

Political Recruitment At Work*

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Linuz Aggeborn[†]

Henrik Andersson[‡]

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Abstract

We investigate whether there is recruitment into politics from workplace networks with Swedish registry data from 2002–2018. Most politicians in Sweden are leisure-politician, meaning that they are employed at regular workplaces in addition of being politicians. In order to identify the causal effect, we restrict the analysis to small cells of individuals that work in close proximity at the same workplace and with the same profession. Our results indicate that an individual is more likely to become a politician if that person had a colleague that was a politician in the previous mandate period. Our mechanism analysis indicates that partisan recruitment may explain the main effect and we also find some evidences for the existence of inbreeding-bias in workplace networks.

Keywords: Political candidacy; Workplace Networks; Sweden

JEL-codes: D72, J01, J16

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[†]Department of Government at Uppsala University, Uppsala Center for Fiscal Studies (UCFS) and Uppsala Center for Labor Studies (UCLS).

[‡]Department of Government and IBF at Uppsala University, UCFS and UCLS.

1 Introduction

Individuals spend a considerable part of their lifetime at work. The main reason is that our livelihood often depend on it: In order to consume, an individual needs to have an income, which for most people is secured by being employed at a workplace. However, the workplace also includes a social dimension, meaning that peers interact and form social networks. These networks can develop during daily work related duties as well as during social activities between tasks and 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. Jackson (2010), Granovetter (2005)) for individuals, firms and other relevant actors.

Previous research on labor market networks has analyzed social networks outside the workplace as cues for recruitment of new workers *into* the workplace (Montgomery, 1991; Kramarz and Nordström-Skans, 2014). However, research on the effect of networks at the workplace – particularly regarding effects on *out-of-work activities* – remain scarce. Our paper will begin to fill this gap. We focus on *political activities outside the workplace*, and ask whether there is evidence that at-work political networks affect recruitment into politics in Sweden.

The primary purpose of the paper is to investigate if there is an effect of workplace networks on the probability of becoming a political candidate in a subsequent election. Second, we discuss and study empirically the intermediate mechanisms to shed light on how workplace networks affects political candidacy in more detail. All in all, the theoretical discussion and the empirical result in the paper provides insights on the link between the labor market and the political arena.

We identify the effect of having a politician co-workers on entry into politics by using changes in occupation *and* workplace composition of coworkers for a given individual over time. This strategy, which relies on a multitude of fixed effects, is only possible due to very detailed registry data from Sweden. Sweden is also a suitable case given that both men and

women work outside of home to a large extent and that there are many politicians in Sweden that have regular jobs and hence a workplace connection. With the exception of parliamentarians and a few full-time politicians on the local level, most Swedish politicians are leisure-time politicians, meaning that the large majority of them are also employed workers. We have data from 2002–2018 with indicators for workplace and profession which allow us to pinpoint smaller cells of workers, who are likely to have social interactions on a daily basis. This data is combined with political candidacy data where we observe all individuals that ran for, and were elected to, political office for the same time period.

Our paper adds to several strands of literature. First and foremost, we add to the so far small literature on the effect of workplace networks on out-of workplace activity. Closest to us is [Nanda and Sørensen \(2010\)](#), who find that social networks established at the workplace may also benefit employees after leaving the actual workplace, specifically considering entry into self-employment. Our paper is also connected to papers on peer-effects among workers on other workers' performance, given by for example wages and innovation ([Bandiera et al., 2010](#); [Cornelissen et al., 2017](#)). [Nix \(2020\)](#) estimates a positive impact of colleagues level of education on the wage level with Swedish registry data, which would indicate that learning, and information, spillovers within workplace networks exist.

There is furthermore an emerging literature on workplace ties and their effects on future political careers, but the evidences 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 has an earlier workplace connection, school connection, or hometown connection with a member that retires. The authors finds no effect of workplace connections and negative effects of school and hometown connection, which is interpreted as competitions within fractions in the Chinese Communist Party. However, [Jia et al. \(2015\)](#) find that provincial leaders are more likely to be promoted if they have a earlier workplace connection within the central government if they have also

delivered economic growth in their district.¹ Our paper contributes to this literature on the connection between workplaces and politics, but with empirical evidences from a democratic country, for which the incentives and rules are quite different from China.

Lastly, we add to the massive literature on political socialization. Earlier research have demonstrated that political socialization foremost takes place within the family during childhood (Jennings, 2007; Sears and Brown, 2013). In this paper, we focus on another period, namely adulthood. Workplace recruitment into party politics 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 there is someone engaging and encouraging the person in question to become involved. While a large number of papers have taken an interest in the connections between social networks and political participation (Lim, 2008), the focus has generally been on strong ties, such as family, as opposed to the workplace context. A couple of papers do focus on workplaces. Adman (2008) empirically test with Swedish survey panel data whether workers who are employed at a workplace where decision making is more democratic has a higher political participation, but find no support for this claim. Mutz (2002) argues that being part of a network may decrease political participation if people in the network, such as coworkers, have conflicting political views. The question is related to the discussion on whether strong social ties, such as family connections, where individuals are more similar have a larger impact on an individual's political behavior than weak social ties where individuals are more likely to encounter more heterogeneous political opinions. Folke et al. (2021) demonstrates with registry data that Swedish politicians have different professions and that their labor market experience is associated with their future ideological position along the economic left–right dimension and the libertarian–authoritarian

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

²For evidence regarding the intergenerational transmission of beliefs and political affiliation in Sweden, see Westholm (1991) and Ageborn and Nyman (2021). Folke et al. (2017) analyze how family connections to politicians in Sweden affect rents and find evidence that children of politicians earn more. However, they conclude that this is likely the results of children remaining in the municipality and postpone their enrollment in higher education.

spectrum. Their paper is an empirical test of the theory presented in [Kitschelt \(1994\)](#) on the labor market foundations of party choice. Our paper is connected to this discussion, but our purpose is instead on labor market networks and not on specific professions and workplaces.

Generally, addressing the impact of workplace networks on political engagement sheds light on the overall importance of workplace networks for outcomes besides employment recruitment or other labor market outcomes. In addition, the question of how individuals are recruited into politics is an important representational question shared by any democracy. Given that the workplace is one of our most important social arenas, the understanding if, and how, political activity is formed at work should be of great importance. Previous research has also highlighted the workplace as a particularly interesting political arena, where employees engage in political discussions with people not holding similar views as themselves ([Mutz and Mondak, 2006](#)).

