

Direct Democracy and Public Employees

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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 has 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 (Timothy Besley and Anne 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, roughly 80 percent of cities allow initiatives, including most of the largest cities. Ballot propositions are also increasingly used to make local policy decisions in Europe, Taiwan, and Japan.

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 (Elisabeth R. Gerber 1996; Matsusaka and Nolan M. 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 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., David S. 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

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¹ Arthur F. Bentley (1908/1995) is an early study of interest group competition. More recent work by George S. Stigler (1971), Sam Peltzman (1976), Gary S. Becker (1983), and Gene M. Grossman and Elhanan Helpman (2002) has spawned a vast literature.

² Matsusaka (2007) 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 Arthur Lupia and Matsusaka (2004) for a survey.

³ The view that voter incompetence allows interest groups to subvert the majority remains one of the central criticisms of direct democracy (Shaun Bowler and Todd 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 attain nonmajoritarian policies because of organizational advantages has been modeled—Peltzman (1976) is an influential example—but it has not been shown that the interest group’s advantage increases when direct democracy is available.)

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 2005, 21.7 million people worked in the public sector, 15 percent of the labor force, and over 64 percent (13.9 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 \$480 billion in 2004, and comprising 38 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 be 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 40 percent of local government workers represented by unions in 2006, 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 theory suggests 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 these interest group problems are real and that the initiative does help to control them. 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. In a sample containing 650+ 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 statistically significant wage cuts on the order of 4 percent. Since collective bargaining is estimated to increase wages by 18 percent, the initiative appears to undo about one-quarter 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 use 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.

Endogeneity of institutions (and the more general issue of omitted variables correlated with initiative availability) is a standard concern in the literature on political institutions. A priori, institutional endogeneity may be less of a concern here than in other contexts because most cities adopted the initiative a century ago and regional ideologies show almost no correlation over this long stretch of time (Robert S. Erikson, Gerald C. Wright, and John P. McIver 1993). But to address the issue more directly, I also provide instrumental variable estimates that exploit the fact that state constitutions and statutes differ in the extent to which they require cities to provide the initiative process. Using an index of state requirements as an instrument for initiative availability in a city, the main results continue to appear, although the wage-cutting effects of the initiative lose statistical significance.⁵

⁴ Numbers in this paragraph are adapted from *Statistical Abstract of the United States: 2008 Edition*.

⁵ I also conduct robustness tests using state fixed effects and an alternative proxy for collective bargaining. The employment-cutting effects are robust, but a somewhat anomalous pattern appears for wages in one specification.

The paper is part of a growing literature on the policy effects of direct democracy. A substantial literature has shown that initiatives tended to cut spending and taxes in American states over the last few decades, and similar patterns have been found for Swiss cantons and communes.⁶ There is little evidence on how the spending cuts are 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., James M. Poterba 1994, 1995; David M. Primo 2007). 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, rather than general dissatisfaction with the size of government.

The paper is arranged as follows. Section I develops a model to frame the empirical analysis. Section II describes and summarizes the data. Section III reports the main results, and Section IV discusses implications.

I. Theory

To motivate the empirical analysis, this section develops a model in which elected officials derive patronage benefits from public workers, collective bargaining allows public employees to drive up wages, and the initiative matters because of its influence on agenda control. The basic setup follows Gerber (1996) and Matsusaka and McCarty (2001) (and thus Thomas Romer and Howard Rosenthal 1979), and draws on the model of public sector wage determination in Linda C. Babcock, John Engberg, and Amihai Glazer (1997), which itself is based on a long tradition in labor economics (Henry S. Farber 1986). While many of the pieces of the model are familiar, the combination of elements from the political economy and union bargaining literature is new, and the interaction of three actors—interest groups, politicians, and unions—introduces some subtle strategic considerations.

A. *Effect of the Initiative when Collective Bargaining Is Not Allowed*

This section considers the case where collective bargaining is not allowed; the next section considers collective bargaining. Both situations are empirically relevant, and an important part of the empirical analysis is to investigate whether the differences predicted by theory are present in the data.

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} .⁷ 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 a numeraire good, x , and utility is increasing and concave in both arguments. Taxes are

⁶ 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), and Feld and Matsusaka (2003). There is a small literature on initiatives and spending in American cities that suggests higher spending in initiative cities, but not consistently (Jeffrey S. Zax 1989; Paul G. Farnham 1990; Matsusaka 2004, ch. 3).

⁷ 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 movements to those arising from political considerations.

