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IS IT WORTH IT? ON THE RETURNS TO HOLDING POLITICAL OFFICE

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**Fiscal Federalism**

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TO HOLDING POLITICAL OFFICE\***

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**ABSTRACT:** Despite the key role played by political payoffs in theory, very little is known empirically about the types of payoffs that motivate politicians. The purpose of this paper is to bring some light into this. I estimate causal effects of being elected in a local election on monetary returns. The claim for causality, I argue, can be made thanks to a research design where the income of some candidate who just barely won a seat is compared to that of some other candidate who was close to winning a seat for the same party, but ultimately did not. This research design is made possible thanks to a comprehensive, detailed data set covering all Swedish politicians who have run for office in the period 1991{2006. I establish that monetary returns are absent both in the short and long run. In stead, politicians seem to be motivated by non-monetary returns, and I show that being elected locally once (for exogenous reasons) can be an effective starting point for enjoying such payoffs.

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# 1 Introduction

Politics is just like any economic activity; for it to be worthwhile, the benefits must outweigh the costs. This notion is prevalent in close to all political economy models from Downs (1957), where a politician is “some agent” whose main objective is to maximize votes and win elections in order to reap some (unspecified) benefits from being in office, to the more modern citizen-candidate models (Besley and Coate, 1997; Osborne and Slivinski, 1996), where the benefits explicitly include the possibility of implementing some desired policy. Despite its key theoretical role, empirical evidence of what types of payoffs that motivate politicians is more or less a black box. The purpose of this paper is to bring some light into this.

To this aim, I first look at monetary returns from politics by estimating causal effects of being elected in a local election on income shortly after being elected as well as up to 15 years later. This is made possible thanks to a newly collected extensive data set covering all Swedish politicians who have run for office at any level (local, regional or national) in the period 1991–2006.<sup>1</sup>

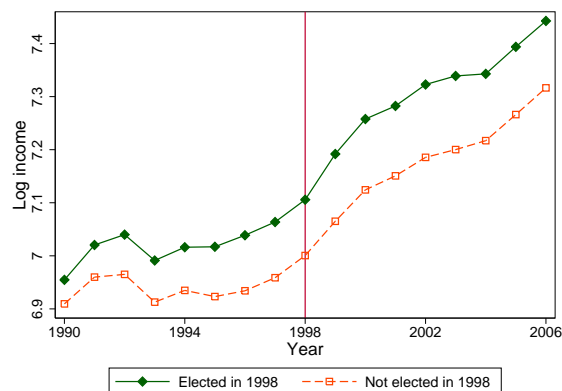
To get a first idea of what these monetary returns could be, Figure 1 displays the income profiles for all candidates who ran for a municipal council in the 1998 election, separately by whether or not they were elected. Although those elected clearly have higher income than those who were not, the gap is almost as large before the election as after. These differences can potentially be the result of selection—i.e., that elected candidates would have earned more than non-elected candidates even in the absence of being elected—as well as of different political histories—i.e., that elected candidates in 1998 are more likely to have been elected also in previous elections. While it is possible to partly control for these and other confounding factors, the figure illustrates quite well the difficulty in identifying the causal effect of being elected.

Instead, the claim for causality in this paper, I argue, can be made thanks to a simple yet compelling research design which, to my knowledge, has never before been applied. It fits into the class of identification strategies that rely on stochastic features of close elections (e.g., Lee et al., 2004 and Folke, 2011), but differs in that identification comes from within-party discontinuities rather than between. The idea is to compare the income of some candidate who just barely won a seat to that of some other candidate who was close to winning a seat *for the same party*, but ultimately did not. Because elections result in a fixed final ranking of each party’s candi-

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<sup>1</sup>The majority of local politicians in Sweden hold regular jobs and, at least partly, devote their spare time to politics. This means that monetary returns from politics can stem both directly from official perquisites and remuneration as well as from a better paid private job, even in the short run.

Figure 1: Disposable income among candidates running for a municipal council in 1998



*Note:* The figure plots average disposable income among candidates who were elected into a municipal council in 1998 and among candidates running for a municipal council in 1998 without getting elected. Income is measured in logs of 100 SEK deflated to 2000 year values.

*Source:* Statistics Sweden & The Swedish Election Authority.

dates,<sup>2</sup> the discontinuity between these candidates—whom I refer to as the *borderline elected* and *borderline defeated*—is well-defined. Moreover, other candidates than these two can be used to detect and control for any possible direct effects of being more highly ranked on income.<sup>3</sup>

Applying this identification strategy, I show graphically and econometrically that monetary returns from politics are absent irrespective if one considers the period right after the election, up to 15 years later or the period right after exiting politics. This result holds for different income measures such as disposable income, total labor income or labor income from the largest source. It is also true on average as well as when considering heterogeneous effects across various dimensions of parties, councils and candidates.

Thus, given that there are no positive monetary returns, politicians are likely *not* motivated by such returns. Rather, it seems that there must be some non-monetary returns that politicians pursue. These can, for example, be political accomplishments, a sense of actively taking part in the

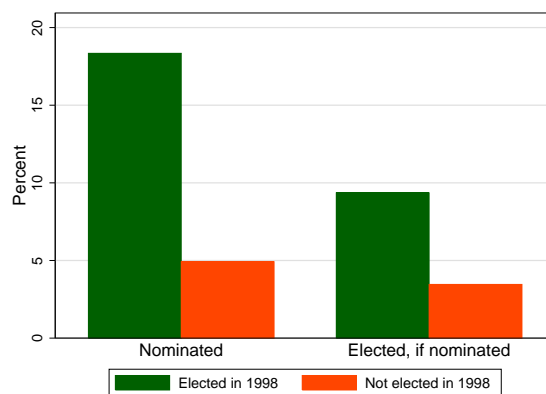
<sup>2</sup>Which to a large extent corresponds to the party's own ballot paper rankings of candidates; see Section 3.1.

<sup>3</sup>As already noted, the identification strategy is clearly related to the regression discontinuity designs that rely on discontinuities in vote shares and focus on elections where some party won with a small margin. Instead, I rely on the discontinuity in candidate ranks induced by the fact that each party will assign only as many seats as were won in the election. To check the robustness of the results I can, however, also use the more traditional vote share discontinuities generated by the seat assignments *between* parties by only focusing on the borderline elected and defeated in those parties that were close to winning/losing an extra seat.

community, the desire to affect society in a certain direction, prestige and power—things that are hard if not impossible to measure. However, such non-monetary returns are to a large extent encompassed by candidates' political careers. Therefore, I proceed to investigate if being elected locally improves future political career prospects.

As a motivation for this, consider the stylized picture in Figure 2 showing the percentages among all elected and all non-elected candidates from the 1998 municipal council elections that went on to national politics in the 2002 and/or the 2006 election. The figure shows that locally elected politicians are 3.5 times more likely to be nominated for parliament (left bars) and, conditional on being nominated, 2.5 times more likely to actually be elected (right bars). Now, a causal interpretation of this picture is, of course, as problematic as the income comparison between elected and non-elected candidates in Figure 1. For evidence of how being elected locally for *exogenous* reasons affects political careers, I therefore apply the same identification strategy as for income and compare the borderline elected and the borderline defeated with respect to their future probabilities of being nominated for parliament as well as their future probabilities of being elected in local elections.<sup>4</sup>

Figure 2: Percentages among municipal council candidates in 1998 nominated for and elected into the national parliament in 2002 and/or 2006



*Note:* The figure shows the percentage that was nominated for and elected into the national parliament in the 2002 and/or 2006 election among candidates who were elected into a municipal council in 1998 and among candidates who ran for a municipal council in 1998 but did not get elected.

*Source:* Statistics Sweden & The Swedish Election Authority.

The main conclusion from this analysis is that being borderline elected into a municipal council improves political career prospects, especially through

<sup>4</sup>Note that because the national parliament only has 349 seats, getting elected is a very rare event. For this reason, the analysis on advancing nationally will be restricted to nominations.

increased chances of advancing to national politics, but also of being elected in future local elections—at least in the short run. Hence, if the goal of politicians is to enjoy non-monetary payoffs such as political accomplishments, prestige and power from a successful political career, being elected once locally is an effective starting point.

The method in the paper is applicable thanks to high-quality data. Lack of proper data is probably the main reason why there is very scant causal evidence of what the returns to politics are. However, one recent study by Eggers and Hainmueller (2009) has overcome the data limitations by collecting estates of deceased members of the British House of Commons. This data together with their empirical approach make this the perhaps most credible study so far. They estimate the effect on wealth (at the time of death) of being elected into parliament using a regression discontinuity design (RDD) where they compare candidates who won/lost with narrow vote margins—a research design similar to that in this paper. The resulting estimates point to substantial wealth effects for Conservative members of parliament but no effects for Labour members.<sup>5</sup>

With an entirely different approach, Diermeier et al. (2005) also aim at quantifying the returns to holding political office. They formulate a comprehensive dynamic structural model of career decisions of politicians, and test the model with data on US congressmen that includes pre-election characteristics as well as post-congressional employment information. However, a problem is that their data is restricted to actual congressmen, implying that the results can only be interpreted *conditional on being elected*. In a sample selection-correction model à la Heckman (1979) with local, regional and national trends in the Democratic/Republican support as an exclusion restriction, they estimate that being *re-elected* once has a positive effect on post-congressional earnings, but that the positive effect vanishes rather quickly with additional experience. Another interesting finding is that non-pecuniary returns from policy accomplishments and realized political ambition are seemingly large.

This paper provides new evidence on what types of returns that motivate politicians in two main ways. First, it is the only study to focus on the local rather than the national political arena. I argue that local politics is the relevant context for studying politicians' motivations, since this is where most political careers start off. For example, among the 349 members of the Swedish parliament in 2006, 75% had previously held a municipal council seat during at least one election period. Furthermore, local politics deals with issues affecting the everyday life of citizens, making its actors an important group to study.

Second, unlike Great Britain and the US, Swedish politics is characterized by a typical multi-party, proportional representation system with less

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<sup>5</sup>Aside from RDD they also use a matching framework, and the results are the same.

focus on the individual candidate and more on the party as such. With such differences in political institutions it is essential to explore whether also the returns to politics differ—and if so, to perhaps start thinking about the consequences for who decides to become a politician.

Another merit of the paper is its high-quality data. It covers all candidates who have run for office at any level (local, regional or national) in any of the five elections held during the period 1991–2006. Two crucially important features are, first, that it contains the same information on all candidates irrespective of whether or not they were elected. Second, for most of the elections, it contains sufficiently detailed information to reproduce the final ranking of candidates resulting from the election, which makes it possible to determine who is the borderline elected. These two features, alone, make the data unique in its kind. Furthermore, rich register-based information on characteristics such as age, sex, foreign background, educational attainment, labor market status, occupation and various income measures is matched to all these candidates using a unique person identifier. The registers are in annual form and cover the years 1990–2006 for all candidates, which makes it possible to (i) follow candidates over a long time period; (ii) verify the identifying assumption with many pre-determined candidate characteristics; and (iii) study heterogeneous treatment effects across candidate characteristics.

Evidence of the types of payoffs that motivate politicians is an important piece to understanding the wider scheme of how politics work. The natural follow-up questions are then if payoffs matter for the selection of politicians and, ultimately, if the selection of politicians matters for policy. Above, I discussed studies that, like the present study, focus on the question of what the payoffs are. In the next section, I review the existing research on these other two related aspects. After that brief literature review, the paper is structured as follows: Section 3 describes the key features of local politics in Sweden and the procedure for ranking candidates within parties. Section 4 states the general assumptions for identifying the effect of being elected, as well as some additional parametric assumptions needed for estimation and inference. The data is described in Section 5 along with a motivation of the choice of outcome variables. Section 6 discusses what the treatment—being elected into a municipal council vs. being close to being elected—is likely to capture. In terms of main results, monetary returns constitute the focus in Section 7 and political careers in Section 8. Preceding the final and concluding section, Section 9 provides a discussion of the heterogeneity and external validity of the results.



## 2 Related literature

In his discussion of recent developments in political economics, Merlo (2006) recognizes the following two questions as important (p. 26): (i) Who chooses to become a politician? (ii) What are the payoffs from becoming a politician?

Like Eggers and Hainmueller (2009) and Diermeier et al. (2005) that were discussed in the introduction, this paper focuses on the second question. To put things in perspective, below I briefly go over the evidence on the first question regarding the selection of politicians.<sup>6</sup>

Theoretical models of the selection effect of rewards reach different conclusions (Besley, 2004; Caselli and Morelli, 2004; Mattozzi and Merlo, 2008; Messner and Polborn, 2004).<sup>7</sup> On the empirical part, two studies with similar focus yield the same results: Ferraz and Finan (2009) and Gagliarducci and Nannicini (2011) both estimate positive effects of increased wages on performance and selection—the former for local politicians in Brazil and the latter for Italian mayors. For Finland, Kotakorpi and Poutvaara (2010) find that a policy-induced salary increase among members of parliament raised the average level of education among female candidates but not among males. Finally, Keane and Merlo (2010) use the framework and data from Diermeier et al. (2005) to simulate a variety of policy changes and study whether the effects are disproportionate across different types of politicians. Their model has two dimensions of ability: (i) “skill”, defined as the ability to win elections; and (ii) “desire for legislative accomplishment”. According to their simulations, congressional wage decreases induce politicians with high ability of type one to exit congress relatively more quickly, but do not affect politicians with high ability of type two.

