

Collective Bargaining and The Performance of the Public Schools

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Students of American politics rarely study public sector unions and their impacts on government. The literature sees bureaucratic power as rooted in expertise, but largely ignores the fact that bureaucrats often join unions to promote their own interests, and that the power of their unions may affect government and its performance. This article focuses on the public schools, which are among the most numerous government agencies in the country, and investigates whether collective bargaining by teachers—the key bureaucrats—affects the schools’ capacity to educate children. Using California data, analysis shows that, in large school districts, restrictive labor contracts have a very negative impact on academic achievement, particularly for minority students. The evidence suggests, then, that public sector unions do indeed have important consequences for American public education. Whether they are consequential in other areas of government remains to be seen, but it is an avenue well worth pursuing.

Prior to 1960, few public workers in the United States belonged to unions and almost none were covered by collective bargaining. All this changed during the next two decades, as most states passed laws making it easier for public sector unions to organize and bargain. The result was a period of explosive growth in which the portion of public workers covered by collective bargaining grew to more than 40% by the early 1980s. It has remained at roughly that level ever since and is much higher in many states, localities, and occupations, particularly outside the right-to-work South. Meantime, unions in the private sector have been caught in a disastrous free fall, with coverage at just 8.1% as of 2006 (U.S. Census Bureau 2008).

Public sector unions have changed the dynamics of American politics. They have compelling incentives to be politically active, because their members depend on government for their livelihoods. And they have acted on these incentives, using impressive reservoirs of money and manpower to make themselves formidable players in electoral campaigns, as well as in every major sphere of governmental decision making (Blais, Blake, and Dion 1997; Johnson and Libecap 1994; Troy 1994).

They have done more, however, than change the nation’s politics. For when they engage in collective bargain-

ing, the contracts that result—each of which may impose hundreds of formal rules—become part of the structure of government. They are often major players, then, in determining how government is organized. State and local governments, where most of the nation’s policies are carried out and most of its money spent, are more affected by collective bargaining than the federal government. And some state and local governments are more affected than others. But such variation simply makes the phenomenon more interesting and worth studying. On the whole, collective bargaining is quite common within the public sector, it is often a source of governmental structure, and—if institutions really do matter—it is likely to be an important (if varying) influence on governmental performance (Freeman 1986; Freeman and Ichniowski 1988; Lewin et al. 1988; Moe 2006).

Students of American politics have had little to say about the rise of public sector unions, and in particular about the impact of collective bargaining on the structure and performance of government.¹ There is a vast literature on public bureaucracy, but that literature—from the early writings of Weber (1946) to recent developments in rational choice (e.g., Epstein and O’Halloran 1999; Huber and Shipan 2002)—has always emphasized that the power of bureaucrats is rooted in their expertise: in

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¹My own work aside (Moe 2006), an exception is Johnson and Libecap (1994). This is a book about federal employee unions, however, and most of the membership and power of U.S. public sector unions are at the state and local levels. See also Carpenter’s (2001) analysis of the early postal unions.

the knowledge and experience that give them leverage with political superiors (and clients). It has also recognized that bureaucratic leaders can establish reputations, build constituencies, and otherwise take entrepreneurial action to gain power and autonomy in politics (Carpenter 2001; Rourke 1984).

While these aspects of bureaucratic power are surely important, so is the largely unappreciated fact that even the most ordinary bureaucrats often get organized into unions. When they do, their numbers and resources may well translate into considerable power over the policies, structures, and performance of government, power that can be used to promote their own occupational interests—in material benefits, job security, work rules, public policy—and that may lead to a host of impacts on government. Many of these impacts are likely to be intentional, as when unions block unwanted policy reforms or secure new work rights. But some may be quite unintentional—as could occur, for instance, if restrictive work rules make public agencies less productive. Either way, they are outcomes of union power, and they stand to have important consequences for what government is and does.

The study of American government has long recognized the power of special interest groups, including unions. But the groups they study are outside of government—and public sector unions arise from the inside, representing the special interests of the government's own employees.² It is time for political scientists to make public sector unions a serious part of their research agenda.

This article is a step in that direction. My focus here is on the public schools, which are among the most numerous government agencies in the United States, and surely among the most important. The key bureaucrats are the schools' teachers, who, outside the South, tend to be heavily unionized and covered by collective bargaining. The basic question here is whether, in using their power to secure contract rules that advance the occupational interests of their members, the teachers unions are (unintentionally) limiting the capacity of the public schools to educate children.

The data are from a sample of schools and districts from the state of California. Collective bargaining contracts are coded in terms of how restrictive they are in limiting control by superiors and imposing a structure favorable to the occupational interests of teachers. The analysis then shows that, in large school districts, the re-

strictiveness of the teacher contract has a negative impact on the capacity of schools to promote student achievement. It also reveals that, in these large districts, restrictive contract rules are especially negative in their effects on the academic achievement of minorities.

The evidence presented here, then, suggests that public sector unions do indeed have important consequences for the performance of the public schools. Whether they are consequential in other areas of government remains to be seen, but it is an avenue of research worth pursuing.

Expectations

Since the publication of *A Nation at Risk* (National Commission on Excellence in Education 1983), the watershed report that warned of a "rising tide of mediocrity" in America's schools, public officials have been under intense pressure to improve academic performance. They have responded with billions of additional dollars and with a commitment to reform that has persisted for a quarter century. The focus has been on academic achievement, but there has been great concern as well for closing the "achievement gap" between white and minority children, and for improving the large urban districts that many minority students attend (Peterson 2003).

Both have been pursued through new accountability systems, which, in imposing more rigorous standards, testing regimes, and consequences for low performance, represent the most aggressive effort yet by public authorities to improve academic outcomes. These reforms spread rapidly across the states during the 1990s, and in 2001 the federal government enacted No Child Left Behind, which imposed accountability rules for the country as a whole: rules designed to spur academic improvement, but also to spotlight the achievement gap and insist on efforts to close it (Peterson and West 2003).

Accountability reforms are means by which state and national officials are attempting to improve the schools. But behavior in the schools is shaped by other structures as well, among them the rules imposed by collective bargaining. What should we expect of these contract rules? Do they contribute to the authorities' efforts to improve academic achievement? Or do they tend to get in the way?

There is good reason to think the latter is more likely, at least on balance. Collective bargaining would not exist except for the power of the teachers unions, and the core interests they pursue in negotiations are rooted in their own survival and well-being as organizations—not in student achievement. As unions, they are centrally concerned with their membership base and financial

²The interest group literature, like the bureaucracy literature, has very little to say about public sector unions. See, e.g., Baumgartner and Leech (1998) and Ciglar and Loomis (2006).

resources, and thus with protecting teacher jobs, attracting members, and keeping dues money coming in. They also have induced concerns—contributing to these more basic objectives—for representing the occupational interests of their members: in better pay and benefits, more autonomy, less threatening methods of evaluation, smaller classes, prohibitions on nonclassroom duties, fewer course preparations, and other rights and protections. The unions secure these objectives through formal contract rules that require or prohibit certain behaviors on the part of management, and most generally, place restrictions on top-down control (e.g., Grimshaw 1979; Hoxby 1996; McDonnell and Pascall 1979; Moe 2001).

Their power to get the rules they want is heavily bound up with politics. The state collective bargaining frameworks that facilitate union organization are themselves political products, of course: achieved in the 1960s and 1970s through the political power of private sector unions and their allies, and then stabilized and expanded by the growing political power of public sector unions themselves (e.g., Freeman 1986; Troy 1994). But even within this structure, politics remains an ongoing determinant of union success in collective bargaining. Most important, local school districts are governed by elected school boards, and the teachers unions—by taking action in school board elections—can play central roles in choosing the very “management” they will be negotiating with: a remarkable advantage that private sector unions can only dream about.³ This being so, teachers unions organize themselves around politics as well as collective bargaining. And the evidence suggests that, while their power surely varies across contexts, they are by far the most organized and active groups in school board elections, and they are often successful at getting their favored candidates elected.⁴

The unions use their power—their basic work-denial power, enhanced by their political power—to get restrictive rules written into collective bargaining contracts. And these restrictions ensure that the public schools are literally *not organized* to promote academic achievement. When contract rules make it difficult or impossible to weed out mediocre teachers, for example, they undermine the most important determinant of student learning: teacher quality (Sanders and Rivers 1996). And when contract rules guarantee teachers seniority-based trans-

fer rights, they ensure that teachers cannot be allocated to their most productive uses (Levin, Mulhern, and Schunck 2005). Much the same can be said about a long list of standard contract provisions. This is to be expected. Except at the margins, contract rules are simply not intended to make the schools effective.

