Workplace Networks And Political Selection*

Linuz Aggeborn† Henrik Andersson‡

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Abstract

Do social networks at workplaces function as cues into the political arena? We consider this question using the case of Sweden, which has many leisure politicians who work at the regular labor market. Restricting our networks to small cells of individuals within the same occupation and workplace, we find that an individual is more likely to become a politician in the future if that person had a colleague who was a politician. We further find that these newly enrolled individuals are placed higher up on the party lists – which to a very large extent dictates which party nominees that are elected – in subsequent elections. Our mechanism analysis indicates that a partisan channel may explain most of the main effect and that high-ability party officials are more prominent than low-ability officials in terms of recruiting from their workplace networks.

Keywords: Political selection; Workplace networks; Sweden

JEL–codes: D72, J01, J16

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†Department of Government and the Institute for Housing and Urban Research at Uppsala University, Uppsala Center for Fiscal Studies (UCFS), and Uppsala Center for Labor Studies (UCLS).

‡Institute for Housing and Urban Research at Uppsala University, UCFS, and UCLS.

1 Introduction

Politicians are key economic agents. They decide over public policy, and they implement legislation that affects the functioning of the market. Studying how and from where politicians are selected is therefore important (Besley 2005). The origin and selection of politicians can determine the overall competence of those in power (e.g., Acemoglu et al. 2010), and the personal characteristics of political leaders have been linked to core economic outcomes such as economic growth (Besley et al. 2011; Jones and Olken 2005).

That the origin and characteristics of officials matter for policy is also predicted by economic theory in the seminal citizen-candidate model (e.g., Besley and Coate 1997; Osborne and Slivinski 1996). In this model, politicians are assumed to have different policy-preferences since they originate from utility maximizing citizens with varying individual characteristics. Taking their point of departure from this model, later theoretical papers have then focused on the connection between the political sector on the one hand and the labor market on the other (Mattozzi and Merlo 2008). It has for example been hypothesized there will be a negative selection of *bad* politicians who have a comparative disadvantage on the regular labor market (Caselli and Morelli 2004; Messner and Polborn 2004). This will matter for policy outcomes, since low-ability politicians presumably provide public goods for a higher tax rate (Caselli and Morelli 2004).

Taking our point of departure in this literature on the relationship between labor and politics, we investigate whether – and if so, how – the *workplace* functions as a selection mechanism for new politicians in Sweden. Many politicians with power over public policy are leisure-time politicians who are employed at regular workplaces, while at the same time being politically active when not on the job. This contrasts with the more stylized pattern in the theoretical literature whereby a political career and participation on the regular labor market are assumed to be mutually exclusive. Hence the workplace is not distinct from the political sector; rather, it can function as an arena for screening, engaging, and recruiting new candidates. Studying the link between the political sector and the regular labor market is important, because the pool of politicians will likely look different if the workplace functions as an arena for their selection than it will if it is other networks that influence who runs for office.

Using registry data from the 2002–2018 period, we investigate whether having colleagues who are politicians has an effect on one’s probability of becoming a candidate in a subsequent election in

Sweden. We also study the intermediate mechanisms involved, in an effort to shed light on *how* workplace networks affect political selection.

We arrive at three main conclusions. First, we find evidence that having had a politician-colleague increases one's probability of becoming a politician in the future. On average, an increase of one standard deviation in the share of politician-colleagues increases the tendency to run for office in the next term by approximately 4%, relative to the average tendency to run for office. Second, our results suggest the workplace is a stepping stone to a political career. Swedish elections largely operate through party lists, on which different parties rank their candidates locally, regionally, or nationally. Most voters vote for a party, rather than for a specific candidate. Consequently, the ranking of a particular nominee on the party lists, as chosen by the parties, directly influences who gets elected. Our results show that nominated individuals get a better rank on the party list already within a single mandate period if they had a workplace connection with an earlier party member. In other words, workplace networks increase the tendency to be nominated for office in the short term, and to be more highly placed on the party list in the long term. Third, the mechanism behind the main effect is first and foremost partisan. Individuals with left-wing politician-colleagues at work are more likely to become (left-wing) nominated politicians themselves; and the same goes for their right-wing counterparts. We also find some evidence for an inbreeding bias: high-ability individuals are more likely to become politicians if they have politician-colleagues who are also of high ability (although high-ability politicians recruit both high- and low-ability colleagues). This conclusion connects up with the theoretical model set out by Mattozzi and Merlo (2015), which states that political parties may recruit low-ability individuals, in order to maximize the output of their officials taken together. Such an equilibrium is also more probable according the authors where proportional representation is employed, as in Sweden.

We identify the effect of having politician-colleagues upon one's entry into politics by using information on occupation *and* the workplace composition of one's co-workers over time. For the sake of simplicity, we refer to politicians within the same occupation and workplace as politician-colleagues. Our research design – which relies on a multitude of fixed effects – is possible due to the detailed administrative data available in Sweden. Sweden is also a suitable case given that both men and women in that country work outside of the home to a great extent, and given that many Swedish politicians have regular jobs and hence a workplace connection. With the exception of members of the national parliament and a few full-time politicians on the local level, most Swedish politicians are leisure-time politicians, meaning that the vast majority of them are also employed outside the political sector. Our data from the 2002–2018 period, with its indicators for workplace and occupation, allows us to pinpoint smaller cells of workers, who are likely to have social interactions on a daily basis. We then combine our data on workplaces with data on political candidacy, whereby we can identify all individuals who ran for and were elected to office for the same time period.

Our paper contributes to several different literatures. First, there is an emerging literature on workplace ties and their effects on future political careers, but in which the evidence originates from a non-democratic setting. Fisman et al. (2020) analyze promotions to the Politburo within the Chinese Communist Party, and test whether a person is more likely to become a member if he or she had an earlier workplace connection, school connection, or hometown connection with a member who is retiring. The authors find no effect from workplace connections, and they find negative effects from school and hometown connections, which they interpret as indicative of competition within fractions of the Chinese Communist Party. However, Jia et al. (2015) find that provincial leaders are more likely to be promoted if they had an earlier workplace connection within the central government if they have *also* delivered economic growth in their district.[1] Our paper contributes to this literature, but with empirical evidence from a democratic country, where the incentives and rules are quite different.

Second, we contribute to the massive literature on political socialization. Earlier research has demonstrated that political socialization takes place above all within the family during childhood (Jennings 2007; Sears and Brown 2013). In this paper, we focus on another period: adulthood. Workplace connections with a politician may thus function as political socialization for a later period in one's life.[2] It is reasonable to expect that, in adulthood, a person is more likely to become involved in politics if another person encourages him or her to get involved. Many scholars have taken an interest in the relationship between social networks and political participation (Lim 2008), but their focus has generally been on strong ties such as those in a family, as opposed to those at a workplace. A couple of papers, though, do focus on workplaces. Using Swedish survey panel data, Adman (2008) tests

[1] See also Persson and Zhuravskaya (2016) for an analysis of the incentives facing Chinese party officials.

[2] For evidence regarding the intergenerational transmission of beliefs and political affiliations in Sweden, see Westholm (1991) and Aggeborn and Nyman (2021). Folke et al. (2017) analyze how family connections with politicians in Sweden affect rents, and they find evidence that children of politicians earn more. However, they conclude, this is probably because the children in question remain in the municipality and postpone their enrollment in higher education. See also Folke et al. (2021a) for an analysis of political engagement of women with family connections to politicians.

whether workers who are employed at a workplace where decision-making is more democratic have a higher rate of political participation, and he finds no support for this claim. Mutz (2002) argues that being part of a network may lower the level of political participation if people within the network (such as co-workers) have conflicting political views. These questions connect up with the discussion on whether strong social networks, such as families (wherein people are more similar to each other) have a greater impact on the political behavior of individuals than do weak social networks (wherein people are more likely to encounter heterogeneous political opinions). Folke et al. (2021b) demonstrate with registry data that Swedish politicians have varying occupations, and that their labor market experience is associated with their future ideological position along the economic left/right dimension and the libertarian/authoritarian spectrum. Their paper provides an empirical test of the theory presented in Kitschelt (1994) on the labor market foundations of party choice. Our paper relates to this discussion, but our focus is instead on labor market networks, not on specific occupations or workplaces.

Third, we contribute to the so far small but growing literature on the impact of workplace networks on out-of workplace activities. The workplace is a place to earn a living, but it also includes a social dimension, where peers interact and form social networks. These networks can develop during daily work-related duties, as well as during social activities between tasks; and they are likely to constitute an important part of the overall social interactions between people. From an economic point of view, workplace networks may provide valuable information (e.g., Granovetter 2005; Jackson 2010). Workers who are politicians may use information from their workplace to find suitable co-workers to recruit to their political parties. Mutz and Mondak (2006) highlight the workplace as a particularly interesting political arena, where employees engage in political discussions with people who do not hold the same views as themselves. In terms of empirical research, scholars whose work is closest to ours include Nanda and Sørensen (2010), who conclude that social networks established at the workplace increase entry into self-employment; and Carlsson and Reshid Abrar (2022), who find evidence of workplace peer effects in terms of the uptake of parental leave. Our study also connects up with papers on peer effects on the performance of other workers, in terms for example of wages and innovation (Bandiera et al. 2010; Cornelissen et al. 2017). Using Swedish registry data, Nix (2020) finds a positive impact on one's wage level from colleagues' level of education, indicating the existence of spillovers from learning and information within workplace networks. This is connected to our research question, but we study another aspect of information spillover: namely, political selection.

2 Theoretical framework and mechanisms at play

In this section, we sketch a theoretical framework for how and why political engagement is found at the workplace. Two channels may be broadly said to operate here: a supply-side channel and a demand-side one.

Supply side Let us begin with the supply side. It is a fact that only a small part of the population runs for political office. In order to understand why a person makes the effort to run for office – instead of just settling for voting for a specific candidate – we must analyze the incentives for entry into politics. One way of modeling entry into politics is offered by the first step in the Citizen-Candidate model (Besley and Coate 1997; Osborne and Slivinski 1996), according to which people become candidates if doing so maximizes their utility. According to this model, the choice to run for office is endogenous: it depends on the policy positions of other candidates, on their ability to enact said policies, and on the costs of entry. Costs of entry include the costs incurred in gathering information on the different parties and establishing contacts with persons who are already politically engaged.

We argue that the step from citizen to political candidate is less costly when one has a workplace connection with a politician. Such a personal acquaintance can provide the necessary information about that person's party, and enrollment in the party may then take place without any process of active recruitment. However, the workplace politician in question may also furnish general information about political engagement, thereby lowering the cost of information-gathering on a more general level. In that case, having a politician-colleague may increase one's likelihood of becoming a candidate for any party – not necessarily that of one's colleague.

A similar line of thought is found in the political science literature. Verba et al. (1995) argue that the most likely reasons for people *not* to be involved in politics are "because they can't, because they don't want to, or because nobody asked" (Verba et al. 1995, p. 5). Being asked is much more likely if one has a politician at one's workplace. This reflects the importance of social networks at the workplace and of interactions between colleagues. Moreover, in addition to helping reduce information costs, a politician-colleague may create a socialization environment for political engagement. This may take the form of political discussions that foster interest among those not already politically engaged.[3] The use of referrals may be seen as a way of screening political candidates informally. The earlier literature on candidate selection has emphasized that "contact capital" and informal rules need to be taken into account if we are to understand why certain individuals are deemed suitable as candidates by the parties (Widenstjerna 2020).[4]

Demand side While it is easy to see how entry costs may dampen political activity, the workplace as a recruitment arena for political parties requires some elaboration.

