

Direct Democracy and Public Employees

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In the public sector, employment may be inefficiently high because of patronage, and wages may be inefficiently high because of the strength of public employee interest groups. This paper explores whether the initiative process, a direct democracy institution of growing importance, can control these political economy problems, as proponents and some research suggests. Based on a sample of 500+ cities in 2000, I find that when public employees are allowed to bargain collectively, driving up wages, the initiative appears to cut wages by about 5 percent but has no measurable effect on employment. When public employees are not allowed to bargain collectively and patronage is a problem, initiatives appear to cut employment but not wages.

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1. Introduction

A central theme in political economy is that policy is the product of competition between interest groups.¹ While some research suggests that interest group competition can result in efficient outcomes (e.g. Becker, 1983), a more common view is that it creates problems for democracy by allowing narrow interests to override broader “public” interests. Concern over the problems of interest group competition have led scholars and practitioners to search for institutions that can counteract the influence of interest groups, and identifying the policy consequences of key institutions has become an important part of the political economy research agenda (Besley and Case, 2003).

The purpose of this paper is to examine the impact of the initiative process, an institution of growing importance, on policymaking in American cities. The initiative is a form of direct democracy in which individuals outside the legislature can propose laws that are adopted or rejected in a vote of the population at large. This institution is widespread: according to a recent survey over 80 percent of cities allow initiatives, including most of the largest cities. Ballot propositions are also increasingly used to make local policy decisions in Western and Eastern Europe, Taiwan, Japan, and South Korea.

The initiative process changes the rules of competition by allowing individuals and groups outside the government to propose policies, breaking the agenda control of elected officials. Theory suggests that opening up the agenda benefits the majority of citizens because the median voter will reject any proposal that adversely changes the status quo (Gerber, 1996; Matsusaka and McCarty, 2001), and existing evidence generally supports this conclusion.² However, the theoretical conclusion that initiatives help the median voter is more tenuous outside the complete information context of most models. When voters are uncertain about the consequences of policies or when politicians are uncertain about voter preferences, giving agenda control to outsiders can make the

¹ Bentley (1908/1995) is an early study of interest group competition. More recent models by Stigler (1971), Peltzman (1976), Becker (1983), and Grossman and Helpman (2002) have spawned a vast literature.

² Matsusaka (2007a, 2007b) documents that initiative states are 15 to 20 percent more likely to adopt the majority policy position than noninitiative states for a set of high profile issues. Gerber (1999) and Matsusaka (2004) contain less direct evidence. See Lupia and Matsusaka (2004) for a survey.

median voter worse off as politicians may accommodate extreme groups to avoid the risk of extreme ballot propositions (Gerber and Lupia, 1995; Matsusaka and McCarty, 2001). In addition, many political observers (e.g. Broder, 2000) hold the view that direct democracy actually may benefit special interests rather than the electorate at large because voters are ignorant and easy to deceive.³

This paper focuses specifically on the connection between the initiative process and employment and wage policies in American cities. The role of direct democracy in public sector employment has not been previously explored, to the best of my knowledge, but it seems ripe for study. As of 2004, 21.5 million people worked in the public sector, roughly 15 percent of the labor force, and over 64 percent (13.7 million) of them were employed by local governments. Local governments are a key point at which many citizens interact with their government, and they are the primary providers of education, police, fire protection, water, sewerage, and other services that are critical to the quality of life. Moreover, labor services are a huge component of local government spending, totaling \$464 billion in 2003, and comprising 40 percent of local government budgets.⁴

Cities provide an appealing environment to study whether the initiative can offset interest group influence because municipal employment practices seem particularly vulnerable to political economy problems. For one thing, employment levels may be inefficiently high if politicians pad the public payroll with patronage employees. In addition, wages may too high if public sector workers are able to organize and bring political pressure to bear. Unlike the private sector, unionization rates remain high among public employees, with 41 percent of local government workers covered by unions in

³ The view that voter incompetence allows interest groups to subvert the majority remains one of the central criticisms of direct democracy (Bowler and Donovan, 1998). Although a formal model of the idea remains to be written, the intuition is that interest groups may have an advantage in informing their supporters and motivating them to vote that allows them to pass laws that hurt the majority. (The idea that interest groups might be able to achieve nonmajoritarian policies because of organizational advantages has been modeled – Peltzman (1976) is an influential example – but what has not been shown is that the interest group’s advantage will be increase when direct democracy is available.)

⁴ The numbers in this paragraph are adapted from various tables in *Statistical Abstract of the United States*, 2007.

2004, and public employee unions are often active in candidate elections and ballot proposition campaigns.

The main finding of the paper is that the initiative is associated with employment cuts in situations where patronage is likely to be a problem, and is associated with wage cuts in situations where excessive compensation is likely to be a problem, suggesting that the initiative does help control these political economy problems. The empirical strategy, common in the literature on institutions, is to compare the policies of cities with and without initiatives (controlling for other factors) and to attribute the differences to availability of the initiative. The potential endogeneity of the institution is less a concern here than in other contexts because most cities appear to have adopted the initiative almost a century ago. In a sample containing 500+ medium-to-large American cities in 2000, I find that wages are higher and employment is lower in cities where collective bargaining is allowed than in cities where it is prohibited, consistent with traditional views of union bargaining. In cities with collective bargaining, the initiative is associated with large, statistically significant wage cuts on the order of 5 percent. Since collective bargaining is estimated to increase wages by 18 percent, the initiative appears to undo about one-third of the union premium. In cities without collective bargaining, on the other hand, where wages are less likely to be excessive, the initiative is associated with large cuts in the number of public sector jobs, but not with reductions in wages, consistent with the idea that voters are using initiatives to roll back patronage hires. Thus, the initiative appears to change policies in a way that counteracts specific political economy problems in public sector employment.

The paper is part of a growing literature on the policy effects of direct democracy. A substantial literature has documented that initiatives tended to cut spending and taxes in American states over the last several decades and similar patterns have been found for Swiss cantons and communes.⁵ There is little evidence on how the spending cuts are

⁵ Matsusaka (1995, 2004, 2005) contain evidence and surveys of the literature. Much of the Swiss evidence is in a stream of studies by Lars Feld and Gebhard Kirchgässner, with coauthors, for example, Feld and Kirchgässner (1999, 2000, 2001), Feld (2002), and Feld and Matsusaka (2003). There is also a small literature on initiatives and spending in American cities that suggests higher spending in initiative cities, but not consistently (Zax, 1989; Farnham, 1990; Matsusaka, 2004, Ch. 3).

achieved, or how reduced revenue affects the operations of government. Since in many cases initiatives take the form of tax and expenditure limits, the paper can also be seen as part of a large literature that investigates the consequences of fiscal constraints (e.g. Poterba, 1994, 1995; Primo, forthcoming). Indeed, the paper's analysis raises the possibility that tax and expenditure limits may be responses to specific political economy problems associated with public employees, and not to a general dissatisfaction with the size of government.

The paper is arranged as follows. Section 2 develops a theoretical framework to motivate and interpret the evidence. Section 3 describes and summarizes the data. Section 4 reports the main results on the different policies of initiative and noninitiative cities. Section 5 concludes.

2. Theory

To motivate the empirical analysis, this section develops a simple model in which elected officials derive patronage benefits from public workers, collective bargaining allows public employees to drive up wages, and the initiative matters through its influence on agenda control. The basic setup follows Gerber (1996) and Matsusaka and McCarty (2001) (and thus Romer and Rosenthal (1979)), and draws on the model of public sector wage determination in Babcock et al. (1997) (which itself is based on a long tradition in labor economics (Farber, 1986)). Because the potential political economy problems are different when public employees are organized than when they are not organized, the model is developed first for the case where collective bargaining by government employees is not allowed, and then compared with an extended model in which collective bargaining is allowed.

