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Politicians, Bureaucrats and Targeted Redistribution: The Role of Career Concerns[*]

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Abstract

Stronger career concerns induce appointed bureaucrats to adopt different policies than elected politicians. In particular, bureaucrats are less likely to use targeted redistribution to achieve personal political goals. I use the example of patronage jobs in local governments in the United States to provide empirical support for this claim. I show that the number of full-time public employees is significantly higher in local governments with elected chief executives. This difference increases during election years. In addition, consistent with the notion that career concerns are especially strong for *young* bureaucrats, I find that the number of full-time public employees increases with the age of appointed chief executives. There is no such relationship in the case of *elected* chief executives.

1 Introduction

One of the central issues in political economy is how different institutional arrangements affect public officials' incentives and the policies they pursue. One such arrangement is the method of selection of public officials, in particular, whether they are elected or appointed. Although there is a strong theoretical argument that differences in the method of selection should play an important role,[1] the empirical evidence about systematic differences in the policies pursued by appointed versus elected public officials remains scarce.

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[1]E.g. Besley and Coate (2003) , Maskin and Tirole (2004), Alesina and Tabellini (2007a, 2007b).

In this paper, I focus on targeted redistribution as one example of a policy that depends on whether a public official is elected or appointed. Diversion of resources from useful public projects to targeted redistribution and pork-barrel projects is a widely recognized source of inefficiency caused by the political process (e.g. Buchanan and Tullock, Lizzeri and Presico, 2001). In this paper, I argue that although both appointed and elected public officials are tempted to use targeted redistribution to achieve their political goals, appointed public officials use it less because of stronger career concerns.

I consider excessive public employment as an example of targeted redistribution and use data from U.S. local governments to support this claim. One for of targeted redistribution is selective transfers to narrow groups of voters designd to increase the chances of staying in office. It provides political benefits to public officials by attracting the support of its recipients. For elected public officials, it increases the number of voters who support them during the elections. For appointed public officials, it increases their chances of staying in office by influencing electoral support of the politicians who are responsible for their appointment.

However, if such transfers are financed from the same budget as public goods, then targeted redistribution will result in underprovision of public goods. Inadequate provision of public goods will result in dissatisfaction among voters not receiving the transfers. It also harms the reputation of public officials as efficient public managers.

In choosing the level of targeted redistribution, public officials take into account not only its effect on their chances of staying in office but also its effect on their job prospects should they not stay in office. For appointed bureaucrats, the next job is more likely to be closely related to the current occupation, and it is relatively more important to maintain a good reputation as efficient public managers. Thus, as long as career concerns are stronger for appointed public officials, they will be more reluctant to use inefficient targeted redistribution than their elected counterparts. This notion is in line with Weber ([1904-1911] 1978) who underlined career-building considerations as an important ingredient in bureaucratic efficiency.

I provide a simple model to illustrate this logic and use data on public employment in U.S. local governments to test for empirical support for this argument. Public employment can proxy for redistribution, because public employees receive a wage premium over private employees at comparable levels (Ehrenberg and Schwarz, 1987; Gregory and Borland, 1999). Following Alesina et al (2000) I focus on the level of public employment, rather than on wages. I show that directly elected chief executives of local governments are more likely to use public employment for redistribution than their appointed counterparts. That is, elected chief executives on average hire more full-time public employees. These results hold with and without community fixed effects. For part-time employees who are less likely to be hired

for political reasons, there is almost no difference among communities with different forms of government.

Next, I show that there are noticeable political cycles in the number of full-time public employees in communities headed by elected chief executives, whereas this is not the case for communities with appointed chief executives, or for part-time employees. I provide additional evidence using data on the privatization of public-service provision. Regardless of the form of government, privatization of a public service decreases the number of public employees involved in the production of that service, but increases the number of public employees involved in the production of all other public services. The resulting effect of privatization on total public employment is negative only in those local governments headed by appointed chief executives. Elected executives find a way to privatize the provision of public services without a corresponding decrease in the total number of public employees.

In analyzing the mechanisms that drive differences between elected and appointed executives, I also examine the relationship between the age of the executives and public employment. I show that for appointed chief executives there is a strong link between the number of full-time public employees and the age of the executive. Consistent with the career concerns explanation, I find that the younger the appointed chief executive, the lower the number of full-time employees. There is no such relationship for elected public officials, for whom career concerns are less important. There is also no relationship between the number of part-time employees and the age of the executive (whether appointed or elected).

Overall, the empirical evidence provides strong support for the claim that appointed bureaucrats are less likely to use targeted redistribution than elected politicians and that this difference is, at least in part, driven by the difference in career concerns.

Excessive public employment is a convenient form of getting electoral support in exchange for transfers to a specific group of voters. First, it allows the targeting of transfers to specific voters. Second, it permits public officials to disguise transfers and to avoid opposition to targeted transfers (Alesina et al, 2000, 2001). Finally, it helps to solve the double commitment problem prevalent in vote buying (Robinson and Verdier, 2003).

Historically, U.S. local governments are notorious for the widespread use of patronage. Distributing public jobs was the cornerstone of political machines in charge of city politics for many years. Politicians themselves admitted the use of patronage. At a press conference, Mayor Richard J. Daley of Chicago told the journalists: "The governor in his message last week said 36 percent of city employees were patronage, and that is inaccurate, untrue and false. There is less than 8 percent." [2] Thus,

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[2] *Chicago Tribune*, April 2, 1969.

3

patronage is a convenient example of targeted redistribution in the context of U.S. local governments.

The remainder of the paper is organized as follows: the next section discusses the related literature. Section 3 presents a simple model to illustrate the argument. Section 4 describes the empirical hypotheses. Section 5 presents results, and section 6 concludes.

2 Related Literature

According to the classical political economy theory, representative democracy produces the policy preferred by the median voter. Since bureaucrats are themselves appointed by elected politicians, they should follow exactly the same policies as directly elected public officials, so earlier literature suggested that the method of selection of public officials has no effect on policy outcomes (Baron, 1988; Laffont, 1996). In particular, there should not be any systematic differences between cities with different forms of government (Deno and Mehay, 1987). More recent literature, however, shows that the method of selection does play a role. According to Besley and Coate (2003) direct elections allow voters to unbundle policy issues and thus reduce the influence of the special interests. Maskin and Tirole (2004) argue that politicians follow more closely the preferences of the general population compared to their appointed counterparts, but are more likely to pander to public opinion and choose popular actions even when they know that they are not in the general interest of the society. A similar argument is developed in Vlaicu (2008). Alesina and Tabellini (2007a, 2007b) provide a general theoretical framework to analyze the criteria that guide the allocation of policy tasks to directly elected politicians or appointed bureaucrats. They argue that the main difference lies in their motivation. Politicians care only about winning elections, whereas bureaucrats try to maximize their perceived competence. They show that the preferable method of selection of public officials depends on the features of the task they have to perform. These two papers are very close in spirit to the argument in my paper, since maximizing perceived competence is closely related to career concerns. However, in these papers the main emphasis is made on the difference in the functional form of the objective functions of politicians and bureaucrats, rather than the role of career concerns. In addition, they do not provide empirical support for their theoretical argument.

Empirical studies provide evidence that the difference in the method of selection of public officials does affect the policies they pursue. Fields et al (1997) demonstrate that elected commissioners from the insurance industry follow more pro-consumer policies than their appointed counterparts. Besley and Coate (2003) find a similar effect in electricity regulation. There is also evidence that states with appointed

judges have higher litigation rates (Hanssen, 1999) and lower levels of discrimination charges (Besley and Payne, 2003), as compared to states with elected judges. Whalley (2010) shows that appointed city treasurers reduce a city's cost of borrowing.

Works comparing the performance of local governments headed by appointed and elected executives were focused primarily on the differences in public spending and produced mixed results, which are not directly related to the extent of targeted redistribution.[3] However, two recent papers are more closely related to the issue of targeted redistribution. Levin and Tadelis (2008) find that cities with appointed chief executives are more likely to privatize provision of public services, whereas Vlaicu (2008) finds pronounced electoral cycles in police officer hiring in mayor-council cities that are absent in council-manager cities. The results in these two papers are fully consistent with the claim that appointed chief executives are less likely to be engaged in targeted redistribution and are discussed in greater details below.

