

# Politicians' Neighborhoods: Where do they Live and does it Matter?\*

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## Abstract

This paper studies the political economy of local politicians' neighborhoods. We use detailed population-wide data on the location of politicians' and citizens' homes and their socioeconomic traits. We combine this information with neighborhood-level data on building permits and proposals to close schools. A descriptive analysis uncovers that politicians live in neighborhoods with more socio-economically advantaged people and more of their own party's voters. Next, we analyze whether having politicians in a neighborhood reduces the likelihood that local public "bads" are placed there. This analysis compares home neighborhoods for politicians with different degrees of political power (ruling majority or opposition) and where power was won in a close elections. We find negative effects on approved building permits for multifamily homes and proposals to close schools. This result is most likely explained by undue favoritism. We conclude that local politicians live in advantaged neighborhoods that they shield from local public bads.

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# 1 Introduction

Geography is at the heart of politics. Many of the most important political decisions are not about which public goods the government should provide, but where it should locate them. This paper examines a specific geographic aspect of politics – the neighborhoods in which politicians live within their electoral district. We describe what types of neighborhoods these are. We then test whether the spatial distribution of politicians’ homes affect their decision-making.

We draw on unique data from Sweden to provide new information about the type of people who become politicians. To date, research on descriptive political representation has focused on politicians’ personal traits. For example, prior studies have found that women, ethnic and racial minorities, and working-class people are chronically under-represented in political assemblies around the world.<sup>1</sup> Politicians’ neighborhoods have been largely overlooked.<sup>2</sup> Contextual theories of politics underscore the importance of these surroundings. A person’s political preferences are a function not only of their own traits, but also the traits and predispositions of individuals in their proximity (Huckfeldt et al., 1993; Enos, 2017).

Our study of spatial decision-making uncovers a new political determinant of these decisions – the location of politicians’ homes. The analysis targets local politicians who make crucial decisions about local development and land use. These local of-

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<sup>1</sup>Some recent examples from this large literature include Carnes and Lupu (2015), Dancygier (2017), Thomsen and King (2020), Carnes (2020), and Barnes et al. (forthcoming).

<sup>2</sup>Bartanen et al., 2018 show that school board politicians in Ohio live in areas with above-average household income and house values, a higher percentage of people with post-secondary qualifications, and below average proportions of racial and ethnic minorities. The study most closely related to ours is the contemporaneously written paper by Harjunen et al. (2021), which shows that Finnish politicians live in richer neighborhoods that are less likely to experience school closures.

officials decide where (and which type of) buildings can be constructed. They also choose where to place public services like schools, public transportation, parks, affordable housing, and cultural and sports facilities. Although these services provide value for the surrounding society, the directly affected neighborhoods might resist specific projects, for example apartment buildings (Hankinson, 2018; Trounstone, 2020). We use the terminology *local public bads* to describe this phenomenon.

Swedish administrative records contain highly accurate demographic and economic variables for the country's entire population. We combine this data with a complete list of the universe of municipal politicians. Sweden's 290 municipalities are divided into voting precincts of similar size following boundaries in the natural environment. We use information on the home precinct of every citizen and politician to approximate local neighborhoods. At the neighborhood level, we add datasets for two common local public bads that fall under the purview of municipal politicians – (1) all approved building permits for new multifamily and single-family housing (from the Building Permit Register) and (2) proposed school closures (collected in a survey of municipalities by Uba (2016, 2020) that the authors extended to more recent years).

Our descriptive analysis shows that politicians' neighborhoods generally share two key features. First, they have larger shares of socio-economically advantaged groups – more high-income earners, residents born in OECD (Organisation for Economic Co-operation and Development) countries, homeowners, and people with tertiary education. This pattern is stronger for politicians in center-right parties than for those on the ideological left. These descriptive findings also hold when we use the detailed coordinate information in the data and construct smaller-scale, individualized, neighborhoods. Second, politicians tend to live in neighborhoods in which their party received a disproportionately large share of votes.

We find that the location of politicians' homes influences decisions about local public bads. During election periods, fewer building permits for multifamily homes are

approved, and fewer proposals to close schools are made, in neighborhoods in which more politicians from the local majority party vs. the local opposition live. We see no effect for single-family homes, which local residents are typically less likely to resist (Einstein et al., 2019; Hankinson, 2018; O’Grady, 2020). We quantify the magnitude of the results by calculating the average impact of a switch in the governing majority. We find that a shift in the governing majority leads to a reallocation of 14% of the new construction of multifamily homes and a 19-percentage-point drop in the likelihood of proposed school closures in neighborhoods dominated by politicians from the new majority.

We argue that our estimates should be interpreted causally. The analysis compares political decisions on local public bads for neighborhoods that have the same *number* of politicians but with different degrees of power. We also restrict the sample to municipalities in which this power, i.e. being in the ruling majority, was decided by a close election. In our sample of three election periods, many municipalities had close elections that quasi-randomly brought left-wing and center-right coalitions to power about half of the time. We add an extensive amount of data from additional government registers and other sources to test the core assumption of the empirical strategy. This analysis shows that the relative power of the politicians who live in a neighborhood is statistically independent of numerous proxies for anticipated public protests and economic profitability rationales for the spatial placement decisions of local public bads.

Does the location of politicians’ homes matter because politicians co-reside with their party’s voters? An extended analysis of the mechanisms suggests that this is not the case. Flexible control variables for the spatial allocation of voters do not affect our main results. By exclusion, this indicates that (conscious or unconscious) favoritism of the home neighborhood is a more likely explanation. Politicians’ impartiality might be compromised by economic self-interest, localized information flows, or personal interactions with aggrieved neighbors.

Our results contribute to the large literature on the political determinants of the spatial distribution of public funds. They echo earlier findings that electoral districts with more politicians receive more public funds in majoritarian systems (e.g., Ansolabehere et al., 2002; Knight, 2008; Elis et al., 2009). They are also in line with the results from previous studies of proportional representation (PR) systems, which have concluded that the regional allocation of funds favors politicians' hometowns (Fiva and Halse, 2016*a*; Baskaran and Lopes da Fonseca, 2018). We add the important insight that *within* electoral districts, decisions about land uses with negative local externalities are strongly affected by the location of politicians' homes.

Our results have important implications for the broader academic discussion of neighborhood traits. Prominent research in economics and other disciplines has underscored that local living conditions are crucial determinants of the life trajectories of adults and children (Chetty et al., 2016; Chetty and Hendren, 2018; Johnston and Pattie, 2014). We show how local political factors help shape these conditions. In turn, our results suggest that local political institutions are a potentially fruitful area of policy intervention to improve the level and equity of local living environments. The increasing geographic segregation and inequality observed around the world highlight the urgency of these findings (Storper, 2018).

