is ultimately a political theory of bureaucratic organization: one that shows
how the structural details of bureaucracy arise out of the political process and how they are con-
nected to the strategies and motivations of those who exercise (or influence) public authority.

Today's theory has developed well beyond this simple core and addresses a wide range of
contiguous topics. Most important, it sheds light on the struggle among legislatures, executives,
and the courts for control of the bureaucracy in systems based on separation of powers, and it
has begun to explore the delegation dynamics of parliamentary systems. This is a theory that is
growing rapidly, and is increasingly best thought of as a theory of the institutional system more
generally, in which the various branches of government are all integrally connected (Strom et al.
2003; de Figueiredo et al. 2006).

My focus here is on the political theory of bureaucratic organization, and thus on delega-
tion, control, and their implications for the structure of government. My aim is to highlight
the major ideas and approaches that have oriented this literature over time, giving special atten-
tion to the light they shed—or fail to shed—on the major substantive issues they attempt to
explore and explain. Along the way, I argue that there are basic analytic problems that remain
to be overcome: problems that arise, in part, because the normal science that now governs the
theory—a standardization that testifies to its very success—has been limiting and even misleading
in important respects. All told, the progress from the early days has been astonishing. But
some rethinking is in order.

The review I am providing, then, puts a spotlight on the basic analytics of the theory, while at
the same time keeping it close to the reality of government—and understanding and evaluating
it accordingly. For readers interested in more detailed attention to the technical details of
delegation models, excellent treatments can be found in Bendor and Meirowitz (2004) and
Bendor et al. (2001). My own view is that, as this literature has developed and matured, technical
issues have come to the fore and essentially have come to dominate the attention of the people
who are prime movers in this field—to the point that very basic analytical and substantive concerns are no longer regarded as very interesting or important. But the tail should not be wagging the dog here. In the end, the purpose of these models is to explain government.  

I should add that even with the more substantive themes I am pursuing here, many theoretical contributions to the larger study of public bureaucracy and its politics must go undiscussed to keep the job manageable. This applies to the literature on ex post control, which is not really about issues of delegation or organization (Hammond and Miller 1987; Ferejohn and Shippman 1990; Eskridge and Ferejohn 1992; Hammond and Knott 1996). It also applies to work on the internal dynamics of bureaucracy, which is mainly about supervisory and employee issues rather than the politics of delegation and control (Hammond and Miller 1985; Miller 1992; Brehm and Gates 1999). All this work is relevant, but the line has to be drawn somewhere.

2. Early Theories of Public Bureaucracy

Rational choice first made its mark on bureaucratic theory during the mid-1960s with the appearance of two innovative books, Tullock’s The Politics of Bureaucracy (1965) and Downs’s Inside Bureaucracy (1967). Both were attempts to show that much can be understood about bureaucracy, and a powerful theory someday constructed, by assuming that bureaucrats are rational actors largely motivated by self-interest.

This approach was a sharp departure from existing scholarship. It was also a quantum change in the way rational choice was then being applied to organizations generally. At the time, the work of Simon and March was the only theory of bureaucracy grounded in rational choice, and theirs was an unconventional version of it to be sure. They relaxed all its key assumptions (about rationality, information, goals), shifted the terrain from economics to psychology, and developed a theoretical tradition centered on the cognitive limitations of individuals and on how, as individuals seek to solve the problems that arise in organizational settings, these limitations ultimately give rise to organizational structure. Along the way, self-interest and its correlates—strategy, conflict, opportunism—were mostly ignored, as were their profound consequences for organization. Tullock and Downs brought these core components of rational behavior to center stage and, for the first time, argued for a full-blown rational choice theory of bureaucracy.

Although both were especially interested in government, they cast their nets widely to address a full gamut of topics on public and private organization. Tullock’s is a theory of authority relationships in general. Downs’s is a theory of all large organizations whose outputs are not evaluated in external markets. In each case, the analysis is informal but is still based on clear assumptions about actors and their contexts, with the spotlight on motivation. Tullock’s perspective is built on the assumption that bureaucrats are motivated by career advancement. Downs creates five motivational types—conservers, climbers, zealots, advocates, and statesmen—and shows how the changing mix of these types shapes the growth and operation of bureaucracy. Even though these analyses are very different in content, their bottom lines are much the same. The

1. For a related (and highly recommended) review that also puts substance ahead of the technicalities of modeling, see Miller (2005), which is about principal-agent models and their implications for various aspects of politics.
rational foundations of bureaucratic behavior, they argue, promote excessive growth, capture, weak accountability, and related problems that work against effective government.

With these two books, rational choice made a stunning entrée into the world of bureaucratic theory, upsetting the good-government vision of public administration and charting a bold new path for analysis. Downs, especially, was widely read by political scientists and cited for what he had to say about agency life cycles, control problems, communication foul-ups, and other central issues. His typology of bureaucratic motivation, which he put to insightful use, became broadly popular.

Future work in rational choice, however, was not destined to build explicitly on either of these contributions. Their sweeping approach to bureaucracy did not provide a clear focus for constructing new theories, nor did it suggest any productive strategies of formal modeling. Many found these books exciting but also complicated and multifaceted. No one really knew what to do with them.

3. The Niskanen Tradition

What the movement needed was some sort of catalyst, a new and promising analytic basis for cumulative work. And it came in the form of Niskanen’s *Bureaucracy and Representative Government* (1971), which was hailed as a pathbreaking contribution and quickly generated a cottage industry of new theoretical work.

The key to Niskanen’s success was that, unlike his predecessors, he restricts his focus and simplifies with a vengeance. Although he too is interested in grand issues—the size and efficiency of government—his analysis centers much more narrowly on public agencies and their budgets, and his model is starkly simple. He assumes that bureaucrats are budget maximizers, thus endowing them for the first time with a simple utility function amenable to formal analysis. And he strips away the daunting complexities of budgetary politics by building his model around just two actors, the bureau and its legislative sponsor.

Their relationship is one of bilateral monopoly, with the bureau holding two pivotal advantages. First, its position as sole supplier gives it a monopoly over information about the true costs of production. Second, the bureau knows how much the legislature values every level of output, and it uses this information to present a take-it-or-leave-it offer (of a given output for a given budget) that it knows the legislature will accept. It has information power, and it has agenda power. These powers enable the bureau to act as a perfectly discriminating monopolist, forcing the legislature to accept an oversized budget it barely prefers to no budget at all, with any surplus kept and spent by the bureau. The upshot is that government is too big and grossly inefficient.

Niskanen’s earliest critics focused on his assumption of budget maximization, arguing that bureaucrats actually maximize something else (Migue and Balanger 1974; Blais and Dion 1991). But the most telling critiques have centered on control issues. As I have suggested, bureaus dominate in his model for two reasons: they control information and they control the agenda. Yet Niskanen is not at all clear about this, and tends to merge the two factors under the general heading of information—as though it is the agency’s control over information that allows it to present the legislature with take-it-or-leave-it budgetary offers, thus giving the agency agenda control. Which is not the case (Bendor 1988).
Efforts to clarify matters soon revealed that agenda control is the big chink in Niskanen’s armor. The first indication came from Romer and Rosenthal (1978, 1979), who show that the power of agenda control depends on the “reversion level”: the budget that would prevail if the bureau’s take-it-or-leave-it offer were rejected by the legislature. Niskanen assumes the reversion level is zero, and thus that legislators must choose between the bureau’s offer and no budget at all, which gives the bureau far greater power than it otherwise would have. A more reasonable assumption, more in line with the real-world budgetary process (where legislatures routinely fall back on status quo budgets in times of impasse), would have produced less gloomy conclusions about the size and efficiency of government.

The larger question, however, is why bureaus have agenda power at all. As Gary Miller and I pointed out (Miller and Moe 1983), Niskanen’s model is curiously one-sided: bureaus are strategic actors, but the legislature is passive and sits by idly while the treasury is looted. An adequate model, we argued, should treat the legislature as a strategic actor too—and recognize that it has authority over the bureau and can structure the bargaining in any way it wants. Their relationship is not simply one of bilateral monopoly. It is an authority relationship, a hierarchical one, in which the legislature has the legal right to tell the bureau what to do. The legislature is the principal, the bureau the agent.

It follows that the legislature need not put up with the kind of agenda control Niskanen grants the bureau. It might force the bureau to submit a complete schedule of budget-output combinations for legislative choice rather than a take-it-or-leave-it offer, for example. It might engage in monitoring to gain information. It might impose sanctions when the bureau is caught lying. And so on (see also Breton and Wintrobe 1975, 1982). The fact is, the bureau must play the budget game according to rules set by the legislature. And in this crucial sense, it is the legislature that sets the bureau’s agenda, not the other way around. A model that incorporates these new elements, we showed, leads to a more variegated—and more moderate—view of bureaucracy and government.

From this point on, Niskanen’s approach to bureaucracy gave way to the new economics of organization. Much of the new work paid little or no attention to Niskanen. But some of it did, at least early on, creating what amounted to a bridge between the two—one that highlighted Niskanen’s attention to budgets but understood the relationship between the bureau and the legislature in game-theoretic or principal-agent terms. Take-it-or-leave-it agenda control was out as a basis of bureaucratic power. The focus was now on asymmetric information, especially the bureau’s private information about costs (its expertise); on the legislature’s authority to set the rules; and on basic mechanisms of political control, such as auditing and sanctions (Bendor et al. 1985, 1987a,b; Banks 1989; Banks and Weingast 1992).

Although no longer a distinctive research program, the Niskanen tradition has clearly had a lasting impact on bureaucratic theory. Pre-Niskanen, the natural inclination among scholars was to see bureaucracy as a complex organization subject to a tangled array of authorities, constituencies, and pressures. Niskanen brought simplicity, clarity, and structure to an otherwise messy field, and in the literature to follow his distinctive stamp would be difficult to miss: bureaucracy would routinely be modeled as a unitary actor driven by a single goal, the focus would be on its relationship with the legislature, and attention would center on the key role of information—expertise—and the leverage it gives bureaucrats in pursuing their own interests.
4. Legislative Control and Congressional Dominance

During the early 1980s, political science was swept by the new institutionalism. Until then, despite the provocative work of Niskanen, most rational choice theorists were little interested in bureaucracy. For them, the rationale for studying institutions arose out of a voting puzzle: the social choice theory of voting predicted endless cycling, whereas voting processes in the real world tended to be highly stable. Why so much stability? Their answer was that institutions impose structure on the voting process, bringing order out of chaos (McKelvey 1976; Shepsle 1979; Shepsle and Weingast 1981).

