VATT Working Papers 78

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Valtion taloudellinen tutkimuskeskus
VATT Institute for Economic Research
Helsinki 2016

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We thank Manuel Bagues, Thushyanthan Baskaran, Matz Dahlberg, Dominik Hangartner, Robert Inman, Tatiana Komarova, Benjamin Lauderdale, Eva Mörk, Oskar Nordström Skans, Heikki Pursiainen, Marko Terviö and Guy Whitten for helpful comments. Seminar audiences at AEA San Francisco, BI Oslo, FEEAM Pori, EPSA Vienna, HECER Helsinki, IIES, IIPF Dublin, Uppsala and VATT Helsinki provided great feedback. We are grateful to the Emil Aaltonen Foundation for funding.

ISBN 978-952-274-177-6 (PDF)

ISSN 1798-0291 (PDF)

Valtion taloudellinen tutkimuskeskus
VATT Institute for Economic Research
Arkadiankatu 7, 00100 Helsinki, Finland

Helsinki, September 2016

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VATT Working Papers 78/2016

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Abstract

We analyze the effect of municipal employees' political representation in municipal councils on local public spending. To quantify the effect, we use within-party, as-good-as random variation in close elections in the Finnish open-list proportional election system. 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 employees. The results are consistent with public employees having an information advantage over other politicians, and thus, being able to influence policy.

Key words: Close elections, political representation, public employees, public expenditures

JEL classes: C26, D72, H72, H75

1 Introduction

In 2013, public sector employees accounted on average for 21% of the total employment in the OECD countries (OECD 2015). They are therefore a large interest group that can influence politics in various ways. In addition to a direct voting channel (see e.g. Garand 1988, Blais et al. 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 (see e.g. 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 employed by the private sector. A concrete example would be a teacher sitting in a municipal council that decides whether the teacher's school should be closed or not, or a public sector nurse taking part 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 (see e.g. Niskanen 1971 and Romer and Rosenthal 1979).

Consistent with such concerns, most countries have imposed ineligibility rules on the political mandates of public sector employees.[2] 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

[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%.

[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 in other countries.

informed candidates.[3] However, there is surprisingly little evidence that when elected, public sector employees would act differently from the other politicians. We start to fill this gap in the literature by documenting a positive and both statistically and economically significant causal effect of municipal employee representation in a municipal council on local public spending using data from Finland.[4]

The Finnish economy provides a particularly interesting context for such an 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, is that being a municipal councilor is not a full-time job.[6] The task typically takes a few hours a week and the monetary compensation involved is not nearly enough to live on. Therefore, most of the Finnish local politicians have a normal day job. In our data, this means that about one quarter (26%) of the local politicians work for their home municipality. 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]

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

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

[5] Figures for Finland from Statistics Finland Labor Force Survey 2015 (see http://www.tilastokeskus.fi/til/tyti/2014/02/tyti_2014_02_2014-03-25_tau_009_en.html, accessed 6.12.2015).

[6] The same applies, e.g., to the UK (Local Government Association 2012).

[7] For example, the Finnish National Broadcasting company YLE ran at the time of the latest municipal council elections an article with the title "Municipal employees decide on their own jobs in municipal councils". See http://yle.fi/uutiset/kuntien_tyontekijat_paattavat_valtuustoissa_omista_tyopaikoistaan/6334347.

Another reason why Finland provides a suitable context for estimating the effect of public sector employee representation on public spending is that the Finnish open list local elections provide us with plausibly exogenous variation in municipal employee representation. To make use of this variation, we resort to candidate-level close contests within party lists to construct a municipality-level instrument variable for municipal employee representation. This procedure allows us to compare municipalities that, by chance, have marginally more municipal employees in their council to municipalities that, by chance, have marginally fewer. 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 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 from another occupation, increases local public spending. Our estimates suggest that in a municipality with a median-sized council (27 seats), the increase in local public spending is about 1 percent on average over the four-year council term.[8] The effect is surprisingly large for two reasons: First, we are probably looking at a relatively unimportant margin, i.e., the last elected candidates within a party to a council that typically consists of tens of councilors. Second, there are explicit restrictions on the types of political positions that Finnish municipal employees can take. Our result is

[8] The estimated municipal employee effect is robust to simultaneously instrumenting for female council share. This finding is crucial for interpretation of the observed effects, because municipal employment status is correlated with gender. However, unlike e.g. Clots-Figueras (2011, 2012), we find no robust effect on public spending from increased female political participation.

nevertheless in line with the previous findings which show that smaller parties and even individual councilors have an effect on policy in proportional representation systems (e.g. Folke 2014 and Freier and Odendahl 2015).[9]

We also provide evidence on the mechanisms that are at work. First, we find that the effect varies by the type of municipal employee and the type of spending: electing one more employee who works for the health care sector leads to an increase in health expenditures, but not in the other (non-health) municipal expenditures. Similarly, when a non-health care employee gets elected, her getting to hold the office leads to an increase in expenditures unrelated to health care.[10] Moreover, we can show that the positive effect on local public spending arises in particular in close elections that involve the largest party in the municipality. This evidence is consistent with municipal employee councilors influencing intra-party decision making. Such decision making has an effect on municipal policy only if the party is sufficiently large.

Taken together, these findings are 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 the politicians on the costs of providing public services. Analogously, our municipal employee politicians have an information advantage over the other politicians in their party about the provision of public services in their own employment sector, but not in the other sectors.

The sector specificity of the effects largely rule out that municipal employees increase spending because they always prefer a larger public sector (see e.g., Knutsen 2005, Jensen et al. 2009, Rattsø and Sørensen 2016). Our findings also imply that the identities of the

[9] For studies on party effects in the U.S. context, see e.g. Ferreira and Gyourko (2009), Gerber and Hopkins (2011) or de Benedictis-Kessner and Washaw (2016). The effects of political representation of other non-partisan interests groups, such as women and minority and occupation groups, on policy outcomes has been studied by e.g. Pande (2003), Chattopadhyay and Duflo (2004), Gehlbach et al. (2010), Ferreira and Gyourko (2014) and Matter and Stutzer (2015).

[10] Data limitations prevent us from analyzing other specific occupation groups in more detail.

politicians matter, and thus that, e.g., the citizen-candidate model (Osborne and Slivinski 1996 and Besley and Coate 1997) is likely to be a more appropriate description of the Finnish local political decision making than the median voter or Tiebout (1956) competition models (see also Ferreira and Gyourko 2009).

Increased sector-specific spending cannot be automatically attributed to rent-seeking. One reason for this is that municipal employees are experts in their area of employment and can therefore provide useful information to other councilors. Such information provision might lead to improved decision-making. We are unable to find systematic evidence for the extra spending being related to rents that the politicians employed by the public sector potentially get from holding the office (through better employment opportunities, or greater wages; see e.g. Dahlberg and Mörk 2006, Brueckner and Neumark 2014). Nor do we find evidence that the increased spending reflects pro-social behavior or competence of public sector employees (e.g. Best and Cotta 2000, Francois 2000, Besley and Ghatak 2003, 2006).

Even though we cannot differentiate between these competing hypotheses relating to the efficiency of the increased spending, it is definitely noteworthy – though 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 and whose local governments were, together with Italy's, the second most indebted in the OECD (OECD 2015). 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 Section 2, we describe the institutional setting and data. We present our econometric identification strategy in Section 3. Section 4 presents the results and Section 5 concludes.

2 Institutional setting and data

2.1 Finnish local governments

Tasks and revenue sources of municipalities: As, e.g., Saarimaa and Tukiainen (2015) describe, Finland has a two-tier system of government consisting of central government and municipalities as the local level. 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. Health care is the most important spending component. 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. As for revenue sources, the most important tax instrument is the local income tax. The tax rate is flat and determined by the municipalities. In 2012, the average share of the income tax of total revenue was 46 percent. Property and corporate taxes are less important, as in 2012 they accounted for about 3 percent of total revenue, respectively.

There are large regional tax base and cost disparities which are offset by a central government grant system. This nationwide leveling is based on estimates of average costs and tax revenues. The municipalities cannot influence the amount of grants received. The grant system covers about 20 percent of the total municipal revenues, but this share varies a great deal. For every fourth municipality, the system accounts for more than half of the revenues.

Decision-making and elections in municipalities: Municipalities are governed by a municipality council which is the most important political actor. For example, mayors are public officials chosen by the councils and have only limited and only executive power.

Moreover, municipal boards (i.e., cabinets) have only a preparatory role and the representation in the boards follows the same proportional political distribution as the representation in the council.

Municipal elections are held simultaneously in all municipalities and each municipality has one electoral district. The elections in our data were held at the fourth Sundays of October in 1996, 2000, 2004 and 2008.[11] The council term starts the next year and lasts four years: E.g., the council elected in 1996 is in power in 1997-2000.

Within each municipality, the seat allocation is based on the proportional representation, as determined by the open list D'Hondt election rule. In an election, each voter gives a single vote to a single candidate and the voters cannot vote for a party without specifying a candidate, even though each candidate has an affiliation to a party list. The total number of votes over the candidates in a given party list determines the votes for each party. The entire vector of these party votes for all parties 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. In this setting, voters (as opposed to parties) decide which candidates are elected from a given party-list, because the rank of the candidates within the party-list is fully determined by the amount of votes that the candidates on the list get.

There are restrictions on the political roles of municipal employees. First, a municipal employee who is in an executive position in some branch of public service production 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

[11] We do not use 2012 elections because the outcome data are not yet available.

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.

The broader institutional context may also limit the opportunities of the municipal employees to influence outcomes while in office. For example, wages are largely set at the national-level wage bargaining between the municipal employer organization (Local Government Employers KT) and various labor unions. However, individual municipalities can pay more than agreed upon nationally, and sometimes they do: for example rural municipalities often need to attract doctors and other specialists with higher salaries.

2.2 Data

Our data come from a number of sources and refers to individual candidates (politicians) and municipalities.

Candidate and elections data: We have obtained data on municipal elections held between 1996 and 2008 form 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 includes the age and gender of the candidates. Information on municipal employment status comes from KEVA (formerly: Local Government Pensions Institution), and we have linked the candidate data also to Statistics Finland data on education, occupation and socio-economic status and to the income data from the Finnish Tax Authority. Overall we have 160,996 candidate-election observations. We do not have income data for the 1996 candidates and the education data are missing for some candidates.

The characteristics of the candidates running in municipal elections held between 1996 and 2008 are shown in Appendix Table A1.[12] For our purposes, a candidate is a municipal employee, if she was employed by a municipality at the end of the election year.[13] Compared to other candidates, municipal employees are more often female (nurse is the most common profession among them), classified as high professionals in their socioeconomic status and running for the Social Democratic Party. These observable differences in candidate characteristics may confound our econometric analysis, despite the estimations being conducted at the municipality level (see e.g. Clots-Figueras 2011). We return to this issue when we present our econometric approach in detail.

Municipal data: We use Statistics Finland's data on municipal expenditures and demographics for years 1996–2012.

Appendix Table A2 reports the summary statistics of municipality and municipal council characteristics, calculated using 1544 municipality-council term observations. 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%.

3 Econometric approach

3.1 Identification strategy

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

[12] We omit 33 elections because those municipalities underwent a municipal merger during the election term. We also omit 2004 data for two municipalities (that merged) due to ambiguities in the candidate-level election data. It seems that the ambiguity results from a popular candidate being disqualified.