Still, there are shades of gray here. When teachers unions pursue their own interests, it may sometimes happen that their interests overlap with those of students. Their pursuit of smaller classes is one example. More generally, because unions tend to secure better wages and working conditions, and because they expand worker autonomy, they may help to attract higher quality workers, promote professionalism, lessen worker absenteeism and turnover, and thus enhance productivity (Freeman and Medoff 1984).

It is plausible to suggest, then, that some of what unions do may be good for academic achievement. And it is even possible that, on balance, these good effects may outweigh the negatives—in which case our statistical analysis should ultimately show that restrictive labor contracts actually have positive impacts on student learning. Yet for reasons we have already discussed, this seems quite unlikely. There are persuasive grounds for expecting the positives to be small relative to the negatives, and outweighed by them.

Any positive effects will tend to be accidental by-products of what the teachers unions do in their own interests. They pressure for smaller classes, for instance, because teachers like them and they require more hiring (and more union members), not because they are good for student achievement. Were there an optimal class size for student achievement (given district budget constraints and other, more productive ways of spending money), this would not stop the unions from pursuing class sizes that are still smaller. The same is true for seniority rights. Unions press for them because they take job assignments away from administrators, not because they increase experience or reduce turnover; and if a lack of managerial control over the allocation of teachers has negative effects on achievement that outweigh any effects on experience and turnover, the unions would have an interest in supporting the seniority system anyway. The same logic applies to issues of professionalism. The bottom line is that the interests of teachers (and unions) are *not aligned* with the interests of children, and the organizational arrangements pursued by unions will ultimately *diverge* from those that are best for students.

We also need to recognize that any contract provisions that happen to be good for student achievement could be adopted—as policies—by school districts *on their own*. And especially in this era of accountability, they

³Note that the actual bargaining on behalf of districts is usually done by administrators hired by the board, but the elected members of the board have ultimate authority to direct and approve what happens and are the political superiors in charge.

⁴For evidence on these scores and more detailed accounts of the role of teachers unions in school board elections, see Moe (2005, 2006) and also Hess and Leal (2005).

would have incentives (absent union power) to move in that direction. They would not, on the other hand, have incentives to adopt provisions that have negative effects on student achievement, but would adopt them only under the pressures of collective bargaining.

The central expectation I will be testing in this article, then, can be stated as follows: the restrictiveness of the collective bargaining contract—its overall limitation on managerial control in the interests of teachers (and unions)—is likely to have negative consequences, on balance, for the performance of the public schools. The more restrictive the contract, the more difficult it will be for schools to organize effectively for student achievement, and the less students will tend to learn.

This hypothesis—and this article—are just starting points. A more detailed inquiry would suggest that, while there are reasons for expecting these labor contracts to have negative effects, there are also reasons for thinking that the magnitudes of the effects are not everywhere the same, but may vary with a range of conditions. Taking on this larger task would require a far more extensive analysis and is beyond the scope of this one article. But once I present the basic empirical findings on restrictiveness, I will go on to discuss two conditions of special concern in American education reform—the size of the district and the concentration of minority students—and show that they do indeed appear relevant to how collective bargaining affects the schools.

Research on the Impact of Teachers Unions

There is a rather large research literature on the impact of unions. These studies generally agree that unionization leads to higher costs in both the private and public sectors. But the findings are mixed on the question of how unions affect overall productivity, and thus whether the higher costs are counterbalanced by increases in output. One reason for the mixed findings is that these studies often use different methods and measures, are carried out on widely varying industries and economic contexts, and are not of equal quality, all of which make summary conclusions difficult. It is perhaps not surprising, then, that even after decades of research the dispute about the overall impact of unions lives on without resolution (e.g., Doucouliagos and Laroche 2003; Hirsch 2004).

A small subset of this research has dealt with teachers unions and whether collective bargaining within the public school system has an impact on student learning. Here too the findings have been mixed. I will not review

these studies individually, but a brief look at their characteristics helps to suggest how extensive the heterogeneity is and why it can easily lead to disparate findings (Argyris and Rees 1995; Eberts and Stone 1984, 1986; Grimes and Register 1990; Hoxby 1996; Kurth 1987; Milkman 1997; Nelson and Rosen 1996; Peltzman 1993; Steelman, Powell, and Carini 2000).

- (1) Many studies are carried out at the state level and are based on heavily aggregated data—average achievement scores for entire states, for example—making it very difficult to discover causes and effects at the district level where collective bargaining actually takes place.
- (2) Collective bargaining is measured differently in different studies. Some use dummy variables to indicate whether a state or district has collective bargaining. Others focus instead on union strength, using the percentage of teachers belonging to unions as a proxy.
- (3) Achievement is typically measured in one of two ways: mathematics test scores or scores on SAT/ACT exams. Yet SAT and ACT tests are only taken by college-bound seniors, who are unrepresentative of students generally. And math scores capture just a small part of the curriculum and are narrow measures of achievement. The populations whose math scores are featured, moreover, vary dramatically from study to study: Eberts and Stone (1984, 1986), for example, focus on fourth graders, Argyris and Rees (1995) target tenth graders, and Milkman (1997) studies twelfth graders.
- (4) Some studies are based on student-level data, and thus on very large samples. Eberts and Stone (1984, 1986), for instance, analyze a sample of approximately 14,000 students in 328 elementary schools, and other studies also have samples in the thousands (Argyris and Rees 1995; Grimes and Register 1990; Milkman 1997). Ordinarily, large samples would be a good thing. But in these studies, many students are drawn from the same schools and districts, and thus should not be treated as independent observations. Were clustering taken into account, standard errors would tend to be higher and statistical significance more difficult to achieve. Yet none of these studies adjusts for clustering. If they did, their findings about the impact of collective bargaining would likely be weakened.
- (5) States with low union density or little or no collective bargaining are almost all southern or border

states, where school systems have historically been low performers. This simple fact makes it difficult to disentangle regional influences from union influences on student achievement. It also raises endogeneity issues, because some of the factors that explain why unions are weak in these states may also explain why the school systems do not perform well; their political cultures, for instance, generate attitudes and laws hostile to unions, but also low spending, low taxing, and low public pressure for good schools. Many of the studies (Hoxby 1996 is the exception) ignore these problems entirely, while others address it by merely introducing a dummy for the South.⁵

In view of how different these studies are and the questions raised by their methods, there can be little surprise that this literature has not led to a coherent set of findings. One study, however, stands apart from the others and is due special weight. This is the analysis by Hoxby (1996), which assesses the impact of collective bargaining by looking at districts before and after the unions gain bargaining rights, a unique design that only she has employed. What she shows, in the literature's most sophisticated analysis, is that collective bargaining increases school inputs—total spending, teacher salaries, teacher-student ratios—but also decreases their productivity, so that the unions' overall impact on school performance is actually negative (as measured by the dropout rate).

A New Study

My own approach departs from the others in this literature in two basic respects. First, I am concerned with the restrictiveness of the contract rather than with collective bargaining or union strength *per se*. And second, I am comparing its effects across jurisdictions that all engage in collective bargaining and whose teachers are virtually all unionized.

The focus on restrictiveness means that, rather than reducing collective bargaining to gross dichotomies (have it/do not have it) or gross proxies for union strength (percentage unionized), we are led to explore the actual contents of the labor contracts themselves, and thus to determine what rules they contain and what implications they have for student achievement. In this way, we can gain a new and more informative angle on the academic impact of collective bargaining.

⁵Endogeneity issues may also arise for other reasons, which will be discussed at a later point in the text.

By looking only at districts that have collective bargaining, we gain additional advantages. The vast majority of school districts (of any size) in this country do engage in collective bargaining. This is business as usual in public education. An approach that compares these “normal” districts to those that do not have collective bargaining is a risky proposition; for as I suggested above, there are reasons the latter districts have never gotten unionized, and these factors (if not measured or otherwise adjusted for) may lead to biases. By looking just at the “normal,” unionized districts, we are looking at districts that have a great deal in common, and the risk of bias should be reduced considerably.

In addition to these basic differences in approach, this study also departs from mainstream studies in more specific ways—reflecting, for the most part, an effort to deal with some of the methodological issues (mainly having to do with measurement) outlined in the prior section. I will introduce these elements below as they become relevant.