As the theory of political institutions got underway, then, the natural focus was on legislatures—whose members do their work by voting, and who are elected through the votes of constituents. The theory that emerged, therefore, was a decidedly legislative theory, and the rest of the political world came to be viewed through legislative lenses. Bureaucracy nonetheless attracted great interest: for the policies that legislators adopt are empty abstractions until they are implemented, and they can be implemented in many ways depending on who controls the bureaucracy, how well, and what they want it to do. This being so, scholars quickly put the spotlight on political control of the bureaucracy—which meant, in almost all cases, congressional control of the bureaucracy.

Barry Weingast stands out as the most influential figure in the early theory of congressional control. Of his several articles on the subject, one co-written with Mark Moran on congressional control of the Federal Trade Commission is widely cited as seminal (Weingast and Moran 1983; see also Weingast 1981, 1984). The theory begins with a social choice model of legislative decision, in which a committee uses its agenda powers to engineer voting outcomes on the floor. Having thus shaped legislative policy, the committee then becomes a principal trying to ensure faithful implementation by its bureaucratic agent: wielding an array of (ex post) control mechanisms—oversight, the budget, threats of new legislation—so formidable that the bureau has overwhelming incentives to go along. The theme is one of congressional dominance.

This theme was also driven home in an influential article by McCubbins and Schwartz (1984) on the nature and potency of oversight. They argue that reelection-minded legislators, as principals, have little incentive to engage in the broadly based “police patrol” oversight of bureaucracy that the traditional literature presumed. Legislators are better off simply responding to the “fire alarms” set off by constituency groups when something goes wrong. Doing so makes the groups happy, gets them to bear the costs of monitoring, and focuses oversight on problems with electoral salience. It also produces tight control: for when the fire alarms go off, Congress’s weapons are so powerful that the bureaucracy will toe the line. Indeed, bureaus will anticipate as much and comply from the outset.

These articles, together with others arguing similar ideas (e.g., Fiorina 1981; Barke and Riker 1982), heightened scholarly interest in political control. Yet their claims of congressional dominance also provoked controversy, and for good reason. As I pointed out at the time (Moe 1987), they do not really develop a theory of control. For they never attempt to model the goals, strategies, or resources of the bureaucracy, and thus cannot shed light on its motivation or capacity to shirk. The profound importance of private information (expertise), which so empowered Niskanen’s bureau, is given short shrift here, along with the entire bureaucratic side of the control relationship. Their focus is entirely on the legislature.
Their presumption, moreover, is that budgets, oversight, and other mechanisms nicely translate into tight control for the legislative principal—when the whole thrust of the economic theory of agency is that control is costly and likely to entail substantial slippage. The theme of a well developed theory of political control, it is reasonable to suggest, should be that Congress has a very difficult time controlling the bureaucracy, and that the latter probably has considerable autonomy. This is precisely what mainstream empirical work by political scientists had long maintained (Wilson 1989).

In some sense, the problem in this early literature on political control was just the reverse of what we found with Niskanen. Niskanen overstated bureaucratic power by assuming a strategic bureau and a passive legislature. The congressional dominance theorists flipped it around, overstating legislative power by assuming a strategic legislature and a passive bureau.

5. Ex Ante Control and the Politics of Structure

The early theory of congressional dominance was a theory of ex post control, asking how legislators could prevent runaway bureaucracy by monitoring the behavior, rewarding the compliance, and punishing the noncompliance of existing agencies. Yet legislators (and presidents) also have the authority to exercise control ex ante: by imposing structures and personnel systems that promote agency compliance from the outset. Through strategic choices about organization, in other words, they can design bureaucracy to do their bidding.

Rational choice theorists quickly recognized as much and moved to incorporate ex ante control into their analyses. This simple step, although obvious in retrospect, may represent the most important single development in the modern theory of bureaucracy, as it paved the way for an explanation of how bureaucracy emerges out of politics and why it takes the organizational forms it does.

The study of ex ante control is rooted in issues of delegation. Several influential studies carried out early on—by Aranson et al. (1982), Fiorina (1982a,b, 1986), and McCubbins (1985)—proposed formal approaches to delegation and focused attention on some of the key questions that needed addressing. Why does Congress delegate authority to an agency rather than writing detailed laws itself that are enforceable in the courts? When it delegates, does it prefer vague mandates that give agencies great discretion or highly specific mandates that severely limit what agencies can do? And when agencies have discretion, how can Congress design their structures so as to channel bureaucratic behavior toward legislative ends?

Soon thereafter came two articles by McCubbins, Noll, and Weingast (1987, 1989) that gained widespread attention, provoked controversy, and established ex ante control as a growth industry. Their big splash was due in part to their audience. McNollgast (as they are sometimes collectively known) addressed themselves to the law-and-economics community, arguing that administrative procedures—this audience’s main organizational focus—are explained not by the traditional normative concerns for fairness, due process, or equity, but rather by the self-interested control strategies of legislative actors. Such an argument grew naturally out of rational choice thinking, but it challenged convention, and it demanded and got a spirited response (e.g., Mashaw 1990).

McNollgast see the relationship between Congress and the bureaucracy as a principal-agent problem, in which an “enacting coalition” within the legislature seeks to minimize bureaucratic
“drift” (shirking). The problem arises because the typical agency has its own policy preferences, often different from those of Congress, and because it may be able to use the information asymmetry built into their relationship—owing to its greater expertise—to go its own way in policy. What can Congress do to keep the bureaucracy in line? The authors argue that, while the prior literature had emphasized ex post control, such efforts to monitor, reward, and sanction agencies are costly to employ and, at any rate, do not work very well. This is an implicit way of saying that the earlier work on congressional dominance—their own work—did indeed have it wrong. Their new claim is that, precisely because ex post control is highly problematic, Congress places great emphasis on ex ante control, which works much better. Ex ante control emerges as the key to understanding how Congress gets its way—which it continues to do, on their account—and why bureaucracy looks and performs as it does.

Properly chosen procedures, McNollgast argue, can mitigate problems of asymmetric information by forcing agencies to take certain kinds of technical or constituency information into account or to publicize their policy aims well in advance of formal promulgation—creating an early-warning system for politicians and ruling out faits accomplis. The Administrative Procedures Act, they argue, is a prime example of how Congress uses procedures to open up agency decisionmaking and make private information public.

Procedures also enfranchise favored constituencies by selectively granting them access and participation rights, thus injecting special interests directly into the informational and early-warning system, as well as shaping decisionmaking according to the balance of group power. In these ways, legislators stack the deck in favor of groups in the enacting coalition and ensure that changes in the interests and relative powers of groups over time are mirrored in agency process and policy. If well designed, the agency should be on autopilot: programmed to do Congress’s bidding.

In a series of articles that appeared shortly thereafter, I developed an analysis of the “politics of structure” that shares central themes with the McNollgast work—regarding the problems of information, for example, and the role of procedures in stacking the deck (Moe 1989, 1990a,b; Moe and Caldwell 1994; Moe and Wilson 1994). Yet the approach is also different in key respects and attempted to take the theory in new (at the time) directions, the most fundamental of which are discussed below. The arguments I made on their behalf are the arguments I made then and still make. As subsequent sections of this chapter show, some of these new directions have since been pursued in various ways. But some, even after all these years, remain virtually unexplored.

5.1. Multiple Principals

The McNollgast analysis submerges presidents in the enacting coalition and pays no special attention to them. But the fact is, presidents are special. Their powers and leadership are the driving forces of modern American government—and they are absolutely fundamental, as well, to the politics of structure. It would be odd if they were not.

Clearly, presidents have distinctive roles to play. They have the power to veto legislation, which allows them to pressure Congress for concessions that produce bureaucratic structures more conducive to their own goals and interests: structures that give them more discretion and control. They also have powers of unilateral action—due in part to their position as chief
executive—that allow them to move on their own to create favorable administrative arrangements (see, e.g., Howell and Lewis 2002; Lewis 2003). Through it all, moreover, they are operating on preferences and following strategies that make them very different from legislators, for they respond to broader constituencies, seek central control over the bureaucracy—and indeed, are threats that many legislators must worry about and guard against.

A theory with presidents, then, points to aspects of bureaucracy that bear a distinctly presidential stamp—some of them structures that presidents themselves create, on their own, to try to gain control. Regulatory review is an obvious example. So is much of the institutional presidency, a defining feature of modern American government that rational choice theorists largely ignore. A theory with presidents also emphasizes that many bureaucratic structures are designed by legislators to insulate parochial interests from presidential influence, and that presidents counter by adding structures of their own. These structures and dynamics are fundamental to American bureaucracy, and they are missed when presidents are lumped into the enacting coalition.

Much the same is true of the courts. They are not active players in the legislative process, as legislators and presidents are. But they have the authority to impose their own structures on the bureaucracy—as they have done with a vengeance, for example, in school desegregation disputes. And the expected outcomes of delegation decisions are clearly very different, and thus the politics of delegation is very different, depending on whether the courts can be counted on to backstop Congress—pushing wayward agencies back where Congress wants them—or can be expected instead to act on their own political preferences.

The more general theoretical point here is that the organization of American bureaucracy arises out the politics of a separation of powers system. In such a system, the legislature is not the only principal that counts. There are typically multiple principals competing for control—the legislature, the president, and the courts—and they use structure not only to impose constraints on the bureaucracy, but also to insulate it from the influence of their competitors. A simple focus on the enacting coalition misses all this.²

5.2. Forward-Looking Actors

McNollgast’s political actors are rational and strategic, but they do not do a serious job of looking ahead. In particular, they fail to take account of the “political uncertainty” inherent in democratic politics. If they did, their incentives and behavior would look very different than McNollgast claims. In American democracy (and in most democracies), today’s power holders cannot count on maintaining their hold on public authority forever. Their future power is uncertain. As a result, they cannot commit tomorrow’s authorities—who may turn out to be their opponents—to respect whatever deals are arranged, and whatever structures and policies are created, in the current period. They face a commitment problem: they want to provide a stream of benefits into the future for their own constituents, and in return receive those constituents’ votes—but they cannot guarantee that the future benefits will actually be provided. Constituents, then, have good reason not to believe their promises. And not to support them or make deals with them. So what can today’s power holders do? How can they commit?