Our study arrives at two conclusions. First, we find evidence of at work recruitment into politics. Our preferred specification suggest that a standard deviation increase in politicians at the workplace increases the tendency to become a politician with 10 % percent compared to the overall tendency to become a politician. Second, the mechanism behind the main effect effect is inherently partisan. An individual with left-wing (right-wing) politicians at work has a higher tendency to become a left-wing (right-wing) nominated politician themselves. We also find some evidences that there seems to be inbreeding bias where high ability individuals are more likely to become politicians if there are politician colleagues that are also of high ability. This conclusion opens up for the possibility that individuals are recruited into politics by politician colleagues.

The rest of the paper is organized as follows. We first present a theoretical framework for the research question at hand where we discuss potential theoretical mechanisms followed by a description of the Swedish institutional setting. After this we present the data material and empirical framework followed by results, mechanisms analysis, and heterogeneity analysis. The paper ends with a discussion and conclusion.

2 Theoretical framework and mechanisms at play

In this section, we sketch a theoretical framework for why, and how, political engagement at the workplace takes place. Broadly we can think of two channels, which we label as a supply and a demand side channel. In short, for the supply side, the main idea is simply this: Coworkers may provide information cues to the political sphere, which lowers the cost of running for office and hence increases the supply of political candidates. Also, the same coworker has more information concerning the productivity of the potential new candidate, meaning that the political parties (demand side) are better of recruiting such an individual in comparison to an individual picked at random.

Supply side Starting with the supply side, it is a fact that only a small part of the population run for political office. In order to understand why a person takes the effort to run for office – and not just settle by voting for a specific candidate – it is important to analyze the incentives for entry into politics. One suitable point of departure on how to model entry into politics is the Citizen-Candidate model (Osborne and Slivinski, 1996; Besley and Coate, 1997). According to this model, the choice to run for office is endogenous. In the first stage of the game, a citizen decide whether to become a candidate, which depends on the policy position of other candidates, the other candidates' capability of enacting these policies, and the cost of entry. Examples of entry costs are information gathering on the different parties and the establishment of contacts with someone that is already engaged. Thus, a person takes the step to become a candidate, because it it utility maximizing.

We argue that the latter part of the decision of go from being a citizen to become a candidate is less costly when someone has a workplace connection to a politician. Such a personal acquaintance could provide the necessary information to the political party represented by the coworker, which results in being enrolled in the same political party (although active recruitment does not take place). However, it is also possible that the

workplace-politician in general provides information about political engagement, which lowers the cost of information gathering on a more general level. In this case, it is possible that having a politician colleague increases the probability of becoming a candidate for any political party, but not necessarily the same party as the colleague in question. In the upcoming empirical analysis, we separate between these two different mechanisms.

A similar line of thought is found in the political science literature, where Verba et al. (1995) argue that the most likely reason for why a person is not involved in politics is “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 is in proximity to a politician at the workplace, because of the social networks at the workplace and the interactions between colleagues. In addition to information costs, a politician co-worker may also create a socialization environment for political recruitment. This may take the form of political discussion, which fosters a political interest among those not already engaged.³

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

Let us assume the existence of office-motivated political parties with the objective function to win political elections. The political parties consists of party members, which are essentially citizens that have decided to run for office, where the pay-off function of political parties equals the productivity of their individual party members. We interpret productivity in terms of political productivity, by which we mean gaining votes in elections. In practice however, we are going to assume that political productivity is a function of, and well approximated by workplace productivity. One way to interpret this is that both productivity aspects are the result of initial endowments and over-the-life-time investment in human capital. Cognitive

³Note that the earlier literature on political socialization has foremost been concerned with early life socialization and socialization during the *impressionable years* (Jennings, 2007; Campbell et al., 1980; Sears and Brown, 2013). Our paper is related to this literature, but we consider a socialization arena that has not been analyzed extensively before, namely the workplace. It is also a different kind of socialization taking place in adulthood.

ability is one good examples, which are determined both by nature endowment (genes) and by human-capital investments (for example education).

Furthermore, there is information asymmetry since the political parties cannot observe the political productivity of non-member citizens. From the political parties' point of view, all new potential party members are observationally equivalent. Political parties must however decide on whether to nominate new members to political office before observing the new member's political productivity.

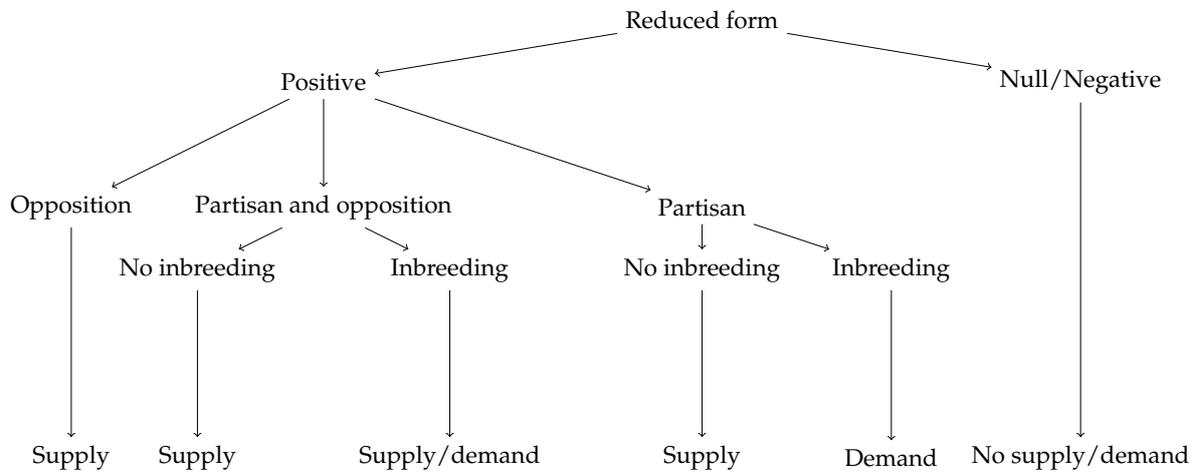
In line with with [Montgomery \(1991\)](#), we argue that there is an *inbreeding bias* among workers where high-productivity workers are more likely to have social ties to other high-productivity workers and vice versa. It is therefore utility maximizing for the political parties to recruit by referrals from high-productivity party members. This line of thought is also closely related to the argument in [Brady et al. \(1999\)](#) who model recruitment of political activists. They argue that politicians have incentives to gather information on activists' potential – or productivity if we keep the economic terminology. [Brady et al. \(1999\)](#) further argue that recruiters have incentives to use cues for political potential.

