Dynastic Political Rents?

Economic Benefits to Relatives of Top Politicians

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March 30, 2016

Abstract

We exploit close elections between the right and left bloc in Swedish municipalities to test whether children or siblings of politicians who enter into a top executive position obtain economic benefits. On a restricted sample of relatives who live in the same municipality as their top-politician relative, we find no benefits for the siblings of leading politicians, but sizeable income gains for children: a parent appointed to the top political executive raises children's average earnings by about 15 percent. These higher incomes accrue to children at the lower end of the earnings distribution and amount to an average of 10 percent of a full-time median wage. Exploiting administrative information on the children's occupational and residence status, we find that the higher earnings are unlikely to be rooted in an illegitimate allocation of jobs. But there is suggestive evidence that children of parents who win an election are more likely to postpone the start of their tertiary education and instead remain in the municipality to work.

Keywords: Reward to politics; family ties; rent extraction; local politics

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1. Introduction

In this paper, we investigate if close relatives to top political executives receive economic benefits. We call these benefits *dynastic political rents* and study their existence in Sweden, a political environment with one the lowest corruption levels in the world (see transparency.org, Wångmar 2013, Dahlström and Sundell 2014). If close relatives benefit economically in Sweden, such benefits are likely to arise in environments with less transparent institutions.

Anecdotal evidence suggests that economic benefits can trickle down through channels in legal gray-zones, which only indirectly relate to the politicians themselves. Local governments in Sweden employ 20% of the country's total labor force and allocate large sums via public procurement contracts. Relatives to politicians in power may be better informed about contract and employment opportunities, and bureaucrats who allocate jobs and contracts may have incentives to favor the relatives of their political superiors. In recent years, Swedish media has reported scandals of allocating publicly funded "summer jobs" to the children of insiders. A recent government inquiry found that the public procurement system lacked adequate oversight (SOU 2013), and investigative journalists have uncovered cases of questionable allocations of contracts to companies owned by relatives to top local politicians.[2]

We add to the insights from recent studies on dynastic political rents (Fafchamps and Labonne 2014, Gagliarducci and Manacorda 2015, Amore et al. 2015), in terms of both data quality and estimation methods. We use register data that is based on personal identification codes and automatically link children and parents at birth. This sidesteps the need to approximate family ties with shared last names. We have access to twenty years of panel data for the universe of municipal politicians, and every one of their children and siblings. These data allow us to differentiate between different sources of income and different employment sectors and jobs, e.g., to test separately for effects on earnings from small businesses and public-sector jobs. We can also test for impacts on the behavior of relatives, most importantly – it turns out – the priorities of children between tertiary education and job opportunities in their top-political parent's municipality. Apart from capturing a wide variety of economic benefits and behaviors, the register data we use are of uniquely good quality, as they are collected by the government via tax returns and mandatory employer surveys.

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[2] For example, investigative journalists in the program Uppdrag Granskning, Kommungranskarna (*Municipal Reviewers*) on Swedish State Television have uncovered public procurement contracts given to a small business owned by a brother-in-law of the local mayor, bypassing the rules of offering the contract in an open bidding process. The mayor argued that this large contract was not nepotistic, because the husband-in-law had had many previous contracts and handled them well (available on YouTube: https://www.youtube.com/watch?v=FmlRUQsEQRw).

Our empirical strategy differs from some of the previous work. Amore et al. (2015) also rely on detailed administrative data, although for Denmark. However, they identify the effect of larger political power from a set of municipality mergers, comparing re-elected politicians in merged (larger) municipalities with re-elected politicians in non-merged (smaller) municipalities. It is not entirely clear how to interpret and extrapolate this kind of treatment effect. Our approach instead relies on close elections in Swedish municipalities allocating positions of power to top-ranked politicians in the party that appoints the top local executive. We compare the economic outcomes for children and siblings of close electoral winners to those of close electoral losers, i.e., top-opposition politicians. Such a treatment effect of political power is more readily interpretable, and more easily extended to other contexts, than one that rests upon the relative size of the local assembly.

Using municipal-level data gives us a large number of observations without sacrificing neither the economic importance of the position nor the potential to extrapolate lessons outside of Sweden. Like elsewhere, Sweden's local political assemblies have substantial economic and political powers, and these are largely concentrated in top political executives (Jonsson 2003, Montin 2007). The municipal-council boards, each led by a mayor, hold employer responsibilities for local public employees amounting to more than a fifth of total employment and are in charge of public spending amounting to about a fifth of Swedish GDP. Among these expenditures, public procurement accounts for between 10-15 percent (Swedish Competition Authority 2015).

Our analysis uncovers several new and interesting results. We find that the relatives of top ranked politicians in the local government obtain higher average earnings than relatives of top ranked politicians in the opposition in the period after a close election. Our identification strategy suggests that this is a causal effect, rather than a correlation based on pre-existing differences. When we examine this effect in depth, it seems to be concentrated to relatives at the lower end of the income distribution. It also appears that the politician's children, rather than their siblings, experience the largest increase in earnings. Further, the children of the highest-ranked politician make the largest gains, conditional on living in the same municipality as their parent.

Looking for the mechanisms behind the children's additional earnings, we find little evidence that they run through shady channels. There is no effect on the probability of holding a job in the municipality's public sector. Neither is there evidence that the children take over the job, e.g., at the family farm, held by their parent before taking up full-time political office. We also analyze children's behavior, in terms of study vs. work, and their municipality of residence. This evidence suggests that children to powerful politicians become more likely to live in the municipality where their parent holds power, but less likely to be students. A possible explanation for the higher earnings found in

the baseline estimation is thus that children delay their entry into university and instead look for a job in their home municipality, when their parent is awarded the top political office.

The finding that electoral victories of politicians may shape the behavior and choices of their relatives – e.g., regarding residence – provides an interesting perspective on the small but growing body of research on dynastic political rents. For example, if family ties are inferred from shared last names between politicians and residents in the same geographical unit, geographical sorting of relatives will bias the results. Shifts in average earnings or the occupation distribution may, as in our case, stem from mobility responses among relatives, rather from illegitimate favors extended to pre-existing relatives in the same region.

We contribute not only to the small literature on dynastic political rents, but also to the wider literature on the roots and consequences of dynastic links. As for the consequences, a sizeable literature following Fisman (2001) investigates the value to firms of having political connections, often by conducting event studies of stock prices. A smaller number of papers examine if dynastic links between politicians generate differences in economic and social outcomes (Labonne et al. 2015, Braganca et al. 2015), and a growing body of work debates when relatives to politicians gain a lower entry barrier into political office (Dal Bó et al. 2009, Querubin 2013, Van Coppenolle 2015, Rossi 2015, Fiva and Smith 2015).

Another strand of research, which we also contribute to, examines the monetary returns from holding political office. A standard finding in this literature is that such returns exist for political offices at the national and state levels (see e.g., Eggers and Hainmueller 2009, Kotakorpi et al. 2013, and Fisman et al. 2014). The evidence is more mixed at the local level (Amore et al. 2015, Lundquist 2013 and Kotakorpi et al. 2013). We contribute to this literature by examining if monetary payoffs could also extend to the immediate relatives of politicians, extending the cost-benefit calculus of holding political office to advantages that may trickle down to the politician's loved ones.

The paper is organized as follows. Section 2 gives some basic institutional information about Swedish municipalities. Section 3 presents our micro data, sample restrictions, outcome variables, as well as some summary statistics. Section 4 explains the estimation strategy. Section 5 presents the baseline results. Robustness checks and discussions of channels behind the results appear in Section 6. Section 7 concludes.

2. Municipal Assemblies and Politicians in Sweden

Sweden has 290 municipal assemblies that vary in size between 31 and 101 seats, depending on the municipality's population (the median municipality has about 15 000 inhabitants). Municipal

assemblies have substantial political and economic powers. Their right to local self-government is granted by the Swedish Instrument of Government, and under the 1991 Local Government Act 2.1 local authorities are themselves responsible for matters of public interests, which relate to the municipal council and its inhabitants and are not the exclusive responsibility of the state. These responsibilities cover broad areas of public spending like child and elderly care, schools, and local infrastructure.

Electoral institutions across all municipalities have list-based Proportional Representation (PR) elections for the council every four years. Voters chose between party electoral ballots and seats shares in the municipal assembly are awarded to reflect the party vote shares.[3] This way, almost all municipalities have some representation of all parties in the national parliament and often of unaffiliated local parties (the latter hold 2 percent of the seats). After the election, a governing coalition is formed and the largest party in this coalition appoints the head of the council board. Sweden has two stable political blocs: the left, i.e., the Social Democrats, Left party, and Green party, and the center-right, i.e., the Conservatives, Center party, Liberal party, and Christian Democrats.[4] Both blocks strongly prefer within-bloc coalitions. Data from 1982-2010 reveal that when either bloc obtained more than 50% of the seats, the largest party in that bloc had a 90 percent probability of appointing the mayor.

Once a governing coalition has been formed, it appoints the most important executive positions, usually to the top politicians in the largest coalition party. The top executive positions include the chair of the Council Board, the chair of the council, and the chairs of subcommittees in different domains. The most powerful position in the municipality is the council-board chair: since this is the Swedish analog to mayor, we use this label throughout the paper. The political opposition usually appoints the vice chairs, with the most influential position usually going to the top-ranked person in the largest opposition party (see the descriptive statistics in Section 3).

