Political Control and the Power of the Agent

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In the study of public bureaucracy, the relationship between political authorities and bureaucrats is commonly understood as one of principal and agent, and analysis centers on how the authorities can try to overcome the information asymmetry at the heart of this relationship—arising from the bureaucrats' expertise and other private information—to exercise a measure of control over their subordinates. In this standard view, information is the source of bureaucratic power. There is a second basis of bureaucratic power, however, that the literature overlooks. Precisely because the authorities are elected, bureaucrats can take political action—especially if organized by public sector unions—to influence who gets elected and what choices they make in office. This gives bureaucrats a political capacity to control their own controllers, putting the usual principal-agent relationship in a very different light. The purpose of this paper, then, is to make a case for the political power of the agent, and to argue for a reorientation of the current theory. In the empirical analysis, the argument is applied to the electoral behavior and impact of public school teachers and their unions—showing that they are quite active and influential in choosing the key authorities that are supposed to be governing them.

1. Introduction

In the study of public bureaucracy, the relationship between political authorities and bureaucrats is commonly understood as one of principal and agent. The authorities are the principals, the bureaucrats are the agents, and analysis centers on how the authorities can overcome the information asymmetry at the heart of the relationship—arising from the bureaucrats' expertise and other private information—to exercise control over their subordinates. Control cannot be perfect, because the informational advantage gives bureaucrats the power to engage in some measure of noncompliant behavior. But even so, the authorities can devise rules, incentives, and monitoring mechanisms that help to mitigate these problems and keep the bureaucrats more or less in line (e.g., Weingast, 1984; McCubbins and Schwartz, 1984; McCubbins et al., 1987).

This now-standard view of public bureaucracy is surely right to emphasize that information problems are central to an understanding of political control. And it is right to recognize that information is a key source of bureaucratic power. But it fails to recognize that when we are dealing with public

organizations in democratic contexts, as is usually the case, the principal-agent relationship is inevitably open to—and may be profoundly shaped by—a second kind of bureaucratic power that is distinctly different from information power.

In democratic systems, the political authorities are elected—and this simple fact means that even the most ordinary bureaucrats, by taking collective action in elections and other realms of politics, may be able to exercise *political* power in determining who their bosses are and what choices the latter will make in office. To the extent this occurs, the principal-agent relationship is fundamentally changed. Principals still face information problems in trying to control their agents. But when agents have a measure of political power over them, the principals may not *want* to exercise much control, and may make choices—on policy, on structure, on funding—that are much more favorable to the agents than the theory now recognizes. There may be a real question as to who is controlling whom.

In the analysis that follows, I have two aims. One is to make a case for the political power of the agent, and thus for a reorientation of the current theory to provide a more balanced understanding of control relationships in the realm of democratic government. The second is to carry out an empirical analysis that sheds light on how bureaucrats take part in the electoral process to choose their own political principals. My focus here is on the most common form of public agency in American government, the public school; on the school system's key bureaucrats, its teachers; and on the elected authorities of greatest relevance to teachers' jobs and workplaces, the members of local school boards.

2. Agent Power and the Theory of Political Control

In content, the theory of political control is about the substance of politics. Most of it deals with the efforts of legislators—and in the background, political interest groups—to control the behavior of bureaucratic agencies (e.g., Huber and Shipan, 2002). But while the substance is clearly political, much of the logic is rooted in the new economics of organization, which first emerged during the 1970s as economists began to explore the organization of business firms (Moe, 1984; Williamson, 1985). The tools economists developed for dealing with these issues—principal-agent models among them—were quite general, and it wasn't long before political scientists began putting them to use in developing their own theories of public organization. In the years since, as a result, political scientists have structured their thinking about political control, and indeed about political institutions generally, in virtually the same way that economists have structured their thinking about economic organization.

In reviewing this new line of economic theory, which they label the theory of incentives, Laffont and Martimort (2002:3) describe its core elements as follows:

The starting point of incentive theory corresponds to the problem of delegating a task to an agent with private information. This private

information can be of two types: either the agent can take an action unobserved by the principal, the case of *moral hazard* or *hidden action*; or the agent has some private knowledge about his cost or valuation that is ignored by the principal, the case of *adverse selection* or *hidden knowledge*. Incentive theory considers when this private information is a problem for the principal, and what is the optimal way for the principal to cope with it.

The principal-agent framework captures an essential feature of work organizations, and indeed of a broad range of hierarchical relationships: that someone needs a task carried out, lacks the expertise or time to do it out personally, and delegates the task to an agent. This primordial act of organization comes with a built-in control problem: for the agent has expertise and other information—about his own diligence and aptitude, for example, or his actual behavior on the job—that are largely unavailable to the principal, and this asymmetry makes it difficult for the principal to ensure that his own interests are being faithfully pursued by the agent.

What can the principal do to overcome information problems? This is what the theory is all about. And what it typically points to are structural solutions—rules, incentive systems, monitoring mechanisms, and the like—that, by creating and filling out the basic features of organization, operate to constrain the agent's behavior in desired directions. It is *because* principals need to overcome information problems and control their agents' behavior, in other words, that organizations emerge and take the structural forms that they do. A theory of control, then, is much broader in scope than its name would seem to suggest. It is really a theory of organization—anchored in problems of information.

The earliest attempts to develop a theory of control in the realm of government were centered on legislative control of the bureaucracy. But they were narrowly concerned with control per se—with legislative oversight, for example, and the manipulation of rewards and sanctions—and did not explore the broader implications for organization (Weingast, 1984; McCubbins and Schwartz, 1984). The first to take this broader view were McCubbins et al. (1987), who argued that organizational design is itself the most powerful means of political control. They "begin with the premise that the political control of agencies is a principal-agent problem" (243) and, true to their analytic moorings, are clear in asserting that the legislature's control problem is fundamentally due to information:

> The crime of runaway bureaucracy requires opportunity as well as motive, and this is supplied by asymmetric information. A consequence of delegating authority to bureaucrats is that they may become more expert about their policy responsibilities than the elected representatives who created their bureau. . .As in all Agency relationships, it may be possible for the agency to take advantage of its private information. . .The challenge for the political overseer is to prevent these outcomes. (247)

They go on to argue that the "administrative procedures" so dear to the legal community are not explained by public-interest concerns, but are mechanisms by which legislators control the bureaucracy. In particular, they argue that by imposing rules, decision procedures, appeals processes, and the like on public agencies—and thus by strategically designing agency structure—legislators can compensate for the agencies' informational advantages, channel behavior in desired directions, and ensure the flow of benefits to constituents.

