HOW TEACHERS' UNIONS AFFECT EDUCATION PRODUCTION*

CAROLINE MINTER HOXBY

This study helps to explain why measured school inputs appear to have little effect on student outcomes, particularly for cohorts educated since 1960. Teachers' unionization can explain how public schools simultaneously can have more generous inputs and worse student performance. Using panel data on United States school districts, I identify the effect of teachers' unionization through differences in the timing of collective bargaining, especially timing determined by the passage of state laws that facilitate teachers' unionization. I find that teachers' unions increase school inputs but reduce productivity sufficiently to have a negative overall effect on student performance. Union effects are magnified where schools have market power.

I. INTRODUCTION

This study is motivated by two related empirical puzzles. The first is that student-level and school-level data often show little evidence of a relationship between student performance and school inputs, after controlling for the student's background [Hanushek 1986; Betts 1995; Grogger 1995].[1] The second is that metropolitan areas with few opportunities for competition among public schools tend to have more generous school inputs—including higher per-pupil spending, higher teacher salaries, and lower student-teacher ratios—but also tend to have worse student performance [Hoxby 1995a]. These empirical results suggest the existence of some school characteristic that tends to increase inputs while tending, at the same time, to lower the effectiveness of each input. Teachers' unions, while not the only candidate for this role, are worth examining since they try to obtain more generous inputs and have the potential to change the efficacy of inputs. Since teachers' collective bargaining is a phenomenon of the past 35 years, the evidence presented not only helps to explain the empirical puzzles mentioned, but can potentially explain differences between studies of school inputs based on cohorts educated prior to 1960, such as Welch [1966], Johnson and Stafford [1972], and Card and Krueger [1992a, 1992b], which often find significant improvement in student performance, and those based on cohorts educated after 1960, such as the studies cited initially, which do not find improvement.

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1. For competing surveys of the literature with different explanations of this result, see Betts [1996] and Card and Krueger [1996].

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How teachers' unions affect the educational production function is an empirical question and an open one at that. Theory suggests two reasons teachers might demand a union. The first assumes that teachers maximize the same objective function as parents, student achievement, but that informational and market imperfections lead teachers to desire different school input levels. These different desires may reflect teachers' superior information about student needs or teachers' superior ability to internalize externalities in education production. The second reason for teachers to demand a union is that they have a different objective function than parents or administrators, presumably one in which school policies that directly affect them, such as teacher salaries, receive greater weight than policies that only indirectly affect them by affecting student achievement. A rent-seeking teachers' union can militate for school inputs that maximize the objectives of teachers, rather than those of parents or administrators.

Thus, teachers' unions may affect the educational production function through at least three channels. First, under either model, unions are expected to change—probably increase—the overall budget that funds school inputs. Second, unions are expected to reallocate any given budget among alternative inputs. This reallocation will generally be efficiency enhancing if the union's different objective reflects superior information but efficiency reducing if the union is rent seeking. Finally, because teachers interact with inputs to produce education, unions may affect the productivity of each input. For instance, if the union conveys superior information and class size reflects teachers' preferences as a result, then teachers may plan on smaller class size and make better use of it. In contrast, if the union performs a rent-seeking role and protects incumbent teachers from outside teachers competing for better-paid jobs, then a teacher salary increase may be less productive in a unionized school than in a nonunion school. All of the potential effects of unions on schools are expected to be magnified when the market for schooling is

imperfectly competitive. This is because monopoly rents will be available for rent-seeking unions and less information will be conveyed by the market in the absence of active choice among schools by parents and teachers.

An empirical study of the effect of teachers' unions on education production functions faces four major obstacles. The first obstacle is obtaining data that have information on unionization, student achievement, and demographics for a large, representative sample of schools at multiple points in time that span the era of unionization (1960 to the present). None of the traditional sources of school data contain this information. I use the Census of Governments (1972, 1982, and 1992) to get panel data on unionization for every public school district in the United States. I match these to data from the decennial Censuses of Population and Housing, summarized by school districts. School district tabulations are available from the Census Bureau for the 1980 and 1990 censuses, but I matched census blocks and enumeration districts to school districts for the 1970 census myself. The resulting panel data on school districts are unique in coverage. The second obstacle is that teachers' unionization is difficult to measure and define because teachers' organizations, such as the National Education Association (NEA), perform union functions such as collective bargaining in some schools while remaining purely professional associations in other schools. I greatly reduce error in the measure of collective bargaining by defining as unionized only those schools where (1) collective bargaining was reported, (2) a contractual agreement existed as the result of collective bargaining, and (3) a teachers' organization of which at least 50 percent of teachers were members was reported. This refined definition avoids error associated with questions like, "Are teachers unionized?" which appear to be frequently misunderstood. The third obstacle is relating the effects of teachers' unions to measures of competition among schools. I leave this issue for the final section of the paper.

The fourth and most serious obstacle is the identification problem caused by the difficulty of differentiating between the effects of a union on a school and the characteristics of a school that makes a union more likely to exist. Even after controlling for observable characteristics of a school district such as demographics, there are presumably unobservable school characteristics correlated with unionization. The unobservable school characteristics that promote unionization may themselves affect

the education production function. For instance, unions may be more likely to form if administrators are incompetent. I attempt to solve the identification problem by several means. First, I try to reduce the omitted variables problem by using detailed demographic information about each school district from the decennial Censuses of Population and Housing. Second, I use first-differences to eliminate school district characteristics that remain constant over time.

My third, and probably best, attempt to solve the identification problem combines differences-in-differences and instrumental variables estimation. The union effect is identified by differences in the timing of unionization, and these timing differences are instrumented by the timing of state laws that facilitate teachers' unionization. The intuition is as follows. Using differences-in-differences, we subtract the last period's changes from this period's changes. This eliminates not only unobservable variables that are constant over time, but also unobservable variables that have constant time trends. Relatively abrupt changes are what remains. The unobservable school characteristics that cause teachers to unionize are likely to be variables that are either roughly constant or have a roughly constant trend over time. At some point, these variables reach a threshold so that the teachers actually unionize and begin collective bargaining. The nature of unionization is that it is a discrete change. For example, a teachers' organization with 49 percent support might not have the right to represent teachers in collective bargaining. The same organization with 51 percent support might be able to represent all teachers, including nonmembers. We expect the discrete event of unionization to bring about new time trends in school inputs and the education production function. Focusing on the event of unionization would be more justified if there were some other discrete event, exogenous to the circumstances of any individual school district, that promoted the discrete event of unionization. State law changes are such events. The forces that promote laws to facilitate public sector bargaining gather strength continuously over time, yet the laws create a discrete change in the ease with which teachers can unionize. Combining differences-in-differences with instrumenting for the timing of unionization with the timing of state law changes means that (1) time-invariant and steadily trending school characteristics and state characteristics that may affect both education production and unionization/law changes are eliminated and (2) only that part of the discrete event of unionization that is correlated with discrete

law changes is used to identify the effects of unionization. This identification strategy is discussed in detail in Section V.

This study continues a large literature on teachers' unions. Eberts and Stone [1986, 1987] use the Sustaining Effects Survey and the High School and Beyond survey, which ask whether teachers are unionized. In the 1987 study they examine changes in students' test scores over time and find that teachers' unions improve productivity. In the 1986 study they infer that schools with teachers' unions have worse productivity. In both studies, however, Eberts and Stone use unionization status at a point in time. They do not observe individual schools before and after unionization. Kleiner and Petree [1988], using state aggregate data from 1972 to 1982, relate teachers' unionization to school resources, SAT scores, and graduation rates. Using both cross-sectional and first-differenced regressions, they find more generous resources and higher student performance in states where a greater share of teachers are unionized. However, Peltzman [1995] uses first-differences on state aggregate data from 1970 to 1991 and finds a negative relationship between a state's unionization and its students' AFQT scores.[2] Since the Peltzman and Kleiner and Petree studies use similar methods on aggregate data, the conflict in their results appears to be due to differences in their unionization measures or the difference in the demographic variables for which they control. In any case, there is clearly little consensus on teachers' unions. This study attempts to build consensus by extending the best features of previous studies—for instance, greatly expanding coverage of individual school districts and using richer longitudinal methods. This study also introduces refined measures of unionization and new strategies to deal with potential endogeneity in unionization. One goal is to clarify the contentious question about whether and why school inputs do or do not matter. Another is to illuminate the mechanism that links competition among schools to better student achievement and school productivity.

II. Teachers' Unions and the Education Production Function

I denote as "rent-seeking" the model in which teachers' unions prefer different inputs than parents do because the

2. The pre-1984 literature largely examined teacher salaries and class size rather than student achievement or productivity. For examples, see Kasper [1970], Hall and Carroll [1973], and Baugh and Stone [1982].

union's objective is not purely maximization of student achievement. I denote as "efficiency-enhancing" the model in which unions prefer different inputs because they have the same fundamental objective as parents but (1) have superior information about students and input efficacy or (2) internalize externalities in education production that parents neglect. The rent-seeking and efficiency-enhancing models have different implications for education production functions, which may operate through three routes. Consider Figure I, which shows iso-achievement curves for student achievement (denoted "iso-A"). The examples of inputs used are teacher salaries and books. Given a school budget and input prices, the tangency between the budget constraint and the iso-achievement map determines maximum student achievement and the optimal allocation of the budget among inputs. For the remainder of this section, I simply assume that unions are able to raise budgets by exercising market power over teacher inputs. In the final section of this paper, I relate unions' ability to exercise market power to the structure of the local schooling market.

Figure I shows the case of a rent-seeking union. A teachers' union, by monopolizing the services of incumbent teachers and creating a political coalition in local elections, may be able to increase the budget and move to previously inaccessible points. Thus, the first possible effect of teachers' unions is through the budget (point A to point B). Even if the union is rent seeking, such an increase in the budget may be social welfare enhancing. This would occur if budgets are otherwise too low owing to uninternalized positive externalities associated with schools or imperfect capital markets for human capital investments (liquidity-constrained parents). The second possible effect of teachers' unions is through allocation of the budget among inputs. If teachers have an objective function that maximizes their utility rather than student achievement, then there is a set of teacher indifference curves (denoted "ID-tchr") whose curvature reflects the direct utility to teachers of certain inputs, such as higher salaries. For any given budget, teachers' indifference curves determine a level of student achievement that is lower than the maximum feasible student achievement (point B to point C). The third possible effect of unions is through the productivity of measured school inputs. Actual school inputs always include a degree of teacher effort or teacher quality, though measured school inputs do not. For instance, the actual school input should be "intensity

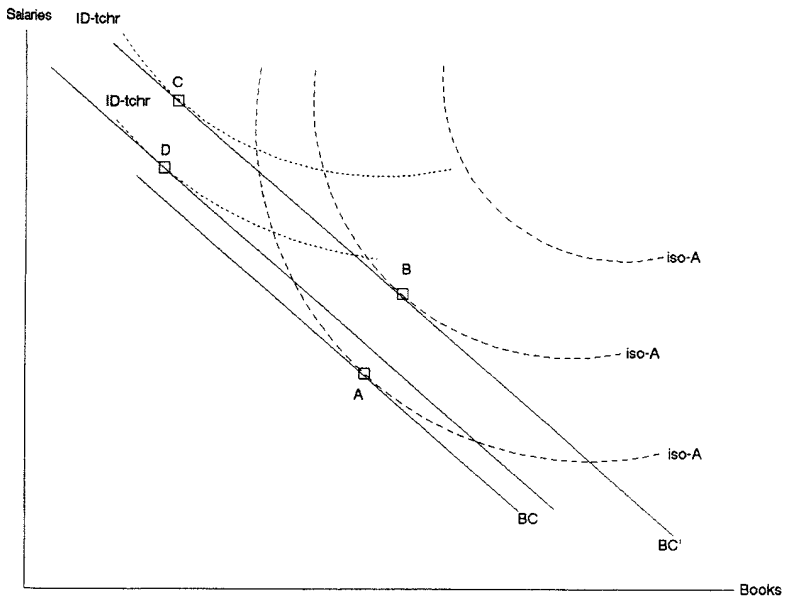

FIGURE I
Input Choices under a Rent-Seeking Union

of student-teacher interactions," which includes teacher effort, but the measured school input is class size or student-teacher ratio, which does not. If unions reduce teacher effort for any given mix of measured school inputs, then the budget effectively shrinks since the price of an actual unit of school input rises with unionization (point C to point D).

