Public employees as politicians: evidence from close elections

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Abstract

We analyze the effect of municipal employees' political representation in municipal councils on local public spending. We use within-party, as-good-as random variation in close elections in the Finnish open-list proportional election system to quantify the effect. One more councilor employed by the public sector increases spending by about one percent. The effect comes largely through the largest party and is specific to the employment sector of the municipal employee. The results are consistent with public employees having an information advantage over other politicians, and thus, being able to influence policy.

Keywords: Close elections, political representation, public employees, public expenditures

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Introduction

In 2013, public sector employees accounted on average for 21% of total employment in the OECD countries (OECD 2015). While heterogeneous, they are a large group that share an interest in sustaining public employment and that can influence politics in various ways. In addition to a direct voting channel (Garand 1988; Blais, Blake, and Dion 1990; Bhatti and Hansen 2012), recent research has emphasized the role of public sector unions and their effects on the cost of government, either directly through collective bargaining or indirectly through politics (Sieg and Wang 2013; Anzia and Moe 2015).

Quite often public sector employees are also politicians themselves.[1] This dual role of public sector employees has raised the concern that when elected, they may be in a better position to extract rents from holding the office than otherwise similar politicians. A concrete example would be a teacher in a municipal council that decides whether the teacher's school should be closed or not, or a public sector nurse participating in deciding on budget cuts in the local public health care sector. In both cases, the public sector employees can possibly exert disproportionate influence in the council due to their information advantage over the other councilors on the true costs and benefits of providing public services in their sector of employment (Niskanen 1971; Romer and Rosenthal 1979). This disproportionate influence may compromise the political neutrality of public service and also undermine the separation powers more generally (Braendle and Stutzer 2016).

[1] For example, Braendle and Stutzer (2016) report that in their sample of 76 countries the average fraction of politicians in national parliaments with a public sector background is 31.3%.

Consistent with such concerns, countries often impose incompatibility and to a lesser extent ineligibility rules on the political involvement of public sector employees.[2] The former force public employees to give up public service if elected and the latter require giving up public service if they run (Braendle and Stutzer 2016). Imposing such restrictions involves a trade-off by limiting the political participation of a group with possibly ample opportunities for rent-seeking at the cost of discriminating against a large citizen group and excluding informed candidates.[3] There is surprisingly little evidence on whether public sector employees would act differently from the other politicians when elected. We start to fill this important gap in the literature by providing causal evidence on how municipal employee representation in a municipal council affects local public spending and on the mechanisms at work.[4]

We use data from Finland. Finland provides a particularly interesting context for our analysis for two reasons. First, almost 30% of employment in Finland is in the public sector and more than 20% of employment is in the local public sector.[5] An important feature of Finnish local politics, and common in other countries as well (e.g. the UK), is that being a municipal councilor is not a full-time job. About one quarter of the Finnish local politicians

[2] Prominent examples include the Hatch Act of 1939 in the US and the House of Commons Disqualification Act of 1975 in the UK. The Local Government Act of 1972 and the Local Government and Housing Act 1989 include similar restrictions for local government employees in the UK. See Braendle and Stutzer (2016) for examples from other countries.

[3] Braendle and Stutzer (2010, 2016) show using German and cross-country data, respectively, that stricter ineligibility and incompatibility rules decrease the share of public servants in parliaments. Rosenson (2006) finds a connection between various ethics laws and representation of occupations. Braendle (2016) reviews the effects of institutions and eligibility rules on political selection.

[4] Prior analyses closest to ours are Braendle and Stutzer (2013, 2016), but neither focuses on estimating causal effects. For example, Braendle and Stutzer (2016) find using cross-country data a positive association between government size and the share of public servants in parliament.

[5] Source: Statistics Finland Labor Force Survey 2015.

work for a municipality.[6] The distribution of power between private and public sector employees in the municipal councils may therefore have a large impact on the size and efficiency of the local public sector. Reflecting this tension and its topicality, the Finnish media has expressed concerns that when elected, municipal employees can make decisions on their own jobs in municipal councils.[7]

The second reason why Finland provides a suitable context for our study is that the Finnish open-list local elections provide us with plausibly exogenous variation in municipal employee representation. The source of this variation is candidate-level close contests within party lists. We use these contests to construct a municipality-level instrument variable for municipal employee representation. Our instrument captures the extent to which the seat share of municipal employees exceeds or falls short of their expected share due to randomness in the outcomes of the close elections. The identifying assumption is that when measured at the candidate level and sufficiently close to the within party election thresholds, the seat allocation between municipal employees and other candidates can be considered to be as-good-as random. This assumption can be tested indirectly by covariate balance tests. We define candidate-level closeness within the party lists to make sure that differences in party representation (party effects) are not driving the results.

Our main result is that electing one additional municipal employee to a council as opposed to a candidate from the same party, but not employed by the public sector, increases local public spending. Our estimates suggest that in a municipality with a median-sized council (27 seats), the spending increases on average by about 1 percent over the

[6] In Finland, municipal employees are eligible to run for a council seat and can hold on to their municipal job if elected. There are, however, other restrictions (see section *Institutional Setting and Data*).

[7] For example, the Finnish National Broadcasting company YLE expressed at the time of the 2012 municipal council elections the concern that municipal employees can decide on their own jobs in municipal councils.

four-year council term. The effect is surprisingly large considering two features: First, we are probably looking at a relatively unimportant margin, i.e., the last elected candidate within a party to a council that typically consists of tens of councilors. Second, as we explain later, there are explicit restrictions on the types of political positions that Finnish municipal employees can take in their home municipality. Our result is nevertheless in line with previous findings which show that smaller parties and even individual councilors have an effect on policy in proportional representation systems (Folke 2014; Freier and Odendahl 2015; Fiva and Halse 2016).[8]

We also provide evidence on the mechanisms at work. First, we show that the effect varies by the type of municipal employee and the type of spending: electing one more employee who works in health care leads to a significant increase in health expenditures, but not in the other (non-health) municipal expenditures. Similarly, when a non-health care employee gets elected, expenditures unrelated to health care increase.[9] This evidence is consistent with Niskanen's (1971) classic bureaucracy model which predicts that bureaucrats can convince politicians to increase public spending due to their information advantage over politicians. The analogy we draw is that municipal employee politicians have both different incentives than and information advantage over the other politicians. Moreover, we find that the positive effect on local public spending arises in particular in close elections that involve the largest party in the municipality and in smaller councils.

[8] For studies on party effects in the U.S. context, see Ferreira and Gyourko (2009), Gerber and Hopkins (2011) and de Benedictis-Kessner and Warshaw (2016). The effects of the political representation of other non-partisan interests groups, such as women, minorities and occupation groups, on policy outcomes are studied by, for instance, Pande (2003), Chattopadhyay and Duflo (2004), Gehlbach, Sonin, and Zhuravskaya (2010), Ferreira and Gyourko (2014), Matter and Stutzer (2015) and Bagues and Campa (2017). Gagliarducci and Nannicini (2013) and Freier and Thomasius (2016) study the effects of politicians' qualifications on fiscal outcomes.

[9] Data limitations prevent us from analyzing occupation groups in more detail.

This evidence suggests that municipal employee councilors influence outcomes through intra-party decision making (Laver and Shepsle 1990).

The increased sector-specific spending cannot be automatically attributed to rent-seeking. A reason for this is that municipal employees are experts in their area of employment and can therefore provide useful information to other councilors and improve decision-making.[10] Even though we cannot conclusively differentiate between the competing hypotheses about the efficiency of the increased spending, it is definitely noteworthy – and somewhat puzzling – that the Finnish municipal councilors employed by the public sector want to increase public expenditures in a country that in 2014 had, at 59% (OECD 2015), the highest public sector ratio to GDP among all OECD countries (during 1996-2012 Finland's position varied between 2nd and 8th). The uniform increase in spending is puzzling because our as-good-as-random design guarantees that the citizens' needs are identical on average in the treated and other municipalities. Viewed from this angle, Niskanen's (1971) concerns about bureaucrats' information advantage leading to excessive spending seem warranted.

The rest of the paper is organized as follows. In the following section, we discuss relevant theory and outline testable hypotheses. Then, we describe the institutional setting and data. We present our econometric identification strategy in the fourth section and the results in the fifth section. The final section concludes the study. Auxiliary results are available in the Online Appendices.

[10] We are unable to find systematic evidence for the extra spending being related to rents that the politicians employed by the municipalities get from holding the office (through better employment opportunities, or greater wages; see Dahlberg and Mörk 2006 and Brueckner and Neumark 2014). Neither do we find evidence that the increased spending reflects pro-social behavior or competence of public sector employees (Best and Cotta 2000; Francois 2000; Besley and Ghatak 2003, 2006).

Theory and Hypotheses

We are interested in i) whether, ii) through which mechanisms and iii) why public employee representation in municipal councils has an effect on municipal spending. We discuss each of these in turn.

Effect on total expenditures: At least two distinct theoretical debates bear directly on whether public employee representation has an effect on local government expenditures. The first view is that public employees have both the economic incentives and the means to maximize the municipal budget to their own benefit (Niskanen 1971; Courant, Gramlich, and Rubinfield 1979; Dahlberg and Mörk 2006). This is likely to obfuscate the separation of powers between the executive and the democratically-chosen political branches of the local government (Braendle and Stutzer 2016). A public sector employee politician may also have a variety of ways to target public spending to certain voters, such as her own political constituency or interest group (Alesina, Baqir, and Easterly 2000). The second view posits that the preferences of public sector employees differ systematically from the individuals employed by the private sector. One reason for the difference is that professional background determines socio-economic conditions and may thus shape identity (Braendle and Stutzer 2016). Consistent with this, public sector employees seem to be politically more active (Bhatti and Hansen 2012) and lean more to the left ideologically (Knutsen 2005; Jensen, Sum, and Flynn 2009; Rattsø and Sørensen 2016). They may also be relatively unwilling to support market-oriented solutions, and thus, a smaller public sector.

Taken together, these views suggest that public sector employees have a tendency to favor a larger public sector. We therefore formulate the following hypothesis:

Hypothesis 1: *The council seat share of municipal employees increases municipal spending.*

We acknowledge the possibility that municipal employees may be more pro-social and better motivated for public service than other candidates from the same party (Francois 2000; Besley and Ghatak 2003, 2006). If that was the case, spending could also decrease (remain unchanged) if the status quo level of per capita spending is too high (optimal) from the social point of view. Municipal employees may also be relatively immune to specific business interests and lobbying (Braendle and Stutzer 2016), which may reduce inefficient spending to public procurement.

Mechanisms at work: How and through which mechanisms could the spending effect come about? This question is of interest, because there are institutional restrictions on the political representation of the municipal employees (see the next section) and because our empirical close-elections approach identifies by design the effect of allocating the last marginal seats to the council. Two key mechanisms suggest themselves: First, holding other things constant, an individual councilor is likely to exert a greater impact in smaller councils because the likelihood of him (or his party) being pivotal is higher. In a smaller council, a single councilor can also pivot informal within-council discussions to his own advantage and influence which issues the council tackles. Second, the literature on coalitional bargaining (Laver and Shepsle 1990) suggests that councilors can influence decision making either between-parties or within-parties. In the former case, the municipal employees would vote in the council as if they had a coalition of their own, independent of the formal parties and the municipal employees' party affiliation. If, on the other hand, the channel of influence is within-parties, the party lines hold, but municipal employee councilors affect the policy position of their own party. This is a plausible channel of influence in our context because public employees may be a relatively loose and

heterogeneous interest group. Moreover, the within-party channel matters for policy outcomes only if the public employees' party is large and powerful enough in the council. In sum, we have the following two hypotheses about the mechanisms of influence:

Hypothesis 2a: *Municipal employees of smaller councils have a greater effect on spending than those of larger councils.*

Hypothesis 2b: *Municipal employees of larger parties have a greater effect on spending than those of smaller parties.*

Rent-seeking vs. efficient provision of public services: Finally, we study whether the effect of public employee representation on public spending mirrors rent-seeking, or is more consistent with efficient provision of public services.

Applied to our context, Niskanen's (1971) classic model of bureaucracy predicts that municipal employee councilors have an information advantage over the other municipal councilors about the provision of public services in their own employment sector and that the municipal employees are less likely to have such an advantage over the other public services. We therefore formulate:

Hypothesis 3a: *Municipal employees never decrease spending and they increase spending especially in their own sector of employment.*

This hypothesis would not get support from the data, if municipal employees lean more to the left and *generically* favor a larger public sector. In this case, municipal employees ought to increase spending also in sectors other than their own sector of employment. Moreover, while the expertise of municipal employees can also be useful for the efficient provision of public services (Braendle and Stutzer 2016), it is unlikely that, holding citizens' needs constant, such efficient provision systematically calls for *greater* spending, especially only in their own sector.

Finally, we also look for more direct signs of rent-seeking (Svaleryd and Vlachos 2009). Inefficiencies may arise also through clientelistic behavior (i.e., explicit or implicit *quid-pro-quo* for political support; see Alesina, Baqir, and Easterly 2000), which are at least partly captured through better re-election prospects. We therefore formulate:

Hypothesis 3b: *Municipal employees enjoy higher returns to office in terms of larger salary and smaller unemployment risk, and/or enjoy from a larger incumbency advantage in subsequent elections than the other candidates.*

Institutional Setting and Data

Finnish Local Governments

Tasks and revenue sources of municipalities: Finland has a two-tier system of government consisting of the central government and municipalities as the local level (see Saarimaa and Tukiainen 2015). Finnish municipalities have extensive tasks. In addition to the usual local public goods and services, municipalities are responsible for providing most of social and health care services and primary and secondary education. The GDP share of municipality spending is large (roughly 18 percent) and the municipalities employ around 20 percent of the total workforce.

Municipalities have extensive fiscal autonomy. The most important revenue source is the flat local income tax, determined by the municipalities. There are, however, large regional tax base and cost disparities. They are offset by a central government grant system, which the municipalities cannot effectively manipulate to their advantage.

Decision-making and elections in municipalities: Municipalities are governed by a municipality council which is the most important political actor.[11] For example, mayors or city managers are public officials chosen by the councils and have only executive power and no political power. Moreover, municipal boards (i.e., cabinets) have only a preparatory role and the representation in the boards follows the political distribution of the council.

Municipal elections are held simultaneously in all municipalities and each municipality has one electoral district. The elections in our data were held on the fourth Sunday of October in 1996, 2000, 2004 and 2008. The council term starts in January of the year following the election year. The term lasts four years.

Within each municipality, the allocation of seats is based on proportional representation, as determined by the open-list D'Hondt election rule. In an election, each candidate has an affiliation with a party list and each voter gives a single vote to a single candidate. The voters cannot vote for a party without specifying a candidate. The total number of votes for the candidates in a given party list determines the votes for each party. The party votes determine how many seats each party gets according to the D'Hondt rule. Given these party seats, the competition for the seats within parties is simply an *n*-past-the-post rule. The rank of a candidate within the party list is determined by his votes, implying that voters – as opposed to parties – decide which candidates are elected from a given party list.

[11] The Finnish law dictates that council size is a step function of population: 13, 15 or 17 for municipal population of 2000 or less, 21 for 2001–4000; 27 for 4001–8000; 35 for 8001–15,000; 43 for 15,001–30,000; 51 for 30,001–60,000; 59 for 60,001–120,000; 67 for 120,001–250,000; 75 for 250,001–400,000 and 85 for over 400,000.

There are nation-wide restrictions on the political roles of municipal employees.[12] First, a municipal employee who is in an executive position of a branch of public service cannot be a council member. For example, the director of a municipality's school authority cannot be a member of the municipal council. Second, a municipal employee cannot be a member of the sub-committee of his own specific sector. For example, a teacher cannot be a member of the sub-committee for education. Third, a municipal employee working in administrative duties directly under the municipal board cannot be a member of the board. Fourth, a municipal employee who is the presenting official for matters dealt by the municipal board cannot be a member of the board. Fifth, the majority of the municipal board cannot consist of municipal employees. Finally, a municipal employee councilor can participate in the decision making in the council meeting even if the matter relates to her own employment, unless she has been directly involved in preparing or presenting the matter as a bureaucrat for the council.

The broader institutional context may also limit the opportunities of the municipal employees to influence outcomes and extract rents while in office. For example, Finland is one of the least corrupted countries in the world. Moreover, wages are largely set at the national-level wage bargaining between the municipal employer organization and various labor unions. However, a municipality can pay more than agreed upon nationally.

[12] Most of Finnish local politicians have a normal day job. The task of being a municipal councilor typically takes a few hours a week and the monetary compensation involved is not nearly enough to live on. The same applies by and large, e.g., to the UK (Local Government Association 2012).

Data

Our data come from a number of sources:

Candidate and elections data: We have obtained data on municipal elections held between 1996 and 2008 from the Ministry of Justice. These data consist of candidate-level election results, in particular party affiliation, number of votes and elected status. The election data also include the age and gender of the candidates. Information on municipal employment status comes from KEVA, which manages local government pensions, and we have linked the candidate data also to Statistics Finland data on education, occupation and socio-economic status and to income data from the Finnish Tax Authority.

Overall we have roughly 160,000 candidate-election observations (see Appendix A for descriptive statistics and descriptions of sample restrictions). For our purposes, a candidate is a municipal employee, if she was employed by a municipality at the end of the election year. Compared to other candidates, municipal employees are more often female (nurse is the most common profession among them), classified as high professionals in terms of socioeconomic status and running for the Social Democratic Party. We return to these observable differences in candidate characteristics when we present our econometric analysis and results.

Municipal data: We use Statistics Finland's data on municipal expenditures and demographics for years 1996–2012. We have 1544 municipality-council term observations (see Appendix A for the summary statistics): On average, municipalities' total expenditures are 5500 euros per capita. The single most important expenditure category is health care (1,700 euros per capita). Municipal employees' seat share is on average 26.4%.

Econometric Approach

Identification Strategy

To estimate the effect of political representation of municipal employees on municipal policy, we use the following regression specification:

$$Y_{mt} = \delta M_{mt} + \boldsymbol{X}'_{mt}\boldsymbol{\beta} + u_{mt}, \qquad (1)$$

where Y_{mt} is the outcome of interest, M_{mt} is the seat share of municipal employees in the council, $\boldsymbol{X}'_{mt}$ is a vector of control variables (possibly lagged), and u_{mt} is the error term in municipality m at time t. The parameter of interest is δ which measures the effect of a change in the seat share of municipal employees on the outcome.