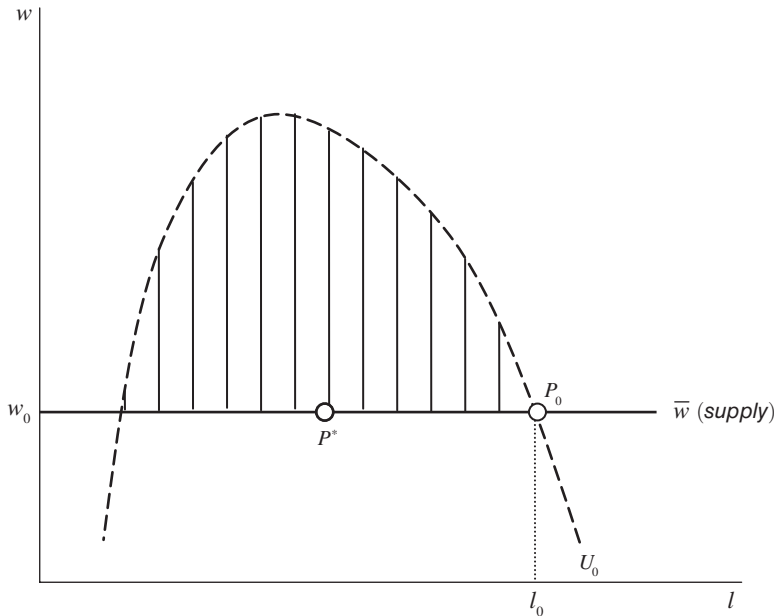


FIGURE 1. VIABLE INITIATIVES WITH NO COLLECTIVE BARGAINING

distributed equally, implying a tax burden of wl for each person. Given income y , denoted in units of the consumption good, 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, the utility function is assumed to be 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 those jobs give them control over patronage, they enjoy running a larger organization, or government workers can help them in elections (James Q. Wilson 1961). Elected officials are 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.⁸ 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 would hire,

⁸ In order to focus on the strategic interaction between groups, I follow a tradition in political economy research by working with a reduced form of the politician's preferences that embeds a direct utility from public employees (e.g., Andrei Shleifer and Robert W. Vishny 1994). This utility function assumes that competition does not result in a fully efficient level of public employment. For microfoundations of this assumption in the patronage context, see James A. Robinson and Thierry Verdier (2006) and Ruben Enikolopov (2007).

$l_0 > l^*$, 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 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 succeed only if they reduce public sector employment. Second, an initiative that proposes a wage increase could be approved—voters 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 of 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 (Tracy M. Gordon 2004). For a statewide measure in California, normally it costs \$1 million or more to put a measure on the ballot. Due to 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 conclude only 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.⁹*

B. Effect of the Initiative when Collective Bargaining Is Allowed

The effect of initiatives is 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 that 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 $P_{CB} = (l_{CB}, w_{CB})$.¹⁰

⁹ If we assumed that all initiatives come from citizens and the politician chooses policy to preempt the initiative, as in the next section, the initiative would cut employment and have no effect on wages.

¹⁰ An alternative assumption is that the union seeks to maximize 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 were increasing in both wages and employment, the collective bargaining outcome would