These ideas led to the growth of an unusually productive literature that has shed light not only on political control per se, but on larger issues of bureaucratic origins and structure, democratic accountability, the durability of public policy, and governmental effectiveness (e.g., Bawn, 1995; Epstein and O'Halloran, 1999; Horn, 1995; Huber and Shipan, 2002; Moe, 1989; Wood, 1988). Not all of these works are explicitly couched in principal-agent terms, but its framework for thinking about political control is at least implicit in virtually everything written on the topic. And in the two decades since its logic first took hold, its influence has been pervasive—ensuring that problems of information remain at the center of scholarly inquiry. In a recent assessment on the literature, Huber and Shipan (2002) put it this way:

> The principal-agent framework from economics has played an extremely prominent and powerful role in this institutional approach to relations between politicians and bureaucrats. The central thrust of this approach has involved developing maps from particular types of informational problems to the best possible institutional solutions. (26)

As it now stands, the theory of political control is political only in the sense that its actors and contexts are political. Its logic is generic, highlighting problems (of information) that readily apply to *any* actors in *any* contexts but ignoring aspects of government that make control relationships in the public realm truly distinctive. In particular, it ignores the crucial fact that the principals are elected—and thus that if bureaucrats are able to exercise power through the electoral process, they can help determine who their principals are and what objectives the latter pursue in office. The more electoral power the bureaucrats are able to wield, the more their principals have incentives to act as "agents of the agents" by doing what their subordinates want them to do. When this is so, all the basic outcomes of top-down political control—the structure of agencies, their levels of funding, their personnel systems, the range of acceptable performance—are likely to be much more favorable to public employees than the standard principal-agent framework would lead us to expect.

This critique of the standard model is consequential only to the extent that bureaucrats actually wield power in elections. But there is an obvious barrier to their doing so. Rank-and-file bureaucrats can exercise power in elections only if they are able to coordinate their behaviors and resources as a group—yet to succeed in this, they somehow need to overcome all the vexing problems associated with collective action, including incentives to free-ride. From a logical

standpoint, this opens up a set of issues that an expanded theory of political control would want to pursue. From an empirical standpoint, however, it is clear that a large percentage of public bureaucrats have *already overcome* their collective action problems—because they belong to, and are forcefully represented in politics by, public sector unions.

Economists still argue about how these unions managed to get established, although it seems clear—for the United States—that changes in collective bargaining laws had much to do with it (e.g., Freeman, 1986). Whatever the explanation, unions in the U.S. public sector achieved explosive membership growth from the 1960s through the early 1980s and have since equilibrated at about 37% of the total public workforce—19% at the federal level, 30% at the state level, and 43% at the local level. Meantime, private sector unions have been in steep decline, dropping from more than 30% of the private, non-agricultural workforce in the early 1950s to about 8% in 2003 (Troy and Sheflin, 1985; Hirsch and Macpherson, 2004). It is the public sector unions, not the private sector unions, that are now driving the American labor movement (Troy, 1994; Stern, 1988).

Political scientists have rarely studied public sector unions. But the major organizations—among them the National Education Association (NEA), the American Federation of Teachers (AFT), the Association of State, County, and Municipal Employees (AFSCME), the Service Employees International Union (SEIU)—are active players in national politics, and they appear to be very powerful (Masters, 1988; Johnson and Libecap, 1994; Troy, 1994; Lieberman, 1997). Their power is probably even greater at the state and local levels, for this is where most government programs are carried out, most government workers are employed, and higher percentages are unionized; there also tends to be less competition from other groups in these settings (Lieberman, 1997; Troy, 1994; Thomas and Hrebenar, 2004).

The power of public sector unions is not restricted to the United States. They are forces to be reckoned with in any society that allows public workers to organize, and their capacity for power grows with the size of government. Bigger government means more public employees, which in turn means more members and resources for public sector unions—and more trouble for political control. The dynamics, in fact, are likely to be self-reinforcing. As growing government generates greater power for public sector unions, the latter have incentives to push for still bigger government and to oppose efforts to cut back or restructure. The obvious application is to the welfare state, which clearly *has* given rise—in Europe, Britain, Scandinavia, the United States, and Japan—to active public sector unions that have pushed hard for higher budgets, taxes, and levels of public employment, and in recent decades have strongly opposed reformist attempts to reduce the size and scope of government (Treu, 1987; Ferner and Hyman, 1992; Blais et al., 1997).

The current theory of political control is constructed as though none of this has ever happened—as though bureaucrats are atomized and powerless except for the private information that works in their favor, and as though

their principals are imposed on them exogenously and come with their own independent objective functions. By missing the electoral connection between politicians and bureaucrats, the theory threatens to vastly understate the ability of bureaucrats to turn control relationships to their own advantage and have their own interests embedded in the structure, funding, and operation of government. In many political contexts, then, particularly those in which bureaucrats are forcefully represented by public sector unions, the theory is likely to be systematically wide of the mark—and overly optimistic about the prospects for meaningful democratic control of the bureaucracy.

Two conclusions follow. One is that the prevailing theory needs to be broadened to recognize the electoral connection between principals and agents that is clearly so fundamental to government. The second is that there needs to be a serious, sustained research agenda on the political power of bureaucrats—on the extent of their power, how they get it and exercise it, how it varies with context and level of government, and what they receive in return. At present there is a huge gap in our knowledge on these scores, and we need to do something about that before we can understand rank-and-file bureaucrats—and their unions—as genuinely political actors.

3. Research Literature

When scholars have studied the political power of bureaucrats, most of their attention has centered on bureaucratic leaders (Carpenter, 2001). The closest thing to an empirical literature on rank-and-file bureaucrats is a rather small set of studies, most of it in the public choice tradition, that explores their voting behavior and political attitudes. This work first emerged at about the same time as the political control literature, but it developed quite separately and has had no impact on the latter's theories or claims.

Motivated by the early public choice concern for big government, these studies typically argue that bureaucrats have job-related incentives to vote, push for higher public spending, and support parties of the left. Their focus, however, is largely on the turnout of public bureaucrats in general elections. They show that bureaucrats do turn out at higher rates than ordinary citizens (e.g., Bush and Denzau, 1977; Wolfinger and Rosenstone, 1980; Frey and Pommerehne, 1982; Bennett and Orzechowski, 1983; Corey and Garand, 2002). In the 1996 presidential election, the differential was about 12%, most of which remains after controls for demographic and attitudinal factors (Corey and Garand, 2002). These studies also suggest, although the evidence is not consistent, that bureaucrats may differ from ordinary citizens in political knowledge, interest, efficacy, trust, support for government spending, and support for parties of the left (e.g., Blais et al., 1991; Garand et al., 1991; Corey and Garand, 2002; but cf. Lewis, 1990).

These studies are steps in the right direction, but they are limited in important respects. They analyze bureaucrats as individual voters, and typically ignore the role of unions. They often see bureaucrats as a single generic type,

without distinguishing specific occupations with specific interests that may differ considerably from one another.[1] They tend to focus on national elections, ignoring the state and local elections that may be far more relevant to the jobs of most public bureaucrats. And they focus on turnout, with little attention to the impact of bureaucrats and their unions on electoral outcomes. There is a second literature of relevance as well, this one dealing with whether labor unions (of all types) can successfully get their members to the polls and get them to vote for favored candidates. From these studies it is clear that union members do tend to vote at higher rates than other citizens in national elections. From 1980 to 1998, the median differential was 9.5% (Asher et al., 2001:136; also Delaney et al., 1988; Sousa, 1993; Leighly and Nagler, 2005). It is also clear that union members are more likely than other citizens to vote Democratic, and that endorsements can affect their votes (Sousa, 1993; Miller and Shanks, 1996; Asher et al., 2001). There is evidence, however, that both the turnout gap and the tendency to vote Democratic are eliminated when strength of party identification and other factors are controlled (Sousa, 1993; Asher et. al, 2001; Miller and Shanks, 1996).[2]

This literature has some of the same drawbacks as the one on public sector employees. Notably, unions and their members tend to be viewed generically, the focus is typically on national elections, and little attention is devoted to the fact that different types of unions and members have different incentives at different levels of government (Asher et al., 2001, do provide some data on Ohio state elections). As a result, it doesn't necessarily tell us much about what unions do and how successful they are in the elections they care about most. And it has little to say about public sector unions in particular.