[13] 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.

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

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 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 employ two methods to estimate the treatment effect of interest (δ). First, we use an instrumental variable (IV) estimator, using a close-elections approach similar to Clots-Figueras (2011, 2012). Our instrument measures 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. 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'. Second, to preserve power, we will invoke the structure of our estimation problem which implies that the coefficient of our instrument in the 1st stage of the IV should in expectation be one. This feature means that in the reduced form of our IV of equation (1), the coefficient of the instrument ought to be very close to the IV estimate of δ.

Unlike much of the recent literature on close elections, the Finnish municipal election system of proportional representation with open party lists does not easily render itself to a

simple regression discontinuity design (RDD) analysis at the municipality-level (see e.g. Lee et al. 2004). 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. Our procedure uses as-good-as random variation at candidate-level in the close elections and aggregates this variation to a municipality-level instrumental variable. To properly capture the treatment effect of political representation of municipal employees on municipal expenditures, we focus on closeness *within party lists*. This choice means that between-party changes 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.[14]

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 multiply it by 100. We denote the variable thus obtained v_{ipmt}.[15] Closeness of each candidate i in party list p in municipality m in election t, C_{ipmt}, is then defined as

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

[14] A small number of recent studies have explored close contests that take place within parties in proportional elections in Finland: Kotakorpi et al. (2016) use them to study the returns to holding political office. Hyytinen et al. (2014) study incumbency advantage and evaluate the performance of close elections RDD using the same Finnish local elections that we study here. Unlike these prior papers, we are interested in municipal level outcomes.

[15] Note that v_{ipmt} cannot be defined for party lists where none of the candidates get elected or all of the candidates get elected. In total, this means that 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. 2014 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.[16] 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 the 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.[17] The reason for using Eq. (3) is that there may be more than

[16] Simply "adding up" candidate level treatments would *not* be appropriate. To see why, consider three municipal employees who are close and compete for one seat. There is no useful variation at the party level in this case, because the outcome cannot be anything else but a municipal employee getting elected.

[17] In an urn problem, the expected value is $n(K/N)$ (both 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.

two candidates that are close and thus subject to randomization and any number of the close candidates can be municipal employees. 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 (and where there is thus no need to consider the expected number of elected municipal employees when constructing the instrument).

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. If, in a given municipality, municipal employees were lucky within one party list and equally unlucky in another, the treatment 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.

Empirically, T_{mt} appears to work as expected (see Appendix A for details of these analyses): First of all, it is symmetrically distributed around zero. Moreover, 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.[18] As we show in Appendix B, this is indeed empirically the case in our data. Finally, even with the smallest possible bandwidth ($\varepsilon = 0$), we have variation in T_{mt}. The reason for this is that there are many parties in each municipality and many ties. Thus, lotteries can take place in any one of them. As we increase the bandwidth, almost all of the municipalities in our data have a close contest within at least one of its party lists. For example, for bandwidth $\varepsilon = 0.4$, we observe either a positive or a negative treatment (as captured by T_{mt}) in 1145 municipalities out of 1544. This does *not* imply that we would use all the variation in the municipal employee council seat share in the data for these 1145 municipalities: We only use the random part of the variation in the seat share (as explained above) for identification of δ.

Two final points about the procedure of constructing our instrument are worth mentioning. First, there is an RDD flavor to our approach. However, we do not have a well-defined forcing variable at the municipality level (even though we have one at the individual candidate level). Second, our procedure can be adapted to other political systems and settings. For example, it can be used to analyze party effects in plurality systems, where quasi-randomization takes place within districts and where such variation needs to be aggregated.

[18] The first stage coefficient equals unity only asymptotically, because municipal employee candidates may be lucky in municipalities where their council share would otherwise have been low (and vice versa).

3.2 Validity tests

In Table 1, we report the covariance balance tests for the narrowest possible bandwidth (ε = 0) and the largest bandwidth that we use in the regressions (ε = 0.4).[19] The smaller bandwidth leads to less precise estimates, because there is less variation in T_{mt}, but the assumption of "as-good-as random assignment" is more plausible for it. 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.

According to Table 1, the pre-treatment variables are well balanced, including the lagged total expenditures, the lagged municipal employee share in the council and its lagged instrument. 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.[20]

Table 2 reports balance tests on council characteristics for the *current* election term.[21] We should take a closer look at them, because municipal employees are more often female and have higher socioeconomic status than the candidates that have other employment status (cf. Table 1). As Panel A of Table 2 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

[19] 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.

[20] We also test covariate balance using regression that controls for year fixed effects. When ε = 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.

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

and education. The only exception to the good balance is the councils' gender composition. This finding mirrors the strong positive correlation between gender and occupation status at the candidate-level. The imbalance is not, however, 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.

Our candidate-level data allows a closer look at the gender imbalance. In Panel B of Table 2, we report the balance tests based on gender. For these tests, we divide the municipal election observations into two groups, depending on whether the seat share of females exceeds or falls short of its expected share. The procedure we used to calculate this gender difference between the realization and outcomes is the same as the one we used for the municipal employees. As the table reveals, the councils that have by chance more females than males also have more municipal employees, but there is no imbalance in the other observed characteristics. In Panel C and D 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. We use this division, because the health care sector is the largest single expenditure category of the Finnish municipalities. The division also allows us to analyze whether the positive correlation between municipal employment status and gender is driven by the health care sector employees. This would be intuitive, because nursing is a female-dominated occupation. From Panel C and D of Table 2, it is indeed evident that the gender imbalance is related to employees in the health care sector. We explore the importance of gender for our econometric findings in greater detail below.

Table 1. 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 2. Post-treatment council covariate balance.

ε = 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: Female	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	596	57.12	8.62	674	56.92	9.35	0.20
Municipal employees %	596	27.62	12.44	674	26.33	12.26	1.28*
High professionals %	596	21.71	12.15	674	22.46	12.17	-0.75
University educated %	596	13.44	10.27	674	13.55	10.20	-0.10
Unemployed %	596	3.63	4.06	674	3.34	3.92	0.29
Panel C: 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 D: 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 gender. In Panel C, the groups are based on health care sector employees. In Panel D, the groups are based on those municipal employees who do not work in the health care sector. The bandwidth in each case is 0.4. 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.

4 Results

4.1 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 both naïve OLS results with different sets of controls (Panel A of Table 3) and the IV results (Panel B

of Table 3) and the reduced form of IV (Panel C of Table 3), using the narrowest possible bandwidth with $\varepsilon = 0$. 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 very well. 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. This what the results reported in Panel B and C bear out: The magnitude 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 F	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 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 includes a second order polynomial of the municipal employees vote share. The first stage *F*-statistic reported for the IV estimations is the Kleibergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Main regression results: To explore whether we can estimate the (apparently positive) 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 F	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 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 includes a second order polynomial of the municipal employees vote share. The first stage *F*-statistic reported for the IV estimations is the Kleibergen-Paap Wald *F*-statistic. ***, ** 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 0.0034-0.0041 on the 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 suggest that 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 given that there are non-negligible institutional restrictions on the political representation of the municipal employees and that we are identifying the effect at a potentially unimportant of margin of allocating the last seats to the council.

A closer look at gender effects: Do 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 directly control for the seat share of females, F_{mt}. 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 hence treat F_{mt} symmetrically to M_{mt}, i.e., either instrument it or replace it in the reduced form directly with the instrument. When F_{mt} is included in the model, we get at the effect of electing a municipal employee while keeping the gender composition 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, F_{mt} 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 (see Appendix B). Somewhat surprisingly, 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. They are well represented in the Finnish political decision making to start with. Indeed, Finland was third in the world to allow female suffrage in 1906 and in our data, the share of female councilors is relatively high, at about 40%.[22]

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

Second, 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 bandwidths. This choice may lead to a selection bias if the municipalities implicitly chosen by the bandwidth choice are different from those that remain outside the bandwidths. We have therefore replicated the results of Table 4 using only those observations in which close elections take place. This amounts to omitting the observations for which the instrument variable is zero. The point estimates from these estimations are almost identical to those reported in Table 4, but standard errors are slightly larger. The estimates nonetheless are mostly statistically significant.

[22] Some research have found a positive correlation between female political representation and public spending in countries more similar to Finland, but typically not using research designs that plausibly identify causal effects (see e.g. Svaleryd 2009).

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 using any of these placebo thresholds.

Finally, we have explored the covariate balance in the close sample (as defined by the choice of bandwidth ε) and the rest of the municipalities. For example, for $\varepsilon = 0$ the covariates balance perfectly. On the other hand, for $\varepsilon = 0.4$, the close sample is different from the other municipalities, because much larger municipalities select into the close sample.[23] However, it is unlikely that this selection compromises the validity of our findings, because our point estimates are robust to changing the bandwidth.

4.2 Mechanisms

Our results show that even though the councilors that we use to identify the municipal employee effect are marginal, they *do* influence local public spending. This is intriguing, because one could argue that these councilors are not the most influential or prominent members of the council. Moreover, the non-elected marginal candidates are typically vice-

[23] 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 small. For example, with a two vote distance to the threshold, the party list needs to be larger than 500 votes for the candidate to be within the bandwidth. Such small bandwidths realize more often in larger municipalities, because in them, the total number of party votes is large enough to generate small vote shares.

councilors who get to attend council meetings if the councilor is absent or may get a council seat if elected councilors step down. The vice-councilors are sometimes given positions also in the municipal sub-committees. We have also shown that the result is not driven by the gender of the elected municipal employees. How, then, does our surprisingly large effect come about?

We are able to rule out a number of possibilities: First, we can rule out that the marginally elected municipal employee councilors would lead to them having a majority in the council or to their party becoming dominated by municipal employees: Such instances are present in the data only very rarely. This suggests that a direct voting mechanism is unlikely to explain the increase in spending.[24] Second, the municipal employee effect appears not to be 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 (not reported).

Even though the marginally elected councilors are probably not the most prominent members of the council, could they have a disproportionate effect within and thus via their party? To consider this possibility, we look at the heterogeneity of the spending effect by party, in particular, whether the effect is different within the largest party than within the second largest party. We report these results in Table 5, where again Panel A reports the IV estimates and Panel B the reduced form estimates. We find a significant effect for the largest party. In contrast, the estimates are smaller and insignificant for the second largest party. This result bears on the literature on coalitional bargaining (e.g., Ansolabehere et al.

[24] Furthermore, a single councilor is fairly unlikely to be pivotal in council with a median size of 27 councilors often enough in our data to explain our findings.

2005), as it indicates that non-partisan interest groups, such as municipal employees, may be able to influence decision making *within* the party. If this party is large, the non-partisan interest groups may have a disproportionate effect on the policy.[25]

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

	Largest party	2nd largest party
Panel A: IV, $\varepsilon = 0.4$	(1)	(2)
Municipal employees	0.0048**	0.0022
	[0.0019]	[0.0034]
First stage F	78.78	43.57
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)
Municipal employees	0.0049**	0.0020
	[0.0020]	[0.0032]
R^2	0.57	0.57
N	1544	1544
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. First stage *F*-statistic reported for the IV estimations is the Kleibergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

To shed further light on the potential mechanisms of influence, we explore whether the link is occupation specific. It is certainly 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

[25] 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.

