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## ABSTRACT

### Returns to Office in National and Local Politics\*

We estimate the effect of getting elected on future income development of political candidates. We present a bootstrap approach for measuring electoral closeness, which can be used to implement a regression discontinuity design in any electoral system. We apply the method to the Finnish proportional open list system. Being elected to parliament increases annual earnings initially by €20,000, and getting elected to a municipal council by €1,000. The returns to office for parliamentarians accrue mainly through higher wages during the time in office. Our results imply that the returns to office may be large even in the absence of corruption.

JEL Classification: D72, J45

Keywords: returns to office, elections, regression discontinuity

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# 1 Introduction

Financial rewards—obtained both during a political career and afterwards—are likely to be a major component of the incentives for entering politics. They may have profound societal implications by affecting the quality and behavior of politicians. There is a large related theoretical literature, but only few empirical studies about the causal effect of a political career on individual earnings. Since a political career has opportunity costs — foregoing the opportunity to have a private sector job — it is not obvious a priori that the returns to office should even be positive. Furthermore, the source of the returns to office, whether they arise through official compensation or through other channels, has important implications for politicians’ incentives.

We use Finnish data on candidates in 11 parliamentary elections from 1970 to 2007 and in four municipal elections from 1996 to 2008 to estimate the effect of getting elected in national and local elections on the future income development of the candidates, and analyze how the returns to office evolve over time and over the career. We employ a regression discontinuity (RD) design to obtain a causal estimate of the returns to office. We tackle the challenge of applying an RD design in a proportional electoral system by introducing a bootstrap approach for measuring electoral closeness, which can be used to apply an RD design in any electoral system.

There are only few papers that estimate the causal effect of getting elected on future income or wealth, and we contribute to this literature by analysing the effects on income both in the short and in the long run (during the political career as well as afterwards), at different levels of government, using high-quality data in an electoral setting that is well suited for an RD design.

Eggers and Hainmueller (2009) were the first to use the RD design in estimating the economic return to getting elected; they had data on the value of estates of British politicians at the time of their death. Two other published papers estimating private returns to holding political office with an RD design use wealth data: Querubin and Snyder (2013) for the 19th century United States and Fisman, Schulz and Vig (2014) for contemporary India. The observed pattern of wealth accumulation is the result of a combination of earnings effects, as well as savings decisions. We provide direct evidence on the earnings effects. Like us, manuscripts by Lundquist (2011) and Willumsen (2011) use register data on income: Willumsen (2011) is the only other paper looking at earnings effects in national politics. He estimates the effect of getting elected on earnings after exiting politics, whereas we analyze the full time profile of the earnings effect both in the

short and in the long run, during and after the political career. Lundqvist (2011) uses data on candidates in local elections in Sweden.

Our income data is of very high quality: it comes from the official tax register, and covers both earnings and capital income for almost two decades. With data from both national and local elections, we can estimate the returns to holding political office at different levels of government. In total our data covers 12,398 and 93,741 unique candidates in parliamentary and municipal elections respectively, and over 1.6 million person-years of income data between 1993 and 2011. The panel nature of the data allows us to examine the evolution of the causal effect of being elected in the long run, up to 40 years after the first parliamentary elections in our data. The ability to track the earnings development of elected vs. defeated parliamentary candidates for a relatively long period after the election is unique in the literature.

We find that getting elected to the parliament caused an increase in earnings of about €20,000 per year during the first electoral period after election. This implies that, among marginal candidates, the earnings of those elected are on average 25% above their outside opportunities. However, the effect fades out over time: the effect is halved during the second electoral period after getting elected, and from the third subsequent electoral period onwards it stays at about €3000-€6000 per year but is no longer statistically significant.

The returns to office for parliamentarians more than doubled after the year 2000, when there was a major salary increase in the Finnish parliament. Before the salary reform, the returns to office in national politics were small or non-existent and only side jobs allowed close winners to achieve the same level of earnings as close losers. While a non-negligible proportion of MPs' earnings still arises from side jobs, our results suggest that direct wage effects are currently the major driving force behind returns to office in national politics.

Earlier literature has found that returns to political office arise through lucrative outside opportunities (Eggers and Hainmueller 2009), insufficient control of politicians (Querubin and Snyder 2013) or outright corruption (Fisman, Schulz and Vig 2014). We analyse returns in a country with a very low level of corruption, but we still find that returns to office arising through official channels can be considerable.<sup>1</sup> Generalizing results across countries with different levels of corruption could result in misleading conclusions about the role of politicians' pay in shaping the monetary incentives of politicians. The

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<sup>1</sup>According to Transparency International, the 12 countries with lowest level of corruption in 2014 were the Nordic countries, New Zealand, Switzerland, Singapore, the Netherlands, Luxembourg, Canada, Australia and Germany.

lower the level of corruption (or other unofficial compensation outside politics), the larger is the role that the actual salaries can be expected to play in the self-selection into politics. For example, earlier work has found that the 2000 salary reform in Finland had an effect on education level of candidates running in parliamentary elections (Kotakorpi and Poutvaara 2011).

Inasmuch as the returns to getting elected accrue as direct benefits while in office, the durability of the causal effect from any given election is determined by the strength of the incumbency effect. How much does getting elected help a candidate to get elected again in the future? The Finnish open-list electoral system where voters vote for individual candidates (rather than parties) enables us to estimate the individual incumbency effect (as distinct from a party incumbency effect). We find that the incumbency effect is much weaker in Finland than what has been estimated for example in the U.S.: winning a seat in a parliamentary election increases the individual's probability of winning a seat in the subsequent election by 18 percentage points. The limited staying power of close winners restricts the durability of any direct earnings benefits from getting elected, and largely explains why the returns to winning fade over time. Hence, our results suggest that the returns to getting elected occur mainly during an individual's political career, whereas effects on subsequent earnings outside politics appear small. This also suggests that there is no "private sector penalty" for having spent years in politics, despite the loss of non-political work experience that it entails.

The returns to office in local politics appear to be much smaller than in national politics. Getting elected to a municipal council increased annual earnings only by about €1000. In municipal elections, the incumbency advantage is small or non-existent.

Estimates by subgroup suggest some differences in the earnings effect between major parties in parliamentary elections, with Centre Party candidates gaining the most and Conservatives the least; this appears to be due to differences in outside opportunities but the estimates are not precise. There are no systematic party differences in municipal elections. Winning either type of election has no discernible effect on subsequent capital income.

To measure the economic returns to a political career, as opposed to just the level of remuneration for politicians, it is necessary to have an idea of the counterfactual income level of politicians had they not been elected. We use the incomes of close election losers as the counterfactual for incomes of close election winners. The basic idea of the RD estimation is to exploit the discontinuity of electoral outcomes in the number of votes: while election winners are likely to differ from losers in many unobservable

ways that affect income, the differences between close winners and close losers should be essentially random—if “close” is appropriately defined. Measuring the "closeness" of electoral outcomes is simple under "first past the post" electoral systems, but there is no obvious closeness measure under more complex electoral rules, such as the open-list proportional system used in Finland.

A particular challenge in applying the RD design to complex electoral systems is that there is no obvious way of defining electoral closeness that would be comparable across districts. In systems with proportional elections and multi-member districts there is no pre-specified vote share that could be used as a similar dividing line. In closed-list proportional systems—where voters vote for parties that compete in multi-member districts—it is still possible to derive closed-form formulae for the distance of a party’s vote share from winning one more or less seat in a district, as shown by Folke (2014). Open-list proportional systems are more complex yet, as voters vote for individual candidates but seat allocation also depends on party vote shares, so individual candidates compete across a myriad of margins both within and across party lines. On the other hand, this complexity also implies that the open-list electoral system used in Finland is particularly well suited for causal identification: there are many more close contests between individuals within parties than between parties for the marginal seat in a district (whereas under closed-list systems there is only competition between parties).

One of our contributions is to present a conceptually simple method for measuring electoral closeness that is straightforward to apply under any electoral rule, no matter how complicated, provided there is data on the distribution of votes. The key idea is to resample votes from the actual vote tally, and then to identify close winners and losers from the probability of getting elected in the simulated elections. Intuitively, candidates who lose many simulated elections but were in reality elected are close winners. The resulting assignment variable is continuous and can be subjected to standard RD validity checks; it is also comparable across elections in different districts and years, where the number of seats and the number of voters may differ. The assignment variable can be used even when the electoral rules differ between elections. Our method can be applied in other settings, not just RD. Hyytinen, Saarimaa, and Tukiainen (2014) use it in their study of municipal mergers. They generate counterfactual election outcomes to measure the impact of the merger on the threat to the personal re-election of incumbent municipal councilors, under the assumption that the distribution of votes for individual candidates would not be affected by the proposed merger. (They find that re-election prospects have a clear impact on how individual councilors vote on merger decisions.)

## 2 Related literature

The seminal contribution in the causal inference of economic returns to office is in the study by Eggers and Hainmueller (2009). They use estate values of deceased Members of the British Parliament and their unsuccessful competitors in the House of Commons elections between 1950 and 1970, and apply the close election RD to estimate the economic return of getting elected. They find that serving in the Parliament almost doubled the estate value for Conservatives (although the estimate is only marginally significant) but no effect is found for Labour politicians. Querubin and Snyder (2013) study the wealth of individuals who ran for the U.S. House of Representatives during the period 1850-1880. They compare wealth accumulation among politicians who had lost or won their first congressional race by a small margin. They do not find evidence of faster wealth accumulation among elected politicians, with the exception of politicians who were first elected during the Civil War. Fisman, Schulz, and Vig (2014) use data from mandatory asset disclosures of election candidates in two recent parliamentary elections in India and find that the assets of winners grew 3-5% faster than the assets of losers. The gains are mainly driven by MPs holding positions in the Council of Ministers, and they are stronger in more corrupt states.

When the focus is on individual electoral outcomes, as in our study, the details of how seats are allocated within parties become important. Many proportional electoral systems, including those in Sweden and Norway, feature closed party lists, which means that the parties decide before the election—through some internal political process—a rank order for their candidates. In a closed system, when a party gets  $N$  seats in a district then it is the top  $N$  candidates as selected by the party who are elected.<sup>2</sup> Lundqvist (2011) analyzes the effect of being elected in local level politics on subsequent income in Sweden, using the rank order of candidates in the party list as a control. The last candidate in a party to get in and the first to be left out are then essentially defined as close winner and loser. She concludes that there is no economic return to being elected to a local council. Willumsen (2011) studies the economic return to office in the Norwegian parliament by comparing the incomes of former politicians at a single point in time (2006-2008). He uses close elections between 1977 and 2001 in an RD design and finds that getting elected to the parliament increased income by 10 – 15%. More recently, Folke, Persson and Rickne (2016) exploit close elections between the left and the right bloc in Swedish municipalities

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<sup>2</sup>Both Swedish and Norwegian systems allow individual voters in principle to opt out of the ordering provided by the party, but in practice this has only rarely affected election outcomes.

to test whether children or siblings of politicians who enter into a top executive position obtain economic benefits. They find that children whose parent is appointed to the top political executive position are more likely to postpone their studies and earn about 15 percent more. There are no effects on politicians' siblings.

To summarize our contribution to the literature on returns to office, our paper is complementary to Eggers and Hainmueller (2009), Querubin and Snyder (2013), and Fisman et al. (2014) in that we look at the effect of getting elected on earnings instead of wealth. The observed pattern of wealth accumulation is the result of a combination of direct and indirect earnings effects, as well as savings decisions. We estimate how getting elected affected the subsequent earnings of candidates. Compared to Willumsen (2011) we look at the time profile of earnings both in the short and in the long run, during and after the political career. Lundqvist (2011) analyzes local politics, whereas we look at candidates in both national and local elections.

One of the main reasons why the economic returns to being elected matter is that they can affect the quality and type of people who decide to enter politics, and the behavior of those who get elected. Recent empirical work has shown that higher pay level of politicians increases the quality of candidates, using differences in compensation across municipalities (Ferraz and Finan 2009; Gagliarducci and Nannicini, 2013) or quasi-experimental evidence stemming from a large salary increase for members of parliament (Kotakorpi and Poutvaara 2011).<sup>3</sup> Higher salary for politicians also makes re-election more attractive, giving politicians stronger incentives to behave as voters wish. Di Tella and Fisman (2004) find a negative correlation between gubernatorial pay and per capita tax payments in US states, which they interpret as pay for good performance; however, Besley (2004) warns that extrinsic motivation in the form of higher pay may crowd out intrinsic motivation.

A considerable part of the returns to a political career may arise after leaving office, or from indirect effects during holding office. Diermeier, Keane, and Merlo (2005) conclude that congressional experience increases subsequent wages both in the private and in the public sector. The wage while in office turns out to have only a small effect on retirement and other decisions. An important caveat here is that Diermeier et al. analyze only decisions by incumbent politicians. Groseclose and Krehbiel (1994) show that politicians' retirement decisions respond strongly to monetary incentives.

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<sup>3</sup>There is a large theoretical literature on the impact of pay for politicians on candidate quality (Besley 2004; Caselli and Morelli 2004; Messner and Polborn 2004; Poutvaara and Takalo 2007; Mattozzi and Merlo 2008).

At a broader level, our paper is also related to the growing literature that uses the RD design to exploit the random variation in close election outcomes to estimate a causal impact on various economic outcomes; Lee and Lemieux (2010) include eight such papers in their review.<sup>4</sup> In studying single-winner electoral systems - such as in Eggers and Hainmueller (2009), Querubin and Snyder (2013), and Fisman et al. (2014) - the measure of closeness is simply the difference in vote share from a pre-defined threshold. In other electoral systems the rules of determining seat allocations can be quite complicated, which makes the measurement of electoral closeness a challenge (more on this in the next section). Pettersson-Lidbom (2008) applies RD design to analyze the impact of political majority on policies in Swedish municipalities, but he first transforms the election results into a two-party framework, by aggregating most parties to either left or right wing party groups (which tend to form ruling coalitions). Folke (2014) was the first to apply the RD design specifically for proportional multi-party election results. He uses Manhattan distance between actual and counterfactual vote vectors, and measures the closeness of an electoral outcome for a party as the distance from the nearest counterfactual vote vector that would result in the gain or loss of a seat for a party. He exploits the discontinuity of the number of seats won by a party in its vote share to study the impact of party strength on various local policy outcomes in Swedish municipalities. Freier and Odendahl (2015) study the effect of a party's political power on tax policy in a multiparty system.