A number of works provide systematic evidence on the importance of the political use of public employment in U.S. cities (Alesina et al, 2000; Riordan, 1994; Glaeser and Shleifer, 2005) and southern Italy (Chubb, 1982). There are also theoretical arguments for why inefficient income redistribution through public employment instead of direct transfers is used to get political support (Robinson and Verdier, 2003). Lopez-de-Silanes et al (1997) and Boyko et al (1996) demonstrate that the decision of local governments to privatize provision of public services is to a large extent motivated by political patronage considerations. They show that privatization is more likely if political benefits from public employment are lower, e.g. if political activity of public employees is forbidden.

An important issue for the identification strategy used in the empirical part of the paper is the endogeneity of the form of government. There are very few rigorous attempts to study the determinants of the form of government. Alesina, Aghion and Trebbi (2007) show that ethnic fractionalization plays an important role in determining electoral rules. I take into account their results by controlling for ethnic fractionalization of the communities in the empirical analysis. To address this problem in the paper, I provide several sets of results that are unlikely to be driven by the endogeneity of political institutions.

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[3]See MacDonald (2008) for the survey of such works. Two recent papers that provide the most sophisticated empirical analysis also do not produce consistent results. MacDonald (2008) found no difference, whereas Coate and Knight (2009) found that municipalities with appointed executive have higher public spending.

3 Simple model

In this section I provide a simple model of public official's career concerns to illustrate the intuition behind the empirical hypotheses. The model follows Holmstrom (1999) analysis of career concerns. In the model a public official chooses the allocation of budgetary resources between provision of public goods and targeted redistribution. Targeted redistribution increases public official's chance of staying in office, but it hurts public goods provision and, thus, has a negative effect on her career opportunities. As a result, the stronger are the career concerns, the lower is the level of targeted redistribution.

There are two periods $t = 1, 2$. Assume that there is no time discount. Each agent serving as a public official is characterized by two-dimensional ability $\Theta = (\theta_1, \theta_2)$, where θ_1 is the ability relevant for provision of public goods and θ_2 is the ability that may be important for other jobs, but is irrelevant for public goods provision. In the first period ability is unknown to everybody, including the agent. The level of public goods provision in the first period depends on the relevant part of the agent's ability and the extent of targeted redistribution:

$$y = \theta_1 - \frac{q^2}{2} + \varepsilon, \tag{1}$$

where $q \geq 0$ is the level of targeted redistribution[4] and ε is noise (random variable with mean zero and variance σ^2). Assume that the utility of the public official in the first period $\bar{U}$ does not depend on the extent of targeted redistribution. In the second period the public official either stays in office or loses her job. If she stays in office she receives the same utility $\bar{U}$. If not, she has to find another job and the quality of the new job depends on her perceived ability. Assume that as in Holmstrom (1999) the next-period wage is set by competition among principals for the public official's services and equals to the market beliefs about the public official's expected ability conditional on the realization of the first-period outcome and the two dimensions of the ability are weighted, so that the utility from the new job is $\Psi(y) = \lambda E(\theta_1|y) + (1 - \lambda)E(\theta_2)$. Here λ reflects how closely the skills required at the new job are related to the skills necessary for the efficient provision of public goods. Assume that $\bar{U} > \Psi(y)$ for all y, so that the public official always wants to stay in office.

Targeted redistribution hurts public goods provision, but increases the chance of staying in office by attracting the votes of the beneficiaries of the redistribution. The marginal negative effect of redistribution on public goods provision is negligible near the optimum, but increases and at some point outweighs the benefits of attracting the votes of the beneficiaries of the redistribution. Thus, the probability of staying in

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[4]In the case of public employment it can reflect the number of excessive emplyees hired.

office is $p(q) \in [0; 1]$, .where $p(q)$ is a quasiconcave function with $p'(0) = +\infty, p''(0) < 0$, and $p'(\hat{q}) = 0$ for some $\hat{q} > 0$. These restrictions guarantee that the problem has an interior solution Thus, the public official chooses the level of targeted redistribution to maximize her expected utility

$$E[U] = \bar{U} + p(q)\bar{U} + (1 - p(q)) E [\Psi(y)] \tag{2}$$

In pure-strategy equilibrium the market anticipates that the public official will choose the level of targeted redistribution q^*. Thus, $E(\theta_1|y) = y + \frac{q^{*2}}{2} = \theta_1 - \frac{q^2}{2} + \frac{q^{*2}}{2}$ and $\frac{dE[\Psi(y)]}{dq} = -\lambda q$. The first order condition for the agent's problem implies that

$$p'(q^*) \left(\bar{U} - E [\Psi(y)] \right) = (1 - p(q^*)) \lambda q^* \tag{3}$$

Using implicit function theorem we can show that under the assumptions made above the following proposition holds[5].

Proposition 1 $\frac{dq^*}{d\lambda} < 0$, so that targeted redistribution is lower if the skills required at the next job are more closely related to the skills necessary for the provision of public goods.

The effect of this perceived ability on job-market opportunities depends on the type of jobs a public official is likely to find after losing office. The closer the skills at the new job related to the skills required to run the government, the stronger the effect. In particular, the effect is likely to be stronger for the jobs in the public sector than for the jobs in the private sector. To provide empirical support for the claim that career concerns are stronger for bureaucrats as compared with politicians, I collect information on the career paths of the chief executives in U.S. municipalities. I start with a sample of 558 elected mayors and 372 appointed city managers who lost their office between 2002 and 2007 according to information from Municipal Yearbooks. Next, I use various internet sources (including e-mail communication with local governments) to collect information on their next job. I was able to find this information for 123 city managers and 69 mayors (see Table 1). The results indicate that more than 90% of the city managers remain in the public sector and almost 80% of them work as city managers in a different city. Among mayors only 32% remain in the public sector, whereas 43% of them move to the private sector. Thus, politicians often find a job which is not directly related to their work in public office, whereas for bureaucrats their next job is usually closely related to their work while in office.

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[5]Second order condition for the interior solution implies that $\frac{d^2 E[U]}{dq^2} < 0$, while $\frac{d^2 E[U]}{dqd\lambda} = -(1 - p(q^*)) q^* < 0$, so that $\frac{dq^*}{d\lambda} = -\left[\frac{d^2 E[U]}{dq^2} / \frac{d^2 E[U]}{dqd\lambda} \right] < 0$.

These results suggest that for appointed chief executives their reputation as efficient public managers is more important to their career then for elected mayors. In terms of the model this means that λ is higher for bureaucrats than for politicians, $\lambda_B > \lambda_P$,[6] and the negative effect of patronage on their job-market opportunities is likely to be higher. Thus, Proposition 1 implies that the level of targeted redistribution should be lower for bureaucrats than for politicians, $q_B^* > q_P^*$.

4   Background information and empirical hypotheses

In the empirical analysis I use the data on public employment in U.S. local governments. Depending on the form of local government, a chief executive can be either directly elected or appointed by an elected council. In both cases, chief executives have substantial influence over public employment policies and can use it for targeted redistribution.

4.1   Patronage

I focus on patronage as a specific mechanism of targeted redistribution. Patronage jobs are all those jobs that are distributed in exchange for electoral support and the pay for which is greater than the value of the public services performed (Weingrod, 1968: 379; Wilson, 1961). Wage premium received by public employees suggests, that on average their wage exceeds their marginal productivity, so that they are earning a rent by working in a public sector (Ehrenberg and Schwarz, 1987; Gregory and Borland, 1999). Patronage provides a convenient way to make transfers. First, it allows transfers to be precisely targeted. Second, it can disguise redistributive policies in order to circumvent opposition to explicit tax-transfer schemes (Alesina et al, 2000, 2001). Third, it provides a way to solve the commitment problem that arises in vote buying (Robinson and Verdier, 2003). Politicians face a commitment problem because policies which would induce people to vote for them are not in their interests to implement ex post. Public employment can serve as a credible way to promise a stream of benefits as long as firing public employees is costly. The latter argument fully works only for full-times employees, since part-time employees can be fired relatively easy.