Our main outcome variable is municipal expenditures. A simple OLS estimation of equation (1) may suffer from both reverse causality and omitted variable bias. This could be the case if, e.g., voters in a municipality demand a high level of municipal services. Such a municipality would have a high number of municipal employees. This calls for greater municipal expenditures and would show up as a greater council seat share of public sector employees as well.

We make use of close elections to estimate the treatment effect of interest (δ). Unlike in much of the recent literature using close elections for identification, the Finnish municipal election system of proportional representation with open party lists does not render itself to a simple regression discontinuity design (RDD) analysis (Lee, Moretti, and Butler 2004). The reason is simple: Despite there being an RDD flavor to our close elections approach, we cannot construct a well-defined forcing variable at the municipality level. We therefore build on Clots-Figueras (2011, 2012) who uses the fraction of women

winning close elections as an instrument for the share of women in the legislature (see also Folke 2014 and Freier and Odendahl 2015, who use IV and close elections data). Our IV procedure uses as-good-as random variation at candidate-level in the close elections and aggregates this variation to get a municipality-level instrumental variable. To properly capture the treatment effect of political representation of municipal employees, we focus on closeness *within party lists*. This choice means that between-party differences do not confound our results. For example, if municipal employees are more often left- than right-wing, between party comparisons would give us the joint effect of municipal employees and party status.[13]

We construct our instrument in the following steps:

Step 1: For each party list p, we define the pivotal number of votes as the average of the maximum number of votes among the non-elected candidates and the minimum number of votes among the elected candidates. The distance to getting elected for each candidate is the number of votes of the candidate minus the pivotal number of votes of her party list. We normalize this distance by dividing it by the total number of votes of the party list and then multiplying it by 100. We denote the variable thus obtained v_{ipmt}.[14] Closeness of each candidate i in party list p in municipality m in election t, C_{ipmt}, is then defined as

$$C_{ipmt} = \begin{cases} 1 \text{ if } |v_{ipmt}| \leq \varepsilon \\ 0 \text{ if } |v_{ipmt}| > \varepsilon \end{cases}, \qquad (2)$$

[13] Using similar Finnish close elections data as we do, Kotakorpi, Poutvaara, and Terviö (2017) study returns to office and Hyytinen et al. (2017) study incumbency advantage and the performance of close elections RDD. Unlike these papers, we are interested in municipal level outcomes.

[14] Because v_{ipmt} cannot be defined for party lists where none of the candidates or all of the candidates get elected, approximately 4800 candidate-election observations are left out.

where ε is some small bandwidth, expressed in percentages (e.g., ε = 0.4 means "0.4%"; that is, 4 votes out of 1000). Due to randomness in the outcomes of elections, candidates just above and below the pivotal number do not differ systematically from each other. Indeed, when ε = 0 in our data, there was a tie within a party list between two (or more) candidates at the threshold of getting into the council. In such a case, a lottery decides which of the candidates are elected (see Hyytinen et al. 2017 for details). There are 1351 candidates who end up in these lotteries and 335 of them are municipal employees.

Step 2: Quasi-randomization taking place within each party list influences how many municipal employees get elected from each list. To capture this list-level variation, we calculate the difference between the realized outcome and the expected outcome of the close races within each party. Formally, this can expressed as

$$T_{pmt} = \left(\sum_i^{N_p} C_{ipmt} D_{ipmt}\, M_{ipmt}\right) - \left[\frac{\sum_i^{N_p} C_{ipmt} M_{ipmt}}{\sum_i^{N_p} C_{ipmt}} \sum_i^{N_p} C_{ipmt} D_{ipmt}\right], \quad (3)$$

where M_{ipmt} is equal to 1 if candidate i is a municipal employee and zero otherwise, D_{ipmt} equals 1 if candidate i in municipality m was elected in election t and zero otherwise and p refers to a party list and N_p to the number of candidates in the list p. The first term is the number of municipal employees that are elected in the close elections. The second term is the *expected* number of municipal employees who get elected in the close elections. The expected number comes from a hypergeometric distribution, because close elections can be seen as a basic urn problem.[15] The reason for using Eq. (3) is that there may be more than two candidates that are close and thus subject to randomization and any number of the close

[15] In an urn problem, the expected value is $n(K/N)$ with and without replacement, where n is the number of available close seats, K the number of close municipal employees and N the number of close candidates.

candidates can be municipal employees.[16] Moreover, the set of candidates defined as close may compete for more than one seat within the party list. These features are the main difference between our and Clots-Figueras' (2011, 2012) approach, because she considers only situations where one male and one female candidate compete for one seat.

Step 3: We aggregate the random variation at the party list-level to construct a municipal-level instrumental variable, T_{mt}. This is done by adding up T_{pmt} over all the party lists within a municipality and by dividing the sum by council size (*CS*):

$$T_{mt} = 100*(\sum_p T_{pmt})/CS_{mt}. \qquad (4)$$

Our instrument, T_{mt}, captures the extent to which the seat share of municipal employees exceeds ($T_{mt} > 0$) or falls short of ($T_{mt} < 0$) their expected share due to randomness in the outcomes of the close elections. In other words, the instrument obtains higher values for those municipalities in which the municipal employee candidates were lucky and smaller values for those municipalities in which they were unlucky. If, in a given municipality, municipal employees were lucky within one party list and equally unlucky in another, the instrument at the municipal-level would be zero. One can think of T_{mt} as the part of the variation in M_{mt} that is as-good-as random. Our IV approach thus assumes that T_{mt} is a determinant of M_{mt}, i.e., the (actual) seat share of municipal employees in the council and uncorrelated with u_{mt} in (1). This assumption can to an extent be tested using municipality-level covariate balance tests. Moreover, the candidate-level bandwidth can be used to check the robustness of the results to the bandwidth choice.

[16] Simply "adding up" candidate level realized outcomes would *not* be appropriate. To see why, consider three municipal employees who are close and compete for one seat. In this case, a municipal employee is always elected.

Empirically, T_{mt} appears to work as expected (see Appendix B for details): First, it is symmetrically distributed around zero. Second, when the seat share of municipal employees increases due to randomness in the outcomes of the close elections (i.e., when T_{mt} increases by one unit), so does their actual share (i.e., M_{mt}). This implies that the coefficient of T_{mt} in the 1st stage of the IV should be close to one in a finite sample and equals unity asymptotically. This is indeed empirically the case in our data (Appendix B). This feature means that in the reduced form of our IV of equation (1), which means regressing the outcome directly on the instrument (and controls; Wooldridge 2002, ch. 5), the coefficient of the instrument ought to be very close to the IV estimate of δ. This observation is useful and in the subsequent section, we will report both the 2SLS and the reduced form of IV results.

Finally, even with the smallest possible bandwidth ($\varepsilon = 0$), we have variation in T_{mt}. as explained above. As we increase the bandwidth, almost all municipalities in our data have a close contest within at least one party list. For example, for bandwidth $\varepsilon = 0.4$, we observe either a positive or a negative instrument in 1145 municipalities out of 1544. This does *not* imply that we would use for identification all the variation in the municipal employee council seat share in the data for these 1145 municipalities: To estimate δ, we only use the part of exogenous variation in M_{mt} that the instrument, T_{mt}, isolates.

Validity Tests

In Table 1, we report balance tests for pre-treatment covariates using the largest bandwidth that is employed in the regressions ($\varepsilon = 0.4$).[17] We divide the data into two groups, based on

[17] We face the standard trade-off that smaller bandwidths lead to less precise estimates: The narrower the bandwidth, the less there is variation in T_{mt}, but the more plausible the assumption of "as-good-as random assignment". The results for the narrowest possible

the seat share of municipal employees exceeding ($T_{mt} > 0$) or falling short of ($T_{mt} < 0$) its expectation. Table 1 shows that the pre-treatment variables are well balanced, including the lagged total expenditures, the lagged municipal employee share in the council and its lagged instrument.[18] This means that the municipalities where the municipal employees won, by chance, more seats are very similar to the municipalities where municipal employees lost, by chance, seats to other occupation groups.

Table 1. Pre-treatment covariate balance at municipality-level.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0.4	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	404	5 334	828	406	5 327	818	7
Health care expenditures (€ per capita)	404	1 631	392	403	1 636	359	-5
Other expenditures (€ per capita)	404	3 703	679	403	3 691	654	12
Population	588	17 488	46 681	557	13 548	33 128	3939
Young inhabitants %	588	18.67	3.29	557	18.63	3.26	0.04
Old inhabitants %	588	17.52	4.65	557	17.90	4.42	-0.38
Council size	588	31.91	11.81	557	30.55	10.80	1.35
Municipal employees %	404	28.38	13.49	403	27.69	12.99	0.70
Municipal health care employees %	404	7.43	5.06	403	7.09	4.81	0.35
Municipal non health care employees %	404	20.95	12.71	403	20.60	12.09	0.35
Incumbents %	404	58.12	8.54	403	57.20	9.06	0.92
Women %	404	33.69	9.02	403	33.12	8.45	0.57
High professionals %	404	23.07	12.84	403	21.79	11.90	1.28
University educated %	404	14.32	10.20	403	12.70	9.63	1.61
Unemployed %	404	3.81	3.79	403	3.58	4.03	0.23

Notes: The statistical significance of the differences is tested using a *t*-test adjusted for clustering at the municipality-level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

bandwidth (ε = 0) and party affiliation balance tests echo the results reported in Table 1 (see Appendix B).

[18] The number of observations varies because some of the pre-treatment variables for the 1996 election term are not available.

Table 2 reports balance tests on council characteristics for the *current* election term.[19] As Panel A shows, the post-treatment council characteristics are well balanced. For example, the municipal employees that by chance won a seat from a candidate from another occupation are of no better or worse quality (see Ferreira and Gyourko 2014 who argue that e.g. gender discrimination would imply that candidates with the same number of votes would be of different quality), as measured by their incumbency and education. The only exception to the good balance is the councils' gender composition. This finding mirrors the strong positive correlation in the data between gender and occupation status at the candidate-level.[20] The imbalance is not a result of failed randomization, but rather an intrinsic feature of municipal employees: When a municipal employee is randomly allocated into a council, a female is more likely to get a seat in the council.

In Panels B and C of Table 2, we divide municipal employees into two categories: those who work in the health care sector and those who work in the remaining (non-health care) sectors.[21] The division allows us to analyze whether the positive correlation between municipal employment status and gender is driven by the health care sector and, in particular, by nursing being a female-dominated occupation. Panel B and C of Table 2 suggest that, indeed, the gender imbalance is related to the health care sector. We return to the importance of gender for our findings below.

[19] The post-treatment seat shares are by definition balanced, because our instrument is based on within party close contests (see Appendix B).

[20] Municipal employees are more often female and have higher socioeconomic status than the candidates that have other employment status (see Table A1 in Appendix A).

[21] We do not disaggregate the latter group into more specific categories, because the data get sparse: First, candidates at finer level occupations are involved in close elections infrequently; second, detailed sector specific spending data are not always available; and third, most occupational groups are small overall (e.g., even education employees have only 3.5% seat share on average).

Table 2. Post-treatment council covariate balance.

$\varepsilon = 0.4$	$T_{mt} > 0$			$T_{mt} < 0$			
Panel A: All municipal employees	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	588	57.26	9.16	557	57.29	8.85	-0.04
Female %	588	34.72	8.76	557	33.18	8.40	1.54**
High professionals %	588	23.34	12.84	557	22.06	11.83	1.27
University educated %	588	14.57	10.72	557	13.47	10.07	1.11
Unemployed %	588	3.47	3.88	557	3.43	3.99	0.04
Panel B: Municipal health care employees	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	305	57.58	8.83	319	58.13	8.88	-0.55
Women %	305	35.86	7.69	319	33.86	8.53	2.00**
High professionals %	305	25.47	13.47	319	24.11	12.47	1.36
University educated %	305	16.35	11.44	319	15.38	10.74	0.98
Unemployed %	305	3.16	3.43	319	3.22	3.88	-0.06
Panel C: Municipal non-health employees	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	522	57.25	9.09	496	57.48	8.95	-0.24
Women %	522	34.45	8.84	496	33.62	8.47	0.83
High professionals %	522	24.02	12.80	496	22.66	12.43	1.36
University educated %	522	14.67	10.79	496	14.03	10.59	0.64
Unemployed %	522	3.61	3.93	496	3.35	3.87	0.26

Notes: In Panel A, the treatment groups are based on all municipal employees. In Panel B, the groups are based on health care sector employees. In Panel C, the groups are based on those municipal employees who do not work in the health care sector. The statistical significance of the differences is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Results

Treatment Effect on Total Expenditures

We start by analyzing the effect of the share of municipal employees in the council on the (log) per capita total expenditures of the local government, measured as the average over the four year council term.

Preliminary regression results: To have a point of comparison, we report naïve OLS results with different sets of controls (Panel A of Table 3) along with the IV results (Panel B of Table 3) and the reduced form of IV (Panel C of Table 3), using the narrowest possible bandwidth of $\varepsilon = 0$. The OLS estimations obviously do not correct for the potential

endogeneity of the seat share of the municipal employees, while the latter two ought to do that. The difference between the four columns of each panel is that they include successively more controls. We use lags (means over the t–1 election term) of the control variables to avoid the possible problem of introducing bad controls (i.e. alternative outcomes) in the models.

As the first three columns of Panel A of Table 3 show, the OLS estimations suggest a positive and statistically significant association between the political representation of public employees and total expenditures. This association vanishes completely once we include a second order polynomial of the vote share of municipal employees (see column 4). This is not unexpected, because the municipal employees' vote and seat shares are highly correlated. While insignificant, the point estimates from the IV (Panel B) and the reduced form of IV (Panel C) estimations provide us with three important empirical insights: First, the IV point estimates are positive and larger in magnitude than the OLS estimates. Second, if our instrument is as-good-as random, the only implication of having more control variables in the model ought to be that they reduce residual variance. The results reported in Panel B and C bear this out: The magnitudes of the IV estimates do not change (much) when the municipal employee vote share is controlled for. This finding indicates that unlike OLS, the IV estimates are not confounded by voter preferences. Moreover, the standard errors of the estimates tend to get smaller when more controls are added. Third, the results reported in Panel B and C suggest that the limited amount of variation in the instrument is a potential problem with using the narrowest possible bandwidth ($\varepsilon = 0$). If so, the first-stage regressions may suffer from low power, especially when fewer controls are included. This is indeed what we observe: The first stage F-tests become larger when we control for the municipal employee vote share (see column (8)).

Table 3. Results for total expenditures: OLS and IV analysis with $\varepsilon = 0$.

Panel A: OLS	(1)	(2)	(3)	(4)
Municipal employees	0.0016***	0.0021***	0.0018***	-0.0003
	[0.0005]	[0.0004]	[0.0004]	[0.0007]
R^2	0.29	0.43	0.58	0.58
Panel B: IV, $\varepsilon = 0$	(5)	(6)	(7)	(8)
Municipal employees	0.0058	0.0046	0.0070	0.0048
	[0.0110]	[0.0103]	[0.0087]	[0.0042]
First stage Kleibergen-Paap *F*-statistic	2.01	1.98	2.44	35.23
Panel C: Reduced form of IV, $\varepsilon = 0$	(9)	(10)	(11)	(12)
Municipal employees	0.0024	0.0019	0.0031	0.0041
	[0.0047]	[0.0042]	[0.0036]	[0.0036]
R^2	0.29	0.42	0.57	0.58
N	1544	1544	1544	1544
Year dummies	Yes	Yes	Yes	Yes
Party controls	No	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes
Vote share	No	No	No	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. Vote share includes a second order polynomial of the municipal employees' vote share. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Main regression results: To explore whether we can estimate the effect of political representation of municipal employees on municipal expenditures more precisely, we use the wider bandwidth of $\varepsilon = 0.4$. The wider bandwidth allows us to bring in more variation from the close elections. These results are reported in Table 4, where Panel A reports our IV estimates and Panel B our reduced form estimates. The estimations that rely on the wider bandwidths can be taken to be more reliable if they produce a point estimate that is similar in magnitude to that produced by the narrowest bandwidth and if the effect can be estimated with greater precision (smaller standard error).

Table 4. Results for total expenditures: IV analysis with $\varepsilon = 0.4$.

Panel A: IV, $\varepsilon = 0.4$	(1)	(2)	(3)	(4)
Municipal employees	0.0034*	0.0046***	0.0040***	0.0041***
	[0.0018]	[0.0017]	[0.0015]	[0.0016]
First stage Kleibergen-Paap *F*-statistic	56.79	59.91	59.65	288.9
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(5)	(6)	(7)	(8)
Municipal employees	0.0032*	0.0043***	0.0037***	0.0036***
	[0.0017]	[0.0016]	[0.0014]	[0.0014]
R^2	0.29	0.42	0.57	0.58
N	1544	1544	1544	1544
Year dummies	Yes	Yes	Yes	Yes
Party controls	No	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes
Vote share	No	No	No	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. Vote share includes a second order polynomial of the municipal employees' vote share. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Starting from the IV estimates in Panel A of Table 4, we find across all specifications a statistically significant treatment effect of 0.0034-0.0046 on municipal spending from having a larger share of municipal employees in the council. The reduced form results in Panel B echo the IV findings: They yield treatment effect estimates that are statistically significant and very similar to those obtained with IV, but somewhat smaller in magnitude. It is especially noteworthy that both estimators deliver point estimates that are very close to those we obtained using the narrowest possible bandwidth ($\varepsilon = 0.0$; see Panel B in Table 3). The fact that the reduced form estimates are a little smaller in absolute value than the IV estimates suggests that the first stage coefficient of the instrument is close to, but somewhat smaller than, one (as it often is; see Appendix B). It is comforting to report that we cannot reject the null hypothesis that the 1st stage coefficient of the instrument is unity.