Let us assume the existence of political parties with the objective function of seeking to win elections. These parties consist of party members, who are essentially citizens, some of whom have decided to run for office. The payoff function of parties equals the productivity of their individual members. We interpret productivity in terms of political productivity, by which we mean gaining votes in elections. In practice, however, we shall assume that political productivity is a function of workplace productivity, and is well approximated by it. One way to interpret this is to say that both aspects of productivity are the result of initial endowments and of over-the-lifetime investments in human capital. Cognitive ability is a good example: it is determined both by natural endowment (genes) and by investments (for example education). For simplicity, we assume citizens can be of two different types: a high-productivity type and a low-productivity one.

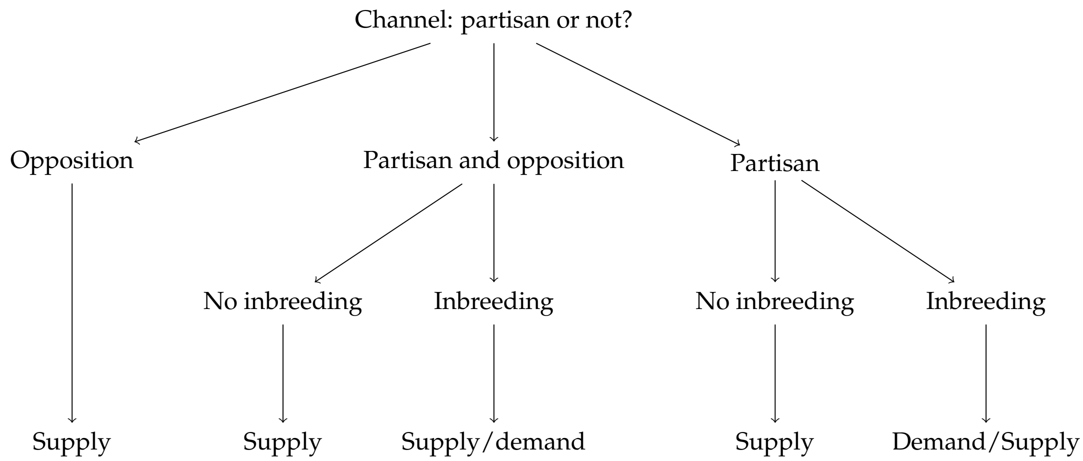
There is information asymmetry, furthermore, since the parties cannot observe the political productivity of non-member citizens. From the parties' point of view, all potential new party members are observationally equivalent. Yet the parties must decide whether to nominate new members to political office – before observing their political productivity.

In line with the seminal labor-economics model set out by Montgomery (1991), we argue there is an *inbreeding bias* among workers, whereby high-productivity workers are likely to have stronger social ties with other high-productivity workers than with their low-productivity colleagues. Simply put,

[3]Note that the earlier literature on political socialization has been concerned above all with early life socialization and socialization during one's *impressionable years* (Campbell et al. 1980; Jennings 2007; Sears and Brown 2013). Our paper relates to this literature, but we explore a socialization arena that has not been extensively analyzed before: namely, the workplace.

[4]This is linked to the question of whether politicians recruit individuals that are similar to themselves (Bjarnegård and Kenny 2015; Kenny 2013). According to the concept of homosocial networks and homosociality, men and women prefer to interact and spend time with people of the same gender (Bjarnegård 2013; Holgersson 2013). Lipman-Blumen (1976) argues that, since certain men dominate the economic and political arenas, men prefer the company of other men over that of women. See Bjarnegård and Zetterberg (2019) for a discussion and analysis of formal selection criteria.

Figure 1: Mechanism channels



individuals at work socialize with others who are like themselves. Because of this, parties maximize their utility by recruiting on the basis of referrals from high-productivity party members, since the latter are more likely to know other people of high productivity. This line of thought also closely recalls the argument presented by Brady et al. (1999), who model the recruitment of political activists. They argue that politicians have an incentive to gather information on activists' potential (i.e., their productivity). Brady et al. (1999) contend further that recruiters have an incentive to use cues to ascertain the political potential of prospective members.

Although the main channel should be that high productivity party members recruit other high productivity individuals from their workplace networks, it bears noting that Mattozzi and Merlo (2015) argue that political parties in proportional representation system could also recruit new party officials of low productivity because parties need such rank-and-file members too. In the upcoming empirical analysis, we take these different hypotheses to the data.

Demand side or supply side? In our mechanism analysis later in this paper, we try to separate what we have labeled a supply-side and a demand-side channel. It is difficult, however, to devise a definitive test that can rule out one channel before the other. In Figure 1, we have summarized the different intermediate channels, and identified the channel to which specific results in our mechanism analysis point.

For the sake of completeness, we should explain that the most clear-cut result would be a null or even a negative reduced form result. In such a case, there would be no recruitment going on from workplaces to the political sphere, and no lowering of entry costs. As such, neither a supply-side nor a demand-side channel would be present. In order, however, to keep Figure 1 parsimonious, we consider only instances with an estimated positive relationship between an increased tendency to become a politician and the share of politician-colleagues. If we find that the share of politician-colleagues increases one's probability of becoming a candidate in the future, either a supply-side or a demand-side channel may explain the result. It is also possible that a supply-side and a demand-side channel are operating simultaneously. Let us now discuss each branch of the mechanism tree.

We begin on the left of the mechanism tree in Figure 1. If we find that the positive reduced form result can be explained by an increase in the probability of becoming a candidate for another party than the one represented by one's co-worker, we may rule out a demand-side recruitment mechanism. In such a case, the explanation is a pure supply-side effect, where the politician-colleague has lowered information costs for entering the political arena in general.

If instead we find that having a politician-colleague both increases the probability of running for the same party as one's co-worker for some individuals, and increases the probability of running for the opposition for others, we end up in a more complicated scenario. In this case, we need to turn to our previous discussion on inbreeding bias, in line with Montgomery (1991). If we find no evidence of an inbreeding bias, then a demand-driven mechanism is not likely to be operating, since such a channel presupposes that parties try to recruit by referrals. Again, therefore, a supply channel is probably the operative mechanism. On the other hand, if we find support for an inbreeding bias, we cannot rule out a demand-side mechanism. However, since we have also found that the probability increases both for running for the political party in question and for running for the opposition in this branch of the tree, we cannot rule out a supply-side mechanism either. Instead, the most likely explanation is a combination of a supply-side and a demand-side mechanism.

Let us turn now to the last part of the mechanism tree. Here we focus on the scenario where we find a positive reduced form, and where this reduced form is driven by an increase in the probability of running for the same party as that of one's co-worker. Which channel, then, is most likely? Again, it depends on whether we find an inbreeding bias or not. If we find no inbreeding bias, the most likely mechanism is again a supply-side mechanism, where the politician-colleague has lowered information costs for his or her political party, but where no recruitment is going on. If we do find an inbreeding bias, however, then a demand-side channel is the most likely mechanism, since we then will have found that co-workers tend to run for the same party, and that the probability of that is furthermore increased if the politician-colleague and the soon-to-be new candidate are both high-productivity individuals.

We return to this discussion in Section 7, when we present our empirical results for the intermediate mechanisms.

3 Institutional background

Swedish case In 2018, 77% of all females and 80% of all males aged 16—64 were employed in Sweden (SCB 2018). A large part of the Swedish population, then, spends a considerable amount of time at a workplace. This mere fact makes the workplace context important to study as an arena of political engagement. We have access to information on where individuals are employed, as well as detailed information on which occupation individuals have within those workplaces. This enables us to pinpoint small cells of workers who are likely to spend time together.

Politics in Sweden We study general elections in Sweden. Elections for three different levels of government take place in that country every fourth year, on a single day. These levels of government are often referred to as the national, regional, and local levels; and the corresponding elections are to the parliament, the county councils, and the municipalities. A person needs to be 18 or older to vote and to run in an election. Local and regional politicians have a substantial say over economic policy. For example, municipalities in Sweden have the right to set their own tax rates, and they are important providers of welfare services. The regions too have the right to collect taxes, and they are primarily responsible for the publicly funded health-care system.

Elections are held with a closed-list PR system, meaning that political parties rank their candidates on local, regional, and national lists. The position on the party list is the most important factor for a candidate's chances of getting elected, because it determines – together with the vote share for the party – which candidates on the list who obtain a seat. Those on the list who are not elected become substitutes for the elected candidates, and it is also common for substitutes to serve on various subcommittees (*nämnder*). The process of determining the composition of the party list varies for the different parties. Some use primaries; others decide by local boards.[5]

Eight parties represented in the Swedish parliament: the Left Party, the Social Democrats, and the Green Party, which together are considered to form the left-wing bloc; and the Center Party, the Liberal Party, the Christian Democrats, and the Moderates (conservatives), which traditionally form the right-wing bloc. Then there are the Sweden Democrats, who entered the parliament in 2010. This party has not been included historically in either of the two traditional blocs. In recent years, however, it has steadily positioned themselves as a right-wing anti-immigration party. As a consequence, the bloc division between left and right has faded slightly in Swedish politics, mainly due to the emergence of the Sweden Democrats.

Dal Bó et al. (2017) have previously demonstrated that Swedish politicians are representative of the general population in terms of class background, but that they are more intelligent than the average. Since more intelligent individuals are likely to work at specific workplaces, the conclusions of these authors are important for the design of our empirical specification, to which we now turn.

4 Data and empirical model

Empirical universe In our panel, which is based on data from Swedish administrative registers, the first year is 1998 and the last one is 2018. This equals six mandate periods in total, $t = \{1, 2, 3, 4, 5, 6\}$. In our empirical analysis, we study five elections for our outcome variable, $t = \{2, 3, 4, 5, 6\}$, – in 2002, 2006, 2010, 2014 and 2018 – in total, given that we use the share of politicians in $t - 1$ as the treatment variable. Politicians nominated in 1998 are thus included on the right-hand side of the regression equation (treatment), but not on the left-hand side (outcome).

The minimum age for standing as a candidate in Sweden is 18, and most Swedes retire at age 67. 18–67 is thus the age span for the individuals included, since we need to have information on whether they are candidates as well as particulars on their occupation and place of work. Also, since our panel estimations make use of information specific to occupations and workplaces over time, we require an individual to be employed at a registered workplace with a corresponding occupation throughout our

[5]Voters can also cast a personal preference vote in the general election for one candidate on the party list. A candidate receiving more than 5 % personal votes in the election district is moved up on the list and is elected, regardless of the initial order of candidates on the list (assuming the party achieves representation). Note that it is very unusual for a candidate to be elected only through personal votes from an otherwise insufficient list position.

panel. For those cases where information is missing with regard to workplace and/or occupation, we interpolate given existing values in the panel for a given individual.[6] If there are remaining missing values for any observation within the panel, we drop the individual from the analysis altogether. For the first mandate period (1998–2002), we use occupation and workplace codes from the last year in the mandate period (2001), because we do not have access to accurate occupation codes for earlier years.[7]

Outcome variable The focus of our main analysis is on nominated politicians: i.e. all individuals who have ever run for the municipal council, the county council, or the national parliament. Being a politician is relatively uncommon, and we want to increase the size of our data sample; in other words, nominated politicians are more suitable for our analysis than elected ones. Nominated politicians are also involved in party politics, even if they are not elected. It is also common for an elected politician to resign during the mandate period – meaning that the nominated substitute politician next in line on the party list (but who was not elected on election day) serves the remainder of the term. Given that our research question concerns the link between workplaces and the political sphere, nominated politicians are also likely to be important in the sense of representing the first-step political enrollment of new party members who may become elected politicians in the future.