4. Teachers and Their Unions

Because so little is now known about the political power of bureaucrats, there are countless opportunities for research. Work needs to be done on both American and non-American settings, at state and local levels as well as the federal level, and across a range of policy areas. Needless to say, major progress will require many studies by many researchers over a long period of time. My aim here is simply to contribute to this larger effort.

The best way to learn something about the political power of bureaucrats is to focus on specific bureaucrats in the specific elections that bear directly on their occupational self-interests. That is what I do in the two studies reported below. Both are studies of public school teachers, and both explore the role that

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1. Wolfinger and Rosenstone's (1980) analysis is an early exception that shows where finer distinctions might lead. They find, for the 1972 election, that most of the turnout differential between public and private employees occurs among those with little education—involving public bureaucrats with very low level jobs—and is concentrated in a minority of states where patronage is still common and public jobs less secure. But this early result has never been replicated, pursued, or built upon by others.

2. It is possible that the unions themselves are responsible for strengthening the party ID of their members, but this has yet to be demonstrated. See the discussion in Asher et al. (2001).

teachers and their unions play in local school board elections—the elections of most immediate relevance to teacher jobs.

In the United States, teachers are the most numerous of all government bureaucrats, and they operate the most common form of government agency, the public school. There are some three million teachers nationwide—four out of five of them unionized—and more than 90,000 public schools.[3]

The teachers unions are formidable organizations. Collective bargaining is their core function and the bedrock of their well-being, because it is through collective bargaining that they gain members and resources, but there is a close and symbiotic relationship between collective bargaining and politics. This is obviously true at the local level, where, if the unions are politically active, they may be able to determine who sits on elected local school boards—and thus choose the very "management" they will be bargaining with. These same superiors will make decisions on a gamut of policy issues—from budgets to curriculum to student discipline—that teachers have a stake in (Lieberman, 1997; Moe, 2001).

Political power is hugely beneficial at higher levels of government too. Policies at these levels can make it easier for unions to organize teachers and gain leverage in negotiations. But more generally, the great value of higher-level politics is that state governments (and to a lesser extent the national government) can adopt virtually any rules, programs, and funding arrangements they want. When unions exercise political power at higher levels, then, they can achieve many objectives they might be unable to achieve through collective bargaining and can automatically achieve them for entire populations of districts.

Given the incentives, it is hardly surprising that the nation's leading teachers unions, the NEA and the AFT, are overtly political organizations at all levels of government. And they have an arsenal of weapons to back their interests. When it comes to political contributions, they regularly rank among the top spenders at both the state and national levels, and in many states they rank number one. Probably more important, they have millions of members, who are a looming presence in electoral districts throughout the country—and who, beyond their potential voting power, provide an army of activists who make phone calls, ring doorbells, and otherwise campaign to see friends elected and enemies defeated (Lieberman, 1997).

By all appearances, the teachers unions are quite powerful. Studies of the U.S. states have asked experts to rank interest groups according to their influence on public policy. Throughout the 1990s, the teachers unions came out number one on the list, outdistancing general business organizations, trial lawyers, doctors, utilities, bankers, environmentalists, and even state AFL-CIO

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3. For data on these and other aspects of the American education system (except for unionization), see U.S. Department of Education (2002). The unionization data are from the National Center for Education Statistics' 2000 School and Staffing Survey. Go to http://nces.ed.gov/surveys/sass on the Web.

affiliates. In the 2002 survey, they came out number two, outranked only by general business groups (Thomas and Hrebenar, 1999, 2004).

This overview is enough to paint the basic picture. Although teachers are "just" bureaucrats who work in the public schools, they and their unions seem to have all the ingredients for real political power and for exercising a measure of control over the public officials who are supposed to be controlling them. But it is one thing to have an impressionistic sense of teacher power and another to understand how much power they actually have, where it comes from, how it is exercised, and with what effects on policy and social outcomes. For this we need systematic research, and at present there is virtually none.

5. Teacher Power in School Board Elections

The first study targets a key issue that the literature simply hasn't pursued: to what extent are public sector unions able to influence the elections they really care about—and thus to choose their own political principals? We'll explore this issue by evaluating the success of teachers unions at getting their favored candidates elected to local school boards.

5.1 Data

The data come from California school board elections for the years 1998 through 2001. Basic information on each race—number of spots being contested, who the candidates were, how many votes each candidate received—was obtained from the League of Women Voters and county agencies. Information on union endorsements was more difficult to get, as it is not systematically recorded; when possible, it was collected from the unions themselves (and sometimes from candidates). These sources produced complete data for 245 district elections and the 1228 candidates who competed in them.

These 245 elections are likely to be a fairly representative sample of the California school districts that regularly have school board elections. They are not, however, representative of all California's 1000-plus school districts. Small districts often have uncontested seats and even have a hard time finding anyone willing to serve, presumably because the stakes are so small. As districts get larger and the stakes rise, elections are more often contested. As a result, the population of districts with contested elections is skewed toward larger districts.

Our own sample is representative of such an electoral population. While 61% of California's school districts have less than 2500 students, just 11% of our districts are this small. At the other end of the spectrum, while 6% of districts statewide have more than 20,000 students, 30% of our districts do. This skewing toward large districts may seem a bit much, but even more would be required if we wanted (which we don't) to match the distribution of California's public school children, for it turns out that the state's smallest 61% of districts enroll just 8% of the kids, while the largest 6% of the districts enroll a whopping 47%.[4]

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4. Data are from the California Department of Education CBEDS [California Basic Educational Data System] files.

If there is a bias to consider, it is not that the sample contains too many large districts, but that it contains only districts in which the unions are active enough to support candidates. When the unions are weak, then, elections might tend to be excluded from the data set, which could bias the results toward union strength. A survey of candidates, however, suggests that the unions make endorsements in the great majority of districts of any size; and when they don't, it is often because they are happy with all the major candidates (which is likely when they are strong, not weak). Even so, we can be cautious and think of the analysis as addressing a more limited question: How much impact do the unions have when they do support candidates (this survey is discussed in Moe, 2005).

6. Method of Analysis

On the surface, the method of analysis would appear straightforward. We have data on the dependent variable (percentage of the vote or win / lose) for each candidate in each election, and by using the standard regression or dichotomous-variable techniques it might seem we should be able to show whether union support has an impact on electoral outcomes. There are complications, however, that prevent such an easy solution. Most importantly:

1. Within each election there is a hard constraint. If the dependent variable is a candidate's percentage of the vote, this variable must sum to 1 across all candidates within each election. If the dependent variable is whether a candidate wins or loses, the number of wins must sum to the number of spots up for election.
2. The outcomes observed for candidates within any given election are not independent of one another, but are correlated. In part, this happens because each election has its own unique set of (largely unmeasured) influences that are essentially shared by its candidates but not by candidates in other elections. More consequentially, though, it happens because—as the hard constraint implies—a vote for one candidate is a vote that competing candidates cannot get, and a win by one candidate makes a win by the others either impossible or less likely.
3. The value of the dependent variable is bounded by 0 and 1.