Top politicians have disproportionate political influence compared to rank-and-file members of the municipal council. Since the establishment of the mayor's office in the 1960s, power has been increasingly concentrated in these offices and individuals (Nilsson 2001). Across municipalities, mayors have similar powers and obligations, most importantly to formulate and implement the

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[3] In 1998, Sweden also introduced the option of casting one preference vote per voter, for any candidate on their selected party ballot. This system has not affected party delegations in the municipal councils much, because two thirds of the voters do not exercise their option of preference voting, most of those who do cast their vote for candidates at the top of party ballots, and there is a high threshold for winning a seat by preference votes alone.

[4] There was also a short spree of representation for the populist center-right party New Democracy in the early 90s, and a very recent hike in seats (in particular at the national level) for an anti-immigration party, the Sweden Democrats. In this paper, we categorize both these parties as local.

agenda of the mayor's party and political coalition (Jonsson 2003). The mayor also represents the municipality in regional and national contexts. The second-most important positions are the subcommittee chairs, who handle the implementation of policies in their respective areas. The chairperson of the council also has some influence, but less than other executives.

3. Data, Sample Restrictions, and Descriptive Statistics

Data Our dataset covers the relatives of all municipal politicians, who were elected to a council in the six elections between 1991 and 2010. All politicians on all electoral ballots are linked to balanced panel data for their close relatives using personal ID codes. These codes are available for every politician, since parties are legally obliged to report to them to the electoral agency for every individual on the ballot. At birth, Swedish residents obtain a personal identification number, which is used for all government record keeping. For our purposes, we use data from the generational and sibling register to link every elected politician to his/her siblings and children. In a second step, we use balanced panel data, for each of the 21 years 1991-2011, registering the earnings, employment, municipality of residence, and other relevant variables for every relative.

None of the data used in this paper is thus self-reported. Parenthood is registered at birth by Swedish public hospitals. Earnings are third-party reported to the tax authorities by the employer. Other registers record everyone's municipality of employment and residence.

In the main analysis, we use three outcome variables for the earnings of the children and siblings of the politicians. Earnings are measured during the election period, excluding election years (more on this below). The first earnings variable, total earnings, sums employment earnings, business income, and those social insurance benefits directly based on wages and conditional on employment (such as parental-leave insurance or unemployment insurance). From this sum, we separate out the components of employment earnings (outcome variable number two) and business earnings (outcome variable number three). All earnings variables are deflated by the CPI.

The empirical analysis is done for earnings measured both in thousands of Swedish Kronor and in logs. The main reason for showing both sets of results is that the children of our politicians have quite low earnings. The log results can thus be misleading, as a small amount can translate into a large percentage deviation from a low average. By comparing estimates for monetary values and logs, we can better interpret the economic significance of our findings. In addition, the logged earnings variables are closer to normally distributed than the monetary values. The Online Appendix shows graphically (Figure W1) the frequency distributions for our main outcome variable, total

earnings, and their log, separately for children and siblings in the full sample. In the log transformation, we use (1+earnings) to avoid missing values.[5]

In the extended analysis, we add three more outcome variables. These are dummies for (i) holding public-sector employment, (ii) being a student, and (iii) living in the municipality of the relative who holds political office.

Sample restrictions   Our estimation sample consists of the children and siblings of working age – i.e., people between 18 and 64 years old. The relatively high age of the top politicians precludes us from looking at labor-market outcomes of their (mostly retired) parents. Spouses are excluded for two reasons. First, electoral victory causes non-random divorces. As we show elsewhere (Folke and Rickne, 2015), younger female politicians are more likely to divorce when they win political power, while the divorce probability for male politicians goes the other way. Comparing the wages earned by spouses of winning and losing politicians is hence confounded with sorting in and out of marriage which alters average wages of partners, because relatively high earners (spouses of the female winners) drop out of the winner sample (Folke and Rickne, 2016). Second, electoral victory may induce income and substitution effects on the spouse's labor supply.[6]

The sample for estimation includes only siblings and children of the top-three politicians on the electoral ballots of the largest parties in the left and right blocs. We use the top three politicians as these have the most political clout to potentially affect the earnings of their relatives. Rank-and-file politicians have less influence and, indeed, running our analysis on the relatives of the rank-and-file members of the municipal council does not yield any significant findings.

Another sample restriction is to drop observations of the election-year outcomes of relatives. In election years, politicians' (potential) rent extraction could be curtailed by heightened scrutiny from voters, other political parties, and the media.[7]

Descriptive Statistics   The total dataset consists of 23,895 individual election-period observations for the earnings of children and siblings of top politicians. Table 1 shows summary statistics for this sample, split by governing status (majority or opposition) of the relative in political office, as

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[5] The results are not sensitive to adding smaller amounts.
[6] Indeed, running our analysis for politician spouses gives a negative estimate on the "political rents" of a spouse winning political power. This is consistent with high-earning spouses (mainly husbands of female politicians) being more likely to drop out of the sample due to a divorce, making the lower-earning spouses (mainly wives of male politicians) over-represented in the sample of politicians in power compared to politicians out of power. Folke and Rickne (2016), find no cross-price effects on labor supply in the form of a reduction in market labor neither among wives nor among husbands of promoted politicians.
[7] See e.g., Persson and Tabellini (2000, Ch.4)

Table 1. Summary statistics for the children and siblings of elected politicians by electoral status, 1991-2011.

| | Electoral status of parent | | | |
| | In power | | In opposition | |
Panel A: Children	Top name	Top 2 and 3	Top name	Top 2 and 3
Age	27.07	27.28	27.50	27.57
All earnings*	149.05	149.66	151.73	150.26
Employed (1/0)**	0.91	0.90	0.91	0.90
Employment earnings	138.06	139.09	141.55	139.20
Business owner (1/0)	0.03	0.03	0.03	0.04
Business income	2.96	2.33	2.29	3.11
Capital income	-14.46	-6.99	17.75	-46.93
Student (1/0)	0.31	0.29	0.30	0.28
Lives in same municipality (1/0)	0.54	0.53	0.50	0.51
Years of education	13.31	13.19	13.32	13.08
Woman (1/0)	0.49	0.49	0.47	0.48
	Electoral status of sibling			
Panel B: Siblings	In power		In opposition	
	Top name	Top 2 and 3	Top name	Top 2 and 3
Age	49.51	48.17	49.95	48.29
Total earnings	236.83	221.21	230.17	208.96
Employed (1/0)	0.89	0.88	0.87	0.85
Labor earnings	222.22	205.05	217.15	192.75
Has business (1/0)	0.09	0.09	0.08	0.09
Business earnings	6.47	7.97	6.14	6.60
Capital income	162.39	10.36	23.24	26.03
Student	0.02	0.03	0.02	0.03
Lives in same municipality	0.33	0.36	0.30	0.31
Years of education	12.74	12.46	12.53	12.48
Woman (1/0)	0.49	0.49	0.49	0.49

* All earnings and income variables are reported in 100 SEK per year (10 SEK = 0.8 USD). ** Employment is defined as having a nonzero annual wage income.

well as by party rank (top name or top three). As expected, siblings are older than children, have higher earnings, and shorter education. About half of children and a third of siblings live in the municipality where their relative holds office. Interestingly, we see few differences in the average earnings of relatives to majority and opposition politicians. There are also few noticeable differences between the relatives of top names vs. second and third-ranked politicians. When we present our empirical results we also provide balance tests for personal traits and lagged outcomes. These tests are conducted for each specific sample we use in the empirical analysis, and all of them show balance in terms of both pre-determined traits and lagged outcomes.

In Table 2, we turn our attention to the politicians themselves. The average politician has 1.6 children (above 18 years of age) and 1.5 siblings. The differences in average age and education between majority and opposition politicians are small. The only stark difference concerns gender:

women are considerably less likely than men to be top ranked, but almost equally likely to be second- or third-ranked.

Table 2. Summary statistics for politicians by electoral status, 1991-2011.

	Majority politicians		Opposition politicians	
	Top name	Top 2 and 3	Top name	Top 2 and 3
# of children	1.68	1.61	1.66	1.61
# of children in home municipality	0.90	0.87	0.82	0.82
# of siblings	1.63	1.50	1.47	1.48
# of siblings in home municipality	0.55	0.54	0.44	0.46
Age	52.44	51.31	52.52	51.20
Years of education	13.30	13.15	13.26	13.15
Woman (1/0)	0.26	0.48	0.28	0.47
# of observations	2,979	5,742	3,004	5,847

The next set of descriptive statistics in Table 3 verify a key assumption in our empirical analysis by comparing earnings of relatives to top politicians in the party that appointed the mayor to relatives to the largest party in the opposition bloc. For example, if the Social Democrats (a left-bloc party) appoint the mayor, we call the center-right bloc party with the most seats the "opposition party". For our design to capture dynastic political rents to political power, ruling-party status should assign top executive posts to the top politicians on this party's ballot. Even if such appointments are standard procedure in a parliamentary system, they should be validated in data.

Table 3 relies on two waves of mandatory surveys to municipalities, following the 2006 and 2010 elections, to record the personal identification number of every individual appointed to an executive position. In our sample, we can thus compute the shares of the top-three politicians on the

Table 3. Percentage of top ranked and 2nd or 3rd rank politicians that hold executive positions, and the average yearly earnings among these office holders.

	Top politician		2nd and 3rd rank politicians		Average yearly earnings (1000s SEK)
	Ruling party	Opposition party	Ruling party	Opposition party	
Mayor (%)	86	0	4	0	539.2
Council Chair (%)	5	2	14	0	346.1
Subcommittee Chair (%)	27	1	35	3	429.7
Vice Mayor (%)	0	57	10	5	443.9
Council Vice Chair (%)	0	4	6	12	310.4
Subcommittee Vice Chair (%)	3	18	10	27	368.3

Note: This table is based on pooled data for appointments in 2007 and 2011, collected by Statistics Sweden in a mandatory survey to municipalities. 1000 SEK ≈ 120 USD.

electoral ballot of the ruling party and in the opposition party, who were indeed appointed to specific top positions. The table also lists average earnings by executive position. Reading the table, one should know that politicians can hold several positions at the same time, even though this is the exception rather than the rule.