Now consider an efficiency-enhancing teachers' union that maximizes student achievement and uses its "voice" [Freeman and Medoff 1984] to announce teachers' superior knowledge (Figure II). Such a union might use its monopoly or political power or both in local elections to increase the school budget to the point that is optimal when positive externalities of education are accounted (point E to point F). Teachers may have more accurate knowledge about the productivity of school inputs or the interactions of school inputs, so that teachers see a different iso-achievement map than parents see (denoted "iso-A (tchr)"). The union chooses a different mix of inputs accordingly (point F to point G). Finally, if a union empowers teachers or makes them behave more professionally (as suggested by union leaders), it

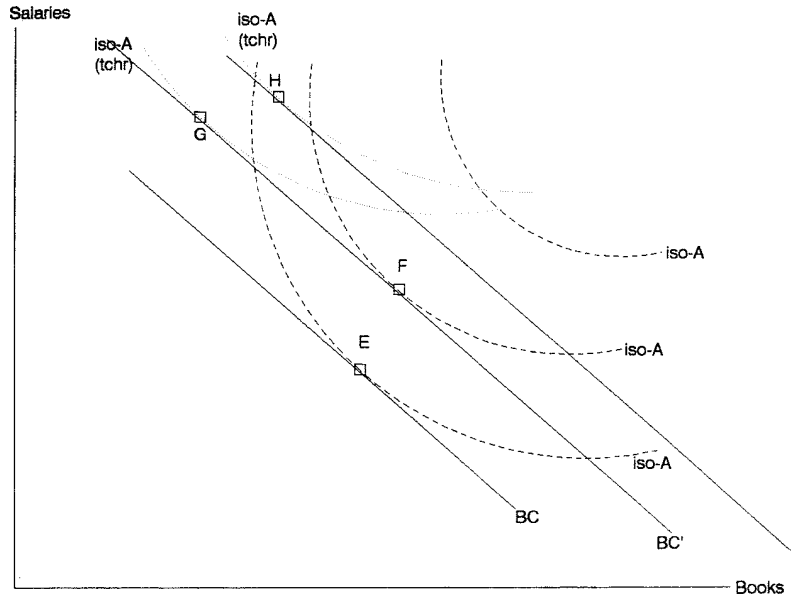

FIGURE II
Input Choices under an Efficiency-Enhancing Union

may enhance the effort they contribute to any given set of measured school inputs. Increased teacher efforts increase actual school inputs and effectively expand the budget constraint (point G to point H).

In terms of equations, suppose that a linear approximation to the education production function is

$$A_i = X1_i\alpha_1 + \ldots + XN_i\alpha_N + e_i\beta + Z_i\delta + \xi_i, \tag{1}$$

where A_i is student achievement in school district i, $X1_i$ through XN_i are school inputs, e_i is (unobservable) teacher effort, Z_i is a vector of school district demographic variables that affect achievement, and ξ_i represents unobservable school district characteristics. Equation (1) is subject to the budget constraint

$$X1_ip_1 + \ldots + XN_ip_N = Y_i, \tag{2}$$

where Y_i is the school budget, and p_1 through p_N are the prices of school inputs.

Let U_i be a (0,1) indicator for teachers' unionization, W_i be a vector of school district demographic variables that affect school

inputs and school budgets (possibly $W_i = Z_i$), and f_1 through f_N and g be functions. A union's possible effects can be summarized by four types of equations. The first type of equation,

(3)
$$X1_i = \gamma_1 U_i + \kappa_1 W_i + \varepsilon 1_i, \ldots, \qquad XN_i = \gamma_N U_i + \kappa_N W_i + \varepsilon N_i,$$

states that measured school inputs are linear functions of unionization. The second,

(4)
$$Y_i = \lambda U_i + \mu W_i + \zeta_i,$$

states that the budget is a linear function of unionization. The third type of equation,

(5)
$$\alpha_1 = f_1(e_i), \ldots, \alpha_N = f_N(e_i), \qquad f_1'(e_i), \ldots, f_N'(e_i) > 0,$$

states that the productivities of measured school inputs are functions are teacher effort. The final equation,

(6)
$$e_i = g(U_i),$$

states that teacher effort is itself a function of unionization.

If a union is rent seeking, we expect that

(7)
$$\lambda > 0,$$

(unionization raises the budget);

(8) $\gamma_j > 0$ for those inputs X_j that benefit teachers; and

(9)
$$g' < 0 \rightarrow \frac{d\alpha_j}{dU} < 0,$$

(unionization reduces teacher effort and input productivity).

If a union is efficiency enhancing, we expect that

(10)
$$\lambda > 0,$$

(unionization raises the budget);

(11) $\gamma_j > 0$ for those inputs X_j that teachers believe to be more productive than nonteachers believe them to be; and

(12)
$$g' > 0 \rightarrow \frac{d\alpha_j}{dU} > 0,$$

(unionization increases teacher effort and input productivity).

Because we (1) expect increased budgets under either type of union and (2) cannot distinguish easily between inputs that simply benefit teachers and inputs that parents undervalue, the productivity of school inputs is the distinguishing feature that separates voice and rent-seeking behavior. This is why estimating productivity, by relating unionization to student achievement as well as to inputs, is crucial.

III. Teachers' Unions in the United States

Teachers' unions, like other public sector unions, are largely a post-1960 phenomenon in the United States. (See Murphy [1990] for a good history of teachers' unionism.) Yet, teachers' unionism has a somewhat unique and confusing history because teachers' unions were formed by *converting* existing teachers' professional associations. The teachers' unionization movement began when the teachers' professional associations of a few large, central city districts began to use union tactics, such as strikes, though they were not recognized as unions per se. One of these was the progenitor of the American Federation of Teachers (AFT) which signaled its status as a union by AFL-CIO affiliation and which grew by converting professional associations and absorbing independent unions. The activities of the AFT induced the National Education Association (NEA), which had long served as the major professional organization for teachers, to begin performing collective bargaining and otherwise functioning as a union in some schools. Currently, schools are unionized on a district-by-district basis and most teachers' unions are affiliated with the AFT or NEA. Nearly all public school districts have a teachers' organization, but many schools with organizations still effectively remain nonunion.

A. Measuring Unionization

In 1963, 93 percent of school districts reported a teachers' organization. However, most of these organizations acted in a purely advisory capacity, and about one-half had no formal relationship with the school board. Just 10 percent had explicitly agreed to "meet and confer," and only 1 percent had a collective bargaining agreement.[3] Even by 1966 only 8 percent of school dis-

3. Computed by the author using statistics from Perry and Wildman [1966].

tricts were bound by collective bargaining agreements, and these were mainly central city districts in Michigan, Massachusetts, and Rhode Island. Table I shows school district reports of teacher organization and unionization from 1963 to 1992. The table demonstrates that, if unrefined definitions of unionization are used, measurement error produces wide variation in estimates of union activity. For instance, in 1992 all districts (100 percent) reported that some teachers belonged to a teachers' organization, yet only 59 percent of districts reported *at least* a memorandum of understanding. A memorandum of understanding is the outcome of "meeting and conferring," which is the least binding type of formal negotiation between administration and union. So, many schools with teachers' organizations had little or no formal negotiation. Similarly, 54 percent of districts reported that collective bargaining occurred, yet only 36 percent reported that a contractual agreement between the teachers' union and administration existed (or had existed previously, for schools in the midst of negotiations) and that at least 50 percent of the teachers were members of the teachers' organization.[4] Since contractual agreements are the outcome of successful collective bargaining and collective bargaining generally will not occur unless at least 50 percent of the teachers are union members, the natural inference is that some respondents do not differentiate (1) between unionization and the possibility of collective bargaining or (2) between collective bargaining and less binding forms of negotiation.

In this study I will consistently use the strict definition of unionization (collective bargaining, contractual agreement, and 50 percent union membership), represented by the right-hand column of Table I. When I use a more permissive measure of unionization, such as just "collective bargaining is the form of labor relations" (represented by the middle column of Table I), results are similar though attenuated, as expected.[5]

The AFT is generally attributed to behave more aggressively than the NEA. I show elsewhere [Hoxby 1995b] that many of the apparent differences between the behavior of AFT and NEA affiliates disappear when the definition of unionization is refined.

4. Since schools that unionize tend to have larger student populations than schools that do not unionize, these numbers understate the number of students affected by unionization (see the numbers in square brackets in Table I). For instance, only 54 percent of schools reported collective bargaining as the form of negotiations in 1992, but 63 percent of students attended such schools.

5. These results are available from the author upon request.

TABLE I
VARIOUS MEASURES OF TEACHERS' ORGANIZATION—GAUGING MISREPORTING

Year	Percentage of school districts reporting at least: [In square brackets: percentage of students enrolled in such school districts]		
	"Meet and confer" provisions	Collective bargaining is form of negotiations	Contractual agreement between teachers' organization and administration; collective bargaining form of negotiations; at least 50 percent of teachers are members
1963	10	1	1
1966	28	8	8
1972	44 [62]	28 [38]	14 [20]
1982	54 [65]	45 [59]	28 [37]
1992	59 [69]	54 [63]	36 [43]

Sources. 1963 data are from Perry and Wildman [1966]; 1966 data are from *National Educational Association* [1967]; 1972–1992 are data from Census of Governments (author's calculations). 1963 and 1966 statistics may be overstated because the surveys oversampled large districts. They may also be understated if some districts that did have agreements neglected to return the agreements even though they returned responses to the survey.

This is because AFT affiliates are very likely to function as unions, while a sizable share of NEA affiliates essentially remain professional associations. Greater consistency of union behavior across union affiliation is another advantage of the strict definition.

B. Laws Facilitating Teachers' Unionization

One of the most important features of the history of teachers' unionization is the change in the legal environment for public sector unions after 1960.[6] In 1960 collective bargaining by teachers was explicitly illegal in several states and probably implicitly prohibited in many others (meaning that lawmakers would have reacted to any collective bargaining activity with explicit bans). Between 1960 and 1990, states typically extended increasingly generous collective bargaining rights to teachers' unions. In some cases, the right extended has been only the right to organize for the purposes for collective bargaining. In other cases, the teach-

6. The volume edited by Freeman and Ichniowski [1988] acutely summarizes the legal transition.

ers' unions have been given the right to meet with administration representatives or even the right to engage in collective bargaining with administration representatives. The last two types of collective bargaining rights make it much harder for a district to evade collective bargaining indefinitely. I use passage of a law explicitly extending the right to meet or to engage in collective bargaining as one instrument for unionization. The first row of Table II shows the passage of such laws by state and decade. Though some states are predictable in the timing of their laws (Michigan, early), others are not (Ohio, late). Farber [1988] and Saltzman [1988] document how state politics and political institutions played an important role in the passage of teachers' unionization laws, so that laws do not merely reflect public attitudes toward unions or pressure from incipient public sector unions. These political and institutional factors ensure that useful variation remains in the timing of laws when we eliminate state trends.