### 3 Measuring Electoral Closeness

#### 3.1 Motivation

In this section we present a resampling method meant to identify close winners and losers under any electoral rule. The aim is to provide a measure of "closeness" to be used as the assignment variable in RD estimation, where the purpose is to estimate causal effects of being elected in the presence of unobserved confounding variables that may be driving individual electoral success.

Most election RD applications have been in the context of First-Past-The-Post (FPTP) elections and two major parties. There a candidate's share of the two-party vote provides

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<sup>4</sup>Caughley and Sekhon (2011) show that narrow winners and narrow losers differ markedly in certain pre-treatment characteristics in U.S. House elections 1942-2008, thus casting doubt on the validity of RD. Eggers et al. (2015) examine U.S. House elections in other time periods, other American elections, and elections in other countries, and conclude that post World War II U.S. House elections are the sole exception where the validity of RD appears questionable.

a simple and reasonable measure of closeness; it also defines a sharp discontinuity at 50%. Not all electoral systems provide such an obvious assignment variable with a predetermined point of discontinuity. A simple way to translate the natural closeness measure of FPTP elections is to calculate the "shortfall" in vote share that a candidate would have needed to get (or lose) in order to "flip" the outcome from a loss to win (or vice versa). This may be reasonable under some electoral systems, but not in general, and our context is a good place to understand why.

In the Finnish electoral system votes can only be given to individual candidates, but party affiliations also matter for seat allocation. The seats in each election (district-year) are apportioned to parties based on the total vote received by all of their candidates. The D'Hondt method is used to calculate a "competitive index" for each candidate: it is the party's total vote count, divided by the candidate's rank in the within-party ordering by votes. Finally, all seats in the district are allocated to the candidates with the highest competitive indices. (There are further details on the Finnish system in the next section.) This system results in three problems for the "vote share shortfall" approach.

The first complication is that every candidate competes on multiple margins both within and across parties. Candidates can be close to multiple members of their own party and to multiple members of other parties at the same time, in the sense that a swing of a small number of votes would switch the status of a candidate from winner to loser or vice versa. The "shortfall" approach admits only one competitive margin, but a candidate who was close on many margins is in practise closer to be elected "in aggregate" than another who had only one but slightly closer margin.

The second complication is that the electoral outcome of a candidate is non-monotonic in the votes of competing candidates from the same party. Consider, for example, what happens to the best-performing losing candidate  $X$  in a party if another losing candidate  $Y$  in the same party were to receive more votes, holding everyone else's votes fixed. At first, a small number of extra votes to  $Y$  is beneficial for  $X$ , because it increases the total vote tally of the party, which can thereby gain an additional seat that is allotted to  $X$ . However, adding even more more votes to  $Y$  takes  $Y$  ahead of  $X$  in the within-party rank, thus taking the seat away from  $X$  to  $Y$ . Finally, adding even more votes to  $Y$  results in the party getting another additional seat, in which case  $X$  is then elected after all.

The third complication is the non-neutrality of candidates' electoral success to changes between vote shares of other candidates. For example, if  $X$  is a marginal winner in one party and  $Y$  a marginal loser in another party, then their outcome may flip even if their vote shares stay the same. This would happen if an inframarginal candidate from the

party of  $X$  loses votes to a candidate in some other party (not necessarily that of  $Y$ ).

A method based on resampling does not require us to be able to enumerate all possible ways in which a candidate may be "close", and it provides an intuitive way of aggregating closeness across multiple competitive margins to a single measure of closeness.

### 3.2 Bootstrap Elections

Consider an election where  $N$  candidates compete for  $S$  seats. Here "election" refers to one electoral district in one election year. The data consists of the empirical vote shares  $\theta = (\theta_1, \dots, \theta_N)$  and an electoral rule  $\mathcal{H} : \Delta^N \rightarrow \sigma^N$ , where  $\Delta^N$  is the unit simplex and  $\sigma^N = \{0, 1\}^N$  the set of feasible seat allocations, so that  $\sum_n \sigma_n = S$ . The actual seat allocation vector is, by definition,  $\sigma = \mathcal{H}(\theta)$ , where  $\sigma_n = 1$  for winners and  $\sigma_n = 0$  for losers. The mapping  $\mathcal{H}$  incorporates all features of the election other than votes that can affect the outcome. In our application  $\mathcal{H}$  includes information about the party affiliation of each candidate, the apportionment rule used to divide seats between parties, and the rule for allocating a party's seats to its candidates. It is useful to think of  $\mathcal{H}$  as an algorithm that processes the data of vote shares and outputs the subset of  $S$  winners from the set of  $N$  candidates (it may even involve randomization, e.g., to break ties).  $\mathcal{H}$  is what stays fixed in the simulation, while new vote share vectors are generated using repeated randomizations.

The simulation consists of bootstrapped elections, where the basic idea is to resample with replacement  $m$  votes from the empirical distribution of votes, then recalculate the winners according to the actual electoral rule  $\mathcal{H}$ . Thus each resample consists of  $m$  trials from a multinomial distribution, where the empirical vote shares  $\theta_n$  define the probability of each trial (simulated vote) being given to candidate  $n$ . The resampled total vote vector, divided by the number of trials  $m$ , yields one instance of a simulated vote share vector  $t^j$ , and a resulting seat allocation  $s^j = \mathcal{H}(t^j)$ . This "bootstrap election" is repeated  $M$  times, and the results are used to calculate for each candidate  $n$  the fraction  $p_n = \sum_j s_n^j / M$  of bootstrap elections where that candidate was elected. Candidates with  $p_n \approx 1$  can be called "safe" and those with  $p_n \approx 0$  "no-hopers." (Note, however, that  $p_n$  is not the probability of being elected in an ex ante sense, but the probability of being a winner in the bootstrap election.) The same intuition works under any voting system, such as transferable voting or indeed under arbitrarily complex electoral rules.

The number of simulated elections  $M$  should be set so high that  $p$  is stable to adding more repetitions. (Under simple electoral rules it would be feasible to calculate the exact

expected value of  $p$  conditional on  $\theta$ ,  $m$ , and  $\mathcal{H}$ ). While bigger  $M$  is always better, it is not desirable to set the vote resample size  $m$  as high as possible, because the simulated vote shares  $t$  would converge to the empirical vote shares. The purpose of the simulation is to provide deviations that are related to actual vote patterns while preserving the actual vote shares as the expected values of the simulated elections. While our motivation for this method is purely heuristic, the bootstrap can also be interpreted as a counterfactual in a probabilistic voting model, where each candidate has a set of supporters who only turn out at some probability. In the simulation, each resampled vote represents a block of voters who support the same candidate and whose turnout realizations are perfectly correlated within block.

To implement the bootstrap for parliamentary elections we set  $M = 20,000$  and  $m = 10,000$  in all 15 districts. For municipal elections the setting is more complicated, due to the larger number of districts (445) and much more variability in the "size" of the election ( $N, S$ ). We adapted the bootstrap parameters ( $M, m$ ) by district in order to economize on computation time. Some elections with very closely tied vote outcomes required many more resamples  $M$  before  $p$  converged. If  $p$  had not converged after 2000 resamples we kept adding another batch of 10,000 resamples until convergence.<sup>5</sup> We also allowed the number of votes  $m$  to be higher in larger municipalities, but less than proportionally; on average  $m$  was 14% of actual vote count.<sup>6</sup>

### 3.3 Usage in RD

It is useful to normalize the measure of closeness in such a way that all losers are below and all winners are above a given threshold. This normalized measure can then be used as the standard assignment variable in sharp RD designs, and can be subjected to standard RD validity tests and bandwidth choice algorithms. To achieve this, for each combination of year, district, and party, we define the "pivotal  $p$ " as the mean of highest unelected  $p_n$  and lowest elected  $p_n$ . For lists where no one is elected the pivotal  $p$  is defined as 100. The variable  $pmargin$  is then calculated as the candidate's level of  $p_n$  minus the pivotal  $p$ . This way all winners have positive and all losers have negative  $pmargin$ , and there is a sharp discontinuity at zero.

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<sup>5</sup>The criterion for convergence was that, within every party, the ordering of candidates by  $p_n$  did not violate the ordering by actual electoral success (save for those with actual tied votes). The code package that implements the simulation is available from the authors.

<sup>6</sup>Number of "voter blocks"  $m$  was set at 20 times the number of seats, which is determined by a legal formula whereby council size increases less than proportionally in population.

The results of the bootstrap procedure for the 1970-2007 parliamentary elections and 1996-2008 municipal elections are presented in Figure 1, which shows the distribution of  $pmargin$ . A large fraction of candidates are "no-hoppers" with  $p_n \approx 0$ , which causes a large peak at low levels of  $pmargin$ . In order to get a clearer picture of the more relevant parts of the histogram, we have cut out the left-most bin, which consists of no-hoppers from combinations of year, district, and party where no one was elected ( $pmargin = -100$ ). The crucial fact for the validity of RD is that there is no jump in the density of the assignment variable at zero, which is confirmed by the McCrary test for both elections. In the end, the validity and usefulness of this forcing variable is in the empirical RD results, including the balancing tests of predetermined variables and the robustness to covariates (these are reported in what follows).

[ Figure 1 here ]

## 4 Institutional background

Finland is a multi-party democracy with a unicameral parliament. The parliament has the legislative power; it has to approve the budget, vote on the confidence of the government, and ratify international treaties.

The parliament has 200 seats divided between fifteen districts. District borders are fixed but the number of seats varies in proportion to the population (an exception is the small autonomous province of Åland with one seat). In each district, parties present a list of candidates and each voter votes for one candidate. Parties are also allowed to form (election- and district-specific) alliances. In an electoral alliance, two or more parties present candidates on a joint list. For the purposes of seat allocation, an electoral alliance is treated as one party.

The electoral system is proportional with a personal vote. Unlike in some other countries with proportional representation, it is not possible to vote just for a party list without specifying a candidate. The seats in each district are apportioned according to the D'Hondt method (see previous section).

Elections are held every four years.<sup>7</sup> The number of seats in the mainland districts varied between 6 and 34 between 1970 and 2007; the median district size was 13 seats. The maximum number of candidates that can be included on a party list equals the number of

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<sup>7</sup>The earliest parliamentary elections in our data are an exception to this rule: they were held in 1970, 1972, 1975, due to early elections in 1972 and 1975. Thereafter elections have been held every four years.

representatives elected from the district, or 14 if the district has less than 14 seats. Local party associations select the candidates, most commonly using a party primary if more than the maximum number of willing candidates have been put forward.

A total of 21 different parties have had at least one seat in the parliament between 1970 and 2007, typically with between 8 and 11 parties having seats at any one time. The three largest parties, the Centre Party (Centre), the National Coalition Party (NCP) and the Social Democratic Party (SDP), have gathered on average 70% of all seats.

The parliament meets in the plenary session usually more than hundred times annually. In addition, much of the work by the MPs takes place in various committees. Therefore, while there is no legal requirement for MPs to quit other jobs (with some exceptions), in practise being an MP is a full-time job and opportunities to spend time on other jobs while in office are limited. In 2011, the last year in our analysis, the average salary of MPs was about €100,000, while the starting salary was €74,400. MP's salary increases with experience, and those holding important positions of trust, like being group leaders or committee chairpersons, also receive an extra compensation. In addition to salaries, MPs receive tax-free compensation for expenses, which depends on commuting distance.

Electoral campaigns are conducted both by political parties and by individual candidates. Most candidates who run a serious campaign also buy electoral ads in newspapers, and distribute leaflets, both on the streets and through mail. In parliamentary elections, a significant fraction of candidates also run ads on television and radio. We return to the campaign costs later when we discuss our findings.

Turning to institutions in local politics, the municipal council is the highest decision-making body at the municipal level. It decides on the municipal budget, including municipal income tax rate, as well as deciding on city-planning and organizing municipal services and administration. Municipalities are the lowest level of government in Finland but they are more important than in most countries, being responsible for the provision of services such as health care and education. Municipal elections take place every four years. Seats are allocated using the same method as in parliamentary elections, with each municipality forming one district. The number of councilors depends on the size of the municipality, with a minimum of 13 for the least populous municipalities, and reaching a maximum of 85 in Helsinki. In municipal elections each party is allowed to present one and a half times as many candidates on its list as the number of seats in the municipal council. Councilors are paid a compensation for participating in meetings, and may also be nominated to some other local government positions with varying levels of remuneration. In the largest municipality, Helsinki, the municipal council meets about 20 times

each year. In some small municipalities, the municipal council may meet just a few times annually. In addition, it is common for councilors to take part in committees, which meet roughly as often as the council. Thus municipal councilor is a part-time position that typically does not interfere with day jobs, but it could reduce the possibility for overtime work in the evenings.

## 5 Data

Our election data covers all candidates in the 11 parliamentary elections between 1970 and 2007 and in the four municipal elections between 1996 and 2008. This amounts to 12,398 unique candidates in parliamentary elections, and 93,741 unique candidates in municipal elections. The data includes information on each candidate's name, date of birth, party, electoral district, number of votes, and whether he or she was elected. For elections since 1995 this data was obtained from the Ministry of Justice, and for earlier elections it was scanned from printed official statistics published by Statistics Finland except for dates of birth. Dates of birth for pre-1995 winners are listed on the official web site of the parliament, but those for losing candidates had to be collected by hand from various sources (archives of the major parties, archives of election councils in some districts). Names and dates of birth were used to match the election data with the earnings data.

We obtained data on candidates' earnings for the years 1993 and 1995-2010 from the official tax registry. Earnings are subject to individual taxation in Finland, and the earnings variable includes the sum total of individual earnings from both primary and secondary jobs. For parliamentarians, therefore, it includes their official salaries as well as any other earnings that they may have. Tax-free compensation for expenses is not included in this variable. (The implications of tax-free compensation for our results are discussed in Section 6.) Naturally, any unofficial income not reported to tax authorities is not included in the variable: Income data from tax records may suffer from misreporting. This potential problem should however be no more severe in our case than in earlier literature. Income data does not include information on bribes, but also wealth and estate data may suffer from underreporting, as someone who is willing to take a bribe may also have an unreported offshore bank account, or have some wealth in other assets that are easy to misreport like cash or valuables.