A. Effect of the Initiative When Collective Bargaining Is Not Allowed

A city with N residents chooses the number of public employees, L , and the wage per worker, w , associated with provision of a public service, such as police or fire protection. Public employees are identical in terms of skill, with an infinitely elastic

supply at the wage \bar{w} .⁶ It is possible to pay a wage greater than \bar{w} , and public employees will want the city to do so. The number of public employees per capita is denoted $l = L/N$.

The citizens of the city are assumed to be identical to abstract away from distributional issues. Each citizen has preferences $u(l, x)$ over the number of workers per capita, l , and consumption of some other good, x , and utility is increasing and concave in both arguments. Taxes are distributed equally, implying a tax burden of wl for each person. Given income y , denoted in units of the consumption good, and normalizing the price of x to one, a citizen's budget constraint is $wl + x = y$. The utility function can then be expressed as $U(l, wl) \equiv u(l, y - wl)$. For citizens, the first-best public employment policy, denoted $P^* = (l^*, w^*)$, is the solution to: $\max U(l, wl)$ subject to $w \geq \bar{w}$. To put a bit more structure on the problem, I also assume that the utility function is such that the demand for public employees is decreasing in the wage. Citizens prefer a policy (l, w) where the wage constraint is binding; they never find it optimal to pay a higher wage than necessary to elicit the desired quantity of labor. Figure 1 depicts a possible outcome.

In the absence of initiatives, employment and wages are chosen by elected officials, the mayor, city manager, city council, and so on. Elected officials are assumed to care about the utility of citizens because they must stand for reelection, but they also receive private benefits from public sector employment per se. Elected officials may benefit from public sector jobs because it gives them control over patronage, they enjoy running a larger organization, or because government workers can help them in elections (Wilson, 1961). Elected officials will be treated as a unitary actor called "the politician" with a utility function of $V(l, w) = U(l, wl) + \alpha l$, where $\alpha > 0$ captures the value of patronage to the politician. Given a free hand, the politician chooses (l, w) to maximize V . The politician, like the citizens, does not want to pay any more than necessary to elicit a given amount of labor so selects a policy $P_0 = (l_0, w_0)$ that lies on the supply curve. It is straightforward to show that the politician hires more public employees than citizens

⁶ Supply could be endogenized by assuming that public employees are drawn from the pool of citizens, in which case the supply curve is simply the opportunity cost of the citizens. A flat supply curve conveniently reduces all wage movement to those arising from political considerations.

would hire, $l_0 > l^*$, and pays them the same wage, $w_0 = w^*$. In the absence of initiatives, policy P_0 prevails, as shown in Figure 1.

Now suppose initiatives are available. Any group can propose an alternative policy, $P_I = (l_I, w_I)$ and citizens choose between the initiative and the status quo P_0 . Because citizens must approve an initiative for it to go into effect, only initiatives that increase the utility of citizens will win. To see the possible winning proposals, Figure 1 shows the citizen's indifference curve U_0 (dashed) through the point P_0 . Policies below the indifference curve deliver higher utility to the citizens, and policies below the supply curve are infeasible, so the set of potentially successful initiatives is the shaded region. Several implications follow. First, initiatives will succeed only if they reduce public sector employment. Second, an initiative that proposes a wage increase could be approved – voters would like lower wages, but may be willing to accept higher wages as part of a package that reduces total expenditure (taxes) if forced to choose between that and the status quo. Third, only initiatives that reduce total expenditure can succeed. Intuitively, initiatives that reduce employment make the voter better off only if they reduce the tax burden.

If there was no cost to proposing an initiative, then some citizen would propose an initiative $P_I = P^*$, voters would approve the proposition, and the outcome would be at the citizen's ideal point. This would rule out the possibility that the initiative increases wages. In practice, however, it is costly to collect signatures and run a ballot proposition campaign. The price tag for placing a measure on the ballot runs \$1 to \$10 per signature, and cities often require signatures equal to 10 to 15 percent of the vote cast in the previous election (Gordon, 2004). For a statewide measure in California, normally it costs at least \$1 million to put a measure on the ballot. Systematic information is not available on the cost of running a local ballot proposition campaign once a measure is on the ballot, but credible statewide campaigns in California often require at least \$20 million, and the record for a single campaign is \$150 million spent by both sides on Proposition 87 in 2006. As a result of the significant costs of making a proposal, there is no guarantee that an initiative at the citizen's ideal point will be proposed. Without further information on which groups are capable of making proposals, we can only

conclude that initiatives will bring about a policy shift into the shaded region, giving the first testable implication.

Implication 1. *In the absence of collective bargaining, introduction of the initiative reduces employment and expenditure, but has an ambiguous effect on wages.*⁷

B. Effect of the Initiative when Collective Bargaining Is Allowed

Policy outcomes and the effect of initiatives are different when collective bargaining is allowed. To study the impact of collective bargaining on local government employees, I follow Babcock et al. (1997) and assume public employee groups seek to maximize the wage paid to their members, w . When initiatives are not available, wage-setting involves only negotiations between the politician and representatives of public employees. The bargaining process is modeled by assuming that public employees choose the wage, subject to providing the politician with a reservation level of utility. This incorporates the idea that the final policy is a compromise between the two sides, and is a version of the “efficient contracts” approach to union bargaining (Farber, 1986). Figure 2 depicts the indifference curve for the politician that corresponds to a reservation utility, V_0 . Public employees maximize their wage subject to the policy remaining on or below V_0 , resulting in a collective bargaining outcome $P_{CB} = (l_{CB}, w_{CB})$.⁸

The most obvious consequence of collective bargaining is to increase wages above what the politician or citizen would prefer, $w_{CB} > w_0$. Employment, on the other hand, is lower with collective bargaining, $l_{CB} < l_0$ (see Proof A in Appendix 1). This can be understood as the politician’s reaction to the higher wage: faced with a higher price for

⁷ If we were to assume that all initiatives come from citizens and that the politician chooses policy to preempt the initiative, as in the next section, the implication would be that the initiative cuts employment and has no effect on wages.

⁸ An alternative assumption about the union’s objective function is that it values both wages and employment. However, as Farber (1986) notes, union contracts always set conditions on wages but rarely specify employment. In terms of Figure 2, if the union’s utility function was increasing in both wages and employment, the collective bargaining outcome would be where the union indifference curve is tangent to V_0 . In this case, collective bargaining also increases wages but may not result in lower employment.

employees and thus a higher price for patronage, the politician prefers to cut back on the quantity of labor. Although the diagram suggests that employment under collective bargaining is higher than the employment at the citizen's ideal point, either relation is possible. The effect of collective bargaining on total expenditure is ambiguous as well.

Now consider what happens if the initiative is introduced. As before, only a proposition that makes the citizens better off than the status quo will be approved. However, determining the status quo is a bit more complicated in this situation. At first glance, P_{CB} might seem like a candidate for the status quo. If so, then any initiative that delivered a utility to the citizens at least equal to the utility from P_{CB} (and was above the supply curve) would be feasible. The reason P_{CB} might not be the status quo is that availability of the initiative is likely to change the bargaining game between the politician and public employees. Both groups are aware that a future initiative may undo the policy they negotiate, which changes the set of policies they will agree to.

To say more about how availability of the initiative affects policy in this case, it is necessary to add more structure about potential initiatives. In principal, initiatives could be proposed by individuals whose preferences are aligned with citizens, the politician, or public employees. In order to focus on the implications of the hypothesis that initiatives help citizens, I assume that proposals originate with citizen groups. In this case, if an initiative appears, it will propose policy P^* . Suppose the cost of putting an initiative before the voters is C , measured in units of utility. A citizen will go forward with an initiative if and only if the status quo offers less utility than $U_C = U(l^*, w^*l^*) - C$. Figure 2 represents the citizen group's indifference curve for some U_C . The indifference curve U_C provides an additional constraint on the negotiation between the politician and public employees: if they agree to a contract outside the bounds set by U_C , the citizen group will override the contract with an initiative.