In addition, I use the passage of two other types of laws as instruments for unionization. These are laws allowing teachers' unions to have agency shops and union shops. A union has an agency shop if it collects dues from all teachers in the bargaining unit, regardless of whether they are union members. A union shop exists if the school district cannot employ teachers who do not become union members. Laws permitting agency and union shops facilitate assertive collective bargaining because they greatly weaken the position of teachers in a district who oppose the union. The tools an individual teacher has to oppose the union are withholding of financial support and withholding of political support. Union and agency shops weaken these tools. The last two rows of Table II shows interesting variation in the passage of agency and union shop laws by state and decade. Although the timing of some laws accords with expectations based on states' union activity, timing in closely related states contradicts these expectations. For instance, Massachusetts and Rhode Island explicitly permitted agency shops early, while Connecticut permitted them late.

I use as instruments only the passage of laws that allow union activities, rather than the passage of laws that explicitly forbid union activities, such as a ban on strikes, because "forbidding" sometimes laws appear to have been a response to the onset of the forbidden activity. In contrast, "permitting" laws appear to lead, rather than lag, bursts of union activity.

TABLE II
CHANGES IN LAWS FACILITATING TEACHERS' UNIONIZATION

	Right gained before 1970	Right gained between 1970 and 1980	Right gained between 1980 and 1990
Administration has (at least) duty to meet with teachers' organization	AK, CA, DE, HI, KS, ME, MD, MA, MI, MN, NV, NJ, NY, NC, OR, PA, RI, SD, VT, WA, WI	FL, ID, IN, IA, MT, NH, OK, SD, TN	CT, IL, OH, ND, NE
Agency shops explicitly permitted	MA, VT	CA, HI, MI, MN, MT, NY, OH, OK, OR, RI, WA, WI	CT, ID, IL, IN, ND, NJ, PA
Union shops explicitly permitted	VT	OR, WA	ID, IL, ND

Sources. For laws passed from 1950 to 1985, the source is the NBER Public Sector Collective Bargaining Law Data Set. See Valletta and Freeman [1988]. For laws passed from 1986 to 1992, the source is the author's searches on LEXIS, a legal information retrieval system.

The combined differences-in-differences/instrumental variables strategy does not require that the timing of laws be generally arbitrary. It requires only that the timing be determined by (1) factors that trend steadily over time within a state and (2) state-level factors that do not directly affect student achievement or school input choices. The essential restriction is that timing of passage of a relevant law is uncorrelated with the timing of an acceleration in other statewide variables that directly affect students or schools.[7]

IV. DATA

In this section I review the principal data sources used in this study. Appendix 1 contains additional details.

I first matched the 1972, 1982, and 1992 Censuses of Governments to create a panel on expenditure, teacher employment, teacher pay, and student enrollment for every public school district in the United States that spans three decades. The Censuses

7. I tried controlling for what seemed to me to be the most likely violation of this restriction—the timing of discrete changes in statewide school finance laws. Such controls did not appreciably affect the results. These results are available from the author upon request.

of Governments also contain several variables useful for defining unionization rigorously. To reduce error, I use the strict definition of unionization: labor relations take the form of collective bargaining; a contractual agreement exists between the administration and the teachers' organization (or had existed previously, in the case of ongoing negotiations); and at least 50 percent of either total teachers or the full-time teachers were members of the teachers' organization.[8]

To extend the panel data on unionization to the 1960s, I use 1966 data on individual school districts' negotiation agreements from *Negotiation Agreement Provisions* [NEA 1967] and 1963 statistics on unionization from Perry and Wildman [1966]. For both of these sources I defined a school as unionized if it had a mutually signed and binding contractual agreement that resulted from collective bargaining. This is what the NEA calls a "level 4" negotiation and is the motivation for my own definition of unionization in the Censuses of Governments.

To get demographic data and a measure of student achievement for each school district, I matched school district tabulations of the 1970, 1980, and 1990 Censuses of Population and Housing to one another and then to the Census of Governments. The school district tabulations of the Census are created by linking each census block group and enumeration district to school district boundaries and summarizing by district. This tabulation is known as the School District Data Book for 1990 and as Summary Tape File 3F for 1980. I created the 1970 tabulation myself. No school district coding scheme completely unifies the three tabulations, so that matching required careful examination of changes in school district coding and school district boundaries. If two or more districts consolidated into one district over the period, the constituent districts were "preconsolidated" in earlier Censuses and indicator variables were created to record the consolidation. Five hundred and eighty-five districts could not be successfully matched and were dropped from the analysis, leaving 10,509 schools districts (about 95 percent of the total in the United States in 1990).

The demographic variables drawn from the Censuses of Population include population, black population, Hispanic population, urban population, population in poverty, adult population with at least twelve years of schooling, adult population with at least sixteen years of schooling, total K–12 enrollment, private K–12 enrollment, black K–12 enrollment, median household income, median gross monthly rent, the unemployment rate, and the percentages of the population aged 16 to 19 and aged 18 to 19. Only one measure of student achievement can be derived from the Census for each school district: the high school dropout rate, defined as the percentage of 16 to 19 year-olds who are not enrolled and do not have high school degrees. This measure has the advantage of reflecting local K–12 education rather than higher education and of being particularly sensitive to the lower portion of the student achievement distribution, which teachers' unions often claim to most affect. Because the measure is potentially sensitive to migration of 18 to 19 year-olds (owing to college, job opportunities, or social opportunities), I control for the percentage of the 16 to 19 year-old population who are aged 18 to 19. It would be good to have additional measures such as test scores, but consistent test scores that span the 1970–1990 period do not exist at the school district level for a universal (or even large) sample of individual school districts. Other measures such as wages and ultimate educational attainment cannot be linked to school districts since mobility among districts is high and nonrandom.

The final major source of data is the NBER Public Sector Collective Bargaining Law Data Set (see Valletta and Freeman [1988]), which summarizes state laws governing teachers' unionization for every year after 1954. From this source (which I updated from 1986 to 1992 using LEXIS searches) I derive indicator variables for the laws assigning schools a duty to meet with teachers' organizations, laws specifically allowing agency shops in schools, and laws specifically allowing union shops in schools.

V. Empirical Strategy

In this section I discuss econometric issues common to the four basic equations I estimate. The per-pupil budget, "Log(PerPupilExp)," equation derives from equation (4). Two school input equations derive from equation (3). One is for the log average teacher salary, "Log(TchSal)," and the other is for the student-teacher ratio, "STRatio." The education production function de-

8. Under this definition, few schools shift both in and out of unionization between years of the Census of Governments (1972, 1977, 1982, 1987, 1992). The most stringent part of the definition is the requirement of a contractual agreement. A definition that uses *only* this requirement does not discernibly affect the results.

rives from substituting equations (2), (5), and (6) into equation (1). It uses the high school dropout rate, "Dropout," as the measure of student achievement. The equations are

$$\text{(13)} \quad \text{Log(PerPupilExp}_{it}) = \lambda\, U_{it} + \mu W_{it} + \zeta_i + \tilde{\zeta}_i t + \zeta_{it},$$

$$\text{(14)} \quad \text{Log(TchSal}_{it}) = \gamma_1 U_{it} + \kappa_1 W_{it} + \varepsilon 1_i + \tilde{\varepsilon} 1_i t + \varepsilon 1_{it},$$

$$\text{(15)} \quad \text{STRatio}_{it} = \gamma_2 U_{it} + \kappa_2 W_{it} + \varepsilon 2_i + \tilde{\varepsilon} 2_i t + \varepsilon 2_{it},$$

$$\text{(16)} \quad \begin{aligned} \text{Dropout}_{it} = {} & \alpha_1 \text{Log(TchSal}_{it}) + \alpha_2 \text{STRatio}_{it} \\ & + \alpha_3 \text{Log(PerPupilExp}_{it}) + \beta_1 U_{it} \\ & + \beta_2 \text{Log(TchSal}_{it}) \times U_{it} + \beta_3 \text{STRatio}_{it} \times U_{it} \\ & + \beta_4 \text{Log(PerPupilExp}_{it}) \times U_{it} + Z_{it}\delta + \xi_i + \tilde{\xi}_i t + \xi_{it}. \end{aligned}$$

W and Z are vectors of demographic variables drawn from the Census, and t indicates the year. In equation (16) per-pupil spending gives an aggregate measure of the school inputs that were not separately measured, and the unionization indicator and its interactions pick up the effects of unionization on productivity.[9] Each error term is broken into three parts to indicate the presence of unobserved variables for an individual school district that are constant over time ($\zeta_i, \varepsilon 1_i, \varepsilon 2_i, \xi_i$), have a constant time trend ($\tilde{\zeta}_i t, \tilde{\varepsilon} 1_i t, \tilde{\varepsilon} 2_i t, \tilde{\xi}_i t$), or are deviations from a time trend ($\zeta_{it}, \varepsilon 1_{it}, \varepsilon 2_{it}, \xi_{it}$).

For each of these equations the empirical strategy is as follows. I first estimate the equation at a point in time (cross-section), hoping to minimize unobserved district characteristics by controlling for detailed demographic characteristics (W and Z), which include state fixed effects. I then estimate the equation by first-differences for the 1970–1980 period and the 1980–1990 period. This eliminates identification problems caused by unobserved district characteristics that are constant over time ($\zeta_i, \varepsilon 1_i, \varepsilon 2_i, \xi_i$). Since many district characteristics, such as the distance from the central city and housing stock are relatively fixed over time, these estimates should be an improvement on the cross-section estimates. However, I expect these estimates to suffer from measurement error bias exacerbated by first-differencing.

Next, I estimate the equation with time differences-in-differences between the 1970–1980 differences and the 1980–1990 differences. Since there are only three years in the panel,

9. To facilitate comparison with typical education production functions, I also estimate equation (16) without the unionization indicator and its interactions.

this is equivalent to estimating the equations with district fixed effects, time fixed effects, and district-specific time trends. The district-specific time trends eliminate unobserved district characteristics that have constant time trends ($\zeta_i t, \varepsilon 1_i t, \varepsilon 2_i t, \xi_i t$). Many district characteristics that affect both unionization and schools trend rather steadily. For example, housing stock in a district may gradually decay, or job opportunities nearby may gradually become more oriented toward professionals and managers. What drives the differences-in-differences results is acceleration or deceleration in the time trends of school inputs or student achievement that is associated with the discrete event of unionization. The reason that differences-in-differences is appropriate to the analysis of unionization is that, because unionization occurs through a teacher vote, the event of unionization is discrete even if its determinants trend constantly.

The identification problem that possibly remains results from potential correlation between shocks to unobserved district-time-specific characteristics ($\zeta_{it}, \varepsilon 1_{it}, \varepsilon 2_{it}, \xi_{it}$) and the discrete change in unionization. A solution is the use of instrumental variables that (1) are uncorrelated with shocks ($\zeta_{it}, \varepsilon 1_{it}, \varepsilon 2_{it}, \xi_{it}$) occurring in school districts but (2) cause unionization to change discretely when they change discretely. State laws that facilitate teachers' unionization are good candidates. The fact that a state law changed discretely does not necessarily indicate that the state, as a whole, experienced any statewide, time-specific shock that might also affect schools (and thus produce omitted variables bias in the instrumental variables estimates). This is because state laws change discretely, even if their determinants have rather constant time trends.

Instrumental variables estimates also mitigate measurement error bias, which we expect to be exacerbated in differences-in-differences estimation.

For the sake of comparison I do estimate the first-differenced equations by instrumental variables, although I report the estimates only in footnotes to the tables. It is ambiguous whether IV first-differences provide better identification than least squares. The ambiguity is caused by the following tension. On the "plus" side we expect instrumental variables to reduce measurement error bias in first-differences estimation. On the "minus" side, if instrumental variables are to solve and not exacerbate omitted variables bias in first-differences estimation, then the timing of law changes must be arbitrary. This is a more restrictive (and less

appealing) assumption than the identifying restriction needed for instrumental variables in differences-in-differences, where the underlying determinants of law passage may have a district-specific trend.