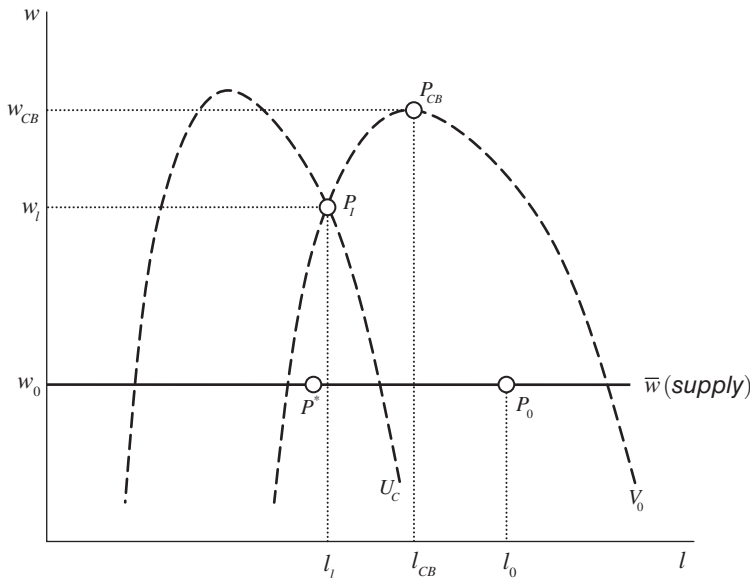


FIGURE 2. INITIATIVE EFFECTS WITH COLLECTIVE BARGAINING

Collective bargaining increases wages above what the politician or citizen would prefer, $w_{CB} > w_0$, but employment is lower with collective bargaining, $l_{CB} < l_0$ (see Proof A in the Appendix). Faced with a higher price for employees and thus a higher price for patronage, the politician cuts back on the quantity of labor. Although the diagram suggests that employment under collective bargaining is higher than 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. At first glance, P_{CB} might seem like a candidate for the status quo, but availability of the initiative changes the bargaining game between the politician and public employees because both groups are aware that a future initiative may undo the policy they negotiate.

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^* . If the cost of putting an initiative before the voters is C , measured in units of utility, a citizen will propose 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, public employees maximize the wage subject to delivering the politician a minimum

be where the union indifference curve is tangent to V_0 . In this case, collective bargaining increases wages but may not result in lower employment.

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.¹¹ Introduction of the initiative reduces the wage ($w_I < w_{CB}$), employment ($l_I < l_{CB}$), and total expenditure (see Proof B in the Appendix).¹²

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

One 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.

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, 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 gives the citizens 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 rare. A spending limit of $wl < k$ can be represented as a hyperbola in Figures 1 and 2, where any policy above or to the right of the 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.

II. Data Sources

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 because there

¹¹ 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 more accommodating to business interests.

¹² 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. More generally, as the number of competing groups increases, we expect a winning initiative closer to the citizen’s ideal point (proposals farther from the citizen’s ideal point do not matter because they cannot win), strengthening the effects identified in Implication 2.

is no central clearinghouse for such information, and the data source used in most previous studies—the ICMA Form of Government Survey—is unreliable.¹³ I utilize a new data source, the *Legal Landscape Database*, which was assembled by examining state constitutions, state statutes, municipal charters, and city codes for the 1,000 largest cities in the United States and 10 largest cities in each state.¹⁴ The 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.

Initiative data were matched to a variety of census data from 2000. Information on city wages and employment by function was taken from *Local Government Employment and Payroll, 2000*. Demographic and economic information was taken from American FactFinder. The employment and payroll data and the demographic and economic data are not comprehensive, resulting in the loss of about 800 observations from the *Legal Landscape Database* after merging.¹⁵ Because initiative information is for a single year, it was not possible to exploit time series variation in initiative status.

Information on collective bargaining laws for local government employees in each state was taken from the *NBER Public Sector Collective Bargaining Law Data Set*.¹⁶ This dataset 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 having 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 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.¹⁷ Finally, information on state-wide unionization rates in 2000 was collected from the Web site of the Bureau of Labor Statistics (BLS).

To give some perspective to the prevalence of direct democracy, Table 1 reports the percentage of cities in the sample that allow initiatives. Overall, 82.2 percent of sample cities allow the initiative, highlighting that the process is widespread.¹⁸ The process is most prevalent in the West

¹³ For example, the 1996 survey reports that only 198 of 311 California cities allow the initiative, even though state law grants initiative rights to citizens in every city in the state (California Constitution, Article 2, Section 11; California Statutes, 9200–9224). The city clerks (often minor functionaries who issue birth certificates and the like) who fill out the International City/County Management Association (ICMA) surveys may be unaware of state provisions, or may misunderstand the ambiguous survey questions. I estimated the main model using the ICMA classification and found qualitatively similar but weaker results.

¹⁴ 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.

¹⁵ For more information on the employment and payroll data, see *Government Employment*, US Census Bureau, March 2000. The source files are 00empst.dat and 00empid.dat. Demographic and economic data were extracted from Census 2000 Summary File 1 (SF1) 110-Percent Data and Census 2000 Summary File 3 (SF3)—Sample Data, at www.factfinder.census.gov. Only cities with 25,000 or more people survived the census match. Atlantic City was deleted because it is a significant outlier in terms of public employment.

¹⁶ Kim Rueben kindly provided her updated version of the dataset that runs through 1996. When data for 1996 were unavailable, I used information from 1991, the most recent year otherwise available.

¹⁷ Thus, “collective bargaining” is codes 4–6 in the dataset and “no collective bargaining” is codes 0–3. I experimented with other cutoff points and with a variable that took on values 1–6, with qualitatively similar results. I also explored right-to-strike and right-to-work laws using information in the dataset, but collective bargaining laws had 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.”