Demand or supply side? In our mechanisms analysis later in the paper, we will try to separate between these intermediate channels. However, it should be noted that a definitive test that can rule out one channel over the other is difficult construct. In Figure 1, we have summarized the different intermediate channels and to which channel specific results in the mechanism analysis would point towards.

The most clear-cut result would be if we find a null or a negative reduced form result. In this case, there there is no recruitment going on from workplaces to the political sphere and no lowering of entry cost. As such, there could neither be a supply nor a demand channel present. This result is illustrated in the rightmost branch of the mechanism tree in Figure 1. Given our reasoning above, we do not think such a result is likely.

Under the assumption that we find a positive reduced form where the number of

Figure 1: Mechanism channels



co-workers that are politicians increases the probability of becoming a candidate in the future, either a supply or demand channel may explain the result. It is also possible that a supply and demand channel operate simultaneously. Let us discuss each branch of the mechanism-tree at the time.

Let us begin to 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 the co-workers, we may rule out a recruitment mechanism. Instead, the likely explanation is a pure supply side effect where the politician co-worker has lowered information costs for entering the political arena in general.

If we instead find that having a politician colleague increases both the probability of running for the same political party as the 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 evidences of a inbreeding bias, a recruitment demand driven mechanism is very unlikely since this channel presupposes that parties try to recruit by referrals. Again, a supply channel is therefore the most likely explanation for such a result. On the other hand, if we find support for inbreeding-bias we cannot rule out a demand side

mechanism. However, since we have also found that the probability increased both for running for the same political party as well as the opposition in this branch of the tree, we may not rule out a supply side mechanism either. Instead, the most likely explanation is a combination of a supply and a demand side mechanism.

Let us now turn to the last part of the mechanism-tree. We now focus on the scenario where we find a positive reduced form and that this reduced form is driven by an increase in the probability of running for the same political party as the co-worker. Which channel is then most likely? Again, it depends on whether we find 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 on his or her political party, but there is no recruitment going on. If we find a inbreeding bias however, a demand side explanation is the most likely channel since we have then found that co-workers run for the same political party and that the likelihood is increased if the politician colleague and the soon-to-become new candidate are both of high productivity. 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 years old were employed in Sweden (SCB, 2018), which means that a considerable part of the Swedish population spends a considerable amount of time at a workplace. This mere fact makes the workplace context important to study as a political recruitment arena. We have access to information on where an individual is employed and detailed information on which occupation within that workplace the individual belongs to. This enables us to pinpoint small cells of workers that are likely to spend time together.

Politics in Sweden Our primary interest is whether there is recruitment into

politics which we study by focusing on the general elections in Sweden. Elections take place through a closed list PR-system to three different levels of government on a single election day every fourth year: The national parliament, the county councils and the municipalities. A person needs to be over 18 years old to vote and to run in an election. There are eight political parties in Sweden represented in the national parliament: The Left Party, The Social Democrats and The Green Party, which together are considered to be 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. There is also the Sweden Democrats that entered the national parliament in 2010, which has historically not been included in either the two traditional blocs. In recent years, the Sweden Democrats has however steadily positioned themselves as a right-wing populist party. As a consequence, the left versus right partition has faded slightly in Swedish politics, mainly due to the Sweden Democrats. During the 2018-2022 mandate period, a minority government led by the Social Democrats and the Green Party were made possible due to a deal with the the Liberal Party and the Center Party over economic policy (a deal which broke down in June 2021 resulting in a fall of the government). [Dal Bó et al. \(2017\)](#) have previously demonstrated that Swedish politicians are representative to the general populations in terms of class background, but that they are more intelligent than average. This finding is important for the design of our empirical specification given that more intelligent individuals are likely to work at specific workplaces. We return to this issue in Section 4

In order for a candidate to be elected, the position on the party list is important because the position on the list determines – together with the vote share for the political party – the number of people on the list that receive a seat. Those on the list who are not elected become substitutes for the elected candidates and it is also common that substitutes serve on various subcommittees (nämnder). The process of determining the party list varies for different parties. Some use a primary election system whereas other parties decide by local boards. The electorate also has the possibility to cast a voluntary personal preferential vote in the general

election for one candidate on the chosen party list. A candidate receiving more than 5 % personal votes in the election district is moved up and will become elected regardless of the initial list position (given that the political party receives representation).

4 Data and empirical model

Empirical universe We use data from Swedish administrative registers where the first year in our panel is 2002 and the end year is 2018. This means that we study five political elections – in 2002, 2006, 2010, 2014 and 2018 - in total. You have to be over 18 years old in order to vote or stand as a candidate in an election, and most Swedes have retired at age 67. 18-67 is thus the age span for the included individuals since we need to have information on whether they are candidates and information on their place of work. Also, since our panel estimations will make use of information specific for occupations and workplaces over time, we require an individual to be employed with a registered workplace for at least two time periods.

Outcome variable Our main analysis focuses on nominated politicians, i.e. all individuals that have ever run for office for the municipal council, the county council or the national parliament. Being a politician is relatively uncommon and we want to increase the data sample, meaning that nominated rather than elected politicians are more suitable for our analysis. Nominated politicians are also involved in party politics even if they are not elected. It is also common that an elected politician resign during the mandate period, meaning that a nominated substitute politician next in line on the party list, but not elected at the election day, becomes the serving politician for the remainder of the term. Given that our research question is about how recruitment into politics takes place at the workplace, nominated politicians are also likely to be important as a first-step political enrollment of new party members that in the future may become elected politicians.

The outcome variable is defined as a dummy variable, which is equal to 100 if someone is nominated as a politician and 0 otherwise (to facilitate interpretation). Note also that we have a strong prior that there is considerable incumbency advantage in politics. We therefore restrict our entire analysis to individuals that themselves have not been nominated previously in the panel.

Treatment variable: Workplace networks To increase our likelihood of individuals actually interacting with each other, we make use of the share of nominated politicians at the same *workplace* and *occupation*. A workplace is in our data defined as a coherent property or address, within the same firm, and is therefore smaller in size than the average size of a company. The median workplace has around 45 employed workers, but the number varies from only a few to several hundred, or in a few cases several thousand employees. Furthermore, we expect many workplaces to share a lack of vertical integration. We therefore make use of the International Standard Classification of Occupation (ISCO-codes).⁴ We use 3-digit codes, which is a separation of workers into different occupational categories. The combination of workplaces and occupations means that we may separate, for example, between researchers and administrative staff that are both employed at the same university. 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.