With this threat in the background, we can now identify the equilibrium policy choice. As before, the public employees maximize the wage subject to delivering the politicians a minimum utility of V_0 , but now also subject to providing the citizens a minimum utility of U_C . Focusing on cases where both constraints are binding, the

solution is $P_I = (l_I, w_I)$, as indicated in Figure 2. This policy choice by construction will not be overridden by an initiative – indeed, there will not even be an initiative on the ballot – but the outcome is nevertheless different from the case where the initiative is unavailable. This is often referred to as the “threat” or “indirect” effect of the initiative, and there is anecdotal and statistical evidence that it is important in practice.⁹ Introduction of the initiative reduces the wage ($w_I < w_{CB}$), employment ($l_I < l_{CB}$), and total expenditure (see Proof B in Appendix 1).¹⁰

Implication 2. *When collective bargaining takes place, introduction of the initiative reduces wages and employment.*

The main message from this model is that the initiative has a different effect on employment policy depending on whether collective bargaining takes place. Without collective bargaining, the initiative cuts employment to alleviate the patronage problem and may or may not change wages. With collective bargaining, the initiative cuts wages to counteract the effect of collective bargaining on compensation. The initiative also cuts employment, but the cuts can be small if the collective bargaining wage is high. Intuitively, collective bargaining drives up wages, which causes the politician to reduce employment, partially solving the patronage problem and making further employment reductions unnecessary.

⁹ An interesting recent example comes from the city of Los Angeles. In November 2006, the city council passed a living wage ordinance that applied to hotels near LAX airport. After business groups began collecting signatures for an initiative, the city council repealed the ordinance in February 2007, and in March 2007, passed a version somewhat more accommodating to business interests. For other evidence, see Gerber (1998) and Matsusaka (2007c).

¹⁰ If other groups can propose initiatives, then P_I can be defeated by any policy below U_C . For example, public employee groups could propose a winning measure that increased wages and cut employment, while elected officials could propose a winning measure that would cut wages further and increase employment. Of course, if these groups were inclined to propose initiatives, then the initial policy would be different. Those extensions unfortunately are beyond the scope of this paper.

C. Implementing Initiative Outcomes and Tax and Expenditure Limits

Initiatives that explicitly specify both wages and the number of government employees are rare. It is natural to ask, then, how the policy changes implied by the theory can be implemented in practice. One way, and perhaps the most common, is by preemptive action of government officials. Rather than run the risk of an initiative that will result in a policy at the citizen's ideal point, they would prefer to adopt a status quo that yields the citizen a high enough utility to deter the initiative.

Another approach would be to adopt an initiative that limits total spending or taxes. Propositions of this nature are not at all rare, and are often coupled with requirements that subsequent expenditure or tax increases must be approved by the voters. A spending limit of $wl < k$ can be represented as a hyperbola in Figures 1 and 2, where any policy choice above or to the right of hyperbola is not permitted. If the hyperbola cuts through P^* , then the optimal policy choice for the politician under the spending constraint would be P^* . Thus, although it might seem like a crude tool, a tax and expenditure limit can be an effective way to force the politician to adopt wage and employment policies favored by the citizens.

3. Data Sources and Basic Patterns

The empirical part of this paper studies the employment and wages of a sample of cities in 2000. The most difficult information to obtain is the initiative status of cities. There is no central clearinghouse for such information, and the data source used in most previous studies – the ICMA's Form of Government Survey – is unreliable. The ICMA data are collected through a survey of city clerks and it appears that the respondents often report incorrectly.¹¹ For initiative information, I utilize a new data source, the *Legal Landscape Database*, that was collected by examining the state constitution, state statutes, municipal charters, and municipal ordinances for the 1,000 largest cities in the

¹¹ For example, in the 1996 survey, only 198 of 311 California cities indicate that the initiative is available, even though California law appears to grant initiative rights to citizens in every city in the state (California Constitution, Article 2, Section 11; California Statutes, 9200-9224). It may be that the city clerks who fill out the surveys are unaware of the state provisions, or may misunderstand the somewhat ambiguous survey questions. For a discussion of the ICMA survey, see Matsusaka (2003).

United States and 10 largest cities in each state.¹² This database describes the direct democracy provisions, if any, for approximately 1,500 American cities in 2005. The variable of interest from this database is a dummy equal to one if a city allows initiatives (proposals for new ordinances or charter amendments that are placed on the ballot by citizen petition), and zero otherwise.

The initiative data were matched to a variety of Census Bureau data from 2000. Information on city wages and employment by function was taken from *Local Government Employment and Payroll Data, 2000*. Demographic and economic information was taken from American FactFinder. The employment and payroll data and the demographic and economic data are based on samples, and not comprehensive, resulting in the loss of about 500 observations from the *Legal Landscape Database* after merging.¹³ Because the initiative information is for a single year, it was not possible to exploit time series variation in initiative status.

Finally, information on collective bargaining laws for local government employees in each state was taken from the *NBER Public Sector Collective Bargaining Law Data Set*.¹⁴ This data set describes state laws pertaining to collective bargaining by local police, firefighters, teachers, and “other” government workers, using a six category classification. For each group of workers, I classified a state as allowing collective bargaining if state law granted employees a right to meet or contained an implied or explicit duty to bargain. A state was classified as not having collective bargaining if

¹² The *Legal Landscape Database* was collected by the USC-Caltech Center for the Study of Law and Politics and the Initiative & Referendum Institute, under a grant from the Haynes Foundation. It is publicly available at www.iandrinstute.org. This does not eliminate the possibility of error.

¹³ For more information on the employment and payroll data, see *Government Employment*, U.S. Census Bureau, March 2000. The source files are 00empst.dat and 00empid.dat. The demographic and economic data are extracted from Census 2000 Summary File 1 (SF1) 110-Percent Data and Census 2000 Summary File 3 (SF3) – Sample Data, available at www.factfinder.census.gov. Only cities with a population of 25,000 or more survived the match with the Census data.

¹⁴ I use the updated numbers for 1996 that were collected by Kim Rueben, who I thank for providing the data. When data for 1996 were unavailable, I used information from 1991, the most recent year otherwise available.

collective bargaining was prohibited, the law was silent, cities were authorized but not required to bargain, or employees had the right to present proposals but no other rights.¹⁵

To give some perspective on the prevalence of direct democracy, Table 1 reports the percentage of cities in the sample that allow initiatives. Overall, 83.3 percent of sample cities allow the initiative.¹⁶ Initiatives are most likely to be available in the West (95.8 percent cities) and least likely to be available in the Midwest (63.6 percent) and Northeast (65.4 percent). A similar pattern appears for state-level initiatives (available in 24 of 50 states), where most states west of the Mississippi allow them. The table also shows that cities are much more likely to permit initiatives in states that allow statewide initiatives than cities in states that do not allow statewide initiatives, 94.5 percent compared to 59.1 percent. Finally, Table 1 shows that initiative availability is more common in large than small cities. Except for cities with a population below 25,000, there is a monotonic relation, rising from 78.6 percent availability to 87.5 percent availability in cities with a population over 250,000.¹⁷

No systematic information is available on when cities adopted the initiative or how often initiative status changes. However, the impression from a variety of less systematic sources suggests that most cities with the initiative adopted it long before the sample period, and the lack of any mention in the sources of cities repealing the initiative

¹⁵ Thus, collective bargaining represents codes 4-6 in the data set and no collective bargaining represents codes 0-3. I experimented with other cutoff points and with a variable that took on values from 1 to 6, and the results were qualitatively similar. I also explored information on right to strike and right to work laws that is included in the data set, but the collective bargaining laws seem to have more explanatory power. When studying city employees in aggregate, I classified a city as having collective bargaining overall if it had collective bargaining for two or more out of police, firefighters, and “other.”