## **2 Political economics of politicians' neighborhoods**

This section builds on previous research to develop three hypotheses about the political economics of politicians' neighborhoods. The first two, discussed in Section 2.1, concern the neighborhoods' characteristics: we predict they have socio-economically advantaged populations and larger vote shares for the politician's party. Section 2.2 examines the third hypothesis – that politicians reduce the allocation of local public funds to their own neighborhoods.

## 2.1 Traits of politicians' neighborhoods

The supply of and demand for politicians can both help us understand where they are likely to live. On the supply side, affluent people are more likely to participate in politics and (of course) to live in well-off areas. Socioeconomic advantage encourages people's political activity by offering skills, money, and networks (Verba and Nie, 1972; Brady et al., 1995; Norris and Lovenduski, 1995). Advantaged groups might also enter politics because they have more to gain. Previous studies report that homeowners are more active than non-homeowners in local politics (Yoder, 2020), likely because the value of their homes is directly tied to local political decisions.

On the demand side, party behavior may exacerbate the selection of affluent citizens. For example, parties may want immigrant minorities as members, but be reluctant to let them run in competitive districts (e.g., Blomqvist, 2005). Such party behavior likely reinforces the well-established pattern that politicians tend to have higher incomes and education than the people they represent (Dal Bó et al., 2017; Bhusal et al., 2020).

Two centrifugal forces also disperse politicians' homes across rich and poor neighborhoods. The first is parties' organizational structures. Local parties often consist of clubs spread out among neighborhoods that recruit members, hold meetings, and oversee the day-to-day aspects of election campaigns. To the extent that clubs recruit members and nominate candidates, the decentralized nature of party organizations leads to the dispersion of politicians' homes.

The second centrifugal force is electoral incentives: voters may prefer candidates who live close to them. One strand of research has observed that factors ranging from personal friendships to shared political preferences and local pride generate a "friends and neighbor voting effect" (Key, 1950; Lewis-Beck and Rice, 1983). Another strand has observed that voters "look for locals" because they interpret local ties as knowledge of the area's needs and concerns (Cain et al., 1987; Carey and

Shugart, 1995). The findings from these studies imply that even geographically centralized parties might benefit from recruiting politicians from an array of neighborhoods (Latner and McGann, 2005). Although this preference for neighbors has mainly been observed in candidate-centered systems, there is also supportive evidence from countries that rely on party-based voting (reviewed by, e.g., Arzheimer and Evans, 2012; Erlingsson and Öhrvall, 2018).<sup>3</sup>

While parties' organizational structures and electoral incentives work to geographically disperse politicians' homes across neighborhoods, advantaged groups' self-selection into candidacy and parties' skewed recruitment likely dominate. We therefore predict that:

**Hypothesis 1a:** Politicians live in socio-economically advantaged neighborhoods.

Politicians from different parties are likely to live in different parts of town, since social classes are segregated across neighborhoods and the ideological divisions between them underpin the traditional distinction between left and right parties (Lipset and Rokkan, 1967). Socioeconomic cleavages have been found to determine both voting (e.g., Kitschelt, 1994) and political candidacy (O'Grady, 2019; Dal Bó et al., 2017, c.f. Katz and Mair, 2009). To the extent that (1) parties systematically recruit politicians from different social groups and (2) these groups are spatially segregated across neighborhoods, we predict that:

**Hypothesis 1b:** Politicians live in neighborhoods in which their party has a larger vote share.

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<sup>3</sup>For instance, Campbell and Cowley (2014) randomize UK candidates' local residency, gender, age, occupation, and education, and find that occupation and place of residence have the largest effects. Also in the UK, Arzheimer and Evans (2012) analyze observational data and show that voters prefer politicians who live closer to them, conditional on other observable traits. Jankowski (2016) shows that voters actively search PR ballots for information about which politician lives in their electoral district and are more likely to vote for that person.

## 2.2 Local public bads and politicians' neighborhoods

We follow Aldrich (2008) and define a local public bad as a project that benefits the surrounding area but has negative (perceived) effects on the immediate vicinity. The general positive effects give politicians an incentive to place these bads *somewhere* in the municipality. But resistance from neighborhoods makes their exact placement a contentious decision.

The two local public bads we study exemplify such a political trade-off. The construction of new housing benefits a municipality by bringing in new taxpayers and alleviating housing shortages. It also makes the city more affordable and protects the quality of life of low- and middle-income families (see Been et al., 2019 for a review). But the closer to a person's home the proposed construction is, the greater the opposition (Tighe, 2010; Hankinson, 2018). This opposition is based on perceived threats to property values, competition for local public goods, physical disruptions including blocking natural light and views, and anti-poor sentiments or racial prejudice (Larsen et al., 2019; Trounstine, 2020; Tighe, 2010; Einstein et al., 2019). These concerns generally grow with the size of the construction, and opposition to multifamily homes is generally more intense than to single-family homes (Einstein et al., 2019).

School closures are a cost-saving measure that benefits a municipality by eliminating schools with few students, weak performance, or large investment needs (e.g., Åberg-Bengtsson, 2009; Witten et al., 2003). But for people in the school's immediate neighborhood, closures mean longer commutes for children (Witten et al., 2003) and the removal of an arena for local identity building and cultural events (Kilpatrick, 2002).

We believe the location of politicians' homes may have a causal effect through two main mechanisms. The first is political representation. If Hypothesis 1b (that politicians live in neighborhoods where their parties have larger vote shares) is correct,



parties could be delivering benefits to these neighborhoods as part of a clientelistic exchange for votes. However, this is unlikely in advanced democracies with PR systems.<sup>4</sup> Programmatic considerations might be more important and arise if party platforms cater to the economic interests of different socio-economic groups. Local public bads might form part of these agendas. For example, constructing new apartments in a neighborhood with more right-leaning (affluent) people might serve a left-leaning party's agenda of economic redistribution.<sup>5</sup>

The second mechanism is favoritism: having politicians' homes in a neighborhood might cause fewer local public bads to be placed there. Favoritism can arise from both conscious and unconscious thought processes. At the most conscious level, politicians may act in pure self-interest. By protecting their neighborhood from local public bads, they also protect the (perceived) value of their own property, or that of friends and relatives who live there. Information flows may lead to favoritism in a more unconscious way. Politicians cannot obtain full information on the costs and benefits of all placement options for every local public bad, and their own neighborhoods' perceptions are likely to skew the information they receive. People obtain information via social networks, and interactions with neighbors serve as vehicles for the transmission of political information and guidance (Huckfeldt and Sprague, 1987; Huckfeldt et al., 1993). These information flows deliver the negative localized public opinion to the politician, for example on the perceived damage of closing a school, or environmental concerns about housing construction.