The benefits of patronage come from an increase in the number of votes from public employees and, possibly, their friends and family. The cost of hiring an extra employee comes from a decrease in the efficiency of public goods production if the pay for the job is greater than the value of the public services performed. This leads to a loss of votes from all the citizens except for public employees.

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[6]Similar effects will take place if appointed public officials care more about their perceived competence because they care more about the assessment of their perfomance by professional peers (Wilson, 1989: Chapter 9) or try to secure their autonomy and independence (Carpenter, 2001).

4.2 Forms of local government in the U.S.

In the U.S., local communities can choose their form of government in a local referendum. For the purposes of the paper, I consider only one difference in the form of government: whether the chief executive officer is elected or appointed.[7] Local governments with appointed chief executives include municipalities and counties with a council-manager (administrator) form of government. In this type of government, an elected council sets a policy, adopts legislation, and approves the budget. It appoints a manager or administrator who has full responsibility of the day-to-day operation of the local government and has authority to oversee department heads, hire and fire local governments' staff (often including department heads), recommend policy to the council and prepare the budget.

Local governments with elected chief executives include municipalities with a mayor-council form of government and counties with a council-elected executive form of government. In this type of government, an elected council serves as a legislative body with a chief executive being elected separately. As in the previous case, the council sets policy, adopts legislation, and approves the budget. The chief executive implements the policies adopted by the council, prepares the budget, and exercises some authority over day-to-day operations of the local government, the extent of which varies from jurisdiction to jurisdiction.

Thus, in both cases the authority to make the decisions related to public employment lies in the hands of the chief executive and the main difference between them is the method of selection of the chief executive.[8]

4.3 Empirical Hypotheses

As long as career concerns are weaker for the case of elected public officials, we expect the extent of targeted redistribution to be higher in the case of elected public officials. As public employees receive a wage premium over private employees of comparable levels (Ehrenberg and Schwarz, 1987; Gregory and Borland, 1999), at the margin public employment can be considered as a form of targeted redistribution. So we expect the following hypothesis to hold for public employment in U.S. local governments:

Hypothesis 1. *The number of full-time public employees in local governments*

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[7]Communities with forms of government that can not be easily interpreted as headed by appointed and elected public officials are excluded from the analysis. These constitute approximately one third of the total number of local governments and include municipalities with commission, town meeting and representative town meeting forms of government and counties with commission form of government.

[8]This is different from the case of decisions regarding the size of the budget that are analyzed in Coate and Knight (2009), since appointed chief executives have no *de jure* authority over these decisions.

headed by elected chief executives is higher than in local governments headed by appointed chief executives.

This prediction relies on the assumption that without patronage there are no systematic differences in the level of public employment between local governments with appointed and elected chief executives. Clearly, there are many reasons why this assumption may be violated. Appointed chief executives can be more efficient in public employment management, so that they can provide the same level of public goods using fewer employees. Alternatively, appointed chief executives can be more susceptible to the influence of special interests that lobby for higher expenditures on purchases of equipment, services etc., at the expense of lower level of public employment. For this reason I formulate additional empirical hypotheses that are in line with the targeted redistribution story, but are not consistent with the alternative explanations.

The main reason behind excessive number of public employees is that it increases the chances of reelection for the incumbent politicians. These considerations are especially important during election years. Thus, we should expect the differences in the extent of patronage between appointed and elected executives to be especially high during election years:[9]

Hypothesis 2.*The difference in the number of full-time public employees in local governments headed by elected chief executives is especially high during election years.*

The logic outlined above can be extended to make predictions about the relationship between privatization of public services provision and public employment. The main argument for privatization is that it helps to achieve greater efficiency, primarily by cutting labor costs (Savas, 1987, Donahue 1989, Kemp 1991). Since privatization should lead to a decrease in public employment, we expect to see less privatization in communities headed by elected chief executives. The results of Levine and Tadelis (2008) confirm this prediction. In addition, when public services are privatized, we expect to see a differential effect of privatization on the level of public employment. Public officials may privatize the provision of public services without a corresponding decrease in public employment by reallocating public employees formerly engaged in the provision of the privatized public service to the provision of other public services. If elected public officials are more concerned with mitigating the effect of privatization on the level of public employment, the effect of the number of privatized public services on the level of public employment should

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[9]This hypothesis is closely related to the extensive literature on political business cycles. This literature provides both theoretical reasons for the existence of political cycles (Rogoff and Sibert, 1998; Rogoff, 1990; Persson and Tabellini, 1990) and empirical evidence (Drazen, 2000; Franzese, 2002; Ahmedov and Zhuravskaya, 2004).

be weaker in local governments headed by elected public officials. Thus, we can formulate the following hypothesis:

Hypothesis 3. *The decrease in the number of full-time public employees following privatization of public services is smaller in local governments headed by elected chief executives.*

All the these hypotheses test whether appointed executives are less likely to be engaged in targeted redistribution compared to their elected counterparts, but they do not test whether this difference is driven by career concerns. As long as career concerns of appointed bureaucrats play an important role in preventing them from using targeted redistribution, we should expect to see less targeted redistribution for younger appointed executives, since they have stronger career concerns (Holmstrom, 1982). For elected executives, for whom career concerns do not play an important role, we should see a weak relationship, if any, between the age of the executive and the extent of targeted redistribution.

Hypothesis 4. *The number of full-time public employees is lower in communities with younger appointed chief executives. In communities with elected chief executives the number of full-time public employees does not depend on the age of the chief executive.*

5   Empirical results

5.1   Data

To compare the level of public employment in local communities with different forms of government I collect data on the form of government, local public employment and various other characteristics of local communities for the years 1987, 1997 and 2002. More information on the data sources, variables' definitions and coverage can be found in the Data Appendix. Summary statistics for the variables used in the analysis are presented in Table 2. I restrict the analysis to communities with a population above 30,000 inhabitants for two reasons. First, the sample is not representative for smaller communities (see Data Appendix). Second, in smaller communities endogeneity of the form of government is likely to affect the results. In smaller communities mayors often work part-time and receive only symbolic remuneration, whereas hiring a city manager requires paying a competitive wage. Thus, among small communities the form of government can be correlated with the wealth of community, which can also affect public employment policies.

Out of the 1,546 local governments in the sample, for 764 governments the information on the form of government is available for at least two years. Of these, 102 local governments changed their form of government (see Table 3). Out of 562

local governments headed by an appointed chief executive, 47 local governments changed their form of government to that of an elected chief executive. Out of 202 local governments that had elected chief executives, 55 local governments changed their form of government to that of an appointed chief executive. For all the local governments in the sample, changes in the form of government occur between 1987 and the subsequent years.

In the analysis of political cycles I use the data on the form of government an election dates in the large U.S. cities.[10], as well as information on public employment and local finance The sample includes yearly observations for 196 cities for the period between 1987 and 2002. In this sample 107 cities have an appointed chief executive, 84 have an elected chief executive, and 5 cities changed their form of government from appointed to elected chief executive.

To examine the relationship between the age of chief executive and the level of public employment I collect information on the age of chief executives. For city managers information was hand collected from the Who's Who in Local Government Management provided by ICMA. For mayors the information was collected from the websites of the local governments. The resulting dataset contains information on 352 city managers and 270 mayors who were in office in 2006-2007.