Following Heckman and MaCurdy [1985] and Angrist [1991], I use a linear probability model for the first stage of the instrumental variables estimation. This approach gives consistent estimates and allows me to use a Lagrange Multiplier test of identifying restrictions.[10] It also lets me adjust the standard errors of the instrumental variables estimates for the fact that laws vary only at the state level while unionization varies at the individual district level. I calculate Huber [1967] or White [1980] standard errors for instrumental variables allowing for group correlation in errors, with the groups defined by states.

VI. Results

Each of Tables III through VI has the same basic structure. Starting with the left-hand column, I show estimates of the equation from cross-section regressions, from first-differences regressions, from differences-in-differences regressions, and finally from IV differences-in-differences regressions.[11] Unless stated otherwise, all estimates I quote are asymptotically significantly different from zero at a 0.05 or lower level. All regressions are weighted by district enrollment.[12]

A. *Effects on per-Pupil Spending and School Inputs*

Table III shows the effects of teachers' unions on per-pupil spending. The dependent variable is the log of per-pupil spending in current dollars. Before discussing the effects of unionization, a brief review of the other determinants of per-pupil spending is

10. See Hausman [1983] for a description of the Lagrange Multiplier test of overidentifying restrictions and its use for instrumental variables estimators.

11. Owing to the potential for division bias (such as occurs when hourly wages created by dividing total wages by a measure of hours is regressed on hours), I also estimated the per-pupil spending and student-teacher ratio regressions without including enrollment as an explanatory variable. This variation did not appreciably affect the results.

12. I weight by enrollment mainly because there are a large number of small rural school districts in the United States that contain relatively few students. It is not clear that such small districts follow the same behavioral model of teachers' unionization that larger districts follow. In practice, unweighted estimates were not consistently larger or smaller than weighted estimates, but the unweighted estimates consistently had larger standard errors. For differenced equations, I weighted by 1982 enrollment.

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TABLE III

EFFECT ON PER-PUPIL SPENDING OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is Log (Per-Pupil Spending/1000 in Current $)

	Cross-section estimates			First-differences		Differences-in-differences	IV differences-in-differences
	1972	1982	1992	1972–1982	1982–1992		
Unionized	−.0025	.0038	.0037	.0305	.0029	.0292	.1233
	(.0055)	(.0035)	(.0047)	(.0065)	(.0064)	(.0067)	(.0332)
Metropolitan area	.0338	.0249	.0135				
	(.0065)	(.0042)	(.0059)				
Log(population/1000)	.0185	.0323	.1459	.0036	.1622	.0290	.0283
	(.0022)	(.0043)	(.0073)	(.0029)	(.0123)	(.0036)	(.0130)
% of population urban	.0002	.0004	.0003	.0004	.0005	−.0007	−.0007
	(.0001)	(.0001)	(.0001)	(.0001)	(.0003)	(.0001)	(.0004)
Log(median HH income/1000)	.1349	.1134	.1331	.3547	1.2299	.1163	.0653
	(.0249)	(.0167)	(.0206)	(.0188)	(.0144)	(.0185)	(.0659)
Log(median rent)	−.0157	.0258	.0434	.5642	−.1269	.2323	.2307
	(.0173)	(.0133)	(.0105)	(.0140)	(.0082)	(.0081)	(.0518)
% of population in poverty	.0017	.0062	.0028	.0029	.0193	−.0068	−.0066
	(.0005)	(.0006)	(.0008)	(.0006)	(.0007)	(.0006)	(.0018)
Unemployment rate	−.0041	−.0023	.0043	.0203	.0007	−.0052	−.0059
	(.0012)	(.0007)	(.0012)	(.0009)	(.0009)	(.0008)	(.0039)
% of population black	.0011	−.0029	−.0081	.0027	−.0018	.0047	.0051
	(.0003)	(.0004)	(.0009)	(.0005)	(.0007)	(.0006)	(.0019)
% of population Hispanic	−.0008	−.0001	.0006	.0079	.0001	.0031	.0037
	(.0002)	(.0002)	(.0002)	(.0006)	(.0007)	(.0007)	(.0015)
% of K–12 enrollment black	.0015	.0032	.0085	.0014	−.0005	−.0007	−.0010
	(.0003)	(.0003)	(.0008)	(.0006)	(.0006)	(.0003)	(.0010)
% of population 12+ yrs of schl	−.0026	−.0029	−.0048	.0153	.0092	.0046	.0048
	(.0004)	(.0003)	(.0005)	(.0005)	(.0005)	(.0005)	(.0026)
% of population 16+ yrs of schl	.0093	.0056	.0069	−.0128	−.0001	.0036	.0049
	(.0005)	(.0003)	(.0004)	(.0007)	(.0008)	(.0007)	(.0013)
% of K–12 enrollment private	.0024	.0054	.0011	.0025	−.0002	.0034	.0041
	(.0003)	(.0003)	(.0004)	(.0005)	(.0006)	(.0005)	(.0015)
Log(Public K–12 enrollment/1000)	−.0332	−.0444	−.1515	−.2396	−.3162	−.4091	−.4077
	(.0024)	(.0040)	(.0073)	(.0097)	(.0129)	(.0110)	(.0551)
State fixed effects	yes	yes	yes				
R^2	.66	.68	.68	.21	.20	.18	
Standard errors adjusted for state random effects							yes
Test of identifying restrictions χ^2 with 2 d.f. (probability)							.1918 (.9085)

10,509 observations; unit of observation is a United States school district; standard errors are in parentheses except that number in parentheses below the χ^2 test is the area of the right-hand tail (the probability of the χ^2); all covariates are indicated; regressions are weighted by district enrollment (1982 enrollment is used for differenced equations); see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

If the first-differenced equations are estimated using instrumental variables, the estimated coefficients on unionization are 0.0872 (.0111) for 1982–1992 and 0.0454 (.0142) for 1972–1982.

warranted because the results are consistent with our expectations. Per-pupil spending is significantly higher in school districts with larger populations, higher median household income, more educated populations, greater shares of enrollment in private schools, and smaller public school enrollments. In the teacher salary, student-teacher ratio, and dropout rate regressions, estimated coefficients on the same explanatory variables are consistent with our expectations, too. Therefore, I emphasize only particularly interesting results for the other regressions.

The effect of teachers' unionization on per-pupil spending is insignificantly different from zero in the cross-section regressions, and the sign is not consistently positive. From the first-differences regression, the effect of teachers' unionization is a 3.1 percent increase in per-pupil spending for the 1972–1982 period and an increase that is insignificantly different from zero for the 1982–1992 period. Thus, though the first-differences specification is unlikely to fully resolve the identification problem, the specification does produce results closer to the expectations generated by almost any model of union behavior. The differences-in-differences estimate of teachers' unionization on per-pupil spending is a 2.9 percent increase. However, the IV differences-in-differences regression indicates that per-pupil spending increases by 12.3 percent when teachers unionize. This estimate is highly significant (p-value $< .001$) despite the adjustment made to the standard errors to account for the fact that laws vary only at the state level. The fact that the IV estimate is so much larger than the first-differences and differences-in-differences estimate suggests that the other estimates suffer from omitted variables bias (districts experiencing negative spending shocks unionize) or measurement error bias or both.

Note that the partial test of whether the instrumental variables are uncorrelated with the error terms ($\zeta_{it}, \varepsilon 1_{it}, \varepsilon 2_{it}, \xi_{it}$), the statistic for which is shown at the bottom of Table III, fails to reject the null hypothesis of no correlation. Also, note that Appendix 2 shows the first-stage linear probability regressions—relating unionization to the passage of facilitating laws—that are implied in the instrumental variables estimates. Each of the laws has a positive effect on the probability of unionization, and the laws are jointly highly significant (p-value $< .006$).

Figures III and IV provide an alternative presentation of the differences-in-differences and IV differences-in-differences re-

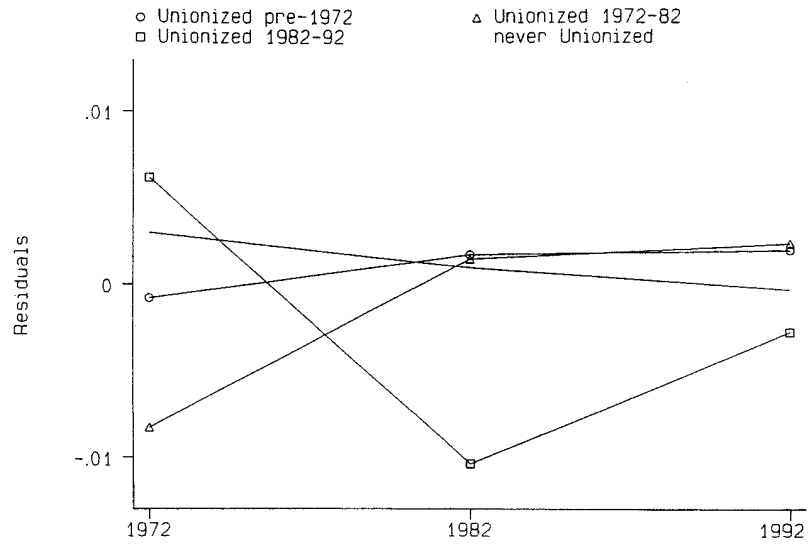

FIGURE III
Residual Log per-Pupil Spending/1000, by Period of Unionization

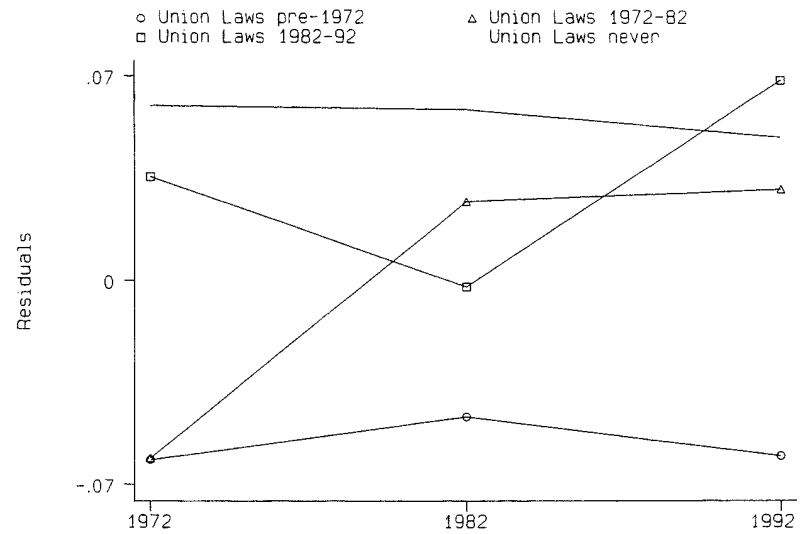

FIGURE IV
Residual Log per-Pupil Spending/1000, by Period of Union Laws

sults.[13] For both figures I calculated the residual from a cross-section regression of per-pupil spending on all the independent variables shown in the first three columns of Table III *except* unionization. For Figure III, I then calculated the weighted average residual for (1) districts that unionized before 1972, (2) districts that unionized between 1972 and 1982, (3) districts that unionized between 1982 and 1992, and (4) districts that never unionized. In Figure III, I plot these average residuals against time. It shows that per-pupil spending between 1972 and 1982 grew fastest in schools that unionized during that period. Similarly, per-pupil spending between 1982 and 1992 grew fastest in schools that unionized that period. Note that unionization appears to have lingering effects: schools that unionized in previous periods do not get as big an increase in per-pupil spending as schools that are currently unionizing, but they get bigger increases than schools that have not yet unionized or never unionize.