To the extent that neighbors protest the local public bads, a politician could perceive

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<sup>4</sup>For example, a careful study of the allocation of spending in Spanish municipalities finds no evidence that parties allocate stimulus funds to more partisan neighborhoods (Carozzi and Repetto, 2019). Likewise, using Swedish data, Dahlberg and Johansson (2002) reject the hypothesis that incumbent governments purchase votes by investing in regions where they already have high levels of support (i.e., no support for the prediction from the model in Cox and McCubbins (1986)).

<sup>5</sup>In majoritarian systems, politicians might cater to personal constituencies in the form of targeted sub-constituencies within electoral districts (Fenno, 1977), for instance by granting pork-barrel favors via committee service, which could include the type of decisions on local public bads studied in this paper. However, personal constituencies are minimal in closed-list PR systems (Fiva and Halse, 2016b; Karlsson, 2018).

citizen opposition to be more intense if it occurs in his or her own neighborhood (as in Huckfeldt and Sprague, 1987). Put differently, neighbors' direct access to politicians might create a local accountability effect around visible projects (following political economics frameworks such as Besley, 2006).<sup>6</sup> It is also possible that a politician's home functions as an opportunity structure for protests and thereby makes them more likely (Meyer and Minkoff, 2004).

Given the ideas summarized in this section, which connect politicians' homes to fewer local public bads due to political representation and favoritism, we predict that:

**Hypothesis 2:** The presence of politicians' homes in a neighborhood reduces the likelihood that local public bads will be placed there.

At least three other aspects may affect the placement of public bads, which might also correlate with the placement of politicians' homes. First, politicians might live in neighborhoods where the placement is less politically feasible because it generates more public protest (e.g., Aldrich, 2008). Second, they might live in neighborhoods with more swing voters who are willing to switch parties between elections, which would create a different type of political rationale for shielding the neighborhood. Third, politicians might live in neighborhoods in which it is less economically profitable to place the local public bad. In the empirical sections that follow, we develop an identification strategy to address omitted variable bias from these sources.

### 3 Swedish municipalities

Municipalities are Sweden's lowest administrative level of government. They are responsible for key policy areas like child care, K-12 education, elder care, and local infrastructure, which together employ approximately 25% of the labor force.

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<sup>6</sup>Jankowski (2016) argues that an expectation of greater accountability to the local population motivates people to preference vote for a politician who lives in their electoral district.

Municipalities also set their own income tax rates, usually around 20% of income.

Each municipality is governed by a municipal council with between 21 to 101 members (the median is 41), depending on population size. Councilors are "leisure politicians" who receive small lump sum payments to attend meetings while holding regular jobs (Dal Bó et al., 2017). There is no evidence that winning a council seat generates monetary gains (Berg, 2020) that would, for example, allow the politician to move to a wealthier neighborhood.

Local elections take place every four years. People vote in their electoral precinct, and turnout is usually high at around 90% of the 18+ population. Under the flexible-list PR system, voters chose between parties but can also cast a voluntary preference vote. Given the low share (approx. 30%) and high vote threshold, individuals rarely win a council seat because of their preference votes (Folke et al., 2016).

Councilors are elected in multi-member districts. Two-thirds of Sweden's municipalities have one district, and most others have two. We ignore these administrative borders since they lack the typical functions of electoral districts. Candidates are not required to live in the district they represent, and parties almost always field the same ballot throughout the municipality.

There are some indications that geographic concerns affect the results of local elections. At least in larger municipalities, many political parties have regional clubs that serve as the initial point of entry for new members. These clubs form the basis for political meetings and grassroots activism. They also function as the organizational basis for candidate nominations. Ballot papers typically list the politician's neighborhood next to their name, along with their age and occupation (74% of the ballots in our sample period contained this information, authors' calculation).

We use electoral precincts to approximate neighborhoods. The median number of precincts per municipality is 10, and there are about 6,000 precincts in the country. Two features make precincts suitable proxies for neighborhoods. The first is their

similarity in terms of size. In our sample period, the median precinct has 1,200 adult inhabitants, and 90% of the precinct-year observations fall within the range of 644 to 1,799.<sup>7</sup> Second, precinct borders usually follow the intuitive divisions of neighborhoods, such as water divisions (streams or islands) or infrastructure (large roads or other hard-to-pass elements). This is because each precinct has a single polling station, and the borders are drawn to facilitate physical access to the polling station.

Parties' seat shares in the council correspond to their vote shares. A single party can form a governing majority if it obtains more than 50% of the seats; otherwise parties come together in coalitions to reach this threshold. A crucial fact for our empirical analysis is that governing majorities nearly always comprise parties from the same ideological bloc – left (Social Democrats, Left Party, Green Party) or center-right (Conservatives, Center Party, Liberal Party, Christian Democrats). The Sweden Democrats, Sweden's radical right party, played an unimportant role in local coalition formation during our study period.

The governing majority controls the municipality's political agenda and appoints all executives, including the mayor and policy committee chairs. The council decides on our two local public bads: the education and construction policy committees send formal proposals for school closures and building permits to it. Bureaucrats are involved in both processes but do not make formal proposals or take decisions, with the exception of small-scale and routine building permits.<sup>8</sup>

## 4 Data

We use population-wide administrative data that covers all local politicians and all permanent residents in Sweden. Variables include education level, income, region

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<sup>7</sup>Appendix Figure A3 shows the full distribution of precinct sizes and precincts per municipality.

<sup>8</sup>Swedish law prohibits the delegation of substantive decisions on building permits to bureaucrats, and in many municipalities bureaucrats are not allowed to make rejection decisions (Boverket, 2020).

of birth, and the electoral precinct of residence. Statistics Sweden collects this data from administrative records and makes it available for research in a de-identified format. However, due to its sensitive nature and legal limitations the data is not publicly available. Table A1 summarizes our final precinct-level data set.

Before each election, political parties must report their candidate lists (ballots) to the government and they also provide all candidates' personal ID codes. The Electoral Agency then collects data on which candidates were elected to the municipal council. We have access to these data for three elections (2002, 2006, and 2010), which totals 39,312 election-person observations of municipal councilors.

The Electoral Agency also maintains precinct-level data on election results. We use variables for turnout and the number of voters for each party. There are 17,427 precinct-year observations in our three cross-sections of data, and more than 90% of these precincts are inhabited by at least one elected politician.

The property register contains data on existing properties 1998-2014, while data on approved housing construction come from the yearly register of building permits (1998–2014).<sup>9</sup> The register contains all approved permits for a floor space above 25  $m^2$ , which number approximately 7,300 per year. Each permit details the electoral precinct, month and year of approval, number of housing units, and total floor space, and whether it is for a single-family or multifamily home.

Data on formal proposals for public primary school closures was kindly shared by Katrin Uba (Uba, 2016, 2020) for the years 2002–2009; we used the same methodology to extend the data to 2010–2014. We requested municipalities to submit a list of proposals and obtained a 60% response rate. We then followed Uba's method of using Google searches and newspaper archives to systematically search for additional proposals. This information was merged with a dataset compiled by the Swedish National Agency for Education, which contains all Swedish schools and their addresses.