5.2   Level of public employment

To test Hypotheses 1, I compare the level of public employment in local communities headed by elected and appointed chief executives. Changes in the form of local governments allow me to conduct analysis using community fixed effects. The benefit of such an analysis is that it allows controlling for the community characteristics that do not change over time. The main drawback of this approach is that it limits the analysis of the direct effect of the form of government to those communities that have changed it during the period under consideration. Since the reasons for these changes might be different from the historical determinants of the form of government in total population of the communities, I report the results both with and without community fixed effects. I use the following panel model:

$$y_{it} = \alpha \cdot Elected_{it} + X'_{it} \cdot \beta + \tau_t + \varepsilon_{it} \tag{4}$$

where y_{it} is the logarithm of the number of public employees (either full-time or part-time) in community i at time t, $Elected_{it}$ is a dummy variable that tells whether the chief executive of the community i at time j was elected, τ_t is a period fixed-effect, and X_{it} is a vector of covariates describing characteristics of community j at time t, which includes the measures of population, income per capita, total

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[10]I thank Razvan Vlaicu for generously providing the data.

12

budgetary expenditures, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of population above 25 years old with at least 12 years of education, and fraction of urban population, the number of years since incorporation, a dummy variable that equals one for a homerule community, and a dummy variable for the type of local government. In addition, depending on the specification, the vector of controls includes either state or community fixed effects. To take into account the panel structure of the data, the errors are clustered at the community level. I also check whether the results of the regressions without fixed effects are robust to relaxing the linearity assumption by using nonparametric matching estimation (see the subsection on sensitivity analysis below).

Since we can expect downward rigidity of public employment, the effect of a switch from appointed to elected chief executive might be different from the effect of a switch in the opposite direction. For these reasons in the regression analysis of the within effects I also use the following panel model:

$$y_{it} = \alpha \cdot BecomesElected_{it} + \beta \cdot BecomesAppointed_{it} + X'_{it} \cdot \gamma + \delta_i + \tau_t + \varepsilon_{it} \quad (5)$$

where y_{it} is a measure of public employment in community i at time j. The variable $BecomesElected_{it}$ equals one if in community i at time t the chief executive was elected, but previously the chief executive was appointed. The variable $BecomesAppointed_{it}$ equals one if in community i at time t the chief executive was elected, but afterwards the chief executive becomes appointed, X'_{it} is the same vector of covariates as described above, α_i and τ_t are community and time fixed-effects respectively.

Results of the estimation indicate that the number of full-time public employees is significantly higher in communities with elected chief executives (Table 4). The magnitude of the effect is large: the number of full-time public employees is higher in communities with elected chief executives by 5 or 7 percent, depending on whether I include communities' fixed effects or not. There is evidence that the number of public employees exhibits downward rigidity: a change from appointed chief executive to elected leads to a 9 percent increase in the number of full-time employees, whereas a change from elected to appointed chief executive does not have a significant effect on the number of public employees.[11]

If the difference in the level of public employment between appointed and elected executives is indeed driven by political considerations, we should expect this difference to be lower for part-time employees, since political benefits from redistribution towards part-time employees are smaller. First, the wage differential is likely to be higher for full-time employees, since part of it comes from the fringe benefits that

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[11]Equality of the two coefficients is rejected at 10% level of significance.

part-time employees are not getting. Second, according to Robinson and Verdier (2003) the level of excessive public employment is lower for public employees that are easier to fire. For part-time employees the costs of firing are significantly lower that for full-time employees, since most of them have short-term contracts and cannot claim the same benefits as full-time employees if they are fired. Consistent with this prediction, there is no evidence that the number of part-time public employees is higher in communities with elected chief executives. All the results are insignificant and in three out of four cases have the wrong sign.

Overall, we find strong evidence in support of Hypotheses 1 that the number of full-time employees is significantly higher in communities with elected chief executives. At the same time, the number of part-time employees is not affected by the form of local government.

5.3 Political Cycles

I examine how the difference in the number of public employees between communities with appointed and elected chief executives changes during the election years. I use the following panel model:

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\begin{aligned}
y_{it} &= ElectionYear_{it} \cdot Elected_{it} \cdot \alpha + Elected_{it} \cdot \beta + ElectionYear_{it} \cdot \gamma + \quad (6) \\
&\quad + \tilde{X}'_{it} \cdot \lambda + \tau_t + \varepsilon_{it}
\end{aligned}
$$

where $ElectionYear_{it}$ is a dummy variable that equals one if in city i there were elections in year t. For cities with elected mayors this is the year of mayoral elections and for cities with appointed city managers this is the year of elections of the city council responsible for the appointment of the city manager. The vector of controls includes the measures of population, income per capita, total budgetary expenditures, as well as the measure of taxes per capita.[12] Standard errors were clustered at the community level.

Results of the estimation are reported in Table 5. As in the results discussed above, the number of full-time employees is significantly higher in cities with elected chief executives. More importantly, the results indicate that the difference increases by an additional 2 to 4 percent during the election years.[13] Vlaicu (2008) argues that the number of police officers can exhibit political cycles not because there are

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[12]Other control variables used in the previous analysis could not be included in this analysis since they are not available on a yearly basis.

[13]An increase in the difference comes primarily from the statistically significant increase in the number of full-time employees in cities with elected chief executives. In cities with appointed chief executives the number of full time employees seems to be decreasing during the election years, but the effect is much smaller and insignificant in the specification that does not control for city fixed effects.

patronage reasons, but because elected executives pander more to public opinion. His argument is that public officials hire excessive number of police officers because public safety is a salient issue and people felt strongly that a larger police force in the streets was unambiguously good for their welfare. To take this effect into account I exclude public employees involved in police protection. The results in column (3) and (4) indicated that in this case the results remain basically unchanged. Similarly to the previous set of results, if we look at the number of part-time employees instead of full-time employees, we find no significant difference either between cities with a different form of government or between election and nonelection years.

Overall, I find evidence in support of Hypotheses 2 : the difference in the number of full-time public employees between cities headed by elected and appointed chief executives is especially high during the election years, primarily because of a significant increase in the number of full-time employees in cities headed by elected mayors. At the same time, for the number of part-time employees the difference is insignificant both in election and nonelection years.

5.4   Effect of Privatization

To estimate the effects of privatization on public employment, I use the following panel model:

$$y_{itg} = \alpha \cdot Elected_{it} \cdot Privatized_{itg} + \beta \cdot Appointed_{it} \cdot Privatized_{itg} + X'_{itg} \cdot \gamma + \varepsilon_{itg}$$

where y_{itg} is a measure of public employment in public service g (or in all public services but g) in community i at time j, the dummy variable $Privatized_{itg}$ tells whether the provision of public services g in community i at time t is privatized, $Appointed_{it} = 1 - Elected_{it}$ and $\tilde{X}_{itg}$ includes all the variables in (4) as well as community, year, and public service fixed effects. Thus, in these regressions I look at how privatization of a provision of a particular public service affects the number of public employees involved in the provision of this public service (or public employees involved in the provision of all other public services). To estimate the effect of privatization on total public employment I use as the dependent variable y_{it} – the measure of the total number of employees in community i at time j. To separate the effect of privatization from the effect of a change in the form of government I restrict the sample to include only those cities in which the form of government remained unchanged through the period under study.

Empirical results indicate that regardless of the form of government, privatization of a public service leads to a decrease in the number of public employees involved in the production of the privatized public service and an increase in the number of

public employees involved in the production of other public services (see Table 6). The effect on the total number of public employees, however, depends on the form of government. In communities with appointed chief executives, privatization leads to a significant decrease in the total number of public employees, whereas in communities with elected chief executives there is no significant relationship between privatization and public employment. In other words, only elected officials find a way to privatize provision of public services without reducing the number of public employees. Thus, the empirical results support Hypothesis 3.

5.5   Age of Chief Executive

All of the empirical evidence provided so far supports the notion that appointed bureaucrats are less likely to be involved in making targeted transfers through public employment, but does not shed light on the mechanisms that cause the difference. To test whether the difference in public employment policy between appointed and elected chief executives is indeed driven by career concerns, I examine the relationship between the number of public employees and the age of chief executive. If career concerns play an important role in limiting the extent of targeted redistribution by appointed bureaucrats, we would expect that the number of public employees would be higher for older city managers, for whom career concerns are weaker, as compared with younger city managers, who face strong career concerns. For elected mayors, however, we do not expect to see any association between their age and the number of public employees, as for predominant majority of mayors career concerns considerations do not play an important role in determining the extend of redistribution. To test this hypothesis I use the following regression model:

$$y_{it} = \alpha \cdot Age_{it} + + X'_{it} \cdot \beta + \tau_t + \varepsilon_{it} \tag{7}$$

where Age_{it} is the age of the chief executive in community i at time t, and the dependent variable y_{it} and control variables X_{it} are the same as in (4).