². For pioneering efforts (at about this time) to model multiple principals and their struggle to control the bureaucracy, see Calvert et al. (1989), Hammond and Miller (1987), and Hammond and Knott (1996—although earlier versions were floating about years before).
The answer is that the policies and structures being created today must be protected from future authorities and thus insulated from democratic control. The best way to do this is through ex ante control mechanisms—decision procedures, civil service rules, independent forms of organization, formal timetables—that not only stack the deck, but also lock in the bias to protect against future changes in power and authority. Today’s enacting coalition, in other words, wants to ensure that tomorrow’s legislature—perhaps led by an opposing coalition—cannot control the bureaucracy.3

Obviously, this logic puts a different spin on things. McNollgast’s enacting coalition fixes its gaze on the bureau, which threatens to drift away. On their account, the coalition relies not only on deck stacking but also on procedures that force the bureau to reveal information, open its internal processes, and suffer outside intervention to keep it in check. In a world of political uncertainty, however, the enacting coalition must also cast a wary eye on the legislature itself, indeed on all future authorities and group opponents, and use structure to insulate against their control. Because of political uncertainty, the coalition often does not want openness or intervention, and it favors structures that shut out most opportunities for control by others.

Because of political uncertainty, then, the shortcomings of ex post control should prove more severe than McNollgast suggest, and they should arise from more than just the usual slippage in the principal-agent relationship. Basic obstacles to control should purposely be created by Congress itself, which should often have incentives to build bureaucracies with considerable autonomy that pursue the original intent of the law—and that resist Congress’s own efforts at ex post control. Ex ante control, then, emerges as a two-edged sword: it promotes the control of today’s Congress by rendering tomorrow’s Congress weak.

5.3. Institutional Context

The McNollgast theory is Congress-centered and peculiar to the American political system. Yet there is good reason to think that different institutional systems have their own distinctive politics of structure and thus their own distinctive bureaucracies. The logic of politics in the United States should be different from the logic of politics in Britain and elsewhere. A focus on political uncertainty—which scholars usually ignore—makes the case very clearly (see especially Moe and Caldwell 1994).

Consider the American system of separation of powers. It fragments power among institutions, and its multiple veto points ensure that new laws are difficult to enact. Whatever does get enacted, however, tends to endure because separation of powers then works to its advantage, setting up obstacles that prevent opponents from changing it. This feature makes the lock-in of current interests possible: whatever protective structures are imposed to insulate today’s creations from change probably cannot be lifted by opponents in the future, even if they are powerful—because supporters can likely find a way to block change. The agencies and policies they seek to protect, then, will remain protected. And the flow of benefits to constituents will endure.

In such a system, all actors therefore have incentives to rely heavily on formal structure to insulate their creations, solve their commitment problems, and solidify their political deals.

3. The importance of forward-looking rationality is emphasized throughout my own work, but it is also recognized in early work by Horn and Shepsle (1989) and in a full-length book by Horn (1995). Interestingly, Horn does not pursue the differences between presidential and parliamentary systems (see below).
Formal structure protects them against political uncertainty, ensuring that whatever is created today will endure tomorrow. And because all players have strong incentives to formalize, the bureaucracy in such a system will tend to be buried in formal rules.

Now consider a pure Westminster-like parliamentary system. In such a system, power is entirely concentrated in the majority coalition, and there are no veto points in the policy process. As a result, political uncertainty is dramatically heightened. Passing laws is relatively easy, but so is overturning them: for if the opposing coalition comes to power, it too will have concentrated power, and it will be able to subvert everything the first coalition has put in place. Formal structure therefore has little strategic value as a protector of interests or solution to commitment problems—and the incentives to formalize are therefore much reduced. This should tend to produce a bureaucracy that, by comparison to its counterpart in the American system, is granted more discretion and is much less burdened by formal constraints. The logic of politics is very different in the two systems, and their bureaucracies should be very different as well.4

Most parliamentary systems are not at the Westminster end of the continuum, of course. Some divide institutional authority in various ways (e.g., through bicameral legislatures), and some have electoral systems that often give rise to minority or coalition governments, yielding institutions whose veto points place them somewhere between the American and the Westminster extremes. For these contexts, the incentives to formalize should also tend to be somewhere between, as would the nature of their bureaucracies. The argument here is not that presidential and parliamentary systems represent dichotomous system types, but rather that the basic features of political systems generally—such as divided authority and veto points—tell us a lot about the politics of structure and about bureaucracy. The American system is not necessarily very representative. A more broadly based, more fully comparative theory is surely preferable.

5.4. Effectiveness

The McNollgast theory is about what Congress can do to prevent runaway bureaucracy, which is an important issue. The question that has traditionally been at the heart of public administration, however—the effectiveness of the bureaucracy—is given no serious attention. The presumption seems to be that, as long as agencies are under control and prevented from drifting, they will perform effectively and constituents will get their benefits.

The theory needs to deal with effectiveness head-on. Actually, the control relationship itself points immediately to potential problems for effectiveness: for when the legislature imposes structures to stack the deck and otherwise constrain agency behavior, it interferes with the agency’s best applications of its own expertise and undermines its ability to perform. Moreover, particularly in the American system, political uncertainty gives the winning coalition incentives to load agencies up with burdensome formal constraints to insulate them from unwanted future influences; and, especially for agencies in complex policy areas or changing technological environments, these constraints may impede performance. Similarly, the American system’s many veto points often require that the winning coalition compromise with the losing coalition in the design of agencies; but the losers often have incentives to impose formal restrictions that purposely undermine agency performance—they have incentives, in other words, to create agencies

that are designed to fail. Still more problems arise because of competition among principals: for attempts by Congress to insulate against presidential influence will create additional layers of formalism and restriction, whose purpose is political and is likely to get in the way of effective performance.

Effectiveness should also vary with institutional context. In a classic Westminster system, burying agencies in informal rules does not work as a strategy for insulating them from political enemies—so such rules should tend not to be imposed, and agencies should be less burdened with ineffective structures. Moreover, the losers in these systems need not be granted a role in designing public agencies, and there are no competing principals to insulate against one another’s influence. These features, too, suggest that bureaucracies should tend to be less burdened with structures that, in the American system, make it difficult for agencies to do their jobs.

The discussion as I have developed it actually begs a deeper question, which is: effective for what? The meaning of effectiveness turns on the specific goals that are to be achieved; and much of the politics of structure, and thus many of the formal structures that are heaped on bureaucracy to insulate and control it, are driven by the fact that different principals—today’s legislative coalition, the president, the legislative losers, the future holders of power—have different political goals. Any given principal surely wants the bureaucracy to use its expertise effectively in pursuing that principal’s own goals. But if other goals threaten to take priority, expertise and effective performance can become bad things, and restricting them becomes a rational strategy. Bureaucratic effectiveness is in the eye of the beholder. What is effective for principal A is not effective for principal B: it depends on whose goals are being pursued and whose ox is being gored.

6. Formal Models of Delegation

Some of the early work on political control was formal (e.g., Bendor et al. 1985, 1987a,b; McCubbins, 1985; Banks 1989; Banks and Weingast 1992; Calvert et al. 1989). But many of the basic ideas—in McNollgast’s work, for example, as well as my own—were set out in arguments that, while firmly rooted in the new economics of organization, were developed informally rather than through modeling. There were advantages to doing so, as it allowed for analyses that were more wide ranging and able to shed light on aspects of political control that would likely have been difficult to explore—and doubtless would have gone unexplored and undiscussed, at least for some time—if the focus had been narrowed from the start by the technical constraints of formal models. Modeling is deductively powerful, if done right. But it can also be conceptually limiting, and there is much value in having a division of labor in which formal and informal analyses work together, with each doing what it does best.

Since the early 1990s, the literature on political control has grown considerably, and it has become much more formal. This newer work incorporates and explores some of the key foundational ideas—but not all of them—and extends the theory in a variety of ways. One stream focuses on ex ante control, most often the legislative decision to delegate. The other is about ex post control, taking the agency and its mandate as given and exploring how legislatures and other political actors can shape the behavior of existing agencies.

Models of delegation have shed light on the foundations of bureaucratic organization, and they are the focus here. As in the early work on ex ante control, the legislature is naturally
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at center stage in these models, because it makes the laws and is in the position of creating, designing, and empowering administrative agencies. But what the newer models do, in effect, is to whittle away at the theoretical arguments of the earlier literature, reducing them to barebones formal representations. (They also, of course, introduce new arguments of their own.) In the process, some of these arguments are more powerfully explored—but for reasons I discuss below, some are dealt with in stilted and unsatisfying ways, and others are not really pursued at all.

One contribution is clear: these models put the earlier claims of congressional dominance firmly to rest. A central theme running throughout these newer models is that the legislature faces a trade-off between expertise and political control. All else being equal, the legislature benefits when it imposes structural restrictions, limits administrative discretion, and gains control over the agency’s policy choices: maneuvers that are intended to get the agency to target the “right” goals. But these very restrictions render it difficult for the agency to adapt to changing circumstances—new technologies, new problems—and thus to use its expertise effectively, even if it does pursue those goals.

This is essentially a variation on the effectiveness dilemma we discussed in the prior section. The legislature fears that the agency will use its expertise to effectively pursue the “wrong” goals—but by imposing restrictions to get it to pursue the “right” goals, it undermines the agency’s ability to pursue those goals effectively. The legislature’s challenge, of course, is to strike the right balance between control and expertise. It is clear, however, that total political control is an extreme solution that is usually not desirable (were it possible) and would be irrational to pursue. An optimal balance would lead, in most cases, to conscious limitations on political control and thus to situations in which agencies are designed to have a measure of autonomy.