Political scientists who study multiparty elections have devised special methods for handling similar situations, with two or more parties competing in multiple electoral districts. But these scholars are fortunate in having elections organized by parties, and their solutions turn on treating one of the parties as a base and transforming the data accordingly (Katz and King, 1999; Tomz et al., 2002). This option is unavailable in the present case, because there are no parties and there is nothing about candidates that would allow us to construct an appropriate base. This literature on multiparty elections is the closest methodologists have come to a general solution to the kind of estimation problem faced here. Lacking such a solution and left to navigate the world of second best, I have settled on an approach that seems reasonable under the circumstances.

It can be shown that, within a linear regression framework, a fixed-effects model—a model that includes a separate intercept for each election—automatically leads to estimates that meet the hard constraint on the dependent variable. That is to say, when the dependent variable is percentage of the vote, the estimated vote percentages for candidates automatically sum to 1 within each election; and when the dependent variable is win / lose, the estimated probabilities of winning automatically sum to the number of spots being contested.

The first step, then, is to use a linear regression model with fixed effects. The second step is to adopt a robust estimator of variance. Such an estimator does not make the usual ordinary least squares (OLS) assumptions, so it allows for the possibility that the errors might be heteroskedastic or correlated. And it is readily adapted to the situation where they are thought to be correlated within specified clusters (like elections) but not from one cluster to the next.

So this is the basic approach—a linear regression model with fixed effects, combined with a robust estimator of variance (with allowance for clustering)—designed to address the first two complications in the data.[5] The third complication, that the dependent variable is bounded by 0 and 1, is not explicitly addressed. It turns out to be of little consequence in the analysis, though, because only rarely are any predicted values out of bounds.

For the dependent variable, I will be focusing on whether a candidate wins or loses, rather than on the candidate's percentage of the vote. There is a trade-off in doing so, as the former is dichotomous and obviously less well suited to a regression framework, all else equal. But what we care about most here, and certainly what the unions care about most, is the ultimate outcome: winning or losing. Moreover, the percentage of the vote needed for victory can vary considerably depending on the number of candidates involved and the number of spots contested, so it may not be very meaningful to say that union support increases a candidate's vote by X%.

The drawback is that, in order to meet the hard constraint within each election, we need to stick with linear regression rather than shift to probit or logit. But this is not likely to be much of a problem. Unlike the usual OLS estimator, the robust estimator allows for the kinds of heteroskedastic disturbances associated with the linear probability model. And except at the extremes, a linear probability model can ordinarily be expected to give results consistent with those of probit or logit models.

There is a possible selection bias to consider. While union support may boost a candidate's probability of winning, the unions may also tend to support "good" candidates who are likely to win anyway, which would inflate estimates of union impact. We have no measures of candidate quality and cannot test or correct for this problem. But I suspect that the bias, if it exists at all, is not serious. School board elections are nonpartisan, and they tend to be

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5. Specifically, the analysis is carried out within STATA using the "areg" estimation command with the "cluster" option (which, in addition to allowing for clustering on elections, invokes the robust estimator of variance).

low-interest, low-information, low-spending affairs (Wirt and Kirst, 1997), so it doesn't take much to be a good candidate who can attract votes. In fact, unlike high-profile races—for state governor, say—in which major-party candidates divide the vote and minor-party candidates get 1% or 2% each, there are rarely school board candidates (in these data) who garner trivial portions of the vote compared with the winners. It is plausible to think that the real difference in school board elections is not between candidates who are inherently good or weak, but between candidates who have the resources to *make* themselves good and those who don't. And that, of course, is what union support is all about. Regardless of who the person is, the unions can offer the money, phone calls, and door-to-door campaigning needed to make her a candidate to be reckoned with. Thus, the selection problem is unlikely to be of much consequence in practice.

This said, we need not attach sole importance to the absolute magnitude of the union impact, because there is a benchmark that allows us to judge it in relative terms as well. For elections generally, it is well known that incumbency has an important impact on electoral outcomes (e.g., Ansolabehere and Snyder, 2002). Presumably, this is especially true in school board elections, where partisan cues are entirely missing, information is low, and incumbents are likely to have a big advantage. We can safely assume, moreover, that all incumbents are good candidates. Thus, we can gain perspective by comparing union impacts with the impacts of incumbency. Does union support appear to vault candidates into the heady electoral realm of incumbents—which would represent an impressive display of power—or does incumbency offer advantages at the ballot box that union support cannot come close to matching?

Here too there is a possible bias to be aware of. The unions may sometimes support incumbents who are likely to win, even if they don't like them much, in order not to antagonize them; and they may be more inclined to oppose unsympathetic incumbents when they are weak. There is some evidence to suggest that they do these things on occasion (Moe, 2005). Interviews with candidates, however, indicate that the unions are typically not shy about taking on incumbents they don't like (to the point of mobilizing recall campaigns). In our own data set, in fact, fully 46% of the incumbents running for reelection are *not* endorsed by the unions. If we assume that the unions have been at least reasonably successful at getting their favored candidates elected, it follows that many and perhaps most incumbents running for reelection are candidates the unions previously supported—which makes it all the more impressive that they refused to support 46% of the incumbents in the data set.[6] By this logic, it

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6. Two points of clarification. First, there is evidence to suggest that school board members sometimes become less sympathetic to unions as a result of their experiences in office—and that this is why the unions sometimes oppose incumbents they previously supported (Moe, 2005). Second, it is important to note that in this data set, the teachers unions failed to endorse 46% of incumbents in *contested* elections. When the unions are satisfied with "their" incumbents, and when the latter are not in danger of losing or are running uncontested, the unions may make no endorsements at all. Thus the real percentage of "unacceptable" incumbents is probably lower than this figure suggests.

seems likely that the unions oppose the great majority of incumbents they don't like, and thus that any bias is small.

7. Analysis

In the models to be estimated, each case represents an individual candidate, and candidates are clustered together based on the elections in which they are participating. The number of cases is thus 1228, the number of clusters 245. The dependent variable is a dummy for whether the candidate in question won or lost. On the right-hand side, there are fixed effects for each of the 245 elections, and the key independent variables are dummies indicating whether the candidate is an incumbent, whether the candidate is union supported, and whether the candidate is both an incumbent and union supported.

It would be best if we could include a host of other variables for control purposes—candidate quality, ideology, views on educational issues, and the like—but the data are not available. We do, however, have information on each candidate's gender, on whether the candidate has a Hispanic name, and on where the candidate's name appears on the ballot, and these variables are likely to prove relevant. In a low-information environment lacking the usual party cues—and most other cues as well—a candidate's gender, ethnicity, or ballot position could easily affect her vote total (e.g., Sniderman et al., 1991). Women voters who know little about the candidates, for example, may tend to favor women candidates. Hispanic voters who know little about the candidates may tend to vote for those with Hispanic names. And all voters who know little about the candidates may tend to vote for candidates whose names appear early on the ballot (Koppell and Steen, 2004). Because these cues are likely to prove more influential when other cues are lacking, moreover, it makes sense to think that they will have greater impact for candidates who are neither incumbents nor union supported (I'll refer to them as "independent" candidates) and who thus have little else to attract attention to themselves. Interaction terms are included in the model to account for this possibility.[7]

The results of the estimation are set out in Model 1 of Table 1. The estimated impacts of both incumbency and union support are huge. If we take as our baseline case a non-Hispanic male whose name appears late on the ballot (which simplifies the calculations), his probability of winning is estimated to increase by 0.47 if he becomes an incumbent, by 0.56 if he gets the union's support, and by 0.76 if he is fortunate enough to become a union-supported incumbent. Perhaps these effects shouldn't be too surprising. In a low-information setting where employee unions are often the only organized political force, incumbency and union campaign support could easily turn

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7. In measuring these variables, gender (woman) and ethnicity (Hispanic) were inferred from the candidates' names and thus should be considered good but imperfect measures. They are 0-1 dummies. Data on ballot position were obtained from the California Department of State. For our purposes, this variable is coded 1 if the candidate has a good ballot position—first in a race for one slot, first or second in a race for two slots, etc.—and 0 otherwise.