Results of the estimation indicate that there is a strong positive relationship between the age of appointed chief executives and the number of full-time public employees (see Table 7). These results confirm the hypothesis that younger appointed chief executives are less involved in targeted redistribution. For elected chief executives the relationship is not significant and has the opposite sign, which is consistent with the hypothesis that for them career concerns do not play important role in determining the extent of redistribution. At the same time, there is no significant relationship between the age of either elected or appointed chief executives and the number of part-time public employees. One potential concern is that the results are driven by the differences in tenure in office of the appointed and

16

elected chief executives. However, the results remain unchanged if we control for tenure in office, which does not seem to play any role in determining the number of public employees.

The magnitude of the effects implies that ten years difference in the age of city managers (which is slightly higher that the standard deviation of 7.4 years) corresponds to a 7 percent difference in the number of full-time public employees. Thus, a switch from appointed to elected chief executive, which leads to a 9 percent increase in the number of full-time public employees, is equivalent to a switch to a city manager who is 13 years older.

Overall, the empirical results confirm both Hypotheses 4 and 4a. These results are especially important for the argument of the paper, since they provide evidence that the difference in the extent of targeted redistribution between appointed and elected public officials is indeed driven by the differences in career concerns. In addition, it is much harder to come up with an alternative explanation for these results that is driven by heterogeneity of the cities that choose different forms of government.

5.6 Sensitivity analysis

I check whether the results regarding the number of public employees are robust to using covariates matching estimation instead of regression analysis. Since a large number of covariates leads to a significant bias in simple matching estimation if matching is not exact (Abadie and Imbens, 2006) I use a bias-adjusted matching estimator (Abadie et al, 2004). Table AII reports the results of the matching estimation. The results confirm that the number of full-time employees is significantly higher in communities with elected chief executives. The difference in the number of part-time employees becomes significant, but its magnitude is noticeably lower than in the case of full-time employees. Since matching estimations do not take into account the panel structure of the data, they are likely to underestimate standard errors of the estimated effects. To address this issue, I restrict the sample to observations in each of the three years separately. For the number of full-time employees the results remain significant in each of the three subsamples, whereas for part-time employees the effect is much smaller and insignificant, except for the year 2002. In addition, I compare the results obtained using different numbers of matches. Increasing the number of matches does not affect the significance or the magnitude of the results.

To see if the results are contaminated by the lack of overlap in the covariate distribution between communities with appointed and elected chief executives, I calculate the propensity score for all the observations based on the same set of covariates as in the benchmark model and exclude those observations for which the

propensity score is either smaller than 0.1 or greater than 0.9. After that I repeat all the estimations on this restricted subsample. The results prove to be very close to the results on the whole sample in terms of size and significance, so that the lack of overlap in the covariate distribution does not affect the results.

To assess the unconfoundedness assumption for the matching results with community fixed-effects, I compare the level of public employment in 1987 in communities that later on experience a change in the form of government with those communities that do not experience such a change. I find no significant difference between such communities. Thus, there is no evidence that the unconfoundedness assumption is violated. I also estimate the treatment effect on the treated using covariates matching, as well as propensity score matching. The results for the number of full-time employees remain highly significant, but somewhat smaller in magnitude. The results for the number of part-time employees remain significant in the case of propensity score matching, but lose their significance in the case of covariates matching.

Vlaicu (2008) argues that the number of police officers differs between elected and appointed chief executives not for patronage reasons, but because elected executives pander more to public opinion. To see if this is the case I compare the number of public employees separately for public employees working in police protection. and for all public employees excluding those working in police protection. The results indicate that the number of full-time public employees is significantly higher in communities headed by elected chief executives even if we exclude public employees in police protection (Table AIII). However, after controlling for community fixed effects the difference is higher for full-time public employees in police protection. Moreover, the difference is also significant for part-time public employees in police protection. These results suggest that patronage considerations drive the difference for all full-time public employees, but for public employees in police protection (both full- and part-time) there is an additional effect described in Vlaicu (2008).

The set of covariates in the benchmark specification does not include the share of privatized public services, since the privatization of public services is itself a choice variable for the chief executives (Levine and Tadelis, 2008) that can be affected by patronage considerations. I check whether the results are robust to controlling for the extent of privatization. All the results preserve the same level of significance and remain quantitatively similar after including the share of privatized public services in the set of controls. In the estimation without community fixed-effects the effect becomes stronger for both full- and part-time employees, whereas the difference between them becomes somewhat smaller. There is no noticeable difference in the results with community fixed-effects.

The results for the effect of privatization on the total number of employees are

18

robust to the inclusion of community-service fixed effects. The results for the effect of privatization of a public service on public employment in other services lose their significance in this case. The same results, however, along with the results for the effect of privatization of a public service on public employment in the same services, prove to be robust to the inclusion of the community-year fixed effects.

Overall, the estimates prove to be robust to using alternative estimators and additional controls.

5.7   Discussion of the results

Overall, the results provide strong support for the claim that the level of targeted redistribution through public employment is lower for appointed chief executives as compared to their elected counterparts. The most straightforward result is that the number of full-time employees is lower in communities with appointed chief executive. However, this result is subject to alternative explanations and does not show the mechanism that leads to this difference.

The results for part-time employees, political cycles, and the effect of privatization address the issue of potential alternative explanations. First, I rule out all alternative explanations that predict similar effects for full-time and part-time employees, by showing that the number of part-time workers does not depend on the form of government or whether it is an election year or not. These results, however, are fully consistent with the explanation proposed in this paper, since part-time employees do not play an important role in targeted redistribution. Second, I rule out explanations that predict a stable difference in the level of public employment between elected and appointed chief executives, by showing that the difference increases during the electoral years. These results indicate that extra public workers are especially important during electoral campaigns, which suggests that patronage is indeed used for attracting votes. Third, I show that elected politicians not only are less likely to privatize provision of public services (Levine and Tadelis, 2008), but even if they do privatize, they accomplish it without a corresponding decrease in the number of public employees. Taken together, these results provide evidence in favor of the claim that in U.S. local governments public employment is used for targeted redistribution and that the extent of targeted redistribution is higher in local governments headed by elected public officials.

The last set of results is the most important for the main argument of the paper, since it provides evidence on the mechanism that causes appointed and elected chief executives to choose different level of employment. The results show that there is a positive relationship between the number of full-time employees and the age of appointed chief executive, but there is no association between the the number of public employees and the age of elected chief executive. The results are fully consistent

with the notion that career concerns are important for appointed bureaucrats, but not for elected politicians. The younger the bureaucrats, the stronger their career concerns and the lower the level of targeted redistribution. For elected politicians career concerns are relatively weak, so that the level of targeted redistribution does not depend on their age.

One potential drawback of the analysis is that I cannot fully control for the endogeneity of the choice of the form of government. As long as the factors that affect the choice of the form of government also affect public employment, this can bias the results of the comparison of the level of public employment. By restricting the sample to communities I take into account one such factor: among smaller communities only the wealthiest can afford hiring a city manager. In addition, it is less apparent how endogeneity of the form of government can drive the results for political cycles, effect of privatization, and age of chief executives.

6   Conclusions

This paper compares the policies of elected versus appointed public officials with regard to targeted redistribution. The results indicate that appointed public officials are less likely to engage in inefficient targeted redistribution because they have stronger career concerns. Elected politicians in U.S. cities are more likely to use excessive public employment for targeted redistribution than their appointed counterparts. Moreover, the extent to which appointed public officials are engaged in this type of targeted redistribution depends on their age: the younger a bureaucrat, the stronger are her career concerns, and the lower the level of targeted redistribution.