The outcome variable is defined as a dummy variable, which is equal to 1 if the person is nominated in a given mandate period, and 0 otherwise. Note also that we have a strong prior here – that there is a considerable incumbency advantage in politics. We therefore restrict our entire analysis to individuals in the panel who have not themselves been nominated previously ($t = \{1, 2, 3, 4, 5\}$).

Treatment variable: Workplace–occupation networks To increase the likelihood of individuals' actually having interacted with each other, we use the share of nominated politicians at the same *workplace* and *occupation*. A workplace is defined in our data as a property or address within the same firm, so it is smaller than the average size of a company. The median workplace has around 45

[6] If the first row is missing, we make use of the second row. If the last row is missing, we make use of the second-to-last row. If there is a donut case, where indicators are missing in the middle of the panel, we make use of the value in t-1. For remaining missing values, we take the mode value for workplace and occupation indicators within the panel.

[7] The occupation codes originate from *Lönestrukturstatistiken*, whereas the workplace indicators come from *RAMS*. The occupational codes cover the entire population of public employees and of employees at large private workplaces; whereas employees at private workplaces with fewer than 500 workers are surveyed from a representative sample. This means that occupational codes are carried over to following years in LISA for some workers, if they have not been surveyed in a specific year. Because of this structure in the registry data, we make use of the interpolations described in footnote 5.

employees; however, the number varies from just a few to several hundred or (in a few cases) several thousand. Furthermore, we expect many workplaces not to be vertically integrated. We therefore employ International Standard Classification of Occupation (ISCO) codes. We use 3-digit codes, which classify workers into different occupational categories. The combination of workplace and occupation means among other things that we can distinguish between researchers and administrative staff who are both employed at the same university department.

The explanatory variable is then defined as the share of politicians within a workplace and occupation cell, standardized with mean 0 and standard deviation 1. For a given individual, we calculate that person's share excluding him/herself, such that the explanatory variable represents the share of politicians among the other workers within the same cell. If the individual later becomes a politician, he or she remains in the panel and thereby increases the share of politician-colleagues for the other workers in the same cell. However, as mentioned earlier in this section, we restrict all regressions to those persons in the panel who have not been nominated previously. In practice, this means there are no individual cascade effects where an increased share due to one's becoming a politician oneself increases the probability that one will remain a nominated politician in the future. It does mean, however, that the treatment variable is increased for the other individuals within the cell who are not yet nominated politicians. In bears noting that for the large majority of the individuals, there are no politician-colleagues in any time period, meaning that the distribution of the independent variable is skewed in both absolute and relative numbers.

Identification strategy in main analysis Our goal in this paper is to estimate the effect of having a politician-colleague on the probability of running for office in the future. However, politicians are not randomly distributed across workplaces or occupations. For example, it is possible that politicians and people with strong nascent political ambitions sort themselves into specific workplaces, where their interest in societal matters tends to be shared (Fox and Lawless 2005). We also know that politicians in Sweden are more intelligent than average members of the population (Dal Bó et al. 2017). These features provide further indications that politicians are not likely to be randomly distributed across either workplaces or occupations.

The ideal empirical strategy would be to assign numbers of politicians randomly to specific occupations and workplaces in time period $t-1$, and then to estimate the partial average treatment effect of having more politician-colleagues on the probability that an individual will become a nominated politician in t. Naturally, however, such an empirical strategy cannot be applied; instead, we have to find a way to compare workers that are similar in all other characteristics, except in the number of politicians with whom they interact at the workplace in a given occupational category in $t-1$.

Our strategy for the main analysis mimics that of Cornelissen et al. (2017), who use a multitude of fixed effects and trend variables to study workplace peer effects. While the outcome of interest in their case is wages, and the treatment is workplace peer quality, many of the obstacles regarding endogeneity – for example the selection of certain individuals into specific workplaces – are shared between the two settings. Our strategy is therefore to study the effect of occupation and workplace specific co-workers who were politicians in the current period on the probability that an individual will enter politics during the following mandate period. This identification strategy makes use of the timing of entry into politics among those individuals who eventually become a politician at some point. Essentially, the identification derives from workers who stay within the same cell of a workplace *and* occupation in which the share of politicians changes over time.

Regression equation We estimate the following linear probability model in our main analysis:

$$\begin{aligned} \mathrm{Nom}_{itwom} &= \beta_0 + \beta_1 X_{t-1,wo} + \gamma W_{iwo} + \beta_2 Z_{it} + \beta_3 E_{t-1,wo} \\ &+ \tau_t + \phi_{w,t} + \epsilon_{ot} + \lambda M_m + u_{itwom} \end{aligned} \tag{1}$$

where subscripts include i for individual, t for mandate time period, w for workplace, o for occupation, and m for municipality. Nom_{itwom}, the dependent variable, is a dummy variable, taking the value of 1 if an individual becomes a nominated politician in t, and 0 otherwise. $X_{t-1,wo}$ is the treatment variable measuring the standardized share of politicians at the workplace within an occupational category (3 digit ISCO codes) in $t-1$. u_{itwom} is the error term.

Formally, the identifying assumption can be written as a conditional independence assumption. Let Γ_{itwom} equal the right-hand side terms in Equation 4 besides the error term (u_{itwom}) and the explanatory variable of interest (X_{t-1wo}).[8] Conditional independence requires:

$$E(u_{itwom}|X_{t-1wo}, \Gamma_{itwom}) = E(u_{itwom}|\Gamma_{itwom}),$$

or, in other words, that after conditioning on our control variables and the multitude of fixed effects (Γ_{itwom}), the share of politicians at the workplace and occupation is independent of any factor potentially causing entry into politics.

W_{iwo} – which is a grouped individual-, workplace-, and occupation-fixed effect – is crucial from the standpoint of identification. By including this fixed effect, we restrict the empirical analysis to individuals who work at a specific workplace in a specific occupation. The identifying variation thus stems from those individuals who sometimes switch from not being a politician to becoming one. In essence, we use the timing for a given individual in the panel to estimate the effect.

It is possible the share of politicians is bundled with other socioeconomic characteristics at the workplace/occupation level. We, therefore also include: (i) $E_{t-1,wo}$, which is a vector consisting of the means for individuals in the workplace/occupation cell in $t-1$ for years of education, share of females, share of immigrants, and average income level among the workers; (ii) ϵ_{ot}, which captures occupation time trends; and (iii) ϕ_{wt}, which measures workplace time trends. (ii) and (iii) compensate for the fact that certain occupations or workplaces attract more politicians over time.

At the individual level we include Z_{it}, a vector of time-changing individual covariates, such as years of education and standardized income for the individual in time period t. We furthermore include mandate period-fixed effects τ_t, so as to compensate for general time differences in the probability of becoming a politician.

Lastly, we include municipality-fixed effects for municipality of residence, M_m, because the likelihood of being nominated varies across Sweden. It is relatively easy to be nominated in a small municipality, but the competition in the larger cities is heavier. Standard errors are clustered at the individual workplace/occupation level.

The multitude of fixed effects we have used raises the question: what variation is left? For a given worker, a change in the share of politician-colleagues may occur due to a politician's

[8] $\Gamma_{itwom} = \gamma W_{iwo} + \beta_2 Z_{it} + \beta_3 E_{t-1,wo} + \tau_t + \phi_{w,t} + \epsilon_{ot} + \lambda M_m$

being hired into, or dropping out of, the same occupation and workplace cell. The share of politicians can also change as a result of an alteration in the overall composition of the workplace/occupation cell. For example, if the number of colleagues who are not politicians within the same cell falls, the share of politician-colleagues within the same cell mechanically rises in tandem. This embodies an intuitive logic: if the *share* of politicians in an occupation and workplace cell increases from 1 in 7 to 1 in 3, chances are the workers within the same occupation and workplace cell will have more interaction with the politician-colleague in question.

LPM vs. conditional logit One lingering issue here is the choice of regression model. Heretofore we have argued for a linear probability model (LPM) with multiple fixed effects. Applied researchers usually prefer an LPM in applied work, since the estimated coefficients can more easily be interpreted as marginal effects. There are, however, some institutional issues that cast doubt on the suitability of an LPM for our particular setting, due to the problem of how to interpret the size of the estimated coefficients. First, the baseline probability of becoming a politician is very low, as can be seen in Figure 2 in the next section. Second, focusing on marginal effects around the mean may be problematic, given that the probability of becoming a politician is so different for different parts of Sweden, on account of the varying ratio between the number of seats in the municipal council and the size of the local population. In general, it is easier to become a politician in a small municipality than in a big city. The marginal effect is therefore hard to interpret – even when municipality-fixed effects are included in an LPM model – in relation to the mean of the dependent variable. The effect may be driven by the smaller municipalities, where the baseline probability of running for office is higher. In a nutshell, relating the estimated LPM coefficients to the mean of the dependent variable becomes problematic for assessing the economic significance of the estimated coefficient.

These features argue for choosing a conditional logit model (e.g., Breslow et al. 1978), where we focus on odds ratios instead of on marginal effects. The problem is that a standard conditional logit model can only be run with one grouping variable. A conditional logit would also be computationally demanding, given the large number of observations we have.

However, we can run a conditional logit with the most important fixed effects W_{iwo} — that is, the grouped individual-, workplace-, and occupation-fixed effects — where we focus on the timing of a person's entry into politics. We can also include the non-binary covariates that are not included as fixed effects. In the analysis below, therefore, we present results both for LPM estimations with all of the fixed effects, and for conditional logit models with a more limited set of fixed effects and covariates. In the Appendix, moreover, we present results where we split the LPM estimations by different municipal population sizes, in order to see if our results are driven by the smaller municipalities.

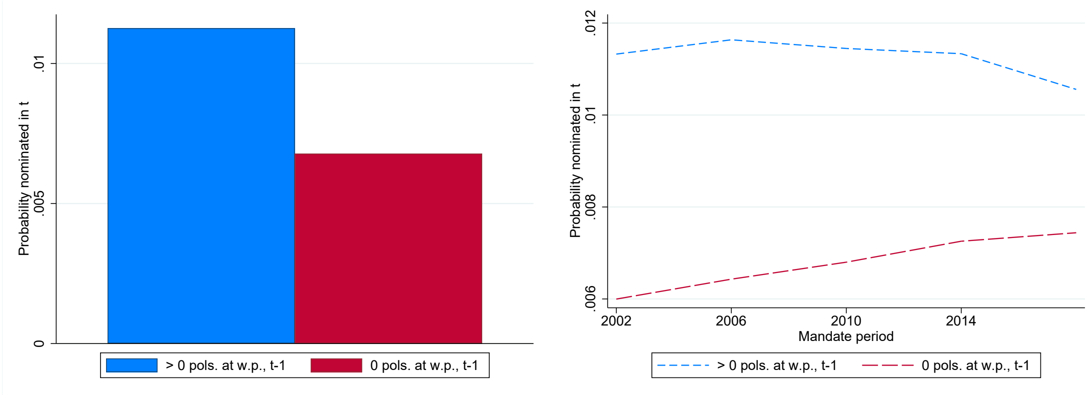
Lastly, the fact we are using fixed effects together with a lagged variable of interest connected to the dependent variable means we are risking a bias along the lines noted by Nickell (1981). We discuss this further in the Appendix. By running simulations, we demonstrate in Section E1 that such a bias is present if we include individual-, workplace-, and occupation-fixed effects separately. When we include the interacted fixed effects (hence using the timing to estimate the effect), the Nickell bias moves toward 0. For our preferred specification, then, the problem of Nickell bias is small.[9]

5 Descriptive statistics

In Figure 2, we have plotted the probability of being nominated in t, depending on whether there are 0 politicians at the workplace and occupation, or at least 1 in time period $t-1$. It is clear from Figure 2 that there is a substantial descriptive difference between these two groups, which decreases slightly over time. This provides an interesting starting point for our analysis below. Figure 2 also highlights an important pattern, namely that relatively few individuals in absolute numbers end up being politicians. Even among individuals who have colleagues that are politicians, only a little more than 1 percent become politicians themselves. This is not surprising; after all, the available seats in political assemblies are few in number, relative to the total population in a representative democracy. However, the future interpretation of the estimated coefficients needs to take this into account, meaning that a small estimated coefficient could still be large relative to the mean value of the dependent variable. Again, it also means that the independent variable is skewed to the right, given that there are many workplace-occupation cells without any politicians.