Selected summary statistics of the earnings data as well as some background characteristics of the candidates are presented in Table I.<sup>8</sup>

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<sup>8</sup>The indicator for "previously elected" is missing for municipal election data, because there we do not

[Table I here]

Table I presents the summary statistics for candidates in parliamentary elections 1970-2007 and in municipal elections in 1996-2008, separately for elected and defeated candidates. The unit of observation is candidate-election year. The variable "Register data found" gives the percentage of candidate-election years which we were able to match with the tax registry. In parliamentary elections, income data was found for nearly all (99,7%) winning candidates, whereas the success rate was 85% for defeated candidates. However, the success rate was much higher for candidates who lost narrowly, with only around 5% of missing data near the threshold of getting elected. For candidates in municipal elections, income data was found in practically all cases, because this data is from recent elections where dates of birth are included in the official election data.

Table I shows that the fraction of men is slightly higher among those candidates who were elected and winning candidates are slightly older than losing candidates. The fraction of incumbents and parliamentary candidates who have been elected in some previous parliamentary election is higher among winning candidates.<sup>9</sup> Elected candidates had higher earnings both before and after the election. Clearly, as expected, winning and losing candidates are different in many dimensions—so conceivably also in those that we cannot observe. Our estimates use data on candidates who were close to getting elected; among these candidates, any differences in both observable and unobservable background characteristics should be randomized out. Indeed, we show in Section 6.2 that close winners and losers are similar by their observable background characteristics.

In what follows, we use three year windows between elections for the purpose of measuring average earnings during a given electoral period ( $e$ ). We drop the earnings from election years, since they include both pre- and post-election earnings.<sup>10</sup> Earnings in the first electoral period ( $e = 1$ ) after getting elected at time  $t$  therefore refers to the average earnings in years  $(t + 1)$ - $(t + 3)$ , the second electoral period ( $e = 2$ ) refers to earnings in years  $(t + 5)$ - $(t + 7)$ , and so on. Similarly,  $e = -1$  corresponds to average earnings in the last electoral period before the election, i.e.,  $(t - 3)$ - $(t - 1)$ . All income variables were deflated by the cost of living index from Statistics Finland and are measured in 2011 euros.

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know who was elected prior to 1996. This data does not exist in digital form and would be difficult to gather.

<sup>9</sup>The information on whether a candidate was elected in pre-1970 elections was collected from the official web site <http://www.eduskunta.fi/thwfakta/hetekau/hxent.htm>.

<sup>10</sup>Parliamentary elections take place in March, and the new parliamentarians start their term right after the election. Municipal elections take place in October, and terms start in the following January.

## 6 Parliamentary elections

### 6.1 Results

We first present a graphical analysis of the data on parliamentary elections. In Figure 2, the candidates have been arranged by the measure of electoral closeness (see Section 3 for details on  $pmargin$ ) and divided into bins of width 1. Extreme bins, where  $|pmargin| > 50$ , are excluded from the figure for clarity.<sup>11</sup> Zero on the horizontal axis is the threshold between losers and winners. In Figure 2, we plot the bin averages of candidates' average earnings in the first electoral period after the election. (Marker size is proportional to the number of observations in the bin). We also fit a series of local linear regressions of the income variable on  $pmargin$  using a triangle kernel and optimal bandwidth as defined by Imbens and Kalyanaraman (2012). The solid line shows the fitted values and the dashed lines show the associated 95% confidence intervals.

[Figure 2 here]

Figure 2 reveals that getting elected to parliament increases the subsequent annual earnings of close winners: there is a clear jump of about €20,000 at the threshold of getting elected. We report RD estimates of the causal effect of getting elected to parliament on subsequent average annual earnings in Table II. We again use the optimal bandwidth in the main specification, but to have a first impression of robustness, we also report the results at 1/2 of the optimal bandwidth. In Section 6.2, we further show that the results are robust to all reasonable bandwidths.

[Table II here]

It is important to note that in each column of Table II we look at the effect of getting elected once, at time  $t$ , on earnings at a different time horizon.<sup>12</sup> The individual may or may not be elected in subsequent elections (i.e. between  $e = 1$  and  $e = 2$ , for example). This does not pose a problem for our analysis: to the extent that such re-election is a consequence of getting elected at time  $t$ , any effect that it has on subsequent earnings is part of the causal effect on earnings of getting elected at time  $t$ , and will show up in our estimates of the earnings effect at longer horizons ( $e = 2$  and later).

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<sup>11</sup>Candidates with extreme values of electoral closeness do not affect RD estimates except by affecting the optimal bandwidth.

<sup>12</sup>For the precise definition of electoral periods ( $e = 1, e = 2, \dots$ ) see section 5.

The first column of Table II shows the impact of getting elected to parliament on average earnings in the first electoral period after getting elected. The estimated effect of about €20,000 per year is quite large, corresponding to approximately 25% of annual pre-election earnings of close candidates. As shown in Table A.1 in the Appendix, this finding is robust to using alternative specifications of the earnings variable. However, the effect declines quite rapidly over time. In the second electoral period after getting elected (column 2), the effect diminishes to about €8,000 but is still statistically significant. By the third electoral period (column 3), the effect is no longer significant at the optimal bandwidth. Column 4 shows that getting elected increases average annual earnings after the election by approximately €8,000 when we average over all post-election years (i.e. all years after time  $t$ ).<sup>13</sup> Pooling data enables us to use data from all elections, but also implies that the effect is measured at very different lags for different individuals, with up to a 30-40 year lag for candidates who got elected in the 1970s. Results at 1/2 of the optimal bandwidth provide a similar picture.

Figure 3 illustrates the duration of the effect on earnings. It shows the estimated effect of getting elected to parliament on average earnings during subsequent electoral periods, up to seven electoral periods after the election. At longer lags the number of observations becomes low and standard errors become very large. The effects in periods 1-3 correspond to the estimates presented in the first three columns of Table II. It is notable that a small effect of around €5,000 appears to persist for a long time, even though it is not statistically significant for any single electoral period beyond the second. A stronger effect can be detected when we pool all electoral periods after the election (see Table II), as we then have twice as many observations and hence more power to discern smaller effects. Figure 3 also shows the estimated effect in two electoral periods before the election, when we should of course observe no effect; this is indeed what we find.

[Figure 3 here]

Finally, column (5) of Table II shows the effect of a parliamentary election win on capital income. It is not obvious which way this effect should go, since capital income depends on factors such as individual savings behavior. We find no clear effect on capital income in the first electoral period after getting elected, and we can rule out large effects to any direction. Figure A1 in the Appendix illustrates the impact on capital income over

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<sup>13</sup>We use the optimal bandwidth of the main specification (column 1) in all specifications in Table II. The optimal bandwidth for longer lags is larger, and yields larger point estimates. However, a larger bandwidth may increase bias (as we would ideally want an estimate of earnings arbitrarily close to the cutoff on each side).

time, analogously to Figure 3. It seems clear that getting elected to parliament does not have sizeable effects on capital income in the longer term either, although a small negative effect (under € 5,000) appears in the third electoral period.

## 6.2 Validity and robustness

We have so far presented two crucial pieces of evidence for the validity of our RD design. First, the distribution of the assignment variable is continuous at zero, as confirmed by the McCrary-test, and seen in Figure 1. Second, the estimated effect of getting elected on earnings *prior* to the election is zero, as seen in Figure 3.

As for bandwidth choice, we have used as baseline the optimal bandwidth as defined by Imbens and Kalyanamaraman (2012), henceforth IK, but there are also many other ways of choosing the bandwidth for RD estimation. The key point about bandwidth choice is to notice that our results are robust to a wide range of bandwidths, as can be seen from Figure 4 where we plot our main RD estimate and its confidence intervals against the bandwidth. Bandwidths that are much more narrow still produce quantitatively similar and reasonably precise estimates; for example, using a bandwidth that is 10% of the IK bandwidth results in a slightly higher and a clearly significant estimate at 25,000 €/year.<sup>14</sup>

[Figure 4 here]

One drawback of the IK bandwidth criterion is that it does not take into account the need to balance predetermined variables. We next check whether there are any jumps in predetermined variables at the threshold of getting elected, by plotting similar figures as Figure 2 for our predetermined variables. Here we use the same set of individuals as used in the main specification and in Figure 2, which means dropping those for whom earnings data could not be matched. We first do this analysis for two measures of pre-election earnings (average earnings in the last electoral period before the election, and average earnings in all years prior to the election, see Figure 5). Appendix shows corresponding figures for our other predetermined variables (incumbency status, the status of never having been elected to parliament, vote share, gender, age, region, and indicator variables

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<sup>14</sup>Our results are also robust to using the bias corrected RD estimation procedure and robust variance estimator introduced by Calonico et al. (2014). The estimate for the earnings effect in  $e = 1$  using a local linear specification and the Calonico et al. optimal bandwidth is €16,940 (s.e. 3,625). A specification using 4th order polynomials on both sides of the cutoff, for candidates with  $-50 < pmargin_i < 50$ , yields an estimate of €16,033 (5,020). See, however, Gelman and Imbens (2014), on the pitfalls of polynomial specifications.

for membership in the three main parties). At the IK bandwidth one of the important predetermined variables, incumbency status, would appear to show a jump at the electoral threshold that is just barely significant at 5% level; the same is true for the ever-previously-elected status, which is highly correlated with incumbency. However, all these variables (and all other predetermined variables) balance cleanly at the more conservative 1/2 of IK bandwidth. This is another reason why we report in our tables, for all RD specifications, the coefficient of interest also as estimated at 1/2 of the relevant IK bandwidth. The full table of all background balance checks at both bandwidths is included in our Appendix.

The most important predetermined variable is, of course, the pre-treatment level of the outcome variable itself. Average yearly income prior to the election is clearly continuous at the election threshold, even at the relatively wide IK bandwidth, as can be seen from Figure 5. We show in the Appendix that our main specification is also robust to the inclusion of various individual control variables.<sup>15</sup>

[Figures 5 here]

### 6.3 Results by subgroup

Table III presents results for our main outcome variable—earnings in the first electoral period after getting elected—for various subgroups. The grouping for continuous variables (age, pre-election income) is done by splitting the sample at the median. There are some differences in the point estimates, e.g. women seem to gain more than men. Centre Party candidates seem to gain more than candidates from the other two major parties (SDP and NCP). These differences are likely to be driven by differences in outside options: women (outside politics) in general earn less than men, and the Centre Party has typically been prominent in rural areas where outside earnings are likely to be lower. However, the differences between the subgroups are not statistically significant, so our evidence of heterogeneous effects is only suggestive.

[Table III here]

There are two instances where we do find significant differences among subgroups. First, low income individuals gain more than high income individuals. This is a mechanical effect of a low outside option. However, a more substantive finding is the difference

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<sup>15</sup>In a valid RD design, adding control variables can make the estimate more precise, but should not have a large effect on its magnitude; this is indeed what we find.

between candidates in elections prior to the year 2000 and those who ran in later elections: the estimated effect for the former is about €12,000 and about €30,000 for the latter, and the difference is statistically significant. This finding reflects the fact that the salaries of Finnish MPs were increased by approximately 35% in September 2000. Note, however, that in the estimates reported in column (17) of Table III, some of the earnings observations of candidates elected in the 1999 election are before and some after the salary reform. Therefore the division at the median election year in Table III does not coincide exactly with the occurrence of the salary reform. The contrast between the estimates in columns (17) and (18) motivates us to discuss the impact of the salary reform in more detail in the next subsection.

## 6.4 Discussion

What might explain the change in earnings caused by getting elected into parliament? First, there is a direct wage effect: it may be that parliamentarians receive a salary that exceeds the earnings that they would have obtained outside politics. It is not obvious that this effect has to be positive: being an MP is a full-time job, so entering national politics has a direct opportunity cost of lost earnings outside politics. Second, there may also be indirect effects: if political connections are a valuable asset outside politics, then becoming a parliamentarian may be a stepping stone into profitable secondary assignments, such as memberships in company boards. In fact, more than half of MPs elected in 2011 had at least one position of trust in a private or state-owned company board or governing council, and some of these positions pay quite well.

Next we present two lines of analysis that complement our main findings on the effect of a parliamentary election win on earnings, and help cast some light on the mechanisms involved. The first set of findings is related to the time profile of the earnings effects. The second set of findings relates to the individual incumbency effect, that is, the causal effect of getting elected at a given election on the chances of getting elected at later elections.

### Time profile of earnings effects

A number of factors point towards direct wage effects being the key factor behind our results. First, we found the largest impact on earnings in the first electoral period after getting elected (see Figure 3). This is a period where winning candidates were all MPs by definition, which points towards a direct wage effect. By contrast, indirect benefits from connections made while in parliament could take some time to come to fruition. To

further investigate to what extent the decline in the earnings effect, evident in Figure 3, is due to close winners leaving the parliament in subsequent elections, we have constructed Figure 6. There each point estimate represents a comparison of earnings at electoral period  $e$  between the following groups of candidates: (i) close winners who are *also* in parliament during electoral period  $e$ ; and (ii) close losers who are *also* not in parliament during electoral period  $e$ . Hence, at each point in time, we are comparing individuals who are still in parliament and thus enjoying a parliamentarian’s salary to individuals who are not in parliament. These estimates cannot be given a causal interpretation, because individuals who are elected on several occasions may systematically differ from others, even if they were all close winners or losers at some point in time. Nevertheless this figure is useful for illustrating different career paths after a close election. Contrary to Figure 3, the time profile of the effects depicted in Figure 6 is not decreasing over time. This suggests that the relatively quick disappearance of the effect on income is due to a significant fraction of initial close winners not getting re-elected or dropping out of parliament. (The picture is very similar if we compare those close winners at time 0 who were elected in *every* election until electoral period  $e$  to close winners at time 0 who were *not* elected in any of the next  $e$  elections. Leaving out the observations after the 2000 salary reform does not affect the overall shape either.) Therefore, it appears that the returns from getting elected occur mainly during the individual’s political career, while the effect on subsequent earnings outside politics is limited.

[Figure 6 here]

We also examine the returns to office separately before and after the year of salary reform, which went into effect in September 2000, as MPs’ salaries were increased by about 35% on average. Figure 7 shows the average yearly earnings in the first electoral period after getting elected, separately for the 1991 and 1995 elections (left), and for the 1999–2007 elections (right).<sup>16</sup> It is clear that the returns to office are sizeable only after the salary reform, where the estimated effect is about €28,000 per year (and highly significant), considerably higher than the effect estimated from the full data reported in Table II. This also suggests that the bulk of the returns to office comes from the direct wage effect.