The point estimates of Table 4 show that, consistent with our *Hypothesis 1*, the council seat share of municipal employees causally increases municipal spending. Increasing municipal employees' seat share by 1 percentage point increases per capita total

expenditures annually by circa 0.4 % over one election term. As one seat is on average 3 percentage points of the total number of seats, the overall average effect of an increase of one seat is roughly (at least) 1%. Because the average annual municipal spending is around 5600 Euros per capita, this effect translates into around 60 euros per capita. The effect is surprisingly large taking three features into account: First, we are identifying the effect at a potentially unimportant of margin of allocating the last seats to the council. Second, the non-elected marginal candidates, to which the elected ones are compared, are often vice-councilors. Our estimate is conservative, because vice-councilors get to attend council meetings if the councilor is absent, may get a council seat if elected councilors step down during the term, and are sometimes given positions in the municipal sub-committees. Third, there are non-negligible institutional restrictions on the political representation of the municipal employees. The effect could have been larger, had there not been no such rules in place. While a detailed comparison is not straightforward, the magnitude of the effect is nevertheless quite comparable to those reported in the prior papers using data from similar countries and identification based on marginal seats (Freier and Odendahl 2015; Fiva and Halse 2016; Fiva, Folke, and Sørensen 2017).

Robustness checks: We have explored the robustness of our main findings and their internal and external validity in a number of ways (see Appendix B).

First, electing public employees has the documented spending effect irrespectively of their attributes (e.g. gender, age, education). However, a consequence of electing a public employee is greater female participation in the council. This increase may in itself increase public spending (Chattopadhyay and Duflo 2004; Clots-Figueras 2011; Svaleryd 2009; Braendle and Colombier 2016). We therefore explore whether the council seat share of municipal employees increases municipal spending also when the gender composition of the marginal seats, i.e., the seat share of females, is accounted for. We instrument this

potentially endogenous share by the share of females who were randomly elected in the close contests. This instrument is calculated using the same procedure that produced the instrument for the share of municipal employees. When female seat share is included in the model, we get at the effect of electing a municipal employee while keeping the gender composition of the council constant. Adding the seat share of females to the estimations of Table 4 has only a minor impact on our results, suggesting that there is a municipal employee effect on spending independent of gender: For example, the IV estimates are still statistically significant and vary from 0.0032 to 0.0035.

Second, the choice of bandwidth $\varepsilon = 0.4$ for our main analysis is somewhat *ad hoc*. The point estimates of the municipal employee effect are quite stable across a wide range of bandwidths and statistically significant for the bandwidths from $\varepsilon = 0.24$ upwards.

Third, we have analyzed the expenditure effects separately for each year instead of the mean over the whole council term. These by-year estimates are all significant, similar in magnitude to what we reported earlier and stable over the council term (no within-term trend). We have also run by-year placebo regressions (four years prior to the council term of interest), and the estimates are insignificant as they should. However, the expenditure effect is somewhat persistent, as it is different from zero and significant for two years after the council term ends. The effect becomes insignificant by the third post-term year.

Fourth, we have also constructed the instrument using placebo thresholds of getting elected within the party lists. Reassuringly, neither the first nor the second stage IV estimates are significantly different from zero when we use the placebo thresholds.

Finally, our main results are based on the entire sample of 1544 municipality-election period observations, even though the instrument can be different from zero only within the chosen bandwidth. This choice may lead to a selection bias if the municipalities implicitly selected by the bandwidth are different from the rest of the municipalities. For example, in

the close sample defined by the choice of bandwidth of $\varepsilon = 0$, the covariates balance perfectly. On the other hand, for $\varepsilon = 0.4$, the close sample is different from the other municipalities, because larger municipalities are selected into the close sample.[22] However, it is unlikely that this selection compromises the validity of our findings, because our point estimates are robust to changing the bandwidth. We have also replicated the results of Table 4 using only those observations in which close elections take place: The point estimates remain unchanged, standard errors are slightly larger, but the estimates are mostly statistically significant nonetheless.

Mechanisms at Work

Our results show that when elected, municipal employees influence per capita local public spending (*Hypothesis 1*). We now turn to our *Hypothesis 2a* and test whether the influence of an additional municipal employee depends on council size. In the two leftmost columns of Table 5, we present the results for which we have divided the sample into two based on the median council size (27 councilors). Consistent with *Hypothesis 2a*, we find that the effect is larger and significant in the municipalities with a smaller council size. The difference between the smaller and larger councils is also statistically significant.[23]

We then test *Hypothesis 2b* and specifically the possibility that the municipal employees have a disproportionate effect within and via their party. Table 5 reports results

[22] The reason for this is that we define the bandwidth within parties in vote shares. This means that even the bandwidth of 0.4 (4 votes out of 1000) is very narrow. For example, a party list needs get more than 500 votes for a candidate with a two vote distance to the threshold to be within the bandwidth. Larger municipalities have such narrow bandwidths more often.

[23] The effect for the larger councils is not significantly different from zero. This does not imply that municipal employees could not affect spending in some types of larger councils, but studying such heterogeneity in detail would call for larger datasets and an alternative identification strategy.

for the largest and second largest parties. Unlike in our earlier regressions, here the endogenous explanatory variable and the instrument refer to the shares of municipal employees within the respective party, not in the entire council. We find a significant effect for the largest party, whereas the estimates are smaller and insignificant for the second largest party (see also Appendix C). However, the effects are not statistically significantly different from each other. Thus, while not conclusive, the evidence is consistent with *Hypothesis 2b*.[24] This result suggests that municipal employees may be a non-partisan interest group that is able to influence decision making especially *within* the party. If the party is large, they have a disproportionate effect on policy.[25]

[24] We should note that the Centre Party is most often the largest party in the Finnish municipalities, due to its considerable support in the smaller rural municipalities (which constitute the bulk of municipalities). Therefore, the effect captured in Table 5 may be a Centre Party phenomenon rather than a more general party size effect.

[25] We have also considered a number of other explanations. First, the marginally elected municipal employees do not lead to municipal employees having a majority in the council or to their party becoming dominated by municipal employees: Such instances are very rare in the data. Second, the effect is not larger in the municipalities where the marginally elected councilor was the only elected municipal employee from his/her party (not reported). Moreover, instances where there would be only one municipal employee in the entire council are very rare in the data. Finally, the increase in the municipal employee representation apparently does not increase the probability that a political leader (chairman of the council board or chairman of the council) would be a municipal employee.

Table 5. Heterogeneity in the total expenditures effect by council and party size.

	Council size ≤ 27	Council size > 27	Largest party	2nd largest party
Panel A: IV, ε = 0.4	(1)	(2)	(3)	(4)
Municipal employees	0.0066***	0.00003	0.0048**	0.0016
	[0.0023]	[0.0023]	[0.0020]	[0.0031]
First stage Kleibergen-Paap *F*-statistic	27.81	27.69	75.22	38.95
Panel B: Reduced form of IV, ε = 0.4	(5)	(6)	(7)	(8)
Municipal employees	0.0051***	0.00003	0.0049**	0.0016
	[0.0016]	[0.0024]	[0.0020]	[0.0032]
R^2	0.59	0.60	0.56	0.57
N	1017	527	1469	1235
Year dummies	Yes	Yes	Yes	Yes
Party and municipality controls	Yes	Yes	Yes	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Evidence on Rent-Seeking

To shed light on whether the effect of public employee representation on public spending reflects rent-seeking, or whether it is more consistent with efficient provision of public services, we test *Hypothesis 3a* and *Hypothesis 3b*.

To start with, we explore whether the link between municipal employees in the council and municipal spending is occupation specific. It is plausible that municipal employees have more information on their own employment sector. However, there is no reason why, for example, a teacher would have better information about the appropriate level of health care spending than an otherwise similar councilor from the private sector. In columns (1) and (3) of Table 6, the outcome variable is municipal expenditures that are not related to health care, whereas in columns (2) and (4) of the panels the outcome variable is

health care expenditures.[26] In these models, the interpretation for the coefficient for municipal health care employees is that it mirrors the effect of increasing their seat share relative to any non-municipal employee occupation.[27]

The results suggest that health care municipal employees increase health care expenditures, but non-health care municipal employees have no effect on them. Similarly, health care employees do not affect non-health care expenditures, but municipal employees in the sectors other than health increase the other (non-health) municipal expenditures. Thus, consistent with *Hypothesis 3a*, spending increases seem to be confined to the sectors that have, by chance, more representation through municipal employees in the municipal council.[28]

While not entirely conclusive, the evidence is consistent with the information advantage of municipal employees (Niskanen 1971; Romer and Rosenthal 1979). Of course, one has to bear in mind that the context of those models is somewhat different from ours as these models focus on how bureaucrats can convince politicians to overspend. Moreover, the evidence speaks – at least mildly – for inefficient spending, because our as-good-as-random instrument ensures that variation in the needs of citizens is not driving the results. This raises the obvious question of why municipal employees' information advantage leads to increased – and not to decreased – spending and only in their own sector of employment. These results also speak against the interpretation that municipal

[26] When there are more than one endogenous variable, we report the Angrist-Pischke first-stage *F*-statistics of individual endogenous regressor produced by the ivreg2 STATA command.

[27] The results for pre-treatment covariate balance tests and the first stage estimations of the IV suggest that the instrument works as expected (Appendix D).

[28] The effects are not statistically significantly different from each other. These results are similar also if we run the analysis by party size or if we add the seat share of females to the models (see Appendix D). The results for the non-health care expenditures are also robust to using other bandwidth choices. However, the effect of the seat share of municipal health care employees on health spending is less robust in this regard.

employees increase spending because they generally prefer a larger public sector (Knutsen 2005; Jensen, Sum, and Flynn 2009; Rattsø and Sørensen 2016). Finally, we would like to point out that intra-party bargaining – for which we already provided support earlier – is an example of an indirect mechanism that could generate the observed sector specific effects: Given that councilors with municipal employment cannot be members of the sub-committee of their own sector, they have to influence sector-specific spending indirectly.

Table 6. Results according to occupation and spending category.

	Outcome: non health care expenditures	Outcome: health care expenditures
Panel A: IV, $\varepsilon = 0.4$	(1)	(2)
Municipal non health care employees	0.0043**	0.0016
	[0.0021]	[0.0036]
First stage Angrist-Pischke *F*-statistic	30.09	30.09
Municipal health care employees	0.0045	0.0081**
	[0.0033]	[0.0039]
First stage Angrist-Pischke *F*-statistic	33.88	33.88
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)
Municipal non health care employees	0.0043**	0.0019
	[0.0021]	[0.0035]
Municipal health care employees	0.0036	0.0076**
	[0.0030]	[0.0036]
R^2	0.43	0.18
N	1534	1534
Year dummies	Yes	Yes
Party and municipality controls	Yes	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variables are the logarithms of the means of per capita expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

The results reported so far are consistent with our *Hypothesis 3a*, i.e., with municipal employees having an information advantage over politicians and being able to convince politicians to spend more on public services. To test *Hypothesis 3b,* which postulates that the extra spending is excessive and related to rent-seeking, we analyze whether municipal

employees enjoy larger returns to office in terms of receiving larger salary increases and/or facing smaller unemployment risk, and whether they enjoy from a larger incumbency advantage than the other candidates. When we use candidate-level data (either lottery outcomes that make the election status truly random or RDD), we find no systematic evidence that that the municipal employees would get higher salaries, be more likely to be employed subsequently, or that they would be more likely to get re-elected or get more votes (in the next election at $t + 1$) than the other candidates due to getting elected at time t (see the Appendix E for details of these results).[29]

These null results do not support *Hypothesis 3b*. Using auxiliary survey data from the Finnish Broadcasting Company (YLE), we have, however, confirmed that municipal employees who run for a council differ from the other candidates in two intriguing ways: Firstly, they oppose more strongly firing of municipal employees in connection with municipal mergers. Secondly, they oppose more strongly restrictions on nomination of municipal employees in municipal boards (see Appendix E for details and further media references). One could argue that these stated views, as well as the concerns expressed in the Finnish media, are harder to reconcile with pro-social behavior than with rent-seeking.

Conclusions

We have produced three novel findings in this paper. First, the political representation of municipal employees has a positive causal effect on overall local public spending. Second, the effect is sector specific: Having more health care sector employees in the council

[29] We have also analyzed whether the political representation of municipal employees shows up in house prices, because high levels of government rent extraction might be capitalized in them (Gyourko and Tracy 1991). Using municipal-level data on real estate transactions, we find no effect on house prices.

increases health care spending and having more non-health sector employees increases non-health care spending, but there are no significant cross-sector effects. Third, the effect appears to be related to the interest group influencing the policy from within the parties.

Our findings hold two lessons for contemporary research in political economics and political science. The first is that politicians' identities matter in local political decision making characterized by proportional representation and open-list D'Hondt election rule. The citizen-candidate model (Osborne and Slivinski 1996; Besley and Coate 1997) is therefore more in line with our evidence than the median voter model or Tiebout (1956) competition. The second lesson is that the marginally elected candidates are able to influence local policy. This influence may explain why in the very same Finnish elections that we have studied in this paper, a greater likelihood of being the pivotal voter increases turnout (Lyytikäinen and Tukiainen 2016).

It is important to interpret our results in the context to which they apply. Our findings refer to a country that has a large public sector and that has traditionally given the local municipalities a major role in the allocation of public resources and production of public services. While we do not find systematic evidence of rent-seeking, our results show that the Finnish municipal councilors employed by the public sector want – *by revealed preference* – to increase public expenditures in a country that in 2014 had the highest public sector ratio to GDP and whose local governments were among the most indebted among all OECD countries. This is puzzling because our as-good-as-random design guarantees that the citizens' needs are identical on average in the analyzed municipalities across all sectors. One can therefore raise the question why, in this context, would informed and benevolent municipal employee councilors increase rather than decrease their own sector's public

spending? Moreover, can it be desirable – in this context and more generally – that municipal spending is strongly affected by one particular interest group?

Making precise statements about the external validity of a close elections analysis is challenging. On one hand, there are about 40 countries in the world using an open-list PR, similar in spirit to what we have studied. Moreover, the Finnish rules governing the political representation of municipal employees have the same broad goal as many other countries' corresponding rules: They have been written in order to prevent public employees from having undue influence on political decision-making. It thus seems possible that our results generalize at least to countries with a similar political system at the local level. On the other hand, details of political processes tend to matter: We should not extrapolate too much, as there is quite a bit cross-country variation in both the precise functioning of the open-list PR systems as well as in the design of ineligibility and incompatibility rules (Braendle and Stutzer 2016).

Rather than offering detailed policy recommendations, we conclude with a call for more research. There are three reasons to this call. First, while our findings support the argument that some regulation of public employees' political involvement is warranted, they do not provide guidance on the optimal design of ineligibility and incompatibility rules. We can only conjecture how large the estimated effect would have been, had there not been any restrictions on political participation of public sector employees in Finland. Moreover, we would need to understand better what the interests of other groups are to optimally design policy. Second, when, how and why ineligibility and incompatibility rules prevent public employees from having undue influence on political decision-making in general and spending in particular is likely to be context dependent. This calls for replicating our analysis in other institutional contexts. Finally, the empirical procedure

presented here can be applied to a wider range of electoral systems than just the open-list PR. For example, one can use similar aggregation of close races to look at effects of council composition in plurality systems, where the council is composed of politicians elected from many (single or multi-member) districts. Subsequent work can thus make use of our procedure to provide more analyses of the desirability to restrict public employees' political participation in different environments.

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Public Employees as Politicians: Evidence from Close Elections

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This document includes Online Appendices to paper "Public Employees as Politicians: Evidence from Close Elections". Appendix A includes descriptive statistics. We discuss various robustness and validity checks related to the results presented in the main text in Appendices B (main results), C (council and party size heterogeneity) and D (sectoral effects). In Appendix E report a battery of tests related to rent seeking. Finally, in Appendix F, we present validity checks for the instrument for female seat share, which is used as a control variable in various specifications.

Online Appendix A: Descriptive Statistics

Table A1. This table reports the descriptive statistics on the candidates in elections held between 1996 and 2008. These data are used to construct, e.g., the instrument and some control variables. To illustrate the differences between municipal employee and other candidates, we also split the sample in two by municipal employee status. Overall we have 161,263 candidate-election observations. The final candidate sample size is 152,987 as we omit 33 elections, because those municipalities underwent a merger during the election term. We also omit 2004 data for two merging municipalities due to ambiguities in the candidate-level election data. It seems that the ambiguity results from a popular candidate being disqualified. In Table A1, 5% of the municipal employees are classified as unemployed due to differences in survey timing and definitions between Statistics Finland unemployment status and our municipal employee status.

Table A1. Candidate characteristics.

	All			Municipal employees			Other		
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Vote share	152 987	1.01	1.21	35 491	1.11	1.28	117 496	0.98	1.18
Party vote share	152 987	6.05	11.28	35 491	6.19	10.53	117 496	6.01	11.49
Number of votes	152 987	59.29	149.33	35 491	68.81	152.36	117 496	56.42	148.28
Female	152 987	0.39	0.49	35 491	0.56	0.50	117 496	0.34	0.47
Age	152 987	46.23	12.30	35 491	45.12	10.49	117 496	46.57	12.78
Incumbent	152 987	0.21	0.41	35 491	0.25	0.43	117 496	0.20	0.40
Wage income (€)	134 034	22 895	25 572	30 964	24 355	14 941	103 070	22 457	27 973
Capital income (€)	134 034	2 408	30 933	30 964	1 025	8 431	103 070	2 823	34 960
High professional	152 913	0.20	0.40	35 482	0.31	0.46	117 431	0.16	0.37
Unemployed	152 913	0.07	0.25	35 482	0.05	0.22	117 431	0.07	0.26
University degree	120 922	0.15	0.36	30 790	0.18	0.38	90 132	0.14	0.35
Coalition Party	152 987	0.19	0.39	35 491	0.17	0.37	117 496	0.19	0.40
Social Dem. Party	152 987	0.22	0.41	35 491	0.27	0.44	117 496	0.20	0.40
Center Party	152 987	0.28	0.45	35 491	0.26	0.44	117 496	0.28	0.45
True Finns	152 987	0.03	0.17	35 491	0.02	0.12	117 496	0.03	0.18
Green Party	152 987	0.04	0.20	35 491	0.05	0.22	117 496	0.04	0.20
Left Alliance	152 987	0.11	0.31	35 491	0.11	0.31	117 496	0.11	0.31
Swedish Party	152 987	0.04	0.19	35 491	0.04	0.19	117 496	0.04	0.19
Christian Dem. Party	152 987	0.04	0.20	35 491	0.04	0.20	117 496	0.04	0.21
Other parties	152 987	0.05	0.22	35 491	0.04	0.20	117 496	0.06	0.23

Notes: Income and education data are missing for some observations for all election years. More importantly, for the 1996 elections, income data are available only for the candidates who run also in 2000, 2004 or 2008 elections. We use 1995 occupation data for the elections held in 1996.