These results might be generalized to other settings in which public officials can use inefficient policies to gain popular support that would help them to stay in office. However, the argument in this paper is more relevant for the top ranked than for lower level public officials. First, those at the top are more likely to have opportunities to be engaged in targeted redistribution. Second, the incentives of the lower-level public officials are likely to be systematically different and to be much more strongly affected by their relationships with immediate superiors in the party or bureaucratic organization. In addition, the argument is most relevant for the case in which bureaucrats expect to continue their career in a related line of business, so that their professional reputation is an important consideration.

In the literature there is an ongoing discussion of the trade-off between electing and appointing public officials. There are many theoretical arguments for and against each of the options. However, the empirical evidence to support these arguments remains scarce. Besley and Coate (2003) have shown that appointed bureaucrats are more likely to be influenced by special interests than elected public

officials. I add to their results by providing empirical evidence in favor of appointed public officials, showing that they are less likely to be engaged in inefficient targeted redistribution. Together, these results suggest that, in each particular situation, the choice of elected versus appointed public officials can depend on the relative importance of the inefficiencies caused by subversion of public policies by special interests and by targeted redistribution.

Data Appendix

Data sources

The data on the form of local government (including counties, municipalities and townships) comes from the 1987 Census of Governments, Government Organization File by the U.S. Bureau of Census; Municipal Form of Government survey conducted by International City/County Management Association (IMCA) in 2001; and Profile of Local Government Service Delivery Choices survey conducted by IMCA in 1997 and 2002. The data on the mode of provision of public services by local governments comes from the 1987 and 2002 Census of Governments, Government Organization Files by the U.S. Bureau of Census and Profile of Local Government Service Delivery Choices survey conducted by IMCA in 1997 and 2002. The data on public employment and budgetary data come from the Employment Statistics and Finance Statistics parts of the Census of Governments for the years 1987, 1997 and 2002. The data on economic and social characteristics of municipalities and counties comes from the Census of Population and Housing for the years 1980, 1990 and 2000. I exclude from the analysis communities with zero median household income. Two communities (Holmedel, NJ and Spokane, WA) were excluded from the dataset due to mistakes in the reported form of government. The final dataset is constructed by merging the databases using unique FIPS places' and MCD's Census codes. A small number of cases were merged manually due to typos in the original datasets.

Variables construction

The surveys distinguish five forms of municipal government (mayor-council, council-manager, commission, town meeting and representative town meeting) and three forms of county government (commission, council-administrator and council-elected executive). Counties with council-elected executives and municipalities with mayor-council forms of government are considered as having an elected executive. Counties with council-administrator and municipalities with council-manager forms of government are considered as having an appointed executive. Other forms of government are excluded from the analysis. With respect to the form of government in the Municipal Form of Government survey, I refer to the variable imis_FOG in the database rather than the response to Question 1 in the survey based on the suggestion by IMCA staff. I check robustness of the results using the response to Question 1 in the survey instead and find no difference in the results.

The surveys that provide information on the method of provision of public services ask whether a particular public service is provided by the local government and what the mode of provision of the public service is. To describe the method of provision of public services, I construct a dummy variable which is equal to one if

the public service is contracted out. For public services that are not provided the variable is missing. For observations from the Profile of Local Government Service Delivery Choices survey it equals one if the service is said to be provided only by "Private for profit", and zero otherwise.

Since data on the method of provision of public services come from different surveys, I check for the comparability of these measures. For the year 2002 there is some intersection between the Census of Governments and Profile of Local Government Service Delivery Choices surveys which allows for such examination. The intersection for the variable that describes the method of provision of public services contains 635 observations for 134 local governments. The correlation between the measures from the two datasets is 0.21 (significant at 0.01 level). In the final dataset, which contains observation only for the local governments for which the variable describing the form of the government is not missing the intersections contains 470 observations for 92 local governments and the correlation is 0.18 (significant at 0.01 level). The fact that I use dummy variables to describe the method of provision of public services can explain relatively low levels of correlation. Thus, the alternative measures of the methods of provision of public services provided by independent surveys are robustly correlated and may be used jointly in our empirical investigation.

In separating the employment by functions education services include all elementary and secondary education, as well as libraries; health and social services include health, hospitals and welfare; environmental and housing includes housing and community development, natural resources, parks and recreation, sewerage, and solid waste management; government administration includes financial administration, judicial and legal, and other government administration; transportation and transit includes airports, streets and highways, water transport and terminals, and transit; utilities include water supply, electric power and gas supply.

Coverage

The data for the year 1987 is based on information from Census of Governments, Government Organization File which contains information on all counties, municipal and township governments (38932 observations). Some of the observations do not contain all necessary information, but overall the sample can be considered as representative.

The data for the year 1997 comes from the Profile of Local Government Service Delivery Choices survey. According to ICMA "The [1997] Profile in Local Government Service Delivery Choice surveys were mailed in Fall 1997 and Spring 1998 to the Chief Administrative Officers in municipalities with populations 10,000 and over and to the Chief Administrative Officers of counties with populations 25,000.

In addition, a random sample of one in eight municipalities from 2,500 to 9,999 in population and one in eight counties with populations 2,500 to 24,999 and from those local governments under 2,500 that are recognized by ICMA. Of the 4,952 municipalities and counties that received surveys, 1,586 responded (32.0%)." Thus, the sample is biased towards large local governments and might not be representative for the municipalities with populations under 2,500.

The data for the year 2002 comes both from the Profile of Local Government Service Delivery Choices survey and Census of Governments with information from the Census of Governments being limited by the availability of information on the form of local government, which comes from the Municipal Form of Government survey. According to ICMA "The [2002] Profile in Local Government Service Delivery Choices survey was conducted in Fall 2002 and Spring 2003. Letters were mailed to the Chief Administrative Officers in municipalities with populations 10,000 and over, in counties with populations 25,000 and over, and to a random sample of one in eight municipalities from 2,500 to 9,999 in population and one in eight counties with populations from 2,500 to 24,999. Of the 5,370 municipalities and counties that received surveys, 1,283 responded (23.9%)." "The 2001 Municipal Form of Government surveys were mailed in Summer 2001 and Winter 2002 to the Municipal Clerks in municipalities with populations 2,500 and over and to those municipalities under 2,500 in population that are in ICMA's database. Of the 7,867 municipalities that received surveys, 4,244 responded (54.0%)." Thus, for the year 2002 the sample is also biased towards large local governments and might not be representative for the municipalities with populations under 2,500. There is also some evidence that the sample is biased towards richer communities. Trebbi, Aghion and Alesina (2007) analyze the difference between respondent and non-respondent municipalities in the 2001 Municipal Form of Government survey. They show that respondent and non-respondent municipalities are not significantly different in terms of ethnic division and total population, but median income in the non-respondent municipalities is somewhat lower with the difference being statistically significant.

Overall, the data for 1987 can be considered representative, whereas for 1997 and 2002 the sample is biased towards larger communities and may be considered as representative of the population of relatively large communities (with populations above 2500 inhabitants) and less representative of the population of smaller communities. There is also some evidence that the sample is somewhat biased towards richer communities.

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26

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Table 1. Subsequent occupation of former mayors and city managers.

Occupation	City Managers		Mayors	
	Number	Percent	Number	Percent
Public office:	112	91.1	22	31.9
City manager	98	79.7	0	0.00
Other city office	2	1.6	8	11.6
County office	6	4.9	4	5.8
State office	1	0.8	5	7.2
Federal office	0	0.0	3	4.3
Other public office	5	4.1	2	2.9
Self-employed	0	0.0	9	13.0
Other private	7	5.7	21	30.4
Non-profit	0	0.0	5	7.2
Jail	0	0.0	2	2.9
Retired	4	3.2	8	11.6
Died	0	0.00	2	2.9
Total	123	100	69	100

The data for city managers and mayors includes information only for the municipalities with council-manager and mayor-council forms of government respectively.

Table 2. Summary statistics.