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. All the specifications include year fixed effects as well as the party and municipality controls (i.e., the specification is the same as in column (3) and (7) of Table 4). The method of estimation is IV in Panel A, and Panel B presents the corresponding reduced form of IV estimates.[27]

As can be seen from the table, the results suggest that health care employees increase health care expenditures, but non-health care 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. Spending increases thus seem to be confined to the sectors that have, by chance, more representation through municipal employees in the municipal council. 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).

Consistent with the posited information advantage of bureaucrats (Niskanen 1971, Romer and Rosenthal 1979), these results suggest that information advantage of the municipal employees of their own employment sector makes it possible for them to influence spending patterns. 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. Intra-party bargaining is an example of an indirect mechanism that can generate the observed sector specific effects. These results also largely rule out the

[26] We cannot easily disaggregate other spending into more specific categories, such as schooling, because the data get sparse (i.e., candidates at finer level occupations are involved in close elections too infrequently for empirical analysis).

[27] The results for pre-treatment covariate balance tests and the first stage estimations of the IV are presented in Appendix C.

explanation that municipal employees increase spending because they generally prefer a larger public sector.

The results reported in Table 6 are robust to adding the seat share of females to the models (see Appendix C). 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.

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	0.0045**	0.0016
	[0.0021]	[0.0036]
Municipal health care employees	0.0033	0.0081**
	[0.0033]	[0.0039]
First stage F	29.73	29.57
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)
Municipal non health care employees	0.0044**	0.0019
	[0.0021]	[0.0035]
Municipal health care employees	0.0025	0.0076**
	[0.0031]	[0.0036]
R^2	0.44	0.18
N	1544	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 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. First stage *F*-statistic reported for the IV estimations is the Kleibergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

4.4. Evidence on rent-seeking

The results reported so far are consistent with bureaucrats having information advantage over politicians and thus being able to convince politicians to spend more on public services. It is not easy, however, to determine whether the documented increased sector-

specific spending is due to rent-seeking. Because municipal employees are experts in their area of employment, they may be able to provide useful information to other councilors. This mechanism ought to lead to better, not worse spending decisions.

Our data do not allow us to conclusively determine which type of spending is useful for citizens, or which spending is more likely to be excessive and related to rent-seeking. We have nevertheless investigated this issue by studying whether the elected municipal employees receive larger salary increases and/or face a smaller unemployment risk, and whether they enjoy from a (larger) incumbency advantage in subsequent elections than the other candidates. We have also analyzed whether the political representation of municipal employees shows up in house prices. We explored house prices, because, e.g., Gyourko and Tracy (1991) argue that high levels of government rent extraction might be capitalized in them.

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). Using municipal-level data on real estate transactions, we find no effect on house prices.

These null results mean that we cannot rule out the use of better information in a pro-social way, nor provide systematic evidence for rent-seeking. 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. In particular, there is a rule in Finland which prevents

municipalities from dismissing (redundant) employees 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.[28] We cannot be irrefutable here, but 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.

5 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 increases health care spending and having more non-health sector employees increases non-health care spending, but there are no significant cross-sector effects. Since the spending effects are in this particular sense sector specific, we can rule out that general preferences for a larger public sector are driving the result. Instead, the results are likely to be driven by information advantage that municipal employees command relative to other councilors in their own sector of employment. Third, the effect appears to be related to the interest group influencing the political agenda within the largest parties.

We have shown that in a municipality with a median sized council, the increase in local public spending is about 1 percent on average over the four-year council term. The effect is surprisingly large because we are probably looking at a relatively unimportant

[28] In its 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."

margin (i.e., the last elected candidates within a party) and because there are restrictions on the political positions that the municipal employees can take.

It is important to interpret these findings in the context to which they apply: We have found that the effect of having relatively more municipal employees in the council on public spending is not due to the increased female political participation. Moreover, 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. One can therefore raise the question why, in this context, would an informed and benevolent municipal employee councilor increase rather than decrease public spending? If the effect is, as we showed, driven by the employment sector of the elected councilors and if it only comes through the largest party, how would the possible efficiency-enhancing effects come about? These are important questions that call for further research on the mechanisms at work.

Our evidence supports the view that the identities of the local politicians matter. This finding holds two lessons for contemporary research in economics and political science: First, the predictions from the median voter model or Tiebout competition appear not to fit local political decision making characterized by proportional representation and open list D'Hondt election rule. Other models, such as the citizen-candidate model (see Osborne and Slivinski 1996 and Besley and Coate 1997) or bureaucracy models (see e.g. Niskanen 1971 and Romer and Rosenthal 1979), which allow the politicians' identities to matter, are clearly more in line with the evidence. Second, the marginally elected candidates seem to

be able to influence local policy. This influence is a necessary condition for the vote of a rational pivotal voter to have an impact at the margin. This 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 (see Lyytikäinen and Tukiainen 2013).

One can only conjecture how large effects we would have documented, if there had not been restrictions on political participation of public sector employees in Finland. Because public employees are a large interest group, their opportunities to gain political power and willingness to use it ought not to be overlooked. For example, fairness and equality considerations at the local level call for continuous monitoring of how the opportunities of public employees to participate in politics ought to be regulated.

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Public Employees as Politicians: Evidence from Close Elections

Supporting information

September 19, 2016

This document includes appendices to paper "Public Employees as Politicians: Evidence from Close Elections". Appendix A includes descriptive statistics and additional figures characterizing the instrument employed in our IV estimations. We discuss various robustness and validity checks in Appendices B (main estimations) and C (sectoral effects). In Appendix D, we report additional robustness checks related to party size. Finally, Appendix E shows and discusses a large battery of tests related to rent seeking.

Appendix A: Descriptive statistics and distribution of the instrument

This appendix shows descriptive statistics on candidates and local governments, discussed in Section 2.2. Moreover, we show the distribution of the instrument T_{mt} for various bandwidths. Figures A1 and A2 show how the variation in the treatment increases as the bandwidth increases but the shape of the distribution remains symmetric, thus implying valid randomization.

Table A1. Candidate characteristics.

	All		Municipal employees		Other	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Number of obs.	155 111		35 977		119 134	
Vote share %	1.01	1.21	1.11	1.28	0.98	1.19
Party vote share %	6.05	11.30	6.18	10.53	6.02	11.52
Number of votes	59.3	148.8	68.8	152.0	56.5	148
Female	0.39	0.49	0.56	0.50	0.34	0.47
Age	46.2	12.3	45.1	10.5	46.6	12.8
Incumbent	0.21	0.41	0.25	0.43	0.20	0.40
Wage income (€)	20 307	26 245	22 625	13 129	19 563	29 190
Capital income (€)	1 864	23 056	881	5 153	2 179	26 327
High professional	0.20	0.40	0.31	0.46	0.16	0.37
Unemployed	0.07	0.25	0.05	0.22	0.07	0.26
University degree	0.15	0.36	0.18	0.38	0.14	0.35
Coalition Party	0.19	0.39	0.17	0.37	0.19	0.40
Social Dem. Party	0.22	0.41	0.27	0.44	0.20	0.40
Center Party	0.28	0.45	0.26	0.44	0.28	0.45
True Finns	0.03	0.17	0.02	0.12	0.03	0.18
Green Party	0.04	0.20	0.05	0.22	0.04	0.20
Left Alliance	0.11	0.31	0.11	0.31	0.11	0.31
Swedish Party	0.04	0.19	0.04	0.19	0.04	0.19
Christian Dem. Party	0.04	0.20	0.04	0.20	0.04	0.21
Other parties	0.05	0.22	0.04	0.20	0.06	0.23

Notes: Income data are not available for 2012 elections, and in 1996 elections they are available only for candidates who run also in 2000, 2004 or 2008 elections (number of observations 96040 for the whole sample, 23317 for municipal employees and 72723 for other candidates). We use 1995 occupation data for the elections held in 1996. Due to missing data, the number of observations for high professional and unemployment status are 155035, 23317, 72723, and for university degree 122720, 31247, 91473, respectively.

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.

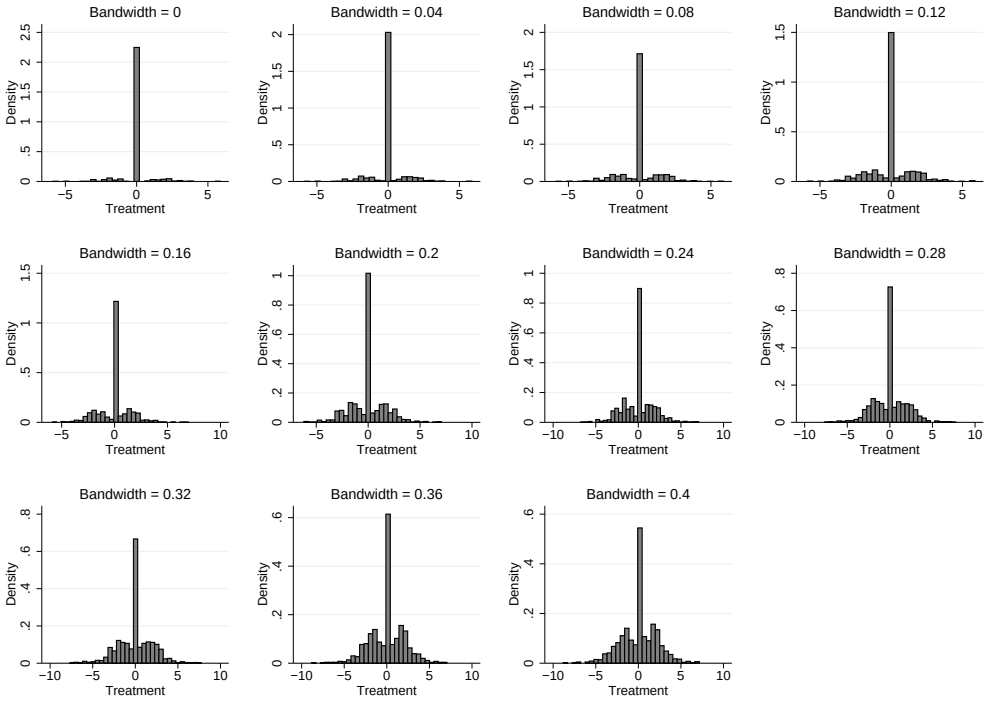


Figure A1. Distribution of T_{mt} .