Table A2. This table reports the descriptive statistics at the municipality level, including both municipality and local council characteristics.

Table A2. Summary statistics for municipal and council data.

Variable	Mean	Std. dev.
Municipality characteristics		
Total expenditures (€ per capita)	5,564	999
Health care expenditures (€ per capita)	1,699	409
Other expenditures (€ per capita)	3,865	822
Population	12,912	36,999
Young inhabitants %	17.7	3.52
Old inhabitants %	19.5	4.90
Council composition		
Council size	29.1	11.3
Municipal employees %	26.4	12.3
Municipal health care workers %	7.02	5.11
Municipal non health care workers %	19.40	11.43
Incumbents %	56.9	9.22
Women %	33.9	8.93
High professionals %	20.9	11.9
University educated %	12.6	9.9
Unemployed %	3.54	4.02
Center Party seat share %	40.5	21.2
Coalition Party seat share %	16.3	10.9
Social Democratic Party seat share %	19.6	11.3
Green party seat share %	1.88	3.52
Left Alliance seat share %	7.82	8.01
Swedish Party seat share %	5.33	18.1
True Finns seat share %	1.75	4.13
Christian Democrats seat share %	2.99	3.94
Other parties seat share %	3.87	9.05

Notes: Unit of observation is a municipality *m* in election period *t*. Number of observations is 1544. Municipality characteristics are calculated as means over the four year council term. Young inhabitants refer to the age group of 0-17 year old and old to 64+ year old.

Online Appendix B: Robustness and Validity of the Total Expenditures Effect

Figures B1 and B2. These figures illustrate that the variation in the instrument increases as the bandwidth increases. The shape of the distribution remains symmetric, implying valid randomization.

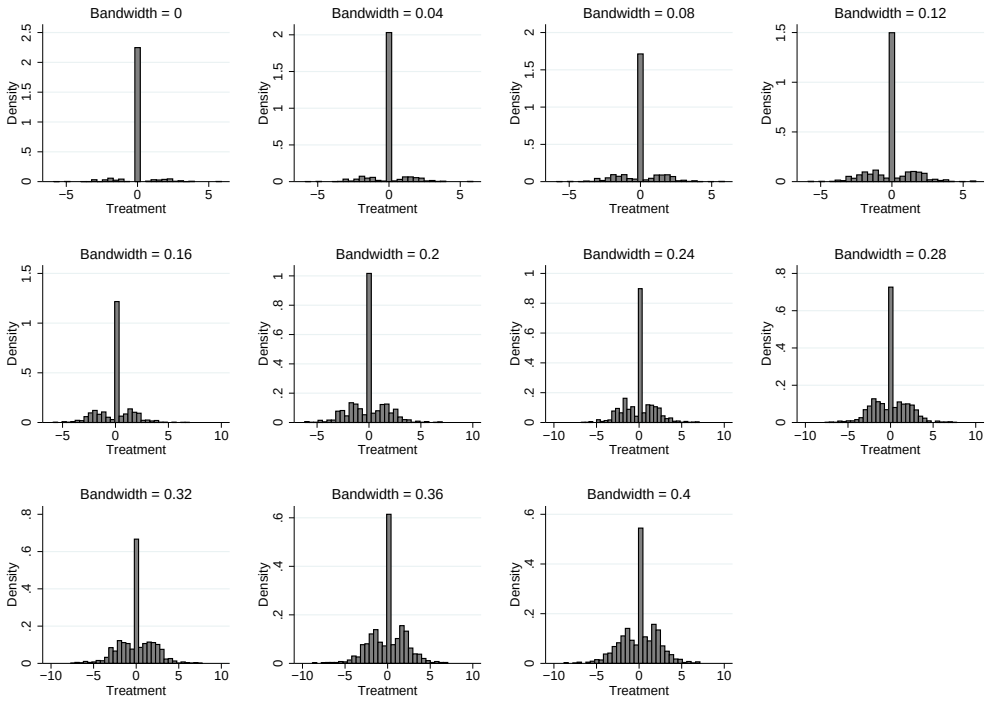


Figure B1. Distribution of T_{mt} .

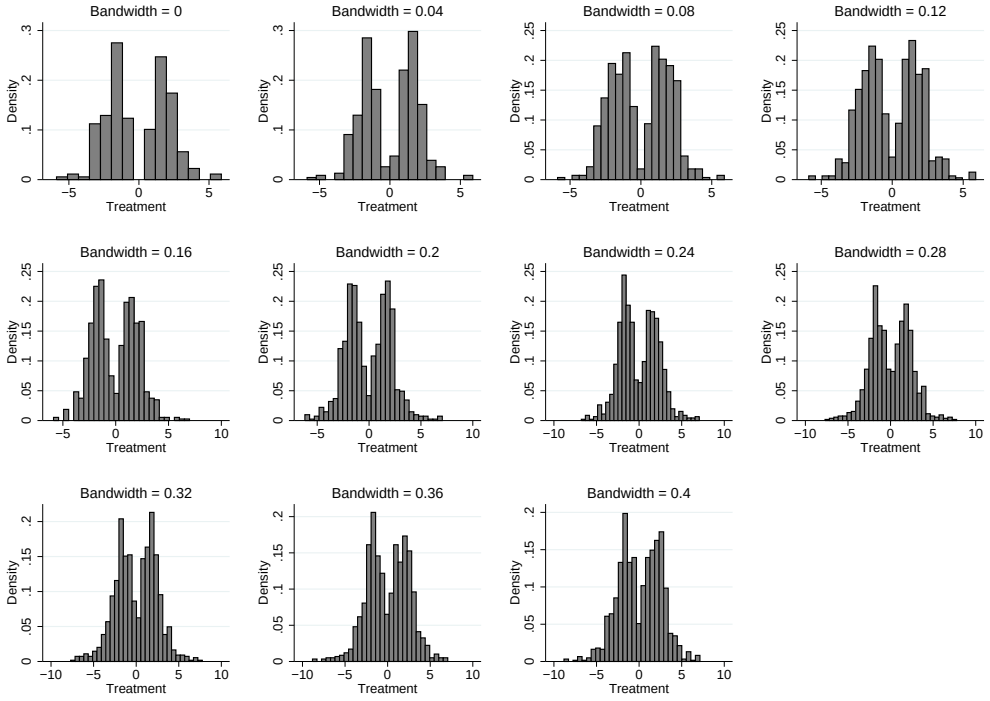


Figure B2. Distribution of T_{mt} (excluding zeros).

Figure B3. We use this figure to explore whether our aggregation procedure produces a correct municipality level instrument. We can do so by running the first stage of IV and checking whether the coefficient of the instrument T_m (ϕ) is indeed one. This regression can also be used to test for the power of our instrument for various bandwidth sizes. In Figure B3, we present estimates of ϕ for various bandwidths (ε), first controlling only for the year fixed effect (the figure on the left) and then for all the municipality controls (the figure on the right). As can be seen, the coefficient is below unity when the instrument is calculated using only the lotteries in the data (i.e., those ties that are actually solved using a lottery), though we cannot reject the null hypothesis that it is unity. However, when using larger bandwidths the point estimate is close to unity, as it should be. The anomaly in the "lottery sample" may simply be a small sample statistical fluke: In particular, the first stages for the instruments for health care employees or females do not contain this anomaly (see Figures D1 and F1).

The first stage is fairly precisely estimated for bandwidths larger than 0.04 (i.e., 4 votes out of ten thousand). The control variables do not increase precision substantially. The "lottery sample" ($\varepsilon = 0$) produces noisier results, but the precision increases as we increase the bandwidth. For a bandwidth of 0.04 the *F*-test statistics for the instrument is around 10 and for the larger bandwidths it is substantially larger than 10 (e.g. for the 0.4 bandwidth with the controls, the *F*-test statistic is 60). From the perspective of statistical power, we should rely on the results that use bandwidths of about 0.08 or larger.

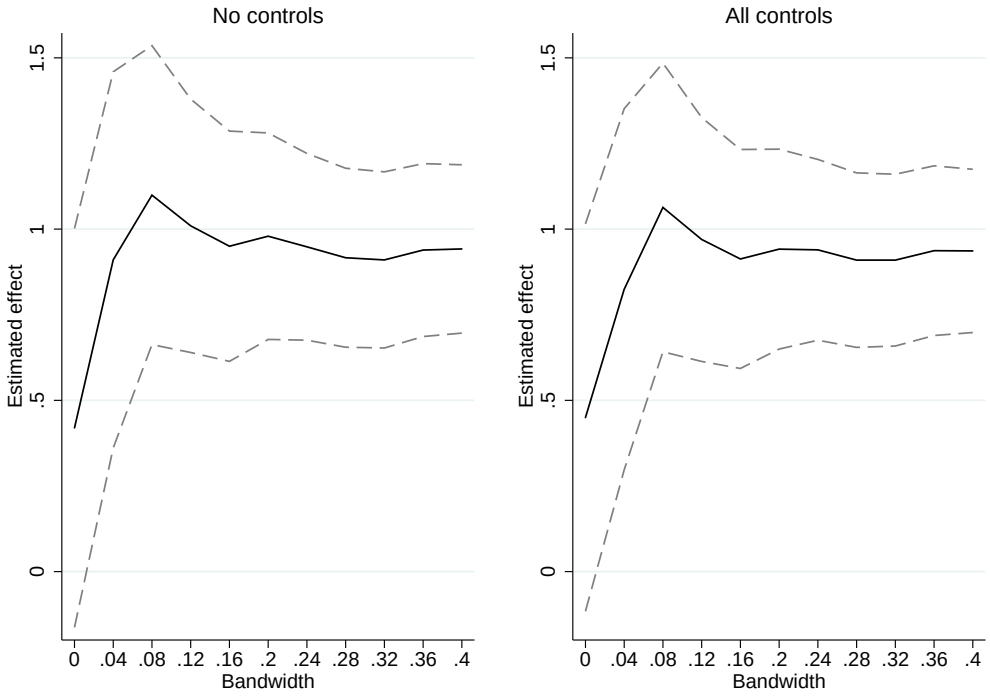


Figure B3. First stage of IV for municipal employees.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

Table B1. This table shows the pre-treatment covariate balance. We divide the data into two groups, based on the seat share of municipal employees exceeding ($T_{mt} > 0$) or falling short of ($T_{mt} < 0$) its expectation and test whether the difference in means is statistically significant. To this end, we employ a simple *t*-test, adjusting for clustering at the municipality level. The number of observations varies because we do not observe some of the pre-treatment variables for the 1996 election term. For example, we do not have the 1992 individual level election data. Furthermore, due to a structural data break in 1997, we do not have comparable expenditure measures for 1993–1996. Only in one case out of 48, we find one difference being statistically significant at 10 % level. Therefore, this table provides support for our instrument capturing truly random variation.

We also test covariate balance using regression that controls for year fixed effects (not reported). When $\varepsilon = 0.4$, the null hypothesis of balance is rejected only for two variables (Coalition Party seat share and Council size) at the 5% significance level. Due to multiple testing, this cannot be taken as a sign of imbalance: the number of rejections is no more than would be expected at the chosen level of significance.

Table B1. Pre-treatment covariate balance at municipality-level.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0 (lotteries)	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	68	5 316	956	75	5 323	838	-7
Health care expenditures (€ per capita)	68	1 600	352	75	1 653	370	-53
Other expenditures (€ per capita)	68	3 716	795	75	3 670	663	46
Population	109	8 524	14 144	118	8 835	11 398	-311
Young inhabitants %	109	18.83	3.67	118	18.67	3.04	0.16
Old inhabitants %	109	18.05	4.61	118	18.02	4.61	0.03
Council size	109	27.75	9.32	118	27.88	10.05	-1.17
Municipal employees %	68	28.69	14.07	75	27.75	11.50	0.93
Instrument for municipal employees	68	0.00	0.08	75	-0.08	0.08	0.08
Municipal health care employees %	68	7.72	5.50	75	7.50	4.49	0.22
Municipal non-health care employees %	68	20.97	12.11	75	20.25	10.69	0.72
Incumbents %	68	56.65	7.57	75	57.11	9.40	-3.76
Women %	68	34.02	9.63	75	34.08	8.36	-0.06
High professionals %	68	18.73	11.42	75	19.56	10.11	-0.83
University educated %	68	11.65	7.43	75	10.57	7.62	1.08
Unemployed %	68	2.81	3.21	75	3.98	4.48	-1.17*
Center Party seat share %	109	40.49	20.08	118	40.53	19.50	-0.03
Coalition Party seat share %	109	16.13	9.63	118	16.07	10.17	0.06
Social Democratic Party seat share %	109	19.97	10.92	118	21.30	10.73	-1.33
Green party seat share %	109	1.89	3.22	118	1.53	3.43	0.36
Left Alliance seat share %	109	9.49	8.83	118	8.90	8.76	0.59
Swedish Party seat share %	109	3.25	13.82	118	3.79	15.75	-0.54
True Finns seat share %	109	2.33	4.70	118	2.11	4.08	0.22
Christian Democrats seat share %	109	3.01	3.89	118	2.73	3.62	0.28
ε = 0.4	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	404	5 334	828	406	5 327	818	7
Health care expenditures (€ per capita)	404	1 631	392	403	1 636	359	-5
Other expenditures (€ per capita)	404	3 703	679	403	3 691	654	12
Population	588	17 488	46 681	557	13 548	33 128	3 939
Young inhabitants %	588	18.67	3.29	557	18.63	3.26	0.04
Old inhabitants %	588	17.52	4.65	557	17.90	4.42	-0.38
Council size	588	31.91	11.81	557	30.55	10.80	1.35
Municipal employees %	404	28.38	13.49	403	27.69	12.99	0.70
Instrument for municipal employees	404	0.17	0.10	404	0.02	0.10	0.15
Municipal health care employees %	404	7.43	5.06	403	7.09	4.81	0.35
Municipal non-health care employees %	404	20.95	12.71	403	20.60	12.09	0.35
Incumbents %	404	58.12	8.54	403	57.20	9.06	0.92
Women %	404	33.69	9.02	403	33.12	8.45	0.57
High professionals %	404	23.07	12.84	403	21.79	11.90	1.28
University educated %	404	14.32	10.20	403	12.70	9.63	1.61
Unemployed %	404	3.81	3.79	403	3.58	4.03	0.23
Center Party seat share %	588	36.83	21.08	557	37.95	21.26	-1.11
Coalition Party seat share %	588	17.15	10.07	557	15.94	10.15	1.21
Social Democratic Party seat share %	588	21.70	11.83	557	21.55	11.56	0.15
Green party seat share %	588	2.40	3.94	557	1.92	3.52	0.48
Left Alliance seat share %	588	9.19	8.64	557	8.85	8.39	0.34
Swedish Party seat share %	588	4.54	16.16	557	5.70	18.47	-1.16
True Finns seat share %	588	1.84	3.92	557	1.63	3.77	0.20
Christian Democrats seat share %	588	3.04	3.65	557	3.08	3.61	-0.04

Notes: The statistical significance of the differences is tested using a *t*-test adjusted for clustering at the municipality-level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Table B2. This table shows the post-treatment covariate balance. The means are mostly balanced between the two groups. However, it should be noted that women's seat share is significantly larger in municipalities with a positive instrument. As we argue in the main text, this is not due to failed randomization but rather to the fact that most municipal employees are women (see also Table A1).

Table B2. Post-treatment council covariate balance for all municipal employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0 (lotteries)	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	109	55.77	8.82	118	56.31	9.96	-0.54
Women %	109	33.55	8.59	118	32.42	8.96	1.14
High professionals %	109	20.29	10.63	118	20.58	10.43	-0.29
University educated %	109	12.07	8.13	118	11.42	8.53	0.65
Unemployed %	109	3.71	4.48	118	3.87	4.36	-0.16
Center Party %	109	42.55	19.84	118	41.07	19.31	1.48
Coalition Party %	109	17.10	9.59	118	17.75	10.84	-0.64
Social Democratic Party %	109	18.06	9.62	118	19.71	10.83	-1.65
Green party %	109	1.59	2.99	118	1.88	3.42	-0.29
Left Alliance %	109	8.62	8.73	118	8.17	8.48	0.45
Swedish Party %	109	3.08	13.22	118	3.80	15.97	-0.72
True Finns %	109	2.04	4.90	118	1.77	3.99	0.28
Christian Democrats %	109	3.06	3.84	118	2.95	4.15	0.11
Other parties %	109	3.89	6.96	118	2.91	6.17	0.98
ε = 0.4							
Incumbents %	588	57.26	9.16	557	57.29	8.85	-0.04
Women %	588	34.72	8.76	557	33.18	8.40	1.54**
High professionals %	588	23.34	12.84	557	22.06	11.83	1.27
University educated %	588	14.57	10.72	557	13.47	10.07	1.11
Unemployed %	588	3.47	3.88	557	3.43	3.99	0.04
Center Party %	588	38.26	20.88	557	38.48	21.00	-0.22
Coalition Party %	588	17.80	10.57	557	16.77	10.64	1.03
Social Democratic Party %	588	20.33	11.27	557	20.62	11.23	-0.29
Green party %	588	2.41	4.05	557	2.02	3.47	0.39
Left Alliance %	588	8.37	8.12	557	8.19	8.04	0.18
Swedish Party %	588	4.40	15.85	557	5.65	18.36	-1.25
True Finns %	588	1.86	4.16	557	1.69	3.76	0.17
Christian Democrats %	588	3.07	3.86	557	3.28	3.91	-0.21
Other parties %	588	3.49	6.74	557	3.30	6.30	0.19

Notes: The statistical significance of the differences is tested using a *t*-test adjusted for clustering at the municipality-level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Table B3. In this table, we analyze whether municipal employees increase public expenditures because they are more often female or because there is a municipal employee effect independent of gender. To address this question, we explore whether the council seat share of municipal employees increases municipal spending also when the gender composition of the marginal seats is accounted for. To this end, we directly control for the seat share of females (*Females*). We instrument this potentially endogenous share by the share of females who were randomly elected in the close contests. This instrument is calculated using the

procedure that produced the instrument for the share of municipal employees. We present validity checks for the instrument for female seat share in Appendix F.