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<sup>9</sup>The Swedish Planning and Building Act of 1987 requires municipalities to report this data.

We dropped cases where a school address was missing in the national register or we could not find a match based on the school name (this happened for 3% of schools with a closure proposal).

While the building permits can be matched exactly to the individual-level data by precinct, the schools are merged with the precinct grid using  $500 \times 500$ -meter geocoded identifiers (details available in Appendix Section A.1). We use the same method to merge our dataset with information on homeownership, from the database GeoSweden, and information about house prices from Swedish network of real estate agents.

## 5 Descriptive analysis of politicians’ neighborhoods

We use a descriptive analysis to test whether politicians live in neighborhoods with more socio-economically advantaged residents (Hypothesis 1a) or more voters from their own party (Hypothesis 1b). Setting up this description is a bit more empirically complicated than one might think. Comparing neighborhoods with and without politicians is not an option, because they live in nine out of ten neighborhoods in the average municipality. We instead use a *concentration measure across neighborhoods* to capture how different groups are geographically concentrated in specific neighborhoods. This measure compares the spatial distributions of a focal group (such as high-income earners) to a non-focal group (for example, *non*-high earners). The measure is the neighborhood sub-component of the most commonly used measure of segregation, the Dissimilarity index (Massey and Denton, 1988).<sup>10</sup> Our concentration measure is defined as:

$$C_{n,m} = \frac{X_n}{X_m} - \frac{Y_n}{Y_m}$$

We measure the size of focal group  $X$  in each neighborhood  $n$  and municipality  $m$ .

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<sup>10</sup>This index is defined as:  $D_m = \frac{1}{2} \sum_{n=1}^N \left| \frac{X_n}{X_m} - \frac{Y_n}{Y_m} \right|$ . The index measures the proportion of the focal group that would have to move to another neighborhood *as a share of the municipality’s total population* in order for the group to be completely evenly spread out among all neighborhoods.

We benchmark its distribution against people who are not part of the focal group ( $Y$ ); together, these two groups make up 100% of the population.

The first term of the measure ( $\frac{X_n}{X_m}$ ) is the share of the focal group that resides in the neighborhood (for example, 20% of the municipality's high-income earners or politicians). The second term is the share of the non-focal group that lives there (for example, 10% of non-high-income earners or non-politicians). In this example, the concentration measure takes the value 0.1, which demonstrates that the focal group is over-represented in the neighborhood compared to the non-focal group by 10 percentage points. Appendix subsection A.3 shows a more detailed example. The concentration measure tells us the proportion of the focal group that would need to leave or enter each neighborhood to make the groups evenly spread out across neighborhoods.

Our concentration measure has at least three attractive features. First, and most importantly, it is independent of the size of the focal group in the municipal population. This is particularly important when we compare the distributions of small groups (such as politicians) to those of large groups (such as people born in an OECD country). Second, the measure can be easily adapted to calculate distributions of variables other than people, such as local public bads. If we use local public bads as the focal group and the full population as the non-focal group, the concentration measure tells us how much more, or less, of the local public bad a neighborhood has relative to its share of the municipal population. Finally, the municipality average of our concentration measure is, by design, zero. Thus our analysis relies on within-municipality variation.

To test the two hypotheses, we first calculate the concentration measure for politicians and then for four focal groups of socio-economically advantaged people. We then relate these to each other by calculating correlation coefficients. This non-parametric method has an important advantage. It normalizes the relationship so that the coefficient can be interpreted as the variation in one variable, in standard

deviations, that follows from changing the other variable with 1 standard deviation.<sup>11</sup>

When calculating the correlation coefficients, and in the rest of the analysis in the paper, we adjust for municipality size with weights. Large municipalities have more neighborhoods, and the three largest cities account for 16% of the precinct-year observations. Weighting observations with the inverse number of neighborhoods gives each municipality equal importance in the calculation of the correlations, regardless of its size. This weighing does not affect the substance of our findings.

**Results: Socio-economic advantage.** We define four focal categories of socio-economically advantaged individuals: (1) people in the *Top income quartile*, i.e. the top 25% of the national distribution of disposable income, (2) *Tertiary educated* people who completed at least one semester of tertiary education, people (3) *Born in the OECD*, and (4) *Homeowners* who own rather than rent their housing unit. The categorization of OECD-born people compared to others captures the main delineation of socio-economic status based on race/ethnicity in the Swedish context. Immigrants from inside the OECD are mostly labor immigrants with similar outcomes to native-born citizens, while those from outside the OECD are mostly refugees (Åslund et al., 2017; Fasani et al., 2018). More details about the variables are listed in Appendix section A.2.

Figure 1 shows the correlation coefficients between the concentration of politicians and the concentration of the four advantaged groups. We pool all politicians in the left-most graph and split the sample by political bloc (left and center-right) in the other two graphs. In the full sample of politicians, their homes are co-located across neighborhoods with the homes of more socio-economically advantaged citi-

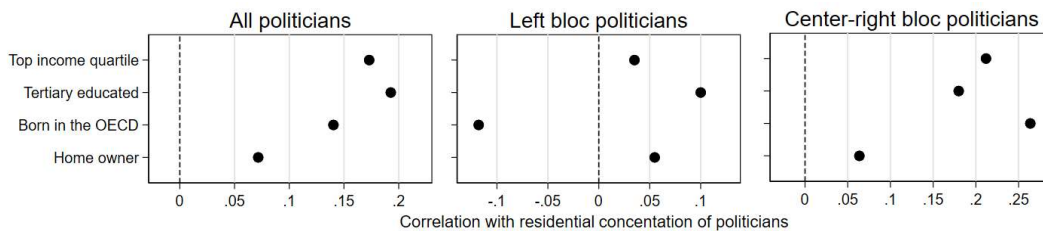
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<sup>11</sup>Another advantage is that by relating two spatial distributions to each other for a *given* set of neighborhoods within a municipality, we avoid the critique that is sometimes raised against the Dissimilarity Index, namely that it is sensitive to both the number of neighborhoods within a municipality and to exactly how the borders of the neighborhoods are drawn. Relating one concentration measure to another can, of course, also be done with regression analysis, which yields the same conclusions.



zens across all four groups, which confirms Hypothesis 1a. Politicians’ neighborhoods have greater concentrations of high-income people, people with tertiary education, homeowners, and OECD-born residents. For high-income, high-education, and OECD-born residents, the correlation coefficients are about 0.15. This means that a 1-standard-deviation higher concentration of municipal councilors is associated with a 0.15-standard-deviation larger concentration of the advantaged group. The correlation with homeowners is also positive, but half the size at 0.07.

Figure 1: Correlations between the residential concentration of politicians vs. socio-economically advantaged groups.