	Appointed Chief Executive			Elected Chief Executive		
	N	Mean	Std.Dev.	N	Mean	Std.Dev.
ln (Number of Full-time Employees)	1697	6.43	1.16	838	6.68	1.37
ln (Number of Part-time Employees)	1697	4.75	1.30	838	4.79	1.52
Age of chief executive	352	48.17	7.41	270	53.49	10.36
Tenure of chief executive	352	3.72	3.26	270	6.03	6.44
ln(Population)	1711	11.22	0.82	840	11.33	0.98
ln(Per Capita Income)	1711	9.43	0.54	840	9.19	0.50
ln(Expenditures)	1682	11.25	1.11	832	11.26	1.49
Urban	1711	0.84	0.31	840	0.78	0.36
Ethnic Fractionalization	1711	0.34	0.18	840	0.27	0.18
Inequality	1711	1.23	0.11	840	1.21	0.10
Unemployment	1711	0.06	0.02	840	0.07	0.03
Population over 65	1711	0.12	0.05	840	0.12	0.04
High School Graduates	1711	0.72	0.13	840	0.68	0.12
Years since Incorporation	1606	116.80	59.51	774	142.60	65.13
Homerule	1662	0.40	0.49	836	0.39	0.49
Privatized	1436	0.14	0.19	747	0.11	0.19

Table 3. Changes in the form of local government across time.

	Total	
	Number	Percent
More than one observation per community:	764	49.42
Switched from Elected to Appointed	55	3.56
Switched from Appointed to Elected	47	3.04
Stayed Elected	147	9.51
Stayed Appointed	515	33.31
One observation per community:	782	50.58
Elected	419	27.1
Appointed	363	23.48
Total	1546	100

Table 4. Effect of the Form of Government on the Number of Employees.

Panel A	ln(Full-Time Employees)					
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.068	0.063	0.049	0.044		
	[0.036]*	[0.018]***	[0.023]**	[0.024]*		
Became Elected After Being Appointed					0.088	0.088
					[0.039]**	[0.039]**
Elected Before Becoming Appointed					0.010	0.001
					[0.027]	[0.031]
ln(Expenditures)		0.768		0.054		0.055
		[0.025]***		[0.162]		[0.163]
Year dummies	No	No	Yes	Yes	Yes	Yes
State-Year dummies	Yes	Yes	No	No	No	No
Community fixed effects	No	No	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2378	2358	1657	1646	1657	1646
Number of communities	1422	1411	653	653	653	653
R-squared	0.84	0.94	0.36	0.36	0.36	0.36

Panel B	ln(Part-Time Employees)					
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	-0.043	-0.035	0.014	-0.015		
	[0.066]	[0.062]	[0.105]	[0.105]		
Became Elected After Being Appointed					-0.105	-0.108
					[0.156]	[0.155]
Elected Before Becoming Appointed					0.135	0.079
					[0.152]	[0.152]
ln(Expenditures)				0.600		0.598
				[0.124]***		[0.124]***
Year dummies	No	No	Yes	Yes	Yes	Yes
State-Year dummies	Yes	Yes	No	No	No	No
Community fixed effects	No	No	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2378	2358	1657	1646	1657	1646
Number of communities	1422	1411	653	653	653	653
R-squared	0.52	0.58	0.10	0.13	0.10	0.13

Robust standard errors clustered at the community level in parenthesis. Community controls include logarithm of population, logarithm of income per capita, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of population above 25 years old with at least 12 years of education, and fraction of urban population. In specifications (1) and (2) community controls in addition include the number of years since incorporation, a dummy variable for homerule community, and a dummy variable for the type of local government.. R-squared does not account for the contribution of community fixed effects. Only communities with population above 30,000 are included. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. Electoral Cycles in the Number of Public Employees by Form of Government.

Panel A	ln(Full-Time Employees)			
	All Public Services		Excluding Police Protection	
	(1)	(2)	(3)	(4)
Elected*Year of Election	0.018	0.018	0.020	0.020
	[0.007]**	[0.007]**	[0.008]**	[0.008]**
Elected	0.059	0.057	0.077	0.075
	[0.028]**	[0.026]**	[0.039]**	[0.039]*
Year of Election	-0.004	-0.005	-0.005	-0.005
	[0.002]*	[0.002]**	[0.003]	[0.003]*
ln(Expenditures)		0.224		0.279
		[0.050]***		[0.061]***
Year dummies	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes
Community Controls	Yes	Yes	Yes	Yes
Observations	2873	2873	2873	2873
Number of communities	196	196	196	196
R-squared	0.24	0.29	0.19	0.24

Panel B	ln(Part-Time Employees)			
	All Public Services		Excluding Police Protection	
	(1)	(2)	(3)	(4)
Elected*Year of Election	0.038	0.038	-0.004	-0.004
	[0.033]	[0.033]	[0.039]	[0.040]
Elected	-0.572	-0.578	-0.574	-0.581
	[0.460]	[0.469]	[0.444]	[0.450]
Year of Election	-0.026	-0.028	-0.028	-0.030
	[0.021]	[0.021]	[0.024]	[0.024]
ln(Expenditures)		0.585		0.679
		[0.228]**		[0.207]***
Year dummies	Yes	Yes	Yes	Yes
Community fixed effects	Yes	Yes	Yes	Yes
Community Controls	Yes	Yes	Yes	Yes
Observations	2873	2873	2873	2873
Number of communities	196	196	196	196
R-squared	0.04	0.04	0.03	0.04

Robust standard errors clustered at the community level in parenthesis. Community controls include population, income per capita, and taxes per capita (all in logarithms). R-squared does not account for the contribution of community fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Effect of Privatization on Public Employment.

Panel A	ln(Full-Time Employees)					
	Same Service		Other Services		Total	
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized*Elected	-1.190	-1.184	0.038	0.041	0.037	0.090
	[0.115]***	[0.115]***	[0.008]***	[0.008]***	[0.061]	[0.060]
Privatized*Appointed	-1.308	-1.301	0.034	0.036	-0.064	-0.077
	[0.078]***	[0.078]***	[0.007]***	[0.007]***	[0.046]	[0.039]**
ln(Public Expenditures)		0.094		0.281		0.266
		[0.128]		[0.056]***		[0.048]***
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Community dummies	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Public service dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9579	9522	9579	9522	1975	1962
Number of communities	1333	1326	1332	1326	1350	1343
R-squared	0.40	0.40	0.34	0.37	0.38	0.43

Panel B	ln(Part-Time Employees)					
	Same Service		Other Services		Total	
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized*Elected	-0.376	-0.380	0.006	0.01	-0.426	-0.431
	[0.085]***	[0.086]***	[0.053]	[0.048]	[0.575]	[0.574]
Privatized*Appointed	-0.634	-0.629	0.058	0.063	0.073	0.032
	[0.055]***	[0.055]***	[0.022]***	[0.022]***	[0.176]	[0.168]
ln(Public Expenditures)		0.172		0.627		0.729
		[0.097]*		[0.178]***		[0.174]***
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Community dummies	Yes	Yes	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes	Yes	Yes
Public service dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9579	9522	9579	9522	1975	1962
Number of communities	1333	1326	1333	1325	1349	1342
R-squared	0.38	0.38	0.13	0.15	0.13	0.17

Only communities with population above 30,000 and constant form of government are included. In specifications (1) –(4) the unit of observation is service-community-year. In specifications (5) and (6) the unit of observation is community-year. Community controls include logarithm of population, logarithm of income per capita, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of population above 25 years old with at least 12 years of education, and fraction of urban population. In (5) and (6) community controls in addition include the number of public services with non-zero employment. Robust standard errors clustered on community level in brackets. Errors are. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7. Effect of the Age of Chief Executive on the Number of Full-Time Public Employees by Form of Government.