When female seat share is included in the model, we get at the effect of electing a municipal employee while keeping the gender composition of the council constant. The effect then refers to either electing a male municipal employee instead of a male with another occupation or a female municipal employee instead of a female with another occupation. When included and properly instrumented, female seat share in turn captures the treatment effect of randomly electing a woman instead of a man into the council, keeping the share of municipal employees constant.

We have reproduced the estimations of Table 4, but with the seat share of females included. As can be seen from Table B3, adding the seat share of females has only a minor impact on the treatment effect estimate of the municipal employees: With IV, we find a statistically significant treatment effect of 0.0032 – 0.0035; with the reduced form model the corresponding figures are 0.0030 – 0.0031. In contrast to Chattopadhyay and Duflo (2004) and Clots-Figueras (2011), who find that increased female participation matter for the type of public spending in India, we find no robust effects from (randomly) increased female political participation, especially when the full set of controls is included. An obvious explanation for this weaker and less robust female effect is that women's position in Finland and India are quite different: Women are well represented in Finnish political decision making. Indeed, Finland was third in the world to allow female suffrage in 1906 and in our data, the share of female councilors is about 40%.

Table B3. Results for total expenditures: IV analysis for both municipal employee and female instruments.

Panel A: IV, $\varepsilon = 0.4$	(1)	(2)	(3)	(4)
Municipal employees	0.0014	0.0032*	0.0034**	0.0035**
	[0.0022]	[0.0019]	[0.0016]	[0.0016]
First stage Angrist-Pischke *F*-statistic	28.54	30.21	29.99	145.66
Females	0.0041**	0.0032**	0.0013	0.016
	[0.0019]	[0.0016]	[0.0013]	[0.012]
First stage Angrist-Pischke *F*-statistic	83.55	86.33	84.55	188.98
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(5)	(6)	(7)	(8)
Municipal employees	0.0017	0.0030*	0.0037**	0.0030**
	[0.0018]	[0.0016]	[0.0014]	[0.0014]
Females	0.0044**	0.0038**	0.0018	0.017
	[0.0017]	[0.0015]	[0.0013]	[0.013]
R^2	0.29	0.43	0.57	0.59
N	1544	1544	1544	1544
Year dummies	Yes	Yes	Yes	Yes
Party controls	No	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes
Vote share	No	No	No	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable in all the models is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. The first stage Angrist-Pischke *F*-statistics of individual endogenous regressors are produced by the ivreg2 command in STATA. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Figure B4. In Figure B4, we plot the IV estimates of the effect of municipal employee councilors on expenditures and respective 95 % confidence intervals using varying bandwidths (ε). We vary the window for individual level closeness between 0 and 0.4, i.e. the smallest and the largest bandwidth that we use in our main text. The estimates remain rather stable across this range of bandwidths.

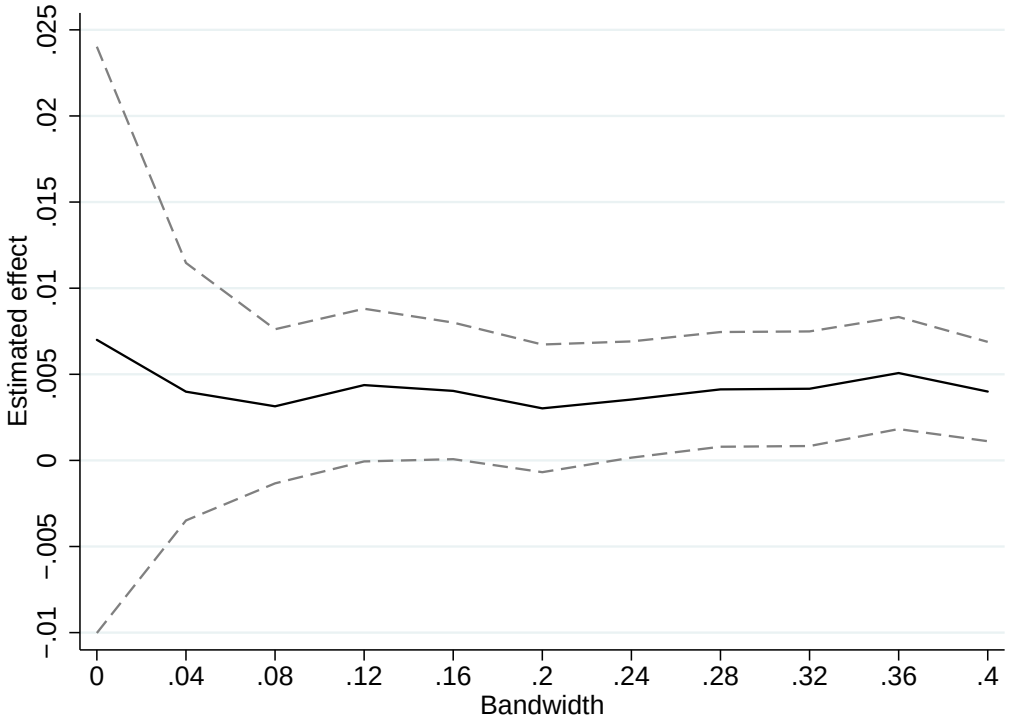


Figure B4. Robustness of the total expenditures IV effect with respect to bandwidth choice.

Notes: The solid line represents the point estimates and the dotted lines the 95% confidence interval. The specification includes year dummies as well as controls for parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Figures B5 and B6. In these figures, we analyze the expenditure effects separately for each year instead of the mean over the whole council term (as done in the main text). These by-year estimates are all significant for the council term of interest, and similar in magnitude to the main results. We have also run by-year placebo regressions (four years prior to the council term of interest), and the estimates are insignificant, as they should. A slightly worrying observation is that the placebo point estimates are quite large even though they are statistically insignificant. Further analysis revealed that this finding is driven solely by the last election term in the data. When we omit that election from the analysis the placebo estimates are closer to zero but comfortingly the estimates of key interest to us remain in this restricted sample very similar (see Figure B4) to those we report in the main text.

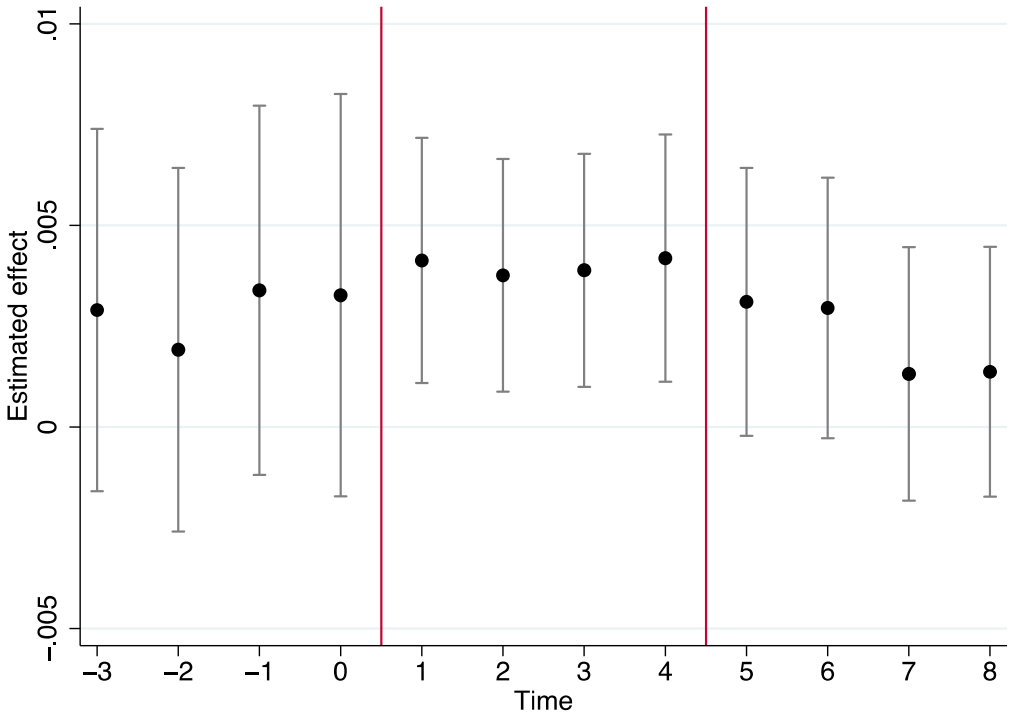


Figure B5. IV effects separately for each year.

Notes: The dots represent the point estimates and the grey lines the corresponding 95% confidence intervals. We report the effects municipal employee representation on log of total expenditures for each year's expenditures separately. Time = 0 denotes the election year and years 1–4 the actual council term in office (separated by the red lines). The specification includes year dummies as well as controls for the parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

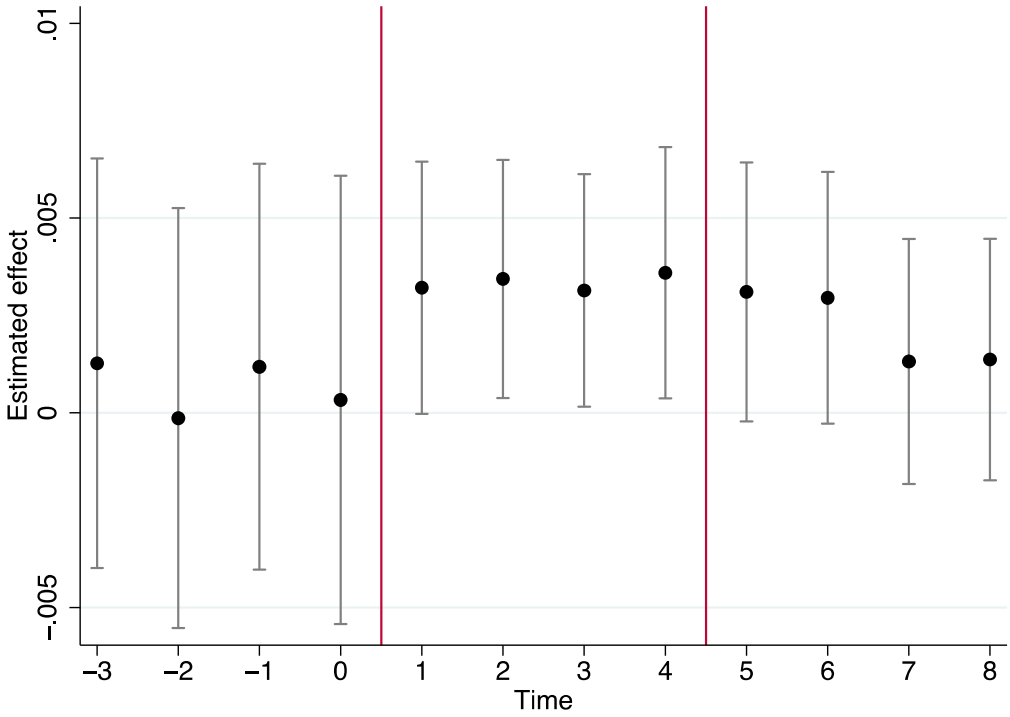


Figure B6. IV effects separately for each year excluding data from the last election term.

Notes: The dots represent the point estimates and the grey lines the corresponding 95% confidence intervals. We report the effects municipal employee representation on log of total expenditures for each year's expenditures separately. Time = 0 denotes the election year and years 1–4 the actual council term in office (separated by the red lines). The specification includes year dummies as well as controls for the parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Figure B7. In Figure B7, we report graphically the results from placebo thresholds analysis. Here, we move the within-party threshold of getting elected by steps of 0.05 when constructing the instrument (as described in the main text). Notice that when we artificially change the election thresholds, also the council size and the council composition artificially change. Therefore, at each of the artificial thresholds, we compute the respective placebo council sizes, seat shares of elected municipal employees and our instruments. For the first stage results reported in the left graph, we regress the *actual* municipal employee council share on the placebo instruments. As expected, the placebo results fluctuate around zero. One placebo estimate is statistically different from zero, but small in magnitude. Given multiple testing, this is not surprising. For the IV results, we use a different first stage, however. For the IV to have any chance of producing non-zero effects, we also use the *artificial* council share of municipal employees as the endogenous variable of interest instead of the real share and instrument it with the placebo instrument. Using the placebo seat share ensures that the first stage of the placebo IV is relevant, as there is one-to-one relationship between the placebo seat share and the placebo instrument even at the fake cut-offs. Both placebo tests are conducted using $\varepsilon = 0.4$ as the bandwidth.

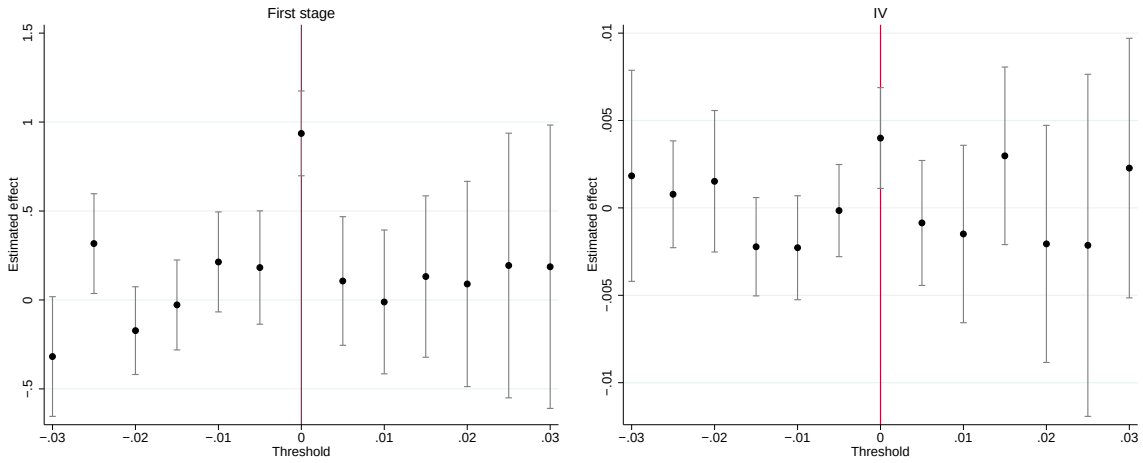


Figure B7. Effects for placebo thresholds.

Notes: The left graph reports the first stage and the right graph the second stage IV estimates. The x-axis measures distance of the placebo threshold from the actual election threshold. The red line corresponds to the actual election threshold. The dots represent the point estimates and the grey lines the corresponding 95% confidence intervals. We report the effects of municipal employee representation on log of total expenditures. The specification includes year dummies as well as controls for the parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Table B4. In this table, we provide a comparison between municipalities with and without close elections. These groups are rather similar for the narrowest bandwidth, but differences show up in the case of the largest bandwidth that we use.

Table B4. Pre-treatment covariate balance between the close sample and others.

	Close elections			No close elections			
ε = 0	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	143	5 320	893	968	5 346	843	-26
Health care expenditures (€ per capita)	143	1 628	362	965	1 638	375	-10
Other expenditures (€ per capita)	143	3 692	727	965	3 708	690	-16
Population	227	8 686	12 762	1317	13 184	37 979	-4 498
Young inhabitants %	227	18.75	3.35	1317	18.45	3.34	0.29
Old inhabitants %	227	18.04	4.60	1317	18.35	4.63	-0.32
Council size	227	27.82	9.68	1317	29.18	11.09	-1.36
Municipal employees %	143	28.20	12.75	965	27.53	13.40	0.66
Municipal health care employees %	143	7.60	4.98	965	6.95	5.00	0.65
Municipal non-health care employees %	143	20.59	11.36	965	20.58	12.63	0.01
Incumbents %	143	56.89	8.55	965	57.22	9.07	-0.32
Women %	143	34.05	8.95	965	32.82	8.93	1.23
High professionals %	143	19.17	10.72	965	20.80	12.08	-1.63
University educated %	143	11.08	7.52	965	12.25	9.69	-1.17
Unemployed %	143	3.43	3.96	965	3.89	4.15	-0.46
Center Party seat share %	227	40.51	19.73	1317	39.21	21.40	1.31
Coalition Party seat share %	227	16.10	9.89	1317	15.61	10.46	0.49
Social Democratic Party seat share %	227	20.66	10.82	1317	20.75	11.93	-0.09
Green party seat share %	227	1.70	3.33	1317	1.87	3.50	-0.16
Left Alliance seat share %	227	9.18	8.78	1317	8.43	8.31	0.75
Swedish Party seat share %	227	3.53	14.83	1317	5.69	18.55	-2.16
True Finns seat share %	227	2.21	4.38	1317	1.67	3.83	0.54
Christian Democrats seat share %	227	2.87	3.75	1317	2.91	3.72	-0.04
Other parties seat share %	227	3.24	6.55	1317	3.88	9.09	-0.64
ε = 0.4	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	810	5 330	919	301	5 376	919	-46
Health care expenditures (€ per capita)	807	1 634	369	301	1 646	369	-12
Other expenditures (€ per capita)	807	3 697	768	301	3 729	768	-33
Population	1145	15 571	3 153	399	3 773	3 153	11799***
Young inhabitants %	1145	18.65	3.51	399	18.07	3.51	0.58*
Old inhabitants %	1145	17.70	4.42	399	20.04	4.42	-2.34***
Council size	1145	31.25	5.75	399	22.45	5.75	8.80***
Municipal employees %	807	28.03	13.48	301	26.50	13.48	1.53*
Municipal health care employees %	807	7.26	5.11	301	6.44	5.11	0.82*
Municipal non-health care employees %	807	20.78	12.67	301	20.06	12.67	0.72
Incumbents %	807	57.66	9.40	301	55.87	9.40	1.80***
Women %	807	33.41	9.38	301	31.82	9.38	1.59**
High professionals %	807	22.43	8.84	301	15.64	8.84	6.79***
University educated %	807	13.51	6.61	301	8.31	6.61	5.20***
Unemployed %	807	3.69	4.63	301	4.18	4.63	-0.49
Center Party seat share %	1145	37.38	20.08	399	45.20	20.08	7.82***
Coalition Party seat share %	1145	16.56	10.68	399	13.15	10.68	3.41***
Social Democratic Party seat share %	1145	21.63	11.60	399	18.16	11.60	3.47***
Green party seat share %	1145	2.16	2.32	399	0.92	2.32	1.25***
Left Alliance seat share %	1145	9.02	7.86	399	7.15	7.86	1.87**
Swedish Party seat share %	1145	5.10	20.03	399	6.14	20.03	-1.03
True Finns seat share %	1145	1.74	4.13	399	1.79	4.13	-0.05
Christian Democrats seat share %	1145	3.06	3.96	399	2.44	3.96	0.62*
Other parties seat share %	1145	3.34	13.29	399	5.05	13.29	-1.70*

Notes: The statistical significance is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Table B5. In this table, we present results from regressions where we have excluded the municipalities without close elections. We obtain results that are very similar to what our main analysis produces.