Table 1. Union Power in School Board Elections (SE)

	Model 1	Model 2
Incumbent	.472***	.469***
	(.073)	(.058)
Union support	.558***	.554***
	(.066)	(.049)
Incumbent × Union Support	−.271***	−.269***
	(.085)	(.076)
Woman	.000	
	(.050)	
Woman × Independent	.094	.094**
	(.064)	(.043)
Hispanic	.023	
	(.066)	
Hispanic × Independent	.144*	.166**
	(.082)	(.068)
Ballot position	.047	.042
	(.047)	(.029)
Ballot Position × Independent	−.013	
	(.065)	
N	1228	1228
R^2	.37	.37
Adjusted R^2	.21	.22

Statistical significance is indicated as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ from two-tailed t-tests. Dependent variable is win = 1, lose = 0. Analysis based on regression with fixed effects for each of 245 elections, robust estimator, and clustering on the elections.

out to be the main determinants of who wins. (Most knowledgeable observers, I suspect, would say that they *are* the main determinants.)

The estimates indicate that union support is at least as influential as incumbency, and may even be more influential (although the difference is not statistically significant). This is saying a lot. Moreover, despite the cautionary concerns about a possible upward bias, we may actually be understating the magnitude of union influence by looking only at its direct effects on election outcomes. We have to keep in mind that when unions get their candidates elected, the latter then become incumbents in future elections, and the power of incumbency works on their behalf to boost their probability of reelection. Thus, in addition to their direct effects on electoral outcomes, the unions influence electoral outcomes indirectly—by producing incumbents and harnessing the power of incumbency to their own advantage. The total impact of union support may thus be a good bit greater than these estimates suggest, caution notwithstanding.

The rest of Model 1 suggests that, if gender and ethnicity have effects on the vote, they probably do so only for the independent candidates who are neither incumbents nor union supported. The findings also suggest that, if ballot position has an effect, it seems to affect all candidates rather equally—which, if true, implies that even incumbents and union-supported candidates need to worry about where their names appear on the ballot. If we keep only the effects in Model 1 that appear consequential and estimate the more

parsimonious Model 2, the results firm up (the standard errors drop). The impacts of gender and ethnicity are now statistically significant, and they are of substantive importance as well: an independent candidate has a 9% greater chance of winning if a woman and a 17% greater chance of winning if a Hispanic. Gender and ethnicity do appear to serve as positive cues for certain voters. Ballot position is not quite as significant, and its impact is smaller. But if this impact is real, it applies to everyone and could sometimes mean the difference between winning and losing: a good ballot position boosts the chances of winning by 4%.[8]

So far, the analysis has been conducted entirely in terms of the characteristics of individual candidates. But it is clear that the electoral context may also have something to do with who wins and loses—and in particular, that it may affect the way that union support and incumbency operate to shape electoral outcomes. Consider the following variables:

1. *District size.* Incumbency and union support may have greater impact in larger districts: where voters stand to have less personal knowledge of the candidates, candidates need more resources to contact and mobilize voters, and the resources that go along with incumbency and unions can make a big difference.
2. *District partisanship.* Although school board elections are nonpartisan, union support may be more effective the more Democratic the district, because much of what the unions want—from bigger budgets to stricter work rules—is compatible with liberal politics.
3. *Off-Year elections.* Citizen turnout in off-year elections tends to be much lower than in on-year elections, and any group that is organized for political action should have an advantage—particularly if, like the teachers unions, it often has little organized opposition.

We can learn more about the roles of incumbency and union support, therefore, by testing to see if their influence varies with particular features of the electoral context. To keep things simple and easily interpretable, I proceed as follows. First, I use ordinal measures for each of the contextual variables, with *district size* varying from 0 to 3, *district partisanship* (% Democrat) varying from 0 to 2, and *off-year elections* a 0-1 dummy.[9] Second, while it is plausible

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8. This small but nontrivial effect is consistent with the findings of Koppel and Steen (2004), whose analysis suggests that appearing first on the ballot in local races is worth something less than 4% of the vote total, and affects the outcome in less than 10% of the cases.

9. More specifically, the variables are coded as follows. District size = 0 if enrollment is less than 5000 students; 1 if greater than or equal to 5000 but less than 10,000; 2 if greater than or equal to 10,000 but less than 20,000; and 3 if greater than or equal to 20,000. District partisanship = 0 if Democrats make up less than 50% of the district's two-party registration; 1 if Democrats are at least 50% but less than 60%; and 2 if Democrats are at least 60%. Off-year election is 1 for an odd election year, 0 for an even election year. I should note that were we to carry out the analysis using actual enrollment figures for district size and percent Democratic for district partisanship rather than the simplified categorical measures, the results of the analysis would have been the same. The simplified measures just make the results easier to interpret.

to think that each contextual variable may condition the effects of all individual-level variables in the model—and thus alter the effects of gender, ethnicity, and ballot position as well—I include contextual interactions only for incumbency and union support.[10]

Note that the contextual variables can enter the model as only interactions, not as separate independent variables. Technically, this is because each contextual variable is a constant within any given election and thus collinear with the fixed effects. Substantively, however, we would not want to include them as separate variables anyway. Precisely because they are constant within any given election, they are the same for all candidates and cannot tell us anything about who is going to win. They are relevant only because they affect how the other variables operate.

When the estimation is run, none of the contextual variables prove to have significant effects. (For this reason and for reasons of space, a table of results is not presented here.) It appears that the power of the teachers unions is fairly uniform across contexts and doesn't vary much with the size of the districts, the partisanship of voters, or the timing of elections.

It is quite possible that context does have an impact, but that it is more nuanced than the rationale above recognizes. For example, while voters in large districts probably know less about their candidates than do voters in small districts, and while the informational impact of union resources probably increases with district size, it is also likely that the unions face more competition from other interest groups in large districts, and this may work to undercut their success. The combination of effects may be a wash, leaving unions about equally powerful in large and small districts (Moe, 2005).

Similarly, Democratic districts could well be sympathetic to union appeals, but in nonpartisan elections the unions may find that they are much more successful running nonpartisan campaigns—claiming to be centrally concerned about what is good for schools and kids, appealing to all segments of the electorate, and making no mention of the union's own role. If so, there may be little connection between a district's partisanship and union success.

And finally, the teachers unions may well have an advantage in off-year elections, because with low turnout they can more easily tip the balance in their favor. On the other hand, the citizens who do turn out may be older, better informed, and less easily influenced than voters in on-year elections (e.g., Rosenstone and Hansen, 1993), and this may tend to limit the union's effect on outcomes. I must admit that I remain skeptical here, and suspect that more and better data would show that the unions do have an advantage in off-year elections.