Congress would not even try to “dominate.”

An early formalization of this balancing act was carried out by Bawn (1995), who, as it happens, did not highlight its implications for congressional dominance. Her model contains two actors, the legislative coalition and a bureaucratic agency. The agency is assumed to have greater expertise than the coalition, and its expertise is defined by reference to a distinction between policies and outcomes—a modeling strategy that was gaining prominence at the time in the larger literature on political institutions (see, e.g., Gilligan and Krehbiel 1990) and would eventually become quite fundamental to the theory of delegation. The agency implements its congressional mandate by choosing a specific policy, located somewhere on a unidimensional continuum, and this choice of policy then generates an outcome (in terms of solutions to social problems or effects on constituents, say) that may be located somewhere else on the continuum. Mathematically, the realized outcome of agency action is assumed to be a function of the agency’s implemented policy and a random error term—and it is the agency’s knowledge about the distribution of the error term, and thus, more generally, its knowledge of what outcomes are most likely when it chooses a particular policy, that constitutes (and within the model, defines) its expertise.

Bawn’s legislative coalition exercises control by using “procedures” to choose the level of agency “independence,” where the latter is broadly defined as the agency’s freedom to make its own decisions without legislative constraint. Her model shows that independence has two contradictory effects. On the one hand, the agency is better able to apply its expertise (operationalized as a smaller error variance in its estimation of outcomes) the more independence it is
granted. This effect is beneficial to the coalition. On the other hand, the agency is more likely
to have policy preferences that diverge from those of the legislature (operationalized as a larger
variance of agency preferences around a legislatively induced mean) the more independence it is
granted. This effect threatens greater bureaucratic drift and is costly to the coalition. The coal-
tion’s optimal solution is to choose the level of independence that gives it the greatest net gains,
and this level varies, depending on conditions specified in the model. When policy is especially
salient to powerful interest groups, for example, bureaucratic drift threatens to be especially
costly, and the optimal level of independence will be lower. In complex policy areas, the value
of agency expertise to the legislature will tend to be higher, and the optimal level of indepen-
dence higher. Sometimes the legislature will exercise substantial control, then, and sometimes
it will not.

Although the themes of Bawn’s analysis have become central to the literature, the model
itself has not. Its assumptions, which express how independence is dealt with operationally,
were particularly complicated and difficult to work with mathematically, and (for that reason)
the model was actually set up as an exercise in decision theory, with the legislature as sole
decisionmaker, rather than as an exercise in game theory that explored the interdependent
decisions of the legislature and the agency.

Writing at about the same time, Epstein and O’Halloran (1994, 1996) developed a much
more straightforward approach to the modeling of delegation and control. Instead of dealing
with procedures and independence, and indeed, instead of dealing with aspects of agency orga-
nization in any explicit way, they collapsed all these considerations into one simple concept that
became the foundation of the entire analysis: the level of discretion that the legislature delegates
to the agency. They also operationalized it in an exceedingly simple way. Their approach was a
conceptual breakthrough, and it blazed a path that the rest of the formal literature on delegation
would ultimately follow.

Specifically, Epstein and O’Halloran assume that the legislature sets a baseline policy, \( p \), and
a level of discretion, \( d \). (The story line is that the legislature crafts \( d \) by writing statutes that
are more or less detailed in their structural constraints—but these elements are not modeled.)
The agency is required to implement a policy of its own, \( p_A \), that is within plus or minus \( d \)
of the baseline policy. If the legislature has done its job right in delegating, the agency will use
its expertise—its superior knowledge about the connection between policy and outcomes—to
choose a policy within its discretionary range that, once an outcome is realized, proves to be
better for the legislature than the baseline policy is and thus better than the legislature could
have done by giving the agency no discretion at all.

Epstein and O’Halloran’s Delegating Powers (1999) is their most comprehensive statement
on the subject and is a tour de force of social science analysis: combining grand theory, an
elaborate formal model, and wide-ranging empirical tests based on original data. They develop
what they call a “transaction cost theory of delegation,” arguing that the legislature’s decision
to delegate is analogous to the make or buy decision that firms confront in the private sector
and is subject to the same hold-up problem. The make or buy component arises because the
legislature can either make policy itself, relying on its own specialized committees to specify
all aspects of policy in excruciating detail, or it can buy the policies externally by delegating to
agencies and giving them the discretion to determine what those details should be. Both options
come with their own costs. If policy is made by committee, there are costs of information, delay,
collective decisionmaking, and the like. However, buying policy from the agency—delegating—threatens the legislature with a hold-up problem (as they characterize it), because the agency has incentives to shirk. The legislature must therefore determine when delegation is more efficient than internal production, as judged by reference to their own political goals.

The logic at work here is a bit strained. In a typical economic analysis, the hold-up problem arises when two firms, A and B, stand to gain from cooperating—B is going to buy its inputs from A, let’s say—but the deal involves investments by A that are specific to the relationship; and once A’s investments are made, B has an incentive to hold-up A by threatening to renege on the deal and demanding lower prices. Because A can anticipate this problem from the outset, it may refuse to enter into the deal at all, leaving both worse off. The classic solution is vertical integration: firm B purchases firm A, overcoming the cooperation problem by bringing the latter under its own authority and control—and giving it the capacity to make the goods it previously wanted to buy.

The situation that Epstein and O’Halloran are analyzing—a situation rooted in government—is actually different. The bureaucratic agency and the legislature are already vertically integrated: the legislature is the superior authority, the agency is the subordinate, and the latter is not free to make its own independent decisions. The legislature might be said to have a make or buy decision, in the sense that one potential producer (the committee) is inside the legislature and one (the agency) is outside. But both producers are under the legislature’s own authority from the outset and are subject to its control. It may have a hard time getting the agency to do exactly what it wants, and presumably a harder time than with the committee; but this is the case for any superior in any organization and is simply a version of the shirking problem that frustrates top-down control efforts. There is no hold-up problem in the usual sense.

More generally, the transaction cost framework, which is presented as the analytic foundation for the book, is not put to very productive use. Indeed, after devoting a good deal of attention to the transaction costs involved in producing policy—the haggling, the collective action problems, the delays, and so on—they ultimately construct a formal model that leaves almost all of these costs out and is not different or better in any distinctive way because it is “derived” from the logic of transaction costs.

That said, the make or buy framing itself, properly interpreted, is helpful. There is value in recognizing that the legislature always has the option of producing policy itself and that its delegation decisions are calculated with reference to the baseline of internal production—its costs and benefits, its problems and advantages. This may seem obvious on reflection, and indeed it was central to the way delegation was approached in the earliest attempts to model it (e.g., Fiorina 1982a,b, 1986). But when the new economics of organization took hold in the 1980s and spawned interest in political control, serious interest in the baseline of internal production fell by the wayside. The focus was simply on how the legislature could delegate to an agency and still maintain control. Epstein and O’Halloran deserve credit for reasserting the importance of the baseline and for trying to build a theory that explores it more fully.5

5. Their effort to do so, however, led to a game-theoretic model (described below) that is far more complicated than the simple discretionary-window component I discussed earlier, for it has a signaling model—in which the committee (as agent) makes proposals to the full legislature (the principal)—nested within it. I suspect it is for this...
In the Epstein-O’Halloran model, there are three players: the legislative floor, the committee, and the agency. And there are three stages to the game: in the first, the committee gains (incomplete) information about the link between policy and outcomes, designs a bill, and sends it to the floor; in the second, the floor either enacts legislation or delegates to the agency with some discretion, \( d \); and in the third, the agency—which is perfectly informed about the link between policy and outcomes—chooses a final policy within the discretionary limit set by the legislature. The legislature can gain from delegating to the agency and allowing it (via \( d \)) to use its perfect information in choosing a “good” policy; but the agency has its own policy preferences and may stray from what would be best for the legislature. Rather than put up with the dangers of agency shirking, the legislature could rely on in-house production of policy through its committee; but the committee has preferences that may also differ from those of the floor, and it is not as well informed as the agency, so there are problems on this end as well. Both options are imperfect, and the floor chooses delegation when it is the better option.

This model leads Epstein and O’Halloran to two basic conclusions, which then become the focus of their extensive empirical tests. They show that, all else being equal, the legislature is more likely to delegate and grant discretion to an agency (1) the more uncertain and complex the policy area, and (2) the closer the agency’s policy preferences are to those of the legislative floor. The first is often referred to in this literature as the uncertainty principle. The second is often referred to as the ally principle. In moving from model to reality, they relate the ally principle to divided government: arguing that the president plays the key role in determining agency preferences, and thus that, when the president and the Congress are controlled by different parties, there is likely to be greater policy conflict (divergence of preferences) between the agency and the legislature, and less delegation and discretion. This assertion about divided government is the best-known and most influential substantive claim to come out of their research program.

In focusing scholarly attention on the simple concept of discretion and in demonstrating how discretion can be modeled and empirically studied, Epstein and O’Halloran have put an indelible stamp on this literature and contributed greatly to its progress. As is perhaps inevitable at this stage, however, their approach fails to incorporate certain aspects of delegation that are quite important and that ultimately need to be taken into account. One of these arises from a key assumption about how the agency responds to political control: they assume that, once the legislature sets the level of discretion, \( d \), the agency chooses a policy in its own best interests—but that it also stays within the discretionary window. In this crucial respect, then, the agency is assumed to be perfectly compliant. This assumption would seem to be out of place in a model of delegation and control, for compliance should be regarded as problematic and worth explaining. A whole realm of agency choice gets assumed away, and power is attributed to the legislature that it may not have in reality.

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reason that other researchers have embraced the discretionary-window framing of the Epstein-O’Halloran model but dropped the make or buy component. The lesson: the internal production baseline is clearly important to the logic of delegation, but no one has yet figured out how to model it in a manageable way, given all the other things that demand inclusion in a theory of legislative delegation.