For Figure IV, I calculated the weighted average residual for states that passed one or more of the three laws facilitating unionization (1) before 1972, (2) between 1972 and 1982, (3) between 1982 and 1992, and (4) never. I then plotted these average residuals against time. Figure IV shows a similar but more pronounced pattern than Figure III shows. The fastest growth in per-pupil spending is in states that are currently passing laws facilitating unionization.

In Table IV, I present the effect of teachers' unionization on teacher salaries. The cross-section estimates predict a small salary increase for unionizing teachers: 2.9 percent from the 1970 regression, 4.0 percent from the 1980 regression, and 1.6 percent from the 1990 regression. The first-differences equations indicate that unionizing teachers receive larger salary increases of 4.8 percent from the 1970–1980 equation and 4.0 percent from the 1980–1990 equation. The differences-in-differences estimate is 5.1 percent. The IV differences-in-differences regression predicts a similar unionization effect: a salary increase of 5.0 percent.

Table V shows estimates of the student-teacher ratio equation. A decrease in the student-teacher ratio is an increase in school inputs, since it allows smaller regular class sizes and more

13. The figures are not strictly equivalent to the regression estimates, owing to the fact that I do not partial out the effect of the demographic variables on the timing of unionization and the union laws.

individualized instruction. Thus, the cross-section and first-differences estimates of the effect of teachers's unionization are predictions that unionization raises the student-teacher ratio or decreases teacher and classroom inputs for each student. Previous studies based on cross-section data have interpreted similar results as evidence that teachers' unions increase the student-teacher ratio so as to get salary increases out of a constant per-pupil budget (for example, Eberts [1984]).

However, in the differences-in-differences and IV differences-in-differences regression, the sign of the union effect on the student-teacher ratio is negative, implying that unionization increases this input measure just as it increases per-pupil spending and teacher salaries. The differences-in-differences regression indicates that unionization decreases the student-teacher ratio by 1.11 students. The IV differences-in-differences estimate is larger: unionization decreases the student-teacher ratio by 1.7 students per teacher. Like the IV differences-in-differences estimates of the effect of unionization on per-pupil spending and teacher salaries, this estimate is not merely statistically significantly but economically important.

Does the predicted increase in teacher salaries combined with the predicted decrease in the student-teacher ratio wholly explain unionization's effect on per-pupil spending? To answer this question, I calculated the increase in per-pupil spending that would have occurred if every school that actually unionized between 1972 and 1982 had received the increase in teacher salaries and the decrease in the student-teacher ratio predicted for unionizing schools.[14] The result was a predicted increase in per-pupil spending of 9.5 percent. Compare this with the full predicted effect of unionization on per-pupil spending: 12.3 percent. About three-quarters of the increase in per-pupil spending is destined for teacher salaries or the student-teacher ratio. The remaining fourth of the spending increase is devoted to other school inputs. In the work on productivity that follows, it is useful to assess unions' effect on inputs *either* (1) through their effect on per-pupil spending or (2) through their effect on the combination of teacher salaries and the student-teacher ratio.

In summary, teachers' unionization appears to increase school budgets and to devote most of this increase to two key

14. I used the IV differences-in-differences estimates. I performed a similar calcuation for schools for unionized between 1982 and 1992, with similar results.

TABLE IV

EFFECT ON TEACHER SALARIES OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is Log (Average Teacher Salary/1000 in Current $)

	Cross-section estimates			First-differences		Differences-in-differences	IV differences-in-differences
	1972	1982	1992	1972–1982	1982–1992		
Unionized	.0291	.0396	.0158	.0484	.0399	.0511	.0502
	(.0040)	(.0033)	(.0037)	(.0057)	(.0080)	(.0080)	(.0202)
Metropolitan area	.0038	.0081	.0019				
	(.0047)	(.0040)	(.0046)				
Log(population/1000)	.0069	−.0062	.0633	.0035	−.0720	−.0145	−.0143
	(.0016)	(.0039)	(.0058)	(.0025)	(.0152)	(.0043)	(.0104)
% of population urban	.0002	.0005	.0004	.0001	−.0001	.0005	.0006
	(.0001)	(.0001)	(.0001)	(.0001)	(.0003)	(.0002)	(.0002)
Log(median HH income/1000)	.1163	.1178	.0819	.2502	1.3413	.1987	.2444
	(.0182)	(.0145)	(.0164)	(.0164)	(.6481)	(.0219)	(.0711)
Log(median rent)	−.0168	.0178	.0799	.3961	.6154	−.0210	−.0194
	(.0126)	(.0126)	(.0084)	(.0121)	(.0212)	(.0097)	(.0312)
% of population in poverty	−.0008	.0020	.0012	.0040	.2097	−.0001	.0001
	(.0004)	(.0005)	(.0006)	(.0005)	(.1102)	(.0006)	(.0020)
Unemployment rate	−.0031	−.0006	.0066	.0132	−.5596	−.0028	−.0037
	(.0009)	(.0006)	(.0009)	(.0009)	(.2602)	(.0009)	(.0051)
% of population black	.0004	−.0007	−.0001	.0001	−.1688	−.0038	−.0041
	(.0002)	(.0004)	(.0007)	(.0005)	(.1275)	(.0007)	(.0029)
% of population Hispanic	.0005	−.0016	.0015	.0036	−.0755	−.0037	−.0043
	(.0002)	(.0002)	(.0002)	(.0005)	(.0391)	(.0009)	(.0013)
% of K–12 enrollment black	.0014	−.0004	.0006	.0005	.0606	.0002	.0004
	(.0002)	(.0003)	(.0006)	(.0003)	(.0437)	(.0004)	(.0009)
% of population 12+ yrs of schl	−.0015	−.0015	−.0016	.0092	.1208	.0034	−.0036
	(.0003)	(.0003)	(.0003)	(.0004)	(.1055)	(.0006)	(.0019)
% of population 16+ yrs of schl	.0043	.0018	.0015	−.0087	.1564	.0007	−.0018
	(.0004)	(.0003)	(.0003)	(.0006)	(.0697)	(.0008)	(.0020)
% of K–12 enrollment private	.0006	.0029	.0002	.0020	−.1608	−.0027	−.0031
	(.0002)	(.0003)	(.0003)	(.0004)	(.0848)	(.0006)	(.0014)
Log(public K–12 enrollment/1000)	.0409	.0441	−.0316	−.0313	−.0571	.1033	.1007
	(.0018)	(.0037)	(.0057)	(.0083)	(.0156)	(.0130)	(.0346)
State fixed effects	yes	yes	yes				
R^2	.79	.68	.79	.12	.14	.11	
Standard errors adjusted for state random effects							yes
Test of identifying restrictions χ^2 with 2 d.f. (probability)							.3504 (.8392)

10,509 observations; unit of observation is a United States school district; standard errors are in parentheses except that the number in parentheses below the χ^2 test is the area of the right-hand tail (probability of the χ^2); all covariates are indicated; regressions are weighted by district enrollment (1982 enrollment is used for differenced equations); see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

If the first-differenced equations are estimated using instrumental variables, the estimated coefficients on unionization are 0.0296 (.0127) for 1982–1992 and 0.0218 (.0140) for 1972–1982.

TABLE V

EFFECT ON STUDENT-TEACHER RATIO OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is the Student-Teacher Ratio

	Cross-section estimates			First-differences		Differences-in-differences	IV differences-in-differences
	1972	1982	1992	1972–1982	1982–1992		
Unionized	.2673	.4378	.0847	.5043	.6304	−1.1120	−1.6924
	(.0888)	(.0537)	(.0507)	(.0813)	(.0963)	(.3382)	(.5676)
Metropolitan area	−.6408	−.1077	−.1274				
	(.1059)	(.0636)	(.0637)				
Log(population/1000)	−.0898	.1230	−.7441	.0941	−1.3446	−.8412	−.8255
	(.0356)	(.0657)	(.0792)	(.0368)	(.1834)	(.0713)	(.2724)
% of population urban	−.0078	−.0055	.0049	−.0003	−.0066	.0289	.0293
	(.0015)	(.0009)	(.0010)	(.0014)	(.0041)	(.0026)	(.0083)
Log(median HH income/1000)	−1.3927	1.4027	1.0546	−.5858	−1.4496	−1.1704	−1.5787
	(.4065)	(.2340)	(.2228)	(.2341)	(.2144)	(.3668)	(.5368)
Log(median rent)	−.0186	.8119	.0071	−2.1608	−1.1715	−1.1669	−.8243
	(.2786)	(.2042)	(.1131)	(.1737)	(.1221)	(.1608)	(.5384)
% of population in poverty	−.0463	−.0093	−.0233	.0585	.1428	.1492	.1445
	(.0086)	(.0086)	(.0083)	(.0071)	(.0102)	(.0115)	(.0400)
Unemployment rate	.0673	.0494	.0753	−.0928	.1093	.1229	.1454
	(.0201)	(.0102)	(.0134)	(.0122)	(.0136)	(.0150)	(.0806)
% of population black	−.0017	.0062	.0990	−.0412	−.0631	−.1431	−.1564
	(.0045)	(.0065)	(.0100)	(.0068)	(.0115)	(.0119)	(.0566)
% of population Hispanic	.0224	−.0147	.0203	−.0796	.0464	−.0649	−.0846
	(.0039)	(.0028)	(.0024)	(.0070)	(.0112)	(.0139)	(.0629)
% of K–12 enrollment black	−.0036	−.0286	−.0899	−.0014	.0208	.0490	.0573
	(.0042)	(.0051)	(.0084)	(.0038)	(.0083)	(.0061)	(.0176)
% of pop 12+ yrs of schl	.0285	.0082	.0581	−.0750	−.0425	−.1289	−.1355
	(.0063)	(.0049)	(.0049)	(.0061)	(.0077)	(.0105)	(.0646)
% of pop 16+ yrs of schl	−.0896	−.0574	−.0859	.0711	−.1188	−.0818	−.1186
	(.0086)	(.0050)	(.0045)	(.0085)	(.0123)	(.0152)	(.0334)
% of K–12 enrollment private	−.0096	−.0382	−.0121	−.0121	−.0355	−.0983	−.1160
	(.0051)	(.0044)	(.0043)	(.0059)	(.0090)	(.0092)	(.0364)
Log(public K–12 enrollmt/1000)	1.0436	.4307	1.1585	2.7530	5.6842	7.3337	7.1317
	(.0413)	(.0625)	(.0786)	(.1227)	(.1923)	(.2173)	(.9834)
State fixed effects	yes	yes	yes				
R^2	.47	.43	.75	.19	.24	.14	
Standard errors adjusted for state random effects							yes
Test of identifying restrictions χ^2 with 2 d.f. (probability)							.1721
							(.9175)

10,509 observations; unit of observation is a United States school district; standard errors are in parentheses except that the number in parentheses below the χ^2 test is the area of the right-hand tail (probability of the χ^2); all covariates are indicated; regressions are weighted by district enrollment (1982 enrollment is used for differenced equations); see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

If the first-differenced equations are estimated using instrumental variables, the estimated coefficients on unionization are 0.5844 (.1107) for 1982–1992 and 0.0652 (.0771) for 1972–1982.

school inputs, teacher salaries and the teacher-student ratio. These increases are consistent with either an efficiency-enhancing or a rent-seeking model of unions. As we move across the tables from the cross-section estimates to the IV differences-in-differences estimates, the estimated effect of unionization tends to increase in size. This pattern suggests that omitted variables bias or measurement error bias is being systemically eliminated. Also, in Tables III through V the partial tests of whether the instrumental variables are uncorrelated with the error terms ($\zeta_{it}, \varepsilon 1_{it}, \varepsilon 2_{it}, \xi_{it}$) consistently fail to reject the null hypothesis of no correlation at the 5 percent level.