## 3 Swedish local politics

This section provides an overview of key features of Swedish local politics and municipal elections. There are 290 municipalities in total, which are responsible for a range of public sector goods and services, including primary and secondary education, child care and care for the elderly. Each municipality is governed by a municipal council elected every fourth year (every third year before 1994) in proportional elections held on the same day as elections to the national parliament and the county councils. Voter turnout is high from an international perspective; usually around 80%.

Around two thirds are single-constituency municipalities, but municipalities with a larger electorate have multiple constituencies. In the case

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<sup>6</sup>The natural follow-up questions are then whether politicians’ types and characteristics matter for their voting decisions (Lott and Kenny, 1999; Washington, 2008), resulting policies (Chattopadhyay and Duflo, 2004; Pande, 2003; Svaleryd, 2009) and, ultimately, for economic outcomes such as growth (Besley et al., 2011; Jones and Olken, 2005).

<sup>7</sup>See also Besley (2005) on how political selection is affected by institutions in general.

of two constituencies or more, candidates are elected separately from each constituency. The municipal council decides on the total number of council seats, subject to minimum restrictions set by the Municipal Law ranging between 31 for municipalities with up to 12,000 eligible voters to 101 for the municipality of Stockholm. The median council size is 41. Seats are distributed between parties based on vote shares via the so-called “modified odd-number method”, and there is no formal vote threshold for a seat.<sup>8</sup> All seven major parties in the national parliament (eight after the 2010 election) operate and have separate organizations at the national, regional and local level.<sup>9</sup> In some municipalities, there are additional local parties.

The municipal council is the highest decision-making body in the municipality and its tasks are regulated in the Municipal Law; it *must* appoint members and replacements for committees, the most important of which is the executive board<sup>10</sup> (i.e., the “government” of the municipality); it *must* decide on issues that are of first-order relevance to the municipality such as the budget, the rate of the proportional income tax, organizational forms for the executive branch, remunerations to elected representatives and local referenda; it *can* delegate decisions on issues that are of second-order relevance to the executive board and to working committees.

Hence, the power of the council as stated in the Municipal Law is quite high. However, a parliamentary report with the purpose of considering measures for improving local democracy suggested, among other things, that the council’s power over the agenda and its overall participation in preparations and decisions of political decisions be increased (Swedish Ministry of Integration and Equality, 2001). This suggestion was motivated by an increasing trend in delegations of decisions to the executive board and to the chairmanships of major working committees, and a more pronounced view of the council as merely being a formal decision-making institution on issues that have in practice been settled much earlier in the political process.

Part of the explanation for the more widespread delegations is the fact that the majority of local politicians have other occupations and devote their spare time to politics—less than 3% of all elected representatives (Öhrvall, 2004; Öhrvall and Persson, 2008) and around 8% of the politicians elected into the council (own data) receive full-time or part-time compensation.<sup>11</sup>

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<sup>8</sup>These and other regulations surrounding elections are mainly stipulated in the Municipal Law and the Elections Act.

<sup>9</sup>Since the founding of the Green Party in 1981, national politics has been dominated by seven parties; besides the Green, there is the Left Party, the Social Democrats, the Center Party, the Liberal Party, the Moderate Party and the Christian Democratic Party. In the 1991 election, the populist party the New Democrats made a short appearance, and in the 2010 election the right-wing extremist party the Sweden Democrats—which had so far only been locally successful—entered the national parliament.

<sup>10</sup>The executive board is appointed such that the resulting distribution of seats between parties mirrors the seat distribution in the council.

<sup>11</sup>At least 40 but less than 100% of full-time pay are classified as part-time, although this

According to a survey of local politicians conducted in 1999, the hours per week devoted to politics are 17.8 among chairs, 8.3 among regular council members and 5.3 among council replacements (Hagevi, 2000). But even though this system implies that time constraints can be significant obstacles, it is generally viewed as desirable because it also has the benefit of sustaining close connections between politicians and voters.

Section 6 returns to the question of what being elected into a municipal council really entails. Now, however, follows a description of the process of actually getting there, which forms the basis for the identification strategy of the paper.

### 3.1 Assignment of seats within parties

Candidates can only be elected to the municipal council via parties. Parties running for election nominate and subsequently rank candidates on ballot papers, somewhat generalized, according to the following procedure (Bäck and Möller, 2003):

1. All party members can nominate candidates. At this stage, special-interest politics plays a role in that youth organizations, women's organizations, unions etc. nominate their preferred candidates. Anyone who has the right to vote in the municipal election can be nominated for their municipality's council.<sup>12</sup>
2. An appointed election committee ranks the nominated candidates who have agreed to run. Naturally, overall popularity plays a role in the ranking but also representativity in terms of gender, age, experience and political standpoints. Some parties hold internal trial elections to assist in the ranking.
3. The ballot paper rankings are fixed. This normally occurs around six months before the election.

A party can run with several ballot papers in a single constituency and/or with one ballot paper in several constituencies, meaning that there can be several *ballot paper rankings* in a single constituency and/or one *ballot paper ranking* for several constituencies. Because the seats are assigned separately for each constituency, there is, however, always one single *final ranking* per constituency. Given the total number of seats that each party has won in the constituency, it is according to this final ranking that seats are distributed within parties.

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is a rough classification since it is not always clear what constitutes a full-time assignment.

<sup>12</sup>There are some minor exceptions to this rule, such as municipal employees in charge of personnel (Municipal Law 4 Ch. 6§).

Starting with the 1998 election, voters can mark *one* preferred candidate on the ballot paper (so-called preference voting). When determining the final ranking, the top is set based on the ranking of such preference votes. The threshold for being elected via preference votes is 5% of the party's votes in the constituency, though this must be at least 50 votes. For candidates who do not reach this threshold, so-called comparison numbers are calculated, which are then ranked.

How the ballot paper ranking translates into the final ranking can be a complicated matter, for example when there are multiple ballot papers per constituency or when candidates run in several constituencies. These complications only arise in a minority of cases, and the details of the procedure are described in the Appendix. For the majority of cases, however, the final ranking mirrors the ballot paper ranking, except that candidates who have reached the preference vote threshold are put at the top.<sup>13</sup> The following section describes how the final candidate ranking is used for identification of the effect of being elected into a municipal council.

## 4 Identification strategy

The potential outcome framework introduced by Rubin (1974, 1990) is useful for conceptually thinking about identification of the effect of being an elected politician on some outcome  $Y$ . Let  $Y_i(1)$  be the potential outcome of individual  $i$  if being treated (i.e., being elected to the municipal council), and  $Y_i(0)$  the potential outcome of the same individual if not treated. The difference between the two potential outcomes,  $Y_i(1) - Y_i(0)$ , is then the treatment effect. While this definition of a treatment effect is intuitive, it is fundamentally impossible to measure. The reason—i.e., the identification problem—is that  $Y_i(1)$  and  $Y_i(0)$  are both *potential* outcomes of which only one can be observed.

Consider the outcome disposable income. Assume that we observe  $Y_i(1)$ —that is, we observe the disposable income of an elected politician.<sup>14</sup> The challenge in determining the treatment effect is then to find the best counterfactual outcome, meaning that one should look for the income that this individual would have earned, had he not been elected. A number of possible counterfactuals can be considered. First, it is possible to exploit time variation and compare the income of the same individual before and after

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<sup>13</sup>In the three elections since the introduction of the preference vote covered by the data, around 15–20% of the candidates reached this threshold. However, considerably fewer were elected *because of* their preference votes, as the majority of those who reached the threshold were also sufficiently highly ranked on their party's ballot paper. Thus, the difference between the ballot paper ranking and the final ranking induced by moving candidates elected via preference votes to the top is, in practice, very small.

<sup>14</sup>I abstract from time indices here but, as will soon be clear, outcomes will be measured in three different periods from the time of election.

he was elected. However, this will fail to identify the treatment effect if other things affecting his income changed during this period besides becoming elected (either directly for the politician or indirectly due to some aggregate shock), an event that seems highly plausible. Second, one could exploit cross-sectional variation and compare the income of the politician with that of other individuals at the same point in time. Unfortunately, this will most likely bias the estimated treatment effect even more, because the politician and “other individuals” differ along numerous other dimensions of which some are likely to be correlated with income.

Ideally, one would like the treatment of being elected into a municipal council to be random, since randomization ensures zero correlation with any outcome. And as elections have stochastic features, for some politicians it is indeed a matter of chance whether or not they are elected. Thus generally, under the assumption that election outcomes cannot be perfectly controlled, close elections induce random variation in who does and who does not get elected.<sup>15</sup>

Specifically, I will use the variation in treatment status between candidates running for the same party, given the number of seats won by that party. The idea is to reproduce the final ranking of candidates, as laid out in Section 3.1 and the Appendix, of a party that won  $n$  seats in some constituency and then compare the outcome (income, say) of the treated  $n^{\text{th}}$  candidate to that of the untreated  $(n + 1)^{\text{th}}$  candidate. Because the  $n^{\text{th}}$  ranked candidate just barely got elected by being assigned his party’s last seat and the  $(n + 1)^{\text{th}}$  ranked candidate was close to being elected but was ultimately not, in what follows I refer to the former as the *borderline elected* and to the latter as the *borderline defeated*.

It is possible that the final ranking is systematically related to the outcome of interest. Or, put differently, it is possible and even likely that there is a systematic difference between the innate “quality” of the borderline elected and the borderline defeated. Other candidates than the borderline elected and defeated can help detect such direct effects. To this aim, visual inspection of the data is particularly illustrative; the treatment effect will be seen graphically as the difference between the borderline elected and defeated that is above and beyond differences between any other two candidates.

Technically, the identification strategy is a regression discontinuity design (RDD) where the forcing variable is the difference between a candidate’s (final) rank and the (final) rank of the borderline elected,  $rank^*$ .<sup>16</sup>

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<sup>15</sup>Following Lee et al. (2004), this idea has been exploited in numerous papers estimating “party effects”.

<sup>16</sup>The first application using RDD was Thistlethwaite and Campbell (1960), which, like in this paper, was based on a discrete forcing variable. Since the formal conditions for identification in the continuous case were derived by Hahn et al. (2001), the applications in economics have been numerous (see Lee and Lemieux (2010)).

The identifying assumption is that parties cannot perfectly anticipate how many seats they will win and thereby rank their candidates accordingly.<sup>17</sup> That is, parties cannot be absolutely certain which  $n$  candidates will be elected so that the quality of the  $(n + 1)^{th}$  candidate is irrelevant. Rather, the direct effect of rank on the outcome must be smooth for ranks around the borderline elected.

Because the forcing variable is discrete, assuming some parametric functional form is necessary in order to estimate the magnitude and standard error of the treatment effect. This is different from an RDD with a continuous forcing variable, which allows for non-parametric identification if there is a sufficiently large number of data points “infinitely close” to the discontinuity point. Lee and Card (2008) discuss identification and inference in RDD in the discrete case. They show that when the assumed parametric form differs from the true parametric form by some error that is identical irrespective of treatment status, the treatment effect is still identified, although the confidence intervals need to be inflated. Inflating the confidence intervals is then done by clustering at the level of the discrete values of the forcing variable. However, this procedure is not feasible in this application, because the forcing variable,  $rank^*$ , can only take a limited number of values.

Instead, underlying the preferred regression specification will be the parametric assumption that the direct effect of  $rank^*$  is linear for a limited sample consisting of the  $n^{th}$ ,  $(n + 1)^{th}$  and  $(n + 2)^{th}$  ranked candidates. I refer to such a set of candidates per party and constituency as the *borderline group*.<sup>18</sup> By limiting the estimation sample to three candidates per borderline group, the error from assuming linearity is likely to be smaller.

The regression to be estimated on the sample of candidates ranked  $n^{th}$ – $(n + 2)^{th}$  is then:<sup>19</sup>

$$Y_{i,g,t+j} = \beta_0 + \beta_1 elected_{i,g,t} + \beta_2 rank_{i,g,t}^* + \sum X_{i,g,t-1} + \varepsilon_{i,g,t+j}, \quad (1)$$

where  $Y_{i,g,t+j}$  is the outcome for candidate  $i$  in borderline group  $g$  running in election year  $t$ ,  $j$  periods ahead. The forcing variable  $rank_{i,g,t}^*$ —the difference between the rank of candidate  $i$  in group  $g$  and the rank of the

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<sup>17</sup>Recall from above that the ballot paper rankings are normally set around six months before the election, implying that this is not a very strong assumption.

<sup>18</sup>The reason for including the  $(n + 2)^{th}$  rather than the  $(n - 1)^{th}$  candidate is to have the sample as representative as possible. In the latter case, the sample needs to be restricted to parties where at least two candidates were elected via comparison numbers. Now, instead, the only restriction is that there is at least one candidate elected via comparison numbers. This is explained in more detail in the Appendix.

<sup>19</sup>For the continuous income outcomes, the estimated model will be a log-linear. For the binary future election outcomes, a linear probability model will be estimated.

borderline elected in group  $g$ —is defined such that it equals 0 for the borderline elected and  $-1$  and  $-2$  for the candidates who would have been elected had the party gained one or two more seats, respectively. The term in parenthesis represents effects of a vector of individual characteristics measured one year prior to the election that will be controlled for in most of the estimations and the graphical counterparts (although they should be redundant for identification purposes). Finally,  $\varepsilon_{i,g,t+j}$  is an error term that is allowed to be arbitrarily correlated within municipality.<sup>20</sup>

Both the graphical analysis and the estimations of equation (1) will consider short-, medium- and long-run outcomes, which for income outcomes translate into the time index  $t+j$  being the average over 1–3, 6–8 and 13–15 years after election  $t$ , respectively. For short-, medium- and long-run election outcomes,  $t+j$  will be the first, second and fourth subsequent election, respectively.<sup>21</sup>

The treatment parameter of interest is  $\beta_1$  and the condition for the causal effect to be identified in equation (1) is that the direct effect of rank relative to the borderline elected is captured by  $\beta_2$ , meaning, once more, that it must be (at most) of order one for candidates ranked  $n^{th}-(n+2)^{th}$ .