Additional research is clearly needed to sort out the details. It is reasonable to suspect that contextual variables are having effects of some consequence here that are being masked by the level of generality at which the analysis is carried out. This said, however, the main conclusion from this study is

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10. Estimations show that there are no consistent contextual impacts on the effects of gender, ethnicity, and ballot position anyway.

a simple one that comes through loud and clear: union support appears to have a major impact on who wins these elections, and it appears to be at least as powerful as incumbency.

8. Teacher Turnout in School Board Elections

A serious research program on the political power of bureaucrats obviously needs to be concerned about the key issue of electoral impact. But it also needs to probe more deeply by exploring *how* bureaucrats are able to get their favored candidates elected to office. What are the sources of their political power?

My second study targets one particular source of power: the turnout of bureaucrats at the polls. Because of unions, turnout is just part of a much larger story—for money contributions, phone banks, door-to-door campaigning, and other factors may be much more important to electoral victory. Nonetheless, turnout is by far the most studied topic in the literature, so it is both useful and appropriate to begin here—and to show that, by approaching this familiar topic from a different angle, focusing on specific bureaucrats in the specific elections that bear on their occupational self-interest, we can shed interesting new light on the subject.

8.1 Data

As part of a larger project, I gathered data on the names and zip codes of school district employees in a stratified sample of 70 California school districts, and matched these names to county voter files to get each employee's voting history. I restrict my attention here to 9 of these districts, all located in Los Angeles and Orange Counties. These 9 are analytically useful because, as they are clustered in close proximity to one another, teachers who don't live in the district where they work often show up as residents of one of the other districts. As I will show, being able to compare these two types of teachers—those who live and work in a district and those who live in a district but work in another—is quite helpful for understanding the basics of teacher turnout, as well as its connection to power.

Teachers in these districts are all represented by the California Teachers Association. Other school district employees (administrators, nurses, librarians, janitors, secretaries, cafeteria workers, and bus drivers) have unions of their own.

8.2 Analysis

A generic analysis of bureaucrats in national elections would go directly to an investigation of turnout, the presumption being that turnout is a measure of electoral clout. Yet if we look specifically at teachers in school board elections, turnout is a second-order issue. The first-order issue is whether teachers live in the districts where they work—because if they don't, they aren't even eligible to vote. This is a key aspect of power that is almost always ignored.

As Table 2 shows, the percentage of teachers who live in their own districts varies a great deal and tends to increase with the affluence of the district. Even

Table 2. Demographics

School District	Enrollment	Percent Students with Free Lunch	Percent Teachers Living in District	Percent Employees Living in District
Charter Oak Unified	6,803	26	22	50
Claremont Unified	6,604	27	50	NA
Covina Valley Unified	14,163	42	20	45
Garvey Elementary	7,164	84	8	40
Montebello Unified	34,256	75	NA	48
Norwalk – La Mirada Unified	23,410	45	17	61
Torrance Unified	23,809	15	44	60
Huntington Beach City High	14,310	20	55	67
Santa Ana Unified	58,043	73	12	49

Note: NA indicates no available data for the district.

in the more affluent ones, however, a strikingly large percentage of teachers (in this sample) do not live where they work—and thus cannot vote. Other district employees are much more likely to live where they work, regardless of the district's affluence. This enhances their value as political allies.

It is unclear how representative these findings are of districts generally. Presumably, living outside the district is most common where multiple districts are packed into an urban area, as is the case here. In districts that are suburban, rural, or geographically spread out, far fewer employees may live outside their own districts. Whatever the pattern, some degree of nonresidency is probably a fact of life in almost all districts. And to the extent it is, teachers and their allies should have a harder time translating their own turnout into power.

Now let's look at the data on turnout. Two types of elections are most relevant here: school board elections and bond elections, both of which are nonpartisan. For school board elections I focus entirely on those that are held during odd years; little else is being voted on during these elections, and they offer the best opportunity for studying how teachers and other district employees act on their job-related incentives. For bond elections I focus on those that are not held at the same time as general elections or school board elections.

The findings are set out in Tables 3a, 3b, and 3c. Notice that turnout among the local population is downright abysmal, even in the more affluent districts. In the off-year school board elections for which I have data, 1997 and 1999, the median turnout is 9%. For bond elections it is 23%. This low turnout gives the unions an opportunity to mobilize support and tip the scale toward candidates they favor.

Do teachers vote at higher rates compared with nonteacher citizens? The answer is clearly yes. In 1997, for instance, only 7% of registered voters in the Charter Oak school district voted in their school board election, but 46% of the teachers who lived there did. In Claremont, 18% of registered voters went to the polls, but 57% of the teachers who lived there did. Similar

Table 3a. Teacher and School District Employee Turnout—Los Angeles County, November 1997 Election (Overall County Voter Turnout = 16%)

School District	District Turnout (%)	Teachers (% voting)			Other Employees (% voting)		
		Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups	Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups
Charter Oak Unified	7	46*** (48)	7 (171)	***	41*** (96)	15** (85)	***
Claremont Unified	18	57*** (72)	23 (30)	***	NA	NA	NA
Covina Valley Unified	11	49*** (72)	32*** (31)	*	28*** (242)	10 (39)	***
Garvey Elementary	9	58** (12)	17 (6)	*	58*** (93)	33 (39)	***
Montebello Unified	16	NA	35*** (46)	NA	35*** (330)	26* (34)	
Norwalk – La Mirada Unified	9	46*** (67)	15 (13)	**	35*** (365)	0 (11)	***
Torrance Unified (also bond election on ballot)	25	75*** (299)	40 (15)	***	75*** (583)	36 (14)	***

Statistical significance is indicated as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Column of "comparison of groups" for teachers indicates whether the difference in turnout levels across the two teacher groups is significantly different from zero, using a one-tailed test. The "comparison" column for other employees does the same for the employee groups. In the columns presenting turnout levels, statistical significance means that the difference between the given turnout level and the corresponding turnout level for the district as a whole is significantly different from zero. NA indicates no available turnout data for the district.

Table 3b. Teacher and School District Employee Turnout—Los Angeles County, November 1999 Election (Overall County Voter Turnout = 13%)

School District	District Turnout (%)	Teachers (% voting)			Other Employees (% voting)		
		Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups	Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups
Charter Oak Unified	7	25*** (53)	2 (228)	***	23*** (104)	6 (118)	***
Claremont Unified	18	56*** (82)	35*** (34)	**	NA	NA	NA
Covina Valley Unified	8	34*** (79)	31*** (35)		24*** (276)	9 (44)	**
Garvey Elementary	6	25* (12)	9 (11)		35*** (100)	13* (47)	***
Montebello Unified	16	NA	24*** (58)	NA	39*** (391)	17 (42)	***
Norwalk – La Mirada Unified	6	25*** (75)	15 (13)		17*** (416)	8 (12)	
Torrance Unified	8	28*** (342)	5 (19)	**	29*** (648)	16 (19)	

Statistical significance is indicated as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Column of "comparison of groups" for teachers indicates whether the difference in turnout levels across the two teacher groups is significantly different from zero, using a one-tailed test. The "comparison" column for other employees does the same for the employee groups. In the columns presenting turnout levels, statistical significance means that the difference between the given turnout level and the corresponding turnout level for the district as a whole is significantly different from zero. NA indicates no available turnout data for the district.