B. Effects on the Dropout Rate

Table VI, which shows the effects of unionization on the high school dropout rate, is split into three parts. Table VIa presents cross-section results. Table VIb presents first-differenced results. Table VIc presents the differences-in-differences and IV differences-in-differences results. As measures of school inputs, I alternatively use (1) per-pupil spending and (2) the combination of teacher salaries and the student-teacher ratio. To facilitate comparison with typical education production functions, I do estimate the function without unionization. However, the target equations are those that allow unionization to affect both the intercept of the production function and the productivity of school inputs. These equations allow unions to affect schools by the several paths discussed.[15]

In dropout equations I include as a covariate the share of 16 to 19 year-olds in a district who are aged 18 to 19. This variable accounts for the effects of migration of 18 and 19 year-olds (due to college attendance, job opportunities, or social opportunities) on the measured dropout rate (the number of 16 to 19 year-olds who are not enrolled and do not have a high school degree).[16]

Unfortunately, it is not possible to perfectly align the dates at which the dropout rates are measured (1970, 1980, and 1990) with the dates at which unionization is measured (1972, 1982, and 1992). This means that the 1990 dropout rate of a district that unionizes between 1990 and 1992 will be associated with unionization. To investigate the sensitivity of my results to this misalignment, I recalculated the results under an alternative definition of unionization. In the alternative, a school was not defined as unionized in 1990 unless it was unionized in 1987 and was not defined as unionized in 1980 unless it was unionized in 1977. (1987 and 1977 are the years of the previous Censuses of Governments.) The alternative results do not differ appreciably from those shown in Table VI.

Table VIa sets the stage for understanding how unionization affects the education production function by showing cross-section predictions of the dropout rate. The three left-hand columns use teacher salaries and the student-teacher ratio as the measure of school inputs. The three right-hand columns use per-pupil spending as the measure of school inputs. (Note that a decrease in the dropout rate indicates improved student performance.) The student-teacher ratio has a statistically insignificant effect on the dropout rate in all three years. Higher teacher salaries are associated with lower dropout rates in 1970 and 1980, although the 1990 coefficient is insignificantly different from zero. Even for 1970 and 1980 the predicted effect of teacher salaries is very small. A 10 percent increase in teacher salary is predicted to improve the dropout rate by about 0.18 percentage points. Per-pupil spending has a statistically insignificant effect on the dropout rate in the 1990 results, but improves the dropout rate by a small amount in the 1970 and 1980 results (the 1970 coefficient has a p-value of only 0.08). These small predicted effects and mixture of statistically significant and insignificant results are fairly typical for a school-level equation estimated on cross-section data (see Betts [1995]).[17]

Table VIb shows first-differenced estimates of the education production function, using the combination of teacher salaries and the student-teacher ratio as the measures of school inputs.[18] In the equations without unionization (the first and third columns), teacher salaries appear to have no effect on student achievement, although the point estimates are of the expected

15. In notes to the tables I report the full effect of unionization—direct and through the inputs and input productivities. The full effect is estimated by excluding measures of inputs and interactions from the equations predicting the dropout rate. I also estimated equations in which unionization and inputs entered only as main effects (no interactions). The results of this specification are shown in Table VIc.

16. Migration of college students is only a problem for those students who establish residence at their college location. Students who live in college housing should be counted with the household of their parents or guardians.

17. The cross-section results show little evidence that unionization affects the dropout rate. The full effects of unionization (noted at the bottom of Table VIa) are all statistically insignificantly different from zero.

18. Similar results are obtained if per-pupil spending is used as the measure of inputs.

TABLE VIa

EFFECT ON DROPOUT RATE OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is the High School Dropout Rate in Percent

	Cross-section estimates					
	1970	1980	1990	1970	1980	1990
Log(per-pupil Spending/1000)				−1.265	−1.484	−0.485
				(0.708)	(0.691)	(0.701)
Student-teacher ratio	0.011	0.044	0.262			
	(0.038)	(0.039)	(0.057)			
Log(avg teacher salary/1000)	−1.776	−1.860	−0.932			
	(0.827)	(0.809)	(0.914)			
Log(population/1000)	0.266	0.697	0.811	0.236	0.774	0.791
	(0.112)	(0.131)	(0.438)	(0.112)	(0.132)	(0.444)
% of population urban	0.012	0.012	0.039	0.010	0.012	0.036
	(0.005)	(0.002)	(0.004)	(0.005)	(0.002)	(0.004)
Log(median HH income/1000)	−1.356	−1.794	−1.297	−1.267	−1.744	−1.218
	(0.538)	(0.474)	(1.130)	(0.538)	(0.474)	(1.134)
Log(median rent)	−2.127	−2.225	−2.845	−2.137	−2.191	−2.868
	(0.273)	(0.408)	(0.584)	(0.273)	(0.408)	(0.579)
% of population in poverty	0.047	0.045	0.027	0.047	0.048	0.027
	(0.027)	(0.017)	(0.042)	(0.027)	(0.017)	(0.043)
Unemployment rate	0.126	0.077	0.127	0.146	0.077	0.158
	(0.067)	(0.021)	(0.070)	(0.067)	(0.021)	(0.069)
% of population black	0.011	0.018	0.015	0.010	0.018	0.016
	(0.012)	(0.013)	(0.051)	(0.012)	(0.013)	(0.051)
% of population Hispanic	0.043	0.045	0.089	0.043	0.042	0.095
	(0.014)	(0.006)	(0.012)	(0.014)	(0.006)	(0.012)
% of K–12 enrollment black	0.008	0.065	0.061	0.003	0.069	0.071
	(0.013)	(0.010)	(0.043)	(0.013)	(0.010)	(0.043)
% of population 12+ yrs of schl	−0.380	−0.447	−0.538	−0.372	−0.448	−0.527
	(0.021)	(0.010)	(0.025)	(0.021)	(0.010)	(0.026)
% of population 16+ yrs of schl	−0.108	−0.024	−0.072	−0.096	−0.021	−0.074
	(0.027)	(0.010)	(0.023)	(0.027)	(0.010)	(0.023)
% of K–12 enrollment private	−0.030	−0.087	−0.024	−0.029	−0.085	−0.022
	(0.016)	(0.009)	(0.021)	(0.015)	(0.009)	(0.021)
Log(pub0. K–12 enrollmt/1000)	0.574	0.323	−0.706	0.544	0.181	−0.706
	(0.138)	(0.125)	(0.436)	(0.129)	(0.125)	(0.445)
% of 16–19 yr-old population who are 18–19 yrs old	−0.029	−0.023	−0.261	−0.024	−0.027	−0.260
	(0.013)	(0.006)	(0.018)	(0.011)	(0.006)	(0.018)
State fixed effects	yes	yes	yes	yes	yes	yes
R^2	.64	.60	.32	.65	.62	.33

If unionization but no school inputs are included in the equation, the "full-effect" of unionization is obtained. The full effects that correspond to the columns in this table are 0.173 (0.296) for 1970, 0.125 (0.108) for 1980, and 0.485 (0.701) for 1990.

For other notes see the notes to Table VIb.

TABLE VIb

EFFECT ON DROPOUT RATE OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is the High School Dropout Rate in Percent

	1980–1990 first-differences estimates		1970–1980 first-differences estimates	
Unionized		1.751		1.252
		(0.841)		(1.225)
Student-teacher ratio	0.140	0.160	0.112	0.192
	(0.045)	(0.052)	(0.048)	(0.050)
Δ (unionized × student-teacher ratio)		−0.062		−0.035
		(0.065)		(0.050)
Log(avg teacher salary/1000)	−0.535	−0.581	−0.126	−0.341
	(0.578)	(0.594)	(0.765)	(0.776)
Δ (Unionized × log(avg teacher salary/1000))		0.196		0.798
		(0.344)		(0.336)
Log(population/1000)	1.200	1.133	−0.154	−0.138
	(0.648)	(0.650)	(0.140)	(0.141)
% of population urban	0.008	0.010	−0.008	−0.008
	(0.015)	(0.015)	(0.006)	(0.006)
Log(median HH inc/1000)	−2.363	−2.266	−2.480	−2.318
	(1.623)	(1.630)	(0.912)	(0.918)
Log(median rent)	−2.870	−2.959	−0.527	−0.653
	(0.475)	(0.477)	(0.813)	(0.827)
% of population in poverty	0.040	0.038	0.230	0.226
	(0.053)	(0.053)	(0.032)	(0.032)
Unemployment rate	0.160	0.153	0.290	0.289
	(0.049)	(0.050)	(0.051)	(0.052)
% of population black	0.075	0.075	−0.032	−0.032
	(0.041)	(0.041)	(0.021)	(0.021)
% of population Hispanic	0.223	0.218	0.119	0.118
	(0.047)	(0.047)	(0.027)	(0.027)
% of K–12 enrollment black	0.016	0.019	−0.008	−0.008
	(0.029)	(0.029)	(0.012)	(0.012)
% of population 12+ yrs of schl	−0.112	−0.108	−0.293	−0.288
	(0.029)	(0.029)	(0.024)	(0.024)
% of population 16+ yrs of schl	−0.038	−0.044	−0.112	−0.115
	(0.046)	(0.046)	(0.035)	(0.035)
% of K–12 enrollment private	0.098	0.094	0.113	0.116
	(0.032)	(0.032)	(0.022)	(0.022)
Log(public K–12 enrollment/1000)	2.537	2.440	3.289	3.482
	(0.662)	(0.625)	(0.421)	(0.428)
% of population 18–19 yrs old	−0.262	−0.262	−0.071	−0.068
	(0.012)	(0.012)	(0.015)	(0.015)
R^2	.13	.14	.25	.26

If unionization but no school inputs are included in the equation, the full effect of unionization is obtained. The full effects that correspond to the columns in this table are 1.067 (0.401) for 1980–1990 period, and 0.651 (0.320) for the 1970–1980 period.

If log(per pupil spending/1000) is used as the input measure instead of the combination of the student-teacher ratio and teacher salaries, similar results are obtained.