Panel A	ln(Full-Time Employees)			
	(1)	(2)	(3)	(4)
Age of Appointed Chief Executive	0.007	0.007		
	[0.003]**	[0.003]**		
Age of Elected Chief Executive			-0.003	-0.003
			[0.004]	[0.004]
ln(Expenditures)	0.657	0.657	0.899	0.897
	[0.066]***	[0.066]***	[0.075]***	[0.075]***
Tenure in Office		0.002		-0.003
		[0.007]		[0.005]
Community controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	341	341	210	210
Number of communities	260	260	126	126
R-squared	0.86	0.86	0.96	0.96

Panel B	ln(Part-Time Employees)			
	(1)	(2)	(3)	(4)
Age of Appointed Chief Executive	-0.005	-0.006		
	[0.011]	[0.011]		
Age of Elected Chief Executive			0.004	0.008
			[0.009]	[0.010]
ln(Expenditures)	0.546	0.545	0.738	0.728
	[0.112]***	[0.112]***	[0.097]***	[0.097]***
Tenure in Office		0.005		-0.019
		[0.018]		[0.013]
Community controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	341	341	210	210
Number of communities	260	260	126	126
R-squared	0.39	0.39	0.60	0.60

Community controls include logarithm of population, logarithm of income per capita, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of population above 25 years old with at least 12 years of education, and fraction of urban population. Robust standard errors clustered at the community level in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table AI. Description of the variables.

Variable	Description
Elected	Dummy variable that equals one for counties with council-elected executive and municipalities with mayor-council form of government. It equals zero for counties with council-administrator and municipalities with council-manager forms of government. Missing for all other forms of government. With respect to the form of government in the Municipal Form of Government survey, I refer to the variable imis_FOG in the database rather than the response to Question 1 in the survey based on the suggestion by IMCA staff. *Source: Census of Governments (1987), Government Organization File; Municipal Form of Government (2001) and Profile of Local Government Service Delivery Choices (1997, 2002) surveys by International City/County Management Association (IMCA).*
Appointed	Dummy variable that equals one minus *Elected*.
Became Elected after Being Appointed	Dummy variable that equals one if the variable *Elected* equals one in the same period and zero in the previous period. Equals zero in all other cases when *Elected* is not missing.
Became Appointed after Being Elected	Dummy variable that equals one if the variable *Elected* equals zero in the same period and one in the previous period. Equals zero in all other cases when *Elected* is not missing.
Full-Time Employees	The number of full-time public employees. *Source: Census of Governments, Employment Statistics (1987, 1997 and 2002).*
Part-Time Employees	The number of part-time public employees. *Source: Census of Governments, Employment Statistics (1987, 1997 and 2002).*
Privatized	Dummy variable that equals one if the provision of public service is contracted out and zero if it is provided in-house or by other government. For public services that are not provided the variable is missing. For observations from the Profile of Local Government Service Delivery Choices survey it equals one if the service is said to be provided by "Private for profit" only and zero otherwise. *Source: Census of Governments (1987), Government Organization File; Profile of Local Government Service Delivery Choices (1997, 2002) survey by International City/County Management Association (ICMA).*
Share of privatized public services	Share of public services for which the variable "Privatized" (see above) equals one.
Age of City Manager	Age of city manager. *Source: Who is Who in Local Government Management database provided by ICMA.*
Age of Mayor	Age of mayor. *Source: Hand-collected from the web-sites of the local governments.*
Population	Total population. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Per Capita Income	Per capita income in dollars. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Expenditures	Total budgetary expenditures of the local government. *Source: Census of Governments, Finance Statistics (1987, 1997 and 2002).*
Inequality	Ratio of mean and median household income. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Unemployment	Civil labor force unemployment rate. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Ethnic Fractionalization	Probability that two people randomly drawn from a city will belong to different ethnic groups. I distinguish six ethnic groups: white, black, Hispanic, American Indians and Alaskan, Asian-Pacific, and others. *Source: Census of Population and Housing for (1980, 1990 and 2000).*

Variable	Description
Population over 65	Fraction of population 65 years or older. *Source: Census of Population and Housing (1980, 1990 and 2000).*
High School Graduates	Fraction of population above 25 years old with at least 12 years of education. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Urban	Fraction of urban population. *Source: Census of Population and Housing (1980, 1990 and 2000).*
Budget Deficit	Difference between government total expenditures and total revenues as a share of total expenditures. *Source: Census of Governments, Finance Statistics (1987, 1997 and 2002).*
Years Since Incorporation	Number of years since the incorporation of the local government. *Source: Census of Governments (1987), Government Organization File*
Homerule	Dummy variable that equals one for homerule local governments. *Source: Census of Governments (1987), Government Organization File*
County	Dummy variable that equals one for counties and zero for municipalities and townships.

Table AII. Estimates using nearest neighbor matching.

Number of Matches	1	3	5	7
	Ln(Full-time employees)			
All Years	0.112	0.112	0.112	0.112
	[4.01]***	[4.48]***	[4.56]***	[4.68]***
1987	0.080	0.091	0.088	0.090
	[2.36]**	[3.04]***	[3.09]***	[3.25]***
1997	0.138	0.134	0.129	0.128
	[2.53]**	[2.47]**	[2.39]**	[2.36]**
2002	0.144	0.154	0.160	0.157
	[2.44]**	[2.94]***	[3.09]***	[3.08]***
	Ln(Part-time employees)			
All Years	0.018	0.022	0.008	0.006
	[0.28]	[0.42]	[0.16]	[0.13]
1987	0.026	0.009	-0.004	-0.009
	[0.41]	[0.15]	[0.06]	[0.15]
1997	-0.109	-0.064	-0.049	-0.038
	[0.57]	[0.44]	[0.36]	[0.28]
2002	0.176	0.151	0.142	0.145
	[1.77]*	[1.77]*	[1.71]*	[1.75]*

Absolute value of robust t-statistics in brackets. The average treatment effect is reported. Covariates include measures of population, income per capita, total budgetary expenditures, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of population above 25 years old with at least 12 years of education, fraction of urban population, years since incorporation and dummy variables for homerule. Matching on year and type of community is exact. Only communities with population above 30,000 are included.* significant at 10%; ** significant at 5%; *** significant at 1%.

Table AIII. Effect of the Form of Government on the Number of Police and Non-Police Public Employees.

Panel A — Employees Excluding Police Protection

	ln(Full-Time Employees)			ln(Part-Time Employees)		
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.079	0.041		-0.047	-0.043	
	[0.021]***	[0.028]		[0.070]	[0.102]	
Became Elected After Being Appointed			0.080			-0.195
			[0.048]*			[0.159]
Elected Before Becoming Appointed			0.002			0.106
			[0.034]			[0.140]
Year dummies	No	Yes	Yes	No	Yes	Yes
State-Year dummies	Yes	No	No	Yes	No	No
Community fixed effects	No	Yes	Yes	No	Yes	Yes
Community controls	Yes	Yes	Yes	`Yes	Yes	Yes
Observations	2354	2509	2509	2354	2509	2509
Number of communities	1409	1518	1518	1409	1518	1518
R-squared	0.93	0.30	0.30	0.57	0.15	0.15

Panel B — Employees in Police Protection Only

	ln(Full-Time Employees)			ln(Part-Time Employees)		
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	-0.014	0.075		-0.072	0.238	
	[0.045]	[0.030]**		[0.086]	[0.134]*	
Became Elected After Being Appointed			0.143			0.310
			[0.043]***			[0.185]*
Elected Before Becoming Appointed			0.009			0.167
			[0.043]			[0.214]
Year dummies	No	Yes	Yes	No	Yes	Yes
State-Year dummies	Yes	No	No	Yes	No	No
Community fixed effects	No	Yes	Yes	No	Yes	Yes
Community controls	Yes	No	No	Yes	No	No
Observations	2354	2509	2509	2354	2509	2509
Number of communities	1409	1518	1518	1409	1518	1518
R-squared	0.68	0.26	0.26	0.33	0.03	0.03

Robust standard errors clustered at the community level in parenthesis. Community controls in all regressions include logarithm of population, logarithm income per capita, logarithm total budgetary expenditures, level of unemployment, ethnic fractionalization, fraction of population 65 years and older, fraction of high school graduates, fraction of urban population. Regressions (1) and (4) also include years since incorporation and dummy variables for county and homerule. R-squared does not account for the contribution of community fixed effects. Only communities with population above 30,000 are included. * significant at 10%; ** significant at 5%; *** significant at 1%.