*Note:* The figure shows the correlation coefficient for the concentration of politicians across neighborhoods within municipalities, and the concentration of citizen types across those same neighborhoods. The exact calculation of these concentration measurements is explained in section 5. Observations are weighted by the inverse of the number of precincts in the municipality (N=17,427).

The split by political ideology reveals a substantial difference. Center-right politicians’ homes are more strongly concentrated in advantaged neighborhoods than left-wing politicians’ homes across all four measurements of advantage. Another striking result is that left-wing politicians are not counterbalancing the locations of center-right politicians in terms of living in the opposite types of places. The skew of politicians’ residences as a whole toward advantaged neighborhoods comes from a strong skew of the center right and a zero (or at least much weaker) skew for the left.

The results across the political blocs beg the question of whether politicians’ homes are skewed compared to the distribution of *their own voters*. Center-right politicians might be more over-represented in more affluent neighborhoods than the left bloc,

but still be more similar to their own voters than politicians from the left bloc. We recalculate the concentration measures for the politicians, replacing all non-politicians as the non-focal group with non-politicians who voted for the political bloc. This analysis shows that both left and center-right politicians are more likely than their own voters to live in socio-economically advantaged neighborhoods (see Figure A5). For three of the four groups (high income, high education and homeowners) this pattern is stronger for left bloc politicians.

While we might suspect that politicians live in more advantaged areas because their political office helped them afford to move there, we replicate Figure 1 for residential neighborhoods in election years for first-time councilors and demonstrate that this is not the case. This graph is strikingly similar to Figure 1 (see Figure A6). In Appendix Section A.5, we also verify that councilors are not more likely to move than the average citizen in the municipality, or more likely to move to a more advantaged neighborhood. We also show the absence of different moving patterns between majority and opposition politicians, which is important for the later analysis of local public goods.

Politicians might live in advantaged neighborhoods because they are themselves advantaged. Unfortunately, there is no straightforward way to find out if this is the case. We can, however, note that large proportions of politicians belong to affluent groups. Municipality averages show that 54% of politicians have a high income compared to 20% of the adult population; 48% vs. 24% have tertiary education; and 97% vs. 96% are OECD born. This over-representation exists in both ideological blocs but is more pronounced in the center-right bloc.<sup>12</sup>

In the Appendix we show that our results are not sensitive to splitting the sample by median municipality size (see Figure A6) or to replacing the concentration measure with simple shares of politicians, high-income people, etc. in the neighborhood's

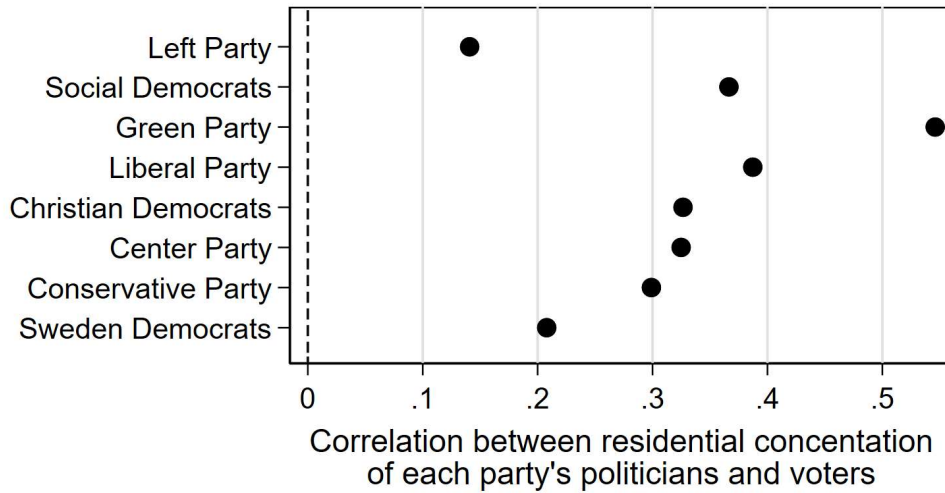
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<sup>12</sup>Among left-bloc politicians, 52% have a high income, 41% a high level of education, and 96% OECD born. In the center right, 59% have high income, 57% a high level of education, and 98% OECD born.

population (see Figure A8). There are potential drawbacks with our approximation of neighborhoods as electoral precincts. Even if precincts follow natural boundaries, they might be larger (or smaller) than the “natural” neighborhoods of a city. The precincts may also be more appropriate for people who live in its center rather than at the border. We construct a sensitivity test for these issues using our fine-grained geocoded data. The coordinate information for people’s homes allows us to calculate individualized neighborhoods by drawing concentric circles around each person based on the  $k$ -nearest neighbor approach (Östh, 2014). We describe the approach and the results in detail in Appendix section A.4. They confirm Hypothesis 1A by showing that politicians live in socio-economically advantaged neighborhoods, and this pattern becomes stronger when we use a more narrow definition of each persons’ neighborhood.

**Results: Voters.** Figure 2 plots the residential concentration of politicians from each of the eight parties in the Swedish parliament against the residential concentration of their voters. When we measure the concentration of voters for a party, we use voters for all other parties as the non-focal category. An alternative approach, which yields highly similar results, expands this reference group to also include people who did not vote (see Figure A7).

Figure 2: Correlations between the residential concentration of each party’s politicians and voters



*Note:* The figure shows the correlation coefficient for the concentration of politicians across neighborhoods within municipalities, and the concentration of voters for the same party. Section 5 explains how these concentration measurements were calculated. Observations are weighted by the inverse of the number of precincts in the municipality. N=17,427 neighborhood-year observations in three yearly, pooled cross-sections, 2002, 2006, and 2010.

All parties have positive correlation coefficients. This confirms Hypothesis H1b by showing that politicians from all parties are more likely to live in a neighborhood where a larger proportion of the party’s voters live. The coefficients are 0.3–0.4 for most parties, which means that a 1-standard-deviation higher concentration of the party’s voters is associated with a 0.3–0.4 standard deviation higher concentration of its politicians. Two parties, the Left Party and the Sweden Democrats, stand out with weaker correlations (0.14 and 0.21), while the Green Party stands out with a stronger one (0.55). The results in this sub-section are robust to the same sensitivity tests that we used in the previous section (see Figure A7 and Figure A8).

## 6 Politicians' homes and the spatial allocation of local public goods

We select all building permits for housing and divide them into two categories: *multifamily homes*, which are apartment buildings, and *single-family homes*.<sup>13</sup> The average municipality approved 5.7 multifamily home permits per election period, comprising 231 apartments, and 103 permits for single-family homes for a total of 141 housing units. The average permit for multifamily homes had 17 times more floor space than the average permit for single-family homes. In all three election periods, nearly two-thirds of municipalities approved at least one permit for multifamily homes, and more than 98% approved at least one permit for single-family homes. Using additional data on completed buildings, we can ascertain that more than 95% of all permits resulted in finished buildings within eight years of the approval (authors' own calculations, see footnote 14).