Table B5. The effect of municipal employee council share on total expenditures using only the close elections sample.

Panel A: IV, ε = 0.4	(1)	(2)	(3)	(4)
Municipal employees	0.0035*	0.0040***	0.0040***	0.0040***
	[0.0019]	[0.0015]	[0.0015]	[0.0015]
First stage Kleibergen-Paap *F*-statistic	54.25	57.76	58.76	59.76
N	1145	1145	1145	1145
Panel B: Reduced form of IV, ε = 0.4	(5)	(6)	(7)	(8)
Municipal employees	0.0032*	0.0042***	0.0037***	0.0035**
	[0.0017]	[0.0016]	[0.0014]	[0.0014]
R^2	0.30	0.42	0.58	0.59
N	1145	1145	1145	1145
Year dummies	Yes	Yes	Yes	Yes
Party controls	No	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes
Vote share	No	No	No	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable in all the models is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. Vote share control is a second-order polynomial of municipal employees' vote share. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Online Appendix C: Robustness and Validity of the Party and Council Size Effect Heterogeneity

Figures C1 and C2. In these figures, we present the first stage of IV for the instrument in the largest and the second largest party using various bandwidths while first controlling only for the year fixed effect and then using all municipality controls. We cannot reject the null hypothesis that it is unity regardless of the bandwidth size.

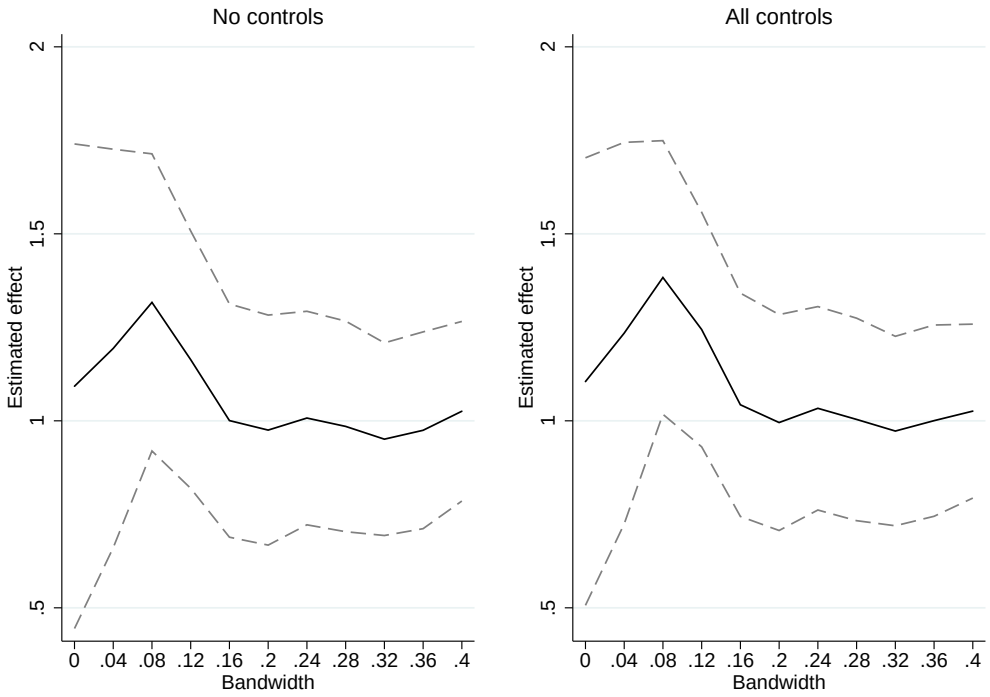


Figure C1. First stage of IV for municipal employees in the largest party.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

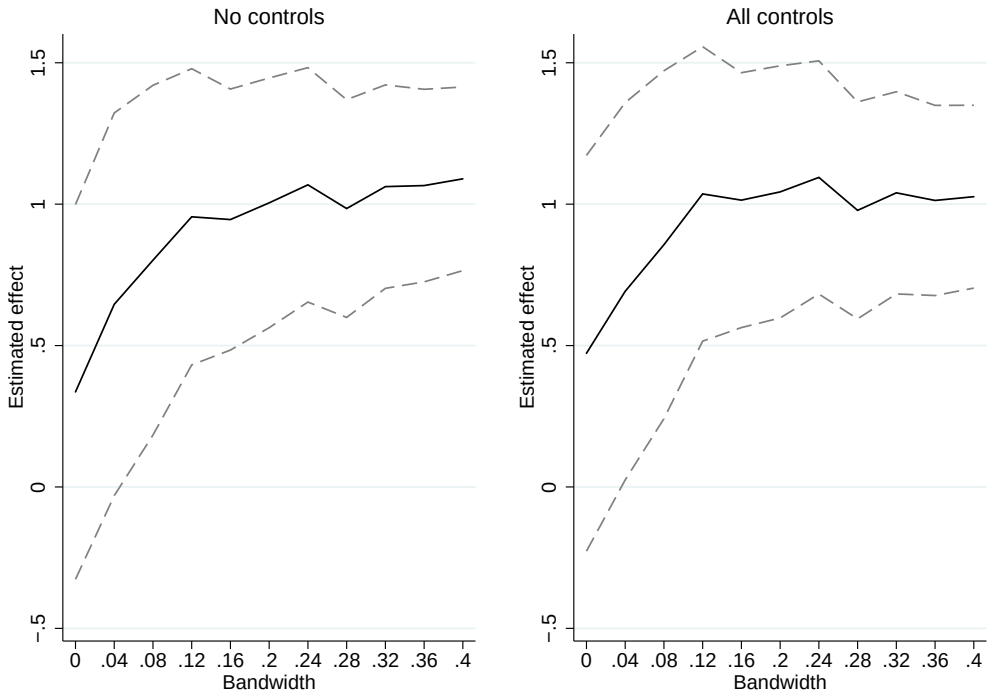


Figure C2. First stage of IV for municipal employees in the second largest party.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

Table C1. In Table C1, we check that the instruments constructed for the largest and the second party are as-good-as-random by comparing differences in pre-treatment means between the municipalities with positive and negative instruments. There are no statistically significant differences between the groups. This supports the validity of our design.

Table C1. Pre-treatment covariate balance at municipality level for the largest and second largest party.

	$T_{mt} > 0$			$T_{mt} < 0$			
Panel A: Largest party							
ε = 0.4	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	310	5,404	829	297	5,353	805	52
Health care expenditures (€ per capita)	309	1,653	400	296	1,624	369	28
Other expenditures (€ per capita)	309	3,757	695	296	3,726	649	31
Population	444	17,292	39,050	426	15,576	44,918	1716
Young inhabitants %	444	18.74	3.48	426	18.78	3.32	-0.05
Old inhabitants %	444	17.59	4.59	426	17.70	4.49	-0.12
Council size	444	32.32	11.70	426	30.97	11.35	1.34
Municipal employees %	309	28.82	13.41	296	28.00	12.92	0.82
Municipal health care employees %	309	7.14	4.75	296	7.24	4.98	-0.10
Municipal non health care employees %	309	21.68	12.74	296	20.76	12.00	0.92
Incumbents %	309	57.52	8.86	296	57.73	9.09	-0.21
Women %	309	33.13	9.43	296	33.13	8.67	0.00
High professionals %	309	23.23	12.89	296	22.27	12.33	0.95
University educated %	309	13.78	10.45	296	13.40	9.99	0.39
Unemployed %	309	3.66	3.93	296	3.67	3.92	-0.01
Panel B: 2nd largest party							
ε = 0.4	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	148	5,231	710	132	5,307	776	-76
Health care expenditures (€ per capita)	148	1,607	349	130	1,637	311	-30
Other expenditures (€ per capita)	148	3,620	537	130	3,677	628	-57
Population	212	31,485	73,880	185	23,460	53,487	8025
Young inhabitants %	212	18.41	2.97	185	18.50	2.90	-0.08
Old inhabitants %	212	16.82	4.62	185	16.95	4.56	-0.13
Council size	212	36.77	13.63	185	34.76	12.84	2.01
Municipal employees %	148	29.63	14.21	130	27.72	12.45	1.90
Municipal health care employees %	148	7.93	4.70	130	7.01	4.24	0.92
Municipal non health care employees %	148	21.69	13.24	130	20.71	12.09	0.98
Incumbents %	148	59.33	7.95	130	58.48	8.25	0.85
Women %	148	35.41	8.26	130	34.40	8.09	1.01
High professionals %	148	27.07	14.30	130	25.37	13.47	1.71
University educated %	148	17.57	12.00	130	14.96	11.22	2.60
Unemployed %	148	3.48	3.64	130	3.04	3.14	0.44

*N*otes: The statistical significance is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Figure C3. In Figure C3, we report the spending effect for the largest and the second largest party, respectively, using various bandwidths. The results for the largest party are quite stable across specifications.

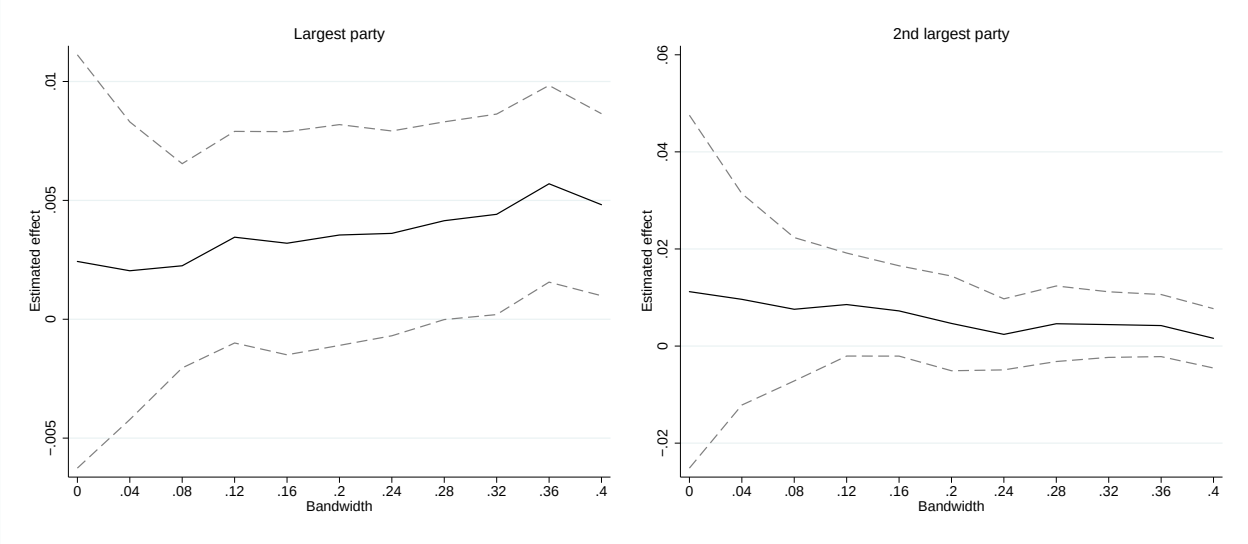


Figure C3. Robustness of the party heterogeneity result to bandwidth choice.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The specifications includes year dummies as well as controls for parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Figure C4. In Figure C4, we report the spending effect for the small (council size ≤ 27) and large councils (council size > 27), respectively, using various bandwidths.

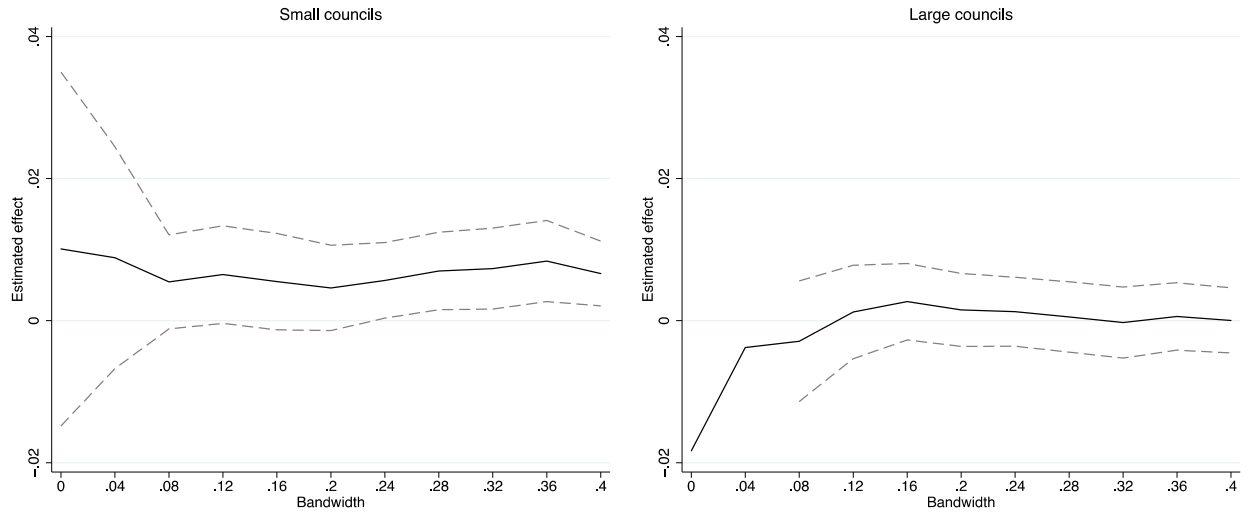


Figure C4. Robustness of the council size heterogeneity result to bandwidth choice.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. We do not report confidence intervals in the right hand graph for the smallest bandwidths, because they get very large. The specifications includes year dummies as well as controls for parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Table C2. In Table C2, we analyze whether the party and council size results for the total expenditures hold when instrumenting also for the female share. These results are largely in line with those presented in the main text also when the (instrumented) female seat share is included.

Table C2. Results for total expenditures by party and council size: IV analysis for both municipal employee and female instruments.

	Council size ≤ 27	Council size > 27	Largest party	2nd largest party
Panel A: IV, ε = 0.4	(3)	(4)	(1)	(2)
Municipal employees	0.0066**	-0.0003	0.0033	0.0028
	[0.0028]	[0.0023]	[0.0021]	[0.0034]
First stage Angrist-Pischke *F*-statistic	15.32	15.90	37.74	20.52
Females	0.0000	0.0018	0.0035*	-0.0033
	[0.0018]	[0.0021]	[0.0019]	[0.0034]
First stage Angrist-Pischke *F*-statistic	53.38	28.96	68.78	34.52
Panel B: Reduced form of IV, ε = 0.4	(7)	(8)	(5)	(6)
Municipal employees	0.0046***	-0.0004	0.0037*	0.0024
	[0.0017]	[0.0024]	[0.0020]	[0.0033]
Females	0.0013	0.0016	0.0034**	-0.0027
	[0.0016]	[0.0019]	[0.0017]	[0.0030]
R^2	0.59	0.60	0.56	0.57
N	1017	527	1469	1235
Year dummies	Yes	Yes	Yes	Yes
Party and municipality controls	Yes	Yes	Yes	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable in all the models is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. The reported first stage Angrist-Pischke *F*-statistics of individual endogenous regressors are produced by the ivreg2 command in STATA. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Table C3. In Table C3, we report sectoral results by party size. The results suggest that also the sectoral results seem to be driven by within party influence when the party is large. While we cannot statistically distinguish the estimates from each other, the pattern of the results is in line with the analysis in the main text.

Table C3. Results for sectoral expenditures by party size.

	Outcome: health care expenditures		Outcome: non health care expenditures	
	Largest party	2nd largest party	Largest party	2nd largest party
Panel A: IV, $\varepsilon = 0.4$	(1)	(2)	(1)	(2)
Health care employees	0.0145**	-0.0007	0.0005	0.0091
	[0.0066]	[0.0105]	[0.0041]	[0.0087]
First stage Angrist-Pischke *F*-statistic	37.63	12.05	37.63	12.05
Non health care employees	0.0041	0.0009	0.0051**	-0.0011
	[0.0050]	[0.0039]	[0.0025]	[0.0045]
First stage Angrist-Pischke *F*-statistic	45.52	23.00	45.52	23.00
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)	(3)	(4)
Health care employees	0.0117**	-0.0006	-0.0008	0.0059
	[0.0051]	[0.0065]	[0.0035]	[0.0052]
Non health care employees	0.0055	0.0011	0.0057**	-0.0017
	[0.0057]	[0.0045]	[0.0029]	[0.0053]
R^2	0.17	0.15	0.42	0.43
N	1459	1226	1459	1226
Year dummies	Yes	Yes	Yes	Yes
Party and municipality controls	Yes	Yes	Yes	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable is either the logarithm of the mean of per capita other than health care expenditures or health care expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. The reported first stage Angrist-Pischke *F*-statistics of individual endogenous regressors are produced by the ivreg2 command in STATA. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Online Appendix D: Robustness and Validity of the Sectoral Effects

Figures D1 and D2. These figures illustrate graphically the first stages of our sectoral IV across a range of bandwidths and test for the validity of the sector specific instruments. Figure D1 shows the first stage graphs with and without control variables for the municipal health care employees, and Figure D2 shows these for the municipal non-health care employees. Both figures support the validity of the instrument.