[Figure 7 here]

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<sup>16</sup>Here we exclude earnings in 2000, since it involves a mixture of pre- and post-reform earnings.

Our individual earnings data does not provide a breakdown by source, so we don't observe individual salaries (as MPs can have other sources of earnings as well). We have obtained average salaries and average starting salaries of new parliamentarians from 1992 to 2011, and these are depicted in Figure 8.<sup>17</sup> These figures include the taxable compensation for expenses that was paid until January 2000. Prior to the reform, the salaries of Finnish MPs were the lowest in the EU.<sup>18</sup> The average starting salary in the two electoral periods after the 1991 election was €46,100. Together with Figure 7 this suggests that the average outside income of sitting MPs was around €15,000. Strikingly, the average starting salary of MPs used to be lower than the average income of close losers, so, prior to the salary reform, only side jobs appear to have allowed close winners to reach about the same taxable income as close losers. After the year 2000, the starting salary was a much higher €71,300.

It should be noted that the actual income of MPs includes not just the taxable salary but also a monthly tax-free compensation for expenses (which does not show up in our individual earnings data). The size of this compensation depends on whether the MP lives in or near Helsinki (where the parliament is located) and on whether they have a second home there. The average annual amount for those living in or near Helsinki was €11,000 prior to the salary reform, whereas those elected from the rest of the country received on average €20,200 if they had a second apartment in Helsinki. (These amounts were not significantly affected by the salary reform.) Taking this tax-free compensation into account, one could argue that there was in fact a small return to office already before the salary reform, because anecdotal evidence suggests that a considerable part of this compensation is, in effect, additional disposable income for the MPs.

[Figure 8 here]

The estimated income gain of about €30,000 per year after the year 2000 means that close winners benefit on average about €120,000 over a four-year term, before possible additional benefits from tax-free compensation for expenses. To put this gain into perspective, private campaign spending by MPs and those elected as deputies in 2011 averaged €12,000, with only 2% spending more than €50,000 of their own money.<sup>19</sup> Total spending, when including money from other sources, averaged €32,000, with less than a

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<sup>17</sup>Data source: Authors' calculations based on data provided by the Parliament Information Department and Accounts Office.

<sup>18</sup>Makkonen (2000).

<sup>19</sup>The closest loser in every district-party with at least one MP becomes a deputy MP. Deputy MPs step in only if an MP becomes incapacitated for the whole remaining term.

fifth spending over €50,000 and none exceeding €100,000.<sup>20</sup> Unsuccessful candidates are, unfortunately, not required to report their campaign costs, so we cannot calculate the expected private return to campaign spending. But those who get elected to the Finnish parliament increase their earnings by much more than is spent on their campaign.

### **Incumbency advantage**

Incumbency effect is the causal effect of winning an election on the probability of winning subsequent elections. In this section we estimate the individual incumbency effect in our parliamentary election data. This is partly of independent interest, but also helpful in further examining whether the longer-term economic returns of getting elected are driven by subsequent re-elections.

There exists a sizeable empirical literature on the incumbency effect, but most of it has studied elections with one-member districts where the incumbency effects for parties and candidates are hard to disentangle; see Gelman and King (1990) and de Magalhaes (2015). Lee (2008) finds a very large incumbency advantage for parties in U.S. House elections using RD: districts where a Democrat narrowly won an election are 45 percentage points more likely to elect a Democrat in the next election than those where a Democrat narrowly lost. Cox and Morgenstern (1995) find that the incumbency advantage has increased over time, but less so in states using multimember districts than in states using single-member districts. Liang (2013) studies the persistence in the number of seats for parties in Swedish local elections, in a manner analogous to the party incumbency effect in Lee (2008), but finds a much weaker effect. Hyytinen et al. (2014) find no incumbency advantage in local elections using the same data as we do; we return to the local election case in the next section.

[Table IV here]

Our estimation of the incumbency effect in parliamentary elections is shown in the first column of Table IV. Incumbency advantage is relatively modest in Finnish parliamentary elections, about 18 percentage points.<sup>21</sup> Note that in a two-party FPTP system, the incumbency effect necessarily includes a party effect (i.e. the candidate's party won the seat in the district), but under the Finnish electoral system, where parties typically win

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<sup>20</sup>Data from <http://www.vaalirahoitus.fi/fi/index/vaalirahailmoituksia/ilmoituslistaus/EV2011.html>, maintained by the National Audit Office. This data is not available for earlier elections.

<sup>21</sup>To be exact, we estimate the impact on the combination of running for election and winning; see Lee (2008) for a discussion.

multiple seats in each district, we estimate the incumbency effect for individuals. The estimated effect is again robust to the bandwidth choice, remaining almost the same when the bandwidth is halved from the IK benchmark. Figure 9 shows the persistence of the incumbency effect, where we plot the effect of getting elected at time 0 on the likelihood of getting elected at an election  $e$  elections later. The effect loses its strength relatively quickly, becoming insignificant in the third subsequent election. This is again consistent with the direct earnings effect driving the estimated return to being elected. The direct effect attenuates over time as the impact of the close win on still being elected wears out.

[Figure 9 here]

## 7 Municipal elections

Next we present a similar analysis as in the previous section for municipal elections. We omit repeating many of the definitions and procedures that are the same as in the previous section, and some of the figures are relegated to the Appendix. Similar to Figure 2, Figure 10 plots candidates' average earnings in the first electoral period after getting elected against our measure of electoral closeness. The figure reveals a slight upward shift in subsequent earnings at the threshold of getting elected, but the effect is an order of magnitude smaller than in parliamentary elections. The much larger number of observations nevertheless allows this small effect to be measured quite precisely.

[Figure 10 here]

We report our main RD estimates in Table V. Getting elected to a municipal council increased annual earnings in the first electoral period by approximately €1300. Over time the effect remains small, and the number of observations and the precision of the estimate keep getting smaller. The "pooled" estimate indicates that getting elected increased subsequent annual earnings by approximately €1000. Getting elected to a municipal council has no effect on capital income.

[Table V here]

As shown in Table A.4 in the Appendix, the results are robust to alternative income measures. We also ran the same battery of validity and robustness checks for the municipal level RD estimation as we did for parliamentary elections, and none of the checks

indicate any problems. Overall the results are qualitatively similar to what we found for parliamentary elections, but all magnitudes are much smaller. Estimates by subgroup (similar to Table III) reveal no significant differences between groups; they are reported in the Appendix.

Municipal councilors receive only a fairly modest compensation for attending meetings. The councilors continue in their civil occupations during their time in office. There may be indirect wage effects from working in politics at local as well as national level: if politicians acquire human capital or form local political connections that are valuable for employers, this can translate to higher pay in one's main job. Again, it is also possible that the opportunity cost of spending time at council meetings or other related activities result in negative monetary returns for some councilors.

Unfortunately, data on compensation received by municipal councilors has not been systematically collected in Finland. The average compensation per meeting (calculated across municipalities) was €60 in 2009, and the average number of meetings per year was 8 in 2007.<sup>22</sup> Multiplying these figures together yields a crude estimate of the average annual compensation at €480. This is approximately half of the size of the effect that we have estimated.

Several factors need to be kept in mind when interpreting the above figure. First, there is large variation in the compensation per meeting as well as the number of meetings across municipalities, with larger municipalities usually holding more meetings and paying higher compensation per meeting. Second, the head of the municipal council, as well as other councilors holding some leading positions within the council, receive higher compensation. Thus the crude proxy calculated above for the average annual compensation is an underestimate of the true compensation. On the other hand, it is also an *overestimate* in the sense that compensation is only paid for those meetings actually attended by each councilor, and the above calculation assumes the attendance rate to be 100%. Unfortunately, we have no data on attendance rates at council meetings.<sup>23</sup>

An important issue to note is that, in municipal elections, the closest losers in each party that win seats become deputy members of the municipal council. Thus in our

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<sup>22</sup>This data is available on the website of the Association of Finnish Local and Regional Authorities at <http://www.kunnat.net/fi/tietopankit/tilastot/kuntavaali-ja-demokratiatilastot/kuntien-luottamushenkiloiden-palkkiot-ja-korvaukset/Sivut/default.aspx> (In Finnish only). The data for the number of meetings is available for 2007 only, whereas municipal-level data for compensation per meeting is available for 2009. Unfortunately these data are not available for the same year.

<sup>23</sup>Average duration of council meetings is available for 2007. Combined with the compensation data for 2009, this allows us to calculate a proxy for the average hourly compensation for attending council meetings. For example, in Helsinki, it was €70 per hour.

municipal election data almost all close losers are deputy councilors. The deputy members attend municipal council meetings when any of the actual councilors from their own party cannot.<sup>24</sup> We do not have data on attendance by deputy members, but anecdotal evidence suggests that this occurs regularly, in particular in large municipalities and for major parties. Further, close runners up are also often nominated to various positions of trust in local politics. These two facts together offer one explanation why the returns to office in local politics were found to be modest, as many narrow losers get part of the same "treatment" as those elected.

Campaign costs in municipal elections are usually rather modest. In 2012, 38.0% of those who were elected members or deputy members in the municipal council of Helsinki (the capital and the largest city) spent less than €1000 of their own money, while only 5.3% spent more than €10,000 of their own money in campaigning.<sup>25</sup>

Finally, for independent interest, we report our estimates of the individual incumbency effect in municipal elections in the second column of Table IV. We find a very modest incumbency effect, about 2.5 percentage points, so much smaller than the 18 percentage points we found for parliamentary elections. This incumbency effect is just barely significant at the 5% level and declines to close to zero when the bandwidth is halved. This is consistent with recent findings of Hyytinen et al. (2014), who use the same municipal election data as we do. They find that the RD-estimated incumbency effect is not robust to bandwidth selection, and that among the subset of 1350 exact within-party ties (where the winner was selected by lot) the null hypothesis of no incumbency effect cannot be rejected. One feature that may explain the lack of an economically significant incumbency effect at the municipal level is that there is not much difference between incumbents and challengers in terms of the amount of media coverage, as both get very little of it.

## 8 Conclusion

We presented a simple bootstrap approach for calculating the electoral closeness of candidates under any electoral rule, and applied it to study private returns to being elected in Finnish politics. We showed that being elected to the parliament increases annual earnings initially by €20,000, corresponding to 25% of annual pre-election earnings of close

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<sup>24</sup>This problem does not arise in the parliament as there are no deputy members who would participate in meetings when the MP is absent. In case an MP dies or resigns during the electoral period, he or she is replaced by the closest loser from the same list.

<sup>25</sup>The data is from <http://www.vaalirahoitus.fi/fi/index/vaalirahailmoituksia/ilmoituslistaus/KV2012/091.html> maintained by the National Audit Office.

candidates, while being elected to the municipal council has only a small positive impact on subsequent earnings. Our data allows us to study the impact on earnings both in the short run and in the long run, and the analysis of the earnings dynamics of politicians is a unique feature of our paper compared to earlier literature on returns to political office. Our analysis of the time profile of earnings shows that the positive effects on earnings wear out fairly quickly, and mainly accrue during the time in office. Most of the positive effects for MPs are direct effects from MPs having higher salary than is the outside option of a typical close election winner. The gain from being elected to the parliament became significant, about €30,000 per year, only after salaries were increased by about 35% in the year 2000. Given that MPs also receive tax-free compensation for expenses that is not included in these numbers, our estimates can be viewed as a lower bound of the earnings effect of being elected to the parliament.

Most previous estimates of returns to office and incumbency advantage have been obtained in the context of majoritarian voting systems. It is not clear, à priori, how these should differ under a proportional system. An MP elected in a majoritarian system has monopoly power in his or her district, which could lead to larger rents, while competition between MPs from the same district could reduce rents in a proportional system. On the other hand, reduced accountability under the proportional system could result in MPs spending less time in public service and more time in collecting rents. It has been argued that one reason why Italy abandoned proportional representation was that it resulted in political fragmentation which in turn allowed politicians to extract considerable rents. In our data the electoral system stays constant, so we cannot estimate the effect of the electoral system itself, but our method of measuring electoral closeness could be applied in contexts where a change between systems takes place, as it has, for example, in France and Italy.

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Table I. Summary statistics for elected and defeated candidates.

VARIABLES	Parliamentary elections 1970-2007						Municipal elections 1996-2008								
	(1)	(2)		(3)	(4)	(5)		(6)	(7)	(8)		(9)	(10)	(11)	(12)
	<i>N</i>	Not elected		S.D.	<i>N</i>	Elected		S.D.	<i>N</i>	Not elected		S.D.	<i>N</i>	Mean	S.D.
Female	16,361	0.338	0.473	2,200	0.312	0.463	114,125	0.403	0.491	47,138	0.346	0.476			
Age	14,077	44.98	12.00	2,196	46.95	9.294	114,125	45.85	12.84	47,138	47.95	10.86			
Incumbent	16,363	0.0277	0.164	2,200	0.619	0.486	114,125	0.0629	0.243	47,138	0.580	0.494			
Previously elected	16,363	0.0431	0.203	2,200	0.666	0.472									
Vote share	16,363	0.005	0.010	2,200	0.0371	0.0411	114,125	0.005	0.005	47,138	0.0225	0.0149			
District size (seats)	16,363	16.53	7.076	2,200	16.42	6.489	114,125	40.29	16.50	47,138	33.68	13.47			
District size (votes)	16,363	228,820	97,078	2,200	226,995	89,696	114,125	21,815	46,124	47,138	9,964	25,182			
Southern districts	16,363	0.672	0.004	2,200	0.673	0.010									
NCP	16,363	0.117	0.321	2,200	0.211	0.408	114,125	0.194	0.395	47,138	0.176	0.381			
Centre	16,363	0.104	0.305	2,200	0.217	0.412	114,125	0.242	0.428	47,138	0.361	0.480			
SDP	16,363	0.114	0.317	2,200	0.266	0.442	114,125	0.222	0.416	47,138	0.211	0.408			
Other parties	16,363	0.666	0.472	2,200	0.305	0.461	114,125	0.342	0.474	47,138	0.252	0.434			
Register data found	16,363	0.851	0.356	2,200	0.997	0.0563	114,125	0.999	0.0317	47,138	0.999	0.0226			
Avg. Earnings 1993-2011	13,508	31,533	28,030	2,081	67,344	41,611	114,010	23,334	14,639	47,114	30,254	19,915			
Avg. Capital income 1993-2011	13,495	3,843	38,334	2,076	7,804	44,325	114,010	1,790	8,787	47,114	4,767	37,374			
Avg. Earnings in $e = 1$	8,622	31,049	23,671	999	81,793	32,947	114,000	24,665	16,688	47,114	32,482	22,451			
Avg. Earnings in $e = 2$	8,017	32,804	25,262	995	80,203	39,482	85,469	25,400	17,880	36,628	32,939	24,338			
Avg. Earnings in $e = 3$	7,052	34,505	27,710	986	79,781	47,713	57,087	25,519	18,883	24,546	32,557	27,278			
Avg. Earnings in $e = -1$	7,249	28,622	27,639	800	67,882	34,343	113,995	22,829	15,705	47,113	29,469	22,095			

Notes: Total number of unique candidates is 12,398 in parliamentary elections and 93,741 in municipal elections. Total number of annual earnings observations is 1,654,635.