¹⁶ This is quite a bit higher than the numbers reported in Matsusaka (2003) that were based on ICMA data. For example, in the ICMA’s 1986 survey, 42 percent of cities reported having the initiative, 42 percent reported not having the initiative, and 16 percent did not know or did not respond. As discussed above, the lower reported prevalence of the initiative in the ICMA survey is probably due to errors made by the city clerks who complete the surveys. The different numbers do not seem to be due to differences in the regions or populations of the sample cities because the differences remain even within region and city size subsamples. Compare Table 1 here with Tables 4 and 5 in Matsusaka (2003).

¹⁷ High initiative availability for the smallest cities is likely to be due to the fact that the dataset oversamples the smallest California cities.

process suggests that it rarely happens. It seems likely that most initiative cities adopted the process between 1900 and 1920, in the midst of the Progressive movement, the period when most statewide initiative processes were adopted. San Francisco and Vallejo in California were apparently the first cities to adopt, in 1898. By 1900, Nebraska and South Dakota had granted initiative rights to most cities, and Arkansas, Colorado, Maine, Montana, Oklahoma, Ohio, Oregon, Utah, and Wisconsin followed in the next decade (Oberholtzer, 1911, Ch. 17). A 1911 survey of states and cities (Bradford, 1911, Ch. 19) found that initiative charter amendments were allowed in 38 of 51 cities examined and state statutes allowed municipal initiatives in 15 of 21 states.¹⁸ The cities that allowed initiatives by 1910 included Oakland and San Diego in California, Colorado Springs in Colorado, Miami and St. Petersburg in Florida, Lewiston in Idaho, Lynn and Haverhill in Massachusetts, Grand Rapids and Pontiac in Michigan, Reno in Nevada, Greensboro and Wilmington in North Carolina, Portland in Oregon, Dallas and Fort Worth in Texas, and Spokane and Tacoma in Washington. Portland was particularly energetic in using the new process, voting on seven initiatives in 1909 and eight in 1911 (Oberholtzer, 1911, p. 428). The apparent fact that most cities adopted the initiative process almost a century ago is important in the empirical analysis to allay some concerns about the endogeneity of the institution.

Figure 3 provides a breakdown of payroll for local governments as of March 2000. Local governments include counties, municipalities, townships, and special districts. Education and hospitals, an important part of local spending, are excluded since most of this money is spent by school districts and counties, not cities. The four most important functions in terms of payroll are police, administration, streets and highways, and firefighters. Accordingly, the analysis that follows focuses on these four functions as well as on overall municipal employment and payroll.

Table 2 reports summary statistics for employment, wages, and payroll function for the cities in the final sample. Here and throughout, employment is expressed as full time equivalent (FTE) employment per 10,000 city residents. Wages are annualized total payroll divided by FTE employment. Payroll is total expenditure on wages ($\text{wages} \times$

¹⁸ California, Idaho, Illinois, Iowa, Kansas, Louisiana, Minnesota, Montana, New Jersey, North Dakota, South Carolina, South Dakota, Washington, Wisconsin, and Wyoming.

employment) on an annualized basis. One limitation of these data is that they do not include information on the benefits part of compensation. Benefits are likely to be a significant part of total compensation, but there is no a priori reason to expect their omission to bias the key coefficients below one way or another.¹⁹ Table 3 shows that for the sample cities, police employment averages 25.97 per 10,000 residents, with an average annual salary of \$45,945, and payroll expenditure of \$9.83 per capita. Consistent with the aggregate information in Figure 3, the sample summarized in Table 2 indicates that the number of employees and payroll expenditure is greatest for police, followed by fire fighters. Average wages are highest for fire fighters, followed by police, administration, streets, and “all other.”

4. Empirical results

The analysis that follows seeks to identify policy changes brought about by the initiative. The approach is to estimate policy difference between initiative and noninitiative cities, controlling for other explanatory factors, with regressions of the form

$$(1) \quad Y_i = \beta_0 + \beta_1 I_i + \beta_2 B_i + \beta_3 I_i B_i + \beta_4 X_i + e_i,$$

where i indexes a city, Y_i is the dependent variable (employment, wages, or payroll), I_i is a dummy variable for whether city i allows the initiative, B_i is a dummy variable for whether collective bargaining is allowed in the state of city i , $I_i B_i$ is an interaction term that allows the effect of the initiative to be conditional on availability of collective bargaining, X_i is a vector of control variables, e_i is an error term, and β_0, \dots, β_4 are coefficients to be estimated. As discussed above, whether or not a city has the initiative in most cases was determined up to a century before the policy choices examined here, so can reasonably be treated as exogenous. Similarly, since a city’s collective bargaining status is determined by state law, it can plausibly treated as exogenous. If the controls are

¹⁹ Another issue that should be kept in mind is that some cities report no expenditure for a given function, presumably because they have subcontracted out the service to the county or a neighboring city. Such observations are not included in the estimates by function although they appear as zeroes in the totals.

adequate, then, policy differences between initiative and noninitiative cities can be attributed to the initiative.

In this specification, the effect of the initiative is conditional on whether or not collective bargaining is available, so the effect of the initiative is given by β_1 if collective bargaining is not available ($B_i = 0$) and is given by $\beta_1 + \beta_3$ if collective bargaining is available ($B_i = 1$). This approach not does reveal the precise mechanism by which the policy changes come about – directly through actual ballot propositions or indirectly through the threat of an initiative – but it captures the full net effect of having the initiative available in a city.

A. All Functions Combined

Table 3 presents three regressions of (1) that seek to explain the employment, wages, and payroll expenditure for city workers as a whole. Each column in Table 3 reports estimates from a regression with the dependent variable indicated at the top of the column. Before discussing the primary variables interest, a few comments on the control variables are in order (summary statistics are in the Appendix B). The purpose of the control variables is to capture factors that might influence employment and wages independent of initiative availability and collective bargaining status. The first three variables are connected to population. If there are fixed costs to providing public services (such as building a fire station), a populous city may enjoy economics of scale in the provision of services, making them a better investment. As can be seen, large cities do employ more workers per capita, pay them more, and spend more overall. A densely populated city may be able to provide public services at a lower cost, for example, requiring fewer fire stations per square mile, than a sparsely populated city. Again, the table indicates that dense cities do employ more public workers. They also pay them more and spend more overall. The third population variable is the growth rate over the previous decade. This variable is included to capture lags in the provision of public services. A rapidly developing city may be slow to increase provision of public services in response to a larger population. The estimates weakly support this interpretation, showing that growing cities employ fewer public workers, pay them less, and spend less overall, but the coefficients are not generally distinguishable from zero. Two income-

related control variables are included to proxy for demand for public services. Income per capita, as expected, is positively related to employment, wages, and expenditure, consistent with the idea that demand for public services increases with income. The poverty rate is positively associated with employment and expenditure, and negatively associated with wages. Since income per capita is also included in the regression, the poverty rate may be capturing information about the dispersion of income, or poor populations may bring specific problems, such as crime, that increase the demand for public services. Finally, two regional dummies are included, one for cities in southern states and the other for cities in western states. These variables are included to capture omitted variables that are correlated with region, such as political culture and supply factors. They are also needed to separate initiative from regional effects since, as shown in Table 1, availability of the initiative is quite different across regions. Consistent with most cross-sectional spending regressions, cities in southern states appear to be more fiscally conservative, spending less and hiring fewer workers. Cities in western states, in contrast, hire fewer workers but pay them higher wages on average. Overall, the employment and spending regressions explain about one-quarter of the variation and the wage equation explains almost two-thirds of the variation. Evidently, much remains to be explained, but these numbers are not bad for cross-sectional regressions of this nature, and suggest that some important sources of variation are being captured.