Identification strategy in main analysis The goal of this paper is to estimate the effect of having a politician co-worker 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 a high nascent political ambition sort

⁴The occupation codes originate from the *Lönestrukturstatistiken* whereas the workplace indicators come from *RAMS*. The occupational codes cover the entire population for public employees and private employees at larger private firms whereas employees at private workplaces with under 500 workers are surveyed from a representative sample. This means that occupational codes are carried over to following years for some workers if they have not been surveyed.

into specific workplaces that match their societal interest (Fox and Lawless, 2005). We also know that politicians in Sweden are more intelligent than the general population (Dal Bó et al., 2017). This further highlight that politicians are not likely to be randomly distributed either across workplaces or occupations.

The ideal empirical strategy would be to randomly assign the number of politicians to specific occupations and workplaces in time period $t - 1$ and then estimate the partial average treatment effect of having more politician coworkers on the extent to which an individual become a nominated politician in t .

Naturally, 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 for the number of politicians they interact with at the workplace within a occupational category in $t - 1$.

Our strategy for the main analysis mimics Cornelissen et al. (2017), in which the authors make use of 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 workplace peer quality, many of the obstacles regarding endogeneity, for example the selection of certain individuals into specific workplaces, is shared between the two settings. Our strategy will therefore be to study the effect of occupation and workplace specific coworkers who were politicians in the current period, on the probability of an individual to enter politics in the following mandate period. The identification strategy make use of the timing in becoming a politician among those individuals that eventually becomes a politician at some point in the panel. Essentially, identification comes from workers who stay within the same cell of a workplace *and* occupation, within which the share of politicians changes over time.

Regression equation we estimate the following model in our main analysis:

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

Where subscripts include i for individual, t for mandate time period, w for workplace, o for occupation and m for municipality. Nom_{itwom} is the dependent variable which is a dummy variable, taking the value 100 if an individual becomes a nominated politician in t and 0 otherwise. u_{itwom} is the error term.

$X_{t-1,twom}$ is the treatment variable of interest measuring the standardized share of politician at the workplace within a occupational category (3 digit SSYK code) in $t - 1$.

Crucial from an identification point-of-view is W_{iwo} , which is a grouped individual, workplace and occupation fixed effects. By including this fixed effect, we restrict the empirical analysis to individuals that work at a specific workplace in a specific occupation. The identifying variation thus stems from those individuals that sometimes during the panel switch from not being a politicians to becoming a politician. In essence, we are using the timing in the panel to estimate the effect.

Z_{itwom} is a vector of time changing individual covariates, such as years of education and standardized income for the individual in time period t . It is possible that the share of politicians is bundled with other socioeconomic characteristics at the workplace-occupation level. We therefore include $E_{t-1,twom}$, which is a vector consisting of the means for individuals at the workplace-occupation cell in a in $t - 1$ for years of education, the share of females, the share of immigrants and the average income level.

Next, we include mandate period fixed effects τ_t , to compensate for general time differences in the probability of becoming a politician. We further include specific mandate period time trends for each workplace, ϕ_{wt} to control for the possibility that certain workplaces becomes more popular among politicians over time. We also include occupation time trends ϵ_{ot} so compensate for that certain occupation attracts politicians over time.

Lastly, we include municipal fixed effects for the municipality of residence, M_m , because the possibility of being nominated varies over Sweden. While it is relatively easy to be nominated in a small municipality, there are more fierce competition in the larger cities. Standard errors are clustered at the workplace occupational mandate period level which equal

the level of treatment.

The multitude of fixed effects used pose the question: what variation is left? For a given worker, such a change may happen due to a politician being hired into or dropping out of the same occupation and workplace cell. The share of politicians can also change from other the composition in the workplace occupation cell changing. For example, if the number of non-politician coworkers in the same cell are decreased, the share of politician coworkers in the same cell will mechanically increase. This serves an intuitive logic: If the *share* of politicians at the occupation and workplace cell increases from 1 of 7 to 1 of 3, chances are the worker employed at the same occupation and workplace cell will have more interactions with the politician in question.

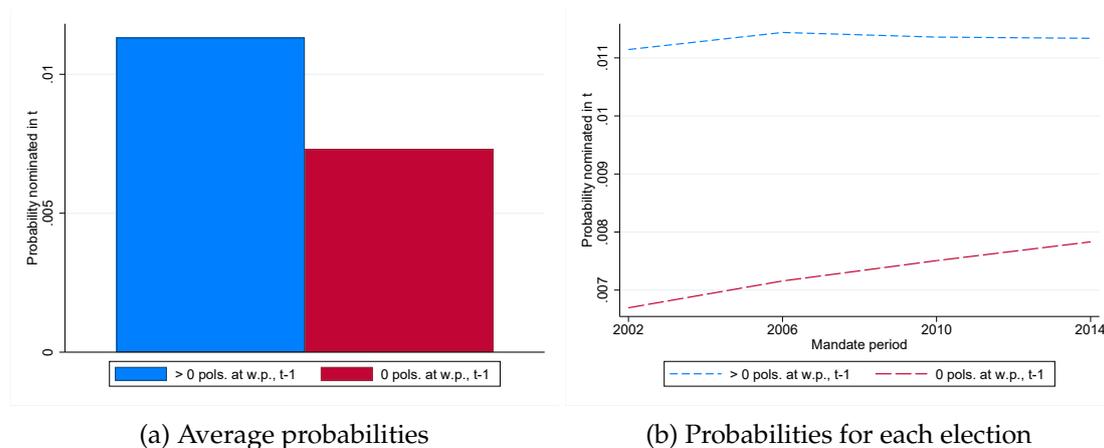
The fact that we are using fixed effects together with a lagged variable of interest connected to dependent variable means that we risk running into bias in line with the conclusion in [Nickell \(1981\)](#). We discuss this further in the online appendix and demonstrate in Section E1, by running simulations, that such bias is present if we include individual, workplace and occupation fixed effects separately. When including the interacted fixed effects (and hence using the timing to estimate the effect), the Nickell-bias goes towards 0. For our preferred specification, the problem of nickel bias is small.⁵

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 $t - 1$. Evident from Figure 2 is that there is a substantial descriptive difference between these two groups which provides an interesting starting point for our upcoming analysis. Figure 2 also highlight an important aspect, namely that relatively few individuals end up being a politician

⁵We use a large set of fixed effects and we are clustering the standard errors. In the presence of singletons within the fixed-effects groups, this may be problematic. We therefore follow [Correia \(2015\)](#) and the `reghdfe` command which drops singletons.