¹⁸ 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. Compare

TABLE 1—PERCENT OF CITIES WITH INITIATIVE

	Percent of cities with initiative	N
All cities in sample	82.2	1,088
West	96.7	399
South	82.2	315
Midwest	59.4	254
Northeast	81.5	119
Population 25,000 to 50,000	78.9	427
Population 50,000 to 100,000	82.4	410
Population 100,000 to 250,000	85.8	183
Population > 250,000	91.1	68

Notes: 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. Cities with population below 25,000 are excluded. 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. 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*.

(96.7 percent cities) and least available in the Midwest (59.4 percent). A similar pattern appears for state-level initiatives (available in 24 of 50 states), where most states west of the Mississippi allow them. Table 1 also shows that small cities are less likely than large cities to allow the initiative, with 78.9 percent availability in cities with fewer than 50,000 residents, and 91.1 percent availability in cities with more than 250,000 residents.

No systematic information is available on when cities adopted the initiative or how often initiative status changes. However, a variety of less systematic sources suggest that most cities with the initiative adopted it long before the sample period, typically in the midst of the Progressive movement (1900–1920) when most statewide initiatives were adopted. San Francisco and Vallejo in California were 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 (Ellis P. Oberholtzer 1911, ch. 17). A 1911 survey of states and cities (Ernest S. Bradford 1911, ch. 19) found that initiative charter amendments were allowed in 38 of 51 cities, and state law allowed municipal initiatives in 15 of 21 states.¹⁹ The apparent fact that most cities adopted the initiative process almost a century ago is important to allay some concerns about the endogeneity of the institution.

Table 2 reports summary statistics for employment, wages, and payroll by function for the cities in the final sample. The four most important functions in terms of payroll are police, administration, street and highways, and firefighters.²⁰ 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 (wages \times employment) on an annualized basis. One limitation of these data is that they do not include information on the benefits part of compensation. Benefits may be a significant part of total compensation,

Table 1 here with tables 4 and 5 in Matsusaka (2003). With almost 200 cities in the noninitiative category, we have a healthy number of both city types in the sample.

¹⁹ California, Idaho, Illinois, Iowa, Kansas, Louisiana, Minnesota, Montana, New Jersey, North Dakota, South Carolina, South Dakota, Washington, Wisconsin, and Wyoming.

²⁰ Education is an important local expenditure, but in most states takes place in school districts that are separate from cities. Hospital spending is also important, but typically a county function.

TABLE 2—SUMMARY OF EMPLOYMENT POLICY

Variable	Mean	SD	Minimum	Maximum	N
<i>Panel A. Employment</i> ^a					
Police	26.34	9.57	0.77	67.32	647
Fire	16.88	7.75	0.05	71.00	613
Streets	8.04	4.39	0.22	29.99	652
Administration	10.15	5.13	1.93	37.05	654
All other	76.81	85.42	1.22	723.24	654
Total	136.87	96.51	12.78	786.03	654
<i>Panel B. Wages</i> ^b					
Police	45,254	11,116	17,449	80,798	647
Fire	48,101	13,289	14,238	96,794	613
Streets	37,871	10,106	7,345	82,682	652
Administration	41,058	8,448	18,933	73,953	654
All other	35,146	7,411	16,415	64,296	654
Total	40,264	8,427	23,189	70,115	654
<i>Panel C. Payroll</i> ^c					
Police	117.99	49.73	1.61	399.89	647
Fire	78.54	37.96	0.16	429.57	613
Streets	29.35	16.35	0.81	127.27	652
Administration	41.30	21.90	3.77	156.59	654
All other	273.80	328.22	5.76	2,800.12	654
Total	534.71	379.54	67.12	3,114.17	654

Notes: The unit of observation is a city. Panel A reports summary statistics for city employment. Panel B reports summary statistics for the wages. Panel C reports summary statistics for total payroll expenditure per resident.

^aFTE employment per 10,000 residents.

^bAverage FTE wage.

^cFTE employment per resident times average wage.

but there is no a priori reason to expect their omission to bias the key coefficients one way or another.²¹ Table 2 shows that for the sample cities, police employment averages 26.34 per 10,000 residents, with an average annual salary of \$45,254, and payroll expenditure of \$117.99 per capita. Average wages are highest for fire fighters, followed by police, administration, streets, and “all other.”