Because of the interactive specification, the differences between initiative and noninitiative cities, what I call the “initiative effects” for short, are conditional and in some cases given by combinations of coefficients, making them difficult to read directly from Table 3. Table 4 reports the effects in a more transparent way, using the estimated models in Table 3. The first row shows the difference in employment policies of cities where collective bargaining is and is not available (given by coefficient β_2). This corresponds to the difference between P_{CB} and P_0 in the model, and gives an estimate of the effect of collective bargaining in the absence of initiatives. Collective bargaining is associated with 18.4 percent higher average wages, and 67.98 fewer workers per 10,000 residents. Both effects are statistically different from zero at better than the 1 percent level. Consistent with the model, collective bargaining drives up wages, and results in

lower employment. The net effect is that the city payroll is \$10.59 per capita lower, significant at about the 6 percent level.

The novel predictions of the theory concern the effect of the initiative process. The main implication is that the initiative has a different effect depending on whether or not collective bargaining takes place. When collective bargaining does not take place, the initiative cuts employment and expenditure, but has an ambiguous effect on wages. The initiative primarily counteracts the politician's tendency to increase patronage. When collective bargaining does take place, the initiative cuts wages, and also may cut employment. In this case, the initiative counteracts the higher wages that emerge from collective bargaining.

Consider first cities without collective bargaining. The second row reports the initiative effects (coefficient β_1). Consistent with the theory, employment is lower by 39.78 workers per 10,000 residents in cities with the initiative than cities without the initiative, and the difference is significant at the 1 percent level. The difference is sizeable compared to the mean employment of 136.58 workers per 10,000 residents. In contrast, average wages are within 1 percent of each other in initiative and noninitiative cities, a difference that cannot be distinguished from zero at conventional levels of significance. Payroll expenditure, shown in the last column, is also lower in initiative than noninitiative cities, but just shy of the 10 percent level of statistical significance. In short, the evidence generally supports the theoretical implication that the initiative will mainly cut employment when collective bargaining does not take place.

The third row reports the initiative effects for cities that do have collective bargaining (given by $\beta_1 + \beta_3$), corresponding to the difference between P_I and P_{CB} in the model. The main finding is in the second column, which shows that wages are 5.1 percent lower in cities with the initiative than cities without the initiative. This effect is different from zero at better than the 1 percent level of significance. Since the collective bargaining wage premium is 18.4 percent when the initiative is not available, the evidence suggests that the initiative rolls back about one-third of the wage premium associated with collective bargaining. The first column shows that the initiative is associated with a modest decline in employment of 3.34 workers per 10,000 residents, a difference that is not statistically different from zero. This finding is consistent with the

idea that collective bargaining “cures” some of the problems of patronage so voters are less focused on further cuts in employment. The effect on total payroll is negative, consistent with the theory, but is not estimated precisely enough to distinguish from noise. In short, when collective bargaining is available, the main effect of the initiative is to cut wages. According to the theory, the effect on employment is modest because collective bargaining already reduces employment, counteracting the tendency of the politician to pad the payroll with patronage workers.

The last row of Table 4 compares the effects of the initiative when collective bargaining is and is not available. In support of the theme of the analysis, the initiative has a different effect on employment and wages when collective bargaining is and is not available, and the differences are statistically different from zero at the 6 percent level of significance or better. The payroll effects are not significantly different from each other.

In order to assess the robustness of these findings, I estimated the regressions with a variety of alternative specifications and subsamples. In particular, I considered additional control variables (median age, urbanization, race of population, crime rate, among others), included financial variables (income, wages) in levels rather than as logarithms, and estimated the models after deleting all Western states. To make sure outliers were not driving the results, I also estimated the regressions after winsorizing the dependent variables at the first and ninety-ninth percentiles. None of these changes resulted in a materially different set of conclusions, suggesting the results are fairly robust.

B. Individual Functions

It is also interesting to examine the connection between the initiative and personnel policies for individual government functions. Citizens and politicians are likely to make different tradeoffs between employment and wages across functions, leading to different initiative and collective bargaining effects. For example, citizens appear to view firefighters with some sympathy, while administrators are sometimes seen as wasteful bureaucrats. Certain job functions may be more appealing for patronage purposes than others. For example, streets and highways jobs might be easier to fill with patronage employees who drop by the office once a day to punch the clock, and administrative and

“other” jobs might be easier to fill with persons who can provide political services to the incumbents such as organizing campaign events, compared to, say, police and fire fighting jobs, which might be scrutinized more closely by the public.

In order to study the impact of the initiative on employment policy for individual functions, I estimated employment, wage, and payroll regressions analogous to those in Table 3 for each of five main functions (administration, firefighters, police, streets and highways, and “all other”). I then used the regression estimates to calculate the marginal effects of collective bargaining and the initiative, as in Table 4. Table 5 contains the results. Each panel reports employment, wages, and payroll expenditure results for a single function. The top row in each panel reports the difference between collective bargaining and no collective bargaining when the initiative is unavailable. As can be seen, collective bargaining is associated with a wage premium in all functions, ranging from 16.7 percent to 23.2 percent, and all effects are different from zero at better than the 1 percent level. Collective bargaining is also associated with lower employment for all functions except administration, and the differences are statistically different from zero at conventional levels of significance except for police.

The second and third rows in each panel show the difference between outcomes in cities with and without the initiative (again called the “initiative effect” for short), conditional on whether or not collective bargaining is available. The last row in each table reports the difference in the initiative effect for cities with and without collective bargaining.

Three functions – firefighters, streets and highways, and “all other” – fit the general pattern that appeared in Tables 3 and 4. These functions together comprise 70 to 75 percent of employment and payroll. For each of them, we see that the initiative cuts employment more in cities without collective bargaining than cities with collective bargaining, and the difference is statistically significant at the 5 percent level or better for streets and all other jobs. The coefficient of -1.55 for streets and spending is large compared to the mean employment level, and suggests that jobs in this function may be particularly attractive to politicians for patronage purposes, leading voters to make big cuts when they have control. The wage evidence is also generally consistent with the theory. The initiative is associated with wage cuts between 2.7 percent and 3.1 percent

when collective bargaining occurs, and for streets and all other jobs, wage cutting is more severe with rather than without collective bargaining, although only the streets difference is statistically significant. As for payroll expenditure, the effects are generally insignificant in statistical terms, except for all other jobs, where the initiative is associated with lower spending in the absence of collective bargaining.

The evidence for police and administration is incongruent with the other findings, suggesting that the political economy of these two functions may be different in important ways. For police, unlike any of the other categories, collective bargaining does not have a statistically significant negative effect on employment, although it is associated with 16.0 percent higher wages and an additional 1.44 spending per capita. Such a pattern would be consistent with the theory if politician utility functions displayed a strong unwillingness to trade off employment for wages or if police unions were willing to accept lower wages in exchange for greater employment. The estimates indicate that the initiative cuts employment by a statistically significant 2.17 workers per 10,000 residents when collective bargaining is present but does not cut wages. This suggests that voters may use the initiative to reduce employment in the face of the higher wage, essentially making the job cuts that the politicians are unwilling to make.

The most anomalous case is administration. Like all of the other spending functions, collective bargaining is associated with higher wages, in this case 18.1 percent higher. Unlike any of the other functions, however, collective bargaining is associated with an increase in employment, and the effect is statistically different from zero at the 5 percent level. Such an outcome could be consistent with the theory if public employees in this function had preferences for both higher employment and higher wages. Administrative workers might prefer having more co-workers because it could reduce their work load and make their jobs less demanding (but that would seem true for other functions as well). The problem with this interpretation is that we would expect the initiative to trigger cuts in wages and spending. However, the initiative appears to be associated with higher employment and wages, although the effects cannot be statistically distinguished from zero. Why voters would not use initiatives to undo part of the collective bargaining consequences is not clear. Equally puzzling is the finding that the initiative is associated with higher wages when collective bargaining is available. It is

hard to explain this pattern in terms of the theory developed above, or any of the more natural extensions of the theory. Perhaps the explanation is that administrative employees are particularly skilled at controlling the initiative agenda, and are able to put proposals that advance their interests on the ballot, apparently to the detriment of the citizens at large. Such an interpretation would seem to square with evidence suggesting that administrative positions are the last to be cut in response to a tax and expenditure limit (Figlio, 1998; Figlio and O'Sullivan, 2001).