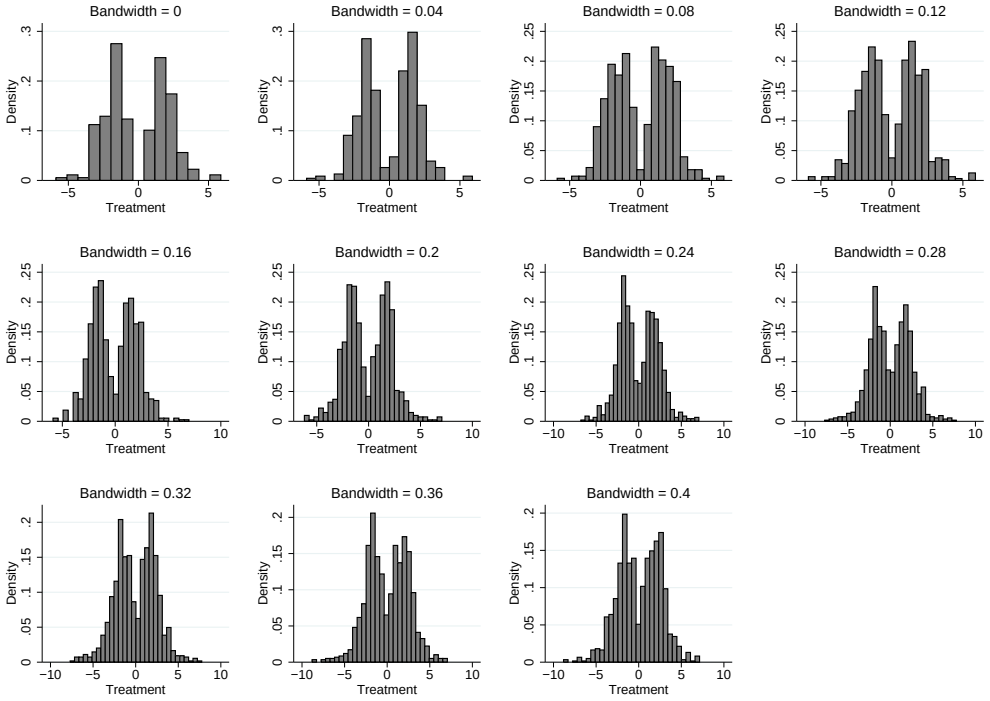


Figure A2. Distribution of T_{mt} (excluding zeros).

Appendix B: Robustness and validity of the total expenditures effect

We present here a number of additional results: First, we report the first stage of our IV across a range of bandwidths as well as show post-treatment balance for more variables than in the main text. Second, we report the robustness tests for the municipal employee results (reported in Table 6 of the main text) over a range of bandwidths. Third, we analyze the expenditure effects separately for each year. Fourth, we compare our close elections sample to the other municipalities and report the robustness of the results to using only the close sample. Fifth, we report results for the IV regression that accounts for the correlation between municipal employee status and gender. In this IV regression, we instrument also the female seat share in the council using the instrument constructed for close contests between female and male. Finally, we report tests for the validity of the female instrument.

One can check whether our aggregation procedure produces a correct municipality level instrument by running the first stage of IV and checking whether the coefficient of T_m (ϕ) is indeed one. This regression can also be used to test for the power of our treatment for various bandwidth sizes. In Figure B1, we present estimates of ϕ for various bandwidths (ε) while first controlling only for the year fixed effect and then for all the municipality controls. The coefficient is below unity when the treatment is calculated using only the lotteries in the data, 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. This anomaly in the lottery sample may simply be a small sample statistical fluke. In particular, the first stage for the treatments when the interest group of interest is non-health care employees or female does not contain this anomaly (see Figures C1 and B5).

The first stage is fairly precisely estimated for bandwidths larger than 0.04 (4 votes out of ten thousand). The control variables do not increase precision substantially. The lottery sample (bandwidth 0) produces noisy results, but 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.

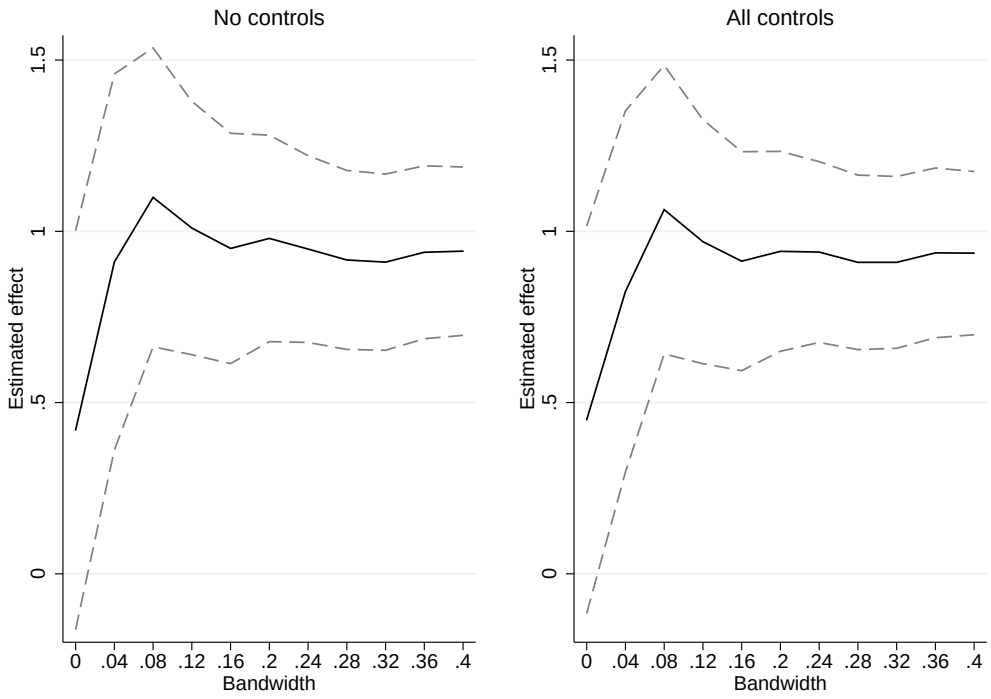


Figure B1. First stage for municipal employees.

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 and the right hand graph includes all the controls used in Table 5 column (3). Standard errors are clustered at the municipality level.

Table B1. 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

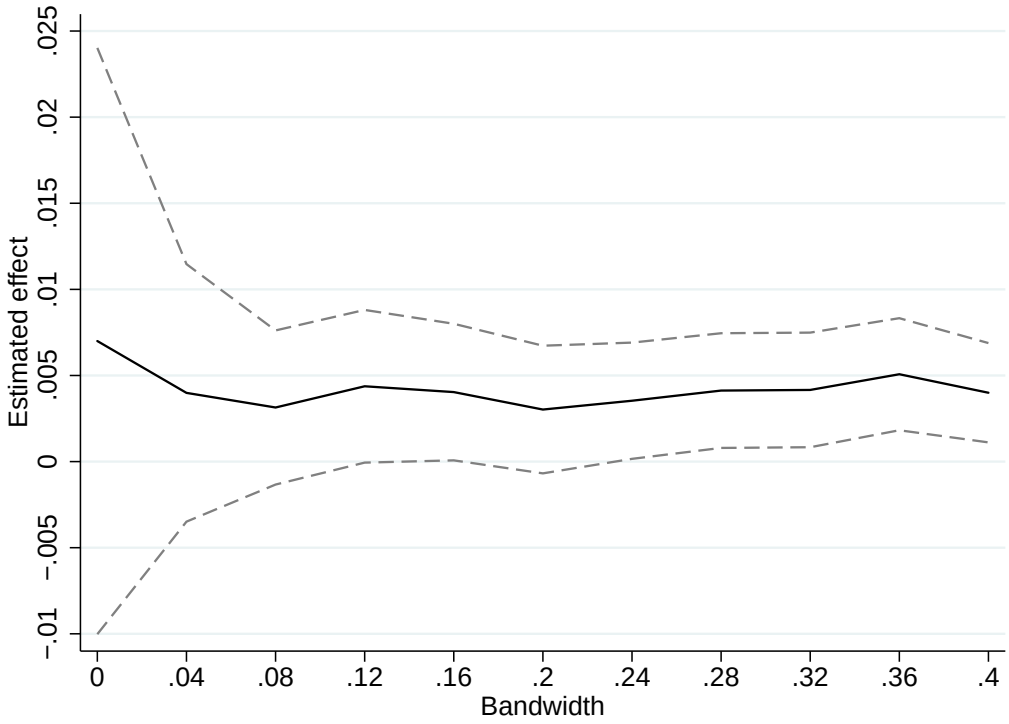


Figure B2. Robustness of the results in Table 6 for different bandwidths.

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.

In Figure B3, 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 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.

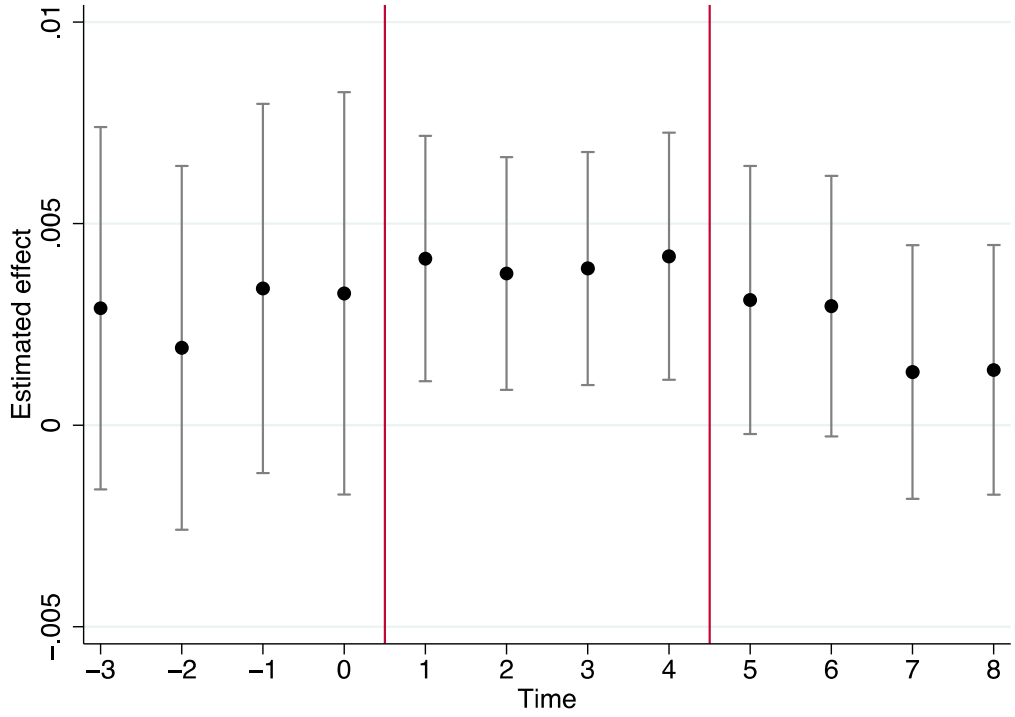


Figure B3. Effect separately for each year.

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.