[9]We use a large set of fixed effects and we cluster the standard errors. This may be problematic, however, in connection with singletons within the fixed-effects groups. We therefore follow Correia (2015) and the reghdfe command, which drops singletons.

Figure 2: Probability of being nominated depending on having colleagues who are politicians



(a) Average probabilities

(b) Probabilities for each election

Notes: The figures display the difference in the probability of being nominated in time period t, depending on whether one has politician-colleagues in t-1 or not.

6 Main results

The main results are presented in Table 1. In this table, we analyze whether the probability of being nominated is affected by having politician-colleagues in $t-1$. In contrast to our descriptive approach in Figure 2, we include various fixed effects in order to pinpoint the causal effect of interest.[10] Columns 1–4 display estimated coefficients from LPM models. Columns 5 and 6 show conditional logit estimates expressed in odds ratios.

Overall, we estimate positive and statistically significant effects for all specifications in line with our hypothesis that having politician-colleagues increases one's probability of running for office in the future. Let us now discuss each column, starting with the LPM coefficients, which may be interpreted as marginal effects. Column 1 in Table 1 displays the raw association between the dependent variable and the explanatory variable. In column 2, we move into our identification strategy, where we include interacted fixed effects for individual, workplace, and occupation. Due to the inclusion of individual fixed effects, we are now in essence only considering the timing of actual entry into politics by the pool of persons who eventually run for office. Including fixed effects in column (2) substantially reduces the estimated coefficient, but it remains statistically significant. In column (3), we add a large number of covariates, including: (i) individual covariates in t (standardized income and years of education); (ii)

[10]In line with our conclusions in Section E1 of the Appendix regarding a possible Nickell bias, we focus only on results where we include the combined individual-, occupation-, and workplace-fixed effects (column 2–4). For transparency, we display column 1 without any fixed effects or covariates.

Table 1: Main results: Probability of being nominated in t when one has politician-colleagues in $t-1$.

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Share politicians in t-1	0.00089***	0.00017***	0.00013***	0.00008***	1.03522***	1.02678***
	(0.00002)	(0.00002)	(0.00003)	(0.00003)	(0.00514)	(0.00534)
Mean dep. var.	0.0029	0.0017	0.0019	0.0018	0.3385	0.3677
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.381	0.408	0.477		
Pseudo-R2					0.001	0.134
Observations	31995947	22917601	18162465	16337407	113189	94903

*Note: Standard errors in parentheses are clustered at the individual/workplace/occupation level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary, and takes the value of 0 or 1. Columns 1—4 display LPM-estimated coefficients. Columns 5 and 6 present odds ratios from conditional logit models.*

aggregated covariates for the workplace/occupation cell in $t-1$ (average standardized income, average years of education, gender disposition, and share of immigrants); (iii) fixed effects for municipality of residence; and (iv) mandate period-fixed effects. In column 4, we further include workplace and occupation mandate trends. Including these fixed effects and covariates decreases the size of the estimated coefficients relative to that in column 2.

In terms of economic significance, the point estimate in the most conservative LPM specification (column 4) equals 0.00008 – meaning that, when the share of politician-colleagues is increased by one standard deviation in $t-1$, the probability of becoming a politician rises by 0.008 percentage points in t. This effect may appear small; however, only a small fraction of the population becomes a politician. If we divide the point estimate of 0.00008 by the mean value of the dependent variable (0.0018), we find that an increase of one standard deviation equals an increase in the relative probability of becoming a politician by approximately 4.5 percent, which must be considered substantial.

As we discuss in Section 4, there are arguments against this assessment of the economic significance and relative magnitude of the estimated coefficients. Let us therefore turn to the estimated coefficients from the conditional logit estimations in columns 5–6. Both coefficients are statistically significant, just as in columns 1–4. In contrast to the LPM model, where all observations contribute to the estimation when there is variation on the right-hand side of the regression equation, the conditional logit model only runs on the sample where there are actual switches in the dependent variable. This is illustrated in the decrease in the number of observations in column 5–6. A connected difference is that the LPM model includes separate intercepts for the $workplace * occupation * individual$ fixed effects, whereas the conditional logit models also allows for different "slopes" for this grouping variable.

In columns (5) and (6), the coefficients represent odds ratios from conditional logit models. In column (5), the coefficient equals 1.035, meaning that a positive event (becoming a politician in t) is more likely than a negative event (not becoming a politician in t) among those who eventually become politicians in the panel, if the share of politicians within the workplace/occupation cell in $t-1$ increases. In relative terms, this means the odds of becoming a nominated politician in the next mandate period are 3.5 % higher among those who have one standard deviation more politician-colleagues in $t-1$.

One cannot directly compare the marginal probability increases in columns 1–4 to the odds ratios in columns 5 and 6, but our overall assessment is that the estimated coefficients in columns 5 and 6 display slightly more moderate effects than the 4.5 % relative effect estimated using LPM in column 4. One plausible reason for the slightly different relative sizes in effects between LPM and conditional logit is that the marginal effect in column (4) was compared to the average tendency to become a nominated politician in the *entire sample*. But in general, in municipalities with smaller populations, a larger share of the local population will be nominated politicians. This varying ratio of politicians to local population arises from the fact that the number of local representatives does not increase proportionately with the number of inhabitants.[11] Running our most conservative LPM model from column (4), but separately for five different sizes of municipalities, we find that the marginal effects are indeed both larger and more precisely estimated for citizens living in smaller municipalities. These results are presented in Figure A1 in the Appendix. In a nutshell, there is a positive treatment effect, but the effect is driven by those municipalities where there is a higher probability of being nominated in baseline.

Robustness checks for main results We have run several sensitivity checks to assess the robustness of the main findings in Table 1. The results are presented and discussed in Section A1 of the Appendix. In Table A1, we run the same analysis as in Table 1, but we exclude all occupations that are

[11] The municipality of Bjuv, for example, has 31 local council members and about 6800 inhabitants, while the municipality of Stockholm has 101 council members for around 1 million inhabitants. Including municipality-fixed effects in a LPM model will not suffice to take this difference in baseline into account.

highly political in nature (PR consultants and lobbyists, for instance). In Table A2, we further exclude the five most common occupations among politicians. Our overall conclusion is that the results in Tables A1 and A2 are in line with the findings presented here in the main text. In Table A3, we change the explanatory variable from the share of nominated politician-colleagues to the share of elected politician-colleagues. Yet again we find positive and statistically significant coefficients that are similar to the ones presented in Table 1. For this specification, we end up with far fewer treated cells, given that the treatment becomes even rarer in this instance. In Figure A2, we assess whether our results are sensitive to the number of individuals employed within a workplace/occupation cell, and we find that our main findings are robust to various specifications. This is likely due to the fact that this analysis is run in line with the most conservative LPM specification in column 4 in Table 1, with a multitude of fixed effects. We have also investigated if our results changes when removing large values in our independent variable in Table A4 and Table A5. This is discussed further in the Appendix, but our overall conclusion is that the main effects are dependent on having a relatively high share of politicians within the workplace-occupation cell (either many politicians or a small network) with whom an individual are likely to have social interactions with.

In addition to the above-mentioned sensitivity checks, we include a number of placebo regressions in Table A6. Using the most conservative LPM specification in Table 1 again, we estimate the effect of politician-colleagues on years of education, parental leave income, income from unemployment protection, labor income, a college education dummy, and disposable income. We estimate small and statistically insignificant coefficients for all variables except the college enrollment dummy which is enters statistically significant, but the point estimate is very small and economically insignificant.

Intensive margin Up until this point, we have focused on the probability of becoming a nominated politician conditional on having politician-colleagues at the workplace. This is essentially an analysis along the extensive political margin. What about the intensive margin?

We investigate the effect of having politician-colleagues in t-1 on the party list-position in t. Here we follow Buisseret et al. (2022), who divide each party nomination list in Sweden into five different categories: top, safe, advantaged, highly contested, disadvantaged, and certain loss. Essentially, the party putting the list together may have a good prior understanding, based on earlier election results, of which slots on the list are electable and which are not.[12] We then use these list categories as dependent variables and apply the most conservative LPM specification (column (4), Table 1). The results are presented in Table 2.

We find that the main effect shown in Table 1 is driven by nominations in the "certain loss" category on the party lists in time period t. In other words, the person nominated is not likely to be elected. This makes a lot of sense, given that we analyze first-time nominated politicians only. That said, these people are nonetheless substitutes, and it is not uncommon for them to serve on municipal subcommittees.[13] In conclusion, we find that workplace networks increase the probability of running for office; however, the main effect may be explained by lower list nominations in the next mandate period.

Let us move on to time periods $t+1$ and $t+2$. The results can be seen in Table 2. For these two analyses, we modify the sample restriction such that the candidate can be a nominated candidate in t (dependent variable measured in $t+1$) and a candidate in both t and $t+1$ (dependent variable measured in $t+2$). The treatment variable remains the share of politician-colleagues in $t-1$. Interestingly, we now find effects further up on the party lists. In $t+1$, we estimate statistically significant effects for both the safe slot and the disadvantaged slot, together with a prevailing effect on the certain loss category. In $t+2$, we estimate a statistically significant effect in the safe category, the highly competitive category, and the certain loss category, although most of these coefficients are only significant on the 10% level.[14]

The results in Table 2 are intuitive, given the nature of political careers. It takes some time to advance within party politics, and a newly recruited party member is not immediately entrusted with a higher position. What our results demonstrate is that workplace networks function as a first stepping stone in a political career. Having politician-colleagues is a way into politics, which may lead later on to a more advantaged slot on the nomination list.

[12] We thank Olle Folke for providing us with code to run these calculations for list categories.

[13] We also run an analysis, shown in Table B1, where we change the outcome variable to elected politician. In line with our results for list position category in Table 2, we find much weaker evidences for positive treatment effect for this outcome.

[14] In Table B2 in the Appendix, we present the corresponding conditional logit results for list positions. For, time period t, the results are in line with those Table 2. However, for time periods $t+1$ and $t+2$, the estimated coeffcients are no longer statistically significant.