Data and Key Variables

The data for this study are drawn from school districts in the state of California. The current reality is that California and all other states administer their own achievement tests, which usually provide good measures of student learning—and better measures than most national data sets, which, if they have test scores at all, typically use fewer test items, focus only on students in certain grades, and have few students per district. The problem is that states tend to use different tests that cannot readily be compared to one another. So while a nationwide analysis might otherwise be desirable, there are grounds in this case for picking a particular state. And California offers a nice compromise. It has good measures of student achievement whose reliability has been carefully maintained over time. It is also a large, diverse state whose districts vary greatly on variables of possible relevance—they are large and small, urban and rural, high minority and low minority, and so on—giving us a broad base for analysis.

Since 1998, when its accountability plan went into effect, California has been administering achievement tests to its students and, based on the scores of all students across all grades in all subjects tested, giving each school a performance score called the Academic Performance Index (API). The test scores of individual students are confidential. But the API scores of schools are very public indeed and are the state's prime means of holding schools accountable. They are also useful for the type of analysis we are carrying out here: they provide an

overall index, for all students in each school, of how much students are learning. This is an attractive alternative to using the math scores of fourth graders or the SAT scores of college-bound high school seniors.⁶

Collective bargaining contracts were gathered randomly from 371 of California's approximately 1,000 school districts. The contracts are filled with rules. Some of these rules deal with teacher pay and benefits, but most—the focus here—impose structure on the workplace by giving teachers formal rights and restricting managerial control. In coding the contracts, I singled out the following types of work rules as potentially important to the operation of schools and typical of the restrictions unions fight for:

- teacher rights to get or keep classroom assignments within the school (.30);
- teacher rights to make voluntary transfers to other jobs within the district (.68);
- teacher rights not to be involuntarily transferred to other jobs within the district (.78);
- requirements that there be a general policy committee with teacher participation (.26);
- requirements that there be a personnel committee with teacher participation (.25);
- restrictions on how teacher classroom performance can be observed (.24);
- requirements that performance goals be jointly determined by principal and teacher (.31);
- restrictions on the teacher evaluation process (.44);
- restrictions on the number of students the teacher must teach (.56);
- restrictions on faculty meetings (.25);
- restrictive legal language describing when teachers can be dismissed (.25);
- guarantees of teacher preparation time (.41);
- requirements that disputes be arbitrated (.25);
- restrictions on noninstructional duties (ultimately dropped); and
- restrictions on how parent complaints must be handled (ultimately dropped).

I used factor analysis to aggregate these dimensions into a single index of restrictiveness. It is one of the advantages of factor analysis—over, say, a simple average—that

⁶Information on API scores can be found on the California Department of Education web site, at www.cde.ca.gov. For 1998–99 and 1999–2000, the API annual achievement scores were based entirely on the Stanford 9 achievement test, which covered reading, language, spelling, and math in grades 2–8, and reading, language, math, science, and social science in grades 9–11. In subsequent years, the department shifted gradually away from the Stanford 9 toward its own achievement tests in calculating the API annual scores.

the various dimensions are not automatically given equal weight, but are weighted unequally based on their correlations with the latent concept. Their individual loadings are given in parentheses above. The most important are voluntary and involuntary transfers, followed by restrictions on the number students per teacher, rules for teacher evaluations, and guaranteed preparation time. Two of the dimensions were ultimately dropped because their loadings were too low.⁷

The empirical analysis to follow, then, is built around the two key variables I have just introduced. One is California's index of school performance, the API, derived from student test scores. The other is an index of contract restrictiveness, derived from a coding of collective bargaining contracts. The question at issue is: how does restrictiveness affect school performance?

The Empirical Model

The purpose of California's accountability system is to improve the performance of the public schools and thus to boost student achievement. This is the goal of public policy. It makes good sense, therefore, to take as our dependent variable the growth in API scores over time, and to frame the empirical analysis as one of determining whether the restrictiveness of collective bargaining contracts makes it more difficult for public schools to achieve increases in their API scores.

The model of school improvement I'll be testing consists of three parts. The first recognizes that how much a school improves is likely to depend on its initial level of performance, the base API. One reason is that schools starting out at low achievement levels have vast opportunities for improvement—nowhere to go but up—while schools starting out at high achievement levels may have to struggle to increase their scores at all, not to mention by amounts comparable to the lower-level schools. A related reason is that the API scale, which runs from 200 to 1,000, gives rise to ceiling effects as scores begin to approach 1,000. While only about 10% of the schools in this sample have scores over 800, the model includes a

⁷Dimensions were eliminated if their loading was below .20. I should add that most of the dimensions listed above were themselves measured by multiple indicators (specific contract restrictions of their particular type). In each of these cases, factor analysis was used to aggregate the multiple indicators into a single measure for the dimension, and that measure was then used in the final factor analysis discussed in the text. The analysis was carried out using the principal factor method within Stata 9.0. A constant of 3 was added to the final index to ensure (for ease of interpretation) that it is always positive. A more detailed discussion of the index construction is available in an appendix, which can be requested from the author.

quadratic term (the square of the base API) to take this nonlinearity into account.

Controlling for its starting point, a school's academic improvement depends on characteristics of the students it is trying to educate, as well as characteristics of the school itself and the district it is a part of. These influences are incorporated in two separate ways, which give rise to the second and third components of the model. The first has to do with the basic levels of these variables. Schools with large minority populations, for example, may find it more difficult to raise achievement than schools that are mainly white. Similarly, schools with larger enrollments, larger classes, or more inexperienced teachers may find it more difficult to improve than schools that are more advantaged on such counts. The second component recognizes that, whatever the levels of these variables for a given school, the school's change in performance over a given time period may also be a function of how these variables have changed over that period. If the student body has shifted from 10% to 20% Latino, or if enrollment has jumped from 500 to 600, these changes might be accounting for some of the movement in student achievement. The model to be estimated therefore takes the following form:

$$\text{APIGrowth}_{ij} = \beta_0 + \beta_A \text{API}_{ij} + \beta_{AS} \text{API}_{ij}^2 + \beta_L X_{ij} \\ + \beta_C \Delta X_{ij} + \beta_R R_{ij} + \varepsilon_{ij}$$

The subscript i is a counter for schools, and j is a counter for districts. X is a vector representing the level that the (nonunion) independent variables take on during the base period. ΔX is a vector representing the change in these X variables over the relevant time period. R is the restrictiveness of the collective bargaining contract. And ε is the random error component. There are several measurement and estimation issues worth discussing here, so let me add a few points of clarification.

The base year of this analysis is the 1998–99 school year: the first year of the California accountability program, and the year the collective bargaining contracts were obtained from the districts. Achievement growth is measured as the total improvement in API scores between this base year and the 2002–03 school year. This five-year period of time is chosen because it is long enough to give the schools an opportunity to demonstrate improvement (or not) but short enough to help ensure that the restrictiveness of the union contract, which is measured only in the base year, remains relatively stable.⁸

⁸The term of most contracts is about three years, and, although many will not change much with renegotiation (or may take a year or two to renegotiate), the reliability of the restrictiveness measure is bound to decline the longer the time period. A period of five years seems a reasonable compromise under the circumstances.

The dependent variable, the growth in API scores, is not the simple difference between the API in 2002–03 and the API in 1998–99. Because of changes in the achievement tests and the rules for taking them (regarding, e.g., which students can be excluded), the Department of Education adjusted each year's API scores to ensure they could be validly compared to the scores of the prior year to yield growth scores. Unique adjustments were carried out each year. The total growth score for any given school over the entire period, then, is the sum of its four year-to-year growth scores.

The analysis is conducted at the school level and is carried out separately for elementary and secondary schools to recognize organizational differences.⁹ In elementary schools, teachers usually have the same students all day long and develop very personal relationships with them, whereas in secondary schools relationships are less personal and more bureaucratic; so it is reasonable to think that collective bargaining (and other variables) may operate somewhat differently in these settings.

Whichever type of school is being analyzed, we need to recognize that schools are clustered into districts, and that, due to a host of unmeasured variables that make up their local environments, the schools within a given district have more in common with one another than they do with other schools. The error terms in this analysis are therefore likely to be correlated within districts, and this violates the usual OLS assumptions. This being so, the analysis is carried out using a robust (Huber-White) estimator of variance that recognizes the within-district correlation of errors across schools (clustering).