Table II. Effect of getting elected to parliament on future income.

	(1)	(2)	(3)	(4)	(5)
	Average annual earnings in $e = 1$	Average annual earnings in $e = 2$	Average annual earnings in $e = 3$	Average annual earnings after the election	Average annual capital income <sup>1</sup> $e = 1$
Elected	19,999** (2,546)	7,823** (2,971)	5,995 (3,115)	7,986** (2,299)	2,671 (4,791)
Elected (1/2 bandwidth)	17,478** (3,544)	7,189 (4,262)	8,606* (4,320)	8,623** (3,222)	1,022 (7,447)
$N$	9,621	9,012	8,038	15,544	8,376

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 40.58, which is the optimal IK bandwidth (Imbens and Kalyanaraman, 2012) for column (1), the main specification.

<sup>1</sup>One outlier candidate with very high capital income is removed.

Table III. Effect of getting elected to parliament on average annual earnings in  $e = 1$ : Estimates by subgroup.

	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Female	Young	Old	South	North
Elected	18,445** (3,308)	23,106** (3,552)	19,828** (2,740)	19,131** (3,936)	17,773** (3,256)	24,489** (3,957)
Elected (1/2 bandwidth)	15,640** (4,312)	21,448** (5,951)	19,052** (4,099)	15,905** (5,288)	13,790** (4,331)	25,168** (6,047)
$N$	5,849	3,772	5,027	4,594	6,700	2,921
	(7)	(8)	(9)	(10)	(11)	(12)
	Low income	High income	Incumbent	Not incumbent	Previously elected	Never previously elected
Elected	40,524** (4,309)	21,121** (3,089)	23,245** (4,687)	19,068** (3,355)	24,483** (4,210)	17,720** (3,513)
Elected (1/2 bandwidth)	40,221** (6,871)	19,890** (4,165)	17,717** (5,802)	17,537** (4,482)	20,164** (5,504)	16,103** (4,656)
$N$	4,022	4,022	812	8,809	989	8,632
	(13)	(14)	(15)	(16)	(17)	(18)
	SDP	Centre	NCP	Other parties	Pre 2000 elections	Post 2000 elections
Elected	18,724** (4,517)	26,751** (4,061)	9,560 (7,284)	22,912** (4,513)	12,552** (2,994)	30,697** (4,141)
Elected (1/2 bandwidth)	10,901 (6,420)	26,728** (5,448)	8,637 (9,171)	24,169** (6,722)	14,366** (4,276)	22,581** (5,751)
$N$	1,121	1,071	1,105	6,324	5,601	4,020

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with same bandwidth (40.58) as in the main specification; see Table II.

Table IV. Incumbency effect.

Dependent variable: Same candidate elected in the next election.

	(1) Parliamentary	(2) Municipal
Elected	0.1788** (0.0361)	0.0254* (0.0126)
Elected (1/2 bandwidth)	0.191** (0.0504)	0.00593 (0.0171)
<i>N</i>	16,559	122,754
Bandwidth	28.35	20.81

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$ .

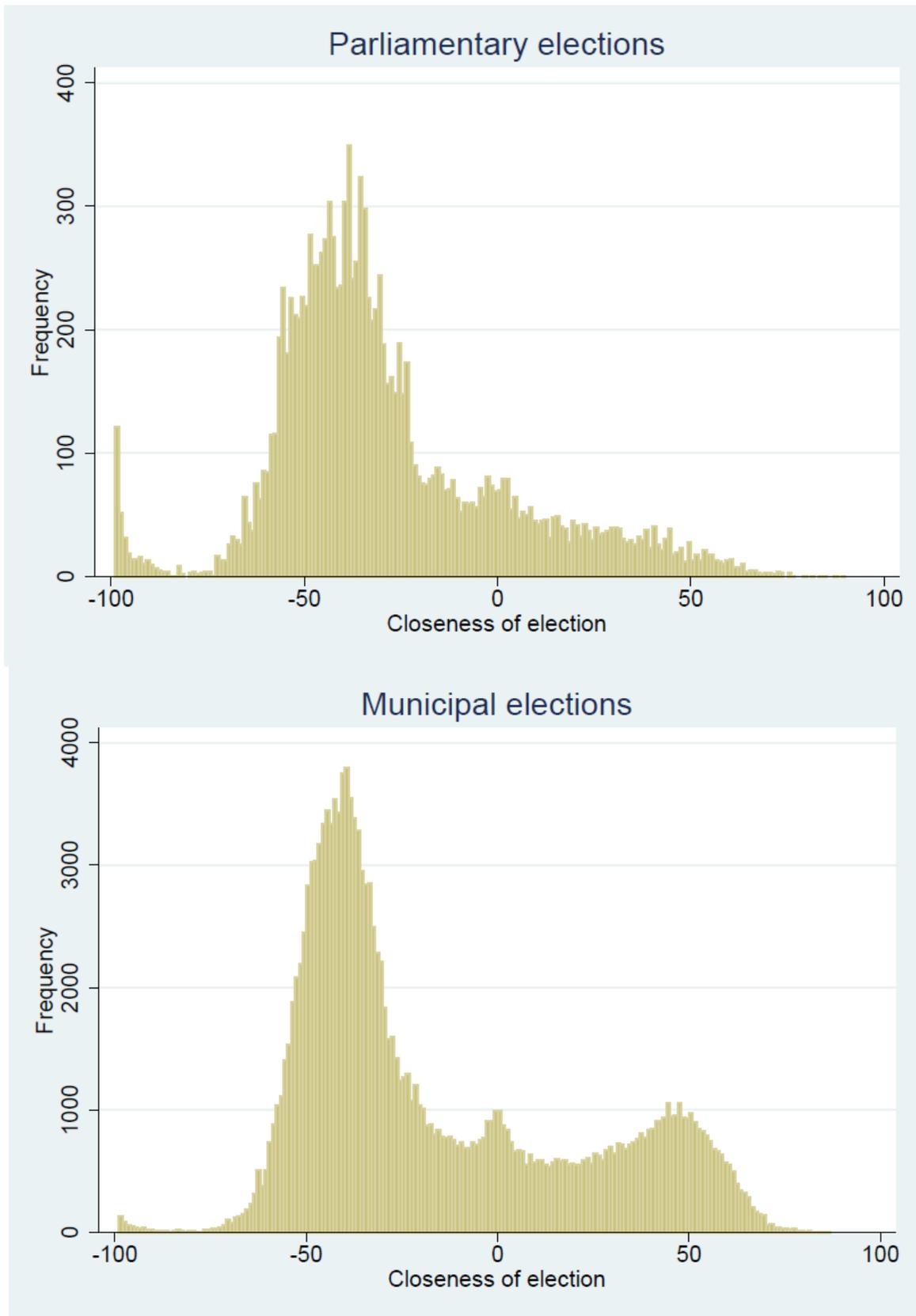
Local linear regression using triangle kernel with IK bandwidth.

Table V. Effect of getting elected to a municipal council on future income.

	(1) Average annual earnings in $e = 1$	(2) Average annual earnings in $e = 2$	(3) Average annual earnings in $e = 3$	(4) Average annual earnings after election	(5) Capital income in $e = 1$
Elected	1,255** (462.8)	882.7 (566.8)	1,444 (777.2)	1,044* (479.9)	188.1 (378.1)
Elected (1/2 bandwidth)	1,188 (611.9)	558 (758.2)	1,353 (1,084.7)	870 (641)	521.0 (464.3)
<i>N</i>	161,114	122,067	81,633	161,116	161,114

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 19.01, which is the optimal IK bandwidth for column (1), the main specification.



**Figure 1. Distribution of the forcing variable. McCrary-test detects no discontinuity at the cutoff (0) in either case: the test statistic has value (std. dev) 0.081 (.084) for parliamentary and -0.0067 (.0294) for municipal elections.**

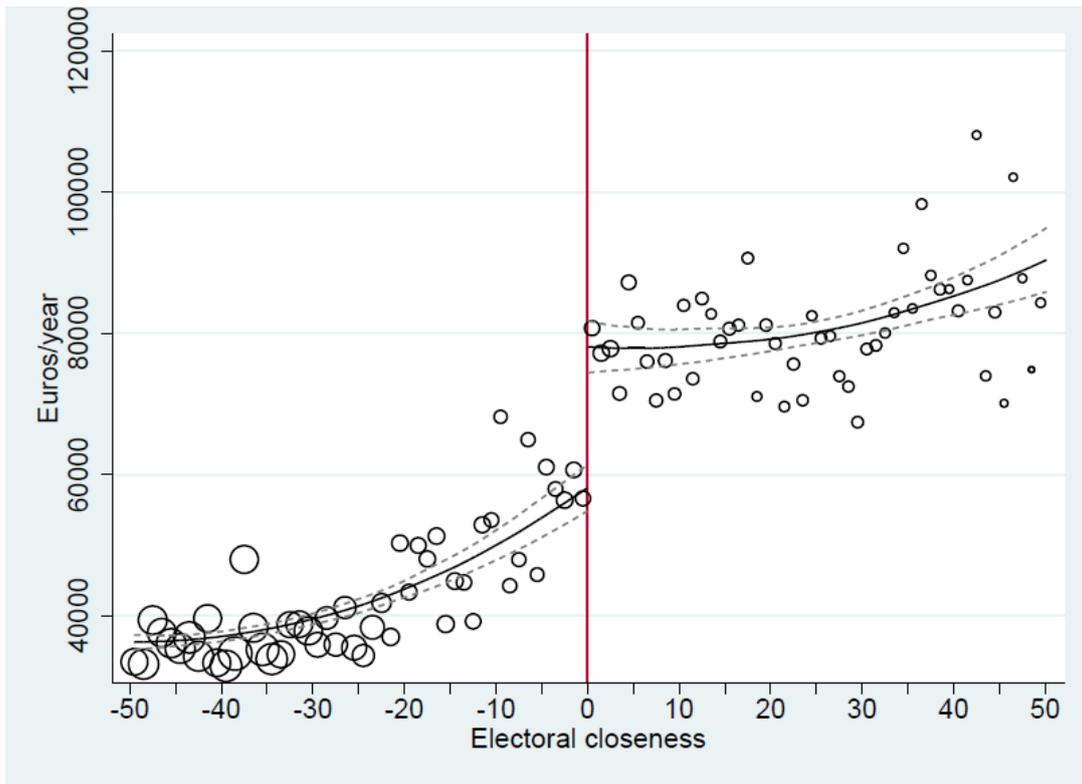


Figure 2. Estimated effect of being elected to parliament on earnings in the first electoral period after getting elected.

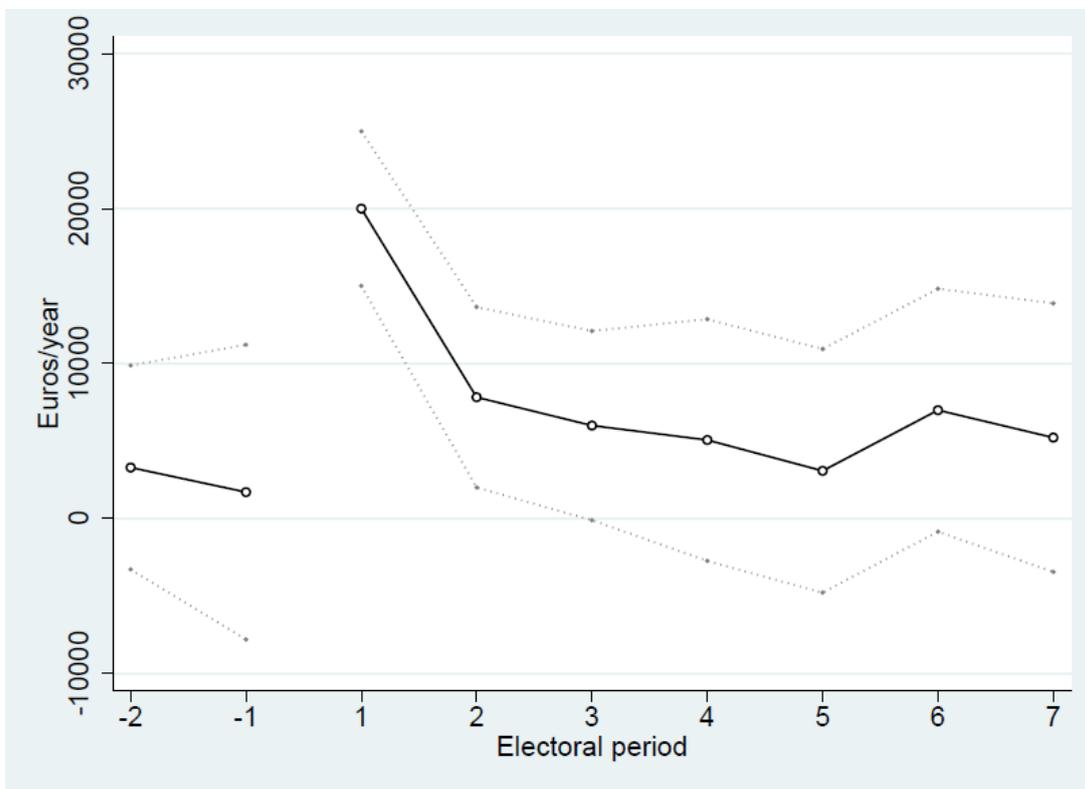


Figure 3. Estimated effect of being elected to parliament on earnings by electoral period. Negative periods refer to electoral periods before the election.