Table 2: List position categories as the dependent variable in t, t + 1, and t + 2

Panel A: t	(1) Top	(2) Safe	(3) Advantage	(4) Highly	(5) Disad.	(6) Cert.Loss.
Share politicians in t-1	0.00000	0.00001	0.00000	0.00000	0.00001	0.00006***
	(0.00000)	(0.00001)	(0.00000)	(0.00001)	(0.00001)	(0.00002)
Mean dep. var.	0.0000	0.0001	0.0001	0.0001	0.0001	0.0011
Panel B: t + 1	TopL1	SafeL1	AdvantageL1	HighlyL1	DisadL1.	Cert.LossL1
Share politicians in t-1	0.00001	0.00007***	0.00002	0.00000	0.00006***	0.00027***
	(0.00001)	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00004)
Mean dep. var.	0.0001	0.0006	0.0002	0.0003	0.0004	0.0027
Panel C: t + 2	TopL2	SafeL2	AdvantageL2	HighlyL2	DisadL2.	Cert.LossL2
Share politicians in t-1	-0.00000	0.00007*	0.00004*	0.00006**	-0.00003	0.00011*
	(0.00002)	(0.00003)	(0.00002)	(0.00003)	(0.00002)	(0.00006)
Mean dep. var.	0.0002	0.0011	0.0003	0.0005	0.0005	0.0040
Regression model	LPM	LPM	LPM	LPM	LPM	LPM
Individual*wp*occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	Yes	Yes	Yes	Yes	Yes	Yes
Individual covs t.	Yes	Yes	Yes	Yes	Yes	Yes
Mandate period FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation mandate trend	Yes	Yes	Yes	Yes	Yes	Yes
Workplace mandate trend	Yes	Yes	Yes	Yes	Yes	Yes
R2 panel A	0.464	0.490	0.479	0.481	0.481	0.473
Observations panel A	16337407	16337407	16337407	16337407	16337407	16337407
R2 panel B	0.582	0.648	0.530	0.514	0.522	0.615
Observations panel B	11230370	11230370	11230370	11230370	11230370	11230370
R2 panel C	0.707	0.721	0.563	0.547	0.545	0.673
Observations panel C	8390981	8390981	8390981	8390981	8390981	8390981

*Note: Standard errors in parentheses are clustered at the individual/workplace/occupation level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the value of 0 or 1. Columns 1–6 display LPM estimated coefficients.*

7 Disentangling the intermediate mechanisms

Let us now return to our discussion in Section 2 on intermediate mechanisms. As the mechanism tree in Figure 1 shows, we considered two broad mechanisms: a supply-side channel, through which contact with politicians at work lowers the costs of information for entry into politics; and a demand-side channel, where politicians at work actively recruit colleagues into their respective party.

Let us first consider whether the positive effects in Table 1 may be explained by an increase in the probability of running for the same political bloc as one's politician-colleague.

Partisan channel Swedish politics has historically revolved around two different blocs: a left-wing bloc and a right-wing one. Our subsequent analysis is based on the division between these two blocs. In the upper panel of Table 3, the outcome variable takes the value of 1 if the individual was nominated for the *left-wing bloc*, and 0 otherwise. In the bottom panel the outcome is similarly defined, but

Table 3: Mechanism analysis: Results by left-wing and right-wing bloc

Panel A	(1) Nom. CL	(2) Nom. CL	(3) Nom. CL	(4) Nom. CL	(5) Nom. CL	(6) Nom. CL
Share CR politicians in t-1	0.00011***	0.00002**	0.00002*	0.00003**	1.02562**	1.02059*
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.01081)	(0.01073)
Share CL politicians in t-1	0.00061***	0.00013***	0.00011***	0.00007***	1.03580***	1.03028***
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00633)	(0.00618)
Mean dep. var.	0.0011	0.0006	0.0007	0.0007	0.3414	0.3727
Panel B	Nom. CR	Nom. CR	Nom. CR	Nom. CR	Nom. CR	Nom. CR
Share CR politicians in t-1	0.00036***	0.00006***	0.00004*	0.00001	1.01766***	1.00945
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00609)	(0.00637)
Share CL politicians in t-1	0.00011***	0.00000	-0.00001	0.00000	1.00145	0.99268
	(0.00001)	(0.00001)	(0.00001)	(0.00002)	(0.00860)	(0.00946)
Mean dep. var.	0.0012	0.0007	0.0008	0.0007	0.3457	0.3776
Regression model	LPM	LPM	LPM	LPM	C. logit	C. logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2 panel A	0.000	0.384	0.411	0.469		
Pseudo-R2 panel A					0.002	0.130
Observations panel A	31995947	22917601	18162465	16337407	41609	34715
R2 panel B	0.000	0.390	0.416	0.490		
Pseudo-R2 panel B					0.000	0.151
Observations panel B	31995947	22917601	18162465	16337407	44255	36659

*Note: Standard errors in parentheses are clustered at the individual/workplace/occupation level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the value of 0 or 1. Columns 1–4 display LPM estimated coefficients. Columns 5 and 6 present odds ratios from conditional logit models.*

regarding nomination for the *right-wing bloc*. In both panels, we include two explanatory variables for the standardized share of politicians at work belonging to the left-wing bloc and the right-wing bloc, respectively.

Our overall conclusion from Panel A is that having both left-wing and right-wing politician-colleagues increases one's likelihood of being nominated for the left-wing bloc. However, in terms of size, the treatment variable for the share of left-wing politicians, representing within-partisan networks, is larger than the estimated coefficient for the share of right-wing politicians across all models. The pattern emerges in Panel B for right-wing nominations too: the statistically significant coefficients are generally found for the share of right-wing politician-colleagues and not for left-wing ones. In terms of size, the estimated coefficients are also larger for the share of right-wing politicians in $t - 1$.[15]

These results rule out the leftmost branch in the mechanism tree in Figure 1, where the entire

[15]Note that local political parties and the Sweden Democrats are excluded from this analysis, since they have not been categorized as falling into either the left-wing or the right-wing bloc. In Table C1 in the Appendix, we include the Sweden Democrats in the right-wing bloc, and we reach a similar conclusion as in Panel A in Table 3 here in the main text.

reduced form is explained by a supply-side channel where having politician-colleagues increases the probability that one will run for the other bloc. While there seems to be some weak evidence that people become politically engaged for the other bloc than the one represented at the workplace, this effect is smaller and less precisely estimated than the impact that having politician-colleagues has on running for the same bloc as those colleagues. All in all, these mechanisms point toward partisan political engagement at the workplace.

Let us continue now to the next part of the mechanism tree, and see whether we find evidence for an inbreeding bias.

Ability channel How can we define inbreeding bias, which stood at center in the discussion in Section 2 concerning Montgomery (1991)? In the section on our theoretical framework, we argued that individuals with similar productivity socialize with each other in the workplace, and that political parties are likely to recruit on the basis of referrals from high-productivity party members. It is important to note here that productivity at the workplace is hypothesized to function as a proxy for political productivity. One way of thinking about this is to assume that both productivity at work and productivity in politics are determined by a more latent variable.

There is no variable for productivity in Swedish registry data, so we need to find a proxy for it. We choose to focus on cognitive ability, which is a good predictor for success on the labor market (Lindqvist and Vestman 2011). Furthermore, cognitive ability is usually viewed as a latent factor that positively affects various aspects of a person's life. This data originates from the Swedish system of military conscription, which encompassed all men (with a few exceptions) up until 2009. A number of tests were carried out in order to sort conscripts into different military positions. One was a modified version of an IQ test, the overall goal of which was to measure the g-factor.[16] Women could enlist on a voluntary basis, but the women who chose to do so were probably not representative, so we only use the data for men. We complement the data on cognitive ability with the grade point average (GPA) from Swedish upper secondary school, which we argue is another proxy for cognitive ability. For cognitive ability, we standardize the measure with mean 0 and standard deviation 1 for each cohort in the data. For the GPA, we standardize the variable for each graduation cohort.[17] If we are to ensure

[16]For more information on Swedish military conscription, see Öhman (2015).

[17]There are some individuals in the data who enlisted more than once. For them we use the first available test result. As for the GPA, there were individuals who attended multiple programs in upper secondary school. One reason for this is that they

that these proxies address inbreeding bias, we need to relate them to the political party that eventually ends up recruiting new members. We define a high-ability variable as taking the value 1 if an individual (1) is nominated by a political party and if (2) this individual's ability is greater than the average ability of the other nominated members within this party. We define a low-ability variable as taking the value of 1 if an individual (1) is nominated for a party and if (2) this individual's ability is lower than the average level of ability within the party. Both of these variables take the value of 0 if conditions 1 and 2 are not fulfilled. It should be noted that we do not have data on cognitive ability or the GPA for all individuals that were included in our main analysis in Table 1. This is because the enlistment data starts in 1969 and the GPA data starts in 1973. Our mechanisms analysis is therefore run on a subset of the individuals.[18]

From these variables we generate two treatment variables: the standardized share of politicians at work who are of high ability on the one hand and of low ability on the other. We then include both of these treatment variables in a split-sample analysis. The results are presented in Table 4.

Let us begin by discussing the results in Panel A in Table 4. The outcome in this panel is being nominated and being of high ability. The estimated coefficients for the share of high-ability politicians at the workplace are larger than the estimated coefficients for the share of low-ability politicians across all specifications. However, the estimated coefficients for the share of low-ability politicians at the workplace are also statistically significant in most models. In Panel B, the results in part go against our hypothesized mechanism. In this case, the outcome is being nominated and being of low ability. In all of the models, the estimated coefficients are larger for the share of high-ability politician-colleagues than for the share of low-ability ones.

The results point toward an inbreeding bias in one dimension: high-ability individuals and low-ability ones are both more likely to be nominated if there are politicians at the workplace in $t-1$ that *are of high ability*. This could indicate that parties make use of referrals from high-ability party members. However, the inbreeding thesis presumes that high-ability members socialize with other

changed which program they attended, or they dropped out and then later returned. For these individuals we choose the latest available GPA, which we deem the best representative for success in upper secondary school.

[18]This construction of ability measures resembles the competence analysis conducted by Besley et al. (2017), who analyze gender quotas and their effect on the competence of politicians. These authors construct a competence measure with mincer-equation. Their purpose is to construct a competence measure under the assumption that voters prefer to be represented by politicians with a background similar to their own. Our measure of ability, which takes its starting point in Montgomery (1991) and the idea of an inbreeding bias along lines of ability, focuses directly on proxies for cognitive ability. Our measure and that provided by Besley et al. (2017) are likely to be highly correlated.

Table 4: Mechanism analysis: Results by high- and low-ability politicians

Panel A	(1) Nom high	(2) Nom high	(3) Nom high	(4) Nom high	(5) Nom high	(6) Nom high
Share low skill t-1	0.00014***	0.00005***	0.00003**	0.00003*	1.03444***	1.01558
	(0.00001)	(0.00001)	(0.00001)	(0.00002)	(0.01104)	(0.00989)
Share high skill t-1	0.00025***	0.00010***	0.00007***	0.00003	1.06450***	1.04251***
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.01555)	(0.01358)
Mean dep. var.	0.0008	0.0004	0.0005	0.0005	0.3397	0.3872
Panel B	Nom low.	Nom low.	Nom low.	Nom low.	Nom low.	Nom low.
Share low skill t-1	0.00009***	0.00003***	0.00002	0.00001	1.02954*	1.01974
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.01635)	(0.01240)
Share high skill t-1	0.00011***	0.00004***	0.00003***	0.00004**	1.04982**	1.03672***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.02037)	(0.01451)
Mean dep. var.	0.0008	0.0004	0.0005	0.0005	0.3494	0.3790
Regression model	LPM	LPM	LPM	LPM	C. logit	C. logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2 panel A	0.000	0.381	0.423	0.486		
Pseudo-R2 panel A					0.003	0.258
Observations panel A	31995947	22917601	18162465	16337407	29719	21919
R2 panel B	0.000	0.392	0.417	0.489		
Pseudo-R2 panel B					0.001	0.137
Observations panel B	31995947	22917601	18162465	16337407	26560	22248

*Note: Standard errors in parentheses are clustered at the individual/workplace/occupation level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the value of 0 or 1. Columns 1–4 display LPM estimated coefficients. Columns 5 and 6 present odds ratios from conditional logit models.*

high-ability workers. We also find that low-ability workers have a higher probability of being nominated in t if there are high-ability politician-colleagues within the workplace/occupation cell in $t-1$. A plausible explanation for our results is that parties consult high-ability members when recruiting, but that these members in turn recruit both high- and low-ability members at the workplace. This would be in line with the theoretical predictions in Mattozzi and Merlo (2015) where parties need low ability members to maximize the overall success for the party. As hypothesized in Mattozzi and Merlo (2015), this scenario is more likely in a proportional representation system, as the one we study in this paper.