More than three candidates per borderline group (i.e., per party and constituency)<sup>22</sup> are required for the treatment effect to be identified if the direct effect of  $rank^*$  is of higher order than one.<sup>23</sup> As a complement to the main specification in (1), a set of results from running the following regression on the borderline elected and several defeated candidates will therefore also be presented:

$$Y_{i,g,t+j} = \beta_0 + \beta_1 \mathit{elected}_{i,g,t} + \sum_{p=1}^{\bar{p}} \beta_{2p} (\mathit{rank}_{i,g,t}^*)^p + \varepsilon_{i,g,t+j}, \quad (2)$$

where the term summing over order of polynomial  $p$  represents the direct effect of  $rank^*$  and  $\bar{p}$  is the highest order of polynomial included in the regression. Several versions of equation (2) will be estimated by varying  $\bar{p}$  between 1 and 3 and the number of defeated candidates included (i.e. the bandwidth) between 5 and 10.

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<sup>20</sup>This variance-covariance matrix may seem too restrictive. However, it turns out that clustering the standard errors at smaller units than municipality—as is done now—does, in fact, not improve the precision of the estimates (the results are available upon request).

<sup>21</sup>Four elections ahead is as far as the data allows the analysis to go. The reason for not studying the third subsequent outcome is simply to keep the number of outcomes down.

<sup>22</sup>The majority of borderline groups are at the constituency level. However, when a ballot paper overlaps several constituencies, the group is at the municipality level; see the Appendix.

<sup>23</sup>Analogously, a simple mean comparison of the borderline elected and defeated identifies the treatment effect if there is no direct effect of  $rank^*$ .

With the empirical setup represented by equations (1) and (2), controlling for group-specific characteristics or a group fixed-effect (or some other more aggregate fixed-effect) is, for identification purposes, more or less redundant. To see this, note that the estimation samples consist of a nearly-balanced panel with borderline groups of candidates with the same  $rank^*$  values. The only exceptions are those groups where there are too few defeated candidates so that it is not possible to assign low values of  $rank^*$  to anyone (cf. Figure 10 in the Appendix). Therefore, unless these exceptions are systematic, any group characteristics must be uncorrelated with  $rank_{i,g,t}^*$  and hence, also with the treatment variable  $electedi_{i,g,t}$  since this is simply an indicator variable  $1(rank_{i,g,t}^* = 0)$ .<sup>24</sup>

The identifying assumption that parties cannot perfectly anticipate which candidates that will be elected may be more likely to hold for some groups than for others. Specifically, parties that have repeatedly won  $n$  seats may anticipate that they will do so also in the next election and, consequently, may not care about the quality of the  $(n + 1)^{th}$  candidate. Figure 3 assesses whether this is likely to be a problem. Separately by party size, it shows the variability of seats for a given party in a given council over elections 1985–2002, measured as the deviation in the number of seats in a particular election from the mean number of seats over the entire period.

Reassuringly, Figure 3 shows substantial variation even for parties that on average have two seats or less (top left plot).<sup>25</sup> To further investigate the validity of the identifying assumption, the empirical analysis will contain robustness checks where I mimic a group-specific unanticipated shock that affects who the borderline elected is. Specifically, the estimation sample will be restricted to only include (i) groups whose total number of seats changed from the previous election; (ii) groups that won their  $n^{th}$  seat or lost their  $(n + 1)^{th}$  seat with narrow vote margins; and (iii) the combination of (i) and (ii). For this exercise, the definition and calculation of minimum changes in votes to win or lose an additional seat in proportional elections as developed by Folke (2011)<sup>26</sup> will be used.

Moreover, to strengthen the notion that  $\beta_1$  really captures the effect of being elected, placebo regressions in which each group is assigned one or two additional seats so that the  $(n + 1)^{th}$  or the  $(n + 2)^{th}$  candidate is the “borderline elected” will be estimated. These estimations will serve as complements to the graphical analysis where such placebo effects can be

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<sup>24</sup>One may still want to include group fixed-effects to increase the precision of the estimates. However, it turns out that doing this neither affects the point estimates nor the standard errors (the results are available upon request).

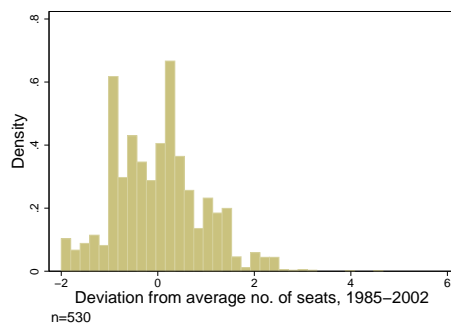
<sup>25</sup>As should be clear from Section 3.1, there is a considerable amount of internal democracy within the parties in setting the ranking, suggesting that the quality of the (borderline) defeated candidates matters even when there is little uncertainty about how many seats the party will win.

<sup>26</sup>I sincerely thank him for generously sharing his STATA code.

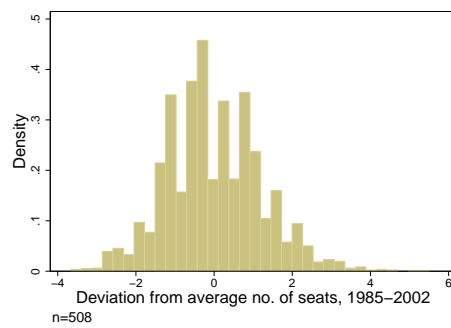


Figure 3: Variability in parties' number of seats

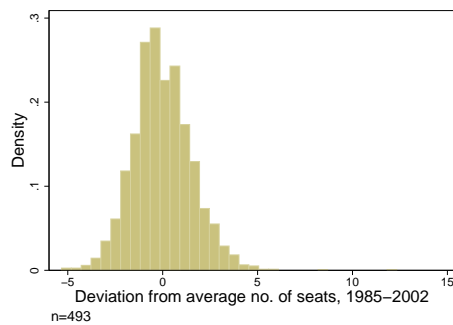
(a)  $0 < \overline{seats} \leq 2$



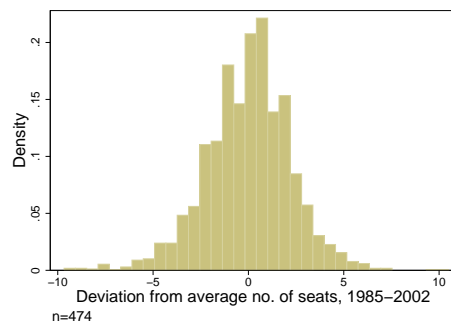
(b)  $2 < \overline{seats} \leq 4$



(c)  $4 < \overline{seats} \leq 9$



(d)  $9 < \overline{seats}$



*Note:* The figures show the distribution of the deviation in the number of seats in a particular election between 1985 and 2002 from the mean number of seats over the entire period, *seats*.  
*Source:* Statistics Sweden.

directly detected.

## 5 Data

Detailed data over political candidates is a necessity for applying the above described research design. The data used in this paper, obtained from Statistics Sweden and The Swedish Election Authority, covers all candidates who have run for office to a Swedish municipal council or to the national parliament in any of the five elections held during the period 1991–2006.<sup>27</sup> The elections to municipal councils in 1991, 1998 and 2002/in 1991 and 1998/in 1991 define the population under study for short-/medium-/long-run outcomes. The number of borderline groups is around 1800–1900 in each of these three elections. Data from the 1994 election is of poorer quality and could not be used to define borderline groups. However, data from all elections between 1994 and 2006 will be used for outcome purposes (see below for details), and the 2006 data additionally contains some useful information that will be used for descriptive purposes. The analysis will not cover local parties but is restricted to the seven parties that have traditionally dominated national politics.<sup>28</sup>

Two crucially important features of the data are, first, that it contains the same information on all candidates irrespective of whether they were elected or not. Second, except for the 1994 election, it contains all ballot paper rankings so that the final ranking that identifies the borderline groups can be calculated.<sup>29</sup> These two features, alone, make the data unique in its kind. Furthermore, rich register-based information on characteristics such as age, sex, foreign background, educational attainment, labor market status, occupation and various income measures is matched to all candidates using a unique person identifier. The registers are in annual form and cover the years 1990–2006 for all candidates, which enables an empirical analysis that (i) follows candidates over a relatively long time period; (ii) can verify the identifying assumptions using pre-determined covariates; and (iii) looks at heterogeneous treatment effects across characteristics such as age and level of education.

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<sup>27</sup>Candidates running for a county council are also covered, but this data will not be used in this paper.

<sup>28</sup>The main reason for excluding local parties is that they are very diverse and would therefore be likely to introduce unnecessary noise.

<sup>29</sup>Because the 1991 and 1998 election data contains somewhat less information than the 2002 election data, some assumptions were needed to find borderline groups in these two elections. See the Appendix for details.

## 5.1 Outcome variables

The effects of being elected into a municipal council will be considered on a short-, medium- and long-run basis which, as described in connection with the identification strategy, for income outcomes translate into the time index  $t + j$  denoting the average over 1–3, 6–8 and 13–15 years after the election in year  $t$ , respectively. For short-, medium- and long-run election outcomes,  $t + j$  denotes the first, second and fourth subsequent election, respectively. Descriptive statistics of all outcomes in the sample of candidates in borderline groups with  $rank^* = \{-2, -1, 0\}$  are provided in Table 11 in the Appendix. Below follows a description and motivation of the choice of variables.

*Disposable income*—This variable is meant to capture all monetary returns from politics. It is individualized but measured at the household level, and is the sum of numerous types of after-tax income of the family, including, e.g., labor income, capital income, pensions and unemployment and sickness benefits. To the extent that there is intra-household bargaining—so that also the income of the politician’s spouse could be affected—this is a proper measure of total monetary returns. Note, though, that with the available data it is also possible to check the sensitivity of the results to alternative income measures.

To reduce the noise that often plagues income data, disposable income is measured in three-year averages. For a candidate in the 1991 election, for example, short-run income is the average income over years 1992–1994, medium-run income is the average over years 1997–1999 and long-run income is the average over years 2004–2006. To avoid results that are driven by outliers, the three-year averages are censored at the 1<sup>st</sup> and 99<sup>th</sup> percentiles. The analysis will be performed on logs of the three-year averages.

Monetary returns from politics will be positive if individuals acquire certain skills that are rewarded in the labor market, if there is a positive signaling effect or if the individuals develop closer ties to certain firms or organizations. Note that such returns could be retained while still in politics, since the majority of local politicians hold regular jobs and, at least partly, devote their spare time politics. While still in politics, there is also the direct effect of official perquisites and remunerations. There is, however, also the possibility of mechanisms operating in the opposite direction: political engagement may require foregone earnings because of time and effort constraints.<sup>30</sup>

Monetary returns in the form of outright bribes will obviously be close to impossible to measure, as these are unlikely to show up in official income registers. But to the extent that politicians attempt to hide parts of their (illegitimate) income by transferring *official* income within the household,

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<sup>30</sup>The Municipal Law (4 Ch. 12§) states that elected representatives have the right to be “reasonably compensated” for foregone earnings due to their political assignments.

such returns will show up in their disposable income.

*Being nominated for/elected into a municipal council*—These are indicator variables measuring the probability of a candidate being nominated to a municipal council in subsequent elections and the probability of being elected into the council in subsequent elections. These outcomes will capture if being randomly elected into a council improves future political career prospects locally.

As for potential effects on the probability of running, one can imagine that being elected establishes closer connections to the local party organization which would increase the likelihood of future nominations, or that being elected has a positive encouragement effect on continuing in politics which would increase the likelihood of accepting a nomination. For some individuals, on the other hand, being elected may imply learning and being disappointed by what local politics really is about which would then discourage future political engagement.

The effects on being elected in future elections, or incumbency effects, may in part operate via similar channels. Parties may reward “good politicians” that, for example, stick to the party line by promoting them and ranking them higher in subsequent elections. If such abilities are better revealed in the council, being elected would thus affect the chances of being reelected. But reelection probabilities may also be affected through more traditional incumbency effects that operate via voters.

*Being nominated for the national parliament*—This is an indicator variable measuring the probability of a candidate being nominated to the national parliament in subsequent elections. Advancing from the local to the national arena is a likely goal among candidates who are motivated by political accomplishments and prestige and who want to pursue a political career.

Because the parliament only has 349 seats, actually getting elected is a very rare event, which is the reason why the analysis on national politics is restricted to nominations. So, what does it mean to be nominated for the national parliament? Naturally, the probability of actually being elected is infinitely greater for those running than for those who do not. But, to some extent, even non-elected parliamentary candidates have advanced from their local political careers, since not all party members that wish to be nominated actually are.

Although there is very little research on the vertical structure of political parties in Sweden (Erlingsson, 2008), one can imagine that the mechanisms operating locally to some degree extend to the national level. According to Bäck and Möller (2003), the local organizations constitute the basis for the political parties as they are platforms for member recruitment and for most meetings, and as they handle nominations of candidates to numerous political assignments. However, although the local party organizations operate separately from their central counterparts, there is arguably still some

degree of vertical interdependence.