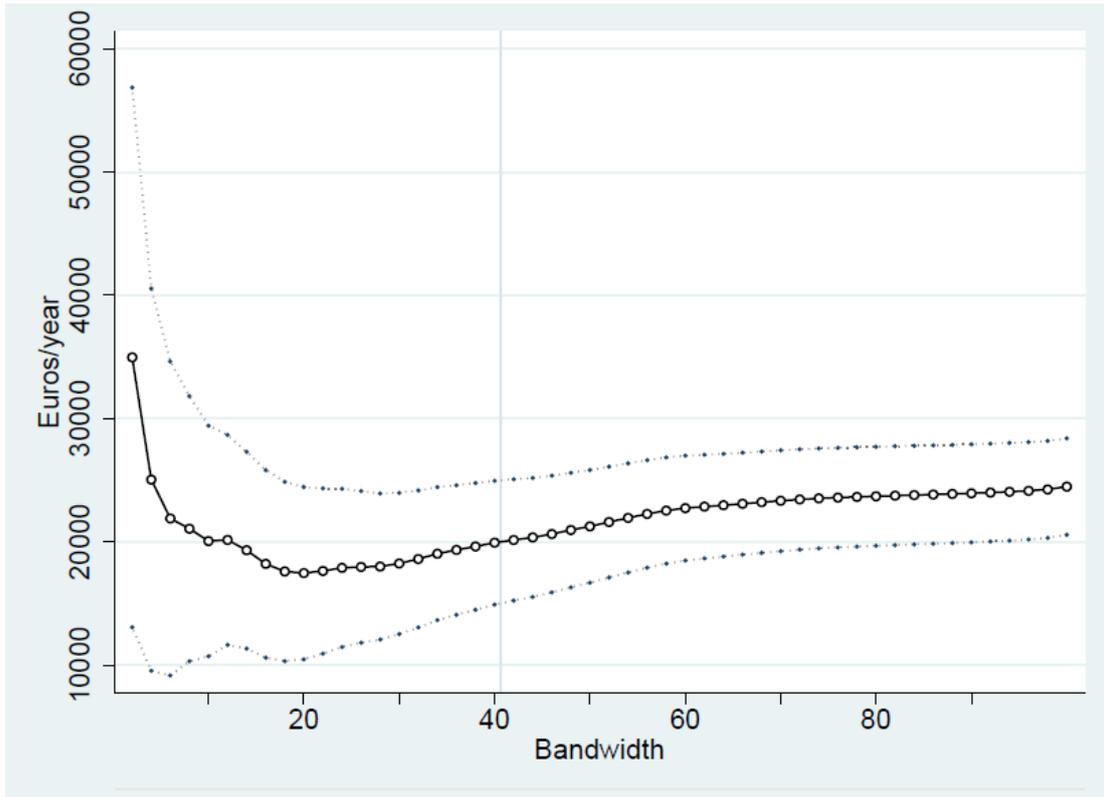


Figure 4. Robustness to bandwidth: Estimated effect of being elected to parliament on earnings in the first electoral period after election. Vertical line marks the Imbens-Kalyanaraman bandwidth.



Figure 5. Continuity of pre-election earnings (parliamentary elections). See Appendix for figures of other predetermined variables.

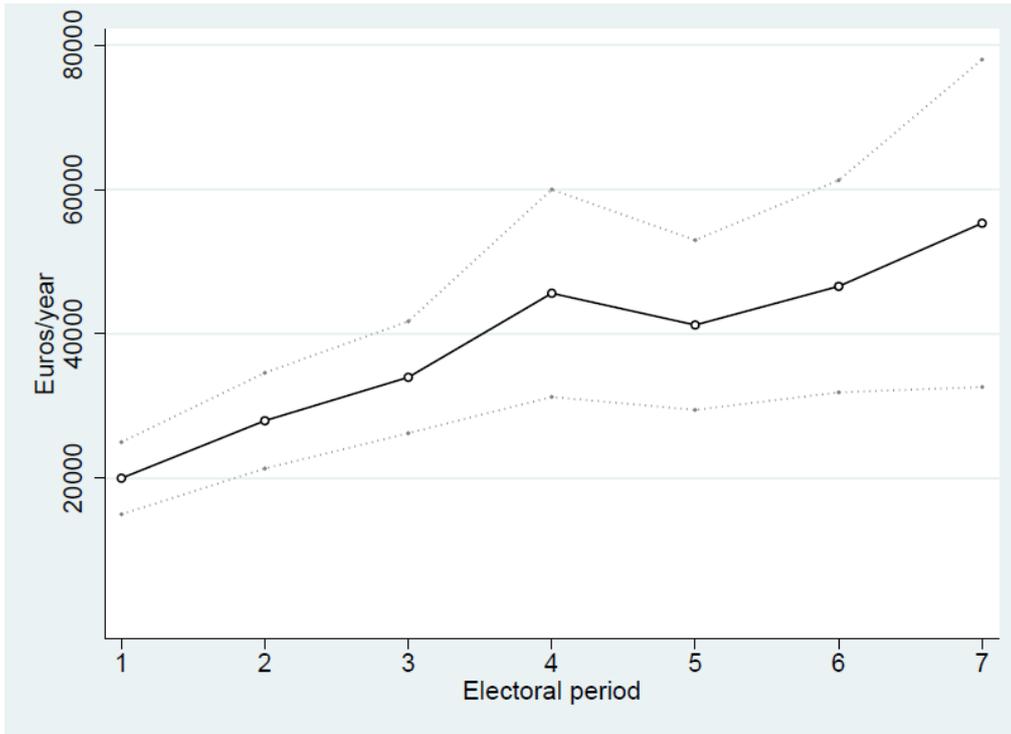


Figure 6. Time profile of the difference in earnings between (i) close winners who are in parliament and (ii) close losers who are not in parliament,  $e$  electoral periods after the close election. This is not a causal estimate (except at  $e=1$ ) as selection out of politics is unlikely to be random.

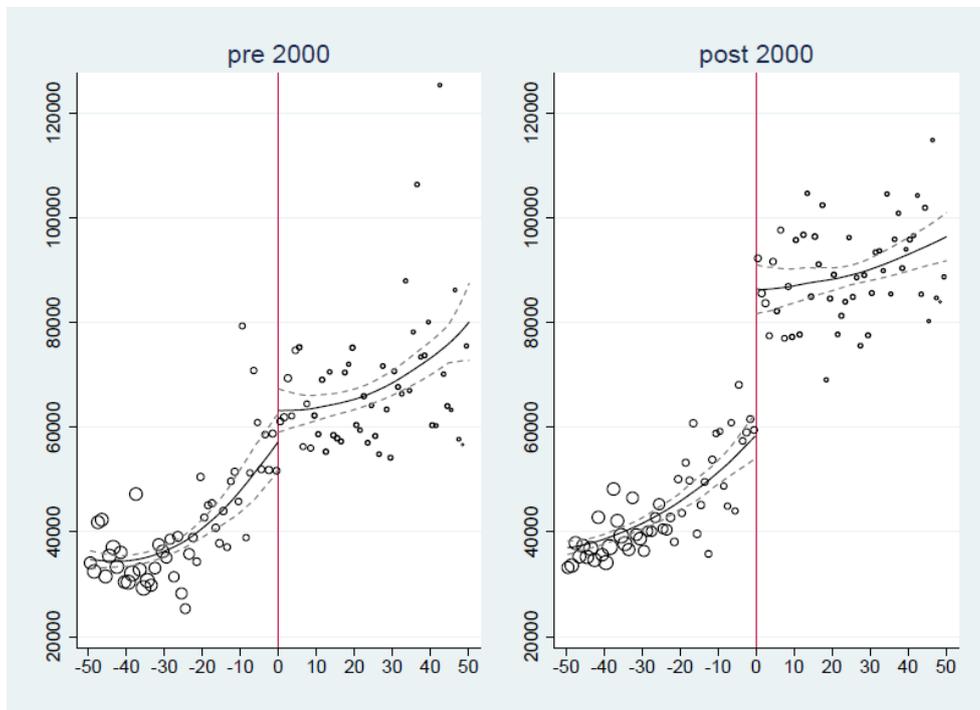


Figure 7. Estimated effect of being elected to parliament in the first electoral period after the election, before and after the salary reform of 2000. Left panel includes candidates in 1991 and 1995. Right panel includes post-2000 earnings for candidates in 1999, 2003, and 2007 elections.

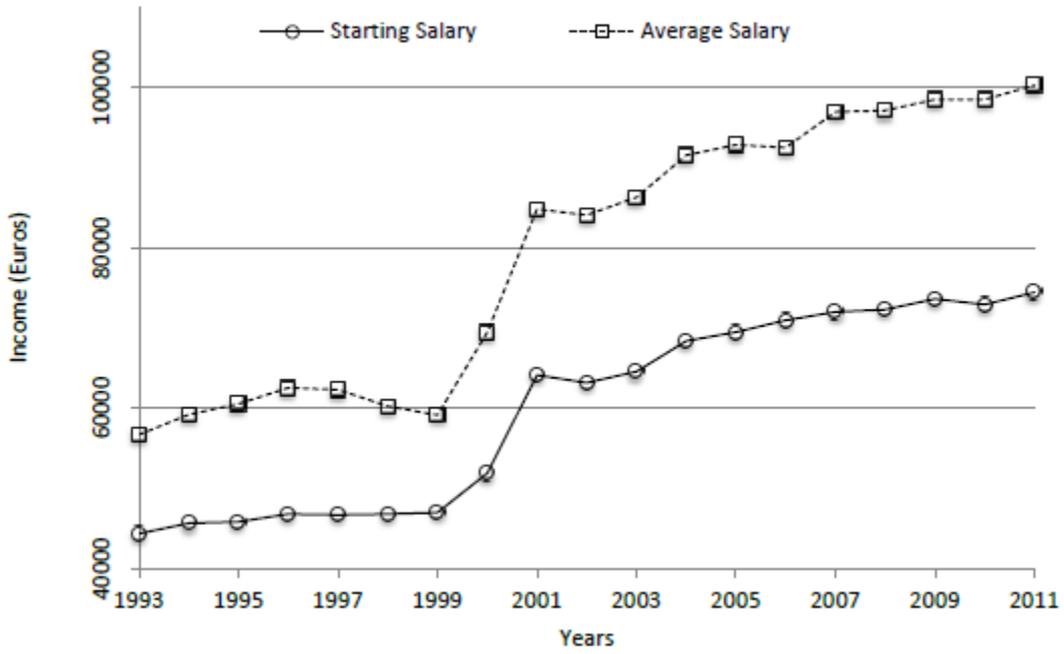


Figure 8. Average and starting salaries of MPs.

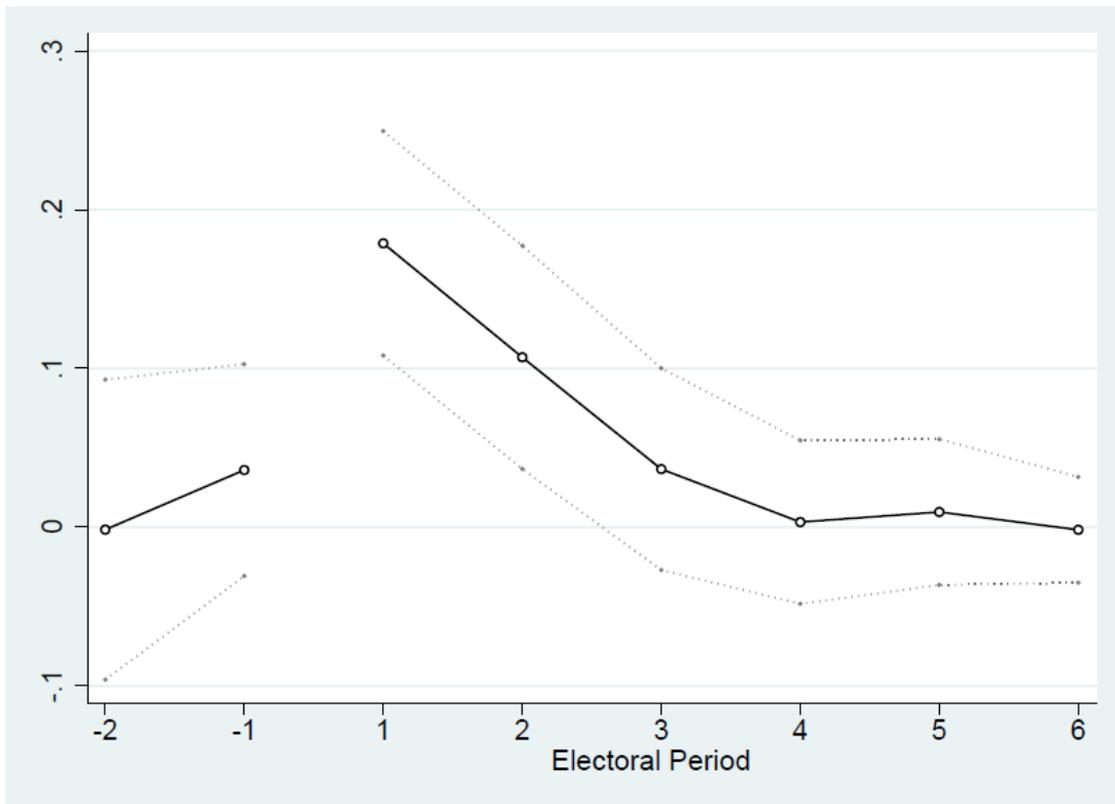
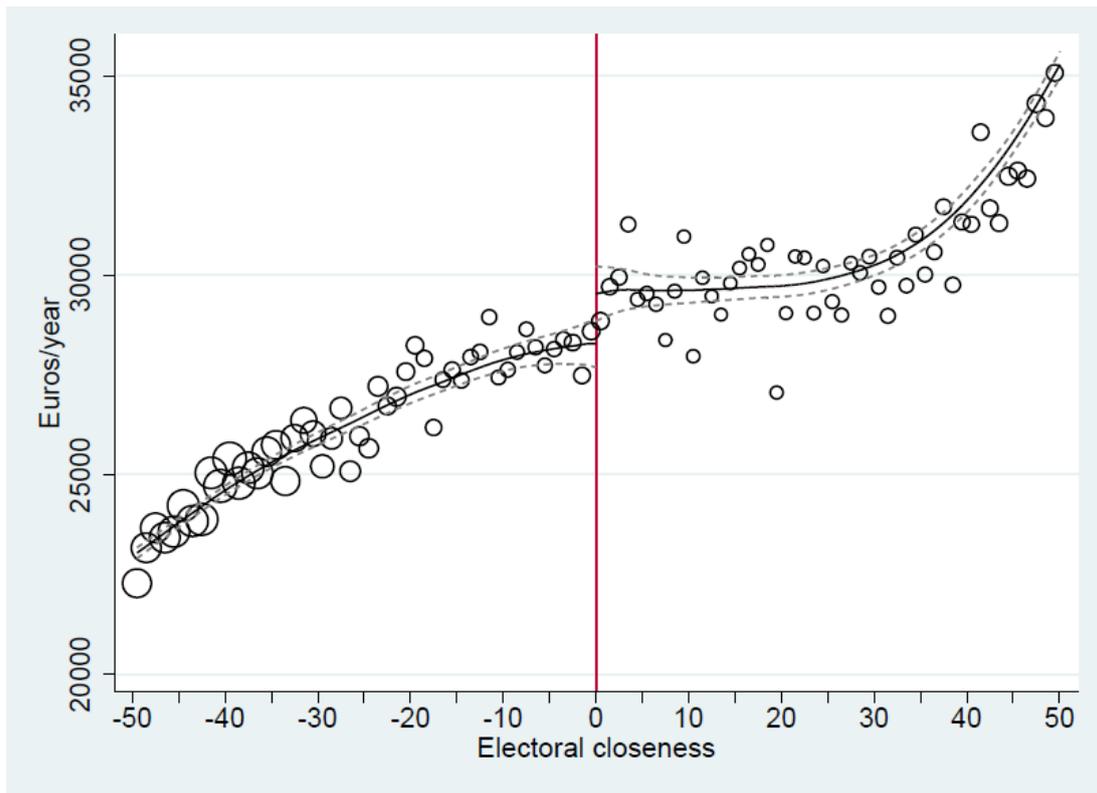