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



Notes: The figures display the difference in probabilities of being nominated in time period t depending on whether you have politician colleagues at the workplace in $t-1$ or not.

in absolute numbers. Even for the group where there are colleagues that are politicians, a little more than 1 percent becomes politicians themselves. This is not surprising because the number of available seats in political assemblies are few in comparison to the total population in a representative democracy. The future interpretation of the estimated coefficients needs, however, to take this into account, meaning that a small estimated coefficient could still be in relative terms large in comparison to the mean value of the dependent variable.

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 the descriptive findings in Figure 2, we include various fixed effects to pinpoint the causal effect of interest.⁶

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

⁶In line with the conclusions in Section E1 regarding Nickell-bias, we only focus on the results where we include the combined individual-profession-workplace fixed effects (column 2–4). However, for transparency, we display column 1 without any fixed effects or covariates.

Table 1: Main results: The probability of being nominated in t when having politician colleagues in $t - 1$.

	(1) Nom.	(2) Nom.	(3) Nom.	(4) Nom.
Share politicians in $t-1$	0.0961*** (0.0028)	0.0168*** (0.0023)	0.0168*** (0.0036)	0.0216*** (0.0040)
Mean dep. var.	0.302	0.210	0.225	0.213
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs $t-1$.	No	No	Yes	Yes
Individual covs t .	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
R2	0.000	0.380	0.444	0.497
Observations	31740853	23728520	12383920	11026801

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 100 to facilitate interpretation.*

interacted fixed effects for individual, workplace and profession. This substantially decreases the estimated coefficient, which is expected, 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) aggregated covariates (average standardized income, average years of education, gender disposition and the share of immigrants) among those working at the same workplace and in the same occupation at that workplace in $t - 1$, iii) fixed effects municipality of residence and iv) mandate period fixed effects. The inclusion of these fixed covariates does not change the estimated point estimate much at all. In column 4, we further include occupation and profession mandate trends. The estimated coefficient of interest is then slightly increased in magnitude. In this specification we are, in essence only considering the timing of actually becoming politician by the pool of those who eventually run for office.

In terms of economic significance, the point estimates in the most conservative

specification equals 0.0216, meaning that when the share of politician colleagues is increased by one standard deviation in $t - 1$, the probability of becoming a nominated politician is increased by 0.0216 percentage points in t . This effect may appear small, but we have to remember that the only a small fraction of the population becomes politicians. If we divide the point estimate of 0.0216 by the mean value of the dependent variable (0.213), a one standard deviation increase equals an increase in the relative probability of becoming nominated by approximately 10 percent $[(0.0216 / 0.213) * 100]$, which should be considered as substantial.

Robustness checks for main results We have run several sensitivity checks to assess the robustness of the main findings in Table 1 and the results are presented and discussed in the online appendix Section B. In Table B1 we change the explanatory variable from the share of nominated politician colleagues to the share of elected politician colleagues. In Table B2, we run the same analysis as in Table 1, but we exclude all professions that are highly political in their nature. This includes, for instance, PR-consultants and lobbyists. In Table B3, we further exclude the 5 most common professions among politicians. The overall conclusions is that the results in the online appendix are in line with the findings presented here in the main text.

In addition to the above mentioned sensitivity checks, we also include a number of placebo regressions in Table B4. We use the preferred specification, and estimate the effect of politician coworkers on years of education, tendency to go on parental leave unemployment, labor income, college education and disposable income. In terms of economic significance the estimated coefficients for these placebo outcomes are insignificant.⁷ In Figure B1 we assess whether our results are sensitive to the number of individuals employed within a workplace-occupation cell and find that our main findings are robust to various specifications.

⁷We estimate 2 statistically significant coefficients (labor income and college enrollment), but in terms of magnitude, these coefficients are very close to 0. See the online appendix for further discussion.

Intensive margin Here in the main text we have focused on the probability of becoming a nominated politicians if having politicians colleagues at the workplace. This is essentially an analysis along the extensive margin. What about the intensive margin? In the online appendix Section C, we present some results that could shed light on this issue. First we investigate the effect on becoming an elected politician, which is one step further in one's political career. We also analyze the the list position as an additional outcome variable. We find no clear effects on any of these outcome variables, which indicate that there are no treatment effects along the intensive margin. This is not that surprising given that we focus on first-time nominated in our empirical analysis. In conclusion, we find that workplace networks increases the probability of running for office, but they do not affect the success in the political arena.

7 Disentangling the intermediate mechanisms

Let us now return to our previous discussion on mechanisms. As presented in the mechanism-tree in Figure 1, we considered two broad mechanism: a supply side channel, through which contact with politicians at work lowers the cost of information to entering politics, and a demand side, where the politicians at work actively recruits colleagues into their respective cause.

Let us first consider whether the positive reduced form in Table 1 may be explained by an increase in the the probability of running for the same political side as the politician-colleague or not.

Partisan channel Swedish politics has historically been centered around two different blocs and our subsequent analysis is split between the right-wing bloc and the left-wing bloc. In the upper panel of Table 2, the outcome variable takes the value 100 if the individual was nominated for the *left-wing bloc* and 0 otherwise and in the bottom panel the outcome is similarly defined but measures nomination for the *right-wing bloc*. In both panels

Table 2: Mechanism analysis: Results by right wing and left wing bloc

	(1)	(2)	(3)	(4)
PANEL A	Nom. CL	Nom. CL	Nom. CL	Nom. CL
Share CR politicians in t-1	0.0117*** (0.0010)	0.0017* (0.0010)	0.0040*** (0.0015)	0.0067*** (0.0018)
Share CL politicians in t-1	0.0669*** (0.0025)	0.0137*** (0.0021)	0.0113*** (0.0032)	0.0111*** (0.0032)
Mean dep. var.	0.116	0.077	0.087	0.089
PANEL B	Nom. CR	Nom. CR	Nom. CR	Nom. CR
Share CR politicians in t-1	0.0389*** (0.0018)	0.0040** (0.0017)	0.0058** (0.0028)	0.0057* (0.0034)
Share CL politicians in t-1	0.0111*** (0.0011)	0.0011 (0.0011)	0.0009 (0.0018)	0.0035* (0.0020)
Mean dep. var.	0.131	0.085	0.093	0.084
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
Observations	31740853	23728520	12383920	11026801

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 100 to facilitate interpretation.*

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.