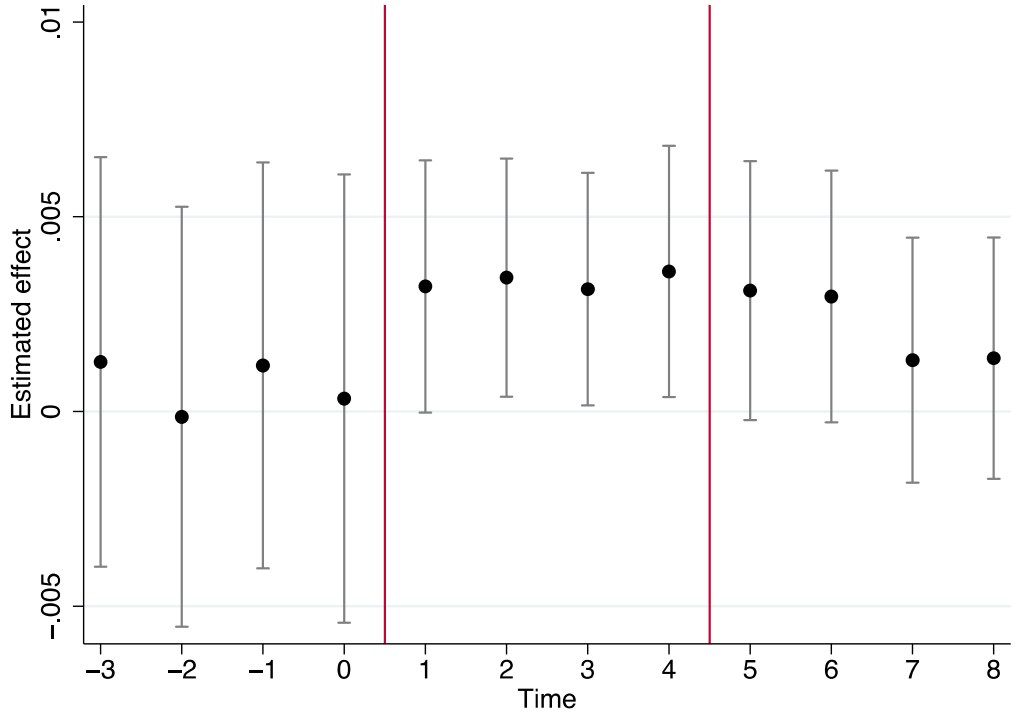


Figure B4. Effects separately for each year excluding data from the last election term.

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 B5 reports 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, which is not surprising due to multiple testing. 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 treatment and the placebo instrument even at the fake cut-offs. Both placebo tests are conducted using $\varepsilon = 0.04$ as the bandwidth.

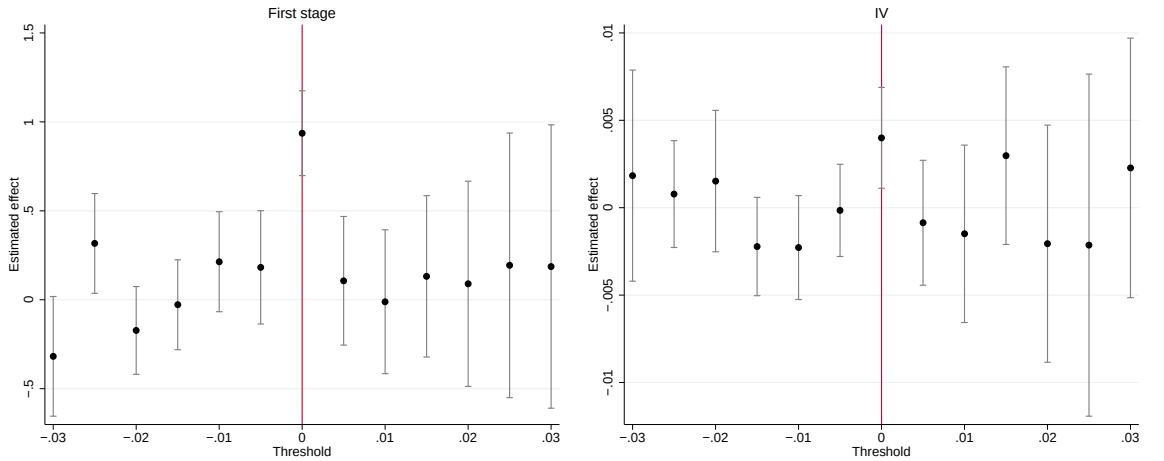


Figure B5. Effects for placebo thresholds.

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 B2. Pre-treatment covariate balance between the close sample and others.

	Close elections			No close elections			
ε = 0 (lotteries)	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	823	301	5 376	919	-46
Health care expenditures (€ per capita)	807	1 634	376	301	1 646	369	-12
Other expenditures (€ per capita)	807	3 697	666	301	3 729	768	-33
Population	1145	15 571	40 687	399	3 773	3 153	11799***
Young inhabitants %	1145	18.65	3.27	399	18.07	3.51	0.58*
Old inhabitants %	1145	17.70	4.54	399	20.04	4.42	-2.34***
Council size	1145	31.25	11.35	399	22.45	5.75	8.80***
Municipal employees %	807	28.03	13.24	301	26.50	13.48	1.53*
Municipal health care employees %	807	7.26	4.94	301	6.44	5.11	0.82*
Municipal non-health care employees %	807	20.78	12.40	301	20.06	12.67	0.72
Incumbents %	807	57.66	8.81	301	55.87	9.40	1.80***
Women %	807	33.41	8.74	301	31.82	9.38	1.59**
High professionals %	807	22.43	12.39	301	15.64	8.84	6.79***
University educated %	807	13.51	9.95	301	8.31	6.61	5.20***
Unemployed %	807	3.69	3.91	301	4.18	4.63	-0.49
Center Party seat share %	1145	37.38	21.16	399	45.20	20.08	7.82***
Coalition Party seat share %	1145	16.56	10.13	399	13.15	10.68	3.41***
Social Democratic Party seat share %	1145	21.63	11.70	399	18.16	11.60	3.47***
Green party seat share %	1145	2.16	3.75	399	0.92	2.32	1.25***
Left Alliance seat share %	1145	9.02	8.51	399	7.15	7.86	1.87**
Swedish Party seat share %	1145	5.10	17.32	399	6.14	20.03	-1.03
True Finns seat share %	1145	1.74	3.85	399	1.79	4.13	-0.05
Christian Democrats seat share %	1145	3.06	3.63	399	2.44	3.96	0.62*
Other parties seat share %	1145	3.34	6.43	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 B3. The effect of municipal employment 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 F	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	0.3	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. First stage *F*-statistic reported for the IV estimations is the Kleinbergen-Paap Wald *F*-statistic. Moreover, we report p-values from testing the joint significance of the treatments. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Table B4. Results for total expenditures: IV analysis for both municipal employee and female instruments.

Panel A: IV, ε = 0.4	(1)	(2)	(3)	(4)
Municipal employees	0.0014	0.0032*	0.0034**	0.0035**
	[0.0022]	[0.0019]	[0.0016]	[0.0016]
Females	0.0041**	0.0032**	0.0013	0.016
	[0.0019]	[0.0016]	[0.0013]	[0.012]
First stage F	24.21	25.91	26.85	147.82
Panel B: Reduced form, ε = 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. First stage *F*-statistic reported for the IV estimations is the Kleinbergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

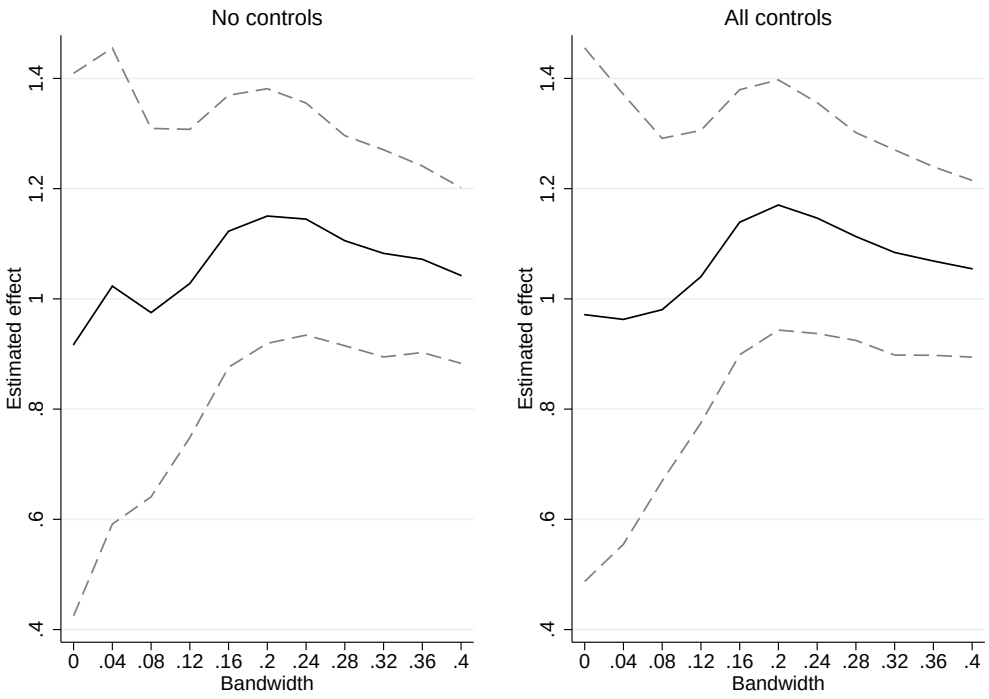


Figure B6. First stage for females.

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 and the right hand graph includes all the controls used in Table 5 column (3). Standard errors are clustered at the municipality level.

Table B5. Pre-treatment covariate balance at municipality level for female.

		$T_{mt} > 0$			$T_{mt} < 0$		
ε = 0	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	95	5 346	969	96	5 184	736	161.37
Health care expenditures (€ per capita)	95	1 610	300	96	1 590	370	20.76
Other expenditures (€ per capita)	95	3 736	790	96	3 595	622	140.81
Population	139	7 518	9 108	135	8 870	16 924	-1 351
Young inhabitants %	139	18.38	3.10	135	18.52	3.35	-0.14
Old inhabitants %	139	18.79	4.67	135	18.18	4.49	0.60
Council size	139	27.17	9.38	135	27.81	9.46	-0.64
Municipal employees %	95	28.33	12.82	96	27.51	11.75	0.82
Municipal health care employees %	95	7.50	5.53	96	7.56	4.83	-0.06
Municipal non-health care employees %	95	20.83	11.92	96	19.95	10.36	0.88
Incumbents %	95	57.02	8.60	96	57.58	8.64	-0.55
Women %	95	33.64	9.49	96	34.08	8.06	-0.44
Instrument for women	95	0.00	0.11	96	-0.14	0.11	0.14
High professionals %	95	17.86	9.69	96	21.24	10.69	-3.38**
University educated %	95	10.34	7.42	96	12.16	8.83	-1.82
Unemployed %	95	3.57	4.52	96	3.96	4.07	-0.40
Center Party seat share %	139	41.41	20.27	135	39.61	19.03	1.80
Coalition Party seat share %	139	15.38	10.26	135	16.66	10.59	-1.28
Social Democratic Party seat share %	139	20.99	11.64	135	21.71	10.54	-0.72
Green party seat share %	139	1.38	2.78	135	1.45	3.02	-0.07
Left Alliance seat share %	139	8.13	8.12	135	8.46	8.38	-0.34
Swedish Party seat share %	139	3.95	15.34	135	4.28	15.50	-0.33
True Finns seat share %	139	2.05	4.88	135	2.23	4.20	-0.18
Christian Democrats seat share %	139	2.39	3.66	135	2.98	4.19	-0.59
ε = 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.