In conclusion, these findings are partly in line with the notion that high-ability party members dominate political recruitment; however, they do not indicate that inbreeding along lines of ability dominates this mechanism. Another possible explanation for this is that high-ability party members use other heuristics for political productivity when recruiting – heuristics which are not captured by the variables we have our constructed for ability.

Supply-side or demand-side channel? Let us return now to the mechanism tree in Figure 1. As is often the case with mechanism analyses, we cannot definitively conclude that a certain channel is the one and only intermediate channel. However, we can rule out some channels. A pure supply-side channel (the leftmost part of Figure 1) is not likely, because we find evidence of partisan recruitment. Given that our results point in part to recruitment by high-ability party members, a demand-driven channel is likely to be operating. Since we also find that the probability of running for the other bloc than the one represented by one's politician-colleague is increased (although to a lesser extent than the probability of running for the same bloc), we cannot rule out that information costs have been generally reduced – meaning that a supply-side mechanism is operating as well. The most likely explanation for our main results in Table 1 is therefore that both supply- and demand-side mechanisms are in play, whereby having politicians at the workplace lowers information costs, at the same time that political parties recruit on the basis of referrals from (high ability) party members at workplaces.

8 Heterogeneity analysis: Political support

We have concluded, then, that there is a positive reduced form treatment effect, and we have discussed and analyzed the intermediate mechanisms. We turn now to heterogeneity analysis. Instead of focusing on a standard split-sample analysis for demographic subgroups, we ask a more fundamental heterogeneity question with regard to political support. Our results so far have demonstrated that having politician-colleagues at the workplace increases one's probability of running for office in the next mandate period. We have also found that a partisan channel is salient for understanding this reduced form effect. Let us now take this finding concerning a partisan mechanism one step further, and investigate if the effect is stronger or weaker depending on whether the politician-colleague represents the largest or the smallest political bloc in the municipality. We hope thereby to shed some further light on the mechanisms behind political engagement at the workplace. Given that we are investigating actual political support in the local legislature and not the electoral support in terms of votes, we focus on the mandates for each bloc in the municipal council.

We shed light on this heterogeneity dimension by running two regression model with interactions: One for the left-wing bloc and one for the right-wing bloc. First, we create a dummy variable taking the value 1 if the left-wing bloc has the plurality of the mandate in the municipal council where the individual resides. We then create a similar dummy variable for the right-wing bloc. These variables are then interacted with the share of left-wing/right-wing politicians in the workplace-occupation cell.[19]

The results are presented and discussed in Section D1 in the Appendix. We find that there is no heterogeneity in this dimensions for center-left politicians. However, for center-right politicians, we find some suggestive evidences that the partisan effect is driven by those cases where the center-right bloc has the upper hand in terms of mandates. In conclusion, there are indications that center-right political selection from workplace networks is more likely when it also yields more political power.

9 Discussion and conclusion

This paper has demonstrated that the workplace functions as a recruitment base for new politicians in Sweden. We find that having a workplace connection with an already active politician increases one's probability of running for office in the future. Moreover, we found that individuals that are recruited through workplace networks are placed higher up on the party lists in subsequent elections. We have shown as well that a partisan channel is the main selection mechanism, and that there is some evidence to suggest that political selection follows lines of ability.

These results contribute to the literature on political selection – e.g Besley (2005) – by demonstrating that politicians can be recruited in networks that are formed in adulthood – and not just earlier in life. It is also of interest that high-ability party officials are more prominent in recruiting from their workplace networks, which has the potential of changing the composition of the pool of nominated and elected politicians. Overall, our results demonstrate that the workplace could influence the composition of the Swedish political class, the members of which are able to affect which public policies are implemented.

If we consider the entire pool of citizens, adding a cue into politics that is not determined already at a young age is likely to broaden the sample of potential candidates as compared with a scenario where

[19] It bears noting that it is not an exogenous event whether a political bloc has a majority in the municipal council. A solution to this endogeneity problem could be to run a regression discontinuity design (RDD) in line with Lee and Lemieux (2010) using the seats shares as a running variable. There are however several obstacles involved in order to run such an analysis in relation to the heterogeneity analysis we have in mind. We have a discrete running variable with few mass points and we find unbalance in observables when implementing the local randomization approach discussed in Cattaneo et al. (2020). We therefore choose to implement a simpler interaction analysis and acknowledge that our results in this section could not be interpreted as the causal effect estimates, but rather whether the partisan effect is stronger or weaker depending on whether a political bloc has the plurality of the seats.

all politicians are selected from active members in political youth organizations. Furthermore, since these individuals also have a workplace connection, the conditions of working individuals are likely to become a more salient issue in policy-making in a scenario where politicians implement their preferred policy (i.e., in accordance with the last step in the citizen-candidate model in Besley and Coate (1997) and Osborne and Slivinski (1996)). Dal Bó et al. (2017) have found that Swedish politicians are more competent than the population as a whole. However, despite this positive selection into politics, personal connections and networks may still play an important role in determining political engagement. Our overall conclusion in this paper is that this is in fact the case.

Our findings add further to the literature on the importance of social networks. The earlier literature has in this regards foremost focused on networks that are formed outside the workplace and how these affects on the work activities (for instance Kramarz and Nordström-Skans (2014) who conducts an analysis on social ties for workplace entry in Sweden). Our focused has instead been on social ties at the the workplace and its consequences for out-of-work activities. Our findings demonstrate that the workplace and its social dimension are consequential not only for related labor market outcomes but also for the entry to the political arena.

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Appendix to "Workplace Networks And Political Selection"

Linuz Aggeborn and Henrik Andersson

A1 Robustness analysis for main results

In this section of the Appendix, we present various robustness checks for the main result presented in Table 1.

First, we run our most conservative LPM specification (column 4, Table 1) separately for five different groups of municipalities in Figure A1. This relates to our discussion in the main text regarding the choice of regression model between a LPM model and a conditonal logit model. In practice, the division is based on the population in the municipality, where <p20 represents the 20 % of observations (individuals and years) with the smallest population in their municipality of residence. Since the separation into municipality groups is based on the individual observations, all five quintiles will have about the same number of observations. It is clear from the figure that the effect becomes less and less economically significant as population increases. As a matter of fact, the effect diminishes almost proportionately with the average probability of being a politician in a given municipality and year. The conclusion is that our main findings are driven by smaller municipalities where the ratio between seats and inhabitants is higher.

To continue, we run a sub-sample analysis where we have excluded occupations that are highly political in nature, in order to ensure that our main results are not driven by those particular occupations. We exclude full-time politicians, lobbyists, high civil servants, elected representatives (not politicians), and PR consultants. The results are presented in Table A1, and they are in line with our main findings in Table 1. We have also run a sub-sample analysis where we exclude both the above-mentioned occupations and the five most common occupations among politicians. The results are presented in Table A2. The results are, yet again, in line with our main findings.

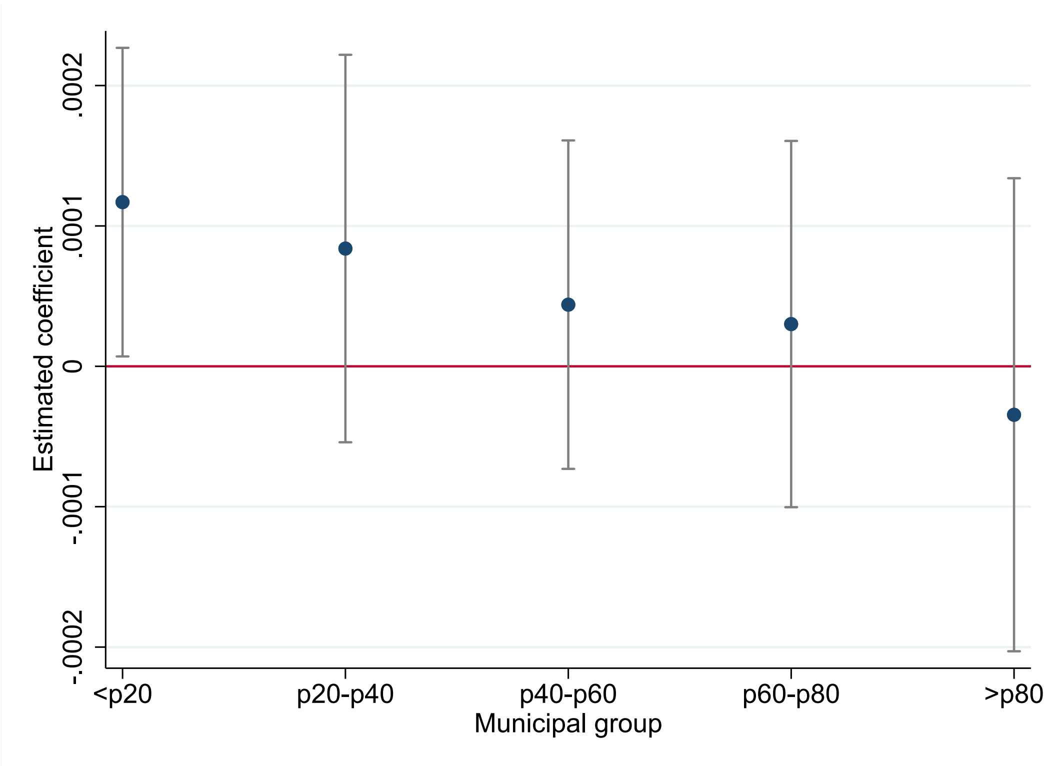
Next we have Table A3, where we have changed the treatment variable from the share of nominated politicians at the workplace to the share of elected politicians at the workplace. The results are in line with those of our main analysis. It is important here to remember that we end up with fewer politicians overall when we focus on elected instead of nominated politicians. Because the reghdfe command drops singletons in line with the discussion in Correia (2015), the analysis cannot be run for the most conservative LPM specification in column 4 in Table A3 because there are so few elected politicians at workplaces. Therefore, this column is blank in the table.