## 5.2 Control variables

The register data includes numerous variables measuring the candidate's characteristics. Table 1 shows the mean and standard deviation of a set of these variables (measured one year before the election) for three different samples taken from the 1991, 1998 and 2002 election data that is the focus of the paper; (i) column 1 includes all non-elected candidates; (ii) column 2 includes all elected candidates; and (iii) column 3 includes candidates with  $rank^* = \{-2, -1, 0\}$  in the borderline groups that constitute the sample for the main econometric analysis. Comparing columns 1–2 with column 3 shows how representative the candidates in the borderline groups are (ignore column 4 for now). For example, in terms of age and marital status, the borderline groups are more similar to the non-elected sample, whereas in terms of education they are more like the elected sample. Hence, the representativity is in general quite good.

Since all time-variant covariates are set at one year before the election, all variables in Table 1 are pre-determined and should hence not be affected by the treatment. Therefore, one implication of the identifying assumption (that the direct effect of rank is the same for ranks around the borderline elected) is that the treatment effect conditional on these variables should not differ from the unconditional treatment effect. This will be explored in the result section.<sup>31</sup>

A mirror implication of the identifying assumption can be tested by running the main equation (1) on pre-determined covariates. If the direct effect of rank is linear among the candidates in the borderline groups, non-linearities in pre-determined covariates should not be expected. In other words, the estimate of  $\beta_1$  should not differ from zero. The rightmost column of Table 1 provides the t-statistics of the  $\beta_1$  estimate from running these regressions, which indeed are small enough to confirm that there are no non-linearities in the direct effect of  $rank^*$ .<sup>32</sup>

Aside from the variables in Table 1, individual controls will further include a set of dummies capturing past political experience by indicating whether the candidate ran for/was elected into a municipal council in the past three elections. Because the earliest election covered by the data is 1991, these dummies are censored or partly censored (set to zero) for borderline groups in the 1991 and 1998 elections.

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<sup>31</sup>Disposable income will be controlled for with quantile dummies, age with dummies for 10-year intervals and number of children linearly. All other control variables are binary.

<sup>32</sup>An analogous test is to run a regression of the binary variable *elected* on  $rank^*$  and all covariates in Table 1 and test for joint significance of the covariates. Doing this, the obtained F-statistic is 0.80 (p-value 0.71), thus strengthening the confirmation of no non-linearities.

Table 1: Representativity and balance in pre-determined characteristics of candidates in borderline groups with  $rank^* = \{-2, -1, 0\}$

	Sample			$\beta_1$
	All non-elected	All elected	$rank^* = \{-2, -1, 0\}$	t-stat.
Disposable income	1189.4 (514.9)	1345.9 (574.0)	1204.6 (522.8)	0.88
Age	47.9 (12.9)	49.3 (10.8)	47.7 (12.1)	0.22
Children under 18	0.81 (1.14)	0.75 (1.10)	0.88 (1.18)	-0.46
Female	0.40 (0.49)	0.40 (0.49)	0.41 (0.49)	-1.08
Married	0.66 (0.47)	0.71 (0.45)	0.66 (0.47)	1.50
Less than high school	0.20 (0.40)	0.16 (0.37)	0.15 (0.36)	0.57
High school graduate	0.43 (0.49)	0.40 (0.49)	0.40 (0.49)	-1.13
< 2 years university	0.061 (0.24)	0.072 (0.26)	0.070 (0.26)	1.41
$\geq 2$ years university	0.30 (0.46)	0.36 (0.48)	0.37 (0.48)	0.15
Graduate studies	0.0083 (0.091)	0.0094 (0.097)	0.0093 (0.096)	-0.92
Born in Sweden	0.94 (0.25)	0.95 (0.22)	0.94 (0.25)	-0.47
Born in other Nordic country	0.029 (0.17)	0.026 (0.16)	0.030 (0.17)	-0.79
Born in non-Nordic Europe	0.020 (0.14)	0.017 (0.13)	0.018 (0.13)	1.09
Born in North America	0.0021 (0.045)	0.0011 (0.033)	0.0023 (0.048)	-0.08
Born elsewhere	0.014 (0.12)	0.0091 (0.095)	0.014 (0.12)	0.94
Both parents foreign-born	0.0087 (0.093)	0.0068 (0.082)	0.010 (0.100)	-0.15
Observations	109369	38229	16738	16738

*Note:* Columns 1–3 report the mean and standard deviation (in parentheses) of variables measured one year before the election. Column 4 reports the t-statistic of the estimate of  $\beta_1$  from running equation (1) on each of the variables on the sample of candidates with  $rank^* = \{-2, -1, 0\}$  in the borderline groups. Income is measured in 100 SEK deflated to 2000 year values (6.50 SEK  $\approx$  1 USD). The education variables indicate highest completed level. Born elsewhere equals one for individuals born in Africa, Asia, Oceania, Russia or S. America. Both parents foreign-born equals one for individuals born in Sweden but with both parents foreign-born. All variables but Disposable income, Age and Children under 18 are binary.

*Source:* Statistics Sweden.

## 6 Characterizing the treatment

The treatment group and the control group consist of candidates who got their party's last seat and those who were next in line to get a seat had their party won enough additional votes, respectively. The idea is that a comparison of these two groups will capture exogenous differences along dimensions such as political experience, power, success and representation. While Section 4 laid out the assumptions under which the exogeneity requirement is fulfilled, I now discuss what the treatment—being elected into a municipal council vs. being close to being elected—is likely to capture.

An important aspect is the appointment of council replacements to stand in for regular council members in the case of defection or absence from a meeting. Based on the ranking on the ballot paper from which each of the regular council members were elected, non-elected candidates are appointed replacements. A replacement can stand in for several regular members, and the total number of replacements to be appointed is decided by the council prior to the election (as a share below half of the total seats won).

Thus, it is quite likely that candidates in the control group (in particular the borderline defeated) serve as council replacements. If actual political experience is what matters for income and political career prospects, it is thus sensible to define treatment as actually having served in the council, rather than being elected into the council on election day. If any regular council member resigns early in the election period and a candidate in the control group thereby gets a permanent seat in the council, and/or if the borderline elected is the one who resigns, the variation in treatment status—defined in this way—will, therefore, be fuzzy at the threshold at  $rank^* = 0$ .

Fortunately, at least for the 2002 and 2006 elections, there is information on early resignations and effective replacements that can tell the extent to which the treatment effects obtained from running the regression in (1) underestimate effects of being de facto treated (i.e., actually having served in the council). If borderline elected candidates are defined as having de facto been treated if they did not resign during the first year after the election date, and if defeated candidates are defined as having been de facto treated if they overtook someone's permanent council seat at least 300 days before the next election,<sup>33</sup> then, according to the 2002 and 2006 data, 95% and 40% of all borderline elected and defeated were de facto treated, respectively. The corresponding percentage among candidates ranked  $-2$  is around 20%.

If this information were available for all elections, a fuzzy RDD with the probability of being de facto treated as a discontinuous function of  $rank^*$  as the first stage would be ideal. As revealed by the percentages just stated, running such a first stage on the 2002 and 2006 data on candidates in the borderline groups with  $rank^* = \{-2, -1, 0\}$  yields an estimate of around

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<sup>33</sup>Note that the new council is not formally in place immediately after the next election.

0.30 (with a t-statistic of 18.5). Thus, although the treatment of having actually served in the council is not deterministically determined by  $rank^*$ , there is still substantial discontinuous variation at the threshold at  $rank^* = 0$ .

Another aspect is that committee work outside of the council provides alternative forums for political engagement. Only politicians in the municipal council are directly elected by the voters. However, when the council subsequently appoints members to working committees (and committee replacements), they can do so both from within as well as from outside the council. The term “elected representative” in the Municipal Law refers both to regular council members directly elected by the voters, municipal council replacements as well as to those appointed to committees by the council. With this definition, the number of locally elected representatives exceeds the number of municipal council members by far.

However, we know that exerting the formal power as placed on the municipal council by the Municipal Law is reserved to council members, and this should be considered as an important part of the treatment. This means that, if—as has been expressed—substantial de facto power is concentrated to the executive board and major committees, council members can influence the composition of committees in a way that is favorable to themselves by, e.g., appointing themselves or fellow council members. That 90% of the executive board are also members of the council (Bäck, 1993; Bäck and Öhrvall, 2004) suggests this to be the case. Information on the number and type of positions held by the politicians in the data available here (unfortunately only for the 2006 election) also supports this argument; 8% of the borderline elected in 2006 are members of the executive board, whereas the corresponding percentage is merely around 1.5–2.5 among candidates ranked  $-1$  or  $-2$ . Furthermore, also according to the 2006 data, the borderline defeated are not compensated with positions in other committees, in the sense that the borderline elected hold, on average, one more regular position than the borderline defeated (1.6 compared to 0.7).

Thus, it is clear that being borderline elected into a municipal council vs. being close to being elected induces differences in dimensions such as political representation and power. The remainder of the paper will show if and how these differences affect income and political career prospects.

## 7 Monetary returns from being elected

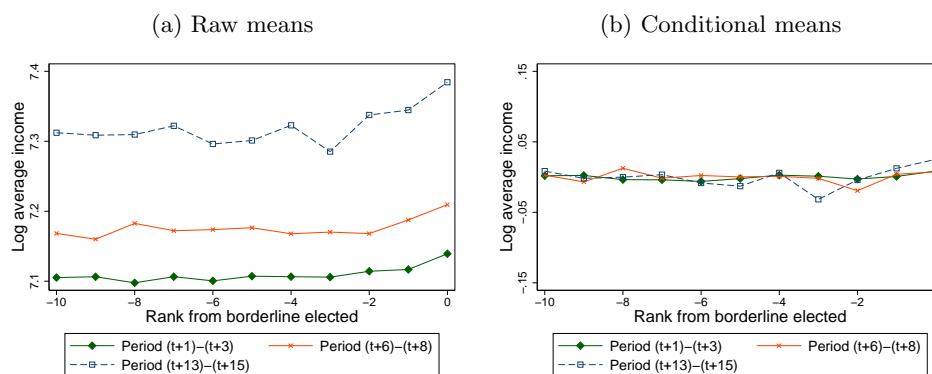
To start investigating what types of payoffs that motivate politicians, this section looks at the monetary returns from politics by analyzing the effect of being elected into a municipal council on short-, medium- and long-run income as measured by the log of disposable income 1–3, 6–8 and 13–15 years after being elected, respectively. The analysis combines graphical pre-



sentations with econometric methods as described in Section 4.

Let us first look at the graphics in Figure 4. It plots the  $rank^*$ -specific means of disposable income in the three different periods. The plot to the left shows raw means, whereas the plot to the right shows conditional means obtained from a regression of the outcome variable on a set of individual controls measured one year before the election; the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Recall that the variable  $rank^*$  is defined as the difference between a candidate's final rank and the final rank of the borderline elected, so that it takes the value zero for the borderline elected and negative values for non-elected candidates.

Figure 4: Short-, medium- and long-run disposable income



*Note:* The figures plot means of disposable income by rank from borderline elected in election year  $t$ . Income is deflated to 2000 year values and measured as logs of three-year averages in the short run (years  $t+1$  to  $t+3$ ), medium run (years  $t+6$  to  $t+8$ ) and long run (years  $t+13$  to  $t+15$ ). Conditional means are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.

*Source:* Statistics Sweden & The Swedish Election Authority.

Direct effects of  $rank^*$  on the outcome are represented by the overall slope of the lines connecting the  $rank^*$ -specific means. Conceptually, the treatment effect is the difference between the borderline elected ( $rank^* = 0$ ) and the borderline defeated ( $rank^* = -1$ ) that is above and beyond the difference between any other two candidates. Visually, a treatment effect therefore corresponds to a kink in the slope at  $rank^* = -1$ . The raw means to the left thus reveal small or zero effects on income from being elected.<sup>34</sup> This is particularly clear for medium-run income, where any kink

<sup>34</sup>Not only are the treatment effects absent, but what might be somewhat surprising is that also the direct effects of  $rank^*$  are negligible. Thus, to the extent that income is a proxy for ability (in some broader sense), candidates around the borderline elected are not ranked according to this.

at  $rank^* = -1$  is completely absent. For short- and long-run income, a slight kink can be detected. For the latter, however, as there is a considerably more distinct kink at  $rank^* = -3$ , this is more likely to be due to random variation than to a treatment effect.

Comparing the left and the right plots, the main difference is that there is a mean-adjustment to zero for all income periods (as these are residuals). Although this adjustment makes the plot less clear, it is suggestive of the same pattern as in the raw means, which thus suggests that to extensively control for pre-determined characteristics would not alter the results.

The econometric counterpart to the plots in Figure 4 is given in Table 2, providing the results from estimating equation (1) on candidates with  $rank^* = \{-2, -1, 0\}$ . Note that the parameter  $\beta_2$  is the marginal effect of  $rank^*$  and thus corresponds to the overall slopes in the plots, whereas  $\beta_1$  is the main parameter of interest that captures the additional effect of having  $rank^* = 0$ , or the effect of being elected.

The results in column 1 are for short-run income without any further controls while column 2 controls for the same set of individual controls as in the right plot with the conditional means. Equivalent results for medium- and long-run income are given in columns 3–4 and 5–6, respectively. As seen in the table, none of the estimated treatment effects are statistically significant and the point estimates are very close to zero either with controls (for the short and long run) or without controls (for the medium run). Note especially that the suspected kinks in short- and long-run income seen graphically are not statistically significant. Qualitatively, the inclusion of controls makes no difference, and—although the size of the estimates changes when controls are included—estimates with and without controls are within the 95% confidence interval of one another. In all regards, the econometric results thus confirm the graphical inspection.