III. Empirical Results

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

$$(1) \quad Y_i = \beta_0 + \beta_1 I_i + \beta_2 B_i + \beta_3 I_i B_i + \beta_4 \mathbf{X}_i + e_i,$$

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, \mathbf{X}_i is a vector of control

²¹ Another issue is that some cities report no expenditure for a given function, presumably because they have sub-contracted 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.

variables, e_i is an error term, and β_0, \dots, β_4 are coefficients to be estimated. As discussed above, whether a city has the initiative was determined in most cases up to a century before the policy choices examined here, so it is plausibly exogenous. Because a city's collective bargaining status is determined by state law, it is also plausibly exogenous. If the controls are adequate, then, policy differences between initiative and noninitiative cities can be attributed to the initiative.

Because the effect of the initiative is conditional on whether collective bargaining is available, the effect of the initiative is given by β_1 if collective bargaining is not available ($B_i = 0$) and 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 of interest, a few comments on the control variables are in order. 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 scale economies in the provision of services, leading to greater provision of the service. As can be seen, large cities do employ more workers per capita and spend more overall. A densely populated city may be able to provide public services at a lower average cost, for example, requiring fewer fire stations per square mile, than a sparsely populated city, again leading to greater provision. The table indicates that dense cities do employ more public workers and spend more on them overall. The third population variable is the growth rate over the previous decade. If it takes time to adjust to the equilibrium level of public services, a rapidly developing city may have fewer public workers than otherwise equivalent cities during the adjustment period. The estimates are consistent with this interpretation, showing that growing cities employ fewer public workers and spend less on them overall.

The population variables also indicate higher wages in large and dense cities, which could be a demand side or cost of living effect. Two income-related control variables are included to proxy for demand for public services. Income per capita 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. These dummies are also needed to separate initiative from regional effects since, as noted above, availability of the initiative varies 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.

TABLE 3—REGRESSIONS OF EMPLOYMENT, WAGES, AND PAYROLL FOR ALL FUNCTIONS

	Dependent variable		
	Employment ^a (1)	Wages ^b (2)	Payroll ^c (3)
Dummy = 1 if initiative available	-42.62*** (13.61)	0.017 (0.021)	-9.61** (4.50)
Dummy = 1 if collective bargaining	-75.71*** (15.14)	0.182*** (0.023)	-14.09*** (5.00)
Dummy = 1 if initiative and collective bargaining	50.70*** (16.63)	-0.057** (0.025)	10.72* (5.49)
Population, natural logarithm	18.21*** (4.13)	0.052*** (0.006)	8.46*** (1.37)
Population density	2.74*** (0.85)	0.010*** (0.001)	1.57*** (0.28)
Population growth, 1990–2000	-0.63*** (0.20)	-0.0002 (0.0003)	-0.19*** (0.07)
Income per capita	2.57*** (0.71)	0.007*** (0.001)	1.28*** (0.23)
Poverty, percent	7.51*** (1.41)	-0.009*** (0.002)	2.56*** (0.47)
Dummy = 1 if Southern state	-4.03 (9.01)	-0.096*** (0.014)	-7.45** (2.98)
Dummy = 1 if Western state	-49.31*** (9.33)	0.134*** (0.014)	-13.07*** (3.08)
Intercept	-118.53** (52.01)	9.763*** (0.079)	-86.67*** (17.18)
R^2	0.260	0.660	0.247

Notes: 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 652 observations. Standard errors are in parentheses beneath coefficient estimates. Employment, wage, demographic, and economic information is for the year 2000. Population density is thousands of persons 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.

^aFTE employment per 10,000 residents.

^bNatural logarithm of the average FTE wage.

^cFTE employment per resident times average wage.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

The coefficient on the collective bargaining dummy shows the difference in policy when collective bargaining is and is not available (corresponding to the difference between P_{CB} and P_0 in the model). Collective bargaining is associated with 18.2 percent higher wages, and 75.71 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 a \$14.09 per capita reduction in the city payroll, also significant at the 1 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 collective bargaining takes place. When collective bargaining does not take place, the initiative is predicted to cut employment and expenditure, but have an ambiguous effect on wages because the initiative primarily counteracts the politician's tendency to increase patronage. When collective bargaining