The summary statistics in Table 3 show several things. First, when a party appoints the mayor, the probability is 86% that this is the individual at the very top of the ballot. Candidates ranked 2 or 3 on the ballot rarely became mayors (4%), but are more commonly appointed subcommittee chair (35%) or council chair (14%). For the largest party in the opposition bloc, the top-ranked person is appointed vice mayor in 57 cases out of 100. The means of total earnings in the rightmost column, show that political hierarchy is reflected in average earnings; the mayor earns the most followed by the vice mayor. Of the municipality executives, the mayor is usually the only one who gets a full-time political wage. (The mayor's annual wage ranks in the top two percentiles of the Swedish income distribution; authors' own calculations). Lower executive posts, such as subcommittee chairs, usually receive piece-rate compensation for their chair appointments, on top of the meeting compensation given to all committee members. Vice mayors usually get a political part-time wage. Most of the time, the lower office holders keep their regular job as a part-time occupation.

4. Empirical Strategy

Basic regression specification Our empirical analysis compares average earnings among relatives to top-ranked politicians in the largest majority party to average earnings among relatives to top-ranked politicians in the largest opposition party. This comparison can be done by in a basic regression specification:

$$Y_{i,t} = \beta P_{p,m,t} + \tau_t + \rho_m + \varepsilon_{i,t} \, , \tag{1}$$

where indexes *i* and *t* denote an individual relative *i* observed in election period *t*. The outcome variable, *Y*, will vary across applications but might, e.g., amount to income from the relative's own business. Our parameter of interest, β, captures the difference between relatives of leading politicians in the majority-bloc party compared to relatives of leading politicians in the largest opposition-bloc party. The key treatment variable, $P_{p,m,t}$, is a binary indicator that takes the value one for relatives of politicians in party *p* that are appointed mayor (council-board chair) in municipality *m* and election period *t*. The same indicator takes a value of zero for relatives to leaders of the largest opposition-bloc party. The specification also includes election-period fixed

effects, τ_t, and municipality fixed effects ρ_m. Because the treatment is decided at the level of the municipality and election period, standard errors are clustered at this level in all specifications.

Handling systematic selection by RDD A concern when estimating specification (1) is that politicians in the ruling party might differ systematically from those in the largest opposition party so that their relatives have different outcomes, irrespective of whether the politician is the mayor or the opposition leader. For example, Folke and Rickne (2016) find survey evidence that the position as mayor requires a great deal more work than that of opposition leader. Perhaps then, some politicians might postpone seeking the position of mayor until their children have grown up or left home. Another possibility is that the internal competition for the position of mayor in majority parties is greater than the competition for the position of opposition leader in minority parties. After all, the power and prestige of being a mayor, by far, exceed that of being an opposition leader. Fiercer internal competition for mayor could attract politicians whose children or siblings have personal traits that systematically differ from those of opposition leaders, and which could also carry over to their earnings.

This concern applies to situations where politicians can plan ahead in time to seek the position as mayor or opposition leader. A natural way to address the concerns would therefore be to use a Regression Discontinuity Design (RDD) to exogenously assign the mayor position by exploiting narrow elections. As already mentioned in Section 2, Swedish political parties indeed form two stable blocs that compete for power. An electoral victory (50 percent of the seats or more) for a bloc often allows the largest party in that bloc to appoint the mayor. Previous studies, such as Pettersson-Lidbom (2008), use left-wing seat majority as the treatment variable in a sharp RDD. It turns out, however, that this treatment variable does not deterministically assign majority-party status to the largest party in the left bloc. In about 10-15 percent of close elections, small parties form coalitions across bloc lines, leaving the largest party in the opposition. To amend this problem of systematic selection, one would need to exploit a fuzzy RDD.

Furthermore, not only the threshold for a left-bloc seat majority but also the threshold for a right-bloc majority influences who is in power. Each of these two thresholds changes the probability of the largest party in the bloc appointing the mayor by about 40% (see Folke and Rickne 2016). In principle, one could thus implement a fuzzy RDD around the two thresholds. In this particular setting, however, we would be left with too few observations to estimate the treatment effect of political power with reasonable statistical precision (more detailed information and estimation results are available from the authors).

An alternative empirical design However, we can use an alternative approach, which builds on the same intuition as an RDD, but buys more statistical precision at the cost of stronger identifying

assumptions. Specifically, we can select elections where the right bloc and the left bloc both receive close to 50 percent of the seats. We assume that the outcome of these elections is uncertain, such that the key political players in the largest parties of the bloc perceive a comparable chance of winning. By restricting our sample to close elections, we can meet the concerns about systematic selection.

In the restricted sample, we can more convincingly argue that our estimates of the treatment effect in equation (1) indeed uncover a true causal effect. To explain the intuition for this approach in a different way, we can make the analogy with estimating a treatment effect on a matched sample. When the propensity scores for treatment – i.e., for winning the election and appointing the mayor – are comparable for the largest parties in the two blocs, we avoid some or all of the selection problems that would plague a regular OLS estimate.

Defining "close" elections is not as straightforward in a PR electoral system with many parties as in a majoritarian system with two parties. We use a technique developed by Folke and Rickne (2016), which relies on simulation to measure the proportion of votes each bloc needs to win, or lose, to gain or lose the seat majority in the municipal assembly. The details of this technique are explained in the Online Appendix (Section W1), and we only give a brief graphical explanation here.

Although Sweden is often described as a stable political environment with Social Democratic dominance in the post-war period, this stereotype is a misleading when it comes to the six elections during our sample period, 1991-2011. These elections were very competitive: in two thirds of our municipality-election observations, either, or both, of the two blocs are within 10 percentage points of the vote share required to either win or lose the seat majority, and 44 percent of our observations are within a margin of 5 percentage points.[8] In this subsample with a 5 percent win margin or less, the ruling bloc changed from left to right, or vice versa, in 44 percent of all elections.

To illustrate, Figure 1 plots binned averages of the dummy variable that the largest party in a bloc appoints the mayor against the simulated value of how close – in terms of vote share – the same bloc was to win (or lose) the seat majority. Clearly, the electoral outcome becomes much more difficult to predict close to the vertical zero line, where the scatters converge. Thus, more competitive elections indeed have more uncertain outcomes. Nevertheless, the event that a bloc receives more than half the votes (and seats) discontinuously shifts up the probability that the largest party in that bloc appoints the mayor. For both blocs, this upward shift is of about the same size, namely 40 percentage points.

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[8] At the national level, the center-right coalition has held power in three of the eight most recent elections, and in five of the ten most recent.
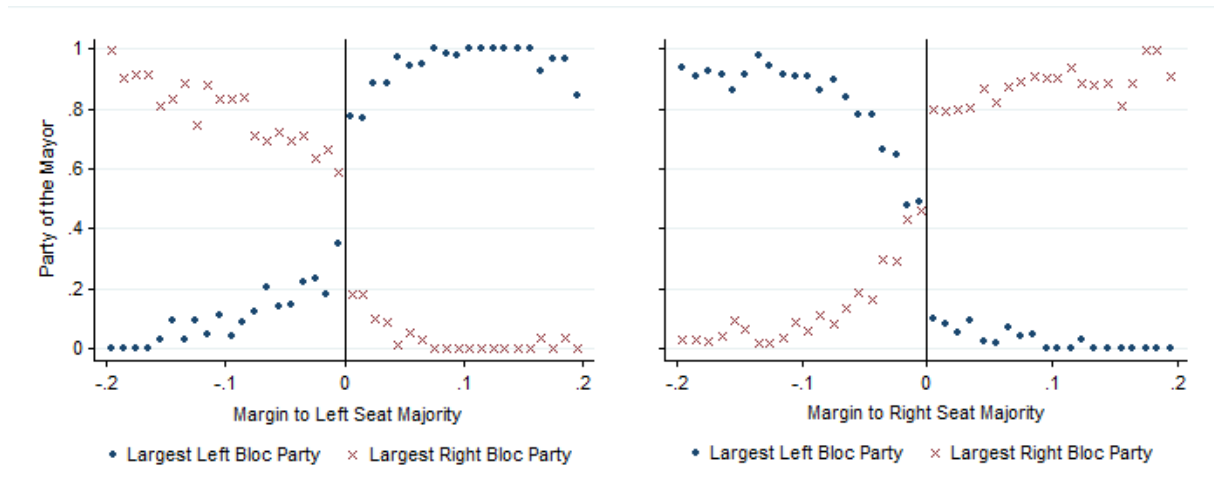
Figure 1. Probability that the largest party in a political bloc appoints the mayor, against the simulated margin to a seat majority for the left bloc (left-hand graph) and the center-right bloc (right-hand graph).

5. Baseline Results

To present our findings, we rely on a sequence of specifications, starting with the widest sample and outcome and then consecutively zooming in on samples and outcomes where we find clear evidence that political power has a clear positive effect on the earnings of relatives. The first set of estimates appears in Table 4.

These estimates pools siblings and children. The top panel (Panel A) shows the effects on all earnings of these relatives from the treatment of one's politician relative obtaining large executive powers. The two lower panels in Table 4 separate out employment earnings (Panel B) and business earnings (Panel C). For every earnings type and specification, we use both the log outcome variable (the upper row of estimates) and the corresponding amount in thousands of Swedish Kronor (the lower row). To help interpret the economic significance of the results, we also report mean earnings within the sample in the row underneath the number of observations.