## 7.1 Returns while in vs. after exiting politics

Some candidates elected in a particular election are still active politicians 6–8 and 13–15 years later, while others are not. The candidates' medium- and long-run income should be seen as the result of optimizing behavior, which may lead to political careers of different length for different people. But it is also of interest to see whether returns to politics kick in after leaving politics. In general, however, looking at income conditional on exiting politics is problematic since exit is endogenous. For example, some politicians may exit because they expect it to be profitable, and others may exit because they were unsuccessful incumbents.

A way of circumventing this problem is to look at the income profile of candidates who were not only elected by chance, but who also left politics for exogenous reasons. An exogenous source of variation in exit rates that lies close at hand is being borderline defeated. To this end, Figure 5 plots

Table 2: Effects of being elected on disposable income

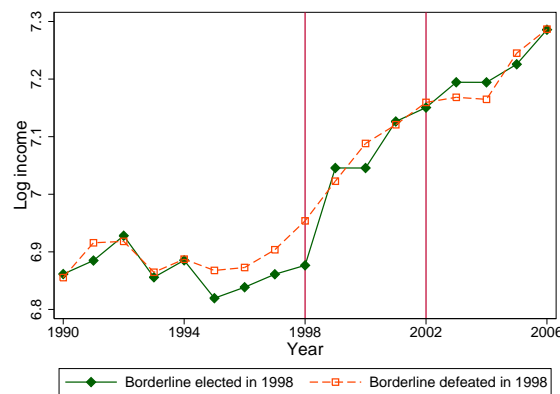
	Period $(t+1)-(t+3)$		Period $(t+6)-(t+8)$		Period $(t+13)-(t+15)$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0199 (0.0124)	0.00412 (0.00888)	0.00271 (0.0176)	-0.0184 (0.0154)	0.0327 (0.0282)	-0.00823 (0.0246)
rank*	0.00260 (0.00714)	0.00409 (0.00495)	0.0192* (0.0105)	0.0230*** (0.00883)	0.00713 (0.0165)	0.0192 (0.0145)
Observations	16673	16673	10915	10915	5283	5283
Individual controls	no	yes	no	yes	no	yes

*Note:* The table reports effects of being elected into a municipal council on disposable income measured as logs of three-year averages 1-3 (columns 1-2), 6-8 (columns 3-4) and 13-15 (columns 5-6) years after the election. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

the income profile of candidates who were borderline elected in 1998 *and* borderline defeated in 2002. This is to be compared with the income profile of candidates who were borderline defeated in 1998 and not elected in 2002 either, whose income profile is also seen in the figure. These candidates were neither elected in previous elections in the data (1991 or 1994), nor in the next election (2006). 975 candidates of the borderline defeated in 1998 satisfy these conditions, but only 59 of the borderline elected in 1998.

As seen in the figure, the income levels are very similar several years before the 1998 election, while being in office as well as after exiting. The exceptions are the few years preceding the 1998 election, when the income of those who get elected later is lower. Although, because of the small sample size, one should perhaps be careful about reading too much into this pattern. That there is no income gain following the exogenous exit from politics in 2002 is, however, clear.

Figure 5: Disposable income while in vs. after exiting politics



*Note:* The figure plots average disposable income among candidates who were borderline elected in 1998 *and* borderline defeated in 2002, and among candidates who were borderline defeated in 1998. Income is measured in logs of 100 SEK deflated to 2000 year values.

*Source:* Statistics Sweden & The Swedish Election Authority.

The above results all lead to the conclusion that monetary returns from politics are, on average, absent irrespective of if one considers the period right after the election, up to 15 years later or the period right after exiting politics. To support this conclusion, I have done further analyses on (i) total labor income and labor income from the largest source in stead of disposable income; and (ii) heterogeneous effects across parties, council size, party size, ruling status of the party and candidate's age, political experience, education level and pre-election income. None of these analyses show any systematic effects of being elected, thus strengthening the conclusion that there is no monetary payoff from politics.

## 8 Effects on future political careers

Given that monetary returns from politics are absent, politicians are likely *not* motivated by that. Rather, it seems that there are some non-monetary returns that politicians pursue. These can be in the form of political accomplishments, a sense of actively taking part in the community, the desire to affect society in a certain direction, prestige and power—types of returns that are hard if not impossible to measure. However, such non-monetary returns are to a large extent encompassed by a candidate’s political career. In what follows I therefore investigate if being elected into a municipal council improves future political career prospects. I begin by studying if, for exogenous reasons, being elected improves the chances of being elected also in future local elections, and then move on to see whether it increases the chances of advancing to national politics. Analogously to the previous section on monetary returns, most of the analysis on political careers is carried out graphically as well as econometrically.

### 8.1 Local politics

I start by assessing if being elected into a municipal council in election year  $t$  has an effect on the probability of running in future elections to a municipal council in the short, medium and long run, corresponding to the first, second and fourth subsequent election, respectively. The results are presented in Figure 6 and Table 3, and are to be read in the same way as above except that the outcome is now the probability of being nominated for election instead of income.

Like those above, these graphs are quite illustrative. Not surprisingly, there is a positive direct relationship between  $rank^*$  and the probability of running in future elections to a municipal council, as seen from the overall positive slopes. However, there is little evidence of any treatment effect of being elected, as there is no kink in the slope between the borderline elected and defeated (at  $rank^* = -1$ ), except maybe in the second subsequent election. This pattern is confirmed by Table 3, where all estimates are statistically insignificant except the one for the election in  $t + 2$  (at the 10% level and only without individual controls). Controlling for individual characteristics barely affects the point estimates and—just like for income—the only graphical differences are in the intercepts.

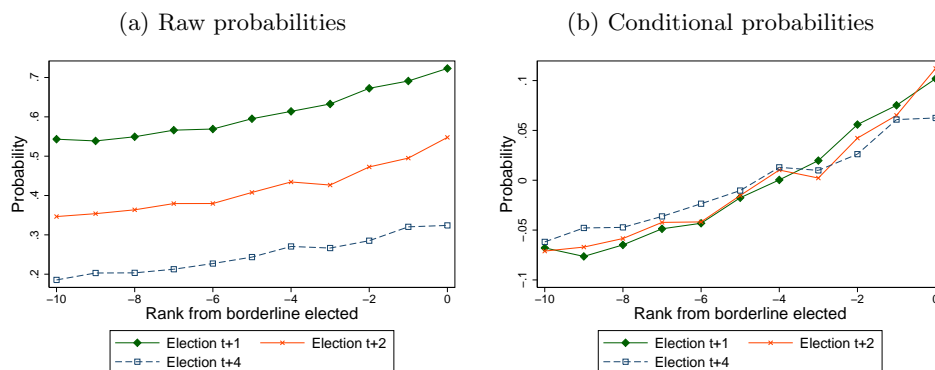
The large average probabilities of running in future elections as seen from the left plot in Figure 6 show that there is a high degree of persistence in who runs for elections, especially in the short run. One possibility is that candidates who are not ranked sufficiently high to be elected in election  $t$  to a large extent also run in subsequent elections because they perceive their chances of being elected to increase, perhaps if they are compensated by being ranked higher. If that is the case, one should expect no effect of being

Table 3: Effects of being elected on the probability of being nominated in future elections to a municipal council

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0133 (0.0133)	0.00960 (0.0133)	0.0302* (0.0183)	0.0251 (0.0180)	-0.0316 (0.0264)	-0.0328 (0.0267)
rank*	0.0186** (0.00778)	0.0198** (0.00782)	0.0224** (0.0109)	0.0234** (0.0107)	0.0353** (0.0157)	0.0348** (0.0159)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

*Note:* The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

Figure 6: Probabilities of being nominated in future elections to a municipal council



*Note:* The figures plot the probability of being nominated in future elections to a municipal council by rank from borderline elected in election year  $t$ . Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.

*Source:* Statistics Sweden & The Swedish Election Authority.

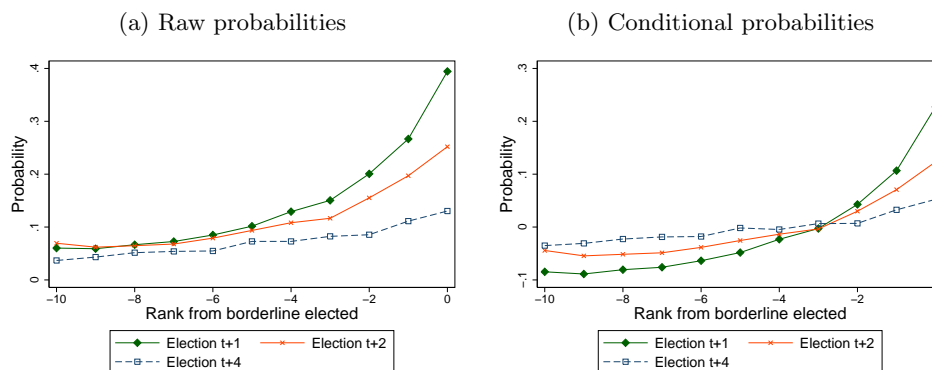
elected at time  $t$  on also being elected in future elections.

These future election probabilities are assessed in Figure 7 and Table 4.<sup>35</sup> Focusing on candidates with  $rank^* = \{-2, -1, 0\}$ , from the graphics one can detect a positive short-run treatment effect (i.e., being elected in the first subsequent election), as there is a kink between the borderline elected and defeated. According to columns 1–2 in Table 4, this effect is a statistically significant 6 percentage points and it is unaffected when controlling for individual characteristics. As suggested by Figure 7 and as confirmed in columns 3–6 in Table 4, there are, however, no effects of being elected in election  $t$  on also being elected in elections  $t + 2$  and  $t + 4$ .

In terms of magnitude, 6 percentage points amount to about the same size as the direct effect of  $rank^*$ , and around 20% of the mean election rate in election  $t + 1$  for the sets of three candidates with  $rank^* = \{-2, -1, 0\}$  in the borderline groups (see the descriptive statistics in the Appendix). However, recall from the discussion in Section 6 that a fair share of the borderline defeated who initially were council replacements in fact overtook a permanent council seat, so that—if treatment is defined as actually having

<sup>35</sup>Note that these are unconditional election probabilities, in the sense that they are not conditional on running. The reason for this is that the decision to run in future elections can conceptually be an outcome due to the treatment, which means that a causal interpretation of the conditional effects on being elected would not be valid. In practice, because of the previous result that there are no large effects on running probabilities, conditioning on running only scales up the future election probabilities without making a qualitative difference.

Figure 7: Probabilities of being elected in future elections to a municipal council



*Note:* The figures plot the probability of being elected in future elections to a municipal council by rank from borderline elected in election year  $t$ . Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.

*Source:* Statistics Sweden & The Swedish Election Authority.

served in the council—treatment status is fuzzy at the threshold at  $rank^* = 0$ . Evaluating the magnitude of such a treatment effect requires scaling up the coefficient by around three (since the first stage is estimated to around 0.30). Thus, the obtained result means that having served in the council in the previous election period explains as much as 60% of the probability of being elected in the next election.<sup>36</sup>

For the borderline elected's 6 percentage points higher probability of being elected in the next election obtained above to be interpreted as a causal treatment effect, the direct effect of  $rank^*$  must be at most of order one for  $rank^* = \{-2, -1, 0\}$ . Since this is an identifying assumption, it is not possible to test it directly. But there are several ways of indirectly investigating whether the obtained effects are likely to be causal. First, it is more likely that the direct effect is linear between this set of three candidates if it is also linear for another set of three candidates close by—i.e., if there are no kinks in the slope of  $rank^*$  of similar magnitude between any other two candidates further down the ranking. This can be more or less inferred from the graphics, but it can also be formally tested with placebo regressions that, falsely, assign the borderline elected status to candidates with  $rank^* = -1$  or  $rank^* = -2$ . Doing this on the probability of being elected in the first subsequent election to a municipal council results in estimated coefficients that are about 30–40% of the size of the effect for the

<sup>36</sup> Although by the same token, if they become council replacements, some of those who are not elected in the next election can also end up serving in the council.



Table 4: Effects of being elected on the probability of being elected in future elections to a municipal council

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0619*** (0.0139)	0.0583*** (0.0138)	0.0131 (0.0157)	0.0112 (0.0155)	-0.00664 (0.0179)	-0.00651 (0.0177)
rank*	0.0659*** (0.00728)	0.0663*** (0.00732)	0.0418*** (0.00858)	0.0417*** (0.00853)	0.0257** (0.0102)	0.0264** (0.0102)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

*Note:* The table reports effects of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

true borderline elected (i.e., around 2–2.5 percentage points) and that are also statistically significant.<sup>37</sup>

One possible interpretation of these placebo estimates is that there is a non-linear direct effect of  $rank^*$  (which is in fact suggested by Figure 7). But if the direct effect of rank is of higher order than one for the set of three candidates with  $rank^* = \{-2, -1, 0\}$  and for candidates further down the ranking, the effect of being elected can still be recovered by—as explained in Section 4—running equation (2) on more than three candidates per borderline group. The resulting estimates from this exercise are found in Table 5. Equation (2) is estimated on the sample of borderline elected candidates plus ten defeated candidates in columns 1, 3 and 5, for the first, second and fourth election, respectively. In columns 2, 4 and 6, five instead of ten defeated candidates are included in the estimations. Each column contains results from three different regressions with a linear, quadratic or cubic function of  $rank^*$ , of which the one preferred by the Akaike information criterion (AIC) is in bold.

The point estimates as well as the significance levels seen in Table 5 are somewhat sensitive to different bandwidths (the number of defeated candidates included) and order of polynomial. But restricting the attention to the AIC-preferred specifications, the previous result in Table 4 from estimating the baseline regression (1) is quite robust; being borderline elected in election  $t$  increases the chances of being elected in election  $t + 1$  by around 5 percentage points (compared to 6 in Table 3), but does not affect the election probabilities in later elections.