TABLE 4—EFFECTS OF INITIATIVE CONDITIONAL ON COLLECTIVE BARGAINING

	Employment ^a		Wages ^b		Payroll ^c	
	Effect	<i>p</i>	Effect	<i>p</i>	Effect	<i>p</i>
<i>Panel A. Estimates in Table 3</i>						
Initiative effect no CB	-42.62***	0.002	0.017	0.403	-9.61**	0.033
Initiative effect CB	8.08	0.433	-0.040**	0.011	1.11	0.744
Effect: CB vs. no CB	50.70***	0.002	-0.057**	0.024	10.72*	0.052
<i>Panel B. Conditional on unionization instead of collective bargaining</i>						
Initiative effect low unionization	-105.54***	< 0.001	0.072***	0.003	-25.93***	< 0.001
Initiative effect high unionization	17.22*	0.083	0.008	0.606	5.34	0.104
Effect: High vs. low unionization	122.75***	< 0.001	-0.063**	0.028	31.26***	< 0.001
<i>Panel C. Estimates with instrumental variables</i>						
Initiative effect no CB	-115.71***	< 0.001	0.026	0.320	-31.09***	< 0.001
Initiative effect CB	-0.99	0.929	-0.024	0.205	-1.84	0.646
<i>Panel D. Estimates with state fixed effects</i>						
Initiative effect no CB	-31.66*	0.069	0.037	0.250	-6.59	0.189
Initiative effect CB	12.53	0.323	-0.024	0.408	4.24	0.371

Notes: The numbers in panel A are based on the coefficients in Table 3. The numbers in other panels are based on similar regressions, as described in the text. The dependent variable is at the top of each column. “Initiative effect” is the difference in employment (or wages or payroll) between cities that do and do not allow the initiative, conditional on collective bargaining being permitted (CB) or not permitted (no CB), or conditional on a high or low rate of unionization. The *p*-values are for the hypothesis that the difference (“effect”) is zero.

^a FTE employment per 10,000 residents.

^b Natural logarithm of the average FTE wage.

^c FTE employment per resident times average wage.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

does take place, the initiative cuts wages, and may cut employment. In this case, the initiative counteracts the higher wages that emerge from collective bargaining.

Because of the interactive specification, the differences between initiative and noninitiative cities, called 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. Panel A of Table 4 reports the effects in a more transparent way, using the estimated models in Table 3. Row 1 reports the initiative effects (coefficient β_1) for cities without collective bargaining. Consistent with the model, employment is lower by 42.62 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 136.87 workers per 10,000 residents. In contrast, average wages are within 2 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, and different from zero at better than the 5 percent level of statistical significance. In short, the evidence generally supports the theoretical implication that the initiative mainly cuts employment when collective bargaining does not take place.

Row 2 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 the initiative cuts wages by 4.0 percent and, more important, that wages are 5.7 (= 4.0 + 1.7) percent lower in cities with the initiative than cities without the initiative. This difference is different from zero at better than the 5 percent level of significance. Since the collective bargaining wage premium is 18.2 percent when the initiative is not available, the evidence suggests that the initiative rolls back about one-quarter of the wage premium

associated with collective bargaining. The first column shows that the initiative is associated with a modest increase in employment of 8.08 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 positive, but not estimated precisely enough to distinguish from noise. 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.

In support of one theme of the analysis, the initiative has a different effect on employment, wages, and total payroll expenditure when collective bargaining is and is not available. For all three policies, the differences are statistically different from zero at the 6 percent level of significance or better.

The collective bargaining variable in panel A indicates whether collective bargaining is required, but not how important it is in practice. I also estimated equation (1) using a dummy for union membership instead of collective bargaining laws. Specifically, each state was coded as having “high” unionization if the fraction of workers belonging to a union exceeded 11 percent, and “low” unionization otherwise.²² This variable is less consonant with the theory—even if unionization is high in a city, we would not expect collective bargaining effects if state law prohibits collective bargaining—but it serves as a robustness check. To conserve space, I do not report the regression coefficients, but only the implied initiative effects. High unionization (compared to low unionization) is associated with 17.8 percent higher wages, 127.18 lower employment, and \$29.91 less payroll expenditure per capita, all of which are different from zero at better than the 1 percent level (not reported). Panel B of Table 4 shows the initiative effects. As in panel A, the initiative is associated with significantly lower public sector employment and payroll expenditure when unionization is low, but modestly higher employment when unionization is high. In contrast to panel A, the initiative is associated with higher wages when unionization is low, inconsistent with the theory, and suggesting some caution is due concerning the wage effect. As for the general point—that the effect of the initiative depends on whether unionization is high or low—all of the differences are statistically insignificant.