In all three panels, the estimation sample is divided in two ways, by the identity of the powerful politician (top ranked, or top-three on the ballot), and by residence of the relative (anywhere in Sweden, or in the same municipality as the politician). Finally, for every outcome and sample, we report results for the full sample of elections and parties (the columns marked "All") and for elections and parties where the vote share of their bloc was within 5 percentage points from winning or losing the governing majority (the columns marked "5%").

Table 4. Effects on earnings of having a parent or sibling in the top tier of the largest governing party.

	Living in any municipality				Living in municipality of politician			
	Relatives to top-three politicians		Relatives to top-ranked politician		Relatives to top-three politicians		Relatives to top-ranked politician	
	All	5%	All	5%	All	5%	All	5%
Panel A. Total earnings								
Log Earnings								
Treatment	0.08***	0.06*	0.08*	0.06	0.11***	0.12**	0.16**	0.17*
	(0.02)	(0.04)	(0.04)	(0.06)	(0.04)	(0.06)	(0.06)	(0.10)
Earnings								
Treatment	6.16***	1.41	6.94	4.76	6.19**	6.69	8.92**	12.07
	(2.21)	(3.71)	(4.28)	(8.23)	(2.63)	(4.17)	(4.47)	(7.37)
Obs.	23,826	10,507	8,315	3,626	10,206	4,568	3,504	1,561
Mean dep var	190.07	193.77	196.23	199.85	162.34	165.13	166.08	164.49
Panel B. Employment earnings								
Log Earnings								
Treatment	0.09***	0.07*	0.06	0.05	0.12***	0.12**	0.14**	0.15
	(0.03)	(0.04)	(0.04)	(0.07)	(0.04)	(0.06)	(0.07)	(0.10)
Earnings								
Treatment	6.02***	1.32	5.96	3.55	6.49**	6.45	7.82*	9.59
	(2.21)	(3.71)	(4.35)	(8.29)	(2.61)	(4.15)	(4.49)	(7.37)
Obs.	23,826	10,507	8,315	3,626	10,206	4,568	3,504	1,561
Mean dep var	176.90	180.64	183.77	187.16	149.45	151.68	153.15	150.64
Panel C. Business earnings								
Log Earnings								
Treatment	-0.02	-0.01	0.02	0.01	-0.03	-0.04	-0.01	-0.04
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.04)	(0.06)
Earnings								
Treatment	0.56	-0.02	0.64	0.65	-0.61	0.28	-0.12	-0.07
	(0.68)	(0.78)	(0.78)	(1.19)	(0.85)	(1.40)	(1.25)	(1.79)
Obs.	23,596	10,387	8,254	3,596	10,104	4,516	3,481	1,551
Mean dep var	4.86	4.96	4.62	4.86	5.58	6.06	5.94	7.13

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

The estimates for the full sample of relatives (Panel A) suggest that dynastic political rents in Swedish local politics are large. The estimates indicate that relatives of the top-ranked politicians in the ruling party earn on average 7 percent, or 5,000 SEK, more per year than the relatives of top-

ranked opposition politicians. As we narrow the sample to close uncertain elections, however, the estimates are slightly smaller and no longer statistically different from zero.

Turning to the relatives who live in the same municipality as their related politician, most estimates become larger. On balance, the estimates now suggest large economic gains for close relatives of politicians with powerful political positions. For those related to the top-ranked politician, the estimates indicate 15 percent, or 8 000 SEK, higher earnings. While these estimated economic payoffs are quite large in relative terms, they are quite small in absolute terms. Although 8 000 SEK corresponds to an estimated increase of 15 percent, it amounts to only 3 percent of the median income of full-time workers in Sweden in the year 2000 (234 000 SEK). This indicates that the economic gains are mainly distributed to relatives at the lower end of the income distribution.

Examining separately the two different types of earnings, from employment and business ownership (Panels B and C), we see clearly that earnings from employment rather than business ownership drive our results. Thus, economic gains are unlikely to stem from corporate contracts or other benefits that could befall business-owning relatives. This is perhaps not surprising given that the gains accrue mainly to those at the bottom of the income distribution.

6. Robustness and Mechanisms

Robustness tests To make sure that our results capture a causal effect of political power on the earnings of relatives, we re-estimate all of the regressions from Table 4 replacing the outcome with the lagged dependent variable. Thus we take the average of the outcome variable in the previous election period, excluding the election year. If there are no pre-electoral differences in the outcome variable, as required by our identifying assumption, we should find no effect of our treatment on previous earnings once we focus on close elections. The results of this analysis appear in Table 5, which is structured in exactly the same way as in Table 4.

The results do not show any differences in lagged outcome variables for the specifications in the close election samples. In the Online Appendix (Table W1), we further test for balance between the treatment and control groups by running the same specifications with pre-determined characteristics (age and years of education) as dependent variables. The differences in age and years of education are small in magnitude and far from statistically significant, which provide further evidence that characteristics are indeed balanced across relatives of top-ranked politicians in the ruling and opposition parties. In the sample of close uncertain elections, we find no statistically significant differences in any pre-determined characteristic for any of the estimation samples. In fact, the differences are quite small even in the full sample.

Table 5. Effects on lagged outcomes

	Living in any municipality				Living in municipality of politician			
	Relatives to top-three politicians		Relatives to top-ranked politician		Relatives to top-three politicians		Relatives to top-ranked politician	
	All	5%	All	5%	All	5%	All	5%
Panel A								
DV: Log total earnings								
Treatment	0.06***	0.03	0.10**	0.10	0.06*	-0.01	0.10*	0.06
	(0.02)	(0.03)	(0.04)	(0.06)	(0.03)	(0.05)	(0.06)	(0.09)
DV: Total earnings								
Treatment	3.31*	-4.26	3.85	-1.15	1.24	-3.21	2.58	0.28
	(1.96)	(3.05)	(3.73)	(6.26)	(2.23)	(3.53)	(4.14)	(7.09)
Obs.	19,694	8,716	6,998	3,062	8,418	3,751	2,940	1,298
Mean DV	181.35	185.06	185.51	190.31	153.56	158.73	156.23	158.03
Panel B Employment earnings								
Log Earnings								
Treatment	0.09***	0.05	0.13***	0.10	0.12***	0.03	0.18***	0.07
	(0.03)	(0.04)	(0.04)	(0.07)	(0.04)	(0.06)	(0.07)	(0.09)
Earnings								
Treatment	3.86*	-3.71	4.30	-2.72	3.47	-1.17	4.56	-1.70
	(2.03)	(3.16)	(3.84)	(6.36)	(2.32)	(3.67)	(4.20)	(6.92)
Obs.	19,694	8,716	6,998	3,062	8,418	3,751	2,940	1,298
Mean DV	168.55	172.04	173.06	178.28	140.94	145.45	142.88	144.35
Panel C: Business earnings								
Log Earnings								
Treatment	-0.03**	-0.01	-0.02	-0.01	-0.08***	-0.06	-0.09**	-0.05
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.04)	(0.06)
Earnings								
Treatment	0.04	0.23	-0.30	1.42	-1.82**	-0.14	-2.00	2.24
	(0.62)	(0.78)	(0.91)	(1.57)	(0.87)	(1.46)	(1.73)	(3.23)
Obs.	19,694	8,716	6,998	3,062	8,418	3,751	2,940	1,298
Mean DV	5.27	5.40	5.42	4.98	5.85	6.45	6.84	7.24

In a third robustness check, we check whether our result hold up when a number of control variables are added to the regression. Importantly, the controls include a binary indicator for whether a party was the ruling party in the previous election. Robustness to this control is central for our identifying assumption. If close elections are indeed uncertain, our effects should not be driven by the party that appointed the mayor after the previous election. The other controls are particularly relevant for the parent-child estimation. Parents may choose to seek more demanding political offices once their children have reached a certain age or obtained a higher level of education. We therefore add

binary indicators for age and education categories of children, and siblings, in the appropriate regressions.[9] The results (Online Appendix, Table W2) show that once we limit ourselves to close, uncertain elections, the results are not sensitive to the controls. Like in the case of Table 5, the estimates illustrate the importance of limiting the estimates to close elections. The full-sample results are much more sensitive to the controls, which is exactly what we would expect if politicians are able to select into top positions by planning ahead in predictable electoral environments.

Altogether, these robustness tests strengthen our argument that the baseline estimates capture a treatment effect of political power, rather than a systematic difference in pre-determined characteristics between relatives of politicians in and out of power. In what follows, we run the same battery of tests for each table of results (Tables 6-8), with all robustness tables relegated to the Online Appendix in the interest of saving space.

Siblings versus children   Next, we split the sample by kinship relations. The results are shown in Table 6, but – given the results in Table 4 – only for relatives who live in the same municipality as the powerful politician. We also restrict the analysis to the measure of all earnings. The results for the children are presented in the left part of the table and those for siblings in the right part.

The results for children convey a clear message. Children of top-ranked politicians obtain a higher income when their parent's party wins power in the election. On average, their earnings go up by about 20 percent (16 000 SEK), relative to the earnings by children of opposition leaders – a result that holds up in the full sample as well as the sample of close, uncertain elections. To the extent that the child is still part of the leading politician's household, this estimate may be a lower bound on the pure earnings effect – if we treat the household as one unit, we would expect the labor supply of the child to fall as the winning politicians income goes up, due to an income effect as well as a substitution effect.[10] The estimated treatment effect corresponds to about a ten to fifteen percent of the average children earnings.