10,509 observations; unit of observation is a United States school district; standard errors are in parentheses; all covariates are indicated; regressions are weighted by 1982 district enrollment; see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

TABLE VIc

EFFECT ON DROPOUT RATE OF TEACHERS' UNIONS AND OTHER VARIABLES

Dependent Variable is the High School Dropout Rate in Percent

	Differences-in-differences		IV differences-in-differences estimates		
Unionized		0.627	2.275	2.410	2.685
		(0.209)	(1.096)	(1.186)	(2.731)
Student-teacher ratio	0.157	0.166		0.105	0.379
	(0.025)	(0.025)		(0.107)	(.205)
Unionized × student-teacher ratio					−0.262
					(0.106)
Log(avg teacher salary/1000)	−1.118	−0.980		−1.043	−7.406
	(0.357)	(0.357)		(1.289)	(3.873)
Unionized × log(avg teacher salary/1000)					2.488
					(1.195)
Log(population/1000)	−1.252	−1.269	−0.333	−0.234	−0.696
	(0.121)	(0.122)	(0.817)	(0.861)	(0.777)
% of population urban	−0.001	−0.002	−0.004	−0.004	−0.003
	(0.005)	(0.005)	(0.019)	(0.019)	(0.023)
Log(median HH inc/1000)	−0.346	−0.231	−0.303	−0.359	−0.256
	(0.583)	(0.584)	(1.258)	(1.287)	(1.276)
Log(median rent)	−3.661	−3.694	−4.113	−4.091	−3.336
	(0.303)	(0.304)	(1.847)	(2.033)	(1.535)
% of population in poverty	0.019	0.020	0.015	0.001	0.013
	(0.022)	(0.022)	(0.085)	(0.088)	(0.095)
Unemployment rate	0.297	0.300	0.436	0.430	0.422
	(0.030)	(0.031)	(0.147)	(0.149)	(0.137)
% of population black	0.034	0.032	−0.003	−0.006	0.025
	(0.017)	(0.018)	(0.092)	(0.095)	(0.089)
% of population Hispanic	0.287	0.299	0.228	0.246	0.230
	(0.028)	(0.028)	(0.060)	(0.056)	(0.048)
% of K–12 enrollment black	0.012	0.016	0.047	0.057	0.036
	(0.010)	(0.010)	(0.075)	(0.072)	(0.072)
% of population 12+ yrs of schl	−0.284	−0.273	−0.235	−0.232	−0.331
	(0.020)	(0.020)	(0.069)	(0.073)	(0.093)
% of population 16+ yrs of schl	−0.015	−0.039	−0.159	−0.178	−0.068
	(0.029)	(0.029)	(0.131)	(0.139)	(0.132)
% of K–12 enrollment private	0.277	0.271	0.248	0.273	0.227
	(0.016)	(0.016)	(0.065)	(0.067)	(0.065)
Log(public K–12 enrollment/1000)	4.182	4.248	2.890	3.308	5.162
	(0.401)	(0.408)	(1.712)	(1.812)	(2.285)
% of population 18–19 yrs old	−0.126	−0.128	−0.203	−0.201	−0.125
	(0.009)	(0.009)	(0.102)	(0.100)	(0.068)
R^2	.12	.13			
Standard errors adjusted for state random effects		yes		yes	yes

TABLE VIc
(CONTINUED)
Dependent Variable is the High School Dropout Rate in Percent

Test of identifying restrictions χ^2 with 2 d.f. (probability)	.1265 (.9387)	.1291 (.9374)	.1399 (.9324)

If log(per pupil spending/1000) is used as the input measure instead of the combination of the student-teacher ratio and teacher salaries, similar results are obtained.

10509 observations; unit of observation is a United States school district; standard errors are in parentheses except that the number in parentheses below the χ^2 test is the area of the right-hand tail (probability of the χ^2); regressions are weighted by 1982 district enrollment; see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

sign. The student-teacher ratio is predicted to have small effects. A decrease of one student in the student-teacher ratio improves the dropout rate by about 0.12 percentage points. The second and fourth columns of the table allow unionization to directly affect achievement and allow input productivity to differ between nonunion and union schools. In these first-differenced equations, the inclusion of unionization modestly "normalizes" the education production function. The estimated effects of the inputs on the dropout rate grow in magnitude (a one-student decrease in the student-teacher ratio improves the dropout rate by between 0.16 and 0.19 percentage points), although the effect of teacher salary remains insignificantly different from zero. What is more interesting is that the point estimates suggest that the efficacy of inputs is lower in unionized schools, although the differences are insignificantly different from zero. Also, unionization is predicted to have a direct worsening effect on student achievement. The 1980–1990 estimates predict, for instance, that unionized schools have dropout rates that are 1.8 percentage points higher, all else equal.[19]

We expect that omitted variables bias and measurement error bias will be ameliorated by estimating with differences-in-differences and instrumental variables. Table VIc shows these results. The left-hand column repeats the exercise of estimating the education production function without accounting for unionization. Eliminating school district-specific trends modestly moves the estimates closer to expectations. Both the student-teacher ratio and teacher salaries are predicted to affect student achievement in the expected directions, although the effects remain small. The second column, which estimates the "full effect" of unionization, shows that unionization is predicted to have an overall worsening effect on the dropout rate. Unionized schools have dropout rates that are 2.3 percentage points worse, all else equal. The most interesting results, however, are contained in the right-hand column. When unions are allowed to both directly affect the dropout rate and change the productivity of inputs, it appears that inputs are quite effective in nonunion schools while unionized schools show the traditional pattern of ineffective inputs. In nonunion schools, a one-student decrease in the student-teacher ratio improves the dropout rate by 0.4 percentage points and a ten percent increase in teacher salaries improves the dropout rate by 0.7 percentage points. These are economically relevant effects of inputs. The estimates for unionized schools, taking into account the coefficients on the interactions between unionization and the inputs, show no statistically significant input efficacy. In fact, unionized schools appear to suffer from the "typical" problem that inputs do not matter. Finally, the main effect of unionization is still to worsen the dropout rate. Although unions increase inputs, their direct effect on students plus the fact that input productivity falls means that their overall effect on student achievement is negative.

Figures V and VI provide an alternative presentation of the differences-in-differences and IV differences-in-differences results, akin to Figures III and IV for per-pupil spending. I calculated the residual from a cross-section regression of the dropout rate on the demographic variables (all the independent variables in the third column of Table VIc except unionization). For Figure V, I then plotted against time the weighted average residuals for districts that unionized (1) before 1972, (2) between 1972 and 1982, (3) between 1982 and 1992, and (4) never. The figure shows that, between 1970 and 1980, the dropout rate rose fastest in schools that unionized during the 1972–1982 period. The dropout rate was falling in schools that unionized later, never, or before 1972. Between 1980 and 1990 the dropout rate again rose in schools that had unionized during the previous decade, but at a slower rate of increase than during the previous period, when unionization was occurring. This lingering effect of unionization

19. The full effects of unionization on the dropout rate (noted at the bottom of Table VIb) are positive. These indicate that the overall effect of unions is to worsen student achievement, even accounting for their ability to obtain greater inputs.

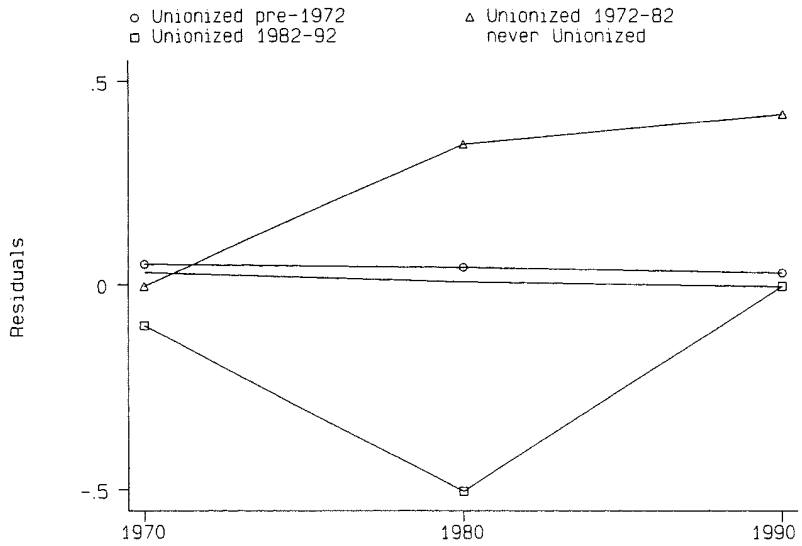

FIGURE V
Residual Dropout Rate, by Period of Unionization

is very plausible, particularly because students' achievement depends on many years of schooling experience. Moreover, between 1980 and 1990 schools that were in the process of unionizing experienced a reversal of the downward trend in their dropout rate. In fact, they experience a rise in their dropout rate similar to the rise experienced between 1970 and 1980 by schools that were unionizing then.

For Figure VI, I plotted against time the weighted average residuals for states that passed one or more of the three laws facilitating unionization (1) before 1970, (2) between 1970 and 1980, (3) between 1980 and 1990, and (4) never. Figure VI shows a pattern similar to that of Figure V. In the period in which they passed laws facilitating unionization, states experienced growth in the dropout rate, all else equal.

In summary, the results indicate that teachers' unions succeed in raising school budgets and school inputs but have an overall negative effect on student performance. Much of the negative effect on student performance can be shown to be the result of decreased productivity of schools inputs. The remaining portion of the negative effect may represent omitted variables such as teacher effort or administrative encumbrances. It is striking that

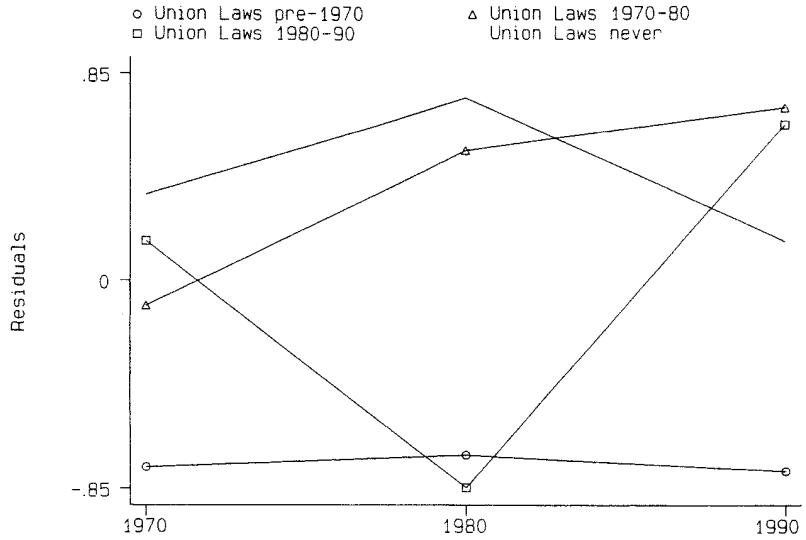

FIGURE VI
Residual Dropout Rate, by Period of Union Laws

unionization is associated with both more generous school inputs and worse student achievement. This is strong evidence that teachers' unions serve, at least in part, a rent-seeking purpose. Teachers' unions are, indeed, a potential answer to the puzzle of increasing school spending and stagnant student performance in the post-1960 period.

VII. TEACHERS' UNIONS AND SCHOOLS' MARKET POWER

In this section I briefly address the question of whether teachers' unions have greater effects on inputs and student achievement when they are located in areas where schools have greater market power. Either model of unions implies that teachers' unions will be able to exercise greater influence when schools face less competition; that is, when residents of school districts are less likely to react to increased or reallocated school spending by moving to another district. In a world where residents were costlessly mobile among school districts, it would be difficult for a teachers' union either to extract rent or to raise school spending to the socially optimal level if it were above the privately optimal level. Given the results of the previous section, I will focus on the

question of whether rent-seeking appears to be more successful in metropolitan areas with less competition among school districts for residents.

Other work has shown that the concentration of enrollment among districts in a metropolitan area is an indicator of residents' ease of mobility among school districts [Hoxby 1995a; Borland and Howsen 1992]. A Herfindahl index based on school districts' enrollment shares in a metropolitan area is a good summary measure of enrollment concentration. It has the advantage of familiarity (from industries) and of easy interpretation. The index varies from zero (an infinite number of equal-sized districts) to one (one district that monopolizes enrollment). The index increases as the number of districts shrinks and as their enrollment shares become more uneven.

In Table VII, I divide districts into those in metropolitan areas with high enrollment concentration (Herfindahl index > 0.15) and low enrollment concentration (Herfindahl index < 0.05). I then reestimate the equations from the previous tables for each group separately. Table VII shows only the coefficients on unionization from the IV differences-in-differences regressions, since I believe that these are the best identified.[20] It appears that unions have a stronger effect in areas with less competition among schools (more concentrated enrollment). For example, unionization raises per-pupil spending by 9 percent but by 4.3 percent in low concentration areas. Similarly, unionization lowers the student-teacher ratio by 2.5 students in high concentration areas but by 1.3 students in low concentration areas. Unionization increases the dropout rate by 2.2 percent in high concentration areas but by only 1.3 percent in low concentration areas. These results indicate that mobile parents may be able to constrain teachers' unions to accept lower budget increases, maintain higher levels of effort, and add fewer administrative encumbrances.