Table 3c. Teacher and School District Employee Turnout—Los Angeles County, Special Elections (Bond Measures)

School District and Year	District Turnout (%)	Teachers (% voting)			Other Employees (% voting)		
		Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups	Live and Work in the District (N)	Only Live in the District (N)	Comparison of Groups
Claremont Unified 2000	29	46** (90)	13 (38)	***	NA	NA	NA
Montebello Unified 1998	14	NA	NA	NA	32*** (342)	18 (39)	**
Torrance Unified 1998	31	67*** (310)	22 (18)	***	63*** (610)	22 (18)	***
Huntington Beach City Elementary 1999	19	93*** (210)	80*** (179)	***	89*** (345)	83*** (75)	*
Santa Ana Unified 1999	23	88*** (132)	11 (57)	***	86*** (439)	45** (22)	***

Statistical significance is indicated as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Column of "comparison of groups" for teachers indicates whether the difference in turnout levels across the two teacher groups is significantly different from zero, using a one-tailed test. The "comparison" column for other employees does the same for the employee groups. In the columns presenting turnout levels, statistical significance means that the difference between the given turnout level and the corresponding turnout level for the district as a whole is significantly different from zero. NA indicates no available turnout data for the district.

figures can be recited for *every* district, and the conclusion is the same whether we look at 1997, 1999, or bond elections. Teachers who live in their districts were from *two to seven times* more likely to vote than other citizens were. The median turnout gap was 36.5%, which is a huge number given the very low turnout overall. By either measure, the gap between bureaucrats and other citizens is *much greater* than the literature suggests.

Why do teachers turn out at such high rates? The answer may well be, as the public choice literature has always claimed, that they have an occupational self-interest that other citizens don't have. But *this key claim is never tested*, and alternative explanations are never considered other than by controlling for the usual demographics. In our case, there is clearly a plausible alternative (that, with slight tweaking, could apply to most bureaucrats): teachers are not only more middle class than the average nonteacher citizen, but also more public spirited, more committed to public education, and thus more likely to vote in school elections *regardless* of their personal stakes. The question that has gone begging in the literature thus far is: can the evidence show that self-interest, and not these other motivations, accounts for the turnout gap?

Our data offer a revealing test. Many teachers in the sample live in one school district but work in another. These teachers are presumably just as middle class, public spirited, and committed to education as other teachers are; but because they don't work in the district where they live, they do *not* have an occupational stake in their local school board elections. Will these teachers vote at the same high levels as teachers who do have such an occupational stake?

Whether we look at the 1997 elections, the 1999 elections, or the various bond elections, the answer is the same: in *every* case that allows a comparison, the teachers who live in a district but don't work there vote at lower rates than the teachers who both live and work there. The size of the difference is almost always substantial (and statistically significant). In Claremont, to take a rather typical example, 57% of the teachers who both lived and worked there voted in the 1997 election, but only 23% of the teachers who lived but didn't work there voted.

A corollary issue is: do the teachers who merely live in a district vote at higher rates than ordinary citizens do? Here the answer is less clear, and the low numbers advise caution. Statistical significance aside, these teachers turned out at higher rates than ordinary citizens in 12 of 18 elections, but in 5 they actually turned out at lower rates. Of the cases in which they turned out at higher rates, moreover, only 6 are statistically significant. Overall, the median difference in turnout rates between these teachers and nonteacher citizens is just 7%, which could simply be due to social class or some other distinguishing factor (see below).

These findings contradict the alternative explanation for the turnout differential and bolster the notion that self-interest is in fact what mainly accounts for it. A plausible addendum, however—although I do not have the data to explore it—is that teacher turnout is probably getting a double boost from self-interest: first, because the teachers themselves have an occupational stake

in voting; and second, because their unions have a self-interest in mobilizing them. The literature places all the weight on the former, but it is very likely that both are at work and that the turnout differential is not solely due to the incentives of individual teachers.

Now consider the other school district employees. In this group, rather low paid workers—janitors, secretaries, cafeteria workers, bus drivers—far outnumber administrators, nurses, and librarians, and some 40% are Hispanic. On class grounds alone, therefore, we would expect this group to vote at much lower rates than teachers. In more affluent contexts (and perhaps others), they should also vote at lower rates than other types of citizens.

These class-based expectations are quite wrong. In *every* district with available data, and for all three sets of elections, other district employees who live and work in their districts vote at substantially higher rates than do ordinary citizens—rates that, on average, are just a shade lower than those of teachers who live and work in the district. The median difference in turnout rates between them and the teachers who live in their own districts is just 4%, which is stunningly small given the underlying differences in social class. Clearly, something else is going on here. And that something is probably that these other school district employees, like teachers, approach elections with their own self-interest in mind, and their unions mobilize them on those grounds.

This interpretation is bolstered by the fact that, when we look at other district employees who live in a district but don't work there, and thus do *not* have an occupational stake in the elections, their turnout proves to be decidedly lower on average than that of the other district employees who both work and live there. The former turn out at lower rates in *all* of the 16 cases for which there are data, and 13 of these are statistically significant. The median difference in vote rates between the two groups is 20%, and it is not uncommon for the gap to be much larger.

As was true for teachers, the other district employees who live but don't work in the school district tend to look pretty much like ordinary citizens in their turnout rates. The median difference is 8%, which is virtually the same advantage we found for teachers. In this case, though, social class obviously does *not* explain the turnout gap. And because this is so, it is reasonable to suspect that it doesn't explain the differential between teachers and ordinary citizens either. Some other common factor probably accounts for both differentials.

What these teachers and the other district employees have in common is that they both take a self-interested approach to elections and they both belong to unions. Because they don't work where they live, they have less incentive to vote and they are not mobilized by the local union (to which they don't belong). But they may also recognize—with reminding by their own unions—that they are all enmeshed in a big collective action problem and that they should vote in their home districts to protect one another's jobs and interests. Because voting is not a very costly act, this could easily account for a turnout rate that is 7% to 8% above that of ordinary citizens.

This analysis of turnout reveals major upsides for teachers and their unions. Teachers turn out at much higher rates than other citizens do and act on their occupational self-interest, and exactly the same is true of the other district employees—which makes them key political allies, and essentially allows the teachers unions to *double* their voting strength. There is also a major downside, however, that weakens their ability to convert these advantages into electoral power. This is the problem of residency. The high turnout rates and the potency of self-interest are of political value in school board elections only to the extent that teachers and other school district employees live in their districts. And many do not.

Even when diluted by the residency problem, though, the turnout advantages are still likely to be consequential for electoral outcomes, and possibly pivotal. A key factor is that the margin of victory in school board elections is often very small. By my own estimate (based on the sample of 245 districts discussed in the previous section), the median gap between the best-off losing candidate and the worst-off winner is about 3%. Thus, it doesn't take much of a vote swing to change the outcome.

Consider some rough calculations for the Charter Oak school district. In the 1997 election, three candidates competed for two seats. The total number of votes cast (two by each voter) was 3506, and the margin of victory was 2.54%, or 89 votes.[11] Are the turnout differentials in Charter Oak large enough to overcome an 89-vote gap and bring victory to a union-backed candidate? The answer is yes. The district had a total of 350 teachers, only 22% living in the district and voting at a rate of 46%. Thus, there were 35 teacher-voters. The district also had 354 other district employees, 50% living in the district and voting at a rate of 41%. This means that there were 73 voters among the other district employees, and, when the teachers are added in, 108 total votes by school personnel. This figure alone exceeds the 89 votes needed for victory, and it makes no allowance for other sources of pro-union votes (e.g., relatives and friends). Similar calculations could be carried out for the other districts, showing that the turnout differential alone is either sufficient to overcome the margin of victory or at least comes close.