The estimates are less consistent when we include children of politicians in the top three of the political hierarchy. We also find no clear effect for the politicians' siblings, although the relatively large standard errors on the coefficients mean that the two sets of estimates – for children and siblings – are not significantly different from each other.

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[9] We control for age for a dummy for each ten year interval of age, i.e., under 20, 20-29, 30 to 39, etc. For education we include dummies for seven different level of education
[10] Cf. the discussion about spouses in Section 3.

Table 6. Effects on total earnings of having either a parent or a sibling in the top tier of the largest governing party.

	Children who live in the same municipality as the politician				Siblings who live in the same municipality as the politician			
	Relatives to top-three politicians		Relatives to top-ranked politician		Relatives to top-three politicians		Relatives to top-ranked politician	
	All	5%	All	5%	All	5%	All	5%
Log Earnings Treatment	0.09*	0.06	0.22***	0.22*	0.09	0.18*	0.01	0.02
	(0.05)	(0.07)	(0.08)	(0.13)	(0.06)	(0.09)	(0.11)	(0.14)
Earnings Treatment	3.34	1.05	16.25***	18.80**	6.99	10.43	-1.81	0.41
	(3.07)	(5.11)	(5.12)	(8.31)	(4.64)	(7.65)	(8.27)	(13.25)
Obs.	6,412	2,876	2,221	991	3,794	1,692	1,283	570
Mean dep var	135.94	137.15	135.13	133.06	206.97	212.69	219.64	219.13

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

The robustness tests for Table 6 are contained in the Online Appendix (Tables W3 and W4). They show that there is balance on pre-determined characteristics and lagged outcome of the children and siblings, and that the results are robust to the addition of control variables (the same ones as those listed above under *Robustness tests*).

Winning versus losing the position as mayor   Our baseline analysis jointly estimates the combination of two treatment effects: positive impacts on earnings by children of electoral winners, and negative impacts on earnings by children of electoral losers. Some of the politicians in our sample have previously been mayor while others have not, which means that some of the politicians in the "opposition" category are actually lose the top executive post. In other words, they fail to get re-selected because their party falls out of political power. Because we are interested in estimating the payoffs from a relative who *wins* office, we follow the approach suggested by Fafchamps and Labonne (2015) to separate out this particular effect.

We thus divide the relatives to top-ranked politicians into two subsamples, relatives of politicians who were never mayors (top ranked in the largest party in the governing majority in any

election period since 1982), and those who previously held this post. In the first subsample we estimate the effect of the relative becoming the mayor instead of becoming, or remaining, opposition leader. In the second sample, we estimate the effect of the relative remaining the mayor compared to losing the position.

Table 7 presents these results. They show that children of first-time mayors do benefit from their parents gaining political power, whereas children of incumbent mayors do not appear to get anything close to the same benefits. Thus the baseline findings seem to be driven by higher earnings when the parent becomes mayor for the first time, rather than remains at that post.

Table 7. Effects on all earnings from having a parent who is mayor, by parent's incumbency status

	Children				Siblings			
	Relatives of first-time chairs		Relatives of incumbent chairs		Relatives of first-time chairs		Relatives of incumbent chairs	
	All	5%	All	5%	All	5%	All	5%
Log Earnings								
Treatment	0.26***	0.40**	0.14	0.09	0.13	0.23	-0.02	0.02
	(0.10)	(0.17)	(0.19)	(0.20)	(0.13)	(0.21)	(0.18)	(0.19)
Earnings								
Treatment	15.74**	22.23**	6.78	7.70	7.00	19.16	-15.91	-21.24
	(6.13)	(10.07)	(16.30)	(22.19)	(9.73)	(18.50)	(14.46)	(22.74)
Obs.	1,617	698	604	293	924	399	359	171
Mean dep var	128.70	124.75	152.36	152.87	216.43	217.47	227.92	223.01

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

The baseline results are also unlikely to stem from negative impacts on the earnings of children of politicians who lose the chair position. For siblings, the point estimates have opposite signs for the two groups but none of the estimates are close to statistically significant. In the Online Appendix (Tables W5 and W6), our battery of robustness tests for lagged outcomes, balance of socioeconomic traits and sensitivity to control variables, corroborate the statistical soundness of the findings in Table 7.

Mechanisms To further explore which mechanisms drive the higher earnings of children to politicians in power, we consider four additional outcomes. Children of politicians may make more

money because they get government jobs through illegitimate channels or get opportunistically hired by local government actors. It is also possible that children of ruling politicians have better information about available jobs than children of opposition politicians. To test these ideas, we use a binary indicator for having a job in the public sector in the municipality where one's parent is a politician.

A second reason for higher earnings could be that the child steps in to fill a vacancy left by the parent when s/he becomes a full-time politician, for example at a family firm or a family farm. Our best proxy for this phenomenon is a binary indicator that takes a value of 1 when the child has the same 5-digit sector of employment as the one held by the parent in the year before becoming a full-time politician. If the child steps in to fill a vacancy left by the politician, they should be more likely to work in the same sector as the one their parent used to work in.

Children's benefits also may arise from changes in their own behavior. One such behavioral change is captured by a dummy for being a student: the child may choose to work rather than study depending on a higher expected relative return from working.[11] For these three prospective sources of higher earnings, our sample includes only children living in the same municipality as the politician-parent. Each of these outcome variables is measured as an average of the relevant binary indicator in the three years following their parent's promotion or non-promotion.

Another behavioral change concerns location itself. We capture this by a dummy for the child living in the same municipality as their top-politician parent: the higher political power of the parent may expand the labor-market opportunities in that municipality. To evaluate the importance of this variable, we have to include all children in the sample. Their location is measured three years after the election.

The estimates are presented in Table 8, with the results for being a municipal employee in Panel A, the results for being employed in the parent's previous employment sector in Panel B, the results for taking part in tertiary education in Panel C, and the results for children co-locating with their politician-parent in Panel D.[12] Tests of the identifying assumption using lagged outcome variables, and sensitivity tests adding controls, are found in the Online Appendix (Table W8).

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[11] Sweden's system of higher education has a universal qualifying exam with the vast majority of schools using only high school grades to guide admissions. It is thus implausible, if not impossible, that an influential local politician would be able to lower their children's entry threshold to an institute of higher learning.
[12] For completeness, the Online Appendix (Table W7) shows the corresponding table for siblings. We do not delve deeper into these results, though, because of the lack of a baseline effect on earnings in the sibling sample.

The results in Table 8 reject the hypothesis that children of mayors obtain rents because they are more likely than children of opposition leaders to get a job in the municipality's public sector. Nor do we find that they are more likely to work in their parent's previous employment sector. The negative estimates for this outcome variable suggest that, if anything, children of powerful politicians are less likely to work in the same sector as their parent did before entering political office.

Table 8. Effects on occupational status, tertiary studies, and geographical residence from having a parent or sibling in the top tier of the largest governing party.

	Children of top-three Politicians		Children of top-ranked politicians	
	All	5%	All	5%
Panel A. Municipal public sector employee				
Treatment	0.01	0.00	0.00	-0.04
	(0.01)	(0.02)	(0.02)	(0.03)
Obs.	6,412	2,876	2,221	991
Mean	0.26	0.24	0.26	0.26
Panel B. Employed in parent's pre-election employment sector				
Treatment	-0.01	-0.01	-0.03**	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
Obs.	6,115	2,739	2,139	957
Mean	0.10	0.11	0.12	0.14
Panel C. Being a student in tertiary education				
Treatment	-0.00	0.01	-0.04*	-0.03
	(0.01)	(0.02)	(0.02)	(0.03)
Obs.	6,412	2,876	2,221	991
Mean	0.29	0.30	0.30	0.31
Panel D. Living in the municipality of the parent politicians				
Treatment	0.03***	0.01	0.04***	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
Obs.	10,207	4,443	3,546	1,561
Mean	0.52	0.52	0.52	0.52

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

The point estimates in Panels C and D suggest that the baseline findings of higher earnings for children may possibly reflect changes in their own behavior. Children of mayors are 3-4 percentage points less likely to be students than children of opposition leaders, although the estimates are not significant in the close-election sample. They are also 2-4 percentage points more likely to live in the

same municipality as their parent than are children of opposition leaders – but again, this result is not significant in the close-election sample.

The robustness tests for Table 8 in the Online Appendix (Tables W8 and W9) show that the lagged outcomes of the children are balanced in the sample of close elections. However, the results for being a student are sensitive to including the full set of control variables (listed above under *Robustness tests*), which reinforces our qualification that these results are only suggestive of a possible mechanism.

Although the results are inconclusive regarding the exact channel behind the higher earnings for children of mayors, they provide clear evidence against a few possible channels. It is thus unlikely that higher earnings reflect rents due to a public-sector job. It is also unlikely that they reflect the children taking over their parents' pre-mayoral job.

Long term outcomes To extend our analysis of the earnings effects of a close relative in political office, we consider a longer time horizon than in the baseline analysis. Instead of examining the average of the outcome variable(s) in the three years following the relative's election, we now consider average outcomes two election periods later, i.e., 8 to 11 years after the initial shift in power. One caveat of this method is that it greatly reduces our sample size. Due to end-of sample time truncation, we can only use power shifts in the 1991-2002 elections. Another caveat is that closely losing politicians in our baseline specifications can gain office in a later period. Therefore, part of the control group is "treated" with a later electoral wins: this applies to about 20 percent of the close losers in our baseline estimation.