5. Discussion

The main message of this paper is that municipal employment policies are different when voters can override elected officials via initiatives, and the differences are consistent with a theory in which initiatives counteract political economy problems stemming from patronage and interest groups. When collective bargaining is unavailable, the initiative mainly cuts employment, consistent with a model in which elected officials tend to pad the public payroll with patronage workers. When collective bargaining is available, the initiative mainly cuts wages, consistent with a model in which voters use the initiative to undo supra-market wages that emerge from collective bargaining. The initiative is also associated with smaller employment cuts when collective bargaining is available than when it is unavailable. This pattern is consistent with the model because higher union wages cause elected officials to cut public sector employment on their own, reducing the need for initiatives to roll back patronage jobs.

In the model that motivates the empirical work, the changes brought about by the initiative are beneficial to citizens. This happens by assumption in the model because only helpful initiatives will be approved by voters, and the politician and public employees understand this when they negotiate. The implication that initiatives can never make the voters worse off is a fairly consistent finding of complete information models (Matusaka and McCarty, 2001). To the extent that the empirical evidence is consistent with the predictions of the model, it lends support to the idea that initiatives lead to policies that make the voter better off. However, this conclusion should be viewed with caution because there are more complicated models with asymmetric information in which citizens are made worse off by having initiatives available. Matusaka and

McCarty (2001) show that when the politician is uncertain about citizen preferences, the politician may adjust policy in a way that favors the interest group in order to deter an initiative campaign by an extreme group. Similarly, Gerber and Lupia (1995) show that the politician may distort policy in a way harmful to the citizens if the voters are uncertain about which policy is closest to their ideal point. It is not immediately clear how the empirical results could be rationalized by either of these models, but absent a careful investigation, we cannot reject the possibility.

The evidence also provides a perspective on tax and expenditure limitations. Beginning in the mid-1970s, voters approved a wave of ballot propositions that limited state and local taxes and expenditures, the most famous of which was California's property tax cutting Proposition 13. Since then a large literature has assessed the impact of tax and expenditure limits (TEL) on government behavior. Many studies find that TELs limit taxes and spending as they were intended to do, but a surprising large number of studies fail to find clear effects (Abrams and Dougan, 1986; Dye and McGuire, 1997; Gerber, Lupia, McCubbins, and Kiewiet, 2001). Poterba and Rueben (1995), a study closely related to this one, finds slower wage growth but only weak evidence for slower employment growth after adoption of a TEL. As discussed above, a tax and expenditure limit is one way the employment and wage reductions associated with the initiative can be brought about. However, it is not the only way. Cuts can also be brought about preemptively by elected officials without the need for an initiative. One possible explanation for the mixed results in the literature is that when voters are interested in implementing employment and spending cuts, they resort to TELs in some cases, but in other cities, astute politicians make the cuts themselves in order to prevent a TEL from coming to the ballot (as happens in the model developed above). If this is the case, there may not be observable differences between states and cities with and without TELs. The main difference would be between states and cities where TELs are possible, namely those states and cities where initiatives are possible. Put differently, the important institutional feature behind tax and spending cuts may be the initiative, not the TEL. The finding in this paper and others of local and state expenditure cuts associated with the initiative lends support for this idea.

Finally, opinion polls concerning TELs often find that voters believe spending can be cut without reducing services, what Sears and Citrin (1985) called the “something for nothing syndrome.” While such beliefs are sometimes viewed with skepticism by journalists and scholars, in the context of the model above, it is entirely possible for a TEL to cut taxes without reducing government services. When collective bargaining drives wages above competitive levels, a TEL that leads to wage cuts can reduce expenditure without reducing the services supplied by government (as measured by employment). When a TEL triggers an employment reduction, public services would presumably decline, but reducing these “patronage” jobs saves voters more in taxes than it costs them in lost services.

Appendix 1. Proofs

Proof A. Policy choice with collective bargaining but no initiative

The union's problem is

$$(A1) \quad \max_{l,w} w \quad \text{subject to } V(l, wl) \geq V_0,$$

with the solution denoted (l_{CB}, w_{CB}) . The constraint must bind, so $w_{CB} \geq w_0$. The union's optimization condition with respect to l is

$$(A2) \quad V_1 + wV_2 = 0.$$

For a given wage, the politician's utility is maximized with respect to l . Equation (A2) defines a relation between l_{CB} and w_{CB} at the optimum, and straightforward differentiation yields a term that implies $dl_{CB} / dw_{CB} < 0$. The term is signed by the assumption that the demand for labor is decreasing in w . Thus, $l_{CB} \leq l_0$.

Proof B. Policy choice with collective bargaining and initiative

The union's problem is (A1) with the additional constraint $U(l, wl) \geq U_C$. Denote the solution (l_I, w_I) and assume that it is possible to satisfy both constraints and both constraints bind. Suppose there is an optimum $P' = (l', w')$ such that $l' > l_{CB}$. The politician's utility function is an inverted U-shape, so there must be another policy $P'' = (l'', w')$ with the same wage but $l'' \leq l_{CB}$ such that $V(l', w'l') = V(l'', w'l'')$. Because the principal is indifferent between P' and P'' , $U(l', w'l') - U(l'', w'l'') = \alpha(l'' - l') < 0$. This implies that the citizen's constraint is not binding at P' , which is to say that P' is not be an optimum. Therefore, the optimum must satisfy $l_I \leq l_{CB}$. To guarantee the politician's reservation utility, it must be the case that $w_I \leq w_{CB}$. The two inequalities together imply $l_I w_I \leq l_{CB} w_{CB}$.

Appendix 2. Summary Statistics for Control Variables

Variable	Mean	S.D.	Minimum	Maximum
Dummy = 1 if initiative allowed	0.77	0.42	0	1
Dummy = 1 if collective bargaining allowed, overall	0.75	0.44	0	1
Dummy = 1 for collective bargaining, police	0.73	0.44	0	1
Dummy = 1 for collective bargaining, fire	0.76	0.42	0	1
Dummy = 1 for collective bargaining, other	0.63	0.48	0	1
Population	11.42	0.83	9.94	15.90
Population density	3.62	2.99	0.01	26.40
Population growth, 1990-2000	12.48	18.94	-21.40	169.40
Income per capita	21.13	5.86	9.76	46.16
Poverty, percent	8.26	3.46	1.40	17.63
Dummy = 1 if Southern state	0.30	0.46	0	1
Dummy = 1 if Western state	0.29	0.45	0	1

Note. The unit of observation is a city. Statistics are calculated using 565 observations, except for income and poverty, which are calculated using 564 observations. Employment, wage, demographic, and economic information is for 2000. Population is expressed as a natural logarithm, density is thousands of people per square mile, population growth is a percent, income per capita is in thousands of dollars, poverty is the percent of the population with an income less than 150 percent of the poverty rate, and Southern and Western states follow Census divisions.