The overall conclusion from panel A is that both left-wing and right-wing politician colleagues increases the probability 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 around twice the estimated coefficient for the share of right-wing politicians. The same pattern emerges in panel B for right wing nominations: all coefficients are positive but the point estimates for the share of right-wing politician coworkers are larger

(and more precisely estimated) than the point estimate for the share of left-wing politician coworkers.⁸

These results rule out the left-most branch in the mechanism-tree in Figure 1 where the entire reduced form is explained by a supply channel where politician-colleagues increases the probability of running for the other bloc. While there seems to be some evidence that people become politically engaged for the other political side than the one represented at the workplace, this effect is generally much smaller than the impact of having politician colleagues and then run for the same political bloc as that colleague. Let us now continue to the next part of the mechanism-tree och investigate whether there are evidences for inbreeding-bias.

Inbreeding bias How can we define inbreeding bias, which was in the center of the discussion in [Montgomery \(1991\)](#)? Returning to our discussion in the theoretical framework section, we argued that individuals with similar productivity socialize with each other in the workplace and that political parties are likely to recruit by 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 that both at-work productivity and political productivity is determined by a more latent variable.

There is no variable for productivity in Swedish registry data so we need to find a proxy. 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 of being a latent factor positively affecting various aspects of a person's life. This data originates from Swedish military conscription, which was mandatory for all men (with a few exceptions) up until 2009. During conscription, a number of tests where performed in order to sort men into different military positions. One of these test was a modified version of an IQ-test with

⁸Note that local political parties and the Sweden Democrats are excluded from this analysis since they have not been characterized into the left-wing or the right-wing bloc.

the overall goal to measure the g -factor.⁹ Women could enlist on a voluntary basis, but those women choosing to enlist are likely not representative so we choose to only use the data for men. We instead complement the cognitive ability data with grades point averages (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 GPA, we standardize the variable for each graduation cohort.¹⁰ In order for these proxies to address inbreeding bias, we need to relate them to the political party that eventually ends up recruiting new party members. We define a high ability variable taking the value 1 if an individual is 1) nominated by a political party and 2) if this individual's ability is greater than the average ability of all other nominated member within this party. We also define a low ability variable taking the value 1 if an individual is 1) nominated for a political party and 2) if this individual's ability is lower than the average ability level within the party. Both these variables takes the values 0 if conditions 1 and 2 are not fulfilled. It should be noted that we do not have data of cognitive ability or GPA for all individuals that were included in our main analysis in Table 1 since the enlistment data starts in 1969 and the GPA data starts in 1973. The mechanisms analysis will therefore be run a subset of the individuals.¹¹

From these variables we generate two treatment variables: the standardized share of politicians at work that are of high and low ability respectively. We then include both these treatment variables in a split sample analysis. The results are presented in Table 3.

⁹For me information on Swedish military conscription, see [Öhman \(2015\)](#).

¹⁰There are some individuals in the data that have enlisted more than once. For these individuals, we use the first available test result. For GPA, there are individuals that attend multiple programs in upper secondary school. One reason is that they change which program they attend or that they drop out and then later return. For these individuals we choose the latest available GPA which we deem is best representative for successfulness in upper secondary school.

¹¹This construction of ability measures reassembles the competence analysis in [Besley et al. \(2017\)](#) where the authors analysis gender quotas and its effect on competence of politicians. The 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 similar background as themselves. Our ability measure, which takes its starting point in [Montgomery \(1991\)](#) and the idea of inbreeding bias along ability lines, directly focus on proxies for cognitive ability. Our measure, and the measure in [Besley et al. \(2017\)](#), are likely to be highly correlated.

Table 3: Mechanism analysis: Result by high and low ability politicians

	(1)	(2)	(3)	(4)
PANEL A	Nom high	Nom high	Nom high	Nom high
Share low skill t-1	0.0154*** (0.0014)	0.0044*** (0.0012)	0.0046*** (0.0018)	0.0035* (0.0020)
Share high skill t-1	0.0261*** (0.0018)	0.0069*** (0.0014)	0.0064*** (0.0023)	0.0043* (0.0026)
Mean dep. var.	0.084	0.056	0.051	0.050
PANEL B	Nom low.	Nom low.	Nom low.	Nom low.
Share low skill t-1	0.0091*** (0.0009)	0.0035*** (0.0011)	0.0038*** (0.0013)	0.0064*** (0.0019)
Share high skill t-1	0.0122*** (0.0012)	0.0058*** (0.0012)	0.0028* (0.0015)	0.0037** (0.0015)
Mean dep. var.	0.080	0.052	0.051	0.050
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
Observations	31740853	23728520	12383920	11026801

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variables is binary and takes the values 0 or 100 to facilitate interpretation.*

Let us begin by discussing the results in Panel A in Table 3. 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 work place is larger than the estimated coefficients for the share of low ability politicians across all specifications. However, it should also be noted that the estimated coefficients for the share of low ability politicians at the workplace is statistically significant in all models. In Panel B, we find a more mixed result. In this case, the outcome is being nominated and being of low ability. For first two model specifications, the estimated coefficients for the share of high ability politicians is higher than the estimated coefficients for the share of low ability politicians. For the more demanding specifications in columns (3) and

(4) in Panel B, we find that the estimated coefficients for the share of low ability politicians are larger. Again, the estimated coefficients are statistically significant.

What do these results tell us? They point towards inbreeding bias, but the results are not clear-cut. The results indicate that high ability individuals are more likely to be nominated if there are politicians at the workplace in $t - 1$ that are also of high ability. The same is true for low ability individuals. However, we also find that high ability individuals are also more likely to be nominated if there are low ability politicians at the workplace in $t - 1$, but to a smaller degree. For low ability individuals, the results are more mixed across model specifications, but the results point towards inbreeding bias for more demanding model specifications.