For each category of permits, we sum up the amount of approved floor space in each neighborhood and election period, starting in October in one election year and ending in September of the next election year. We then calculate the concentration of approved floor space across neighborhoods within each municipality. As mentioned above, we measure the concentration of permits relative to the distribution of the municipal population across neighborhoods. The concentration measure should therefore be interpreted as the neighborhood's share of all the approved floor space in the election period relative to its share of the municipal population. Figure A10 illustrates the distributions of both concentration measures for each type of building permit.

For school closures, we create a binary dependent variable that takes the value 1 if there was a proposal to close at least one school in a neighborhood during the election

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<sup>13</sup>Single-family homes include small proportions of semi-detached houses (0.5% of the permits) and townhouses or row houses (4%).

period and 0 for neighborhoods that had at least one school but no proposals. Two-thirds of the neighborhoods have at least one school and there are a total of 909 proposals. In all three election periods, there was at least one proposal in 40% of the municipalities.

Notably, we do not analyze finished buildings or implemented school closures. This reduces measurement error caused by long implementation times, which often drag into the next election period, which would make it more difficult to identify the politicians in charge. While a potential limitation of our approach is that since decisions may be reversed before implementation, our variables may fail to capture actual changes in people’s life conditions. Such reversals are very rare for building permits,<sup>14</sup> but are likely more common for proposals to close schools.

**Identification strategy.** We want to test whether politicians’ homes cause fewer local public bads to be located in a neighborhood (Hypothesis 2). Our identification strategy for this causal effect has two components that seek to address the problem of omitted variables. The high likelihood of such a problem is indicated by the previous result that politicians live in advantaged neighborhoods. These neighborhoods not only have more politicians living in them; they also have greater anticipated or actual public protests, and a systematically smaller or larger profitability of different local public bads. Because they have more affluent residents, they are also more likely to house swing voters (see the models in Lindbeck and Weibull, 1993; Dixit and Londregan, 1996).

As a first component of our identification strategy we disaggregate the concentration measure for politicians by accounting for political power. Specifically, we calculate the concentration measure separately for majority and opposition politicians,

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<sup>14</sup> For a subset of our data, we can provide the construction times for buildings. Starting in 2010, each newly constructed building in the Swedish property register can be linked to its building permit. Of all permits issued in 2010, 95% resulted in finished buildings eight years later, i.e. before the end of our sample period in 2018. Far fewer (6%) were finished in the same year as the approval, one-third were finished within two years, and two-thirds within three years. The average completion time was longer for multifamily homes than for single-family homes.

and subtract one from the other in each neighborhood. The new variable captures whether a neighborhood contains more (or fewer) homes of majority relative to opposition politicians (Figure A9 shows the distribution). A map that marks where individual politicians live within a municipality that had a close election illustrates our identifying variation (see Figure A2). Politicians’ homes are not only unevenly spread out across neighborhoods; politicians from the two blocs are also unevenly spread out relative to each other. As power shifts hands between the blocs, this becomes our identifying variation. Second, we restrict the analysis to close elections. The intuition for this restriction mimics that of a regression discontinuity design (RDD) analysis. Where levels of political competition are high, the winning bloc of political parties is determined by chance. Thus, in these elections, which neighborhoods have more majority than opposition politicians should also be determined by chance. This makes the characteristics of neighborhoods that are dominated by majority vs. opposition politicians more comparable on both observable and unobservable characteristics.<sup>15</sup>

We use two definitions of close elections. First, we define an election as generating a *power shift* if power shifted between the two blocs at least once in the last three elections (49% of the data). Electoral data is available to calculate this variable for all three elections in our sample. Second, we define an election as having a small *win margin* if either bloc’s margin to a seat majority was below 5% of the total vote share (48% of the data).<sup>16</sup> The results are not sensitive to variations in these definitions, as we show below. Both types of close elections are spread out across

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<sup>15</sup>A fully fledged RDD cannot be implemented in our context. The seat majority for one bloc does not *always* lead to a ruling majority from that bloc. In about 10–15% of close elections, small parties form coalitions across bloc lines. Defining the forcing variable is also complicated by the existence of two relevant seat thresholds – a center-right seat majority and a left-wing seat majority. A fuzzy RDD analysis could, in theory, use either of these thresholds separately, but this would generate quite imprecise estimates (results are available upon request). Nor is it possible to conduct an RDD analysis of individual politicians who get elected (or not) to the council. The marginal “losers” typically sit on the council as substitutes and progress into office as their colleagues leave during the term.

<sup>16</sup>For a description of how the win margin is calculated, see Folke and Rickne (2020)

municipalities throughout the country (see Figure A1).

We regress each of the three dependent variables ( $L_{nt}$ ) on the difference in residential concentrations between majority and opposition politicians ( $P_{nt}$ ) using the following equation:

$$L_{nt} = \alpha_{mt} + \beta P_{nt} + \gamma \mathbf{X}'_{nt} + \epsilon_{nt} \quad (1)$$

where  $\beta$  is our estimate of interest. Fixed effects for the combination of municipality and election period are included in the analysis of school closures. They are not needed for the building permits since those dependent variables only contain variation within the municipality-election period. We report the results from this bivariate regression before and after adding a vector of control variables ( $\mathbf{X}_{nt}$ ). As we show in the next section, these controls are uncorrelated with the treatment and therefore have little effect on the size or significance of our coefficient of interest.

**Testing the identifying assumption.** Our identifying assumption is that the treatment variable is independent of other variables (both observable and unobservable) that could also affect the allocation of local public bads. Following our previous discussions, such omitted variables are likely to stem mainly from public protests, swing voter residents, and profitability concerns.

Protests make neighborhoods less attractive for a local public bad. They raise costs by causing delays, increasing the bureaucratic workload, and even forcing the early termination of projects before completion. It is therefore no surprise that empirical research shows that politicians take anticipated protests into account when deciding where to site local public bads (Aldrich, 2008; Grimes and Esaiasson, 2014). A neighborhood's protest potential is shaped by who lives there. Money and personal networks are particularly important resources in mounting political opposition to unwanted projects (van Stekelenburg and Klandermans, 2013). High socio-economic status predicts protest as part of its general positive impacts on political participation (as discussed above, and see also empirical work on protests by, e.g., Grimes and



Norén Bretzer, 2008; Einstein et al., 2019). Empirical studies also show that people in single-family homes tend to protest more than others (Pendall, 1999; Whittimore and BenDor, 2019).