An alternative way of investigating the linearity assumption underlying the baseline results in Table 4 is to test whether the estimates differ when it is unlikely that the parties could have known who would be the borderline elected to win the last seat. The idea is that the ranking of candidates would be different if it was a priori certain who would actually be elected. I propose a number of instances when there was presumably more uncertainty regarding this, and present the results in Table 6 (where column 1 reproduces the baseline results with controls in column 2 of Table 4): (i) the party’s number of seats changed from the previous election, cf. column 2; (ii) the party won their last seat or were close to winning an additional seat with a vote margin of less than 1 and 0.5%, cf. columns 3–4; and (iii) a combination of (i) and (ii), cf. column 5.<sup>38</sup> The table shows estimates that are quite robust across the different specifications. For example, the estimate hardly changes even when the sample size is cut in half as the vote margin of the last seat is restricted to 0.5%. This is reassuring evidence that parties in

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<sup>37</sup>The placebo estimates are found in columns 1–2 of Table 12 in the Appendix.

<sup>38</sup>The robustness checks are performed on the probability of being elected in the first subsequent election to a municipal council where the baseline estimates were significant. As vote margins, I use the minimum changes in votes to win or lose an additional seat in proportional elections as defined and calculated by Folke (2011).

Table 5: Effects of being elected on the probability of being elected in future elections to a municipal council; allowing non-linear effects of  $rank^*$

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected, $\bar{p} = 1$	0.156*** (0.00774)	0.104*** (0.00898)	0.0744*** (0.00810)	0.0409*** (0.00981)	0.0232*** (0.00824)	0.0178* (0.0102)
elected, $\bar{p} = 2$	0.0827*** (0.00948)	<b>0.0511***</b> <b>(0.0136)</b>	0.0240** (0.0104)	<b>-0.000853</b> <b>(0.0151)</b>	<b>0.0130</b> <b>(0.0107)</b>	<b>-0.00437</b> <b>(0.0165)</b>
elected, $\bar{p} = 3$	<b>0.0532***</b> <b>(0.0124)</b>	0.0210 (0.0257)	<b>0.00972</b> <b>(0.0143)</b>	-0.0132 (0.0297)	0.00233 (0.0156)	-0.0227 (0.0361)
Observations	54798	32504	36430	21620	18239	10888
$rank^* \geq$	-10	-5	-10	-5	-10	-5

*Note:* The table reports effects of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

Table 6: Robustness checks of the effects on being elected in the first subsequent election to a municipal council

	(1)	(2)	(3)	(4)	(5)
elected	0.0583*** (0.0138)	0.0590*** (0.0162)	0.0472*** (0.0158)	0.0585*** (0.0202)	0.0420** (0.0186)
$rank^*$	0.0663*** (0.00732)	0.0612*** (0.00870)	0.0697*** (0.00840)	0.0588*** (0.0113)	0.0675*** (0.00994)
Observations	16754	12692	13283	7737	10080
Vote margin (%)	no restr.	no restr.	1	0.5	1
$ \Delta seats  \geq 1$	no	yes	no	no	yes
Individual controls	yes	yes	yes	yes	yes

*Note:* The table reports effects of being elected into a municipal council on the probability of being elected in the first subsequent election to a municipal council. Column 1 reproduces the baseline results in column 2 of Table 4. All regressions include individual controls (cf. Table 4). Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

general cannot perfectly anticipate how many votes they will win and rank their candidates accordingly.

## 8.2 National politics

If politicians are motivated by politics-specific non-monetary payoffs such as power or prestige, their goal should be to pursue a successful political career. So far, the analysis has focused on local political careers. This section investigates whether being a local politician can be a path to national politics. Specifically, I investigate whether being elected into a municipal council in election  $t$  affects the probability of being nominated to the national parliament in election  $t + 1$ ,  $t + 2$  and  $t + 4$ . The outline is the same as in the previous section; I begin by presenting graphical and econometric results, and then perform a robustness check of the baseline results.

Effects are presented graphically in Figure 8 with associated estimates in Table 7, which reveal a clear and significant positive effect in the short and medium run (first and second subsequent elections). The magnitude is around 2–3 percentage points in both elections (although somewhat higher in the short run), which is around twice the size of the estimated direct effect of  $rank^*$  and around 30% of the overall mean probability of running for the national parliament among candidates with  $rank^* = \{-2, -1, 0\}$  in the borderline groups. Considering, instead, the treatment of actually having served in the council (of which the average probability jumps by around 0.30 for the borderline elected)<sup>39</sup>, an alternative interpretation of these results is that short- and medium-run chances of being nominated to the national parliament are almost fully explained by municipal council experience.

Compared to the graphical analysis on local election probabilities above (cf. Figure 7), the linearity assumption in the direct effect of  $rank^*$  seems less restrictive here. Moreover, supporting the interpretation of the kinks at  $rank^* = -1$  as representing an effect of being elected is that placebo regressions of the short- and medium-run probabilities of being nominated to the national parliament that assign the borderline elected status to candidates with  $rank^* = -1$  or  $rank^* = -2$  yield small and insignificant estimates—a result in accordance with what is suggested by the graphs.<sup>40</sup>

For long-run effects—that is, for effects on being nominated in election  $t + 4$ —the graphs are suggestive of a kink at  $rank^* = -2$ . In a placebo regression, this “effect” is estimated to 2 percentage points and is significant at the 10% level. Because this is a long-run outcome, a possible explanation is that the borderline defeated in election  $t$  ran successfully for the munic-

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<sup>39</sup>Recall from Section 6 that this figure is obtained by using the 2002 and 2006 election data to estimate a first stage with the probability of actually having served in the council as a discontinuous function of  $rank^*$ .

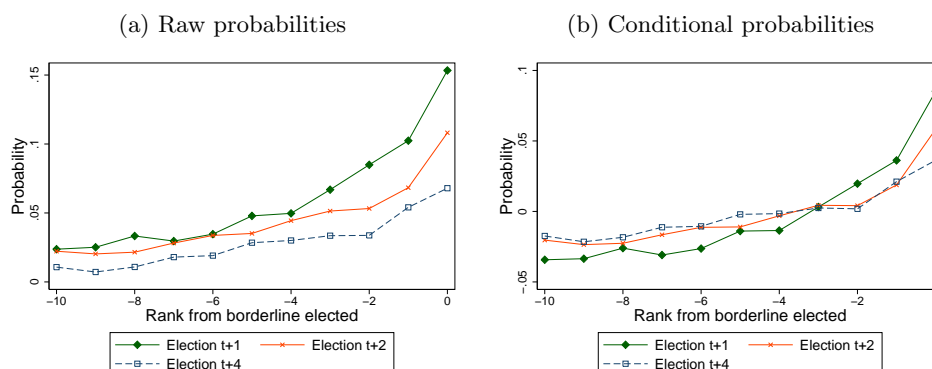
<sup>40</sup>Placebo estimates for the probability of being nominated for the national parliament are provided in Table 13 in the Appendix.

Table 7: Effects of being elected on the probability of being nominated in future elections to the national parliament

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected	0.0334*** (0.0102)	0.0333*** (0.0100)	0.0247** (0.00977)	0.0259*** (0.00965)	-0.00647 (0.0125)	-0.00229 (0.0126)
rank*	0.0175*** (0.00510)	0.0169*** (0.00497)	0.0151*** (0.00554)	0.0145*** (0.00548)	0.0203*** (0.00672)	0.0185*** (0.00680)
Observations	16754	16754	11208	11208	5710	5710
Individual controls	no	yes	no	yes	no	yes

*Note:* The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. Individual controls measured one year before the election are: number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

Figure 8: Probabilities of being nominated in future elections to the national parliament



*Note:* The figures plot the probability of being nominated in future elections to the national parliament by rank from borderline elected in election year  $t$ . Conditional probabilities are the residuals obtained from a regression of the outcome variable on the following individual controls measured one year before the election: the number of children aged below 18 and a set of dummies for age, gender, marital status, income quantile, highest completed education, foreign background and past political experience.

*Source:* Statistics Sweden & The Swedish Election Authority.

ipal council in election  $t + 1$ , which increased the chances of running for parliament in election  $t + 4$ .

As in the previous section, the baseline results in Table 7 from estimating equation (1) on the set of three candidates per borderline group are complemented with results from estimating equation (2), expanding the bandwidth to include five or ten defeated candidates. The results are given in Table 8. These estimates are more robust across the different bandwidths and polynomials, and thus give little reason to doubt the effect of a 2–3 percentage point increase in the probability of being nominated in the first and second subsequent election to the national parliament.

To further support this conclusion, Table 9 tests the robustness of the statistically significant effects on being nominated for parliament in elections  $t + 1$  and  $t + 2$ . Column 1 reproduces the baseline estimates with controls in columns 2 and 4 of Table 7, and columns 2–4 are the result from the same set of robustness checks as in Table 6 for local politics above. As can be seen, the effects in the first subsequent election are very robust, while the effects in the election thereafter only partly survive the various sample restrictions. For the latter, the drop in the coefficient is especially large when the sample is restricted to parties that were 0.5% votes from winning or losing an additional seat (column 4, bottom panel). This drop is most likely due to a combination of the sample size reduction of 50% and—as is shown in Lundqvist (2011)—the fact that effects are quite heterogeneous across party and council size.

Table 8: Effects of being elected on the probability of being nominated in future elections to the national parliament; allowing non-linear effects of  $rank^*$

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected, $\bar{p} = 1$	0.0566*** (0.00560)	0.0395*** (0.00648)	0.0415*** (0.00541)	<b>0.0350***</b> <b>(0.00645)</b>	0.0182*** (0.00612)	0.0149** (0.00717)
elected, $\bar{p} = 2$	0.0318*** (0.00688)	<b>0.0238**</b> <b>(0.0102)</b>	<b>0.0318***</b> <b>(0.00687)</b>	0.0316*** (0.00876)	<b>0.0108</b> <b>(0.00776)</b>	<b>-0.00208</b> <b>(0.0117)</b>
elected, $\bar{p} = 3$	<b>0.0240**</b> <b>(0.00934)</b>	0.0458** (0.0181)	0.0327*** (0.00861)	0.00986 (0.0180)	0.00689 (0.0104)	-0.0274 (0.0234)
Observations	54798	32504	36430	21620	18239	10888
$rank^* \geq$	-10	-5	-10	-5	-10	-5

*Note:* The table reports effects of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. The AIC-preferred polynomial is in bold. Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

Table 9: Robustness checks of the effects on being nominated in the first and second subsequent elections to the national parliament

	(1)	(2)	(3)	(4)	(5)
	Election $t + 1$				
elected	0.0333*** (0.00990)	0.0249** (0.0110)	0.0450*** (0.0107)	0.0325** (0.0139)	0.0354*** (0.0117)
$rank^*$	0.0149*** (0.00490)	0.0143** (0.00565)	0.00816 (0.00543)	0.00877 (0.00707)	0.00697 (0.00620)
Observations	16754	12692	13283	7737	10080
	Election $t + 2$				
elected	0.0259*** (0.00965)	0.0171 (0.0107)	0.0276** (0.0107)	0.0123 (0.0138)	0.0217* (0.0116)
$rank^*$	0.0145*** (0.00548)	0.0161** (0.00655)	0.0129** (0.00585)	0.0158** (0.00787)	0.0119* (0.00673)
Observations	11208	8662	8947	5276	6934
Vote margin (%)	no restr.	no restr.	1	0.5	1
$ \Delta seats  \geq 1$	no	yes	no	no	yes
Individual controls	yes	yes	yes	yes	yes

*Note:* The table reports effects of being elected into a municipal council on the probability of being elected in the first (top panel) and second (bottom panel) subsequent election to the national parliament. Column 1 reproduces the baseline results in columns 2 and 4 of Table 7. All regressions include individual controls (cf. Table 7). Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

## 9 Heterogeneity and external validity of the results

The results from the above section on future political careers can be summarized as revealing positive effects of being borderline elected into a municipal council on, first, the chances of being elected also in the next local election and, second, on the probability of being nominated to the national parliament in subsequent elections. Before concluding the paper, this section first provides a few notes on the heterogeneity in these average effects, and then discusses whether the results can be generalized from marginal, borderline politicians to more prominent, successful ones.

Interestingly, the analysis of heterogeneous effects conducted in Lundqvist (2011) shows that the effects on political career prospects do not differ to any considerable extent across candidates with different levels of education (at least not when considering the chances of advancing to the national arena), across different age categories or across candidates with or without previous political experience.<sup>41</sup> The only differences of any significance are those across council and party size; the positive effects of being borderline elected on future local election probabilities and national nominations are especially pronounced for candidates running for smaller parties and smaller councils. One possible mechanism behind this pattern is that those borderline elected into larger parties are marginal politicians who are less visible, whereas those borderline elected into smaller parties can be quite prominent figures. Under this mechanism, the interpretation of the results is that what is of importance for political career prospects is being a prominent local representative, and not simply being elected.<sup>42</sup>

The distinction between the prominent figures and the more marginal politicians motivates a discussion of the external validity of the results obtained with the method in the paper focusing on borderline candidates. In particular, returns to politics for these candidates may not be representative for successful politicians (or even for the “average politician”). I address this concern by checking whether there are any successful politicians at all among the borderline elected.

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<sup>41</sup>A caveat with the comparisons of some of the subgroup specific effects, however, is that they are estimated with rather poor precision.