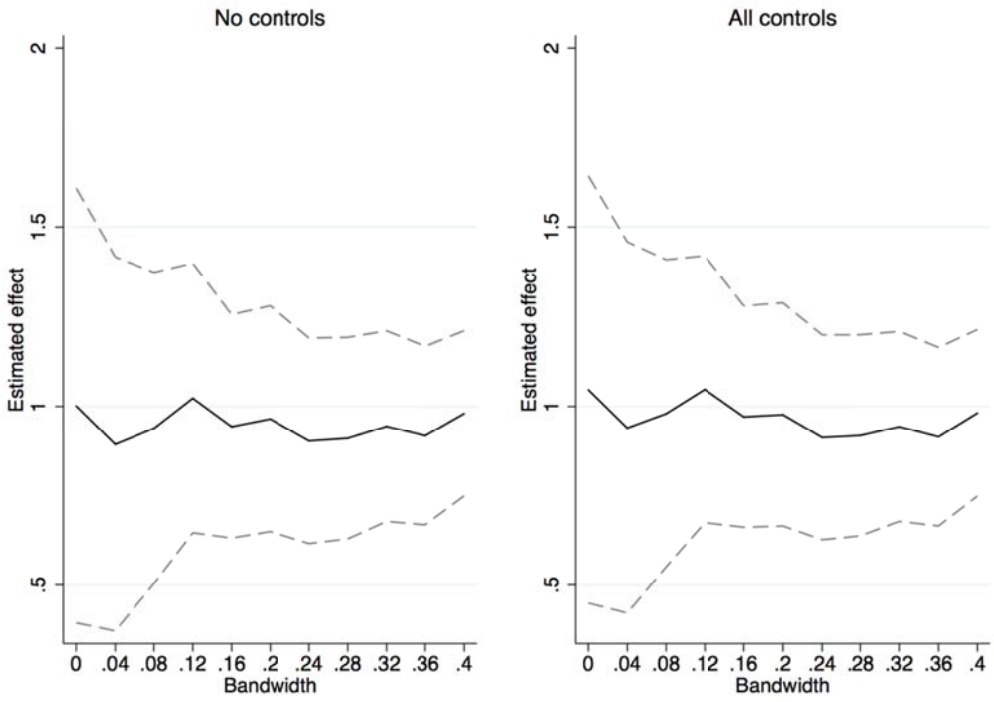


Figure D1. First stage of IV for municipal health care sector employees.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

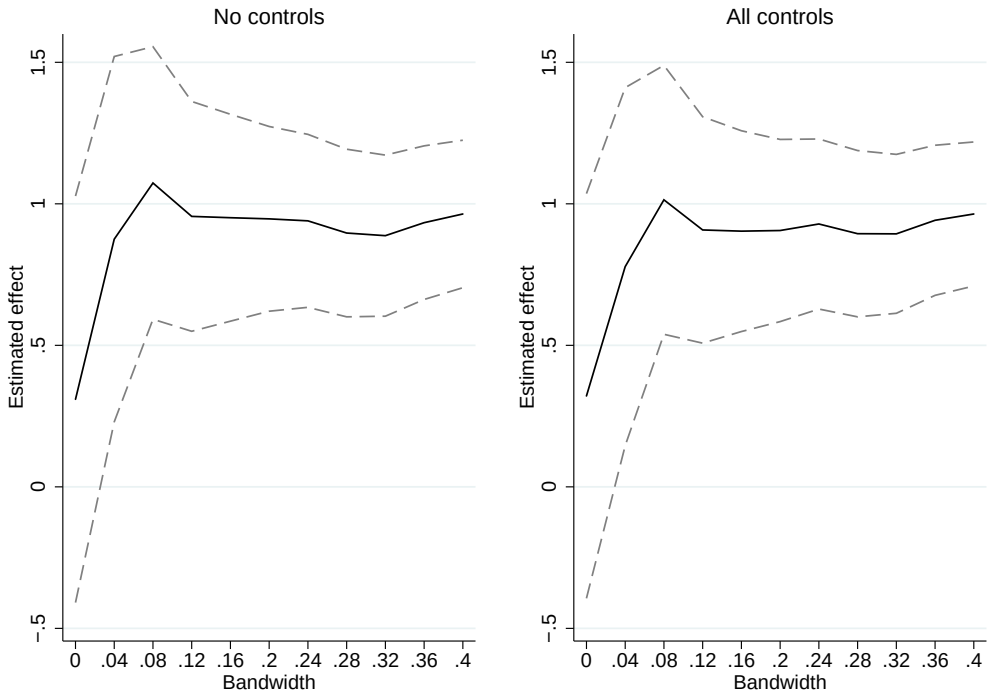


Figure D2. First stage of IV for municipal non-health sector employees.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

Tables D1 and D2. In Tables D1 and D2, we check that the sector-specific instruments are as-good-as-random. We divide the data into two groups, based on the seat share of municipal employees exceeding ($T_{mt} > 0$) or falling short of ($T_{mt} < 0$) its expectation and test whether the difference in means is statistically significant. There are no statistically significant differences between the groups. This supports the validity of our design.

Table D1. Pre-treatment covariate balance at municipality level for non-health care employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
$\varepsilon = 0.4$	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	334	5 330	810	359	5 363	808	-33
Health care expenditures (€ per capita)	333	1 626	384	357	1 633	364	-7
Other expenditures (€ per capita)	333	3 708	685	357	3 729	655	-21
Population	522	18 381	48 476	496	15 341	36 231	3 041
Young inhabitants %	522	18.77	3.22	496	18.67	3.31	0.10
Old inhabitants %	522	17.21	4.54	496	17.76	4.52	-0.56
Council size	522	32.71	11.78	496	31.30	11.41	1.41
Municipal employees %	333	28.82	13.23	357	27.81	13.62	1.01
Municipal health care employees %	333	7.34	4.72	357	7.03	4.88	0.31
Municipal non-health care employees %	333	21.48	12.60	357	20.78	12.28	0.70
Instrument for non-health care employees	333	0.18	0.11	357	0.09	0.11	0.09
Incumbents %	333	57.90	8.40	357	57.99	8.97	-0.09
Women %	333	33.76	9.18	357	33.13	8.48	0.63
High professionals %	333	24.00	12.80	357	22.71	12.71	1.29
University educated %	333	14.43	10.43	357	13.77	10.20	0.66
Unemployed %	333	3.79	3.93	357	3.57	3.98	0.22
Center Party seat share %	522	36.03	21.10	496	37.59	21.45	-1.56
Coalition Party seat share %	522	17.45	9.94	496	15.93	10.32	1.52
Social Democratic Party seat share %	522	22.46	12.12	496	21.18	11.38	1.29
Green party seat share %	522	2.52	4.00	496	2.09	3.66	0.43
Left Alliance seat share %	522	9.39	8.74	496	8.90	8.30	0.49
Swedish Party seat share %	522	3.98	14.97	496	5.85	18.69	-1.88
True Finns seat share %	522	1.97	4.19	496	1.66	3.64	0.31
Christian Democrats seat share %	522	3.04	3.56	496	3.20	3.59	-0.16

Notes: The statistical significance is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Table D2. Pre-treatment covariate balance for health care employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
$\varepsilon = 0.4$	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	222	5 314	790	227	5 234	777	79.21
Health care expenditures (€ per capita)	222	1 642	381	226	1 588	348	54.06
Other expenditures (€ per capita)	222	3 668	579	226	3 648	675	19.76
Population	305	23 734	60 686	319	18 758	43 304	4 976
Young inhabitants %	305	18.57	3.17	319	18.94	3.26	-0.37
Old inhabitants %	305	17.13	4.75	319	16.96	4.33	0.17
Council size	305	34.48	12.77	319	33.10	11.80	1.38
Municipal employees %	222	30.60	14.60	226	28.77	12.32	1.83
Municipal health care employees %	222	8.16	5.30	226	8.00	4.68	0.15
Instrument for health care employees	222	0.09	0.08	226	-0.11	0.08	0.20*
Municipal non-health care employees %	222	22.44	13.45	226	20.77	11.95	1.67
Incumbents %	222	59.18	8.72	226	57.74	8.68	1.44
Women %	222	34.02	8.59	226	34.48	8.64	-0.46
High professionals %	222	24.96	13.68	226	24.94	12.69	0.02
University educated %	222	15.74	10.61	226	15.10	10.92	0.64
Unemployed %	222	3.57	3.47	226	3.43	3.77	0.14
Center Party seat share %	305	34.51	21.18	319	35.14	20.90	-0.63
Coalition Party seat share %	305	17.21	9.88	319	17.75	10.09	-0.54
Social Democratic Party seat share %	305	22.95	11.65	319	22.69	11.79	0.26
Green party seat share %	305	2.99	4.44	319	2.44	4.03	0.56
Left Alliance seat share %	305	9.37	8.41	319	9.31	8.45	0.06
Swedish Party seat share %	305	4.85	16.61	319	4.29	16.53	0.56
True Finns seat share %	305	1.44	2.95	319	1.67	3.89	-0.23
Christian Democrats seat share %	305	3.24	3.56	319	3.22	3.40	0.02

Notes: The statistical significance is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

Figures D3 and D4. In Figures D3 and D4, we report the effect of health care and non-health care employees on non-health care and health care spending, respectively, using various bandwidths. The results for the non-health outcome are rather stable across specifications.

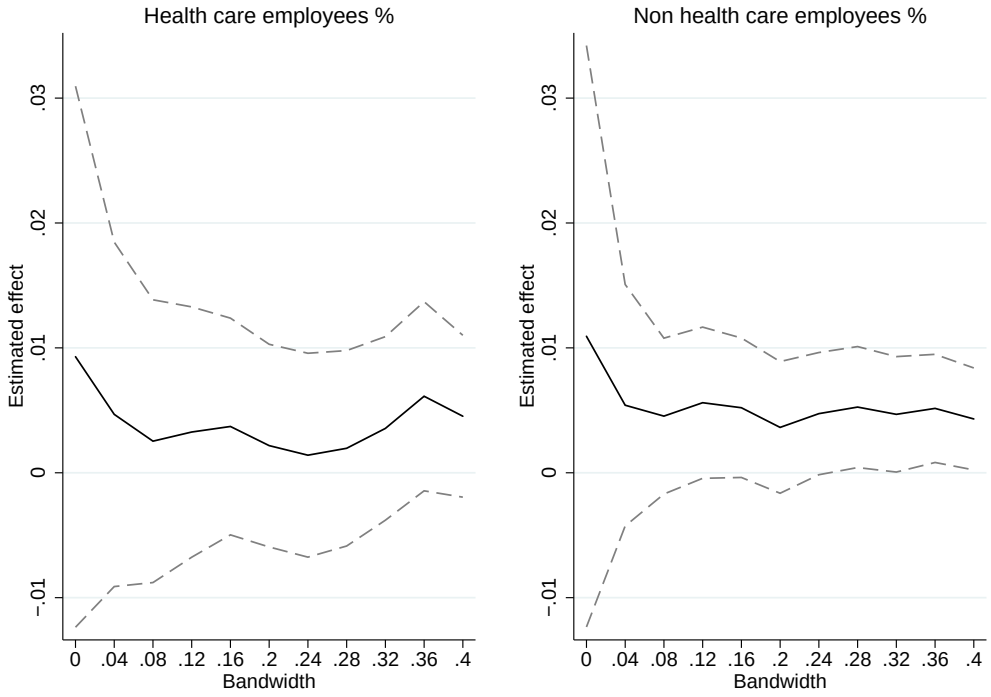


Figure D3. Robustness of the non-health expenditures results with respect to bandwidth choice.

Notes: The solid line represents the point estimates and the dotted lines the 95% confidence interval. The specification includes year dummies as well as control for parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

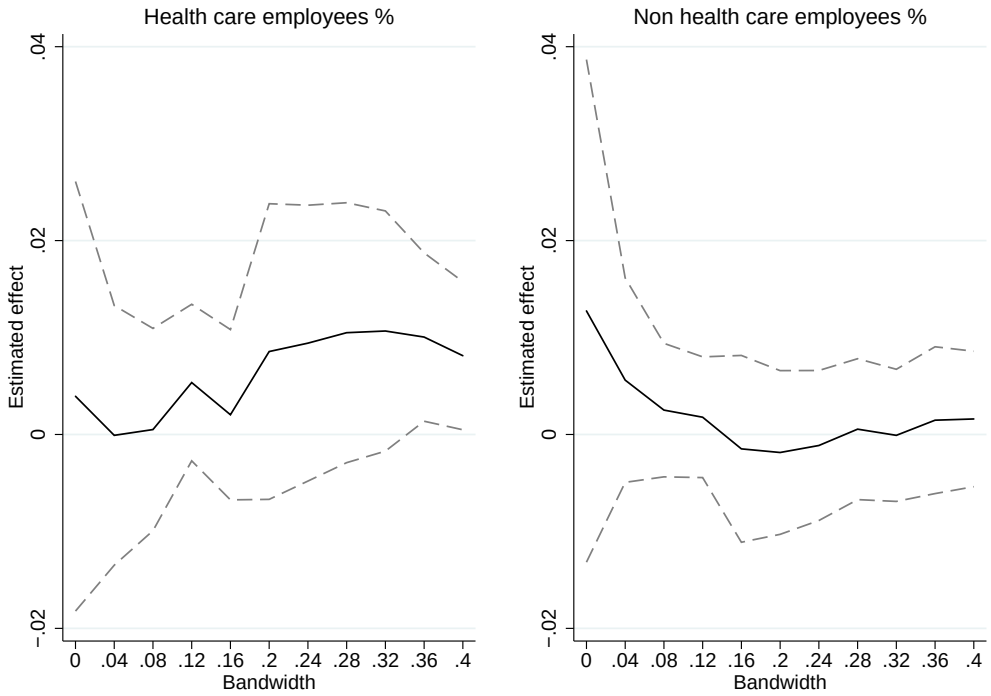


Figure D4. Robustness of the health expenditures results with respect to bandwidth choice.

Notes: The solid line represents the point estimates and the dotted lines the 95% confidence interval. The specification includes year dummies as well as control for parties' seat shares, population, squared population and shares of young and old citizens (all controls are lagged). Standard errors are clustered at the municipality level.

Table D3. In Table D3, we show robustness to accounting for the correlation between the municipal employee status and gender by instrumenting also for the female seat share in the council. While the IV results are not statistically significant (Panel A), the reduced form estimations deliver very similar estimates to the ones reported in the main text. All in all, also the sectoral results appear to be robust to the inclusion of the (instrumented) female seat share.

Table D3. Results for sectoral expenditures: IV analysis with $\varepsilon = 0.4$ for both sectoral municipal employee and female instruments.

	Outcome: non health care expenditures	Outcome: health care expenditures
Panel A: IV, $\varepsilon = 0.4$	(1)	(2)
Municipal non health care employees	0.0037	0.0006
	[0.0023]	[0.0038]
First stage Angrist-Pischke *F*-statistic	20.36	20.36
Municipal health care employees	0.0033	0.0062
	[0.0034]	[0.0038]
First stage Angrist-Pischke *F*-statistic	22.72	22.72
Female	0.0018	0.0028
	[0.0017]	[0.0032]
First stage Angrist-Pischke *F*-statistic	57.57	57.57
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)
Municipal non health care employees	0.0038*	0.0012
	[0.0020]	[0.0035]
Municipal health care employees	0.0020	0.0056*
	[0.0031]	[0.0034]
Female	0.0024	0.003
	[0.0017]	[0.0031]
R^2	0.43	0.18
N	1534	1534
Year dummies	Yes	Yes
Party and municipality controls	Yes	Yes

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable in all the models is the logarithm of the mean of per capita total expenditures over the council term. Standard errors are clustered at the municipality level and reported in brackets. Party controls include parties' lagged seat shares. Municipality controls include lagged population, squared population and shares of young and old citizens. The reported first stage Angrist-Pischke *F*-statistics of individual endogenous regressors are produced by the ivreg2 command in STATA. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Online Appendix E: Rent-Seeking Results

Table E1. In this table, we report results concerning whether municipal employees enjoy larger returns to office in terms of receiving larger salary increases and/or facing smaller unemployment risk, and whether they benefit from a larger incumbency advantage than the other candidates. To do so, we regress a dummy variable for getting elected at election period *t* on four different outcomes: change in (log) wage from *t* to *t*+1, being unemployed in *t*+1, getting elected in *t*+1 and vote share in *t*+1. We control for individual characteristics in some of the specifications. These controls include gender, age, incumbency status, unemployment status, student dummy, entrepreneur dummy, high professional dummy, party affiliation and vote share *t*–1. We estimate the effect separately for municipal employees and other candidates and use a sample of candidates who were tied for the last seat within their party list ("lottery sample"). Thus, the treatment status is randomized in these regressions (see Hyytinen et al. 2017 for details). We do not find any statistically significant differences between municipal employee politicians and others in terms of the returns to office.

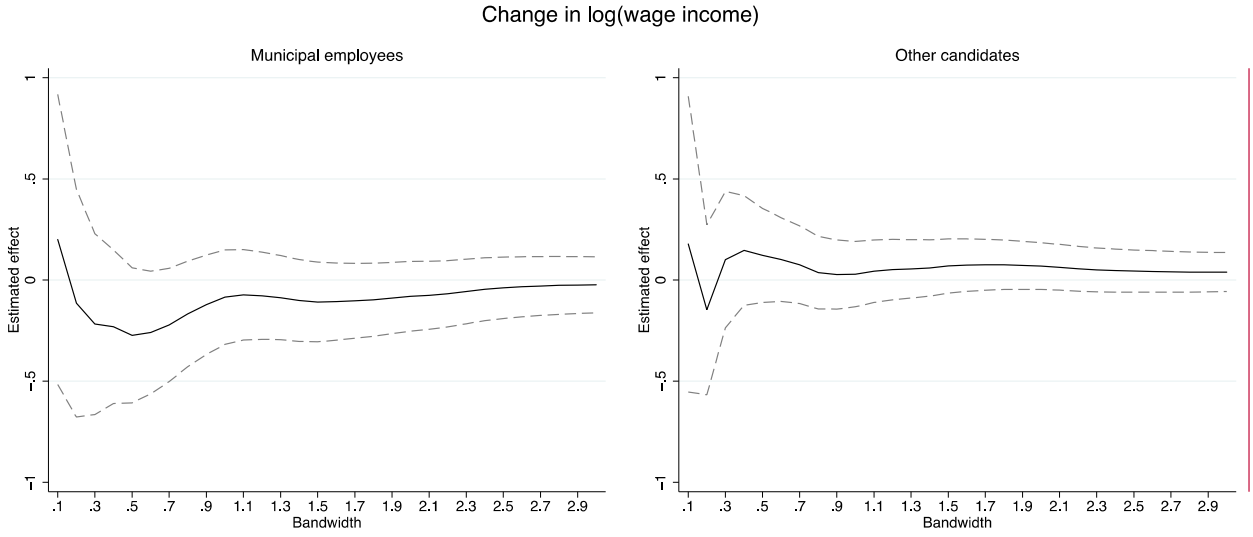
Table E1. Returns to office for elected municipal employees and other candidates.