In Table A4, we exclude large values in our treatment variable. If the treatment variable – i.e. the share of nominated politicians within a workplace-occupation cell – is larger than the 95 percentile, we drop the entire individual from the analysis. Important to remember is that the distribution of the share of politician-colleagues is skewed to the right with many values equal to 0 and a few workplace-occupation cells with larger values. For columns 1 and 2 in Table A4, we still estimate positive and statistically significant effects. In columns 3 and 4, the estimates are no longer statistically significant, small, and the sign has flipped. Our explanation for these results are that the main effect is foremost driven by those workplaces where the share of politicians within the workplace-occupation cell is relatively large where individuals are likely to socialize on a more daily basis. When excluding those individuals, the results becomes more sensitive to the choice of regression model. To further investigate this issue, we also present an analysis where we have changed the treatment variable to the number of politicians instead of using the share of politicians. The results are presented i Table A5 and they are in line with the discussion above, namely that the the *share* of politician colleagues is the crucial factor for our results. The estimated coefficients in Table A5 are only statistically significant in the the naive model in column 1 and for the conditional logit results in column 6 (and it this case much smaller in comparison to the main effects presented in Table 1 in the main text). In conclusion, the treatment effect is dependent on having a rather high share of politician (either a small network or a larger network with many politicians). We should also briefly comment on the somewhat puzzling estimated odds ratios in columns 5 and 6 in Table A4 which are larger than equivalent models in Table 1 in the main text. The fact that the coefficients are larger means that the S-shape logit function provides a better fit in the middle of the distribution when larger values in the tail of the treatment variable are dropped. However, it is difficult to compare the odds ratios to the linear models in column 1–4 especially when larger shares have been dropped which we expect to be important for socialization within workplace-occupation cells.

In Table A6, we have run a number of placebo checks. We have changed the outcome variable from becoming nominated in the next mandate period to six labor market outcomes: labor income, disposable income, parental leave income, unemployment benefits, years of education, and college enrollment in $t-1$. Because we study individuals with a workplace connection, we focus on the intensive margin for labor income, disposable income, unemployment benefits (which could be in the form of part time unemployment) and parental leave income. Hence, we focus on actual positive values where the outcome variable is above 1000 SEK (approximately $100) in order to not focus on cases with very low values for these variables which should be considered close to 0 in practice. To compensate for inflation over the years, we first take the residual from a regression where labor income, disposable income, unemployment benefits, and parental leave income is run on a set of year dummies. These residual values are then logged to facilitate interpretation of the estimated coefficients. Years of education is expressed in levels and college enrolment is a dummy variable taking the value 0 or 1 in a given year in the panel. Looking at the estimated coefficients in Table A6, five of the six estimated coefficients are small and statistically insignificant. We estimate a statistically significant negative coefficient for college enrollment, but it is very small in magnitude. If the share of politicians at the workplace is increased by one standard deviation, the probability of being enrolled in college in t *falls* by approximately 0.03 percentage points. This is a zero effect in terms of economic significance.

In Figure A2, lastly, we assess whether our main findings are sensitive to the number of employees at the workplace. The further to the right we go in the figure, the greater the number of individuals we allow for within a workplace and occupation category. The overall conclusion is that our main findings are stable across these specifications. This is likely to to the fact that we have already included a large vector of fixed effects and covariates and the results are therefore not sensitive to workplace size.

Figure A1: Marginal effects, using LPM, stratified for municipality population size



*Notes:*The figure display the estimated coefficient and 95% confidence interval corresponding to the specification in column 4 in Table 1, i.e. the most conservative LPM-specification in the main analysis. Municipality size in quintiles by individuals and time: in other words the <p20 coefficient shows the analysis for the 20 % of individuals and years living in the population-wise smallest municipalities.

Table A1: Robustness: Excluding politician occupations

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Share politicians in t-1	0.00083***	0.00017***	0.00013***	0.00009***	1.03690***	1.02671***
	(0.00002)	(0.00002)	(0.00003)	(0.00003)	(0.00544)	(0.00555)
Mean dep. var.	0.0028	0.0017	0.0019	0.0018	0.3391	0.3674
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.382	0.407	0.478		
Pseudo-R2					0.001	0.132
Observations	30908730	22238755	17609544	15802598	108405	91474

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Same as main results in Table 1, but full time politician, lobbyist, high civil servant, elected representative (not politician) and PR-consultants are excluded from the estimations. The dependent variable is binary and takes the values 0 or 1.*

Table A2: Robustness: Excluding politician occupations and 5 most common occupations

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Share politicians in t-1	0.00075***	0.00014***	0.00010***	0.00006*	1.03309***	1.02164***
	(0.00002)	(0.00003)	(0.00003)	(0.00003)	(0.00596)	(0.00615)
Mean dep. var.	0.0026	0.0015	0.0018	0.0017	0.3377	0.3701
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.380	0.409	0.484		
Pseudo-R2					0.001	0.134
Observations	26777365	19179145	14902052	13228774	87659	72440

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Same as main results in Table 1, but five most common occupations and full time politician, lobbyist, high civil servant, elected representative (not elected politician) and PR-consultants are excluded from the estimations. The dependent variable is binary and takes the values 0 or 1.*

Table A3: Robustness: Elected politician instead of nominated as treatment variable

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Share elected pol. in t-1	0.00074***	0.00011***	0.00009***		1.01823***	1.01873***
	(0.00003)	(0.00003)	(0.00003)		(0.00478)	(0.00506)
Mean dep. var.	0.0029	0.0017	0.0019	0.0018	0.3385	0.3677
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.381	0.408	0.477		
Pseudo-R2					0.000	0.133
Observations	31995947	22917601	18162465	16337407	113189	94903

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 1. The regression model cannot be run for column 4, becasue there are too many singletons.*

Table A4: Robustness: Excluding large values in treatment variable

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Share politicians in t-1	0.00100***	0.00083***	-0.00022	-0.00002	1.80027***	1.36236**
	(0.00014)	(0.00020)	(0.00022)	(0.00029)	(0.25000)	(0.20282)
Mean dep. var.	0.0024	0.0015	0.0017	0.0015	0.3330	0.3654
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.374	0.404	0.496		
Pseudo-R2					0.000	0.145
Observations	27009713	19542870	15123948	13323540	85861	69933

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Table A5: Robustness: Number of politicians as treatment variable

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.
Number of politicians in t-1	0.00026***	0.00002	-0.00001	-0.00003	1.00106	1.00856***
	(0.00002)	(0.00004)	(0.00004)	(0.00006)	(0.00239)	(0.00251)
Mean dep. var.	0.0029	0.0017	0.0019	0.0018	0.3385	0.3677
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.381	0.408	0.477		
Pseudo-R2					0.000	0.133
Observations	31995947	22917601	18162465	16337407	113189	94903

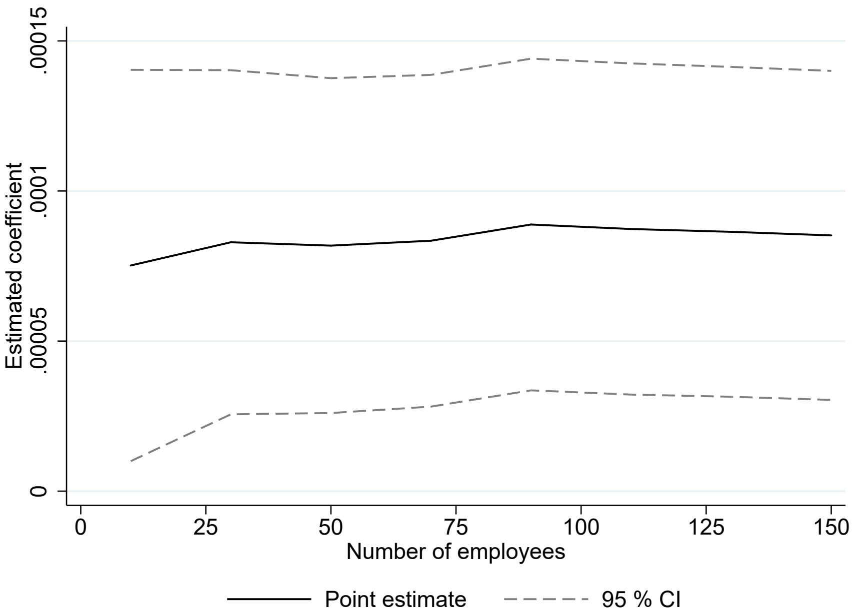
*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Table A6: Placebo outcomes

	(1) Lab.Inc	(2) Disp.Inc	(3) Par.Leave	(4) UnemBen	(5) Y.educ	(6) College.P
Share politicians in t-1	-0.00000	0.00000	-0.00000	-0.00000	-0.00010	-0.00029***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00030)	(0.00008)
Mean dep. var.	12.206	12.098	8.385	8.224	12.496	0.055
Individual*wp*occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	Yes	Yes	Yes	Yes	Yes	Yes
Individual covs t.	Yes	Yes	Yes	Yes	Yes	Yes
Mandate period FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation mandate trend	Yes	Yes	Yes	Yes	Yes	Yes
Workplace mandate trend	Yes	Yes	Yes	Yes	Yes	Yes
R2	1.000	1.000	1.000	1.000	0.958	0.604
Observations	12279936	15047200	1498207	297207	15188957	15188957

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Figure A2: Robustness: Main results for different number of employees



*Notes:*The figure display the estimated coefficient, together with a 95% confidence interval corresponding to the specification in column 4 in Table 1, i.e. the most conservative LPM-specification in the main analysis.

B1 Robustness analysis along the intensive margin

In this section, we shed some more light on the results on the intensive margin. In the main text in Table 2, we demonstrated that our main results where driven by nominations in the certain loss category in t (although we found effects for higher categories in $t+1$ and $t+2$). In Table B1, we change the outcome variable to becoming an elected politician in t. The conclusion is that the estimated coefficients are all positive, but they are smaller and less precisely estimated in comparison to the one in Table 1. This is in line with the conclusion in Table 2, where we demonstrated that the results where driven by the certain loss list position category in t. This result is hence not surprising, given that we focus on first-time nominated politicians in t in Table B1.

Next, we present sensitive checks for the list position results in Table 2 in the main text. Here in the Appendix, we present the corresponding results from conditional logit models in Table B2. For time period t in panel A, the results are in line with the LPM coefficients in Table 2, where the effect manifests itself in the certain loss category. However, for time periods $t+1$ and $t+2$, the estimated coefficients (here expressed as odds ratios) are no longer statistically significant. The likely explanation is that the conditional logit models are more data demanding. Furthermore, we cannot run the conditional logit models with all fixed effects that were included in Table 2, meaning that the results are not entirely comparable.

Table B1: Robustness: Elected as the dependent variable

	(1) Elect.	(2) Elect.	(3) Elect.	(4) Elect.	(5) Elect.	(6) Elect.
Share politicians in t-1	0.00018***	0.00002*	0.00001	0.00000	1.02177*	1.01789
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.01277)	(0.01459)
Mean dep. var.	0.0004	0.0002	0.0003	0.0003	0.3413	0.3724
Regression model	LPM	LPM	LPM	LPM	C.logit	C.logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.385	0.412	0.483		
Pseudo-R2					0.000	0.132
Observations	31995947	22917601	18162465	16337407	15454	12906

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

Table B2: Robustness: Conditional logit estimates for list position in t, $t+1$ and $t+2$

Panel A: t	(1) Top	(2) Safe	(3) Advantage	(4) Highly	(5) Disad.	(6) Cert.Loss.
Share politicians in t-1	1.05998	1.01237	1.03534	1.02480	1.02301	1.03098***
	(0.06961)	(0.02059)	(0.02737)	(0.02350)	(0.01874)	(0.00674)
Mean dep. var.	0.3517	0.3862	0.3639	0.3762	0.3761	0.3656
Panel B: $t+1$	TopL1	SafeL1	AdvantageL1	HighlyL1	DisadL1.	Cert.LossL1
Share politicians in t-1	1.05215	1.00711	1.01090	1.00483	0.98531	1.00474
	(0.03798)	(0.01370)	(0.02076)	(0.01708)	(0.01674)	(0.00668)
Mean dep. var.	0.3864	0.4180	0.3921	0.3918	0.3960	0.4081
Panel C: $t+2$	TopL2	SafeL2	AdvantageL2	HighlyL2	DisadL2.	Cert.LossL2
Share politicians in t-1	0.92843*	1.00922	1.01197	1.00245	0.97250	0.99883
	(0.04184)	(0.02162)	(0.01811)	(0.01940)	(0.01722)	(0.00751)
Mean dep. var.	0.4359	0.4483	0.4218	0.4170	0.4187	0.4385
Regression model	C.logit	C.logit	C.logit	C.logit	C.logit	C.logit
Individual*wp*occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	Yes	Yes	Yes	Yes	Yes	Yes
Individual covs t.	Yes	Yes	Yes	Yes	Yes	Yes
Mandate period FE	No	No	No	No	No	No
Municipal FE	No	No	No	No	No	No
Occupation mandate trend	No	No	No	No	No	No
Workplace mandate trend	No	No	No	No	No	No
Observations panel A	1086	5894	3190	4872	6649	56289
Observations panel B	1677	8100	4397	6560	8006	52960
Observations panel C	1693	7751	4111	5964	7227	43856

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

C1 Robustness analysis mechanism section

In the mechanism analysis in the main text, we ran an analysis where we investigated whether our reduced form effects could be explained by a partisan channel. The results were presented in Table 3. For that analysis, we did not include the Sweden Democrats in any of the two blocs. In Table C1, we include the Sweden Democrats into the right-wing bloc. The results are similar to those in the main text. The share of right-wing-politicians (including the Sweden Democrats) increases the probability of becoming nominated for any of the right-wing parties (including the Sweden Democrats). The share of left-wing politicians does not have an impact (although the point estimate in the naive specification in column 1 is statistically significant).