Table B6. Post-treatment covariate balance at municipality level for female.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0 (lotteries)	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	139	56.46	8.19	135	56.09	8.91	0.37
Municipal employees %	139	27.97	11.31	135	25.22	12.48	2.75*
High professionals %	139	18.41	10.71	135	20.45	10.44	-2.04
University educated %	139	10.83	8.28	135	12.31	9.51	-1.48
Unemployed %	139	3.66	4.30	135	3.72	4.74	-0.06
Center Party %	139	43.04	20.30	135	40.71	19.06	2.33
Coalition Party %	139	15.81	10.98	135	17.12	11.28	-1.32
Social Democratic Party %	139	19.40	10.75	135	20.16	10.72	-0.76
Green party %	139	1.51	2.93	135	1.44	3.09	0.07
Left Alliance %	139	7.16	7.54	135	7.87	8.32	-0.71
Swedish Party %	139	3.86	15.11	135	4.25	15.44	-0.39
True Finns %	139	2.12	5.27	135	1.85	4.36	0.27
Christian Democrats %	139	2.47	3.52	135	3.48	4.68	-1.01*
Other parties %	139	4.64	11.02	135	3.13	5.96	1.51
ε = 0.4							
Incumbents %	596	57.12	8.62	674	56.92	9.35	0.20
Municipal employees %	596	27.62	12.44	674	26.33	12.26	1.28*
High professionals %	596	21.71	12.15	674	22.46	12.17	-0.75
University educated %	596	13.44	10.27	674	13.55	10.20	-0.10
Unemployed %	596	3.63	4.06	674	3.34	3.92	0.29
Center Party %	596	39.20	21.28	674	39.41	21.65	-0.21
Coalition Party %	596	16.65	11.00	674	16.82	10.72	-0.17
Social Democratic Party %	596	19.96	11.27	674	19.86	11.40	0.10
Green party %	596	2.08	3.49	674	2.13	3.86	-0.05
Left Alliance %	596	8.45	8.09	674	7.84	8.03	0.61
Swedish Party %	596	4.95	16.87	674	5.81	19.04	-0.86
True Finns %	596	1.80	3.95	674	1.74	4.31	0.06
Christian Democrats %	596	2.97	3.85	674	3.16	4.05	-0.19
Other parties %	596	3.96	8.08	674	3.25	6.70	0.71

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.

Appendix C: Validity and robustness of the sectoral effects

We present here the robustness of the results in Table 7 over a range of bandwidths. We also show robustness to accounting the correlation between the municipal employee status and gender by instrumenting also for the female seat share in the council. We also report the first stages of our sectoral IV across a range of bandwidths and test for the validity of the sector specific instruments.

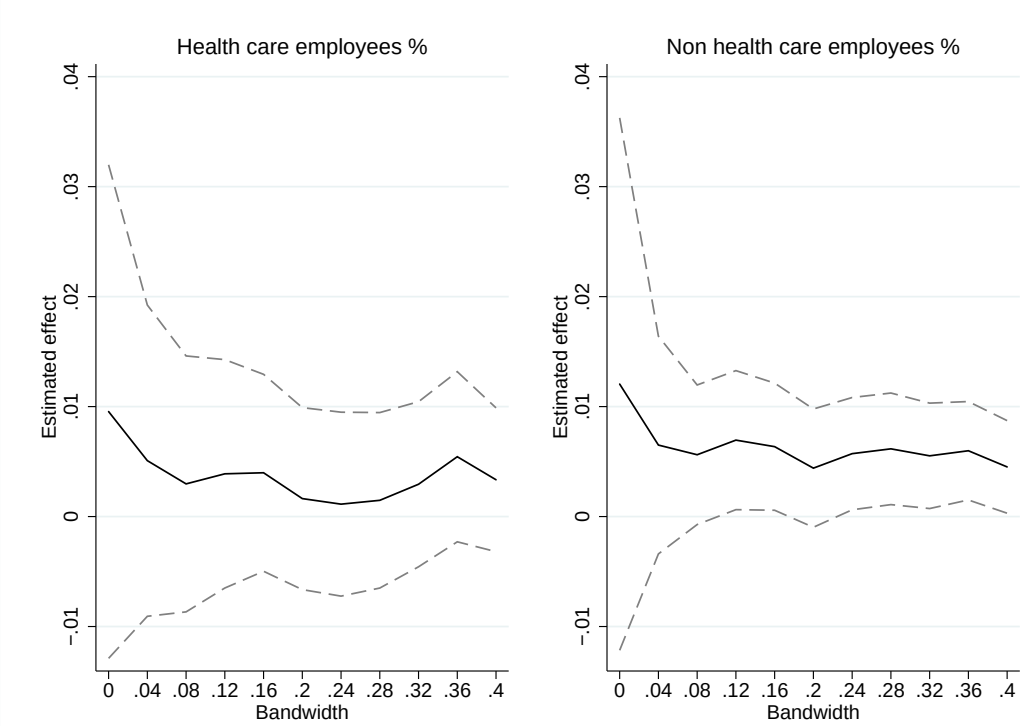


Figure C1. Robustness of the non-health outcome results in Table 7 for different bandwidths.

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.

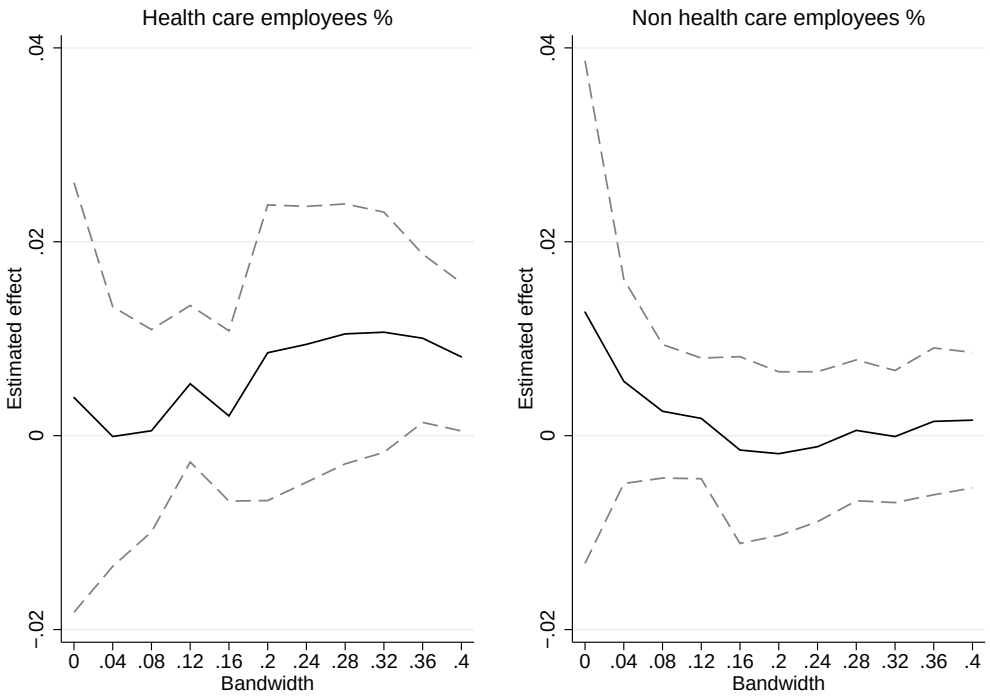


Figure C2. Robustness of the health outcome results in Table 7 for different bandwidths.

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 and the right hand graph includes all the controls used in Table 5 column (3). Standard errors are clustered at the municipality level.

Table C1. 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	0.0050**	0.004
	[0.0023]	[0.0036]
Municipal health care employees	-0.0013	0.0021
	[0.0028]	[0.0033]
Female	0.0018	0.002
	[0.0016]	[0.0028]
First stage F	3.54	3.51
Panel B: Reduced form of IV, $\varepsilon = 0.4$	(3)	(4)
Municipal non health care employees	0.0037*	0.0012
	[0.0021]	[0.0035]
Municipal health care employees	0.0005	0.0056*
	[0.0032]	[0.0034]
Female	0.0030*	0.003
	[0.0017]	[0.0031]
R^2	0.44	0.18
N	1544	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. First stage *F*-statistic reported for the IV estimations is the Kleinbergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

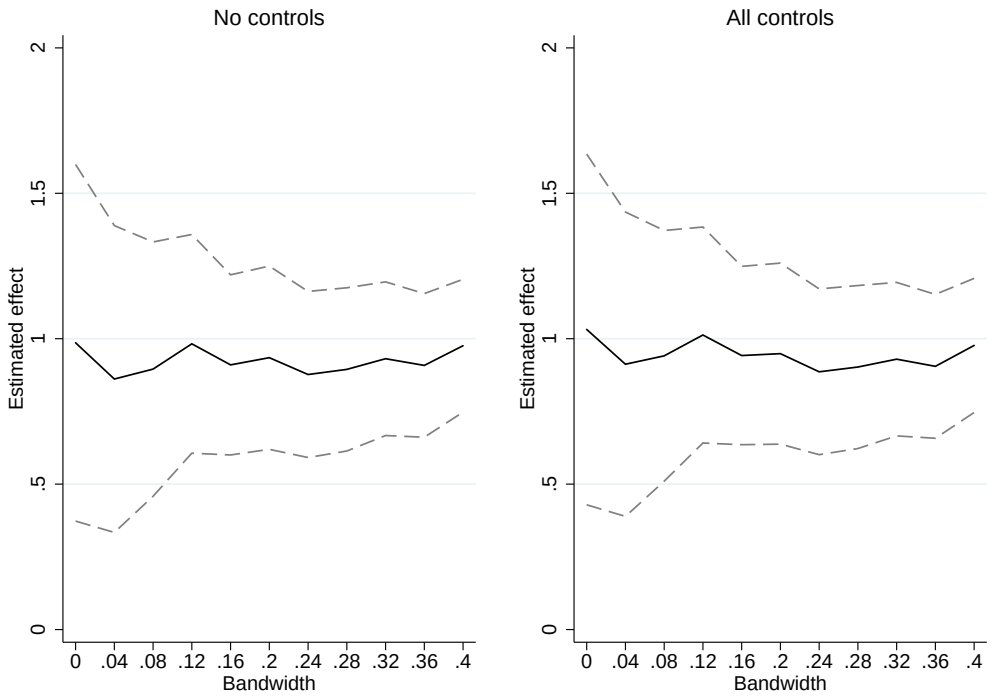


Figure C3. First stage for municipal health sector employees.

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 and the right hand graph includes all the controls used in Table 5 column (3). Standard errors are clustered at the municipality level.