Supply or demand channel? Let us now return to the mechanism tree in Figure 1. As is often the case with mechanism analyses, we cannot fully conclude that one channel is the one and only intermediate channel. However, we may rule out some channels. A pure supply channel (the leftmost part of Figure 1) is not likely since we find evidence of partisan recruitment. Given that we also find results that point towards inbreeding bias, a demand driven channel is likely to be present. Given that we find that the probability of running for the other bloc than the one represented by the politician-colleague is also increased (although to a lower degree than running for the same bloc) we cannot rule out that the cost of information has been generally decreased meaning that a supply mechanism is also present. The most likely explanations for our main results in Table 1 is therefore supply and demand where having politicians at the workplace lowers information costs and that political parties recruit by referrals from party members from workplaces.

8 Heterogeneity analysis

Remains to be investigated.

9 Discussion and conclusion

Sweden often picture itself as a country where people are recruited into politics because of their competence. Indeed, [Dal Bó et al. \(2017\)](#) found that Swedish politicians were more competent in comparison as the population as a whole. Despite this positive selection into politics, personal connections and networks may still play an important role. The overall conclusion from this paper that it is in fact so.

The workplace has been center of our analysis. The workplace and the network that there exist have previously been analyzed in economics, but there is an apparent void when it comes to studies that have analyzed the effects of networks formed at the workplace for out of workplace activities. The political sphere is one such arena and we have demonstrated that the workplace plays a role of political engagement in this paper. Our findings connects to earlier results regarding networks and their impact on political recruitment. [Folke et al. \(2021\)](#) find, by using Swedish and Irish data, that family connections play a role in political recruitment for women in particular. Both our studies demonstrates that networks and social contexts needs to be taking into account when discussing recruitment into politics.

Political socialization has foremost been hypothesized to take place during childhood or during adolescence. This means that people may missed out on gaining the necessary political socialization if growing up without political role-models. What our findings demonstrate is that political socialization may also have an impact much later in life when a person has left the education system. This could be an argument for increasing the number of leisure politicians in a constituency since this would translate into more frequent cues to the political sphere. On the other hand, politicians are likely to be clustered in certain spheres on the labor market, which makes it a difficult task to overview the net effect of such a policy reform.

We also analyzed the mechanisms behind our main findings and found that the main effects follows partisan lines and that there are some evidence of inbreeding bias in workplace networks and how they affects political recruitment.

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A1 Data availability

The data that we use for our empirical analysis in this research project consists of Swedish administrative data. There are various rules for how this data needs to be handled and stored. For this reason, we cannot make the data available online for replication purposes.

There are two ways for other academics to replicate our empirical findings. The first way 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 to obtain a 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 options needs to reach out to us since we need permission from the Ethical review Board to temporarily add a researcher to our research group. Please note that there are geographical restriction of where to log in into the remote desktop system.

We will make all do-files and a list of used variable available.

B1 Robustness analysis for main results

In this section of the online appendix, we present various robustness checks for our main result presented in Table 1. We begin by Table B1 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 our main analysis, although the estimated coefficients are somewhat smaller in magnitude. It is here important to remember that we end up with fewer politicians overall when we focus on elected instead of nominated.

Next, we run a sub sample analysis where we have excluded professions that are highly political in their nature to assess that our main results are not driven by these particular professions. We exclude full time politicians, lobbyist, high civil servants, elected representatives (not politicians) and PR-consultants. The results are presented in Table B2 and they are in line with our main findings in Table 1.

We have also run an additional sub sample analysis where we both exclude the above mentioned professions and the five most common professions among politicians. The results are presented in Table B3. The results are, yet again, in line with our main findings.

In Table B4, we have run a number of placebo checks. We have changed the outcome variable from becoming nominated in the next mandate period into 6 labor market outcomes: Labor income, disposable income, parental leave uptake, years of education and college enrollment. Four of the 6 estimated coefficients are small and statistically insignificant. For labor market income, we estimate a statistically significant coefficient, but in terms of economic significance it is insignificant. The outcome is standardized with mean 0 and standard deviation 1 and the interpretation is that when the share of politicians at the workplace increase by one standard deviation, the labor income is increased by 0.001 standard deviations. In practice, this is a zero effect. We also estimate a statistically significant negative coefficients for college enrollment, but again the estimated coefficient 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 is decreased by 0.05 percentage points. Alas, this is also a zero effect in terms of economic significance.

Lastly, in Figure B1 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 we allow for more and more individuals within a workplace and occupation category. The overall conclusion is that our main findings are stable across these specifications.

Table B1: Robustness: Elected politician instead of nominated as treatment variable

	(1)	(2)	(3)	(4)
	Nom.	Nom.	Nom.	Nom.
Share elected pol. in t-1	0.0804*** (0.0037)	0.0127*** (0.0027)	0.0144*** (0.0042)	0.0157*** (0.0047)
Mean dep. var.	0.302	0.210	0.225	0.213
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
R2	0.000	0.380	0.444	0.497
Observations	31740853	23728520	12383920	11026801

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is binary and takes the values 0 or 100 to facilitate interpretation.*

Table B2: Robustness: Excluding politician professions

	(1)	(2)	(3)	(4)
	Nom.	Nom.	Nom.	Nom.
Share politicians in t-1	0.0910*** (0.0025)	0.0175*** (0.0023)	0.0182*** (0.0036)	0.0241*** (0.0040)
Mean dep. var.	0.300	0.209	0.225	0.212
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
R2	0.000	0.380	0.444	0.497
Observations	30950933	23236591	12132891	10788400

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 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 100 to facilitate interpretation.*

Table B3: Robustness: Excluding politician professions and 5 most common professions

	(1)	(2)	(3)	(4)
	Nom.	Nom.	Nom.	Nom.
Share politicians in t-1	0.0835*** (0.0027)	0.0162*** (0.0025)	0.0148*** (0.0039)	0.0216*** (0.0045)
Mean dep. var.	0.277	0.193	0.210	0.195
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
R2	0.000	0.376	0.442	0.502
Observations	26118985	19399110	9773388	8532260