Table C1: Robustness: Center right bloc + Sweden Democrats

	(1) Nom. CR + SD	(2) Nom. CR + SD	(3) Nom. CR + SD	(4) Nom. CR + SD	(5) Nom. CR + SD	(6) Nom. CR + SD
Share CR + SD politicians in t-1	0.00036***	0.00006***	0.00005**	0.00001	1.02018***	1.01195*
	(0.00002)	(0.00002)	(0.00002)	(0.00002)	(0.00601)	(0.00644)
Share CL politicians in t-1	0.00012***	0.00000	-0.00000	0.00001	1.00159	0.99477
	(0.00001)	(0.00001)	(0.00001)	(0.00002)	(0.00793)	(0.00870)
Mean dep. var.	0.0014	0.0008	0.0010	0.0009	0.3378	0.3663
Regression model	LPM	LPM	LPM	LPM	C. logit	C. logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.382	0.407	0.483		
Pseudo-R2					0.000	0.142
Observations	31995947	22917601	18162465	16337407	56680	47567

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 1.*

D1 Heterogeneity analysis

The question we ask in this section is whether the effects that we found in the main text display some heterogeneity. We focus on the question if the partisan effect is larger if the politician-colleague represents the bloc which has the upper hand in terms of mandates on the municipal level.

We run two heterogeneity analyses: One for the right-wing bloc and one for the left-wing bloc. The focus is on partisan effects, which we know from Table 3 is present. To do this, we focus on one treatment variable so that the independent variable and the dependent variable concerns the same political bloc. The analysis take the form of an interacted regression model for each bloc separately, where three variables are included in each case. First we have the partisan treatment variable, which is the standardized share of center-left/center-right politicians in a workplace–occupation cell. We then include a dummy variable taking the value 1 if the center-left/center-right bloc is the largest political bloc in the municipal council and 0 otherwise.[20] Lastly, we include an interaction variable between the other two. The results are presented in Table D1 and Table D2. It bears noting that these interacted models are data demanding given that we also include many fixed effects.

Beginning with Table D1 and the analysis for center-left nominations, we find that the share of left-wing politicians in the workplace-occupation cell is statistically significant and positive, but the interaction term is close to zero and insignificant. Continuing with Table D2, we find the opposite: the probability of becoming nominated for the center-right bloc is not increased when having politician-colleagues if the center-right bloc is smaller than the left-wing bloc ($\texttt{CRplur.} = 0$). However, when the center-right bloc is the larger bloc ($\texttt{CRplur.} = 1$), then the interaction effect is positive and statistically significant for the less conservative regression models. This would mean that the main partisan effect found in Table 3 is driven by those cases where the center-right bloc has the majority in the municipal council. We do not want to overstate these findings given that the interaction terms in general are not statistically significant. However, the findings provide some (suggestive) evidence that center-right politicians are more prone to be politically engaged when it yields more political power.

[20] Fiva et al. (2018) argue that a given vote share in a municipal election could result is different seat share pluralities depending on the relative support of the different political parties and how mandates in municipal councils are allocated (a modified Sainte-Laguë-method is applied in Sweden). This may then result is different probabilities that the left-wing bloc or the right-wing bloc end up having an actual majority of the seats. We therefore choose to focus on seat shares for this analysis.

Table D1: Heterogeneity analysis center-left bloc

	(1) Nom. CL	(2) Nom. CL	(3) Nom. CL	(4) Nom. CL	(5) Nom. CL	(6) Nom. CL
Share CL politicians in t-1	0.00062***	0.00016***	0.00013***	0.00009**	1.04577***	1.03374***
	(0.00004)	(0.00004)	(0.00004)	(0.00004)	(0.01222)	(0.01217)
CL plur.	0.00007***	-0.00009***	-0.00003	-0.00005	0.82674***	0.91382**
	(0.00001)	(0.00002)	(0.00002)	(0.00004)	(0.03165)	(0.03913)
Share CL pol. in t-1 * CL plur.	0.00000	-0.00004	-0.00002	-0.00004	0.98625	0.99770
	(0.00005)	(0.00005)	(0.00005)	(0.00005)	(0.01215)	(0.01286)
Mean dep. var.	0.0011	0.0006	0.0007	0.0007	0.3511	0.3743
Regression model	LPM	LPM	LPM	LPM	C. logit	C. logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.392	0.412	0.470		
Pseudo-R2					0.003	0.136
Observations	29460594	20440954	17431864	15633068	37731	32858

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 1.*

Table D2: Heterogeneity analysis center-right bloc

	(1) Nom. CR	(2) Nom. CR	(3) Nom. CR	(4) Nom. CR	(5) Nom. CR	(6) Nom. CR
Share CR politicians in t-1	0.00034***	0.00002	-0.00000	-0.00003	1.00736	1.00170
	(0.00002)	(0.00003)	(0.00003)	(0.00003)	(0.00862)	(0.00803)
CR plur.	0.00041***	0.00007***	0.00015***	0.00016***	1.12181***	1.06411
	(0.00001)	(0.00002)	(0.00002)	(0.00004)	(0.03902)	(0.04195)
Share CR pol. in t-1 * CR plur.	0.00006	0.00008**	0.00009**	0.00008*	1.01831	1.01728
	(0.00004)	(0.00004)	(0.00004)	(0.00004)	(0.01167)	(0.01186)
Mean dep. var.	0.0013	0.0007	0.0008	0.0007	0.3548	0.3792
Regression model	LPM	LPM	LPM	LPM	C. logit	C. logit
Individual*wp*occupation FE	No	Yes	Yes	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes	No	Yes
Individual covs t.	No	No	Yes	Yes	No	Yes
Mandate period FE	No	No	Yes	Yes	No	No
Municipal FE	No	No	Yes	Yes	No	No
Occupation mandate trend	No	No	No	Yes	No	No
Workplace mandate trend	No	No	No	Yes	No	No
R2	0.000	0.396	0.417	0.492		
Pseudo-R2					0.001	0.153
Observations	29460594	20440954	17431864	15633068	40920	35239

*Note: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 1.*

E1 Nickell bias

Our aim in this paper is to estimate the effect of having a politician at the workplace in $t-1$ on the probability that an individual becomes a politician in t. To pinpoint the effect and to purge the estimations from selection bias, we want to include fixed effects. The problem in this case is that the variable of interest on the right-hand side is a lagged variable which is related to the dependent variable.

This discussion relates to the conclusion reached by Nickell (1981). This paper shows that, in a dynamic panel data model with fixed effects, there will be a bias in the estimates if the number of time periods does not go to infinity.

However, the set-up in our paper is not the equivalent of a dynamic panel data model with a lag of Y. Instead, our variable of interest is the share of politicians at the workplace in $t-1$. Judson and Owen (1999), furthermore, have shown that the bias is above all present for estimated coefficients for lagged Y, whereas the biases for other included variables are small even when T is small.

Table E1 displays a simulation of the Nickell bias for our set-up. We generate 40,000 observations, and we assume a true causal effect equal to 0. We furthermore assume there are 2 % politicians in the population. Column 1 displays the expected null association between the share of politicians in t-1 and the probability of becoming a politician in t. The inclusion of individual-fixed effects does not substantially change this estimate (Column 2). The problems arise in Column 3–Column 6. Here we include fixed effects for workplace and occupation. The results in Column 3 – Column 6 suggest that, by including these fixed effects, we estimate a negative coefficient, even when the true causal effect is 0. However, this negative coefficient is estimated when only an interacted workplace- and occupation-fixed effects are included (Column 3); when an interacted workplace- and occupation-fixed effects together with individual-fixed effects are included separately (Column 4); and when an interacted workplace- and occupation-fixed effects, individual-fixed effects, and year-fixed effects are included (Column 5).

In Column 6 and Column 7, we interact workplace- and occupation-fixed effects together with individual-fixed effects. In essence, the only identifying variation we use is the timing of an individual's entry into politics. In this case, we find no evidence of any Nickell bias. This is also the strategy we use in our empirical analysis in the paper.

Table E1: Simulation of Nickell-bias

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.	(5) Nom.	(6) Nom.	(7) Nom.
L.share1	-0.0025	0.0001	-0.2489***	-0.2464***	-0.2591***	-0.0319	-0.0294
	(0.0515)	(0.0515)	(0.0592)	(0.0592)	(0.0602)	(0.0569)	(0.0578)
Individual FE	No	Yes	No	Yes	Yes	No	No
FirmOccupation FE	No	No	Yes	Yes	Yes	No	No
FirmOccupationFE*Ind	No	No	No	No	No	Yes	Yes
Year F.E.	No	No	No	No	Yes	No	Yes
R2	0.000	0.003	0.003	0.006	0.006	0.318	0.318
Observations	39211	39211	39211	39211	39211	39211	39211

Note: The table displays results from simulations. The true causal effects is assumed to be 0 and we simulate a 2 % share of politicians. We restrict the sample so that the individual cannot be a politician in $\mathtt{t}-1$ *Standard errors in parentheses . **** $p < 0.01$*, *** $p < 0.05$*, ** $p < 0.1$*.*

F1 Data and code availability statement

The data we use for our empirical analysis in this research project consists of Swedish administrative data that are stored at Statistics Sweden. There are various rules governing how this data must be handled and stored. All of the data sets we use are made available to our research group at a secured server which we have to log into in order to run our empirical analyses. For this reason, we cannot make the data freely available online for replication purposes.

There are two ways for other academics to replicate our empirical findings. The first is to order the exact same data that we have used from Statistics Sweden. Please follow this link for more information:https://www.scb.se/en/services/guidance-for-researchers-and-universities/). Before such an order can be processed, the researcher needs permission from the Ethical Review Board to process this kind of data.

The second way to replicate our findings is to use the same secured remote desktop system that we have used. A researcher interested in this option needs to contact us, since we need permission from the Ethical Review Board to add a researcher to our research group on a temporary basis. Please note that there are geographical restrictions on where one can log into the remote desktop system.

We will make all do-files available upon request.

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