Endogeneity bias is always a concern, but it seems unlikely to be a problem that demands correction here. There is some possibility that achievement has a causal effect on restrictiveness, e.g., with teachers wanting more protections when students are performing at low levels. But our dependent variable is the *change* in achievement, not achievement per se, and this should lessen any problems of bias—particularly because the change in achievement occurs in the years *after* the labor contracts have been negotiated and cannot have caused their restrictiveness. We are also explicitly controlling for the base year level of achievement on the right-hand side, as well as for an array of student background factors. The correlation between achievement and restrictiveness, moreover,

⁹The analysis is carried out at the school level because during this period California provides API scores for schools, not districts. Even if it were possible to do the analysis at the district level, the school level is preferable because it is possible to get multiple measures of performance within each district, as well as more precise measures of the possible causes of that performance (the student demographics and organizational features of each school). Learning takes place within schools.

is just $-.02$ for elementary schools and $.06$ for secondary schools.¹⁰ Another possibility is that endogeneity problems may arise from factors that are causally connected to achievement and also correlated with restrictiveness but omitted from the model. We might worry, for example, that highly bureaucratic systems are more likely to have restrictive contracts, and that bureaucracy creates conditions that lead to bad teaching; or we might worry that unions get more restrictive contracts in large, urban, high-minority systems that tend to be poor performers. But our model includes a long list of student, school, and district controls, including measures of bureaucracy, teacher quality, district size, and student ethnicity. We cannot know that all influences of any relevance are included, but our controls are quite extensive, and it is reasonable to think that any omitted factors have rather small effects.¹¹

Independent Variables

As set out above, our model contains a number of unspecified X s that are potential determinants of school improvement. Let's take a more detailed look at these X variables.

Research on student achievement has arrived at one central conclusion that is essentially beyond dispute: the characteristics of the students—socioeconomic status, ethnicity, language problems—are the key determinants of achievement and are consistently more powerful than characteristics of schools and districts (Hanushek 2003). In our model, the following variables are designed to capture the composition of each school's study body:¹²

- Student ethnicity: percent black, percent Asian, percent Latino, percent other minority (with percent white the omitted category).
- Student socioeconomic status: percent qualifying for free or reduced-price meals.
- Student language problems: percent categorized as English language learners.

Research on school and district characteristics, by contrast, has not led to a coherent set of findings. It might seem that higher district spending, smaller school size, smaller class size, higher teacher salaries, and other common-sense factors would have strong connections to student learning, but the findings have generally been quite mixed (Hanushek 2003). There is recent experimental evidence that smaller classes may have a modest impact on achievement, at least in the early years (Mishel and Rothstein 2002). There is also good evidence that teacher quality is the most important organizational determinant of achievement (Sanders and Rivers 1996). Teacher quality, however, is difficult to measure in the absence of very detailed data. One proxy for (low) teacher quality is inexperience, for studies have shown that teachers with only one or two years in the classroom are less effective than experienced teachers (Rivkin, Hanushek, and Kain 2001). Another proxy is teacher credentials, although the evidence linking credentials with achievement is weak (Kane, Rockoff, and Staiger 2006).

In the interests of having adequate (and commonly used) controls, but without expecting most of the school and district variables to show much relationship with school improvement, I have included in the model a rather lengthy list of such variables for which measures are available. They are as follows:¹³

- District size: the log of district enrollment.
- School size: the log of school enrollment.
- District location: dummies for rural and suburban (with urban the omitted category).
- District education level: the percentage of college graduate adults in the district.
- Type of district: dummies for high school district and elementary district (with unified district the omitted category).

¹⁰These correlations are at the district level. The level of contract restrictiveness in 1999 is due to the accumulated influences of factors in the past, so contemporaneous correlations can only be suggestive. Even so, it is worth noting that restrictiveness is contemporaneously correlated at much higher levels with other variables, e.g., district size (.62), rural location ($-.40$), and percent black (.31).

¹¹It is de rigeur among some researchers to "correct" for endogeneity problems if there is any hint they might exist. But corrections introduce new problems of their own that can easily lead to poorer rather than better estimates. Good instruments are often difficult to find, and that is the case here. The most obvious candidate is district size, which is strongly correlated with contract restrictiveness; but as the analysis later shows, it may also be connected in various ways to student achievement, which rules it out as an instrument. Other candidates are the percent of district voters who are Democrat, or the percent of district adults who are unionized (or belong to public sector unions); but these turn out to have very weak correlations to contract restrictiveness, making them weak instruments.

¹²All of the data for these measures are obtained from the API databases, which can be found (as of March 2007) at www.cde.ca.gov/ta/ac/ap/apidatafiles.asp.

¹³The data on class size are taken from the API data sets, referenced earlier. All other data are from the California Department of Education's CBEDS data files, except for (1) the data on teacher salaries, total spending, and bureaucracy, which are from the Department's J-series financial data files, located on the web at www.cde.ca.gov/ds/fd, and (2) the data on the educational level of the district population, which are from the National Center for Education Statistics' Census 2000 School District Demographics Data Files, which can be found on the web at www.nces.ed.gov/surveys/sdds/selectgeo.asp.

- Type of school: dummy for high school (in secondary school analysis).
- Class size: for elementary schools, average class size for grades K–3 and for grades 4–6 (two separate measures); and for middle and high schools, average class size of core academic classes.
- Teacher inexperience: percentage of a school’s teachers who have been teaching for fewer than three years.
- Teacher credentials: percentage of a school’s teachers who are fully credentialed.
- District spending: log of district total expenditure per student.
- Teacher salaries: log of teacher salary at “step 10” (an identifiable mid-level category).
- Administrative overhead: ratio of total spending on administrative positions to spending on teacher salaries.

Basic Findings

Let’s begin with a descriptive look at some of the key variables. Contract restrictiveness ranges from 0 to 6, with a mean of 3.23 and a standard deviation of .77. A common notion is that large districts tend to have more restrictive contracts than small districts do, and Figure 1A reveals as much: there is indeed a marked bivariate re-

FIGURE 1A Restrictiveness by District Size

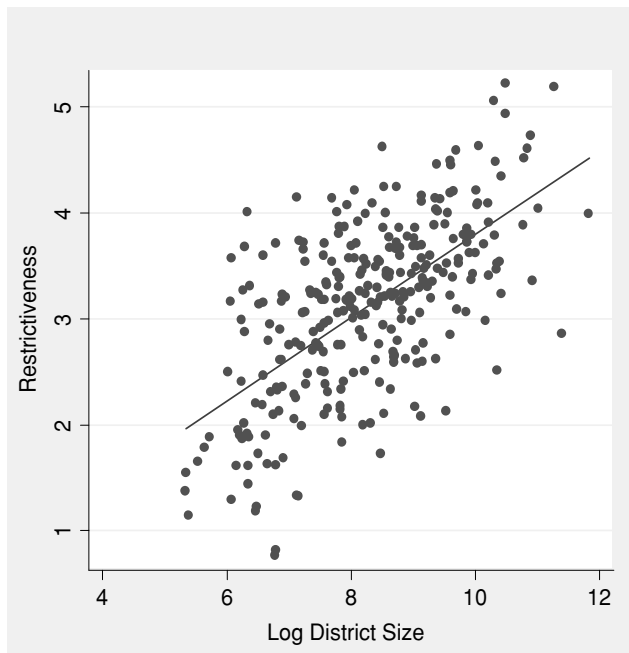
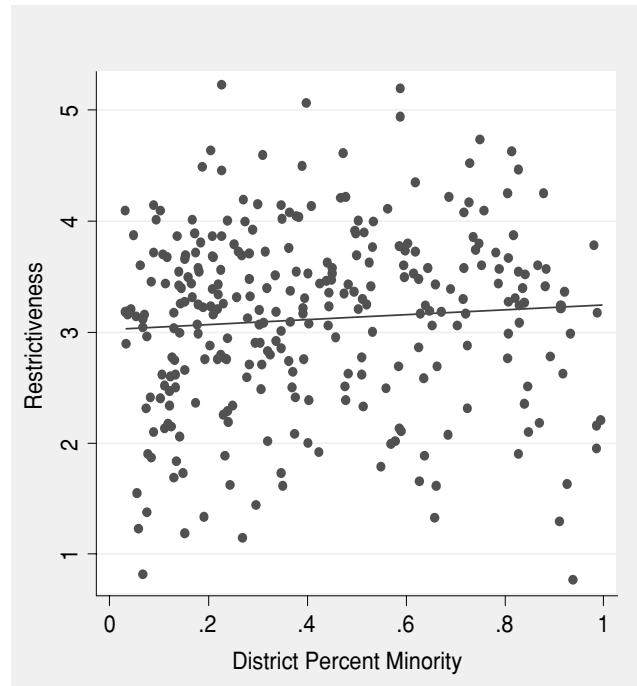