We examine two outcomes, total earnings and years of education. Our interest in years of education mirrors the earlier results on mechanisms. If the children of winning politicians opt to work in their home municipality for a few years, they may simply delay their university education to the proximate future. If so, we would not see any treatment effect on the amount of education cumulated two electoral periods later.

Table 9 presents the estimates, for earnings in the upper panel and education in the lower panel. They suggest that the hike in earnings extends to the long term: the children of mayors still make more than the children of opposition leaders 8-11 years out from the original appointment, even though a fifth of the original opposition leaders become mayors in later periods. The estimate retains statistical significance at the 10-percent level despite this dilution effect and the smaller sample, at least when examine the log of earnings.

The results for years of education do not support a permanent difference between children of mayors and opposition leaders. While the point estimate is still negative, it is small in size (only 0.18

years) and non-significant. It appears that the short-term impact on the probability of studying entails a postponed university education rather than an abandoned one.

Table 9. Effects on long-term earnings and years of education from having a parent or sibling in political power.

	Children of the top-three politicians		Children of the top-ranked politicians	
	All	5%	All	5%
Panel A. Total earnings				
Log Earnings				
Treatment	0.10**	0.06	0.13	0.18
	(0.05)	(0.07)	(0.08)	(0.15)
Earnings				
Treatment	10.46**	6.20	12.03	27.07*
	(4.54)	(8.13)	(8.02)	(15.88)
Obs.	4,269	1,850	1,479	638
Mean dep var				
Panel B. Years of education				
Treatment	0.14	0.13	0.02	-0.18
	(0.09)	(0.14)	(0.16)	(0.26)
Obs.	4,269	1,850	1,479	638
Mean dep var				

Notes: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

7. Conclusions

We analyze dynastic political rents in a developed and stable democracy. Drawing on uniquely comprehensive register data for siblings and children of politicians, our baseline findings uncover a positive and large effect on the earnings of children from their parent's ascension to the top position of local political power. We do not find any significant impact on earnings for the siblings of top political executives (although the noisy estimates for the latter renders the difference between children and siblings non-significant). The hike in dynastic political rents for children is quite robust, passing test for balance in pre-determined characteristics of parents that win and lose political power, and tests for addition of a number of control variables.

Our extended analysis uncovers suggestive results on how children of mayors behave relative to children of opposition leaders. These results suggest an interpretation of our baseline findings. Earnings only go up for the children who reside in the same municipality as the parent ascending to power. This may appear intuitive, in that the clout of municipal politicians is regionally concentrated to their administrative unit. But when we use the residence of children as an *outcome* variable, we

find that children sort geographically according to the electoral success of their parents. As indicated by the positive treatment effect on co-residence and the negative treatment effect on being a student, electoral victories appear to encourage children to remain at home for work rather than to leave the municipality for a university education. As indicated by the null effect on employment of children in the municipality's public sector, remaining in the municipality does not reflect better work opportunities in the sector under political control. Thus it is private employers who are more likely to hire the mayor's son or daughter – possibly at better conditions – consistent with the findings by Gagliarducci and Manacorda (2015).

The attraction to live in the municipality of an electorally successful parent could be viewed as a personal gain to parents from holding a top office (assuming they enjoy the company of their children). By revealed preference, location choices by children also indicate a benefit on their own behalf, quite possibly drawing on a higher status in the local community due to their parent's hold on political power. However, these rents do not reach very far, as they do not extend to children of politicians who belong to the party elite but do not occupy the unique position as mayor.

All in all, our study suggests that dynastic political rents are, at most, a marginal phenomenon in Sweden. This contrasts sharply to the findings for Italy by Gagliarducci and Manacorda (2015), who argue that jobs to the politician relatives make up 0.65 percent of private-sector employment. Our results also contrast to the findings for the Phillipines by Fafchamps and Labonne (2014), who argue that politicians staff local bureaucracies with their own relatives.

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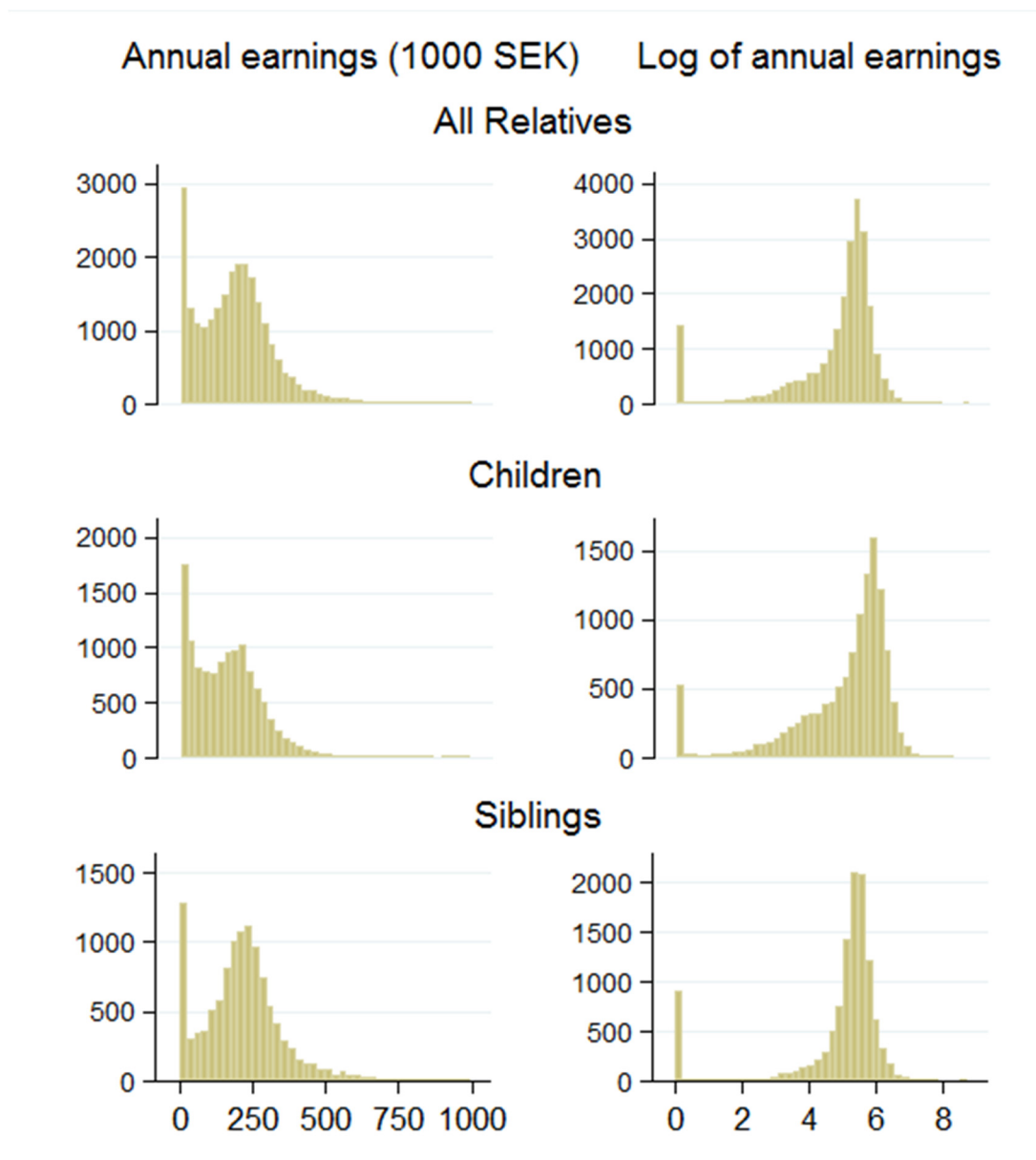
Online Appendix

Figure W1. Frequency distributions of total earnings.

Note: The distributions in the left column have been truncated at one million SEK in annual income. The number of truncated observations is 47 among the siblings (0.4% of the sibling sample) and 11 among the children (0. 1% of the child sample).

W1. Measuring the closeness of elections in a PR system

Measuring close elections in a PR system is a challenge as the vote share of a single party, or a bloc of parties, is not a deterministic function of the seat share. Instead, the distribution of seats to a party or a bloc is jointly determined by the allocation of votes to the party itself and to other parties. Thus, neither vote shares nor seat shares uniquely measure a close election.

To measure electoral closeness, we rely on a simulation-based approach developed in Folke and Rickne (2015). This approach constructs a forcing variable, which is continuous (rather than discrete, as the seat share) and which does not give sorting or a low density of observations close to the threshold of winning more than 50% of the seats. This simulated forcing variable takes account of two important features of the electoral system. The first feature is that multiple electoral districts of different size may exist within the same municipality, and the second that shifting a vote to or from one bloc to the remaining parties has a different impact on the seat share of the bloc winning or losing the vote depending on which party within the winning and losing bloc that won or lost it.

Our simulation departs from data on electoral outcomes. We want to measure how close the election is by capturing which shift of votes to or from a political bloc would have caused 1) a winning bloc to lose its seat majority, or 2) a losing bloc to gain a seat majority. In each election, we will thus have two forcing variable values, one for each bloc. When we measure closeness for a certain bloc, the other bloc always includes local parties.[13] The closeness variable is measured in percentage terms, answering the question of "which percentage of votes was needed, in a specific election, to give (or take) the seat majority from each of the two political blocs?"

For a bloc that won a seat majority, we start from the electoral result in the relevant election and move successively in the negative direction, incrementally removing 0.01 percentage points of the bloc's votes, starting from 0.01, 0.02, etc. For a losing bloc we do the opposite, adding small increments of votes. The goal is to find out, for each bloc at the time, how large a percentage of votes we need to shift in order to shift the seat majority across blocs.