6. Another basic conclusion has to do with the internal baseline: Congress is less likely to delegate to an agency the closer the committee’s ideal point is to its own (and vice versa). But the empirical tests focus on delegation to agencies, and that is the focus of the book as well, so this aspect of the analysis is not highlighted in their empirics.
The other limitations of the Epstein-O’Halloran model have to do with its failure to move in the new directions discussed earlier:

- The president is essentially ignored, aside from their assumption—now standard—that he sets the agency’s policy ideal point (always setting it equal to his own). Epstein and O’Halloran say that omitting the president is not a problem, because it does not affect the comparative statics of their model. But this is just a way of saying that their model is limited. Yes, the direction effects of policy uncertainty (the uncertainty principle) and policy conflict (the ally principle)—the two effects they focus on—would be the same, president or no. But the reality of American politics is that the president has an obvious stake in getting as much discretion for executive agencies as possible and thus in opposing (perhaps by vetoing) legislative attempts to restrict it. And any model that adds the president would certainly lead us to expect more discretion for agencies overall—a very important conclusion—as well as variations in discretion, depending on how particular agencies bear on his agenda and leadership. In general, it is difficult to see how, with the president such a powerful and central player who places enormous value on bureaucratic discretion, an adequate theory of delegation can be built by leaving him out.

- Epstein and O’Halloran’s actors are not forward looking and thus are completely unconcerned about the political uncertainty associated with future shifts in political power. A new president could take office and radically change agency ideal points. A new party could take control of the legislature itself and begin redirecting public agencies. New legislators could take control of key committees and assert control over the agencies in their jurisdictions. Today’s decisionmakers obviously need to recognize these possibilities and shape their delegations accordingly. To do otherwise would simply be a mistake.7

- Their model is legislature centered and peculiar to the American system of separation of powers. Yet it does not have a president, and it does not contain any of the veto points that make the American system what it is. (The model does contain an internal committee, but the latter is not a veto player.) It is also not designed to explore the dynamics of a parliamentary system, nor of other forms of government. So in general, what it has to say about the effects of institutional context is limited.

- The model speaks to the trade-off between control and expertise but otherwise does not explore the various ways in which political control can undermine agency effectiveness. Questions of whether agencies can actually do their jobs well and how their capacity for effective performance is affected by the control activities of politicians need to be brought more fully into the theory.

Subsequent work has helped to address these issues. Volden (2002), for instance, focuses specifically on Epstein and O’Halloran’s omission of the president. Using their agency discretion framework, he develops a more general model that incorporates the president and allows agencies to have their own policy preferences that are not presidentially imposed. He also

7. Epstein and O’Halloran (1994) actually did devote a small portion of an early article to questions of political uncertainty, but the subject never became part of their research agenda and was not incorporated into their 1999 book.
departs from Epstein and O’Halloran in another significant way. He notes that, although they essentially allow Congress to start from scratch in dealing with agencies—as though the latter were brand new—the typical situation, empirically, is that Congress is dealing with existing agencies that already have policy positions and levels of discretion. Thus, Volden assumes that each agency comes with a status quo policy and a status quo level of discretion, which then serve as reversion levels should Congress decide not to act or be unable to.

Volden’s analysis clearly shows that, when presidents are taken into account, agencies do indeed get more discretion. He shows, in addition, that there is an important asymmetry inherent in the dynamics of delegation. This occurs because, when an agency has a low status quo level of discretion, the president will tend to go along with any legislative attempts to increase it—but when an agency has a high status quo level of discretion, he will tend to veto any legislative attempts to decrease it. Thus, over time, increases in agency discretion tend to be difficult to reverse: they can move up much more easily than they can move down.

That such a ratcheting effect might arise out of the politics of delegation is a thought-provoking result and potentially important. And it would never have been discovered, needless to say, in a model that simply ignores the president. It is worth adding, however, that Volden’s model—in a feature that embraces, rather than departs from, its lineage—assumes that actors are not forward looking. A legislature capable of looking down the road (or engaged in a long-running repeat-play delegation game) would surely recognize that today’s delegation of discretion would become tomorrow’s status quo level of discretion and be difficult to reverse. It would see the status quo level of discretion (and policy) as endogenous, not simply as fixed and given, and it would recognize Volden’s ratcheting effect. This would affect its decisions today, probably inducing it to choose less discretion. A more complete picture awaits a model of forward-looking decisionmakers.

The most ambitious attempt to build on the Epstein-O’Halloran base is Deliberate Discretion? by Huber and Shippan (2002). Like Volden, Huber and Shippan include presidents in their model of the American system of separation of powers. But their larger purpose is to move beyond the traditional Americanist focus of the literature to develop a comparative theory of delegation: one that explains delegation decisions across a range of institutional contexts. Like Epstein and O’Halloran’s Delegating Powers, this analysis is a legitimate tour de force, an exercise in theory and empirics that stands as one of the landmarks in the field.

8. The delegation models common to this literature assume that the superior makes one delegation decision. For an attempt to understand delegation as a repeated game (but focused, substantively, on the relationship between the legislature and its committees, not the bureaucracy), see Diermeier (1995).

9. I should note that Volden’s analysis contains an ambiguity that reflects a confusion of sorts in the larger literature. He begins his model by fully embracing the Epstein-O’Halloran agency discretion framework, but he describes it as one in which the agency is required to choose a final outcome that is within of the baseline policy, and he constructs his own model accordingly. This is not the Epstein-O’Halloran set-up. In their model, the agency must choose its own policy position so that it falls within the discretionary region—but because the final outcome is a function of agency policy and random error, the final outcome could well fall outside that region. In Volden’s model, the outcome must be inside the region, but the policy may be outside of it. The two models are very different, then, in how they define the discretionary region, but they are presented as being the same. Their differences, moreover, cannot help but lead to different conclusions about delegation, making comparison difficult.
Using an agency discretion framework, Huber and Shiban give due regard to the two main
concerns at the heart of the modern literature: policy conflict between the agency and the
legislature (which leads to the ally principle) and policy uncertainty (which leads to the un-
certainty principle). But these factors are insufficient, they say, for understanding how statutory
controls—and thus discretion—vary across institutional systems, and they highlight three ad-
ditional sets of factors that need to be taken into account.

The first is the capacity of the legislature to write specific statutes, as this determines how
costly it is for the legislature to restrict agency discretion; legislatures with part-time mem-
bers who meet every year or two, for example, would find it very difficult to write finely tuned,
well-researched statutes to control agency behavior in just the right ways, whereas professional,
full-time legislatures would be much more capable of it.10 The second is whether delegation
decisions involve veto players, which may be presidents but may also be a second house of the
legislature. And the third consists of “nonstatutory factors”—the courts, legislative oversight
and vetoes, corporatist arrangements—that can intervene after the delegation decision is made
and can affect its outcomes.

Embedded in this third component is another expansion of the theory, and a particularly
important one: the agency may decide to choose a policy outside its discretionary region, and
thus not to comply with legislative directives. If it does go this route, there is some probability
that nonstatutory factors may intervene to catch and sanction it—as might occur, for example,
if its noncompliance is overturned by the courts. This probability may be large or small, de-
dpending on how well any given institutional system is set up to detect and respond to agency
noncompliance.11

In formalizing a broader array of influences on the delegation decision, Huber and Shiban
(2002) generalize the theory. But at least as important—certainly in explaining its reception
among students of government—the authors also provide insightful discussions of how the var-
ious formal components should be thought about and empirically operationalized in different
institutional contexts. In a separation of powers system, for example, policy conflict is under-
stood in terms of the contrasting preferences of the legislature and the agency, with the latter
assumed to be in line with the president; but this obviously does not work in a parliamentary
system, where there is no separation between the legislature and the executive. In a parliamen-
tary system, they argue, policy conflict arises when the parliamentary majority is made up of
more than one party, as it is in coalition and minority governments. In these cases, the parties
will have different preferences, and those that do not control the relevant ministry (with jurisdic-
tion over the agency) will have incentives to use statutory controls to restrict discretion. A
divided majority, then, is the parliamentary analogue to divided government. Similar differences
in operational meaning apply to the other key variables. It is this empirical maneuver, more than

10. Note that this is an indirect and very simple way of taking into account Epstein and O’Halloran’s internal
production baseline, as it allows the legislative floor to make its delegation decisions based on a recognition of how
costly it would be to make all policy decisions in house.
11. See also Gailmard’s (2002) analysis of bureaucratic subversion. His model allows bureaucrats to move
beyond the discretionary bounds at a cost and arrives at interesting and provocative conclusions. Among them:
that the legislature is actually better off when subversion is relatively cheap (because it often benefits when agencies are
able to use their expertise in choosing policies better than the uninformed legislature had asked for), and agencies
are better off when subversion is made more expensive (because if it is easy to subvert, the legislature tends to
constrain discretion).
anything else, that makes their analysis comparative: the same theoretical concepts are given
different empirical referents in different systems, and these differences are primarily what account
for expected differences in delegation and discretion across institutional contexts.

The downside is that the formal models themselves are not doing enough of the work here,
and are disappointing in certain respects. One problem has to do with the way nonstatutory fac-
tors are handled. Huber and Shipan lump all these factors (the courts, legislative oversight, etc.)
together, assume there is a fixed probability they intervene to correct agency policy choice—
and further assume that, if such intervention occurs, policy automatically moves to the legislature's
ideal point. But why should the legislature be able to count on being perfectly backstopped (with
some probability) by the courts? Why shouldn't it worry that the courts might have their own
preferences and move policy to their own ideal points (Segal and Spaeth 2002)? Simplification
is one thing, but Huber and Shipan's assumption creates a best-of-all-possible-worlds scenario
for the legislature and ensures that it will write more discretionary laws when the probability
of intervention is high. Outside intervention is always good. More realistic, less benign assump-
tions about the political environment would obviously lead to very different conclusions about
delegation and discretion.12

These are not the only assumptions that should raise eyebrows. Huber and Shipan also
assume, for example, that the probability the agency is caught and punished for noncompli-
ance is exactly equal to the probability that nonstatutory factors will intervene, and that this
probability is fixed. Fixing the probability, however, ensures that the agency's risk from engag-
ing in noncompliant behavior has nothing to do with how "bad" its policy choices are—how far
outside the discretionary range it chooses to go. This assumption is obviously quite unrealistic,
and it distorts the agency's true incentives for noncompliance.