FIGURE 1B Restrictiveness by Minorities

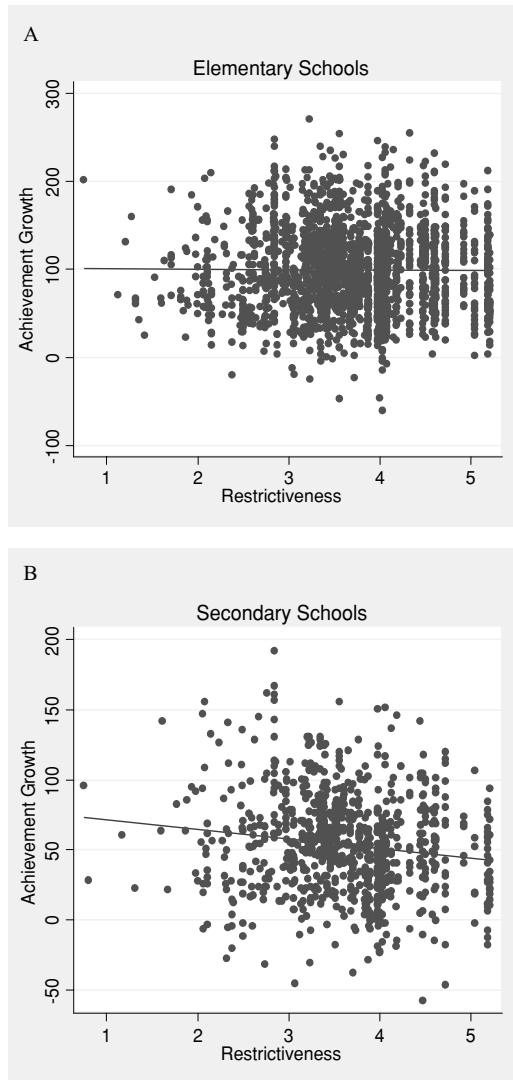


lationship between district size and restrictive contracts ($R^2 = .40$).

One might think that labor contracts would also be especially restrictive in districts with many minority students, if only because, in the nation as a whole, large districts tend to be high-minority. But in California this is not the case. Most minorities in California are Latino, and they are well represented in small as well as large districts throughout the state. As Figure 1B shows, there is only a slight relationship between minority enrollment and the restrictiveness of the contract. Generally speaking, minority kids in California are no more likely to attend rule-bound districts than nonminority kids are.

Finally, let’s take a look at the relationship that most concerns us here: the one between contract restrictiveness and the growth in student achievement. Because achievement growth in California has been higher among elementary schools than secondary schools, scatterplots are presented separately for each. Figure 2A shows that, for elementary schools, there is no bivariate relationship between restrictiveness and achievement at all. For secondary schools, Figure 2B shows a relationship consistent with our central hypothesis: contract restrictiveness is associated with lower achievement growth.

A look at the raw data is useful in getting the initial lay of the land. But simple two-way relationships of this sort may obviously be misleading, because they fail to take

FIGURE 2 Achievement by Restrictiveness

into account the effects of other variables. In the presence of controls, the relationship between restrictiveness and achievement growth may turn out to be quite different than these figures suggest.

The full model is estimated in Table 1. Impact scores provide a common metric for measuring the effect of each variable on school improvement. They answer the following question: by how many standard deviations does school improvement change when the relevant independent variable shifts from a low value (one standard deviation below its mean) to a high value (one standard deviation above)?

The results make it clear, as research has repeatedly shown, that the most powerful impacts on school per-

formance are the background characteristics of students. Controlling for initial levels of achievement, both elementary and secondary schools are more likely to make gains the lower the percentage of African American and Latino students, the higher the percentage of Asians, the lower the percentage on free and reduced-price meals (the measure of SES), and the lower the percentage having difficulty with English. As the impact scores indicate, the effects of these variables on achievement growth are considerable, in the range of .30 to .40 standard deviations of improvement. The impacts would be even higher, moreover, were the background variables not competing for influence, for they are partially measuring the same things. A school with many Hispanic students, for example, also tends to have many kids on free and reduced-price lunch and many with language problems. Were one or more of these variables omitted, the estimated impact(s) of the remaining variable(s) would be even larger, and the few with weak or near-zero impacts would rebound substantially.

As prior research also leads us to expect, the school and district variables are anemic by comparison. School size and teacher inexperience have statistically significant impacts at the elementary level. But their impacts, while in the right direction, are not significant for secondary schools; and for elementary schools, the change component of the size variable has a significant impact in the wrong direction. Most of the other school and district variables—district size, district spending, teacher pay, bureaucracy—do not have significant effects at either level.¹⁴

Two “significant” findings are exceptions, but probably spurious. Specifically, for both types of schools, improvement grows when class size goes up. Also, for elementary schools, achievement increases when district spending goes down. There is neither a research base nor a theoretical rationale to support either of these results, and we should not make much of them here.

Overall, the school and district variables are a disappointment, as expected. In light of this result, and in light of the literature’s consistent inability to document reasonable connections between the school organization and student achievement, it is notable that the collective bargaining contract does indeed seem to have such a connection. As Table 1 indicates, contract restrictiveness has a statistically significant impact on achievement growth in both elementary and secondary schools, estimated separately. The direction of the impact is what we ought to

¹⁴Chubb and Moe (1990) found that bureaucracy does have a significant impact on achievement, but their measure of bureaucracy was based on measures of administrative influence, whereas the measure employed here is based on spending. (Influence items are not available in the California data.)

TABLE 1 The Impact of Contract Restrictiveness on Achievement Growth

Variables	Elementary Schools			Secondary Schools		
	Coef.	St. Error	Impact	Coef.	St. Error	Impact
API, base year	-.31**	.075	-1.73	-.22**	.11	-1.48
API Squared	-.00*	.00	-.67	-.00	.00	-.38
District Size (log)	1.33	1.98	.07	-2.59	2.17	-.18
Δ District Size (log)	2.81	18.00	.01	31.67	21.07	.16
School Size (log)	-11.05***	3.40	-.16	-6.16	3.89	-.20
Δ School Size (log)	10.71*	5.47	.07	8.90	6.54	.09
% Black	-.56***	.12	-.32	-.80***	.16	-.52
% Asian	.43***	.08	.23	.29**	.12	.20
% Hispanic	-.04	.10	-.04	-.21*	.12	-.30
% Other Nonwhite	-.05	.17	.01	-.07	.14	-.03
Δ % Black	-2.62***	.41	-.39	-2.45***	.45	-.38
Δ % Asian	1.24***	.34	.20	.62*	.35	.12
Δ % Hispanic	-1.06***	.24	-.25	-.93***	.32	-.25
Δ % Other Nonwhite	-.55**	.24	-.08	-.49*	.27	-.12
% Free Meals	-.37***	.08	-.46	-.24*	.13	-.35
Δ % Free Meals	-.04	.04	-.04	.01	.14	.00
% English Learners	-.46***	.12	-.41	-.09	.18	-.08
Δ % English Learners	-1.31***	.16	-.36	-.87	.26	-.30
Rural	-11.29	7.16	-.24	-8.52	6.57	-.23
Suburban	-1.78	2.89	-.04	-4.02	3.64	-.11
% College Educated	43.48***	13.70	.23	25.17*	15.16	.19
Elementary District	-6.24	3.82	-.13	—	—	—
High School District	—	—	—	-1.01	4.90	.03
High School	—	—	—	-20.78***	3.61	-.58
Class Size, Grades K–3	1.92**	.93	.13	—	—	—
Δ Class Size, Grades K–3	2.13**	.84	.16	—	—	—
Class Size, Grades 4–6	.89**	.43	.11	—	—	—
Δ Class Size, Grades 4–6	.17	.30	.03	—	—	—
Class Size, Core Acad.	—	—	—	1.25*	.66	.21
Δ Class Size, Core Acad.	—	—	—	.70	.59	.11
% Inexper. Teachers	-.25*	.15	-.11	.18	.25	.08
Δ % Inexper. Teachers	-.48***	.13	-.20	-.20	.19	-.10
% Credentialed Teachers	.14	.21	.06	-.26	.29	-.13
Δ % Credentialed Teachers	.24	.16	.09	.07	.18	.03
District Spending (log)	-42.76*	22.00	-.14	-3.07	21.03	.01
Δ District Spending (log)	-13.42	24.38	-.03	17.81	20.84	.07
Teacher Salary (log)	19.80	19.22	.07	23.58	18.78	.11
Δ Teacher Salary (log)	-39.61	29.41	-.11	2.53	24.29	.01
Bureaucracy	-26.23	52.70	-.03	-30.46	48.21	-.06
Δ Bureaucracy	-33.32	54.16	-.03	17.14	44.53	.03
Contract Restrictiveness	-7.56***	2.55	-.24	-7.65***	3.26	-.32
Constant	548.02***	261.72	—	99.87	259.24	—
N Schools	1947	—	—	829	—	—
N Districts	241	—	—	250	—	—
Regression Adj. R ²	.56	—	—	.41	—	—