	Panel A: Change in log(income)			
	(1)	(2)	(3)	(4)
Elected	0.0757	0.0047	-0.1843	-0.1548
	[0.0760]	[0.0725]	[0.1570]	[0.1661]
N	148	148	521	521
R^2	0.01	0.21	0.00	0.05
	Panel B: Unemployed *t*+1			
	(5)	(6)	(7)	(8)
Elected	0.0104	0.0046	0.0033	-0.0006
	[0.0219]	[0.0228]	[0.0123]	[0.0124]
N	202	202	584	584
R^2	0.00	0.04	0.00	0.11
	Panel C: Elected *t*+1			
	(9)	(10)	(11)	(12)
Elected	0.0373	0.0332	0.0027	0.0043
	[0.0508]	[0.0521]	[0.0288]	[0.0291]
N	324	324	974	974
R^2	0.00	0.05	0.00	0.04
	Panel D: Vote share *t*+1			
	(13)	(14)	(15)	(16)
Elected	0.0966	0.0207	-0.0518	-0.0519
	[0.1372]	[0.1348]	[0.0887]	[0.0854]
N	197	197	594	594
R^2	0.00	0.18	0.00	0.23
Sample	Municipal employees		Other candidates	
Individual characteristics	No	Yes	No	Yes

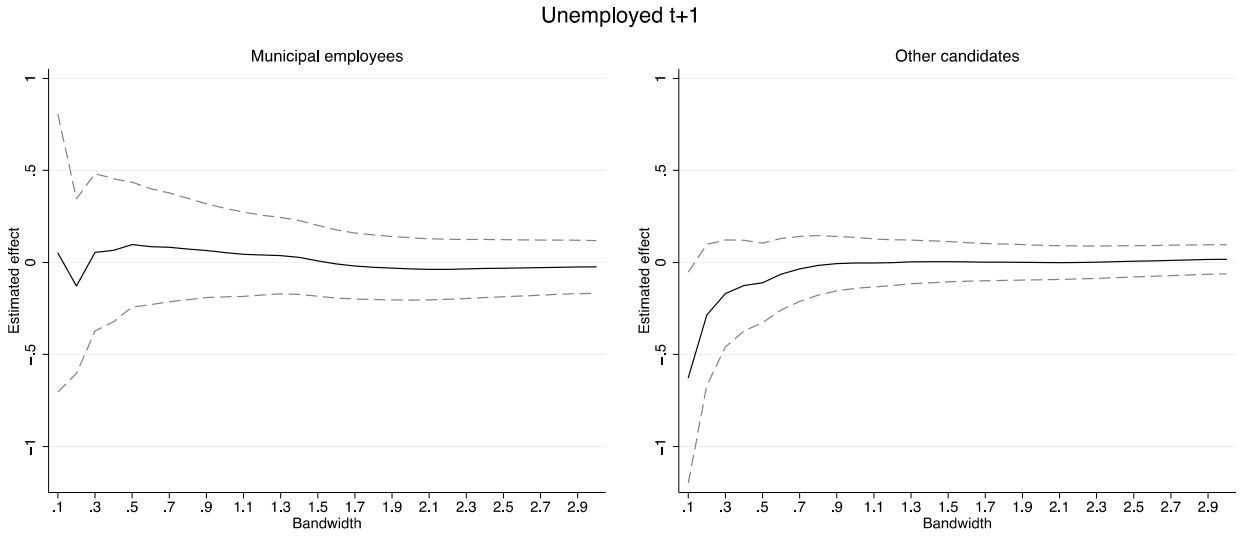
Notes: Unit of observation is individual candidate at election period *t*. Individual characteristics include gender, age, incumbency status, unemployment status, student dummy, entrepreneur dummy, high professional dummy, party affiliation and vote share at *t*–1. In panel B, we include only the candidates that are employed at time *t* to make the other candidates group comparable to municipal employees group. In panel C, candidates who do not re-run have elected *t*+1 status of zero. In panel D, those who do not re-run are excluded. Standard errors are clustered at the municipality level and reported in parentheses.

Figure E1. In Figure E1, we plot the regression discontinuity estimates across a wide range of bandwidths for the same outcomes as reported in Table E1. As already suggested by Table E1, there are no statistically (or economically) significant differences between municipal employee and other politicians' outcomes.

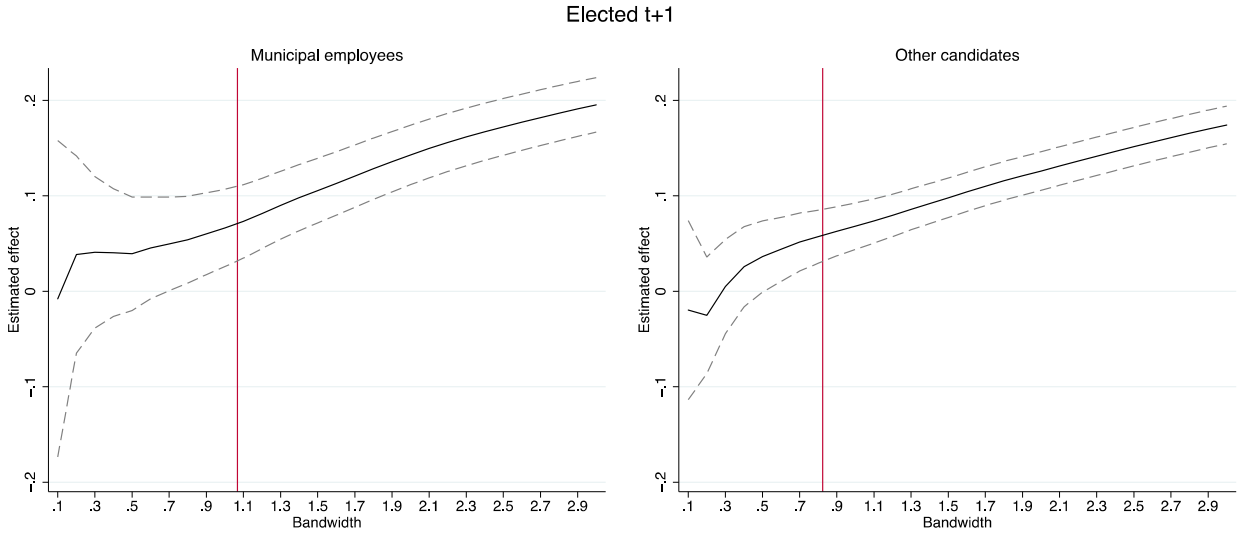
Panel A: RDD effect of getting elected at *t* on change in log of wage income for a range of bandwidths.



Panel B: RDD effect of getting elected at *t* on unemployment at *t*+1 for a range of bandwidths.



Panel C: RDD effect of getting elected at *t* on elected at *t*+1 for a range of bandwidths.



Panel D: RDD effect of getting elected at *t* on vote share at *t*+1 for a range of bandwidths.

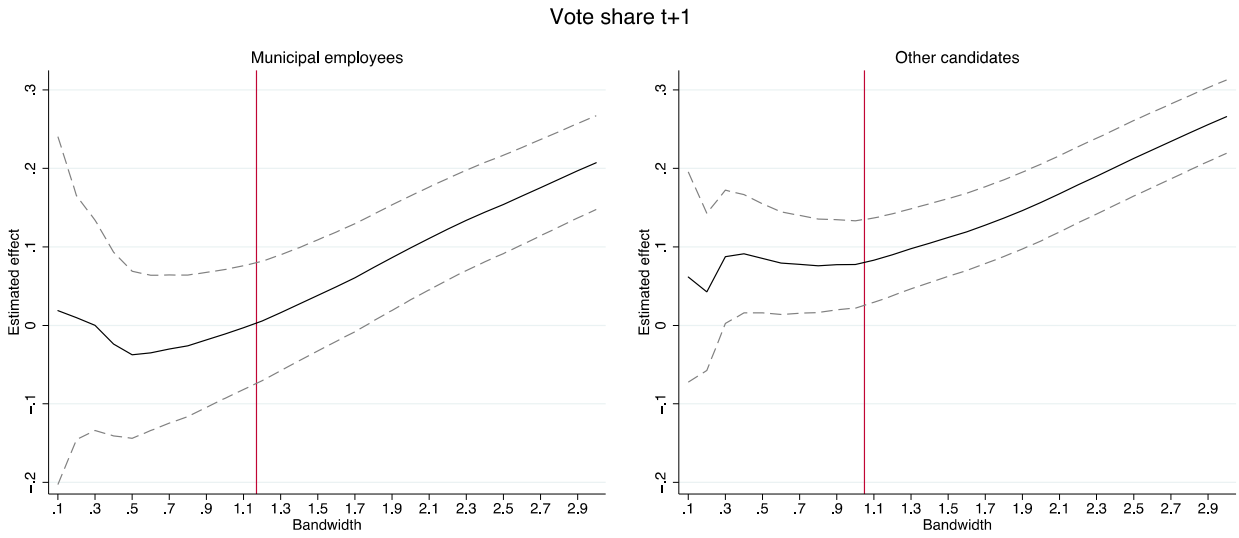


Figure E1. RDD estimates for returns to office for a wide range of bandwidths.

Notes: The solid line represents the point estimates and the dotted lines the 95% confidence interval. The results are from the conventional local linear RD specifications for various bandwidths. Standard errors are clustered at the municipality level. In all the panels, the left hand graph applies to the sample of municipal employees and right hand graph for the other candidates. The red line marks the (clustered) MSE-optimal bandwidth (Calonico et al. 2016).

Table E2. We then turn to our analysis of municipal house prices. We exclude 309 municipality-election period observations from the sample because these small municipalities do not have many housing market transactions. Table E2 reports the effect of municipal employees on (log) house price per square meter. We find no effect on house prices.

Table E2. Results for house prices.

Outcome: log(house price per m^2)		
Reduced form of IV, ε = 0.4	IV	Reduced form of IV
Municipal employees	-0.0001	
	[0.0020]	
Municipal employees		-0.0001
		[0.0020]
First stage Kleibergen-Paap *F*-statistic	64.71	
R^2		0.77
N	1235	1235

Notes: The unit of observation is a municipality *m* in election period *t*. The dependent variable is the logarithm of the mean of per square meter house prices over the council term. Standard errors are clustered at the municipality level and reported in parentheses. Controls include year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Figure E2. In Figure E2 we explore the stated preferences of municipal employee candidates and the candidates from other occupations with respect to questions concerning the role of municipal employees in local politics. We use survey data from the Finnish Broadcasting Company (YLE) concerning the 2012 municipal elections. The data are from an election aid survey in which both candidates and voters respond to the same set of questions and the software application provides voters with information on the best matches.

Firstly, according to Figure E2, municipal employees oppose more strongly firing of municipal employees in connection with municipal mergers compared to other candidates. In particular, there is a rule in Finland which prevents municipalities from dismissing (redundant) employees during the period of five years after a municipal merger. Municipal employees who run for a council disagree more often with the statement that this period is too long. Secondly, they oppose more strongly restrictions on nomination of municipal employees in municipal boards.

Furthermore, in a recent article that dealt with the political power of public sector employees in Finnish municipal councils, the Finnish National Broadcasting company YLE also cited the survey answers given by municipal council election candidates. For example, YLE reported that “80% of those candidates that are municipal employees think that privatization of health services brings neither efficiency gains nor savings to municipalities. 67% of other candidates shared this opinion.”

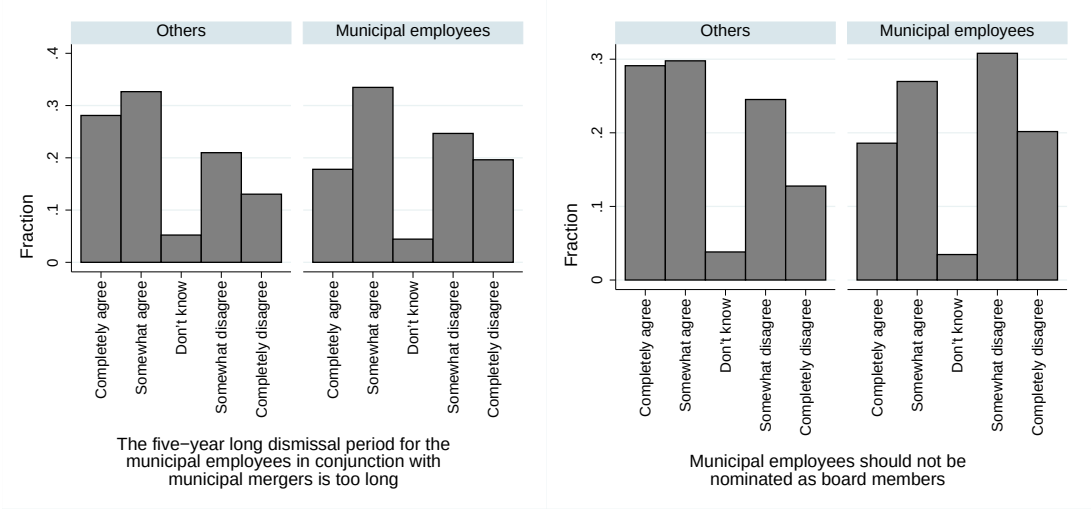


Figure E2. Survey responses ($N = 4215$).

Online Appendix F: Validity of the Female Instrument

Figure F1. In this figure, we present the first stage of IV for the female instrument using various bandwidths while first controlling only for the year fixed effect (figure on the left) and then using all municipality and party controls (figure on the right). We cannot reject the null hypothesis that it is unity regardless of the bandwidth size.

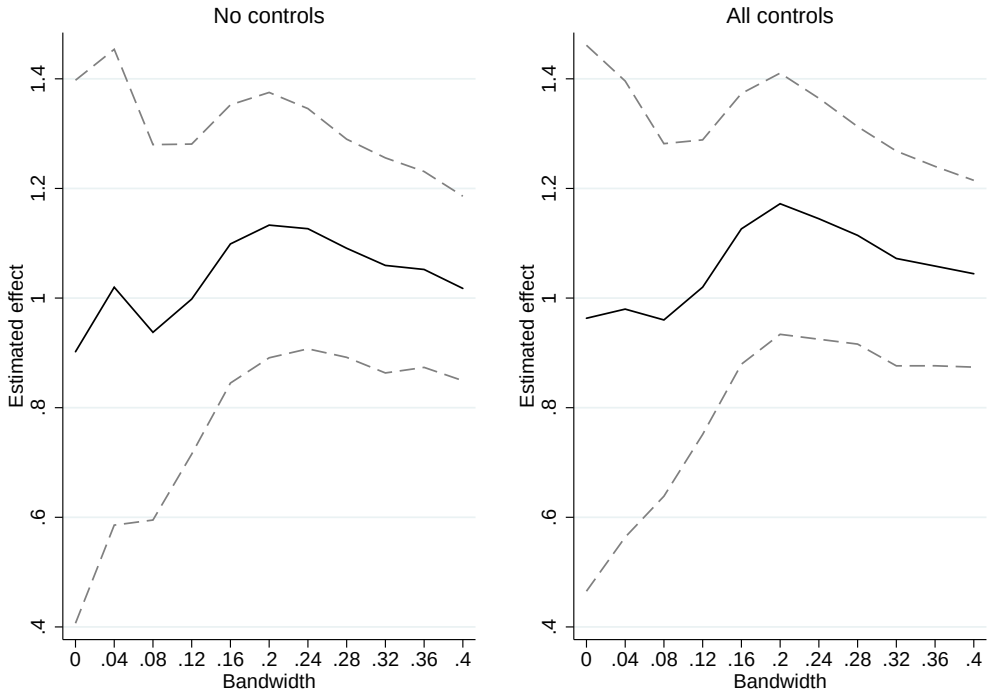


Figure F1. First stage of IV for the female instrument.

Notes: The solid line represents the first stage point estimates and the dotted lines the 95% confidence interval. The left hand graph includes only the year dummies as controls. The right hand graph includes year dummies, parties' lagged seat shares, municipality population, squared population and shares of young and old citizens (all lagged). Standard errors are clustered at the municipality level.

Table F1. In this table, we demonstrate that the pre-treatment covariates are balanced also for the female instrument (F_{mt}).

Table F1. Pre-treatment covariate balance at municipality level for female instrument.

	$F_{mt} > 0$			$F_{mt} < 0$			
$\varepsilon = 0.4$	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	428	5 382	863	485	5 272	778	110.00
Health care expenditures (€ per capita)	427	1 653	361	483	1 623	366	29.95
Other expenditures (€ per capita)	427	3 729	678	483	3 649	635	79.90
Population	596	14 154	33 116	674	14 708	43 222	-553.71
Young inhabitants %	596	18.53	3.20	674	18.83	3.39	-0.30
Old inhabitants %	596	17.98	4.59	674	17.72	4.48	0.26
Council size	596	30.62	11.30	674	30.36	11.09	0.26
Municipal employees %	427	28.85	13.73	483	27.34	12.79	1.51*
Municipal health care employees %	427	7.14	5.08	483	7.30	4.83	-0.16
Municipal non-health care employees %	427	21.71	12.80	483	20.04	11.90	1.66
Incumbents %	427	57.53	8.85	483	57.46	8.87	0.07
Women %	427	33.14	8.69	483	33.24	8.65	-0.10
Instrument for women	427	-0.05	0.12	483	-0.29	0.11	0.24
High professionals %	427	21.23	11.72	483	22.41	12.48	-1.17
University educated %	427	12.77	9.47	483	13.18	10.07	-0.41
Unemployed %	427	3.78	4.19	483	3.81	3.95	-0.03
Center Party seat share %	596	38.22	21.51	674	38.22	21.44	0.00
Coalition Party seat share %	596	16.13	10.47	674	16.19	10.31	-0.06
Social Democratic Party seat share %	596	21.03	11.61	674	21.30	11.98	-0.27
Green party seat share %	596	2.03	3.50	674	2.04	3.76	0.00
Left Alliance seat share %	596	9.18	8.52	674	8.59	8.43	0.59
Swedish Party seat share %	596	4.98	16.97	674	5.91	19.18	-0.93
True Finns seat share %	596	1.78	3.91	674	1.62	3.86	0.15
Christian Democrats seat share %	596	2.89	3.68	674	3.01	3.71	-0.12

Notes: The statistical significance is tested using a *t*-test adjusted for clustering at the municipality level. ***, ** and * denote statistical significance at 1 %, 5 % and 10 % level, respectively.

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