Figure 9. Incumbency advantage in parliamentary elections: Estimated effect of getting elected in electoral period 0 on the likelihood of getting elected  $e$  electoral periods later.



**Figure 10. Estimated effect of being elected to a municipal council on earnings during the first electoral period after the election.**

## Appendix

Supplementary Tables and Figures for "Returns to office in national and local politics" by Kotakorpi, Poutvaara, and Terviö (2016).

Table A.1. Effect of getting elected to parliament on earnings: alternative measures.

	(1)	(2)	(3)
	Difference in average annual earnings between $e = 1$ and $e = -1$	Log earnings in $e = 1$	Log difference in earnings between $e = 1$ and $e = -1$
Elected	21,260** (3,734)	0.4189** (0.0429)	0.3294** (0.0507)
Elected (1/2 bandwidth)	24,247** (4,925)	0.353** (0.0648)	0.395** (0.0695)
$N$	8,044	9,525	7,931
Bandwidth	55.88	44.94	46.60

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with IK bandwidth.

Table A.2. Balance of predetermined variables (Parliamentary elections).

	Incumbent (1)	Never elected (2)	Female (3)	Centre (4)	NCP (5)	SDP (6)	Other (7)	South (8)
Elected <sup>1</sup>	0.105*	-0.0935*	0.0176	-0.0273	0.00434	0.0458	-0.0228	0.0241
	(0.0428)	(0.0442)	(0.0427)	(0.0406)	(0.0380)	(0.0395)	(0.0391)	(0.0429)
Elected (1/2 bandwidth)	0.0235	-0.00215	-0.00747	0.0285	-0.0318	-0.000222	0.00348	-0.0128
	(0.0613)	(0.0636)	(0.0591)	(0.0566)	(0.0553)	(0.0571)	(0.0548)	(0.0604)
<i>N</i>	9,621	9,621	9,621	9,621	9,621	9,621	9,621	9,621

	Average annual earnings in $e = -1$ (9)	Average annual earnings before election (10)	Vote share (11)	Age (12)	Year of birth (13)	Year (14)	Number of seats (15)	Number of votes (16)
Elected <sup>1</sup>	1,709.4	1,229.8	0.00251	-0.391	0.240	-0.151	-0.853	-13,270.1
	(4,853.1)	(2,972.9)	(0.00210)	(0.880)	(0.948)	(0.516)	(0.718)	(9,760.3)
Elected (1/2 bandwidth)	-607.8	-1,079.4	0.00125	-0.857	1.119	0.262	0.0110	-178.2
	(4,899.4)	(3,660.9)	(0.00255)	(1.214)	(1.286)	(0.713)	(1.018)	(13,897.8)
<i>N</i>	8,044	8,044	9,621	9,621	9,621	9,621	9,621	9,621

Standard errors in parentheses \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 40.58, which is the optimal IK bandwidth in the main specification; see Table II.

Table A.3. Robustness to control variables (Parliamentary elections).

Dependent variable: Average annual earnings in $e = 1$ .			
	(1)	(2)	(3)
Elected <sup>1</sup>	19,894 ** (2,517)	19,861 ** (2,520)	19,638** (2,398)
Elected (1/2 bandwidth)	17,346** (3,499)	17,248** (3,443)	17,771** (3,278)
$N$	9,621	9,621	9,621
Year	x	x	x
District		x	x
Individual controls <sup>1</sup>			x

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 40.58, which is the optimal IK bandwidth in the main specification; see Table II.

<sup>1</sup> Age, age squared, gender, incumbency.

Table A.4. Effect of getting elected to a municipal council on earnings: alternative measures.

	(1)	(2)	(3)
	Difference in average annual earnings between $e = 1$ and $e = -1$	Log earnings in $e = 1$	Log difference in earnings between $e = 1$ and $e = -1$
Elected	655.6** (221.5)	0.0470** (0.0175)	0.0190 (0.0098)
Elected (1/2 bandwidth)	983.9** (295.8)	0.0461 (0.0240)	0.0226 (0.0139)
$N$	161,100	160,278	159,669
Bandwidth	33.74	22.01	48.40

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with IK bandwidth.

Table A.5. Balance of predetermined variables in (Municipal elections).

	Incumbent (1)	Female (2)	Centre (3)	NCP (4)	SDP (5)	Other (6)	Year of birth (11)	Year (12)	Number of seats (13)	Number of votes (14)
Elected <sup>1</sup>	0.00498 (0.0114)	-0.0228 (0.0119)	-0.0103 (0.0116)	-0.0342 (0.00976)	0.00365 (0.0101)	0.000799 (0.0105)	0.279 (0.301)	-0.0598 (0.110)	0.266 (0.332)	127.1 (610.3)
Elected (1/2 bandwidth)	-0.00513 (0.0154)	-0.00914 (0.0161)	-0.00124 (0.0159)	-0.00128 (0.0132)	0.00260 (0.0134)	-0.000882 (0.0141)	0.386 (0.405)	-0.020 (0.149)	0.189 (0.449)	90.73 (834.9)
<i>N</i>	161,114	161,114	161,114	161,114	161,114	161,114	161,114	161,114	161,114	161,114

	Average annual earnings in <i>e</i> = -1 (7)	Average annual earnings before elections (8)	Vote share (9)	Age (10)	Year of birth (11)	Year (12)	Number of seats (13)	Number of votes (14)
Elected <sup>1</sup>	331.8 (417.2)	245.1 (382.3)	-0.0000971 (0.000159)	-0.338 (0.291)	0.279 (0.301)	-0.0598 (0.110)	0.266 (0.332)	127.1 (610.3)
Elected (1/2 bandwidth)	15.45 (542.4)	-146.7 (500.2)	-0.0000633 (0.000221)	-0.406 (0.392)	0.386 (0.405)	-0.020 (0.149)	0.189 (0.449)	90.73 (834.9)
<i>N</i>	161,100	161,100	161,114	161,114	161,114	161,114	161,114	161,114

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 19.01, which is the optimal IK bandwidth in the main specification; see Table V.

Table A.6. Robustness to control variables (Municipal elections).

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Dependent variable: Average annual earnings in  $e = 1$ .

	(1)	(2)	(3)
Elected	1,292 ** (459.1)	1,187 ** (441.3)	1,056* (426.8)
Elected (1/2 bandwidth)	1,206* (606.9)	1,132 (581.4)	1,051.5 (561.5)
$N$	161,114	161,114	161,114
Year	x	x	x
Council size		x	x
Individual controls <sup>1</sup>			x

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Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 19.01, which is the optimal IK bandwidth in the main specification; see Table V.

<sup>1</sup> Age, age squared, gender, incumbency.

Table A.7. Effect of getting elected to a municipal council on average earnings in  $e = 1$ : estimates by subgroup.

	Incumbent	Non-incumbent	Female	Male	Large municipality	Small municipality	Young	Old
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Elected <sup>1</sup>	985	1,366*	445	1,630**	1,728	1,094*	1,034	1,539*
	(850)	(557)	(679)	(618)	(925)	(499)	(575)	(729)
Elected (1/2 bandwidth)	1,399	1,097	-192	2,005*	1,216	1,188	521	1,966*
	(1,095)	(741)	(879)	(824)	(1,264)	(654)	(784)	(947)
$N$	34,484	126,630	62,253	98,961	67,023	94,091	81,463	79,651
	Low income	High income	SDP	Centre	NCP	Other parties	1996-2000 elections	2004-2008 elections
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Elected <sup>1</sup>	847*	978	1,216	887	1,671	1,575*	1,293*	1,293
	(379)	(655)	(748)	(667)	(1,483)	(793)	(548)	(750)
Elected (1/2 bandwidth)	773	680	1,280	1,260	908	1,312	938	1,512
	(506)	(860)	(1,030)	(898)	(1,893)	(1,054)	(752)	(969)
$N$	80,550	80,550	35,257	44,589	30,398	50,870	82,777	78,337

Standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$

Local linear regression using triangle kernel with bandwidth 19.01, which is the optimal IK bandwidth in the main specification; see Table V.

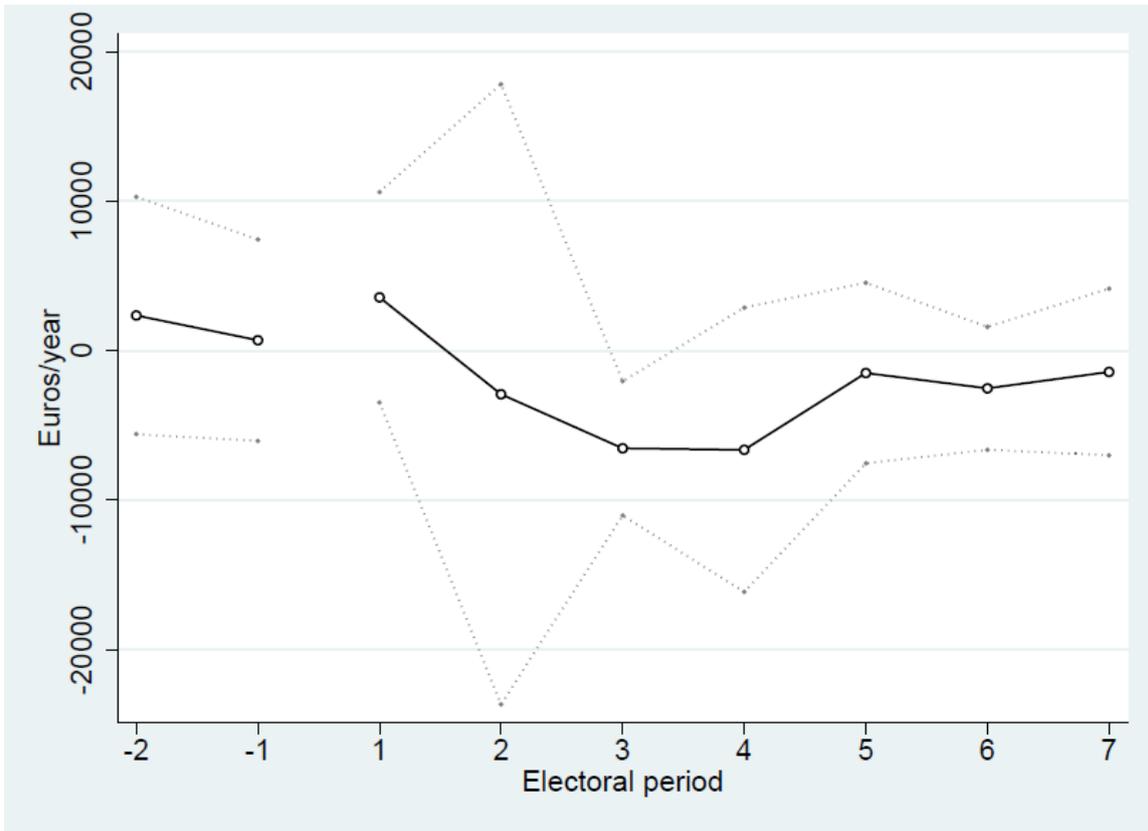


Figure A1. Estimated effect of being elected to parliament on capital income by electoral period. Negative periods refer to electoral periods before the election.