Their formal models are also disappointing for a more fundamental reason. Huber and
Shipan build three distinct (but overlapping) models that can be compared and contrasted:
a parliamentary model, a veto model, and a bicameral model. As a strategy for providing a
comparative analysis of institutional systems, this multiple-models approach makes good sense.
But there is less here than meets the eye. The parliamentary model is nothing more than a
simple game between the legislature and the agency—and, shorn of peripheral assumptions,
is essentially the same as Epstein and O'Halloran's model of legislative delegation in the United
States: which, of course, is not supposed to be about parliamentary systems at all. What exactly
is parliamentary about the Huber-Shipan model? Only that it lacks a veto player (namely,

12. Bendor and Mierowitcz (2004: 301) agree that this perfect-backstopping assumption by Huber and Shipan
is a problem for their analysis, essentially for the reasons I have stated—adding that it gives superiors "a perverse
incentive to induce agents to shirk," so that the outsiders can catch them and bring them back to the superiors' ideal policies.

13. Note that the veto model is supposed to represent a (unicameral) presidential system, and the bicameral
model is structured in such a way as to represent a separation of powers system, not a bicameral parliamentary system.
There is, at least, institutional variation to explore here, and the focus on veto points (none, one, or two) is basic and important. Still, the institutional set-up is overly spare for gaining insight into parliamentary government—particularly given that, in all three models, actors are thinking only of the here and now. They are not thinking about how power alignments might change in the future, nor are they thinking about the long-term durability of what they are creating—and so the fact that political uncertainty is especially severe in parliamentary systems (and durability a much more difficult problem less amenable to solution through formal structure) plays no role whatever. The rest of the literature is guilty of this same omission, of course. But for Huber and Shipan, the omission is more critical, for their analysis is much celebrated precisely because it aims to construct a comparative theory of delegation—and what they leave out is quite fundamental to an understanding of why delegation should differ across parliamentary and presidential systems, and why bureaucracies should differ as well.

This is an opportunity missed, and it promotes conclusions that are likely to be misleading. Indeed, their formal analysis indicates that the effects of their key variables—policy conflict, legislative capacity, the probability of intervention by outside forces—are the same for presidential and parliamentary systems. Empirically, differences arise because the measured levels of conflict, capacity, and intervention vary from system to system. But the underlying analytic relationships are the same, as is the basic logic of delegation. The only fundamental contrast is that, in their models, the level of discretion is always at least as great in a presidential system (all else being equal), because presidents want more of it and use their veto power to get it. As discussed earlier, however, the logic of delegation should be distinctively different in presidential and parliamentary systems once actors are allowed to be forward looking and take political uncertainty into account. And the implications run in precisely the opposite direction of the Huber and Shipan model: pointing to much less discretion in a presidential system (all else being equal). The empirical evidence, moreover, suggests that American bureaucracy is, in fact, far more constrained by statutory restrictions than bureaucracies in parliamentary systems are (e.g., Moe and Caldwell 1994).

Epstein and O’Halloran (1999) and Huber and Shipan (2002) are the keystones of the modern literature on political control of the bureaucracy. Both have their drawbacks, but that is to be expected in work that strives to push the envelope. There is simply too much that needs to be done, and it cannot be done all at once. The important thing is that both have clearly succeeded in generalizing the theory and framing the way political control is thought about and studied. In the years since their publications, they have stimulated a spate of new work that builds on their analytic base and elaborates the theory still further. I do not have the luxury of discussing all of these developments, but I want to spotlight several that, because of the substantive topics they address, are especially promising avenues of inquiry.

6.1. Appointments

This is a legislature-oriented literature that only minimally explores the powers and impacts of presidents. Huber and Shipan (2002) and Volden (2002) include the president as a veto player in

14. To be clear: presidents use their leverage within a separation of powers system to push for bureaucratic discretion—but bureaus should still tend to have far less discretion in a separation of powers system than in a parliamentary system.
the legislative process, but the president’s power of appointment—which is obviously relevant here—has received little attention. Typically, appointments come into play only indirectly in delegation models: bureaucratic agencies are assumed to have ideal points identical to the sitting president’s (or this “result” is derived from assumptions that readily guarantee it). Little is learned, therefore, about the strategic use of appointments by presidents, its impact on the legislative delegation, and the consequences for agency behavior—all of which ought to be integral components of the theory.

McCarty (2004) attempts to do something about this shortcoming, developing a model of political control in which the president and the legislature are essentially co-equal actors. For reasons that are unclear, however, he does not adopt the agency discretion framework and crafts his model instead along quite different lines. The three actors are familiar: the legislature, the president, and the agency. But the set-up is not, for it is built around budgets. The legislature has the power to make budgetary proposals for funding the agency, and it also has a role in confirming presidential appointments. The president makes appointments and can veto legislative budgetary proposals. And the agency makes policy choices under the constraint that any departure from the status quo is costly and requires budgetary funds provided by the legislature. The upshot of this last assumption is that, when the legislature grants the agency a budget, it is essentially placing bounds on how far in either direction the agency can move from the status quo. In this sense, it retains one of the trappings of the agency discretion framework, with the budget determining how much discretion the agency has.

The thrust of McCarty’s argument is that there is a dilemma inherent in the ongoing struggle between the legislature and the president to control the agency. The nub of the problem is that the president cannot commit to the appointments he makes—because once the legislature decides on a budget, the president can remove his initial appointees and replace them with others whose views on policy are closer to his own. Realizing this ex ante, however, the legislature may decide to give the agency a smaller budget, and thus less discretion, than it would actually like to. Both the president and the legislature are likely to be worse off as a result. The dilemma could be resolved if appointment and budgetary powers could somehow be centralized in the same hands, as in a parliamentary system. But more practically, the problem can be mitigated through independent commissions, civil service, and other devices whose statutory restrictions essentially allow the president to commit. Once he is able to do so, he can effectively obtain higher levels of discretion (budgets) for the agency by trading off appointments more to the liking of the legislature.

This analysis is interesting, but it is also problematic. One reason is that the idiosyncrasies that set it apart from the rest of the literature—creating an awkward fit, and making comparison and cumulative work more difficult—are also hard to justify. A key driver of this model’s results is the assumption that any agency shift away from the status quo in either direction is costly, but this does not square well with reality. A regulatory agency might find it enormously expensive to launch waves of new inspections and enforcement actions, for example, whereas cutting back on inspections and enforcement would cost it nothing—indeed, it might save tons of money. So even when agencies make important shifts away from the status quo, these shifts are not necessarily costly at all. This is especially true when the shift involves doing less work—which is precisely the way many agencies do in fact change their policies. The model’s basic assumption about agencies—which, in turn, determines how legislatures and presidents deal with them, and with one another—seems off the mark.
More fundamental still, this is an article about a commitment dilemma, but whether it really captures something empirically central to the struggle for political control is debatable. Presidents certainly do not have a history—even at the margins—of making initial appointments, waiting for the legislature to make key policy choices, then firing the appointees and making new, more extreme appointments. The legislature could punish any president who behaved in this way, not only by refusing to confirm the new appointees (regardless of the latter’s policy views) but also by undermining his policy agenda, launching investigations, and in myriad ways making his life miserable. And perhaps most importantly, there are heavy costs to presidents if they continually appoint and fire bureaucrats—for it creates disorganization, discontinuity, lack of expertise, and weak leadership within the agencies, and it takes time and attention from the president’s own staff. Removing appointees is something presidents do not want to do.

Thus, the commitment problem at the heart of McCarty’s model would seem to be a non-problem in practice and not the place to start in understanding the role of presidential appointments. Even so, this is a useful contribution, because it puts the focus squarely on appointments, argues the need for modeling their dynamics, and encourages future work along these lines. Presidential appointments need to be an integral part of the political logic of delegation.

6.2. Development of Agency Expertise

At the heart of these delegation models is the agency’s advantage in expertise. How or why the agency develops its expertise, however, is left unexplored. Typically, the agency is simply assumed to have perfect knowledge of the connection between policy choices and policy outcomes, the legislature is assumed to be uninformed on this count, and the models go on to show why it is beneficial for the legislature to delegate discretion to take advantage of what the agency knows. With expertise so crucial to an understanding of delegation, though, there is good reason to explore its role in greater depth by making it endogenous to the theory, and thus a product of decisions by key actors. 15

A provocative step in this direction has recently been taken by Gailmard and Patty (2007). Using an agency discretion framework, they build their model around two types of bureaucrats: the zealot who is purely motivated by policy and the slacker who is purely motivated by material gain. They use these types to shed light not only on the legislature’s decision to delegate discretion but also on the bureaucrats’ decisions about whether to invest in expertise and whether to continue their jobs with the agency (or leave for the private sector).

The model reveals that only the zealots have incentives to invest in expertise and stay with the government long term—and that the legislature, to take advantage of agency expertise, essentially pays for it by giving discretion to bureaucrats (the zealots) who have their own policy preferences and can be expected to depart from the legislature’s ideal. Moreover, the legislature has incentives to provide these zealots with tenure in their jobs, for otherwise they would not make investments in expertise—which can only pay off with continued service to the agency over time.

15. Incentives to specialize have been explored, however, in models of legislative organization, where the question is whether and to what extent committees will choose to pay the costs of becoming experts in their substantive jurisdictions. See, for example, Gilligan and Krehbiel (1990). See also Bendor and Mierowitz (2004) for applications to delegation.
There are a few questionable assumptions here that shape the analysis. Gailmard and Patty assume, for instance, that expertise has value to the bureaucrat only within government, and that its value is zero in the private sector. For many government jobs, this is clearly not true. Indeed, the well-known “revolving door” phenomenon, in which governments lose employees to the private sector, is largely driven by the expertise these employees gain from their public service.\footnote{Accounts are legion. See, for example, Katzmann (1981) on how young attorneys flocked to the Federal Trade Commission to gain experience in anti-trust and consumer cases so that they could move into lucrative positions in private law firms after a short time.} The authors also assume that the private sector wage is always greater than the agency wage—which gives all slackers incentives to leave the agency very quickly (with zealots staying on), a cataclysmic result that influences the entire analysis. Yet such a blanket (and radically consequential) assumption is unwarranted; for the private sector wage advantage is more myth than reality, especially when benefits (e.g., health and retirement) are taken into account.\footnote{Compensation across sectors, although bundled differently, is sufficiently comparable in total value that there is considerable debate over which sector has the advantage. See, for example, Federal Reserve Bank of Chicago (2009).} It also ignores attitudes toward risk, as well as the likelihood that government jobs may selectively attract many people (including slackers) who are risk averse and willing to accept a lower government wage in return for greater job security (Brehm and Gates 1999).