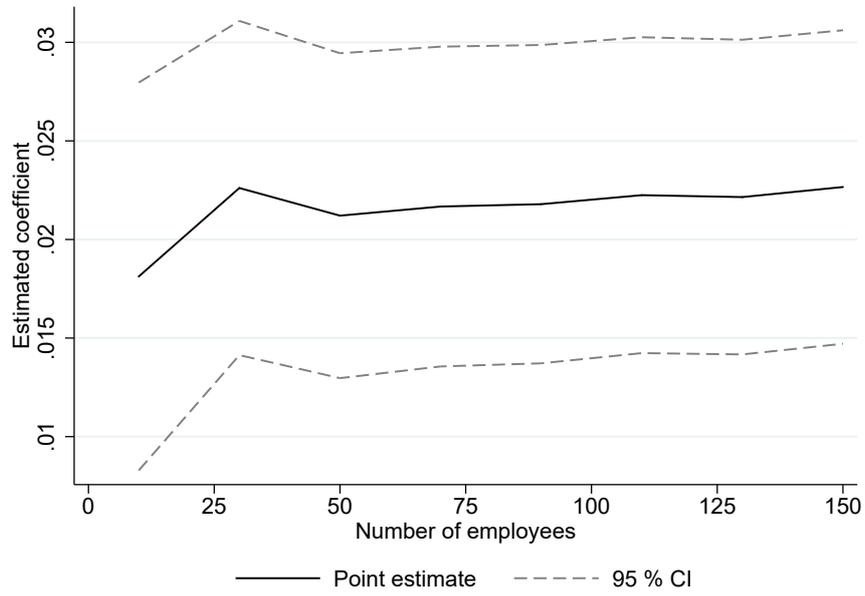
*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Same as main results in Table 1, but five most common occupations and full time politician, lobbyist, high civil servant, elected representative (not elected politician) and PR-consultants are excluded from the estimations. The dependent variable is binary and takes the values 0 or 100 to facilitate interpretation.*

Table B4: Placebo outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Lab.Inc	Disp.Inc	Par.Leave	UnemBen	Y.educ	College.P
Share politicians in t-1	0.0010*** (0.0003)	-0.0002 (0.0002)	0.0001 (0.0004)	-0.0004 (0.0005)	0.0002 (0.0004)	-0.0005** (0.0003)
Mean dep. var.	0.234	0.028	0.054	0.013	12.448	0.143
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	0.819	0.635	0.561	0.531	0.946	0.591
Observations	11026801	11026801	11026801	11026801	11026801	11026801

Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure B1: Robustness: Main results for different number of employees



Notes: The figure displays the estimated coefficient, together with a 95% confidence interval corresponding to the specification in column 6 in Table 1, i.e. the most conservative specification in the main analysis.

C1 Analysis along the intensive margin

In this section we shed some light regarding the results on the intensive margin.

In Table C1 we change the outcome variable into becoming an elected politician in the next mandate period. The conclusion is that we do not find any effects. This result is not that surprising given that we focus on first time nominated. In Table C2 we investigate the effect of our treatment variable on list position. A negative point estimate in this case equals a lower list position, meaning that the politician has a higher probability of becoming elected. For this analysis, we are only making use of those politicians that are nominated and ask whether having a politician colleague increases the probability of receiving a higher list position. We find one small statistically significant point estimate for the first specification without any fixed effects or covariates. When including the fixed effects and covariates, the point estimate becomes insignificant and remains small.¹²

The overall conclusion in this section is that workplace networks do not have an impact on political participation along the intensive margin.

¹²The `reghdfe` command drops singletons, meaning that the number of observations becomes small when including all these fixed effects. In fact, we cannot run this specification with the trends because we end up with too few observations.

Table C1: Elected as the dependent variable

	(1)	(2)	(3)	(4)
	Elec.	Elec.	Elec.	Elec.
Share politicians in t-1	0.0205*** (0.0015)	0.0009 (0.0009)	0.0003 (0.0015)	0.0006 (0.0018)
Mean dep. var.	0.044	0.028	0.028	0.027
Individual*wp*occupation FE	No	Yes	Yes	Yes
WP occupation covs t-1.	No	No	Yes	Yes
Individual covs t.	No	No	Yes	Yes
Mandate period FE	No	No	Yes	Yes
Municipal FE	No	No	Yes	Yes
Occupation mandate trend	No	No	No	Yes
Workplace mandate trend	No	No	No	Yes
R2	0.000	0.375	0.438	0.492
Observations	31740853	23728520	12383920	11026801

Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C2: List position as the dependent variable

	(1)	(2)	(3)
	ListPos	ListPos	ListPos
Share politicians in t-1	-0.0023*** (0.0004)	0.0014 (0.0047)	0.0080 (0.0051)
Mean dep. var.	0.599	0.631	0.645
Individual*wp*occupation FE	No	Yes	Yes
WP occupation covs t-1.	No	No	Yes
Individual covs t.	No	No	Yes
Mandate period FE	No	No	Yes
Municipal FE	No	No	Yes
Occupation mandate trend	No	No	No
Workplace mandate trend	No	No	No
R2	0.028	0.611	0.681
Observations	89489	1460	734

*Note: Standard errors in parenthesis are clustered at the workplace-occupation-mandate period level (the level of treatment). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*

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 the 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 right-hand side variable of interest is a lagged variable which is related to the dependent variable.

This discussion relates to the conclusion in [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 period does not go to infinity.

However, the set-up in our paper is not the equivalent of a dynamic panel data model where we have included a lag of Y . Instead, our variable of interest is share of politicians at the workplace in $t - 1$. [Judson and Owen \(1999\)](#) furthermore showed that the bias is foremost present for estimated coefficient 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 assume a true causal effect equal to 0. We furthermore assume that there is 2 % politicians in the population. Column 1 displays the expected null association between the share of politicians in $t-1$ on the probability of becoming a politician in t . The inclusion of individual fixed effects does not substantially change this estimate (Column 2). The problems arises in Column 3–Column 6. Here we include a fixed effects for the firm and occupation. This cell equals the cell for which the independent variable is calculated. The results in Column 3 – Column 6 suggest that by including this fixed effect, we estimate a negative coefficient, even when the true causal effect is 0. This negative coefficient is estimated when only the firm and occupation fixed effects is included (Column 3); when the firm and occupation fixed effect together with the individual fixed effects are included separately (Column 4); and when firm and occupation fixed effects, individual fixed effects and the year

fixed effects are included separately (Column 5).

In Column 6 and Column 7, we interact the firm and occupation fixed effects together with the individual fixed effects. In essence, the only identifying variation that we use is the timing of when an individual becomes a politician. In this case, we do not find any evidence of nickell-bias. This is also the strategy that we use in the analysis in the main text.

Table E1: Simulation of Nickell-bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Nom.	Nom.	Nom.	Nom.	Nom.	Nom.	Nom.
L.share1	-0.0025 (0.0515)	0.0001 (0.0515)	-0.2489*** (0.0592)	-0.2464*** (0.0592)	-0.2591*** (0.0602)	-0.0319 (0.0569)	-0.0294 (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 $t - 1$. Standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.*