How does our simulated shift in votes affect the distribution of seats? This impact will of course differ between countries depending on the electoral system. In the Swedish case, seats are distributed based on the Highest Averages Method, using the modified St. Lagué formula. After shifting a small proportion of votes either to or from a bloc, we use this formula to compute the new seat distribution. For each shift of votes, we randomly simulate 1 000 alternatives to how the votes shifted in terms of winning and losing parties and districts. Each time, we also compute the new

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[13] In the Swedish case, local parties - defined as not having representation in the parliament - hold on average 2% of the municipal assembly seats.

allocation of seats. In this simulation, we assume that large parties have a greater variance in their vote shares than small parties, but that the variance is not 100% proportional. The simulations also abstract from the fact that votes can shift between parties within a bloc.[14] Having computed the new seat allocation in each of the 1 000 shifts of the vote distribution, we tally the number of times that the bloc either lost (for winning blocs) or won (for losing blocs) the seat majority under the new distribution. Out of all the simulations for each shift in the vote share, we then set the value of the forcing variable to the size of the smallest vote shift that caused a shift in the bloc's majority status in at least 50% of the 1 000 vote shifts.

Figure W2 illustrates the process of creating the forcing variable for a specific municipality and election, the municipality of Upplands Väsby in 2006. In this municipality, the center-right bloc won the governing majority, receiving 52.7 percent of the votes and 54.9 percent of the seats. The left bloc won 42.9 percent of the votes and 43.1 percent of the seats. Suppose that we want the value of the forcing variable for the left bloc, i.e. the minimum proportion of votes that the bloc would need to win to gain the seat majority. The *x*-axis in the figure shows the proportion of votes shifted and the *y*-axis shows the proportion of times, out of our 1 000 simulated vote shifts, that caused the left bloc to win 50% of the seats or more. The upward slope of the line indicates that the larger proportion of votes shifted to the bloc, the greater the probability of a 50% seat shift. As illustrated by the vertical line, the left bloc gains a seat majority in about half of the simulations when we give it an additional 5.0 percentage points of the votes. This assigns the value of the forcing variable to 5.0 percent for the left bloc in this election.

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[14] In detail, the simulations are carried out as follows. First, we take a random number between 0 and 1 for each party in the giving and receiving blocks. We then multiply this random proportion with the party's vote share plus a constant of 0.1. For a party with a random shock of 0.4 and a 20% vote share we get a number of 0.4*(0.2+0.1). We can call this variable q. Within each block, we then normalize the parties' q values so that they sum to 1, calculating $q_w = \frac{q_p}{\Sigma_1^p q_p}$ where q_p are the initially computed party shocks and q_w are the normalized shocks. The next stage is to subtract fractions of the vote shift, for example 0.01 percentage points of the total votes, from one block and reward it in fractions to the other parties in a way that corresponds to the randomly drawn shocks. Finally, a new vote allocation is used to calculate the seat allocation, using the Swedish election formula.
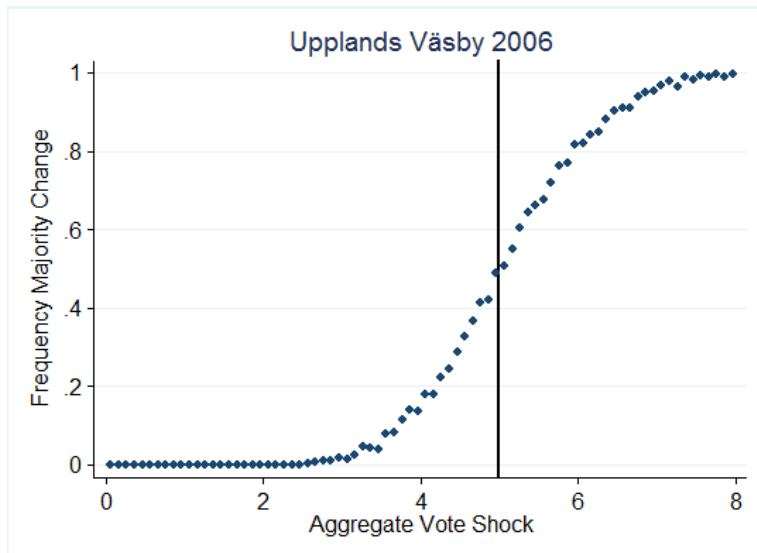
Figure W2. Proportion of seat majority shifts to the left bloc in Upplands Väsby municipality in the 2006 election (y-axis) depending on 1,000 simulations of shifting a certain proportion of votes (x-axis) from the center-right to the left bloc.

A general concern with RDDs is that the density of the forcing variable is not smooth across the threshold. A higher density of observation on either side of the seat threshold indicates that treatment is not random, or that the forcing variable is wrongly specified in some way. In Figure W3, we show that this is not the case for our analysis. For both forcing variables, the frequency of observations is smooth as we cross the seat-majority threshold.
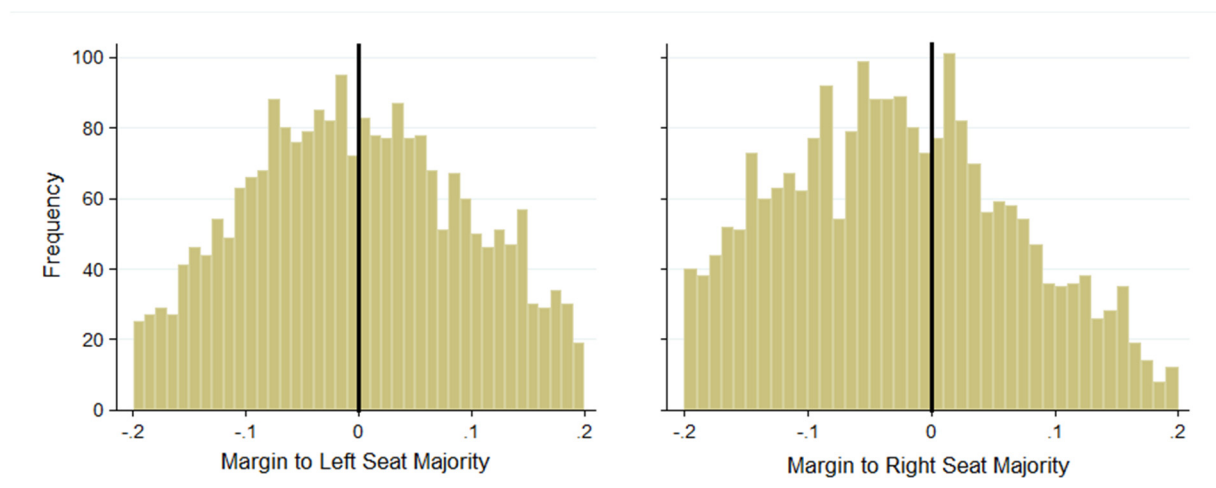
Figure W3. Frequency of observations, as a function of the margin to a seat majority for the left bloc (left hand graph) and the right bloc (right hand graph). Each bar corresponds to 0.01 units of the margin to the seat majority.

W2. Tables for Robustness Analysis

Table W1. Effect of having a relative in the ruling party on pre-determined characteristics.

	Relatives in all municipalities				Relatives in the same municipality as the politician			
	Relatives to top-three politicians		Relatives to top-ranked politicians		Relatives to top-three politicians		Relatives to top-ranked politicians	
	All	5%	All	5%	All	5%	All	5%
Panel A: Years of Education								
Treatment	0.06*	0.01	0.04	-0.00	0.03	0.01	-0.01	0.00
	(0.04)	(0.06)	(0.06)	(0.10)	(0.05)	(0.08)	(0.09)	(0.14)
Obs.	23,448	10,313	8,171	3,578	10,020	4,465	3,431	1,539
Mean	12.90	12.94	13.02	13.06	12.25	12.30	12.41	12.40
Panel B: Age								
Treatment	-0.06	-0.19	0.17	0.11	0.31	0.25	0.77	0.33
	(0.18)	(0.25)	(0.30)	(0.45)	(0.27)	(0.40)	(0.48)	(0.75)
Obs.	23,448	10,313	8,171	3,578	10,020	4,465	3,431	1,539
Mean	37.64	37.44	38.08	37.82	33.88	33.77	34.28	34.10

Note: Robust standard errors clustered at the level of the municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

Table W2. Effects on earnings of having a parent or sibling in the top tier of the largest governing party, including control variables.

	Living in any municipality				Living in the same municipality as the politician			
	Relatives to top-three politicians		Relatives to top-ranked politicians		Relatives to top-three politicians		Relatives to top-ranked politicians	
	All	5%	All	5%	All	5%	All	5%
Panel A								
DV: Log total earnings								
Treatment	0.03	0.04	0.07*	0.08	0.03	0.06	0.11*	0.12
	(0.02)	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)	(0.09)
DV: Total earnings								
Treatment	3.41	0.31	7.71	6.45	0.65	1.54	5.56	7.61
	(2.39)	(3.63)	(4.72)	(7.97)	(2.53)	(3.72)	(4.04)	(6.22)
Obs.	22,863	10,082	8,145	3,570	9,828	4,397	3,435	1,542
Panel B								
Log Earnings	0.04	0.04	0.06	0.07	0.06	0.06	0.10	0.11
	(0.03)	(0.04)	(0.04)	(0.06)	(0.04)	(0.06)	(0.07)	(0.09)
Earnings								
Treatment	3.39	0.13	7.02	5.20	1.50	1.56	4.46	5.22
	(2.39)	(3.63)	(4.82)	(8.09)	(2.60)	(3.86)	(4.31)	(6.51)
Obs.	22,863	10,082	8,145	3,570	9,828	4,397	3,435	1,542
Panel C: Business income								
Log Earnings	-0.01	0.00	0.01	0.02	-0.04	-0.03	-0.02	-0.03
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.06)
Earnings								
Treatment	0.28	0.02	0.21	0.59	-1.01	-0.08	-0.37	-0.03
	(0.61)	(0.75)	(0.79)	(1.16)	(0.87)	(1.33)	(1.37)	(1.95)
Obs.	22,863	10,082	8,145	3,570	9,828	4,397	3,435	1,542

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less. Control variables include the lag of the binary indicator for being the ruling party, dummy variables for ten-year age intervals (-20, 20-29, 30 to 39, etc.), and dummy variables for seven levels of educational attainment.