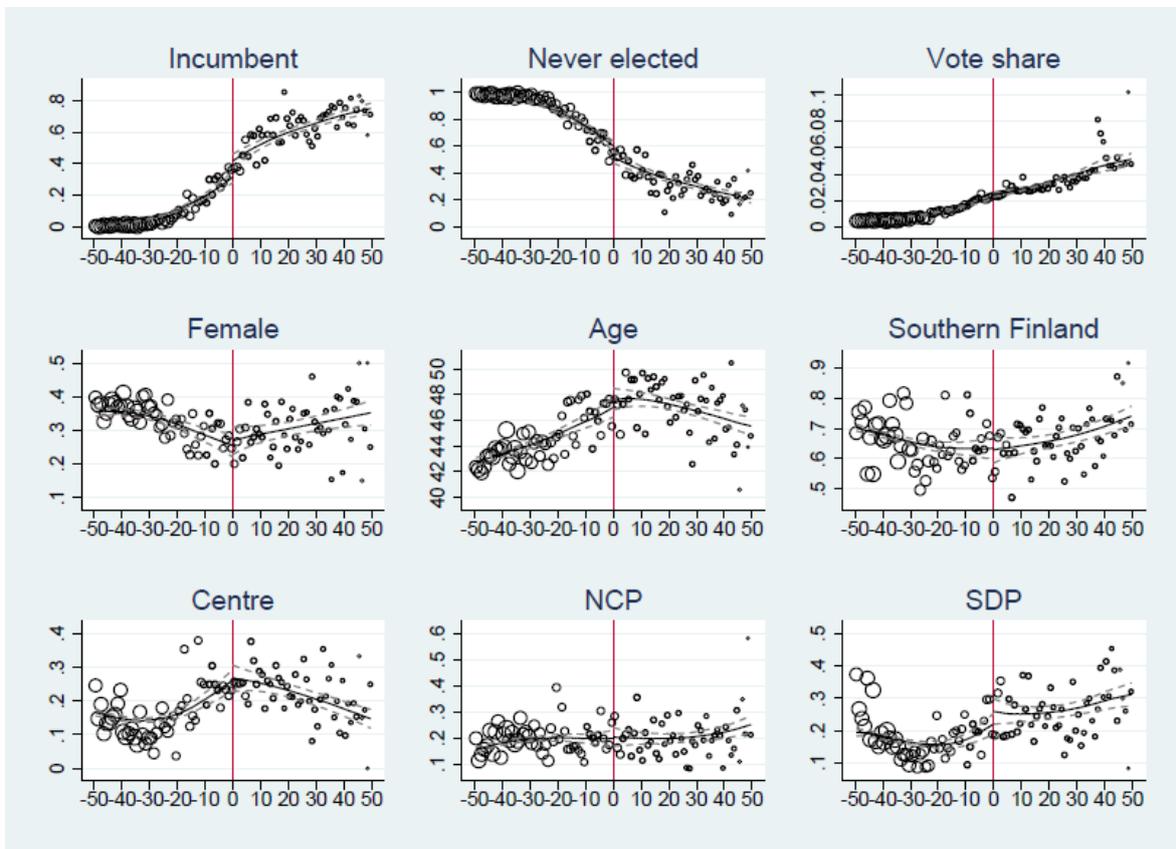
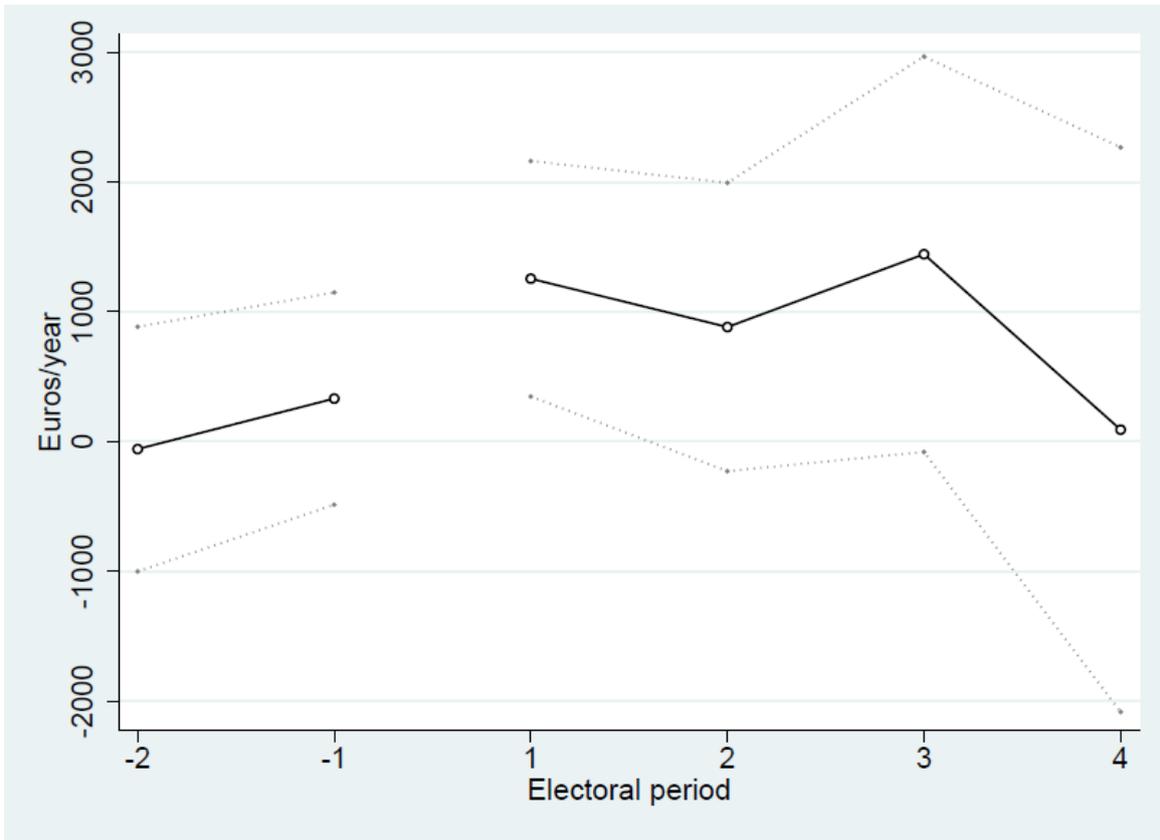
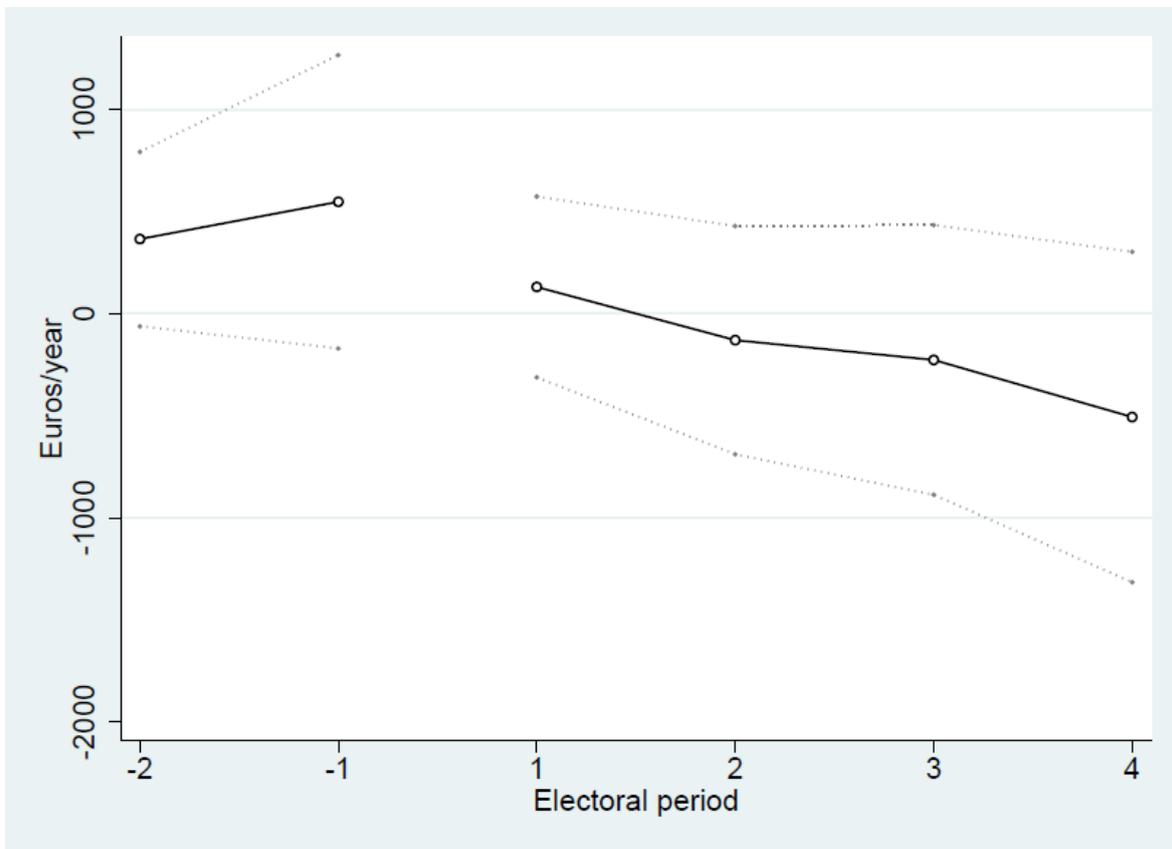


Figure A2. Continuity of predetermined variables (parliamentary elections). For pre-election earnings see Figure 5.



**Figure A3.** Estimated effect of being elected to a municipal council on earnings by electoral period. Negative periods refer to electoral periods before the election.



**Figure A4.** Estimated effect of being elected to a municipal council on capital income by electoral period. Negative numbers refer to electoral periods before the election.

## Average earnings before the election

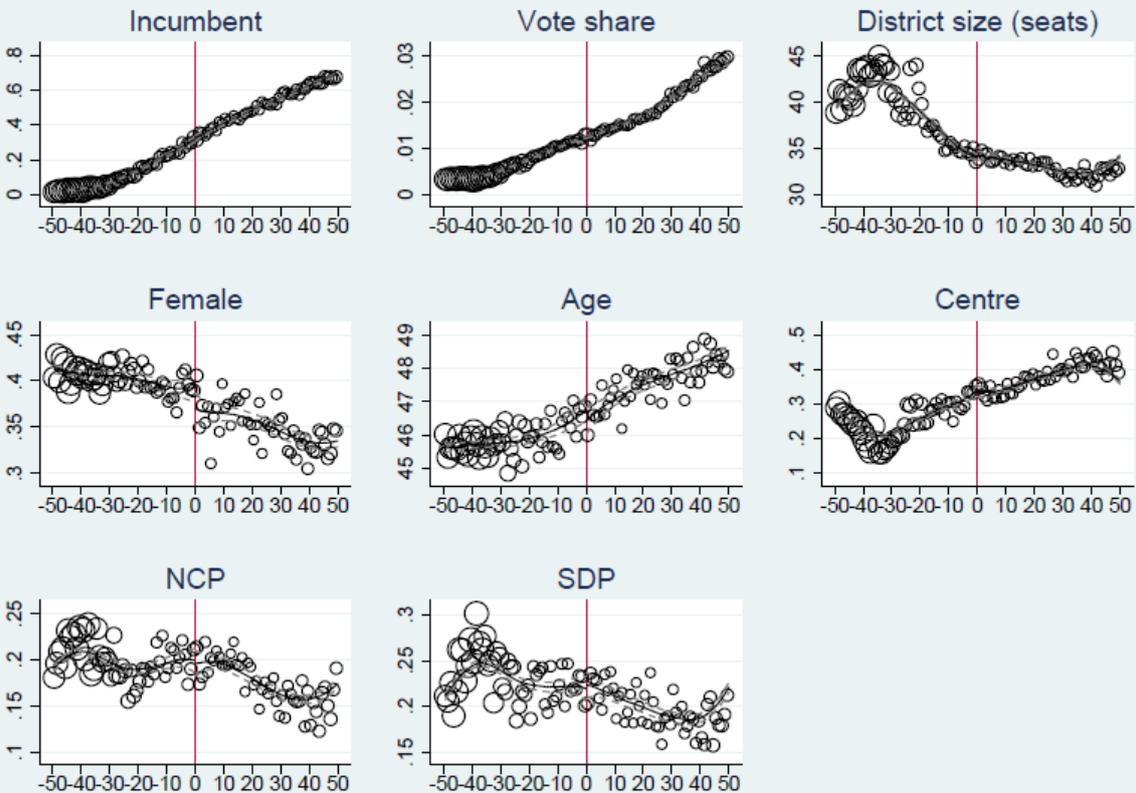
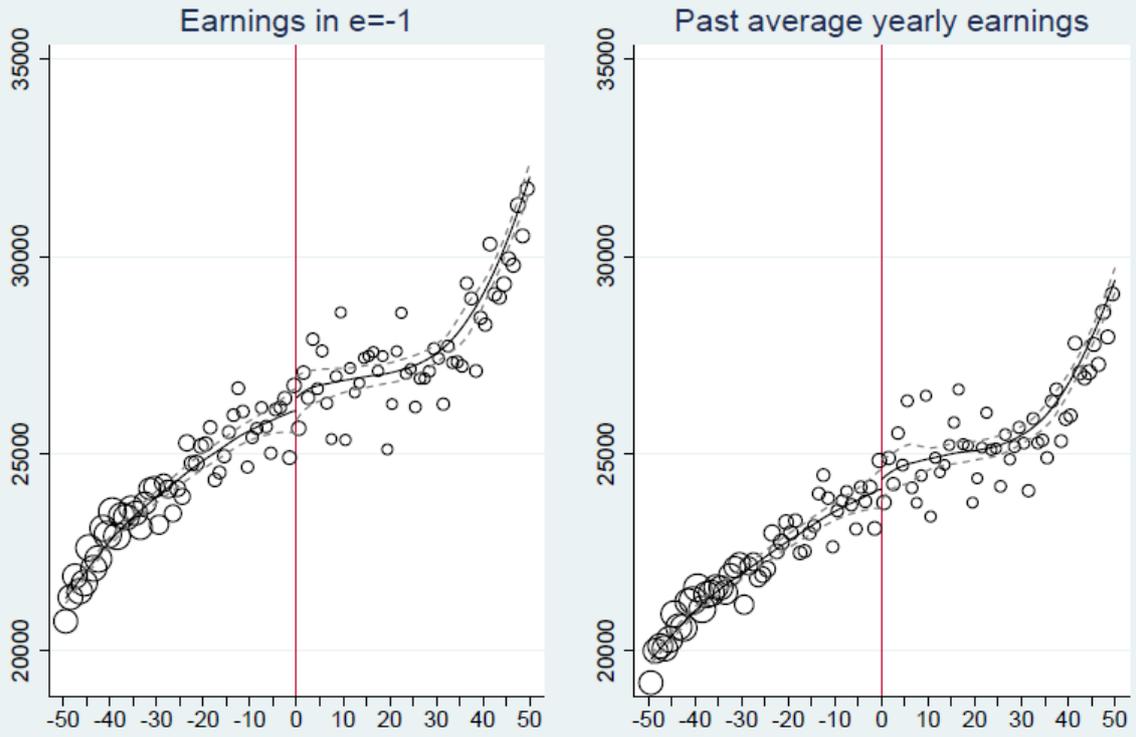


Figure A5. Continuity of predetermined variables (municipal elections).