These are serious problems that raise red flags about this particular model. But as a general avenue of inquiry, the Gailmard and Patty (2007) analysis is among the most innovative in the literature. It helps point the way toward a better understanding of how bureaucratic expertise may develop as an integral part of the nexus of decisions involved in delegation. And it goes further, suggesting how key components of the civil service system—such as tenure—might arise from the politics of delegation, be rational and productive for the players involved, and contribute to an explanation of bureaucratic expertise. Although the specifics of their model can be questioned, then, it encourages new work that connects delegation to the internal organization of the bureaucracy. And it gets back to the kinds of issues that were central to the control literature years ago, before formalization shifted the focus to discretion.

6.3. Bureaucratic Capacity

The usual delegation model is built around the information problem and thus around the legislature’s reliance on an expert bureaucracy to carry out policy. It is the bureaucracy’s expertise that makes it distinctive and drives much of the analysis. But Huber and McCarty (2004) rightly argue that there is another aspect of the bureaucracy—its capacity for effective performance—that deserves central attention as well. Implicitly, the delegation literature has assumed that capacity is not an issue and that all bureaucracies are highly capable. Yet even though agencies may have considerable expertise about their policy environments, they may also be quite incapable—due to mismanagement, corruption, or patronage, among other things—of carrying out policy effectively. Expertise and capacity are simply different dimensions, and both are likely to be important for understanding delegation.

The model Huber and McCarty develop is similar to the others in basic respects. The authors use the agency discretion framework, treat bureaucratic expertise in the usual way (with policy
outcomes partly determined by random shocks that the agency knows with certainty), and—following Huber and Shipan (2002)—assume that the agency is caught and punished with some probability if its implemented policy falls outside the discretionary region. The difference here is that the agency’s implemented policy consists of two parts: an action $a$ chosen by the agency and a random adjustment $w$ that reflects its capacity for effective performance. Unlike the random shocks associated with expertise, the agency does not know $w$. But it does know the variance of $w$’s distribution. The larger the variance, the lower the agency’s capacity for effectiveness, and the farther the implemented policy is likely to depart from the agency’s chosen action $a$.

Given the literature’s implicit focus on high-capacity agencies, Huber and McCarty center their analysis on those that have low capacity. They begin by showing that low-capacity agencies, because their own actions have so little impact on implemented policy (stemming from the high variance of $w$), actually have little control over their own fates—and this reduces their incentive to even try to comply with legislative policy directives, making them harder to control. This implication is interesting, but they go on to derive conclusions that are even more instructive. For instance, they show that the ally principle, long at the theoretical core of the delegation literature, tends to fall apart once capacity is taken into account. The legislature can often obtain better policy outcomes by delegating to competent agencies with distant political preferences than to low-capacity agencies that are closer by.

They also derive much more general implications for institutional reform. They show that, although politicians always benefit from reforms that enhance bureaucratic capacity, their incentives to engage in such reforms are weakest in systems populated by low-capacity agencies. In these environments—which appear in some areas of the United States but are quite common in Latin America and other developing regions (Geddes 1994)—politicians have reduced incentive to develop their own policy expertise, reduced incentive to bolster institutions (such as courts) that would improve bureaucratic compliance, and enhanced incentive to politicize the bureaucracy: all of which create a drag on reform and tend to keep the system in an incompetency trap.

Overall, this is a remarkable analysis. Bureaucratic agencies are the government’s means of carrying out public policy, and their capacity for effective performance is clearly central to how any rational politician, interest group, or citizen would go about understanding them or assessing their value. Yet the delegation literature has focused all its attention on the information problem and brushed capacity aside. Huber and McCarty have now put a spotlight on it and shown that it does indeed have far-reaching theoretical consequences. In so doing, they have taken a key step toward moving the theory away from its American origins and making it truly comparative in scope, because low-capacity bureaucracies are a serious problem throughout the world. This analysis not only sheds new light on these non-American contexts, but it also shows that the logic of delegation—and reform—may work quite differently in such settings than scholars had previously thought.

7. Discussion

The theory of public bureaucracy has made dramatic progress over the past few decades. From the pioneering work of Downs and Tullock to Niskanen’s theory of the budget-maximizing bureau to the more recent theories of political control and delegation, the field has put to-
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Together a sophisticated analytic base for understanding how the organization and performance of bureaucracy arise out of politics. The modern literature is defined by work that is formal and game-theoretic, and its trajectory has propelled it from the realm of early breakthroughs into the realm of normal science. Modelers in the field now see themselves as building incrementally on a common analytic foundation, and they agree on what good work looks like.

The most influential achievement is the analytic foundation itself, whose rigor has allowed this literature to move beyond informal argument to deductively powerful theories with testable implications. The most basic of these implications—the ally principle, the uncertainty principle—are the cornerstones of the modern field and central to how the dynamics of delegation are understood. But the literature has also expanded its horizons over time, as scholars have sought to incorporate a range of actors and topics—presidents, courts, appointments, bureaucratic capacity, legislative capacity, bicameralism, the endogenous development of agency expertise—that are clearly relevant to the politics of delegation and demanding of attention. Today’s theory, as a result, is a less legislature-centered and more broadly institutional approach that explores key features of the American separation of powers system and extends to parliamentary governments as well. Along the way, the legacy of congressional dominance has weakened, as formal models have clearly shown that legislators face a trade-off between control and expertise, that it is rational for them to accept imperfect control, and that agencies should often have substantial autonomy.

All literatures have their problems, and this one is no exception. Despite the expansion of the theory, the legislature is still the center of its political universe, and other institutional actors—the president, the courts, even the bureaucracy—are given far less serious attention than they deserve. Consider the president. It is not unusual for delegation models to ignore his veto power—even though the president clearly benefits from agency discretion and can reject legislation that does not provide enough of it. These same models typically assume that the agency has policy preferences identical to the president’s, implicitly claiming that he has no problem getting the agency to do his bidding—which is far from accurate and diverts attention from the fact that, as chief executive, the president has his own problems controlling the bureaucracy. Some of these problems arise because Congress is trying to control it too. But many arise simply because top-down control is highly imperfect in any hierarchy, including the executive branch.

18. Bendor and Meirowitz (2004) explore the robustness of the theory of delegation by examining how well its better known implications stand up as fundamental assumptions are varied. They show that the ally principle stands up quite well, but that (not surprisingly) it does not hold under all conditions. For example, when information varies across bureaucrats—which can happen when bureaucrats are not perfectly informed, information is costly, and some specialize more than others—then political superiors may pass up an incompetent ally for a competent agent whose ideal point is farther away. To take another example, they show that, if agents can precommit to particular policies—which is precisely the point, for instance, of setting up an independent central bank (a bureaucratic institution) for monetary policy—then superiors may find it advantageous in some cases to delegate discretion to bureaucrats whose ideal points are not close to their own. See also Bendor et al. (2001). As for the uncertainty principle, Bendor and Meirowitz show that it is a robust conclusion and that the value political superiors place on (bureaucratic) expertise has little to do with attitudes toward risk—the superiors do not, in particular, have to be risk averse for the principle to hold. The decision to delegate is mainly driven by their desire to achieve policies close to their ideal, and by other factors (e.g., the variance of the random shock) that are separate from attitudes toward risk.
Both give presidents strong reasons to use his powers—of the veto, of appointments, of unilateral action—to impose structures that enhance his own control. Indeed, much of the organization of the federal bureaucracy was created by presidents or their appointees, not Congress (Howell and Lewis 2002; Lewis 2003). A theory that aims to explain the organization of public bureaucracy, then, needs to treat presidents as the pivotal actors they are, not as a planet orbiting the legislative sun. 19

The courts have also been marginalized. If they are incorporated at all, they tend to be treated as faithful backstoppers of the legislature, beefing up its power. 20 But judges clearly have preferences of their own, they act on them, and there is a large empirical literature on judicial behavior that documents as much (e.g., Segal and Spaeth 2002). Like presidents they can take unilateral action, without legislation, to impose their own structures on the agencies. And like presidents they have serious control problems to worry about and address: for they cannot count on having agencies faithfully obey their written orders, or indeed obey them at all. Ultimately, then, this literature needs to incorporate all the major institutional actors as (roughly) co-equal players and model the dynamics of their interactions. The legislature, the president, and the courts all have authority over the bureaucracy; they all have problems controlling it; and they all are involved in imposing structures to engineer more desirable behavior and policy outcomes. This is what the theory ought to be about, not just delegation by legislatures.

Even though the legislature is the star player in this literature, it has actually been explored in surprisingly little depth. Congress is a complex institution with two houses, two parties, huge numbers of committees (several of which may be relevant to any given decision), and hundreds of members driven by parochial interests; and it is plagued by severe transaction costs when it attempts to take action. As such, it is not an actor in the same sense that the president is. Yet despite the centrality of transaction costs to the new economics of organization, and notwithstanding Epstein and O’Halloran’s claim to have developed a transaction costs theory of delegation, Congress is modeled as a coherent actor that instantaneously, smoothly, and flawlessly makes optimal decisions in pursuit of its well-defined policy objectives. 21 By uncritically going down this modeling path, the formal literature fails to recognize Congress’s nature as an institution and is out of sync with what are supposed to be its own theoretical origins. Theorists ought to be trying to model the very ponderousness and decisional difficulty that make Congress what it is—and that cannot help but shape the larger struggle for control.

19. For a recent attempt to explore explicitly presidential effects on the politics of delegation, see Wiseman (2009). Wiseman argues that, when the president can engage in review and adjustment of agency decisions subsequent to delegation—notably, through the regulatory review activities of the Office of Information and Regulatory Affairs (OIRA)—both the president and Congress can actually be better off. According to Wiseman, this reasoning helps explain why OIRA continues to survive.