<sup>42</sup>An alternative but related explanation for this pattern is that what matters for political career prospects is actual council experience, rather than being elected into the council on election day. This would imply that the null effects among the larger parties are explained by a lack of sufficient variation in council experience between the borderline elected and defeated candidates. The reason for this—i.e., why the de facto treatment of actually having served in the council is more “fuzzy” between elected and defeated candidates in the larger parties—is that, because a replacement can stand in for several regular members, defeated candidates from larger parties who are council replacements are more likely to take over a permanent seat (as compared to defeated candidates from smaller parties).



For this purpose, “political successes” that are measurable with the available data need to be defined, and here I distinguish between local and national careers: A *local career* is defined to be successful for politicians who are appointed chair/vice chair in the executive board or for those whose political assignments constitute their main job (defined as receiving political remuneration amounting to at least 75% of a full-time assignment).<sup>43</sup> Since this information is only available in the 2006 data, all local careers are defined in that election period. A *national career* is defined to be successful for politicians who are elected into the national parliament. Motivated by the extensive analysis of national nomination probabilities above, I also define “quasi-national successes” as those who are nominated for but not elected into the national parliament. National careers are defined in any election following the one in which the candidate was borderline elected into the local council, but not later than the 2006 election since this is where the data ends.

With these definitions, 38 of the borderline elected (from the 1991, 1998 or the 2002 election) have a successful local career in the executive board, 37 have a salaried local political assignment, 1032 are only nominated for the national parliament and 39 are indeed elected into the parliament. While—except for the nationally nominated—these are too few to allow for any rigorous econometric analysis like that above, a statistical description of these more prominent figures is definitely worthwhile. Hence, columns 2–5 of Table 10 provide summary statistics characterizing these four groups of politicians with different political careers. For comparison, the same is given in column 1 for the remaining 4587 borderline elected who do not fall into any of the four success categories.<sup>44</sup>

The top of the table, showing the distribution over election periods in which they were borderline elected, reveals an interesting picture: Compared to those without a successful political career (in the sense as just defined), a much smaller fraction of the successes are borderline elected in the 2002 but instead already in the 1991 election (except for the “Salaried” where the majority is from the 1998 election). Given how the successes are defined, this pattern indicates that politicians are required to stick around for many election periods before their career peaks. The next two rows support this claim by showing that many indeed do so; of the five elections held between 1991 and 2006 covered by the data, politicians with successful local careers on average ran 4–4.4 times and were elected as many as 3.6–3.8 times. Also those without any success ran quite frequently—on average in three out of

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<sup>43</sup>Recall that only around 8% of the politicians elected into the council receive full-time or part-time compensation.

<sup>44</sup>There is some overlap between the four success categories: 16 candidates are both in the “Executive” and “Salaried” category; 15 candidates are both in the “Executive” and “Nominated” category; 21 candidates are both in the “Salaried” and “Nominated” category and 1 candidate is both in the “Salaried” and “Elected” category.

Table 10: Descriptive statistics of borderline elected candidates with different future political careers

	No career	Local career		National career	
		Executive	Salaried	Nominated	Elected
$t = 1991$	0.32 (0.47)	0.47 (0.51)	0.35 (0.48)	0.47 (0.50)	0.59 (0.50)
$t = 1998$	0.33 (0.47)	0.29 (0.46)	0.49 (0.51)	0.28 (0.45)	0.26 (0.44)
$t = 2002$	0.35 (0.48)	0.24 (0.43)	0.16 (0.37)	0.25 (0.43)	0.15 (0.37)
# nominated locally	3.07 (1.38)	4.42 (0.89)	4.05 (0.91)	3.67 (1.22)	3.64 (1.20)
# elected locally	1.86 (1.06)	3.87 (1.26)	3.57 (1.21)	2.50 (1.22)	2.87 (1.30)
# nominated nationally	0.20 (0.54)	0.79 (0.99)	1.19 (1.24)	2.14 (1.18)	3.36 (1.31)
# elected nationally	0.00065 (0.033)	0 (0)	0.054 (0.33)	0.0048 (0.082)	1.87 (1.10)
Party size	7.98 (7.18)	10.1 (7.02)	12.9 (7.75)	4.36 (4.75)	6.44 (7.87)
Council size	47.3 (12.9)	46.4 (9.61)	59.5 (16.1)	49.5 (14.2)	59.5 (19.5)
Observations	4587	38	37	1032	39

*Note:* The table reports means and standard deviations (in parentheses) in the sample of borderline elected candidates to a municipal council in elections  $t \in \{1991, 1998, 2002\}$ , separately by their future political careers. Local careers are defined in the 2006–10 election period. National careers are defined in any election  $t + j$  covered by the data. Party/Council size indicate the number of seats held by the party/the total number of seats in election period  $t$ .

*Source:* Statistics Sweden.

five elections—but had a notably lower election rate.

Naturally, those who had a *national* career after having been borderline elected locally ran and were elected in fewer *local* elections than those with a successful *local* career, but still more frequently than the unsuccessful ones. It is also interesting that those who were later indeed elected into the parliament were on average nominated thereto in more than three out of five elections, and that conditional on being elected at least once later, they were on average elected close to one additional time (where only a few were elected nationally in the same or previous elections as when being borderline elected locally)<sup>45</sup>.

The last two rows in the table are motivated by the heterogeneous effects across party and council size as found by Lundqvist (2011) and alluded to above—the smaller average party size in column 4 partly reflects the more pronounced effects on national nomination probabilities for candidates running for smaller parties.<sup>46</sup> However, politicians with a successful local career on average run for larger parties. This is most likely explained by the fact that larger parties have more seats in the executive board (by default) as well as a larger number of salaried positions.

Table 10 portrays the borderline elected candidates with the five different definitions of future careers in the political dimension. But what about income: Do different future political careers also imply different income streams? Annually over 1990–2006, Figure 9 plots disposable income for borderline elected candidates from the 1991 election, grouped as in Table 10 but with the two different local careers merged into one category.<sup>47</sup> Although the different income series start out very similar, as is revealed quite clearly, those with a future successful local or national political career do indeed have substantially higher income later on as compared both to the “quasi-national successes” and the unsuccessful ones. A caveat is of course the small number of successes (only 25 and 23 of the borderline elected in 1991 are defined to have a successful career locally and nationally, respectively). But even aside from the small sample, the picture in Figure 9 should be interpreted with care. Specifically, the higher income among those with a successful local career and those elected nationally as compared to those who are only nominated nationally and the unsuccessful ones is not necessarily

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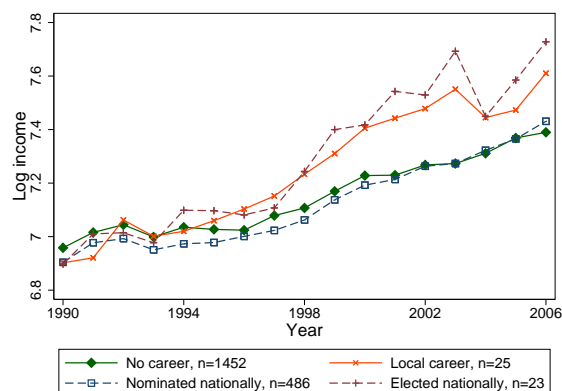
<sup>45</sup>Note that the variables *# nominated/elected locally/nationally* are defined over *all* election periods covered by the data, whereas the success categories are defined in election periods *after* the candidate was borderline elected. This explains, for example, why some individuals who were nominated to the national parliament at least once over the entire period are categorized as “No career” and not as “Nominated”.

<sup>46</sup>Although only partly so since the estimated heterogeneous effects in Lundqvist (2011) are separate within-differences in outcomes for the borderline elected and the borderline defeated, whereas Table 10 displays between-differences of the borderline elected.

<sup>47</sup>There are several reasons for combining the two categories of local careers; (i) the small number of candidates in each of the two; (ii) the candidate overlap between the two; and (iii) that the two turn out to be quite similar.

an *effect* of having a successful political career, the reason being that the latter two are not necessarily unbiased counterfactuals for the former. In other words, since it is not random who are successful, one cannot know what the income of the successful ones would have been had they not been successful.

Figure 9: Disposable income among candidates borderline elected in 1991



*Note:* The figure plots average disposable income among candidates borderline elected into a municipal council in the 1991 election, separately by their future political careers. Local careers are defined in the 2006–10 election period. National careers are defined in any election  $t + j$  covered by the data. Income is measured in logs of 100 SEK deflated to 2000 year values.

*Source:* Statistics Sweden & The Swedish Election Authority.

Thus, the pattern seen in Figure 9 does not conflict with the result from Section 7 that monetary returns from politics are absent. Rather, this section has shown a pattern indicating either that successful political careers pay well, *or* that those with high earnings-potential have successful political careers. The section has also shown that it is indeed possible to start out as a borderline elected candidate and end up chairing the influential executive board or being elected into the national parliament. Even though they are not very frequent, these examples suggest that the results obtained in the paper can likely be generalized to more prominent political figures, as opposed to only pertaining to marginal, less visible politicians. Furthermore, what can be deduced from the descriptives is that commitments spanning quite long time periods seem necessary (but perhaps not sufficient) for climbing the political career ladder.

## 10 Concluding remarks

By estimating causal effects of being elected in local elections on income and political career prospects, this paper has looked for empirical evidence of what types of payoffs that motivate politicians. I argue that local politics

is the relevant context for studying politicians' motivations, since this is where the majority of political careers start off.

Using a regression discontinuity design where the income of elected candidates who just barely won a seat (*the borderline elected*) is compared to that of non-elected candidates who were close to winning a seat (*the borderline defeated*), the paper has shown that monetary returns from politics are absent. This seems to be true irrespective of whether one considers the period right after the election, up to 15 years later or the period right after exiting politics. It is also true on average as well as when considering heterogeneous effects across various dimensions of parties, councils and candidates. Note, though, that this is not to say that there are no politicians making money. In particular, those (relatively few) borderline elected candidates who later had a successful political career indeed also had higher income. As to the interpretation of this finding, however, with the current framework it is not possible to distinguish between a positive causal effect on income of having a successful political career and a selection effect (i.e., that those with successful political careers are those with a high earnings-potential in general).

Returns to politics can also be non-monetary, such as power, prestige or reaching some ideological goal. Although very hard to measure as such, these politics-specific payoffs are to a large extent encompassed by a candidate's political career. Hence, the paper looked at whether—for exogenous reasons—being elected in a local election improves future political career prospects. A key, robust finding from this analysis is that being borderline elected into a municipal council on average increases the probability of being nominated for the national parliament in subsequent elections by around 30%. Considering, instead, the treatment of actually having served in the council, these results imply that the chances for nominations are almost fully determined by municipal council experience. Another finding is that being borderline elected into a municipal council also seems to increase the chances of being elected in the next local election.

The result of no positive monetary returns from politics stands in contrast to findings from the few previous studies: Diermeier et al. (2005) find that political experience increases earnings for US congressmen upon exiting politics, and Eggers and Hainmueller (2009) estimate substantial effects on wealth (at the time of death) for conservative politicians who were elected into the British parliament with narrow vote margins. These contrasting results can potentially be explained with their focus on national rather than local politics. Another interesting potential explanation is the difference in political institutions. Sweden is characterized by a typical multi-party, proportional representation system where the parties are the main political players. Therefore, it is sensible that monetary returns are larger in countries like the US and Great Britain where the focus lies (more or less) on individual politicians and where strong characters can make a career

rather quickly. In contrast and as shown here, in a party-dominant context such as Sweden, it is indeed possible to start out as a marginal, borderline elected candidate and end up chairing the influential executive board or being elected into the national parliament, but climbing the career ladder of political parties takes time. Since such time investments do not seem to be rewarded in monetary terms, investigating how this and other differences in political institutions affect the selection of politicians is a very interesting (but challenging) avenue for future research.

If time devoted to politics does not—for the vast majority—pay off financially, then why is it—as shown by the data—so common for politicians to stick around for so many election periods? It could be that they perceive their chances of becoming a successful, well-paid politician to be high enough to motivate them to continue. But given that the average probability is very low, a more likely answer is that they are motivated by non-monetary politics-specific payoffs such as actively taking part of the community and striving towards some ideological goal.

This paper has shown that being elected once by chance (i.e., being borderline elected) can be quite an effective starting point for enjoying such payoffs. To conclude, a couple of real-life examples retrieved from the data further illustrate this effect:

- A pair of male candidates aged around 40 and members of the Christian Democratic Party ran in 1991 for a seat in the local council in a median-sized municipality (population around 15000) located in the Stockholm metropolitan area. One was borderline elected, the other was borderline defeated. The borderline defeated left politics—i.e., he never ran again. The borderline elected continued to run in all local elections up until 2006, and did so successfully except for in the 1994 election. He was also nominated for the national parliament in 2006, where he was nine rank positions away from being elected. In the election period 2006–10, he held a regular position in the executive board and one additional working committee.
- A pair of female candidates aged around 40 and members of the Left Party ran in 1991 for a seat in the local council in a small-sized, northern municipality (population around 8000). One was borderline elected, the other was borderline defeated. The borderline defeated continued to run also in the 1994 and 1998 elections, but was not elected then either. The borderline elected was further elected locally in the 1994 and 1998 elections. She was also nominated to the national parliament in all subsequent elections up until 2006, when she was finally elected.

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## Determining the final ranking

This Appendix describes some of the complications in determining the order in which candidates for a party with a given number of seats is elected—i.e., how the ballot paper rankings translate into the final ranking. The full procedure is stipulated in the Elections Act.