Dependent variable is the growth in the API score from 1998–99 to 2002–03. Statistical significance is indicated as follows: *** $p < .01$, ** $p < .05$, * $p < .10$. Analysis carried out in Stata with clustering on the school district. All tests are two-tailed except for the test on contract restrictiveness, as we are testing a one-sided hypothesis in that case. Except for dummy variables, “Impact” refers to the effect on API growth, in standard deviations, of a shift in the relevant independent variable from a low value (one standard deviation below its mean) to a high value (one standard deviation above). For dummy variables, “Impact” has the same meaning, except it captures the effect of a shift in the independent variable from 0 to 1.

expect: it is negative, making achievement gains more difficult. And its impact scores, $-.24$ for elementary schools and $-.32$ for secondary schools, are greater than those of any other organizational variables in the analysis.

One complication, which is inevitable, is that contract restrictiveness presumably affects student achievement by affecting the organization of schools and districts, represented here by such variables as class size, teacher inexperience, teacher credentials, and bureaucracy, which we are controlling for as potential determinants of achievement. By including these variables in the analysis, however, some of the impact of restrictiveness may be channeled through these factors, and not picked up in its own coefficient, which represents only its direct effect on achievement. It would take a much more extensive analysis to sort all this out—constructing models of class size, models of teacher inexperience, and so on—and I will leave such work to future research. For present purposes, I will simply point out that these other variables have precious little impact in this analysis on the outcome variable anyway. And when models are estimated in which these variables are excluded entirely, the impacts of contract restrictiveness on achievement remain virtually unchanged, as do their significance levels.¹⁵

Conditional Effects

So far, the estimation indicates that contract restrictiveness has a negative effect on student learning in both elementary and secondary schools, and that it is more influential than any other organizational variable in the model. The model we have employed, however, assumes that collective bargaining has an effect on student achievement that is the same under all conditions. This is the simplest assumption to make and a reasonable place to begin. But reality may be more complicated, and the impact of collective bargaining may actually vary depending on the conditions in which it operates.

I argued earlier that collective bargaining should tend to have negative consequences for achievement, at least on balance. This argument is rooted in fundamentals—

¹⁵Ideally, this would not be the case. It may occur here because the full array of important organizational variables is simply not represented or well enough measured. It may also be because the relationship between contract restrictiveness and some of these variables is conditional and/or nonlinear—which is surely the case for inexperienced and credentialed teachers, because transfer rules (a basic component of the restrictiveness index) may lead to more experienced and credentialed teachers in some schools (affluent ones) and less experienced and credentialed teachers in other schools (disadvantaged ones).

in the basic interests of unions and teachers—and its expectations are still at the core here. But if we go beyond fundamentals and consider specific conditions that might affect how collective bargaining actually operates, these core expectations could be modified. Collective bargaining could prove to be quite negative for achievement under some conditions, but not so negative under others, and even positive under still others—pointing the way toward a more finely grained understanding of union impact.

Because an exploration of conditional effects takes us into new territory that could easily get quite complicated, and because space is limited, I will focus on just two aspects of public schooling that seem good candidates for qualifying the impacts of collective bargaining. One is the size of the school district. The other is the minority composition of the school.

In California, as in most states, districts of all sizes engage in collective bargaining. But it is plausible to suspect that a given set of formal rules, including union contract rules, may in practice be less binding in smaller districts. For in smaller districts, the people involved in personnel decisions—district leaders, administrators, teachers—are more likely to know one another and function as a community. This being so, they may sometimes agree to bend formal rules when their enforcement would conflict with what is best for children. As districts get larger, relationships among participants should tend to get more impersonal and rule-governed, and contract rules are more likely to be followed even if the consequences for children are clearly not good. This argument may or may not be correct, but it is well worth considering, and it leads to a hypothesis about conditional effects: that the impacts of collective bargaining will vary with district size, and will be more negative the larger the district.

Now consider the minority composition of the school. A rationale that has gained attention in the research literature is that rule-based standardization by unions may be good for “average” children, but ill suited to children from disadvantaged backgrounds, who need more specialized attention (e.g., Eberts and Stone 1984, 1986; Milkman 1997). I am not persuaded by this argument, because union rules are pegged to the interests of teachers, not the needs of the average child. They easily come into conflict with the education of all children, not just those who are disadvantaged.

A more persuasive rationale is that the parents of children in high-minority schools are likely to be less affluent, less educated, and less politically active than parents of children in other schools, and thus are likely to be less influential—with district officials, with principals—in preventing the implementation of contract rules that

affect their own schools adversely.¹⁶ By this logic, high-minority schools may be more negatively affected by contract rules than low-minority schools, even if the contracts are equally restrictive. These effects may show up across districts, with the highest minority schools in the state being the most negatively affected. They can also show up within districts, where what counts is a school's minority composition relative to that of other schools in its same district.

Minority composition is also relevant because of its connection to how teachers get distributed across schools (within districts). Studies have shown that teachers tend to leave high-minority for low-minority schools, making it more difficult for disadvantaged schools to retain experienced, quality teachers (Hanushek, Kain, and Rivkin 2004; Levin, Mulhern, and Schunck 2005). Seniority-based transfer rights, which are common in the more restrictive contracts, exacerbate this problem: giving teachers the right to transfer from less desirable to more desirable jobs and preventing administrators from putting their best teachers in schools with the greatest needs. Thus, within districts, restrictive contracts may have more negative effects on schools that are relatively high in minority enrollment.

Finally, if the rationale behind the size hypothesis is valid—and thus if rules tend to be more binding in larger districts—then the conditioning effects of minority composition are themselves likely to vary with district size. It would be in the larger districts that the weak influence of minority parents would tend to burden their own schools with more onerous rules, and in the larger districts that transfer rights would have greater effect. If the logic is correct, district size has an overarching role to play.

Although tests for the conditioning effects of both district size and minority composition can be carried out within the same model, it is useful to begin with district size—because in a few quick steps, we are led to results that are quite remarkable and instructive. Step 1 involves a simple reestimation of the basic model employed earlier, augmented with a new term that interacts contract restrictiveness with district size. The findings, set out in the top portion of Table 2 (which presents results only for the key variables of interest here), show that the interaction term is negative and significant for both the elementary

¹⁶Note this is about the implementation of union rules, not their adoption. Collective bargaining occurs behind closed doors, and the rules contained in contracts are probably unknown to virtually all parents. Specific rules only become relevant to parents when they result in unpopular decisions that affect their own schools, and it is at this stage—the implementation stage—that one would expect differences in parent influence to have an effect.

and secondary school samples. It appears, then, that the effects of collective bargaining do indeed vary with district size and, as expected, become more negative the larger the district.¹⁷

But now suppose we separate out the larger districts from all the other districts, where the former are those enrolling more than 20,000 students. Reestimation should show the impact of restrictiveness to be more negative for the larger districts than for the others. And this is what we find—see Table 2—but with the surprising twist that, for the smaller districts, the impact of restrictiveness is essentially zero. This is the case, moreover, in both the elementary and secondary samples, estimated separately.¹⁸

These results are incomplete, of course, but they do suggest that contract restrictiveness may not make much difference for school improvement except in the larger school districts. The notion that contract rules are not entirely binding and can be gotten around when they conflict with the needs of students thus finds empirical support. This is all the more impressive given that, in the state of California, districts with fewer than 20,000 students make up some 94% of all districts.

But the flip side is impressive as well. While districts with more than 20,000 students are much fewer in number, they enroll a whopping 47% of all students in the state. And in these districts, the impact of contract restrictiveness on school improvement is much more negative than our earlier estimates implied: .44 standard deviations among elementary schools and .57 standard deviations among secondary schools. (The impacts were .24 and .32, respectively, in the prior analysis.) These effects are comparable in magnitude to those of student background characteristics, and tower over those associated with other aspects of school and district organization.