20. This is true of the delegation literature (Huber and Shipan 2002), but it is also true of the work on ex post control. The courts are portrayed as Congress’s helpers. See Ferejohn and Shiplan (1990) and Eskridge and Ferejohn (1992).

21. Epstein and O’Halloran (1999) begin to get at these internal decision problems in their make or buy framework, and Huber and Shipan (2002) do so by recognizing variations in legislative capabilities, but these are not attempts to model the truly burdensome transaction costs that afflict Congress, and they only scratch the surface. I should add that legislatures in parliamentary systems are much less burdened in this respect—a variation of theoretical consequence that ought to be central to this literature.
And then there is the bureaucracy itself, which, despite its universal role as the agent in these analyses, has actually received very little attention. The early work on ex ante control—by McCubbins, Noll, and Weingast, Moe, and others—was explicitly about bureaucratic organization. It argued that legislatures (and presidents) used various aspects of organization—the location of the agency, its decision criteria, its decision processes, appointments, personnel rules, rules for appeals, interest group participation, reporting requirements, and more—to gain control over agency behavior. My own work, moreover, also put the focus on bureaucratic effectiveness and how it tends to be undermined by politically imposed structures. But these organizational concerns are less central today. With the recent formalization of the theory, the focus is on how much discretion the bureaucracy is delegated. Various aspects of bureaucratic organization are still discussed—informally—as the means by which discretion is restricted. But they are not explicitly modeled, and they are not explored in terms of the different effects they might have on bureaucratic behavior. They are essentially lumped together into one homogeneous mass, as the structural means by which discretion is restricted. This characterization does not tell us much about why agencies are organized as they are, nor about how their politically imposed structures affect their capacity for effective performance. For now, the organizational aspects of bureaucracy have gotten organized out of the formal theory.  

It seems clear that, with the focus and momentum that normal science imparts to a research community, the theory will naturally be expanded and improved over time along most of the dimensions I have just discussed. So in this sense, progress is inevitable. Yet normal science is constraining as well as empowering, because its consensus on analytics inherently produces a research dynamic that pushes the theory in “normal” directions—and pushes it away from other avenues that, even if potentially productive, are not compatible with the accepted way of thinking about things. Over time, as the field is guided by this framing, a kind of path dependence sets in that makes certain kinds of progress more difficult to achieve. An obvious example is the one I just discussed: precisely because this literature is now built around the concept of discretion, all aspects of bureaucratic organization get telescoped into this one concept, propelling inquiry in directions that are less about organization than they might otherwise be.  

Another troubling constraint built into the normal framing, an exceedingly consequential one, is that actors are not forward looking. It has long been part of the working knowledge of the field that, because tomorrow’s power holders may be very different from today’s—different policy agendas, different ideologies—rational actors in a position to design bureaucratic agencies have incentives to look ahead and take this political uncertainty into account. The greater the likelihood their enemies may gain power in the future, the more they will have incentives today to adopt restrictive structures that shield “their” agencies from unwanted influences. But even though this forward-looking logic could not be more basic to rational behavior and is often discussed, it is simply omitted from formal models of delegation and control. Among those doing the modeling, this omission is not regarded as a problem, or even an issue worth pointing out. It is normal.

22. Some of Ting’s (2002, 2003) work, however, links delegation to issues that are explicitly organizational—exploring, for example, how superiors might try to use redundant bureaucratic agents (multiple agents doing the same tasks) to obtain better outcomes and increase effectiveness (Ting 2003), and how superiors might be able to obtain better outcomes by assigning different jurisdictions to different agencies (Ting 2002).
In essence, then, existing models assume that their actors behave in rather stupid ways. They are fixated on the present, as though politics is never going to change. When the legislature makes its delegation decisions with an eye to the president—assuming, for instance, that he alone determines the agency’s policy preferences—it is looking only at the current president and ignoring the fact that he will soon be out of office, replaced by another person. And another and another. If legislators want to maximize the long-term benefits to themselves and their constituents, it is obviously a big mistake for them to base their entire calculation on the sitting president alone. Would it have made sense, in 2007 and 2008, for the Democratic Congress to make its delegation decisions under the assumption that the only president of relevance was George W. Bush, when they knew for certain he would be gone by January 2009? Of course not. But this is just what the existing models of delegation assume. And because it is normal, no one thinks twice about it.

This problem is pervasive and carries over to the literature’s attempt to build a comparative theory of delegation. Parliamentary governments are not all alike. But it is surely the case that, in most of them, there are far fewer veto points than in the American system; it is much easier for the government to pass new laws; and because of this, the current government needs to be very concerned that its creations will be undermined or destroyed when the opposition

23. To be more accurate: legislators do think ahead, in the sense that they need to ponder what agencies are likely to do with whatever discretion they are granted. But legislators do not think ahead in other basic respects, for example, by recognizing that the sitting president and the agency’s presidential appointee are temporary and will be replaced by others with perhaps very different policy preferences. Also, legislators do not think ahead with regard to themselves: today’s legislators need to be concerned about what will happen down the road if opposing legislators grab the reins of power and take control of the agencies being created today. In short, existing models allow the players to be forward looking about agency compliance and shirking, but not about the structure of power and control.

24. The most notable exception—outside of one attempt by Epstein and O’Halloran (1994) to model political uncertainty—is the formal treatment given to the concept by de Figueiredo (2002). His analysis is not about delegation per se but about whether interest groups will favor overturning one another’s policies when their party gains power, and whether they will insulate their agencies (and suffer the inefficiencies that might go along with it) in separation-of-power systems that allow such restrictions to endure. He argues that insulation is pursued by weak groups, not strong ones, in separation of powers systems and that groups often respond to political uncertainty in parliamentary systems by cooperating and thus agreeing not to overturn one another’s policies. He also argues that, in general, political uncertainty works somewhat differently than I have suggested in my own work and is less likely to produce insulation and bureaucratic inefficiency. This is not the place for me to write out a response, needless to say. I do think that there is much to agree with in de Figueiredo’s basic conclusions about the effects of political uncertainty, although he has a tendency to couch them in more extreme terms than is warranted. Despite his categorical claims about the model’s clear-cut implications, there is actually no clear, definitive way to translate some of his mathematical results into statements about the empirical world—because it involves, for example, interpreting what it means for a given parameter to be “sufficiently high.” My own view is that softer versions of his stated conclusions are more likely on the mark: that weak groups have greater incentives to insulate than do strong groups, and that groups in parliamentary systems are sometimes able to cooperate in not overturning one another’s policies. These conclusions are entirely compatible with my own logic (e.g., Moe and Caldwell 1994). More generally, however, the main point to drive home here is that de Figueiredo’s model is just one of many ways that political uncertainty might be modeled—and other reasonable approaches would surely lead to different conclusions. The bottom line is that more scholarly work is needed on political uncertainty. It is a concept that—unless we want our actors to behave stupidly—should be taken seriously, integrated into our models, and debated.
comes to power. Simply burying agencies in formal restrictions does not work very well, as such restrictions can be lifted by the next government. American lawmakers need not be nearly as worried, because they are protected by all the checks and balances built into their system: whatever they create and embed in formal structures will tend to endure. Thus, when actors look forward, the logic of delegation is clearly very different across these political systems. Yet the existing literature does not allow them to look forward, and it entirely ignores this fundamental source of cross-system difference. The same is true, interestingly enough, of recent parliamentary analyses that are entirely informal, and thus are not constrained by the need to simplify and mathematize their key ideas. They are very informative in describing how delegation operates in these systems, how linear and hierarchical it is compared to the American case, the key role of parties, and the like—but they pay no attention to political uncertainty and its crucial implications, staying very much within the normal science bounds (Strøm et al. 2003).

Finally, I want to mention one more aspect of the normal framing that I find especially important but have not discussed at all to this point—because it is completely missing from this literature. The normal assumption is that bureaucrats are subordinates in the hierarchy of government, pure and simple. In terms of public authority, of course, this is accurate and makes perfect sense. But it systematically diverts attention from a feature of democratic politics that has clear relevance to control and delegation. In a democracy, political superiors are elected; and this means that, if ordinary bureaucrats can get organized to take collective action in politics—through public sector unions, for example—they can exert political power over their own superiors. When this happens—and it regularly does, in the United States and throughout the developed world (Blais et al. 1997)—they can play major roles in determining who gets elected and what policies the latter pursue once in office. This being so, the kind of “control” that political superiors want to exercise may often—depending on the specific group and the power it wields—tend to favor bureaucratic interests: by favoring policies and structures, say, that promote job security, enhance autonomy, protect established programs, and lead to higher spending and taxing. In contrast, the normal way of thinking about bureaucrats assumes that they have no political power at all: bureaucrats are difficult to control, and have a measure of power, because they have informational leverage over their superiors. They are experts. The idea that they may also have political power and that, in some realms of behavior, politicians may actually be agents of the bureaucrats—and acting as such in their delegation decisions—is entirely foreign and never seriously considered. As a result, an important part of the delegation story is missed, and theorists tend to underestimate what bureaucrats can do to get their way (see Moe 2006).

8. Conclusion

There is reason, then, for both enthusiasm and concern in assessing the contributions of formal models to the study of public bureaucracy. Tremendous progress has been made, and along the way one of the most traditional and underdeveloped fields in all of political science has been transformed into a juggernaut of cutting-edge work. In important respects, the trajectory continues to look promising, and will likely take the theory further beyond its legislature-centered origins to capture some of the distinctive dynamics of separation of powers and parliamentary systems. But just as formal models are powerful tools in the development of theory, so they are
also powerful means of constraining it. As their influence has taken hold, they have generated a normal science that inherently mobilizes research around certain sets of ideas—and discourages the pursuit of others, including some that are quite important to an understanding of control, delegation, and bureaucracy.

The real challenge for scholars in the years ahead, therefore, is not simply to push for incremental progress along the same trajectory. It is to push against the constraints, to be open to abnormal ideas, and to think actively about shifting the theory onto new paths that might be more productive.

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