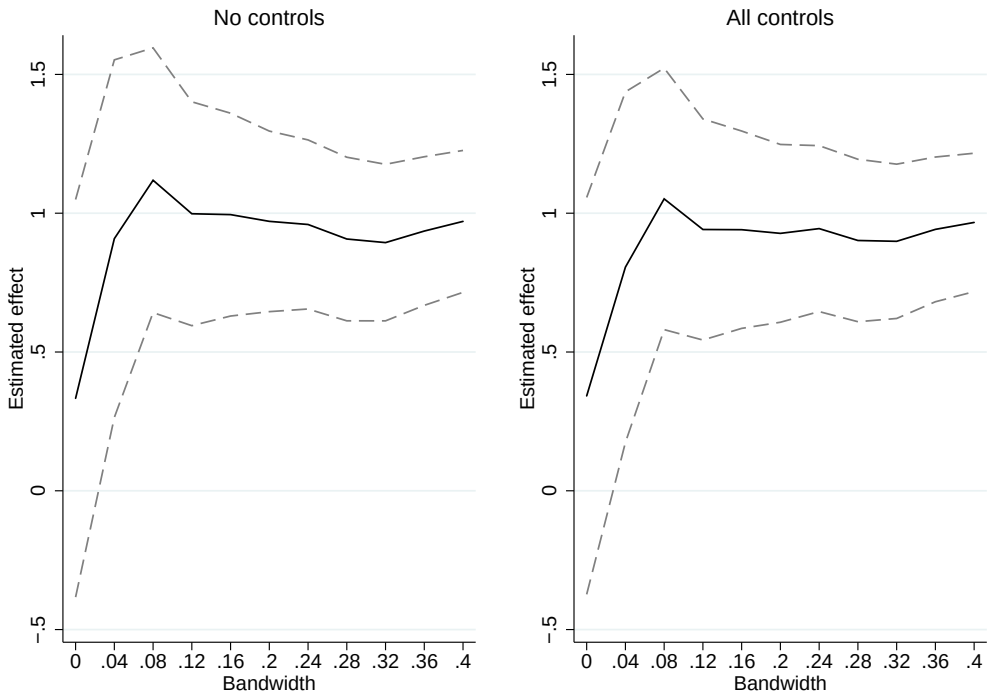


Figure C4. First stage for municipal non-health sector employees.

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 and the right hand graph includes all the controls used in Table 5 column (3). Standard errors are clustered at the municipality level.

Table C2. Pre-treatment covariate balance at municipality level for non-health care employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	45	5 427	1 029	59	5 407	943	20
Health care expenditures (€ per capita)	45	1 596	326	59	1 681	410	-84
Other expenditures (€ per capita)	45	3 831	888	59	3 727	745	105
Population	79	6 699	6 043	93	8 731	11 688	-2 032
Young inhabitants %	79	18.63	3.46	93	18.47	3.14	0.16
Old inhabitants %	79	18.27	4.31	93	18.22	4.77	0.04
Council size	79	27.18	7.98	93	27.80	10.20	-0.62
Municipal employees %	45	29.25	13.98	59	26.03	10.94	3.22
Municipal health care employees %	45	8.03	5.06	59	6.65	4.21	1.39
Municipal non-health care employees %	45	21.22	12.47	59	19.38	10.34	1.84
Instrument for non-health care employees	45	-0.04	0.07	59	-0.05	0.07	0.01
Incumbents %	45	57.24	7.68	59	57.18	8.72	0.06
Women %	45	34.84	10.12	59	33.87	8.68	0.98
High professionals %	45	19.77	10.45	59	18.76	10.42	1.01
University educated %	45	10.92	6.95	59	10.37	7.22	0.55
Unemployed %	45	2.75	3.27	59	4.10	4.78	-1.35
Center Party seat share %	79	39.21	17.60	93	42.17	19.53	-2.96
Coalition Party seat share %	79	16.44	9.68	93	15.12	9.78	1.32
Social Democratic Party seat share %	79	21.55	10.70	93	21.11	10.50	0.44
Green party seat share %	79	1.69	3.13	93	1.75	3.76	-0.06
Left Alliance seat share %	79	9.55	8.64	93	9.20	9.01	0.35
Swedish Party seat share %	79	2.70	13.65	93	2.84	12.62	-0.14
True Finns seat share %	79	2.44	5.04	93	2.13	4.15	0.31
Christian Democrats seat share %	79	3.21	4.05	93	2.61	3.36	0.61
ε = 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

*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.

Table C3. Post-treatment council covariate balance for non-health care sector municipal employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0 (lotteries)	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	79	55.45	8.99	93	57.33	9.55	-1.88
Women %	79	34.30	8.72	93	32.79	9.32	1.51
High professionals %	79	20.18	9.05	93	20.16	10.53	0.02
University educated %	79	11.00	7.40	93	11.33	8.79	-0.33
Unemployed %	79	4.07	4.81	93	3.96	4.60	0.11
Center Party %	79	41.55	16.97	93	42.41	18.79	-0.86
Coalition Party %	79	17.33	9.44	93	16.83	10.48	0.50
Social Democratic Party %	79	19.54	9.34	93	19.78	10.34	-0.24
Green party %	79	1.49	2.69	93	2.07	3.77	-0.57
Left Alliance %	79	8.70	9.02	93	8.71	8.90	0.00
Swedish Party %	79	2.47	12.90	93	2.80	12.68	-0.33
True Finns %	79	2.03	5.36	93	1.77	4.15	0.26
Christian Democrats %	79	3.07	3.88	93	2.53	3.47	0.53
Other parties %	79	3.82	7.33	93	3.10	6.33	0.72
ε = 0.4							
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
Center Party %	522	37.50	20.92	496	38.29	21.23	-0.79
Coalition Party %	522	18.15	10.54	496	16.78	10.73	1.37
Social Democratic Party %	522	21.02	11.46	496	20.27	11.06	0.75
Green party %	522	2.53	4.04	496	2.22	3.72	0.31
Left Alliance %	522	8.56	8.35	496	8.23	7.97	0.32
Swedish Party %	522	3.84	14.57	496	5.78	18.56	-1.94
True Finns %	522	1.87	4.24	496	1.75	3.78	0.12
Christian Democrats %	522	3.08	3.79	496	3.30	3.81	-0.23
Other parties %	522	3.46	6.69	496	3.37	6.31	0.09

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 C4. Pre-treatment covariate balance for health care employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Total expenditures (€ per capita)	35	5 225	835	27	5 220	607	5.14
Health care expenditures (€ per capita)	35	1 588	388	27	1 581	229	6.82
Other expenditures (€ per capita)	35	3 637	598	27	3 639	480	-1.67
Population	44	12 334	21 380	38	9 540	10 939	2 794
Young inhabitants %	44	19.35	4.01	38	19.02	2.46	0.34
Old inhabitants %	44	17.26	5.09	38	17.44	3.97	-0.18
Council size	44	29.32	11.44	38	28.63	9.77	0.69
Municipal employees %	35	29.20	14.30	27	32.80	12.87	-3.61
Municipal health care employees %	35	7.71	6.34	27	9.85	4.99	-2.14
Instrument for health care employees	35	0.02	0.06	27	0.08	0.07	-0.06
Municipal non-health care employees %	35	21.49	11.42	27	22.95	11.25	-1.47
Incumbents %	35	57.73	7.66	27	59.43	10.11	-1.70
Women %	35	32.53	10.22	27	34.47	9.14	-1.94
High professionals %	35	19.40	13.30	27	23.10	10.31	-3.70
University educated %	35	12.65	7.94	27	12.04	8.27	0.61
Unemployed %	35	3.27	3.35	27	3.89	3.49	-0.63
Center Party seat share %	44	42.96	23.28	38	37.11	18.55	5.85
Coalition Party seat share %	44	15.98	9.50	38	18.68	10.19	-2.70
Social Democratic Party seat share %	44	17.09	10.74	38	21.31	11.41	-4.22
Green party seat share %	44	2.08	3.25	38	1.00	2.09	1.08*
Left Alliance seat share %	44	10.48	9.69	38	9.51	9.00	0.97
Swedish Party seat share %	44	3.26	12.02	38	4.88	19.74	-1.62
True Finns seat share %	44	1.55	3.19	38	1.62	3.33	-0.07
Christian Democrats seat share %	44	2.65	3.53	38	3.04	4.00	-0.40
ε = 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.

Table C5. Post-treatment council covariate balance for health care sector employees.

	$T_{mt} > 0$			$T_{mt} < 0$			
ε = 0 (lotteries)	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Difference
Incumbents %	44	56.60	9.03	38	53.67	10.48	2.93
Women %	44	33.57	8.39	38	32.21	7.95	1.35
High professionals %	44	21.41	12.75	38	22.06	9.83	-0.65
University educated %	44	13.79	9.08	38	11.69	7.98	2.10
Unemployed %	44	2.54	3.24	38	3.24	3.48	-0.70
Center Party %	44	44.41	23.40	38	38.26	19.33	6.16
Coalition Party %	44	17.06	10.27	38	19.74	11.02	-2.68
Social Democratic Party %	44	15.12	9.48	38	18.79	11.43	-3.68
Green party %	44	1.85	3.49	38	1.37	2.12	0.48
Left Alliance %	44	9.51	8.96	38	8.34	8.58	1.16
Swedish Party %	44	3.21	11.82	38	4.94	20.19	-1.74
True Finns %	44	1.62	3.08	38	1.59	3.02	0.03
Christian Democrats %	44	2.98	3.81	38	3.89	5.11	-0.92
Other parties %	44	4.26	6.69	38	3.07	6.66	1.18
ε = 0.4							
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
Center Party %	305	35.96	21.03	319	36.06	20.81	-0.10
Coalition Party %	305	17.80	10.35	319	18.40	10.73	-0.60
Social Democratic Party %	305	21.52	11.29	319	21.18	11.47	0.35
Green party %	305	2.98	4.66	319	2.54	3.85	0.44
Left Alliance %	305	8.71	7.98	319	8.61	8.10	0.10
Swedish Party %	305	4.74	16.33	319	4.16	16.12	0.58
True Finns %	305	1.75	3.54	319	1.88	3.79	-0.13
Christian Democrats %	305	3.32	3.79	319	3.49	3.79	-0.17
Other parties %	305	3.22	6.27	319	3.69	7.02	-0.47

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.

Appendix D: Robustness of the party size heterogeneity in the effect

We analyze here whether also the sectoral results are stronger for the largest party and whether the by party results for the total expenditures hold when instrumenting also the female share.

Table D1. 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, ε = 0.4	(1)	(2)	(1)	(2)
Health care employees	0.0133**	-0.0413	0.0008	0.3327
	[0.0066]	[0.5556]	[0.0042]	[1.6703]
Non health care employees	0.0039	0.0136	0.0051**	0.0172
	[0.0048]	[0.0490]	[0.0024]	[0.1400]
First stage F	41.81	0.02	41.81	0.02
Panel B: Reduced form, ε = 0.4	(3)	(4)	(3)	(4)
Health care employees	0.0104**	-0.0040	-0.0007	0.0039
	[0.0050]	[0.0054]	[0.0035]	[0.0052]
Non health care employees	0.0054	0.0017	0.0057**	0.0012
	[0.0056]	[0.0040]	[0.0028]	[0.0049]
R^2	0.18	0.18	0.43	0.43
N	1534	1534	1534	1534
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. First stage *F*-statistic reported for the IV estimations is the Kleinbergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Table D2. Results for sectoral expenditures: IV analysis for both municipal employee groups and female instruments.

	Largest party	2nd largest party
Panel A: IV, ε = 0.4	(1)	(2)
Municipal employees	0.0033	0.0030
	[0.0021]	[0.0036]
Females	0.0035*	-0.0025
	[0.0018]	[0.0030]
First stage F	38.33	17.68
Panel B: Reduced form, ε = 0.4	(3)	(4)
Municipal employees	0.0037*	0.0026
	[0.0020]	[0.0032]
Females	0.0034**	-0.0017
	[0.0016]	[0.0025]
R^2	0.57	0.57
N	1544	1544
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. First stage *F*-statistic reported for the IV estimations is the Kleinbergen-Paap Wald *F*-statistic. ***, ** and * denote 1, 5 and 10 % statistical significance levels respectively.