Table W3 Effects on lagged earning and on pre-determined characteristics of having a parent or sibling in the top tier of the largest governing party.

	Children who live in the same municipality as the politician				Siblings who live in the same municipality as the politician			
	Relatives to top-three politicians		Relatives to top-ranked politicians		Relatives to top-three politicians		Relatives to top-ranked politicians	
	All	5%	All	5%	All	5%	All	5%
Panel A: All earnings								
Log Earnings								
Treatment	0.05	0.04	0.14	0.23	0.10*	-0.03	0.09	-0.11
	(0.05)	(0.08)	(0.09)	(0.15)	(0.06)	(0.09)	(0.12)	(0.16)
Earnings								
Treatment	0.62	-0.97	5.98	9.30	6.38	-2.19	1.97	-6.54
	(3.25)	(5.44)	(6.22)	(12.25)	(4.26)	(6.79)	(8.22)	(12.58)
Obs.	4,977	2,214	1,729	763	3,441	1,537	1,211	535
Mean	6.38	6.42	6.37	6.39	6.99	7.05	7.05	7.12
Panel B: Years of Education								
Treatment	0.08	-0.02	-0.07	-0.19	0.04	0.11	0.18	0.39
	(0.06)	(0.09)	(0.11)	(0.18)	(0.10)	(0.16)	(0.17)	(0.28)
Obs.	6,412	2,876	2,221	991	3,794	1,692	1,283	570
Panel C: Age								
Treatment	0.07	-0.22	0.52	0.51	0.52	1.22*	0.44	0.16
	(0.19)	(0.30)	(0.32)	(0.52)	(0.40)	(0.65)	(0.66)	(1.04)
Obs.	6,412	2,876	2,221	991	3,794	1,692	1,283	570

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

Table W4. Effects on earnings of having a parent or sibling in the top tier of the largest governing party. Including control variables.

	Children who live in the same municipality as the politician				Siblings who live in the same municipality as the politician			
	Relatives to top-three politicians		Relatives to top-ranked politicians		Relatives to top-three politicians		Relatives to top-ranked politicians	
	All	5%	All	5%	All	5%	All	5%
Log Earnings								
Treatment	0.00	0.02	0.13*	0.20*	0.09	0.14	0.03	-0.02
	(0.04)	(0.07)	(0.07)	(0.12)	(0.06)	(0.09)	(0.10)	(0.13)
Earnings								
Treatment	-1.83	-0.87	10.45**	14.79**	4.05	2.61	-1.69	-4.39
	(2.86)	(4.44)	(4.52)	(7.06)	(5.07)	(7.88)	(7.91)	(12.60)
Obs.	6,228	2,790	2,185	980	3,600	1,607	1,250	562

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less. Control variables include the lag of the binary indicator for being the ruling party, dummy variables for ten-year age intervals (-20, 20-29, 30 to 39, etc.), and dummy variables for seven levels of educational attainment.

Table W5 Effects on all earnings from having a parent who is Mayor, sample divided by the parent's "incumbency" status

	Children of politician				Siblings of politician			
	Relatives to first-time chairs		Relatives to incumbent chairs		Relatives to first-time chairs		Relatives to incumbent chairs	
	All	5%	All	5%	All	5%	All	5%
Panel A: All earnings								
Log Earnings								
Treatment	0.14	0.19	-0.06	-0.02	0.15	-0.06	-0.10	-0.29
	(0.13)	(0.23)	(0.23)	(0.33)	(0.14)	(0.28)	(0.18)	(0.25)
Earnings								
Treatment	3.69	-2.17	-2.40	2.92	7.65	7.24	-17.59	-32.05*
	(9.69)	(22.52)	(13.49)	(20.95)	(9.98)	(19.25)	(11.38)	(17.63)
Obs.	1,080	438	649	325	802	344	409	191
Mean	122.40	123.07	130.08	131.05	200.28	206.51	200.70	196.80
Panel B: Years of Education								
Treatment	-0.18	-0.24	-0.09	-0.10	0.20	0.64	0.36	0.37
	(0.13)	(0.22)	(0.42)	(0.50)	(0.23)	(0.45)	(0.30)	(0.36)
Obs.	1,617	698	604	293	924	399	359	171
Panel C: Age								
Treatment	0.26	0.57	0.44	0.60	0.22	-0.19	-1.21	-1.58
	(0.37)	(0.67)	(1.05)	(1.28)	(0.86)	(1.41)	(1.22)	(2.08)
Obs.	1,617	698	604	293	924	399	359	171

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

Table W6 Effects on all earnings from having a parent who is Mayor, sample divided by the parent's "incumbency" status, including control variables

	Children of politician				Siblings of politician			
	Relatives to first-time chairs		Relatives to incumbent chairs		Relatives to first-time chairs		Relatives to incumbent chairs	
	All	5%	All	5%	All	5%	All	5%
Log Earnings Treatment	0.22**	0.37**	0.00	-0.16	0.18	0.21	0.02	0.01
	(0.09)	(0.15)	(0.14)	(0.17)	(0.12)	(0.20)	(0.17)	(0.22)
Earnings Treatment	13.45**	20.43**	-5.08	-23.62	6.11	14.95	-15.36	-26.62
	(5.45)	(8.78)	(12.22)	(17.27)	(10.40)	(18.73)	(14.13)	(21.97)
Obs.	1,581	687	604	293	891	391	359	171

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less. Control variables include the lag of the binary indicator for being the ruling party, dummy variables for ten-year age intervals (-20, 20-29, 30 to 39, etc.), and dummy variables for seven levels of educational attainment.

Table W7. Effect on occupational and geographical residence of having a sibling in the largest governing party.

	Siblings to the top three Politicians		Siblings to top-ranked politicians	
	All	5%	All	5%
Panel A: Municipal employee				
Treatment	0.01	0.03	0.05	0.10**
	(0.01)	(0.02)	(0.03)	(0.05)
Obs.	3,794	1,692	1,283	570
Mean	0.27	0.28	0.30	0.30
Panel B: Employed in parent's old industry				
Treatment	0.00	-0.00	-0.00	-0.01
	(0.00)	(0.01)	(0.01)	(0.02)
Obs.	3,794	1,692	1,283	570
Mean	0.01	0.02	0.01	0.01
Panel C: Student				
Treatment	-0.02**	-0.02*	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Obs.	3,794	1,692	1,283	570
Mean	0.03	0.04	0.02	0.02
Panel D: Living in the same municipality				
Treatment	0.03***	0.03*	0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
Obs.	9,199	3,853	3,209	1,368
Mean	0.32	0.33	0.31	0.32

Table W8. Effect on the lag of children's occupational and residence status of having a parent in the largest governing party.

	Children to top-three		Children to top- ranked	

	politicians		Politicians	
	All	5%	All	5%
Panel A: Municipal employee				
Treatment	-0.00	-0.01	0.01	-0.01
	(0.01)	(0.02)	(0.02)	(0.03)
Obs.	4,977	2,214	1,729	763
Mean	0.26	0.24	0.28	0.27
Panel B: Employed in parent's old industry				
Treatment	0.00	0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)
Obs.	4,480	2,002	1,616	715
Mean	0.07	0.07	0.06	0.06
Panel C: Student				
Treatment	0.01	0.00	0.01	-0.03
	(0.01)	(0.02)	(0.02)	(0.04)
Obs.	4,977	2,214	1,729	763
Mean	0.32	0.32	0.33	0.34
Panel D: Living in the same municipality				
Treatment	0.03***	0.01	0.03**	0.02
	(0.01)	(0.01)	(0.01)	(0.02)
Obs.	9,773	4,387	3,476	1,564
Mean	0.53	0.52	0.53	0.52

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.

Table W9. Effect on children's occupational and residence status of having a parent in the largest governing party, including control variables.

	Children to top-three politicians		Children to top-ranked Politicians	

	All	5%	All	5%
Panel A: Municipal employee				
Treatment	-0.01	-0.01	-0.02	-0.04
	(0.01)	(0.02)	(0.02)	(0.03)
Obs.	6,228	2,790	2,185	980
Panel B: Employed in parent's old industry second				
Treatment	-0.00	-0.01	-0.02	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
Obs.	6,093	2,725	2,130	952
Panel C: Student				
Treatment	0.01	0.02	-0.01	-0.00
	(0.01)	(0.01)	(0.02)	(0.03)
Obs.	6,228	2,790	2,185	980
Panel D: Living in the same municipality				
Treatment	0.02*	0.01	0.03**	0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Obs.	9,890	4,305	3,495	1,542

Note: Robust standard errors clustered at the level of municipality and election period in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. In the column titles, "all" refers to the sample of all elections, and "5%" refers to the sample of elections where the win margin of the winning political bloc was 5% or less.