As I said, these are rough estimates. And of course we can't really expect all employees to vote as a bloc.[12] But even so, these sorts of calculations help to show that high turnout rates can translate into electoral power even when considerably diluted by the residency problem. This is especially impressive given that bureaucrats and their unions have many *other* sources of power as

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11. Figures on votes and margins of victory were obtained from the Los Angeles County registrar.

12. Indeed, a standard claim of the literature is that unions have a hard time delivering the votes because their members have diverse political preferences (Rozell and Wilcox, 1999; Asher et al., 2001). The evidence, however, typically derives from national elections in which there are myriad nonoccupational issues at stake, from abortion to gun control to national security. In the case of teachers and school board elections, the issues are much more narrowly occupational and the basis for bloc voting much greater.

well—sources that, when combined with turnout, are likely to further enhance the prospects for electoral victory, perhaps by a lot.

9. Conclusion

My purpose in the empirical analysis is not to make definitive claims about the power of teachers or of public employees generally. It is to illustrate what kinds of issues become relevant when we think of rank-and-file bureaucrats as truly political actors, and to generate some findings that contribute to a broader program of research.

Two studies are reported here. Both depart from the current empirical literature in basic approach: they get away from the usual focus on generic bureaucrats in national elections and look instead at specific bureaucrats in the specific elections they care about—which is a far better way to understand the nature and consequences of bureaucratic power.

The first study takes this approach in investigating an obviously crucial topic that has so far gone virtually unexplored: the impact of bureaucrats on electoral outcomes. It provides evidence that teachers, acting through their unions, are quite successful at getting their favored candidates elected to local school boards. When a candidate is supported by the unions, her probability of winning increases dramatically, so much so that the impact of union support appears to be roughly the same as the impact of incumbency. In terms of total impact, union influence may be even greater than this suggests, because union victories literally produce incumbents—and the power of incumbency then works for union candidates to boost their probability of victory still further in future elections.

The second study sheds new light on this literature's most researched topic: bureaucratic turnout. Aided by its distinctive approach to the subject, it shows that public bureaucrats' turnout advantage over other citizens is much greater than the existing literature would lead us to expect. It also offers persuasive new grounds for believing that their high turnout is indeed motivated by occupational self-interest—and more generally, that they are actively and purposely engaged in an electoral effort to control their own superiors. In addition, it points to a downside that the literature on bureaucratic turnout generally overlooks—the problem of residency—and shows that this problem weakens, but does not eliminate, their ability to convert turnout into electoral power. The net result is a deeper, more nuanced understanding of the role of turnout in bureaucratic politics.

These two studies offer useful findings on the political power of bureaucrats and their unions. But they are rightly viewed as a incremental contributions to a much larger research agenda. We need more research on how public sector bureaucrats of *all* types, in the United States and elsewhere, attempt to exercise power in gaining control over the political authorities that govern them. As things now stand, very little is known about these groups and what they do in politics. Almost everything remains to be learned.

Any progress we make is likely to have a big impact on our understanding of political control, and of government generally. For wherever there

are governments, there are bureaucrats who have incentives to take political action—for bigger budgets, higher taxes, more programs, greater job security, restrictions on management, protections from change. And the authorities have incentives to be responsive as long as the bureaucrats can bring political power to bear, which their numbers, resources, and organization make eminently possible.

This is not to say that all groups of bureaucrats are politically powerful or that bureaucrats are equally powerful across political systems. Many of the interesting research questions have to do with precisely this kind of variation and detail: with which groups of bureaucrats are powerful, how they are able to become powerful, how their power varies with the type of political system, and what the consequences are—for political control, for public policy, and for the organization, size, cost, and effectiveness of government.

To take a simple example from the United States, consider the difference between teachers and employees of the federal government. About 80% of the nation's 3 million teachers belong to unions, which gain strength as political organizations not only from having huge numbers and resources, but also from having members who share occupational interests and are present in virtually every political district in the country. From a political standpoint, federal workers are not nearly so well off. There are lots of them, some 2.7 million, and they are distributed all around the country (with 16% concentrated in the Washington, D.C., area). But in the aggregate they are not even as numerous as teachers, and less than 20% are actually dues-paying union members (Hirsch and Macpherson, 2004; U.S. Census Bureau, 2005). Moreover, the bigger federal unions, such as the American Federation of Government Employees, organize workers who perform many different jobs in countless agencies throughout the executive branch. This limits their common interests and the kinds of political goals they can pursue. For these and other reasons, the federal unions appear to be at a disadvantage in "their own" elections. Beyond narrow issues of civil service, they probably have much less clout with Congress than do the teachers unions (Johnson and Libecap, 1994).

To take this one step further, it may well be that national bureaucrats in the United States have less influence over the national government than state and local bureaucrats have over governments at lower levels—and that, as a rule, the power of public sector unions tends to be greater the lower the level of government. For as governments in the United States get closer to where the bureaucrats in those governments live, the numbers and resources of bureaucrats become more politically effective and their interests more coherent; public workers are more likely to be unionized; and there is likely to be less competition from organized competitors. While students of American politics have devoted most of their research to national-level institutions, state and local governments—which employ some 87% of the nation's public workers (U.S. Census Bureau, 2005:298) and either formulate or carry out most of the nation's public policies—are the arenas where public sector unions probably have the greatest impact and where their activities especially need to be studied. A national focus misses most of what is going on.

Outside the United States, decision making is often much more centralized, and a national focus may be entirely appropriate. This is surely the case for many European countries, which, in addition to building a new political and economic system for themselves, have been caught up in one of the most important political retrenchments of our time: the attempt to reduce the welfare state in response to rising costs, globalization, and needs for economic efficiency and competitiveness. Public sector unions have generally opposed these retrenchments, which threaten their most fundamental interests (e.g., Ferner and Hyman, 1992). But how much power have they been able to mobilize, and how successful have they been at blocking reform? It is reasonable to expect that the answers, when they come, will show that union power varies with the context—possibly with the degree of centralization, but also with the electoral system, collective bargaining and civil service laws, the degree of corporatism, and the like—and that unions may be much more successful at blocking reform in some nations than in others. These are the sorts of things an empirical research program would begin to uncover once it takes bureaucrats seriously as political actors.

In the meantime, we need to rethink our theories of political control. The prevailing theories treat bureaucrats as mere subordinates, controlled from above by political authorities. But the control relationship can run both ways, and not just because bureaucrats have expertise and other sources of private information. In a democratic system the authorities are elected, and this gives bureaucrats an opportunity to exercise electoral power in determining who will occupy positions of authority and what choices they will make in office. It would be odd indeed if public bureaucrats and their unions did *not* invest in this kind of reverse control—and there is ample evidence that they *do*.

It is time, then, for our theories to treat bureaucrats as truly political actors, and to recognize the political power they may have over public officials. Once this is done, the standard notion that the bureaucracy is "under control" becomes a good deal harder to maintain. And the relationship between bureaucrats and politicians begins to look a lot more interesting.

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