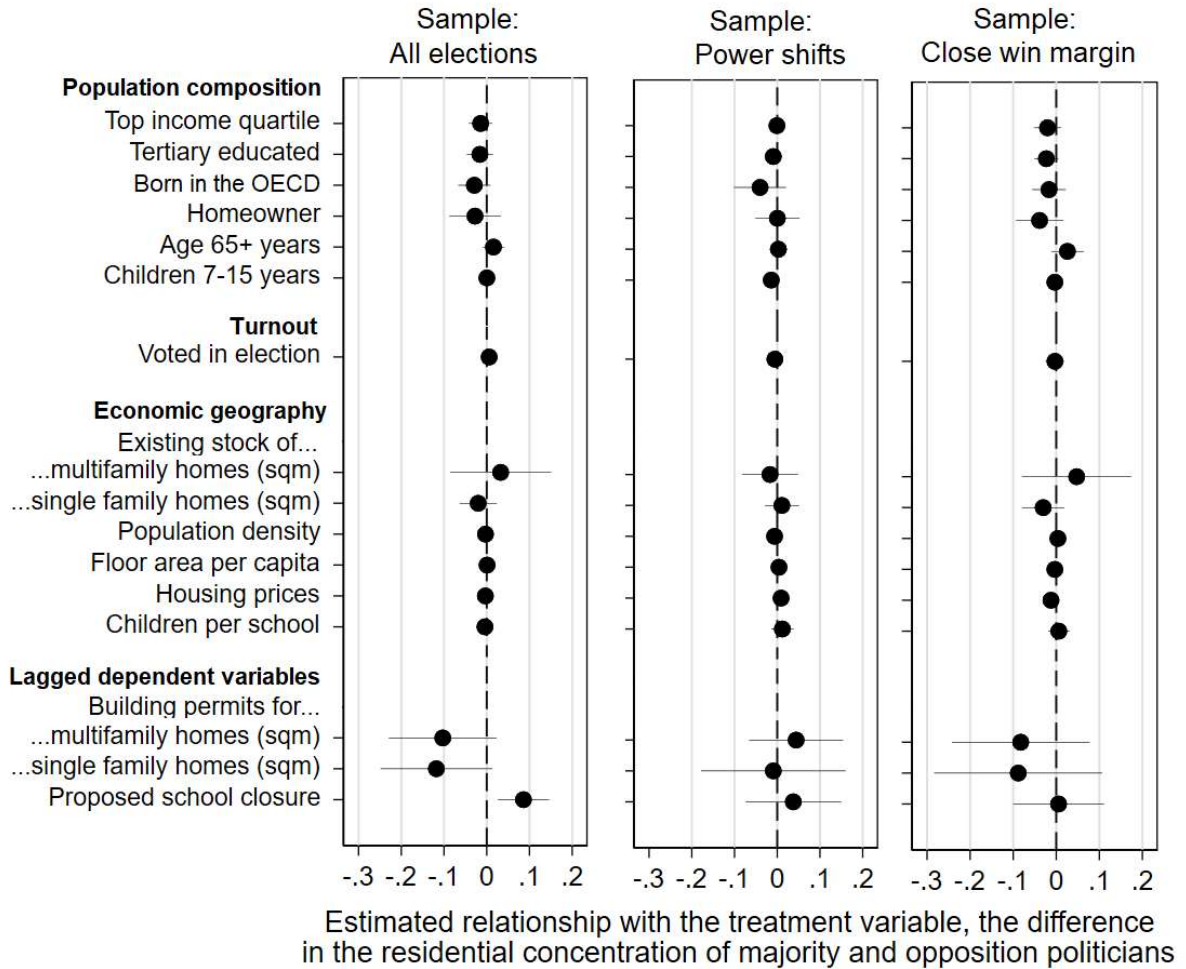
The party might center its efforts on neighborhoods where they are most likely to produce more votes. For evidence that political parties cater to swing voters in Swedish politics, see e.g., Dahlberg and Johansson, 2002; Johansson, 2003. Similar to the protest potential, the share of swing voters in a neighborhood has an expected correlation with socio-economic status. According to the probabilistic voting models in Lindbeck and Weibull (1993) and Dixit and Londregan (1996), there is a trade-off between political preferences and income. The stronger citizens' political preferences are and the richer they are, the harder they are to "buy" for the incumbent. The likelihood that voters will switch party allegiances may, hence, be associated with traits that are correlated with income, implying that swing voters might be unequally distributed over neighborhoods (c.f. Hypothesis 1). Our empirical analysis will control for these types of concerns to study the causal impact of politicians' homes.

A neighborhood's profitability for a specific local public bad is shaped by both its demographics and economic geography. Housing construction may be more profitable in affluent areas, which tend to have more complementary infrastructure and more willing construction companies, and where land sales bring in more money to the public budget. For school closures, it is more profitable to target smaller, rural schools in depopulating areas and older schools with greater investment needs (Uba, 2016; Larsson Taghizadeh, 2016). Such closures help the municipality save money by exploiting economies of scale and adapting the school system to changing demographics.

We create a number of variables to capture these dynamics. We then show that they are uncorrelated with the treatment variable by plotting estimates from bivariate regressions between the two, using the full sample and the two sub-samples of municipalities with close elections.

Starting at the top of Figure 3, we see a lack of correlations for six variables capturing neighborhoods' population composition – the concentration measures for the four socio-economically advantaged groups analyzed previously, as well as people aged 65+ and children of primary school age (7–15). The next estimate shows no correlation with the concentration of people who voted, a common proxy for anticipated protest intensity in previous research (Aldrich and Crook, 2008; Grimes and Esaiasson, 2014). Next, we show that there are no correlations with the six economic geography variables, which capture the pre-existing built environment in the neighborhood. They are calculated in the first year of each election period. We examine the concentration of *existing* square meters of multifamily homes and single-family homes, the population density, the existing floor space per capita, and the neighborhood's average house prices. As an approximation of school size we also look at the number of children (aged 7–15) per primary school. The bottom rows of the figure show estimates for the one-period time lag of the three dependent variables, i.e. their values in the previous election period. In the full sample of elections, the treatment variable has sizeable and near-significant correlations for all three lagged dependent variables. This could reflect a treatment effect in the previous election period in a subset of political strongholds. If the same political majority stays in power over time, and continues to live in the same neighborhoods as the opposition, and decides to place fewer public bads in their neighborhoods, this correlation would arise. But it could also reflect a more problematic situation. Majority and minority politicians might live in neighborhoods with different probabilities of receiving local public bads for reasons other than our intended treatment effect of politicians' homes, but which are not captured in correlations with the other predetermined variables. Reassuringly, the middle and leftmost panels demonstrate that these imbalances go to zero in the samples of close elections, particularly for the sample with power shifts. This finding suggests that political strongholds explain the correlation with the lagged dependent variable in the full sample.

Figure 3: Correlations between neighborhood characteristics and the spatial concentration of politicians.