Given these findings, it makes sense to proceed by looking separately at the larger and smaller districts. In each case, the base model is augmented by interaction terms that test for whether the effects of restrictiveness are conditional upon the minority composition of the school and the size of the district.

¹⁷Note that, while the coefficient of the restrictiveness variable itself is positive in both equations, the total impact on school improvement of a change in restrictiveness is always negative, due to the (negative) contribution of the interaction term.

¹⁸While we are limited here by the relatively small number of districts in the sample with more than 20,000 students (39) and those with 10,000 to 20,000 students (39), regressions using different cut-off points suggest that 20,000 is about the level at which collective bargaining proves to make a difference.

TABLE 2 The Impact of Contract Restrictiveness on Achievement Growth, by District Size

Districts	Variables	Elementary Schools			Secondary Schools		
		Coef.	St. Error	Impact	Coef.	St. Error	Impact
All	Contract restrictiveness	11.81	11.32		23.44	14.17	
	Restrictiveness * district size	-2.01**	1.22		-3.31***	1.64	
	N Schools	1947			829		
	N Districts	241			250		
	Adj. R ²	.56			.44		
Larger	Contract restrictiveness	-16.31***	2.20	-.44	-15.65***	3.97	-.57
	N Schools	980			363		
	N Districts	32			33		
	Adj. R ²	.61			.54		
Smaller	Contract restrictiveness	-.91	2.63	-.02	-.41	2.63	-.02
	N Schools	967			466		
	N Districts	209			217		
	Adj. R ²	.57			.43		

Dependent variable is the growth in the API score from 1998–99 to 2002–03. Estimated model includes all the independent variables listed in Table 1. Statistical significance is indicated as follows: *** $p < .01$, ** $p < .05$, * $p < .10$. Analysis carried out in Stata with clustering on the school district. The test for contract restrictiveness in the first regression is two tailed, because the presence of the interaction term means that it could either be positive or negative and still be consistent with the hypothesis of an overall negative effect. The tests for all other variables are one tailed, as the hypothesis in each case is one-sided: that the coefficient is negative. “Impact” refers to the effect on API growth, in standard deviations, of a shift in the relevant independent variable from a low value (one standard deviation below its mean) to a high value (one standard deviation above). Impacts are not calculated for the first regression because the impact of restrictiveness on API growth depends on district size, and thus varies.

- (1) District size is included to allow for the possibility that, even within size categories, size may have a more finely grained conditioning effect that still needs to be taken into account.
- (2) Minority composition is defined by reference to the percentages of African American and Latino students in the school. As these are separately controlled for in the basic model, one way to proceed here is to include separate interaction terms for each minority group. A simpler approach is to combine them into a summary interaction term, using percent minority. I estimate models for both.
- (3) The above terms measure the “absolute” minority composition of the school. The “relative” minority composition of the school is measured as the difference between a school’s percent minority and the percent minority of the median school in the district.¹⁹

I should note that “relative” minority composition, whose postulated effects derive (in effect) from intradistrict competition among schools for resources, is at a

disadvantage here. By far the best test of its impact occurs in the elementary-level analysis of large districts, because its median district has 26.5 elementary schools—plenty for competition. In the secondary-level analysis, by contrast, even the larger districts have very few schools—a median of six middle schools and four high schools—and any competition is limited anyway: teachers typically don’t transfer from middle schools to high schools, or vice versa. For the smaller districts, the situation is much worse. In the secondary analysis, there is a median of just one middle school and one high school per district, which rules out most competition; and in the elementary-level analysis, the smaller districts have a median of just four elementary schools—better, but not by a lot. Although I will include the “relative” minority composition variable in all these analyses, it is in the elementary analysis of larger districts that the test is most meaningful.

Now let’s turn to the findings, set out in Table 3 (which, again, presents only the key variables). Consider the results for elementary schools in the larger districts.²⁰ The key interaction terms behave as expected. In Model 1,

¹⁹The main effect here is now included as a control variable, as it was not part of the original equation.

²⁰As in the prior table, the positive coefficient on the restrictiveness variable itself does not mean that the overall impact of a change in restrictiveness is positive, for the overall impact also depends on

TABLE 3 Testing for Conditional Effects—Does the Impact of Contract Restrictiveness Depend on Minority Composition and District Size?

Dist. Size	Variables	Elementary Schools				Secondary Schools			
		Model 1		Model 2		Model 1		Model 2	
		Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Larger	Restrictiveness	108.88**	42.96	117.34***	39.12	-1.66	84.45	-4.42	90.00
	Restrictiveness * black	-.32**	.18	—	—	-.18	.19	—	—
	Restrictiveness * Hispanic	-.14*	.10	—	—	-.25**	.11	—	—
	Restrictiveness * minority	—	—	-.19***	.08	—	—	-.23***	.09
	Restrictiveness * relative minority	-.18*	.11	-.16	.12	.16	.16	.17	.16
	Restrictiveness * log district size	-10.64***	4.04	-11.45***	3.66	-.52	7.95	-.24	8.51
	N Schools	980		980		363		363	
	N Districts	32		32		33		33	
	Adj. R ²	.63		.62		.54		.54	
Smaller	Restrictiveness	-22.35	17.35	-16.84	17.14	-32.31	19.98	-28.68	19.48
	Restrictiveness * black	-.65*	.44	—	—	-.43	.56	—	—
	Restrictiveness * Hispanic	-.05	.08	—	—	.09	.07	—	—
	Restrictiveness * minority	.19	.18	-.06	.08			.08	.07
	Restrictiveness * relative minority	—	—	.18	.19	-.07	.35	-.11	.35
	Restrictiveness * log district size	3.00	2.15	2.12	2.10	3.55	2.41	2.93	2.29
	N Schools	967		967		466		466	
	N Districts	209		209		217		217	
	Adj. R ²	.58		.58		.43		.43	

Dependent variable is the growth in the API score from 1998–99 to 2002–03. Estimated model includes all the independent variables listed in Table 1. Statistical significance is indicated as follows: *** $p < .01$, ** $p < .05$, * $p < .10$. Analysis carried out in Stata with clustering on the school district. The test for the contract restrictiveness variable itself is two tailed, because the presence of the interaction term means that it could either be positive or negative and still be consistent with the hypothesis of an overall negative effect. The tests for all the interaction terms are one tailed, as the hypothesis in each case is one-sided: that the coefficient is negative.

the impact of contract restrictiveness on achievement growth is *more* negative the higher a school's percentage of African Americans, the higher its percentage of Latinos, the higher its minority composition relative to other schools in the district, and the larger the district is. All these conditioning impacts are statistically significant. In Model 2, which uses percent minority rather than the two separate minority terms, the conditioning effect of a school's "absolute" minority composition remains negative and statistically significant, as does district size; the effect of the "relative" measure of minority composition remains negative as well, but it drops a bit and just loses its statistical significance.

The findings are quite different for elementary schools in smaller districts. In Model 1, the effects of restrictiveness are significantly more negative for African American students, but this is the only significant in-

teraction in either Model 1 or Model 2—and the vast majority of minority kids in California are Latino, not African American. If we look at the impact of percent minority in Model 2, it is negative, but it is statistically insignificant and much smaller in magnitude than the corresponding negative impact in the larger districts. The coefficient of the "relative" minority composition variable (at a disadvantage here) is positive and insignificant, as is the coefficient for district size. Thus, it is in the larger districts that we find conditioning effects for minority composition (both absolute and relative) that are in line with what we would expect, while in the smaller districts not much of any consequence is happening, except for African American students.

The results are similar for secondary schools in smaller districts. The coefficient for African American students is big and negative; and although statistically insignificant, it raises a red flag—along with the elementary results—that union rules may work against the achievement of black students even in smaller districts.

the (negative) contributions of the interaction terms. See Table 4 for the calculation of impacts.

TABLE 4 Impact Coefficients for the Larger Districts

Type of School	Minority Disadvantage		District Size	Impact
	Minority	Disadvantage	Size	
Elementary	High	High	High	-.61
	High	Low	High	-.50
	High	High	Low	-.39
	High	Low	Low	-.29
	Low	High	High	-.29
	Low	Low	High	-.18
	Low	High	Low	-.08
	Low	Low	Low	.03
Secondary	High			-.84
	Low			-.40

The data are from the largest districts, as defined in the text. The impact coefficient indicates the effect on achievement growth, in standard deviations, as contract restrictiveness moves from one standard deviation below its mean to one standard deviation above, given that the stipulated conditions obtain. A “high” value for the relevant condition represents the score of a school at the 75th percentile on that condition, and a “low” score represents the score of a school at the 25th percentile.