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Figure 1. Viable Initiatives with No Collective Bargaining

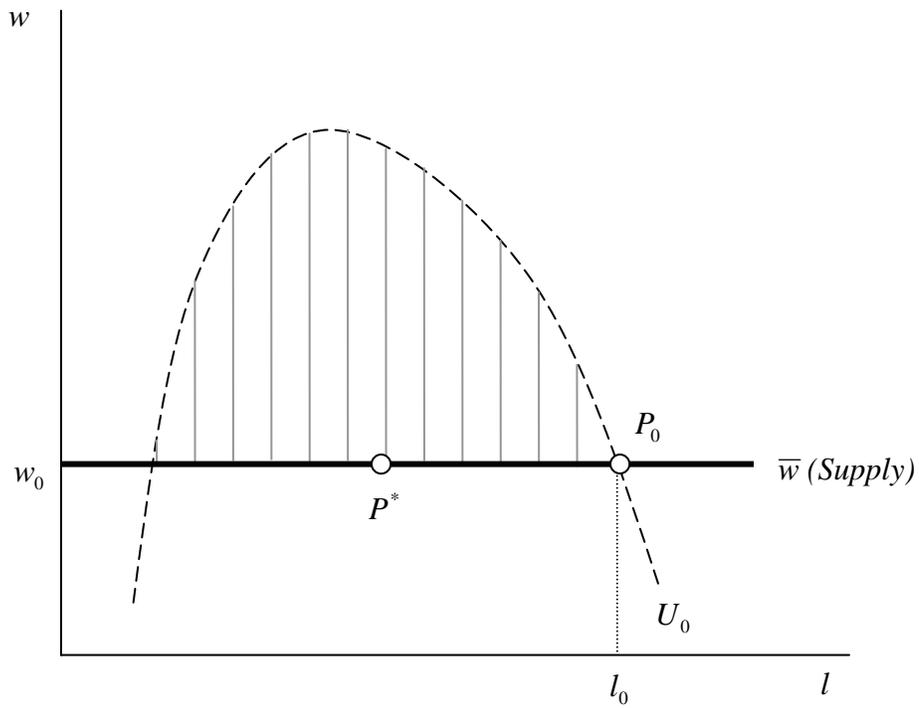


Figure 2. Initiative Effects with Collective Bargaining

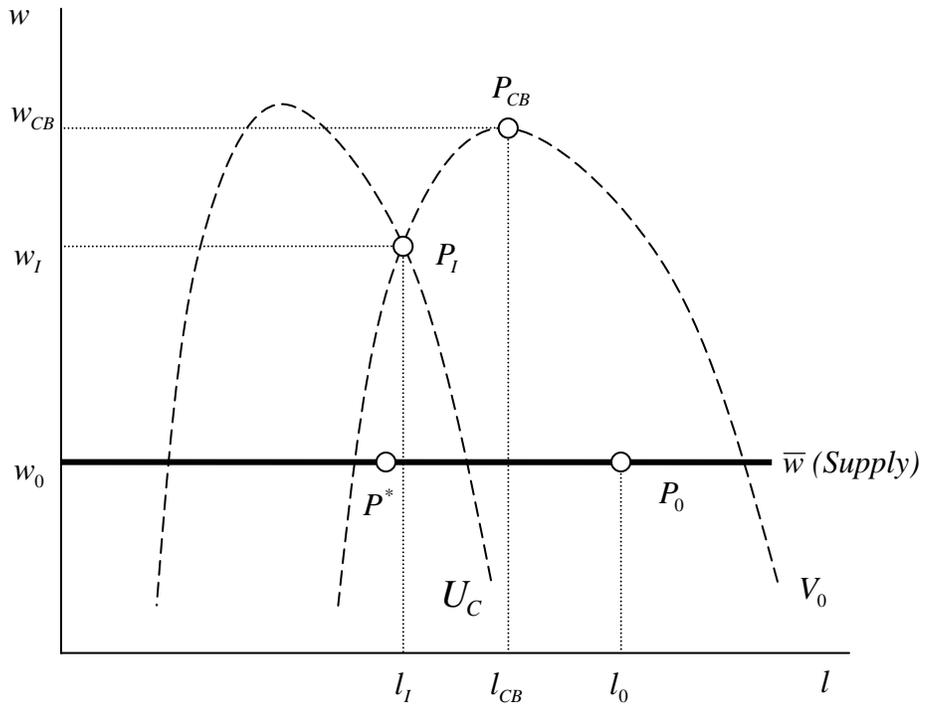


Figure 3. Payroll (dollars per capita) for Local Governments, 2000

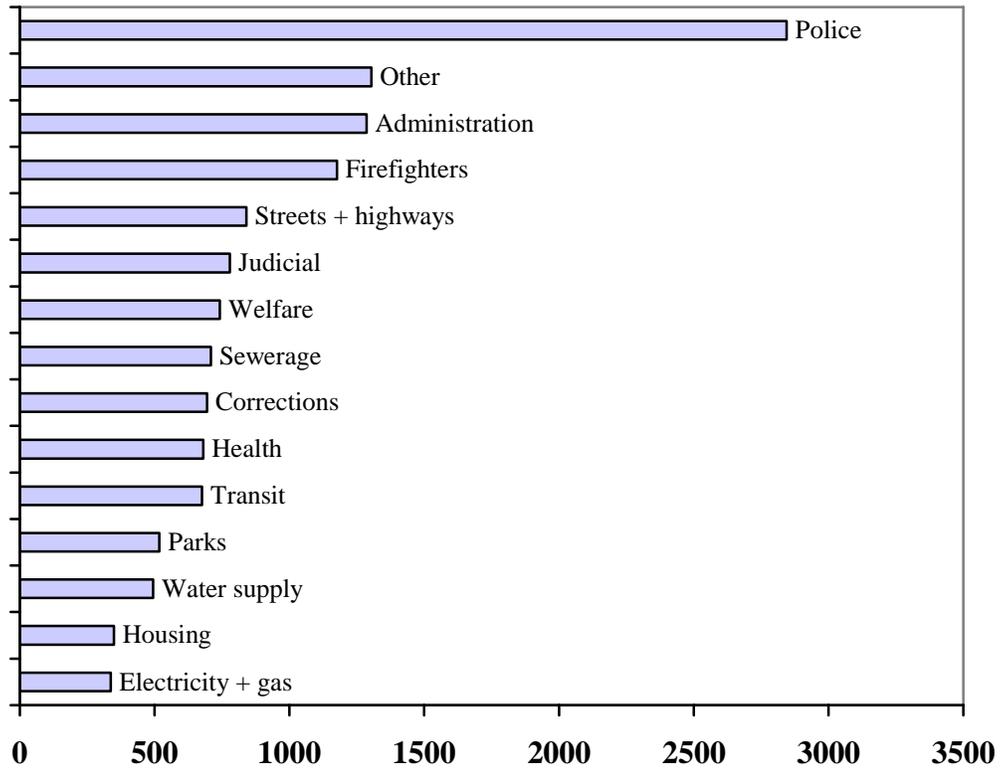


Table 1. Percentage of Cities with Initiative

	Percent of cities with initiative	N
All cities in sample	83.3	1,311
West	95.8	641
South	80.1	301
Midwest	63.6	261
Northeast	65.4	107
Initiative states	94.5	895
Noninitiative states	59.1	416
Population < 25,000	88.6	360
Population 25,000 to 50,000	78.6	365
Population 50,000 to 100,000	81.6	353
Population 100,000 to 250,000	84.0	169
Population > 250,000	87.5	64

Note. The sample includes (with a few exceptions) the largest 1,000 cities in the United States and the 10 largest cities in each state, as of 2005. Regions follow Census definitions: West includes AK, AZ, CA, CO, HI, ID, MT, NM, NV, OR, UT, WA, WY; South includes AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV; Midwest includes IA, IL, IN, KS, MI, MN, MO, NE, ND, OH, SD, WI. Population numbers are Census estimates for July 1, 2005. Cities are classified as having the initiative if they allow citizens to propose charter amendments or ordinances by petition, and the proposals are put to a vote of the citizens at large. Initiative and population data are from the *Legal Landscape Database*.