Although availability of the initiative is plausibly exogenous for these data, relieving one source of spurious correlation, there still could be an unmeasured city characteristic that is correlated with initiative availability and the policy outcomes. To address this issue, I estimated the regressions using an index of state laws pertaining to local initiatives as an instrumental variable. Based on state constitutions and statutes, each state was coded 3 if all cities were required to provide the initiative, 2 if only home rule cities were required to provide the initiative, 1 if a narrower set of cities were required to provide the initiative, and 0 if no cities were required to provide the initiative. The idea behind this instrument is that these state laws are unlikely to be correlated with unobserved characteristics of a specific city and are unlikely to be correlated with the policy outcomes. The state-law index is a highly significant ($p < 0.001$) predictor of city initiative status. As before, to conserve space I do not report the coefficient estimates, but only the implied initiative effects in panel C of Table 4.²³ The magnitudes are different from panel A, but the results are qualitatively similar. In cities without collective bargaining, the initiative is

²² The cutoff is approximately the median, and was chosen to fit a natural break in the data. I use dummy variables for ease of interpretation and to facilitate comparison with the other estimates in Table 4.

²³ The results in panel C are from two separate regressions for cities with and without collective bargaining. I did not estimate a pooled regression because the endogenous variable (initiative status) appears in the regression directly as an interaction term. Because the endogenous variable is dichotomous, I estimated a two-stage treatment effects model.

associated with significant employment and expenditure cuts. In cities with collective bargaining, the initiative is associated with wage cuts but the effect is not statistically significant. The effect of the initiative continues to vary when collective bargaining is and is not available, and in the direction predicted by the model.

A final (demanding) test is to estimate the regressions with state fixed effects, which controls for 100-year persistent differences in cities that are in the same state, and effectively uses only within-state variation to measure the effects. The initiative effects from these estimates are reported in panel D, based on separate regressions for states with and without collective bargaining. Even though the state fixed effects run the risk of absorbing a lot of meaningful information, the results remain qualitatively similar. Significance levels generally fall, but the initiative is still associated with employment cuts when collective bargaining is not required, but not when collective bargaining is required. The initiative is associated with wage cuts in collective bargaining cities, but the effect is not statistically significant. The initiative effect across all three policies continues to vary with collective bargaining requirements, with employment and expenditure cuts greater when collective bargaining is not required, and wage cuts greater when collective bargaining is required.

In order to assess further the robustness of these findings, I estimated a variety of other regressions that I do not report. In particular, I considered additional control variables (median age, urbanization, race of population, crime rate, unemployment, form of government, state-level tax or expenditure limit, 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 trade-offs 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, street and highway jobs might be easier to fill with patronage employees who drop by the office once a day to punch the clock. 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 15.2 percent to 23.5 percent, and all effects are different from zero at 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

TABLE 5—EFFECTS OF INITIATIVE AND COLLECTIVE BARGAINING (CB) BY FUNCTION

	Employment ^a		Wages ^b		Payroll ^c	
	Effect	<i>p</i>	Effect	<i>p</i>	Effect	<i>p</i>
<i>Panel A. Administration</i>						
CB effect no initiative	1.93**	0.041	0.186***	< 0.001	1.06***	0.001
Initiative effect no CB	1.05	0.150	0.048**	0.035	0.42*	0.092
Initiative effect CB	0.94	0.152	-0.006	0.782	0.32	0.160
Initiative effect: CB vs. no CB	-0.11	0.913	-0.054*	0.074	-0.11	0.751
<i>Panel B. Firefighters</i>						
CB effect no initiative	-3.32***	0.006	0.152***	< 0.001	-0.21	0.687
Initiative effect no CB	-1.57	0.164	-0.036	0.260	-0.75	0.125
Initiative effect CB	-0.20	0.805	-0.004	0.879	0.03	0.928
Initiative effect: CB vs. no CB	1.77	0.191	0.033	0.397	0.78	0.183
<i>Panel C. Police</i>						
CB effect no initiative	-2.09	0.116	0.168***	< 0.001	0.87	0.147
Initiative effect no CB	0.48	0.681	-0.003	0.910	0.14	0.799
Initiative effect CB	-0.85	0.360	-0.010	0.657	-0.30	0.472
Initiative effect: CB vs. no CB	-1.33	0.363	-0.007	0.849	-0.44	0.509
<i>Panel D. Streets and highways</i>						
CB effect no initiative	-1.76**	0.026	0.235***	< 0.001	1.60	0.525
Initiative effect no CB	-1.05*	0.083	0.030	0.265	-0.03	0.883
Initiative effect CB	0.69	0.210	-0.038	0.118	0.12	0.485
Initiative effect: CB vs. no CB	1.73**	0.030	-0.069*	0.056	0.15	0.556
<i>Panel E. All other</i>						
CB effect no initiative	-55.38***	< 0.001	0.152***	< 0.001	-11.84**	0.012
Initiative effect no CB	-46.08***	< 0.001	0.001	0.980	-11.28***	0.002
Initiative effect CB	9.57	0.351	-0.019	0.367	1.75	0.593
Initiative effect: CB vs. no CB	55.66***	< 0.001	-0.020	0.525	13.03***	0.007