In other respects, however, restrictiveness shows no impacts; all the variables are statistically insignificant. For secondary schools in larger districts, by contrast, minority composition clearly does have an impact. In Model 1, the coefficient for Latinos is negative and statistically significant—an important result, given the prevalence of Latinos in California schools. And although the coefficient for African Americans does not achieve significance (a reflection of their low numbers), it is negative too and almost equal in size. When minority composition is simply measured as percent minority, as in Model 2, its coefficient is negative and highly significant. “Relative” minority composition is insignificant in both models, but is perhaps to be expected given the small numbers of secondary schools within most districts.

Table 4 illustrates how the impact of collective bargaining varies with district size and minority composition, based on the estimates in Model 1 for the larger districts.²¹ The most benign effects arise when schools are in districts at the low end of the size distribution, lowest in minority enrollment, and most advantaged relative

²¹At the elementary level, calculations are carried out for eight different sets of conditions: depending on whether the district is near the lower or higher end of the size distribution, whether the school is low minority or high minority in composition, and whether it is advantaged or disadvantaged relative to other schools in the district based on its minority enrollment. For each variable, the lower cutoff point is the 25th percentile of the larger-district sample; the upper cutoff point is the 75th percentile of the larger-district sample.

to other schools in their districts. With this combination, the impact of collective bargaining is close to zero (at .03). As conditions change—as districts get larger, as schools enroll more minorities—the effects of collective bargaining get more and more negative, reaching a substantial $-.61$ for schools that are in the largest districts, highest in minority composition, and most disadvantaged within their districts. A perusal of the table shows that this total change in impact (from .03 to $-.61$) can be broken down as follows: $-.21$ of it is due to the increase in district size, $-.32$ is due to the increase in minority composition, and $-.11$ is due to the increase in intradistrict disadvantage. We have to remember, however, that these calculations are all based on schools in the larger districts, and that the impact of collective bargaining for schools in the smaller districts is essentially zero. The conditioning effect of district size, therefore, is much greater than the numbers in Table 4 suggest.

For secondary schools, the calculations are simpler, because the earlier estimation showed that district size does not have additional impacts within the set of larger districts, and that a school’s “relative” minority composition does not matter either. Thus, we need only carry out the calculations for changes in minority composition.²² When this is done, as Table 4 indicates, the impact of collective bargaining on achievement growth is a substantial $-.40$ even for low-minority schools, and it jumps to a much more negative $-.84$ for those that are high minority. In general, not only does minority enrollment make a big difference here, but it also appears that the negative effects of collective bargaining are much greater for secondary schools than for elementary schools. A high-minority secondary school is more seriously affected by restrictive labor contracts than a high-minority elementary school.

In sum, then, the findings indicate that the impact of collective bargaining on achievement gains is not constant across schools, but depends on the size of the district and the minority composition of the school. This is true for both elementary and secondary schools, estimated separately. In each case, contract restrictiveness appears not to have much effect in the vast majority of school districts, but in the larger districts—which enroll roughly half of the state’s students—it has a very negative impact indeed, especially at the secondary level, and the magnitude is greater for high-minority schools.

²²This is carried out using Model 1 from Table 3, assuming that district size is at its median (within the set of larger districts) and that the “relative” minority composition variable is equal to zero (and thus at the median in minority composition for the school’s district).

Conclusion

This article is about public education, but it illustrates a phenomenon of great relevance to much of American government: that ordinary public employees, by getting organized for collective action, can wield power in pursuit of their own special interests—and in so doing, can have important impacts on the policies, organization, and performance of government.

These impacts are brought about by two means. First, public sector unions participate actively in politics, taking advantage of huge memberships, deep financial pockets, and extensive political organization to bring their interests to bear in electoral and policymaking arenas at all levels of government. And second—the subject of study here—these same unions represent their members in collective bargaining, and use their power to press for rules that become part of government organization—and embed their interests in its structure. These two realms of union activity are symbiotically connected: collective bargaining is the source of the members, money, and organization that translate into political power, but a prime use of that power is to protect and enhance their collective bargaining rights, and to elect sympathetic officials who will help them achieve their contract objectives. Collective bargaining promotes political power. Political power promotes collective bargaining.

Although public sector unions have been actively engaged in both avenues of influence for more than three decades now, students of American government have paid little attention to them. The literature on bureaucracy recognizes that ordinary bureaucrats can gain power by having expertise unavailable to superiors, and that bureaucratic leaders can gain power by taking entrepreneurial action of various sorts. But scholars have little to say about the capacity of ordinary bureaucrats to exercise power through collective action. Similarly, the literature on interest groups focuses on a vast array of groups that arise outside of government to promote the interests of their constituencies, but not on groups that arise from inside government to pressure on behalf of public employees. Groups like the National Education Association (NEA), the American Federation of State, County, and Municipal Employees (AFSCME), and the Service Employees International Union (SEIU)—which have millions of members and are major forces in American politics at all levels, as well as in collective bargaining—are barely on political scientists' radar screens. The same is true of smaller unions—of police officers, firefighters, nurses, prison guards, and many more—that clearly play important political and collective bargaining roles in their own policy realms at the state and local levels, but are rarely studied.

This is an oversight with important consequences. Some of the consequences are intellectual: it is the job of political scientists to understand government, and they cannot do this adequately if they fail to recognize and explore the collective power of the government's own employees. Some of the consequences are more substantive: attempts by policymakers to build effective public agencies, or to improve those that are not effective, must rest on an understanding of the determinants of agency performance—and this is not possible if public sector unions are left out of the equation, as though they are somehow irrelevant to how government is organized and does its work.

Public education is a case in point. Since the early 1980s, improving public school performance has consistently been a top priority of American government. Attention has especially centered on large, urban school districts, which have high concentrations of minorities and are often abysmally ineffective at educating them. Many factors contribute to these problems. But it is only reasonable to ask whether public sector unions, by imposing structures that are not designed to promote academic achievement, are at least *part* of the explanation for why the schools are not effective and why they are so difficult to improve. There can be no answer if the question is not even raised, and if political scientists continue to think about government in ways that have nothing to do with the collective power of public employees.

The analysis of this article is not definitive, of course, but it moves the ball downfield. First, it suggests that teachers unions do matter for school performance. In particular, it supports (with qualification) the core expectation that, because union and teacher interests are not aligned with the educational interests of children, the restrictions built into labor contracts should on balance tend to have negative consequences for academic achievement. Second, in exploring whether the effects of collective bargaining may depend on the conditions in which it operates, the analysis offers a more finely grained understanding of the connection between collective bargaining and the schools.

- Collective bargaining appears to have a strongly negative impact in the larger districts, but it appears to have no effect in smaller districts (except possibly for African American students—which is important indeed if true). This supports the conjecture that, because smaller districts are more like communities and larger districts more bureaucratized, formal contract rules are more likely to be enforced—and to have consequences—in the larger districts.

- Among the larger districts, collective bargaining has more negative effects for high-minority schools than for other schools. This supports the argument that minority parents are less influential and less able to stop the application of rules that aren't good for their schools.
- Although the findings are weaker on this count, the best evidence indicates that the impact of collective bargaining is especially negative for schools that are "relatively" high minority within a given (larger) district. This supports the argument that restrictive contracts put high-minority schools at a disadvantage in the competition for teachers and resources within districts.

If these findings are on the mark, their substantive implications are important. For they tell us that collective bargaining does have negative consequences for student achievement, and that the effects are concentrated on precisely those districts and schools—large districts, high-minority schools—that, over the years, have been the worst performers and most difficult to improve. It follows that efforts to boost achievement in these contexts, as well as to reduce the achievement gap between whites and minorities, need to recognize that collective bargaining may be part of the problem—and that it deserves to be taken seriously as a target of reform. There is no magic bullet here. But the evidence suggests that reducing the restrictiveness of labor contracts could have significant payoffs for public education.

More research is needed, of course, to be confident about these findings and implications. That is always the case for any empirical project, but especially one on a subject that, like this, is so little studied. The greater need, however, is for broadly based research on the power of public sector workers generally—in collective bargaining, in elections, in the policy process—and for theoretical perspectives that link their power to an understanding of government.

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