Starting with the 1998 election, voters can mark *one* preferred candidate on the ballot paper (so-called preference voting). The top of the final ranking is set based on the ranking of such preference votes, given that a candidate has reached the preference vote threshold of 5% of the party's votes in the constituency, which must be at least 50 votes.

For candidates who do not reach the preference vote threshold (or for all candidates prior to the 1998 election), comparison numbers are calculated and ranked. The comparison numbers are calculated based on votes per ballot paper and the so-called “whole-number method”. In the case of one ballot paper per constituency, the ranking of comparison numbers simply boils down to the party's ballot paper ranking of candidates who did not reach the preference vote threshold. These relatively simple cases constitute around 90%. Matters become much more complex in the case of multiple ballot papers per constituency, where comparison numbers and the associated final ranking depend on a combination of the number of votes per ballot paper and the number of ballot papers and how high each candidate was ranked on the various ballot papers.

Additional complications in determining the final ranking arise when candidates are sufficiently highly ranked in several constituencies (or for several parties, although this rarely happens), for example as a consequence of their party running with the same ballot paper in several constituencies. This happens in around 30% of the cases. A candidate can only fill one seat, which leaves the remaining seats to be assigned to someone else—a procedure known as “double-election replacement”.

## Finding the borderline groups

When the final ranking is completely known, it is quite straightforward to determine which candidates constitute the borderline groups. However, not all data is in sufficient detail to allow for completely determining the final ranking and hence, to find the borderline groups without making some assumptions.

Due to the lack of ballot paper rankings, it is not possible to determine any borderline groups in the 1994 election. Also the 1991, 1998 and 2002 election data is in different levels of detail—the later the election, the more detailed the data.

For the 2002 election, data is sufficiently detailed to reproduce nearly the exact final ranking. The exception is preference votes, where the information

is limited to whether or not a candidate reached the threshold but not by how much, hindering ranking of such candidates and implying that identifying a borderline elected is only possible when at least one candidate is elected via comparison numbers. This also implies that the borderline elected is never elected via preference votes but always via comparison numbers.

Determining the final ranking in the 2002 election by applying the rules as stipulated in the Elections Act to the various combinations of ballot paper rankings and ballot paper votes results in the error event that a candidate is labeled as elected in a particular *constituency* when in fact he is not, or vice versa, that amounts to 0.8%. The corresponding percentage at the *council* level is as low as 0.03.<sup>48</sup>

For the 1991 and 1998 elections, some assumptions were needed about the interdependence of ballot papers in the case of multiple-constituency municipalities and/or constituency-overlapping ballot papers to identify the borderline groups. Applying the assumptions used for the 1998 election to the 2002 election results in about 90% identical borderline groups consisting of the sets of three candidates with  $rank^* = \{-2, -1, 0\}$ .

The majority of borderline groups are at the constituency level. However, when a ballot paper overlaps several constituencies, the group is at the municipality level. The reason is that it is hard to think of a candidate as being borderline elected in some constituency if other candidates on the same ballot paper were also elected, but in a different constituency. This can happen as a consequence of the double-election replacement procedure.

Candidates with missing values on either of the control variables are dropped in all estimations. Furthermore, only candidates from borderline groups that have a borderline elected are included. Groups missing a borderline elected mainly occur because the candidate is dropped due to missing values on control variables, or because no candidate within the group was elected via preference votes.

The final number of borderline elected candidates amounts to 1917, 1838 and 1837 from the 1991, 1998 and 2002 election, respectively. Because the preference votes were only introduced in 1998 and as only candidates elected via comparison numbers can be borderline elected, the 1991 number is somewhat larger. Figure 10 shows the corresponding number of candidates at each  $rank^*$  in the range  $-20 \leq rank^* \leq 3$ , but summed over all three elections. The reason why the number of observations decreases to the left of the borderline elected (at  $rank^* = 0$ ) is that some groups lack a sufficiently large number of defeated candidates to assign low values of  $rank^*$  to anyone.<sup>49</sup> Analogously, the main reason why the number of observations decreases to the right of the borderline elected is that many parties (and hence groups)

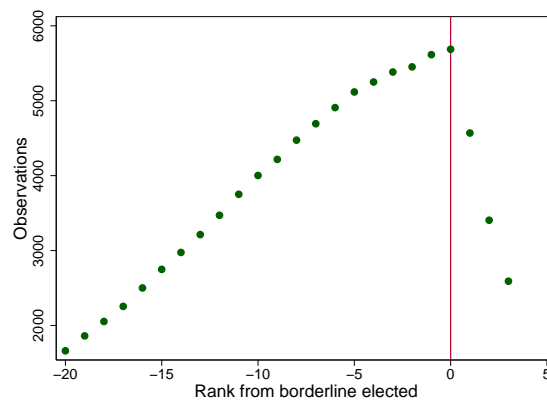
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<sup>48</sup>These error events can be calculated using an indicator contained in the data for whether or not a candidate was elected.

<sup>49</sup>Note that with the largest bandwidth used in the paper, the sample is restricted to  $rank^* \geq -10$ .

only have a few seats in the council, so that being ranked several positions higher than the borderline elected is not possible. Compared to the lack of a sufficiently large number of defeated candidates, it is much more likely that the lack of candidates ranked higher than the borderline elected that follows from being a small party is systematically related to the outcome. This is the rationale for estimating the direct effect of *rank*\* using additional defeated candidates rather than additional elected candidates.

Figure 10: Number of observations by rank from borderline elected



Source: Statistics Sweden & The Swedish Election Authority.

## Descriptive statistics

Table 11: Descriptive statistics of outcome variables for candidates in the borderline groups with  $rank^* = \{-2, -1, 0\}$

	Short run: 1–3 years/1 election later				
	mean	std.dev	min	max	obs
Average disposable income	1358.7	597.4	361.3	4059.3	16673
Run for municipal council	0.70	0.46	0	1	16754
Elected into municipal council	0.29	0.45	0	1	16754
Run for national parliament	0.11	0.32	0	1	16754
	Medium run: 6–8 years/2 elections later				
	mean	std.dev	min	max	obs
Average disposable income	1477.7	736.5	377	5163.3	10915
Run for municipal council	0.51	0.50	0	1	11208
Elected into municipal council	0.20	0.40	0	1	11208
Run for national parliament	0.077	0.27	0	1	11208
	Long run: 13–15 years/4 elections later				
	mean	std.dev	min	max	obs
Average disposable income	1764.1	925.6	436.3	6099	5283
Run for municipal council	0.31	0.46	0	1	5710
Elected into municipal council	0.11	0.31	0	1	5710
Run for national parliament	0.053	0.22	0	1	5710

*Note:* The sample for short-run outcomes includes borderline groups from the 1991, 1998 and 2002 elections, the sample for medium-run outcomes includes borderline groups from the 1991 and 1998 elections and the sample for long-run outcomes includes borderline groups from the 1991 election. Income is measured in 100 SEK deflated to 2000 year values (6.50 SEK $\approx$ 1 USD), all other variables are indicator variables.

*Source:* Statistics Sweden & The Swedish Election Authority.

## Placebo estimates

Table 12: Placebo estimates on the probability of being elected in future elections to a municipal council

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected <sup>placebo</sup>	0.0196* (0.0117)	0.0264** (0.0118)	0.00949 (0.0138)	0.0230* (0.0132)	0.0263 (0.0167)	-0.0120 (0.0150)
rank*	0.0469*** (0.00664)	0.0209*** (0.00691)	0.0326*** (0.00767)	0.0108 (0.00756)	-0.0000506 (0.00905)	0.0120 (0.00870)
Observations	16450	16085	10958	10633	5498	5268
Cut-off at rank*:	-1	-2	-1	-2	-1	-2
Individual controls	yes	yes	yes	yes	yes	yes

*Note:* The table reports placebo estimates of being elected into a municipal council on the probability of being elected in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to a municipal council. All regressions include individual controls (cf. Table 4). Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

Table 13: Placebo estimates on the probability of being nominated in future elections to the national parliament

	Election $t+1$		Election $t+2$		Election $t+4$	
	(1)	(2)	(3)	(4)	(5)	(6)
elected <sup>placebo</sup>	0.000257 (0.00814)	0.000233 (0.00777)	0.0151 (0.0100)	-0.00791 (0.00958)	0.0193* (0.0113)	-0.00506 (0.0100)
rank*	0.0169*** (0.00477)	0.0170*** (0.00441)	-0.000325 (0.00554)	0.00779 (0.00538)	-0.000347 (0.00617)	0.00451 (0.00549)
Observations	16450	16085	10958	10633	5498	5268
Cut-off at rank*:	-1	-2	-1	-2	-1	-2
Individual controls	yes	yes	yes	yes	yes	yes

*Note:* The table reports placebo estimates of being elected into a municipal council on the probability of being nominated in the first (columns 1–2), second (columns 3–4) and fourth (columns 5–6) subsequent election to the national parliament. All regressions include individual controls (cf. Table 7). Standard errors clustered on municipality are in parentheses. \*\*\*, \*\* and \* denote significance at the 1%, 5% and 10% level, respectively.

## 2011

- 2011/1, **Oppedisano, V; Turati, G.:** "What are the causes of educational inequalities and of their evolution over time in Europe? Evidence from PISA"
- 2011/2, **Dahlberg, M; Edmark, K; Lundqvist, H.:** "Ethnic diversity and preferences for redistribution "
- 2011/3, **Canova, L.; Vaglio, A.:** "Why do educated mothers matter? A model of parental help"
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- 2011/7, **Duch, N.; García-Estévez, J.:** "Do universities affect firms' location decisions? Evidence from Spain"
- 2011/8, **Dahlberg, M.; Mörk, E.:** "Is there an election cycle in public employment? Separating time effects from election year effects"
- 2011/9, **Costas-Pérez, E.; Solé-Ollé, A.; Sorribas-Navarro, P.:** "Corruption scandals, press reporting, and accountability. Evidence from Spanish mayors"
- 2011/10, **Choi, A.; Calero, J.; Escardíbul, J.O.:** "Hell to touch the sky? private tutoring and academic achievement in Korea"
- 2011/11, **Mira Godinho, M.; Cartaxo, R.:** "University patenting, licensing and technology transfer: how organizational context and available resources determine performance"
- 2011/12, **Duch-Brown, N.; García-Quevedo, J.; Montolio, D.:** "The link between public support and private R&D effort: What is the optimal subsidy?"
- 2011/13, **Breuilé, M.L.; Duran-Vigneron, P.; Samson, A.L.:** "To assemble to resemble? A study of tax disparities among French municipalities"
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- 2011/15, **Montolio, D.; Trillas, F.:** "Regulatory federalism and industrial policy in broadband telecommunications"
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- 2011/22, **García-Quevedo, J.; Mas-Verdú, F.; Montolio, D.:** "What type of innovative firms acquire knowledge intensive services and from which suppliers?"
- 2011/23, **Banal-Estañol, A.; Macho-Stadler, I.; Pérez-Castrillo, D.:** "Research output from university-industry collaborative projects"
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- 2011/26, **Caruso, R.; Costa, J.; Ricciuti, R.:** "The probability of military rule in Africa, 1970-2007"
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- 2011/30, **Montolio, D; Piolatto, A.:** "Financing public education when altruistic agents have retirement concerns"
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- 2011/33, **Pedraja, F.; Cordero, J.M.:** "Analysis of alternative proposals to reform the Spanish intergovernmental transfer system for municipalities"
- 2011/34, **Jofre-Monseny, J.; Sorribas-Navarro, P.; Vázquez-Grenno, J.:** "Welfare spending and ethnic heterogeneity: evidence from a massive immigration wave"
- 2011/35, **Lyytikäinen, T.:** "Tax competition among local governments: evidence from a property tax reform in Finland"
- 2011/36, **Brühlhart, M.; Schmidheiny, K.:** "Estimating the Rivalness of State-Level Inward FDI"

- 2011/37, **García-Pérez, J.I.; Hidalgo-Hidalgo, M.; Robles-Zurita, J.A.:** "Does grade retention affect achievement? Some evidence from Pisa"
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- 2011/44, **Turati, G.; Montolio, D.; Piacenza, M.:** "Fiscal decentralisation, private school funding, and students' achievements. A tale from two Roman catholic countries"

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- 2012/1, **Montolio, D.; Trujillo, E.:** "What drives investment in telecommunications? The role of regulation, firms' internationalization and market knowledge"
- 2012/2, **Giesen, K.; Suedekum, J.:** "The size distribution across all "cities": a unifying approach"
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- 2012/5, **Durán-Cabré, J.M.; Esteller-Moré, A.; Salvadori, L.:** "Empirical evidence on horizontal competition in tax enforcement"
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- 2012/29, Buonanno, P.; Durante, R.; Prarolo, G.; Vanin, P.:** "Poor institutions, rich mines: resource curse and the origins of the Sicilian mafia"
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**2013**

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- 2013/1, Sánchez-Vidal, M.; González-Val, R.; Viladecans-Marsal, E.:** "Sequential city growth in the US: does age matter?"
- 2013/2, Hortas Rico, M.:** "Sprawl, blight and the role of urban containment policies. Evidence from US cities"
- 2013/3, Lampón, J.F.; Cabanelas-Lorenzo, P.; Lago-Peñas, S.:** "Why firms relocate their production overseas? The answer lies inside: corporate, logistic and technological determinants"
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- 2013/11, Segarra, A.; García-Quevedo, J.; Teruel, M.:** "Financial constraints and the failure of innovation projects"
- 2013/12, Jerrim, J.; Choi, A.:** "The mathematics skills of school children: How does England compare to the high performing East Asian jurisdictions?"
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