Table 2. Summary of Employment Policy

Variable	Mean	S.D.	Minimum	Maximum	N
<u>A. Employment^a</u>					
Police	25.97	9.66	0.77	77.93	558
Fire	16.59	7.50	0.05	71.00	527
Streets	7.99	4.39	0.22	29.99	563
Administration	10.17	5.38	1.93	37.05	565
All other	77.32	89.79	1.22	723.24	565
TOTAL	136.58	101.58	12.78	786.03	565
<u>B. Wages^b</u>					
Police	45,945	11,130	17,449	80,798	558
Fire	49,141	13,289	14,238	96,794	527
Streets	38,547	10,295	7,345	82,682	563
Administration	41,422	8,541	18,933	73,953	565
All other	35,609	7,419	16,416	64,296	565
TOTAL	40,866	8,431	23,320	70,115	565
<u>C. Payroll^c</u>					
Police	9.83	4.11	0.13	35.05	558
Fire	6.58	3.11	0.01	35.80	527
Streets	2.48	1.38	0.07	10.61	563
Administration	3.49	1.93	0.31	13.05	565
All other	23.18	28.62	0.48	233.34	565
TOTAL	44.98	33.15	5.59	259.51	565

Note. The unit of observation is a city. Panel A reports summary statistics for city employment. Panel B reports summary statistics for the average wage. Panel C reports summary statistics for total payroll expenditure per resident.

^a FTE employment per 10,000 residents.

^b Average FTE wage.

^c FTE employment per resident times average wage.

Table 3. Regressions of Employment, Wages, Expenditure for All Functions

	Dependent Variable		
	Employment ^a (1)	Wages ^b (2)	Payroll ^c (3)
Dummy = 1 if initiative available	-39.78 ^{**} (16.19)	.009 (.23)	-8.53 (5.28)
Dummy = 1 if collective bargaining	-67.98 ^{***} (17.52)	.184 ^{***} (.025)	-10.59 [*] (5.71)
Dummy = 1 if initiative and collective bargaining	36.44 [*] (19.22)	-.060 ^{**} (.027)	5.13 (6.27)
Population	16.78 ^{***} (4.83)	.047 ^{***} (.007)	7.82 ^{***} (1.57)
Population density	3.57 ^{**} (1.41)	.012 ^{***} (.002)	1.89 ^{***} (0.46)
Population growth, 1990-2000	-0.45 [*] (0.24)	-.0002 (.0003)	-0.11 (0.08)
Income per capita	3.15 ^{***} (0.84)	.008 ^{***} (.001)	1.55 ^{***} (0.27)
Poverty, percent	8.92 ^{***} (1.66)	-.008 ^{***} (.002)	3.09 ^{***} (0.54)
Dummy = 1 if Southern state	-2.93 (10.66)	-.082 ^{***} (.015)	-6.71 [*] (3.48)
Dummy = 1 if Western state	-51.69 ^{***} (11.06)	.143 ^{***} (.016)	-13.58 ^{***} (3.61)
Intercept	-127.81 ^{**} (59.64)	9.803 ^{***} (.085)	-90.66 ^{***} (19.45)
R^2	.253	.652	.238

Note. Each column reports estimates from a regression, the dependent variable of which is indicated at the top of the column. The unit of observation is a city and the sample includes 564 observations. Standard errors are in parentheses beneath coefficient estimates. Employment, wage, demographic, and economic information is for 2000. Population is expressed as a natural logarithm, density is thousands of people per square mile, population growth is a percent, income per capita is in thousands of dollars, poverty is the percent of the population with an income less than 150 percent of the poverty rate, and Southern and Western states follow Census divisions. Significance levels on coefficients are indicated as: * = 10%, ** = 5%, *** = 1%.

^a FTE employment per 10,000 residents.

^b Natural logarithm of the average FTE wage.

^c FTE employment per resident times average wage.

Table 4. Conditional Effects of Initiative and Collective Bargaining (CB)

	<u>Employment</u> ^a		<u>Wages</u> ^b		<u>Payroll</u> ^c	
	Effect	<i>p</i>	Effect	<i>p</i>	Effect	<i>p</i>
CB effect no initiative	-67.98***	<.001	.184***	<.001	-10.59*	.064
Initiative effect no CB	-39.78**	.014	.009	.693	-8.53	.107
Initiative effect CB	-3.34	.781	-.051***	.003	-3.40	.384
Initiative effect, difference between CB and no CB	36.44*	.058	-.060**	.028	5.13	.413

Note. The numbers are based on the coefficients in Table 3. The dependent variable is at the top of each column. The “CB effect | no initiative” is the difference in employment (or wages or payroll) between cities that do and do not allow collective bargaining, conditional on the initiative not being available. The “initiative effect” is the difference in employment (or wages or payroll) between cities that do and do not allow the initiative, condition on collective bargaining being permitted (CB) or not permitted (no CB). The *p*-values are for the hypothesis that the difference (“effect”) is zero. For convenience, significance levels are indicated as: * = 10%, ** = 5%, *** = 1%.

^a FTE employment per 10,000 residents.

^b Natural logarithm of the average FTE wage.

^c FTE employment per resident times average wage.

Table 5. Effects of Initiative and Collective Bargaining (CB) by Function

	<u>Employment</u> ^a		<u>Wages</u> ^b		<u>Payroll</u> ^c	
	Effect	<i>p</i>	Effect	<i>p</i>	Effect	<i>p</i>
Panel A. Administration						
CB effect no initiative	1.91*	.061	.181***	.000	1.02***	.004
Initiative effect no CB	0.47	.584	.068***	.009	0.28	.344
Initiative effect CB	0.33	.630	.012	.549	0.16	.488
Initiative effect, CB vs. no CB	-0.14	.896	-.056*	.080	-0.11	.751
Panel B. Firefighters						
CB effect no initiative	-2.85**	.020	.172***	.000	0.24	.651
Initiative effect no CB	-1.32	.260	-.050	.162	-0.74	.151
Initiative effect CB	-0.39	.638	-.031	.217	-0.47	.189
Initiative effect, CB vs. no CB	0.94	.494	.019	.645	0.27	.656
Panel C. Police						
CB effect no initiative	-0.83	.552	.160***	.000	1.44**	.019
Initiative effect no CB	0.92	.469	-.010	.765	0.15	.781
Initiative effect CB	-2.17**	.023	-.014	.574	-1.11***	.008
Initiative effect, CB vs. CB	-3.08**	.043	-.004	.918	-1.26*	.059
Panel C. Streets and Highways						
CB effect no initiative	-1.83**	.028	.232***	.000	0.23	.397
Initiative effect no CB	-1.55**	.026	.045	.152	-0.08	.717
Initiative effect CB	0.44	.433	-.027	.278	0.04	.842
Initiative effect, CB vs. no CB	1.99**	.020	-.072*	.061	0.12	.670
Panel E. All other						
CB effect no initiative	-53.70***	.002	.167***	.000	-10.38*	.053
Initiative effect no CB	-56.65***	.000	.012	.638	-13.32***	.003
Initiative effect CB	-1.23	.913	-.029	.171	1.65	.646
Initiative effect, CB vs. no CB	55.42***	.002	-.041	.201	11.67**	.034

Note. The numbers in each panel are based on three regressions, one for each of the dependent variables indicated at the top of each column. The specification of the model and the explanatory variables are the same as in Table 3, except that the collective bargaining variables in this table pertain specifically to firefighters, police, and “other” jobs. Administration, streets and highways, and all other jobs use the “other” collective bargaining classification. The regressions include 564 observations for administration and all other, 526 observations for firefighters, 557 observations for police, and 562 observations for streets and highways. Significance levels for the null hypothesis that the effect is zero are indicated as: * = 10%, ** = 5%, *** = 1%. The *p*-values are also reported.

^a FTE employment per 10,000 residents.

^b Natural logarithm of the average FTE wage.

^c FTE employment per resident times average wage.