Appendix E: Rent-seeking results

We report the rent-seeking estimations using candidate level data in Table E1 and the house-price regressions using municipal level data in Table E2. Last, we probe the robustness of the results in Table E1 to different bandwidths.

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

	Panel A: Log(Change in income from t to t+1)			
	(1)	(2)	(3)	(4)
Elected	0.1468	-0.0399	-0.1696	-0.0856
	[0.2183]	[0.2118]	[0.1222]	[0.1199]
N	114	114	347	347
R^2	0.00	0.20	0.01	0.15
	Panel B: Unemployed t+1			
	(5)	(6)	(7)	(8)
Elected	0.0104	0.0040	0.0033	-0.0008
	[0.0214]	[0.0221]	[0.0122]	[0.0123]
N	207	207	588	588
R^2	0.00	0.04	0.00	0.11
	Panel C: Elected t+1			
	(9)	(10)	(11)	(12)
Elected	0.0407	0.0396	0.0013	0.0039
	[0.0506]	[0.0516]	[0.0283]	[0.0285]
N	330	330	990	990
R^2	0.00	0.06	0.00	0.04
	Panel D: Vote share t+1			
	(13)	(14)	(15)	(16)
Elected	0.1113	0.0265	-0.0538	-0.0531
	[0.1348]	[0.1328]	[0.0882]	[0.0847]
N	202	202	598	598
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 *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.

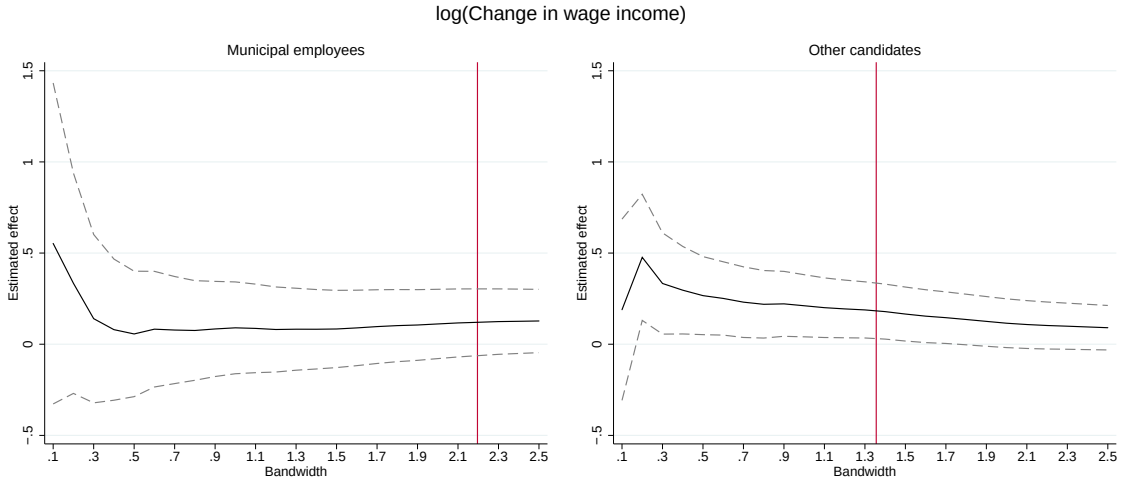
We then turn to our analysis of municipal house prices. We exclude 309 municipality-election period observations from the sample because these small municipalities do not have many housing market transactions.

Table E2. Results for house prices

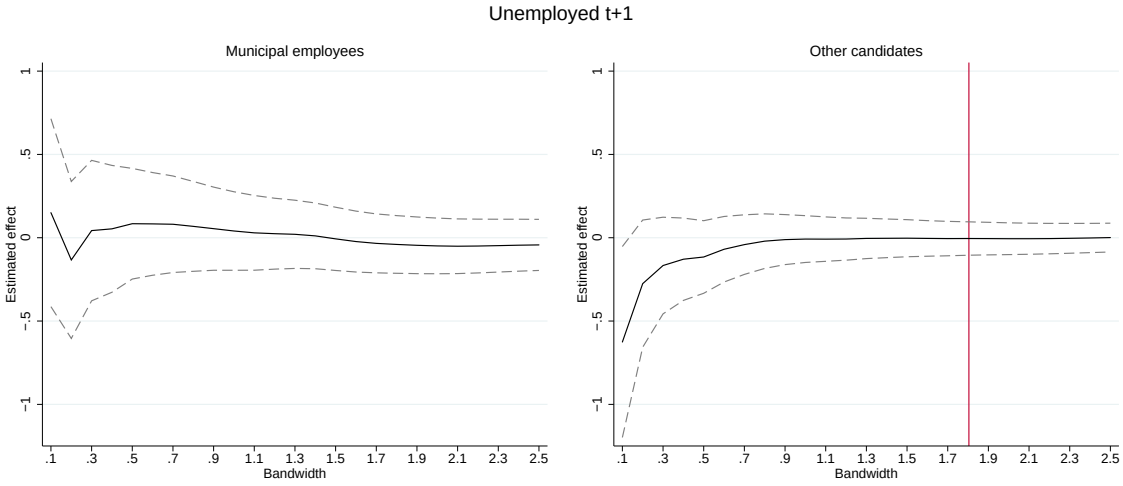
Outcome: log(house price per m^2)	
ATE, ε = 0.4	(1)
Municipal employees	0.0000
	[0.0021]
Female	-0.0002
	[0.0019]
R^2	0.77
N	1235

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.

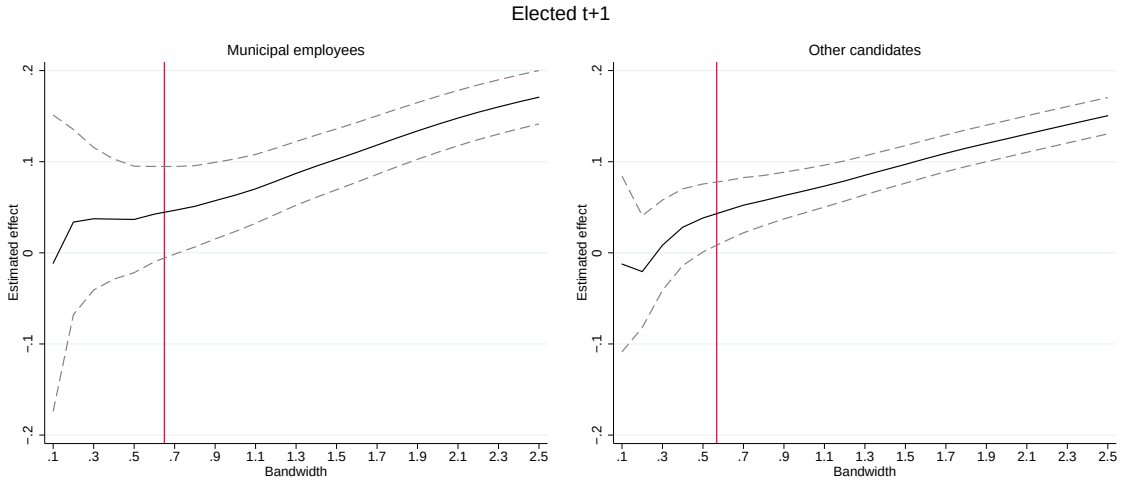
Panel A: RDD effect of getting elected at t on earnings at t+1 for a range of bandwidths.



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.

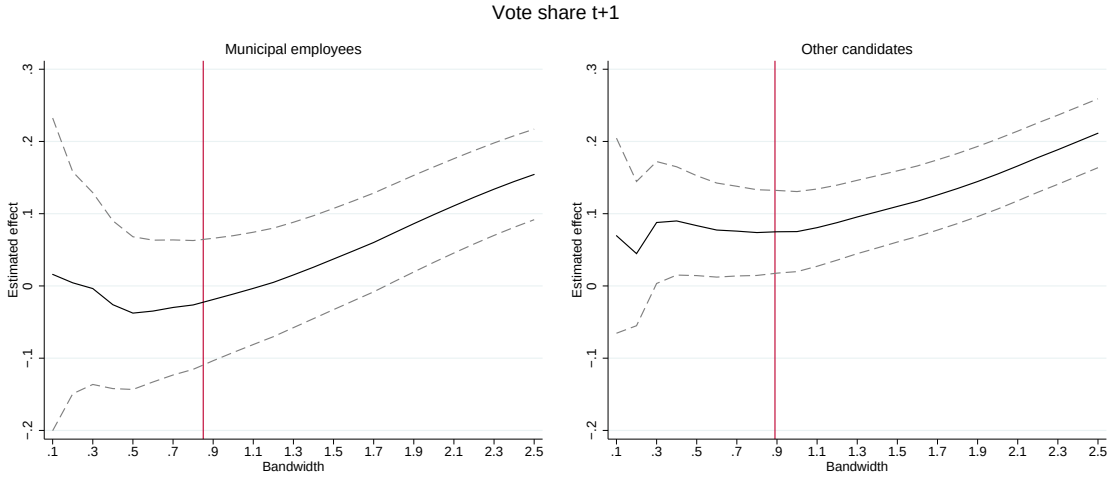


Figure E1. Robustness of the results in Table 7 for using RDD and for a wider range of bandwidths.

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 Imbens and Kalyanaraman (2012) optimal bandwidth.

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 is from an election aid survey in which both candidates and voters respond to a same set of questions and the application provides voters with information on the best matches.

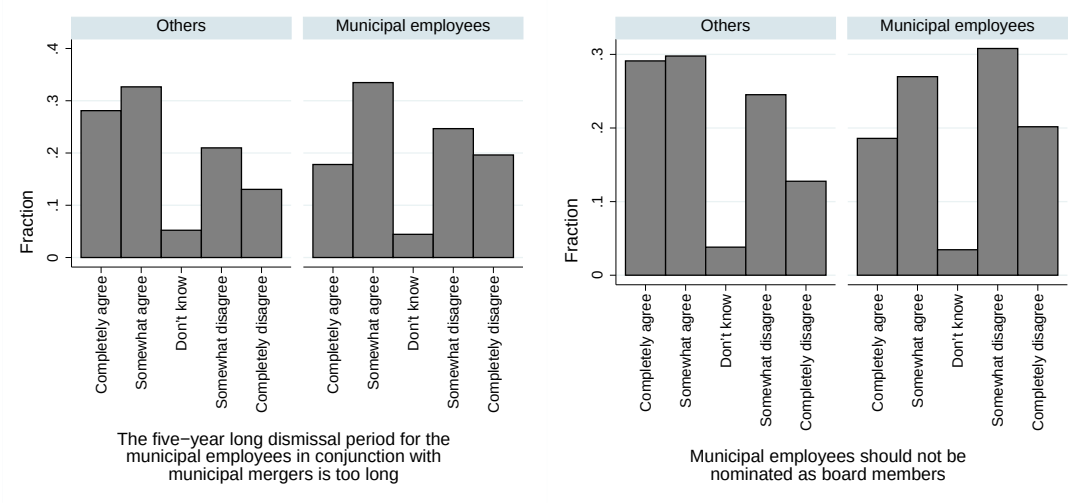


Figure E2. Survey responses ($N = 4215$).

References for the appendices

Imbens, Guido – Kalyanaraman, Karthik (2012): Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *Review of Economic Studies* 79(3): 933–959.