VIII. Conclusions

This study examines the effect of teachers' unionization, using panel data on United States school districts and state laws that facilitate teachers' unionization. Improvements over previous work include the careful measurement of teacher unionism,

20. The differences-in-differences estimates are reported in the notes accompanying the table.

TABLE VII
The Effect of Teachers' Unions in Metropolitan Areas with and without Highly Concentrated Schools
Estimates Shown are IV Differences-in-Differences Coefficients on Teachers' Unionization for the Specified Dependent Variable

Dependent variable	Metropolitan area has high enrollment concentration (Herfindahl index for enrollmt > 0.15)	Metropolitan area has low enrollment concentration (Herfindahl index for enrollmt > 0.05)
Log(per-pupil spending/1000)	0.090	0.043
	(.026)	(.035)
Log(avg teacher salary/1000)	0.057	0.044
	(.030)	(.033)
Student-teacher ratio	−2.508	−1.316
	(.561)	(.558)
High school dropout rate (no interactions in eqn., compare with third column of Table VIc)	2.542	1.238
	(1.465)	(1.267)

1950 observations in the high concentration sample, 5226 observations in the low concentration sample; unit of observation is a United States school district; standard errors are in parentheses; see Appendix 3 for means and standard deviations of variables. Equations are identical to IV differences-in-differences equation in Tables III, IV, V, and VIc (third column) but are estimated separately for metropolitan school district for which the Herfindahl index of enrollment concentration is high ($H > 0.15$) and is low ($H < 0.05$).

The differences-in-differences estimates (rather than the IV differences-in-differences estimates) of the same coefficients are as follows. For log(per-pupil spending/1000) as the dependent variable, the estimates are .039 (.016) [high conc.] and .019 (.007 [low conc.]. For log(average teacher salary/1000) as the dependent variable, the estimates are .033 (.019) [high conc.] and .015 (.009) [low conc.]. For student-teacher ratio as the dependent variable, the estimates are −2.356 (.352) [high conc.] and −1.320 (.2457) [low conc.]. For the high school dropout rate as the dependent variable, the estimates are 1.981 (.9646) [high conc.] and .523 (.587) [low conc.].

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Dataset.

the use of a number of district-specific demographic controls, and the panel nature of the data. Because I observe school districts before, during, and after the decade of unionization, I can attempt to distinguish the effects of unionization from factors that cause a school's teachers to unionize, even if those factors are trending over time. To further isolate the effects of unionization from events in a school that might cause unionization, I use the events of passages of relevant statewide laws to instrument for events of unionization. The resulting estimation strategy combines instrumental variables and differences-in-differences. I find that teachers' unions are primarily rent seeking, raising school budgets and school inputs but lowering student achievement by decreasing the productivity of inputs.

The results thus shed light on the two empirical puzzles that

motivated this paper. Teachers unions may be one reason why educational production functions estimated on cohorts schooled before 1960 often find that school quality "matters," while functions estimated on post-1960 cohorts only occasionally show similarly significant results. Second, teachers' unions may be a primary means whereby a lack of competition among public schools translates into more generous school inputs and worse student performance. Unions help us understand how the market structure of schooling affects actual school behavior and student achievement.

APPENDIX 1: DATA

A. Censuses of Governments

The 1972, 1982, and 1992 Censuses of Governments were matched using the unique government identification code. The student-teacher ratio is the ratio of total enrollment to full-time-equivalent instructional personnel. This is not the same as class size, but is more desirable for an analysis of teachers' unionism since teachers' unions draw members from all instructional personnel. Anecdotes suggest that teachers' unions expand employment of nonclassroom teachers, partly as a means of removing unsuccessful teachers from the classroom without job loss. Per-pupil spending is total district current expenditure divided by total enrollment. Average teacher's salary is the payroll for full-time instructional personnel divided by the number of full-time instructional personnel. For districts that consolidated between 1972 and 1992, statistics were computed as weighted averages of preconsolidation statistics. Consolidation histories were obtained from individual state reports issued by the Department or Office of Education. Due to unsuccessful year-to-year matching or Census of Governments–Census of Population matching, 585 districts were dropped, leaving 10,509 districts. See United States Department of Commerce [1976, 1986, 1995] for additional details.

B. Census of Population and Housing School District Files

School district summary files from the 1970 Census, 1980 Census (Summary Tape File 3F), and 1990 Census (School District Data Book) were used. I created the 1970 Census summary file by matching enumeration district and census block group data to school districts using the Master Area Reference File. The three Censuses were matched to one another and the Census of Governments data using school district codes from the National Center of Education Statistics. School districts with nonmatching codes were matched by name. The dropout rate is the number of 16 to 19 year-olds who are not enrolled in any school and do not have a high school diploma divided by the total number of 16 to 19 year-olds. Other variables derived from the Census are standard. See United States Department of Commerce [1973, 1982, 1983] and United States Department of Education [1994] for additional details.

C. NBER Public Sector Bargaining Law Data Set

The state-level law variables are matched by state and year. The "duty to (at least) meet" variable is equal to one if the collective bargaining rights variable in the data set is greater than or equal to two. The "agency shop explicitly allowed" variable is equal to one if the agency shop variable in the data set is greater than or equal to two. The "union shop explicitly allowed" variable is equal to one if the union shop variable in the data set is greater than or equal to two. See Valletta and Freeman [1988] for additional details. LEXIS (an online legal information retrieval system) searches resulted in a few updates of these variables for the period from 1985 to 1990 (beyond the period covered by the NBER data set).

D. Descriptive Statistics

APPENDIX 2: FIRST-STAGE LINEAR PROBABILITY EQUATIONS
Dependent Variable is Unionization

	First-differences (used only for footnotes to tables)		Difference-in-differences (Used for IV diffs-in-diffs)
	1970–1980	1980–1990	
Collective bargaining rights are "duty to meet" at least	.114	.140	.178
	(.068)	(.009)	(.033)
Agency shops explicitly allowed	.097	.061	.011
	(.042)	(.010)	(.026)
Union shops explicitly allowed	.107	.043	.160
	(.015)	(.008)	(.074)
Log(population/1000)	.018	−.002	.021
	(.011)	(.015)	(.010)
% of population urban	.001	.001	.001
	(.001)	(.001)	(.001)

APPENDIX 2: CONTINUED
Dependent Variable is Unionization

	First-differences (used only for footnotes to tables) 1970–1980	1980–1990	Difference-in-differences (Used for IV diffs-in-diffs)
Log(median HH inc/1000)	.024	.116	−.038
	(.036)	(.026)	(.032)
Log(median rent)	.070	−.003	.001
	(.025)	(.016)	(.013)
% of population in poverty	.003	.003	.001
	(.001)	(.001)	(.001)
Unemployment rate	.001	−.002	.002
	(.002)	(.001)	(.001)
% of population black	.001	.004	.001
	(.001)	(.001)	(.001)
% of population Hispanic	−.001	−.001	.001
	(.001)	(.001)	(.001)
% of K–12 enrollment black	.001	−.002	−.001
	(.001)	(.001)	(.001)
% of population 12+ yrs of schl	−.001	−.001	.001
	(.001)	(.001)	(.001)
% of population 16+ yrs of schl	.002	−.001	.003
	(.001)	(.001)	(.001)
% of K–12 enrollment private	−.000	.001	.001
	(.001)	(.001)	(.001)
Log(public K–12 enrollment/1000)	−.012	−.013	−.010
	(.015)	(.020)	(.016)
F-statistic, test that law variables jointly equal zero (prob > *F*-stat)	203.36	5.75	4.26
	(.000)	(.0006)	(.0052)
Standard errors adjusted for state random effects	yes	yes	yes

10,509 observations; unit of observation is a United States school district; standard errors are in parentheses except that the number in parentheses below the *F*-statistic is a *p*-value (probability > *F*-statistic); all covariates are indicated; regressions are weighted by 1982 district enrollment; see Appendix 3 for means and standard deviations of variables.

Data Sources. 1972, 1982, and 1992 Census of Governments; school district tabulations of the 1970, 1980, and 1990 Census of Population and Housing; NBER Public Sector Bargaining Law Data Set.

APPENDIX 3: DESCRIPTIVE STATISTICS—UNWEIGHTED DISTRICT MEANS AND STANDARD DEVIATIONS

Variable	Mean	Std. Dev.
Metropolitan area	.37	.48
Student-teacher ratio 1992	15.93	3.76
Log(avg teacher salary/1000 current $) 1992	3.67	.29

Log(per-pupil spending, current $) 1992	8.50	.34
Dropout rate (in pct.) 1990	15.60	15.12
Unionized 1992	.35	.47
Log(population/1000) 1990	1.91	1.50
Pct. of population urban 1990	40.12	41.60
Log(median household income/1000 current $) 1990	3.28	.36
Log(median rent, current $) 1990	5.67	.64
Pct. of population in poverty 1990	13.62	9.42
Unemployment rate 1990	6.50	3.82
Pct. of population black 1990	5.69	12.61
Pct. of population Hispanic 1990	5.42	13.11
Pct. of K–12 enrollment black 1990	6.97	15.36
Pct. of population with 12+ yrs school 1990	73.86	11.42
Pct. of population with 16+ yrs school 1990	14.91	10.14
Pct. of K–12 enrollment private 1990	9.07	8.32
Log(public K–12 enrollment/1000) 1990	.09	1.51
Student-teacher ratio 1982	14.28	3.25
Log(avg teacher salary/1000 current $) 1982	2.93	.23
Log(per-pupil spending, current $) 1982	7.77	.29
Dropout rate (in pct.) 1980	12.26	8.73
Unionized 1982	.28	.45
Log(population/1000) 1980	1.66	1.34
Pct. of population urban 1980	32.43	39.44
Log(median household income/1000 current $) 1980	2.74	.29
Log(median rent, current $) 1980	5.33	.29
Pct. of population in poverty 1980	12.84	7.73
Unemployment rate 1980	7.16	4.21
Pct. of population black 1980	4.12	10.58
Pct. of population Hispanic 1980	3.84	10.29
Pct. of K–12 enrollment black 1980	4.55	12.45
Pct. of population with 12+ yrs school 1980	63.98	12.75
Pct. of population with 16+ yrs school 1980	12.41	8.22
Pct. of K–12 enrollment private 1980	7.82	7.37
Log(public K–12 enrollment/1000) 1980	−.09	1.39
Student-teacher ratio 1972	18.11	3.81
Log(avg teacher salary/1000 current $) 1972	2.24	.24
Log(per-pupil spending, current $) 1972	6.72	.32
Dropout rate (in pct.) 1970	12.95	10.00
Unionized 1972	.13	.34
Log(population/1000) 1970	1.32	1.55
Pct. of population urban 1970	48.26	33.76
Log(median household income/1000 current $) 1970	2.13	.25
Log(median rent, current $) 1970	4.47	.26
Pct. of population in poverty 1970	15.60	10.35
Unemployment rate 1970	4.74	2.60
Pct. of population black 1970	5.44	11.49
Pct. of population Hispanic 1970	4.21	11.36
Pct. of K–12 enrollment black 1970	7.36	12.89
Pct. of population with 12+ yrs school 1970	50.29	12.77
Pct. of population with 16+ yrs school 1970	8.79	5.70

APPENDIX 3: CONTINUED

Variable	Mean	Std. Dev.
Pct. of K–12 enrollment private 1970	7.29	7.52
Log(public K–12 enrollment/1000) 1970	−.08	1.71

Information about passage of laws facilitating unionization may be found in Table II.

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