Notes: 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 652 observations for administration and all other, 612 observations for firefighters, 646 observations for police, and 650 observations for streets and highways.

^aFTE employment per 10,000 residents.

^bNatural logarithm of the average FTE wage.

^cFTE employment per resident times average wage.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

between cities with and without the initiative (again called the “initiative effect” for short), conditional on whether 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”—in many respects match the patterns in Table 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 highways and all other jobs. The coefficient of -1.76 for streets and highways is large compared to the average employment level, and could mean that jobs in this function are particularly attractive to politicians for patronage purposes, leading voters to make big cuts when they have control. The initiative is associated with wage cuts when collective bargaining occurs, and the wage cut is greater than without collective bargaining except for fire fighters. The difference between initiative and noninitiative cities is statistically significant only for street and highway workers.

For police, collective bargaining is not associated with a significant employment reduction, the estimated initiative effects are quantitatively small, and none of them is statistically significant. The tests could lack power, but taken at face value, the estimates suggest that the initiative has little effect on public employment policies for police. In terms of the model, this could happen if citizen and politician utility functions are similar (meaning police provide few patronage benefits to the politician).

The most anomalous case is administration. Like the other spending functions, collective bargaining is associated with higher wages, in this case 18.6 percent higher. Unlike the other functions, however, collective bargaining is associated with *more* employment, and the effect is statistically different from zero at the 5 percent level. Such an outcome could be consistent with the theory if administrative workers had preferences for higher employment and higher wages. Administrative workers might prefer having more coworkers because it reduces their workload and makes their jobs less demanding (but that would seem true for other functions as well). If so, we would expect the initiative to trigger cuts in wages and spending, but the initiative effects are not significant and the employment effect even goes in the wrong direction. Why voters do not use initiatives to unravel the collective bargaining outcomes is not clear. It is also puzzling—and difficult to understand in terms of the theory developed above—why the initiative is associated with higher wages when collective bargaining is unavailable. A speculative explanation might be that administrative employees are able to control the initiative agenda and put proposals on the ballot that advance their interests to the detriment of the citizens at large. Such an interpretation would square with evidence suggesting that administrative positions are the last to be cut in response to a tax and expenditure limit (David N. Figlio 1998; Figlio and Arthur O’Sullivan 2001).

IV. 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 (although this finding should be treated with more caution because it is less robust than the employment finding). The initiative is associated with smaller employment cuts when collective bargaining is available than when it is unavailable. This pattern appears in 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 almost by assumption in the model because only helpful initiatives are 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 standard result from complete information models. 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 as tentative because, in more complicated models with asymmetric information, citizens can be made worse off by having initiatives available. Matsusaka and McCarty (2001) show that when 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 that is harmful to citizens if 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 surprisingly large number of studies fail to find clear effects (Burton Abrams and William R. Dougan 1986; Richard F. Dye and Therese J. McGuire 1997; Gerber et al. 2000; Thad Kousser, Mathew D. McCubbins, and Ellen Moule 2008). Poterba and Kim 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 that the employment and wage reductions associated with the initiative can be brought about. However, 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 available. Put differently, the important institutional feature behind tax and spending cuts may be the initiative, not the TEL. The finding that local and state expenditure cuts are 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 David O. Sears and Jack 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 can lead to wage cuts without changing the number of public sector employees, thereby reducing the tax bill without affecting services.

APPENDIX: PROOFS

PROOF A (Policy choice with collective bargaining but no initiative):

The union's problem is

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

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 $U(l, w_l) \geq U_C$. Denote the solution (l_l, w_l) 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 an optimum. Therefore, the optimum must satisfy $l_l \leq l_{CB}$. To guarantee the politician's reservation utility, it must be the case that $w_l \leq w_{CB}$. The two inequalities together imply $l_l w_l \leq l_{CB} w_{CB}$.

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