*Note:* The figure shows estimates from bivariate ordinary least squares (OLS) regressions between the treatment variable and predetermined neighborhood characteristics. The treatment variable is the difference in residential concentration of politicians from the political majority and opposition. Horizontal lines show 95% confidence intervals. Standard errors are clustered at the municipality level, and the inverse number of precincts per municipality is used as sample weights. The data is three cross sections in 2002, 2006 and 2010. The two samples of close elections each constitute about 50% of the 870 elections in these three cross-sections. The power shifts sample includes municipalities with at least one shift in the governing majority within the three last elections. Close margin elections are those in which the vote margin to seat majority for either the left or center-right blocs was less than 5 percentage points. All variables are described in Appendix Section A.2.

In sum, Figure 3 verifies our identifying assumption by showing that the treatment variable is uncorrelated with a host of predetermined variables. Analysing the dif-

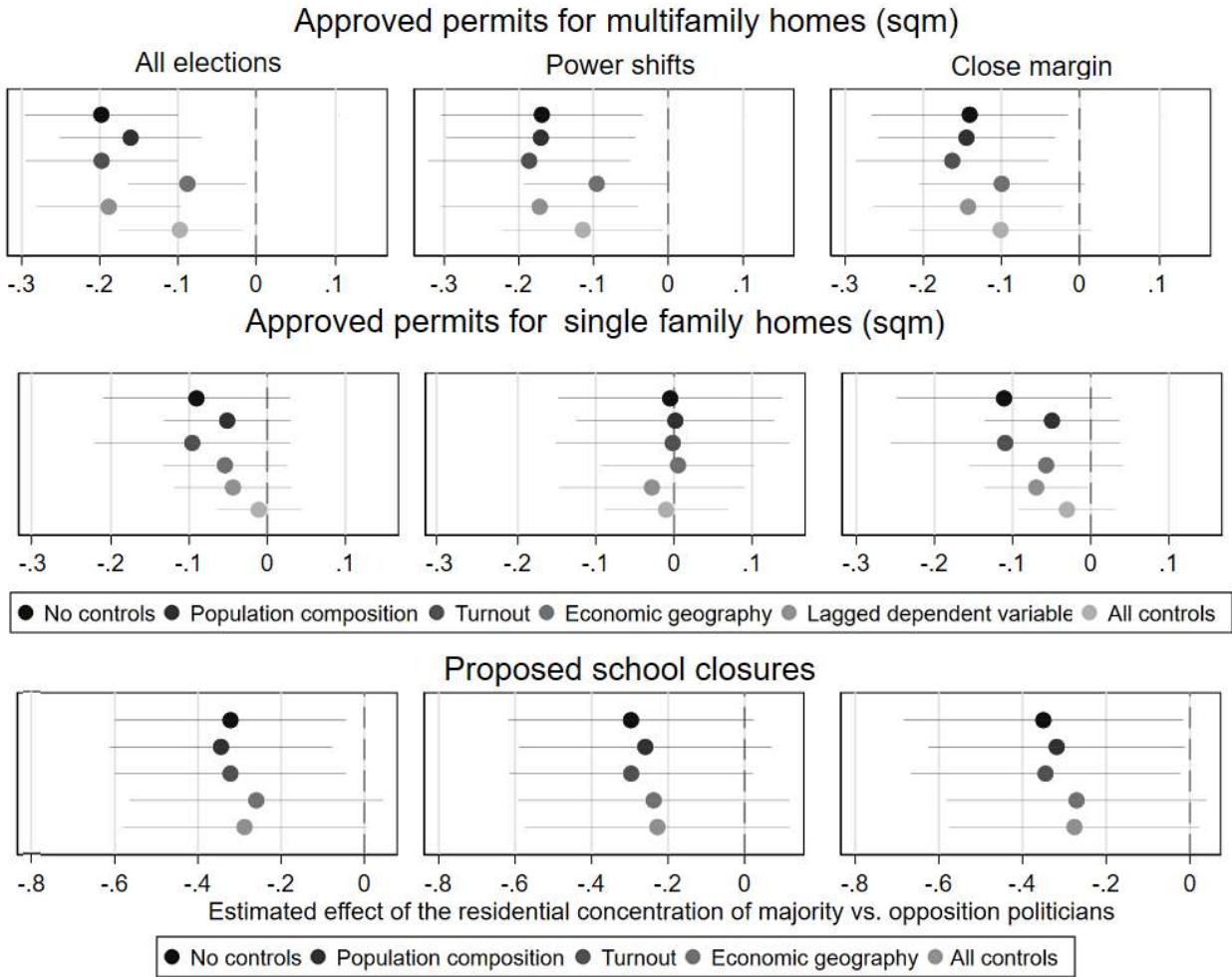
ference between where majority and opposition politicians live, rather than where all politicians live, addresses the correlations with demographics seen in the first part of the paper. In the sub-sample of close elections, and especially when relying on power shifts, we minimize the correlations between the treatment variable and proxies for confounding factors. Arguably, because these correlations approach zero, differences in unobservable neighborhood traits are also likely to be balanced.

**Results.** We estimate Equation 1 and report the estimated treatment effect in Figure 4. The left graphs show estimates for the full sample of municipalities, and the other two columns report the results for the close election sub-samples. The bivariate estimates are shown first, followed by estimates from specifications that include the variables from the previous section as controls; these capture the population composition, turnout, and economic geography of the neighborhood (for variable details, see Section A.2).

The results show fewer permits for multifamily homes in neighborhoods with more majority politicians than opposition politicians. The estimate with no controls in the upper-left graph shows that a 10-percentage-point increase in the relative concentration of majority politicians reduces the allocation of approved multifamily homes by 2 percentage points. For the two sub-samples of close elections, the effects remain statistically significant at the 5% level and are only marginally smaller.

The only set of control variables that affects the point estimates are those for economic geography, which includes a control for the concentration of existing multifamily homes. While these variables are only weakly related to the treatment variable (recall Figure 3), they are very highly predictive of the concentration of new permits, which is why they shift the estimates. However, while the size of the estimate drops by over half in the full sample, the reductions are less than a third in the close elections sub-samples. Importantly, the standard errors are also reduced by over 20% when we include economic geography controls, which implies that the reduction in the estimates could be attributed to increased precision rather than

Figure 4: Main results.



*Note:* The figure shows OLS estimates of Equation (1). Horizontal lines indicate 95% confidence intervals. The two sub-samples of close elections each constitute about 50% of the 870 elections. The power shifts sub-sample contains municipalities with at least one shift in the governing majority within the three last elections. Close margin elections are those in which the vote margin to seat majority for either the left or center-right bloc is less than 5 percentage points. The control variables in each category are listed in Figure balance. Standard errors are clustered at the municipality level.

reduced bias.

We do not find that the concentration of majority politicians' homes impacts the allocation of permits for single family homes. The estimated coefficients are very close to zero in the sub-sample with municipalities that have power shifts. In the full sample and close margin sub-sample, the bivariate estimate is statistically significant and sizeable, but approaches zero as the control variables are included.

We now turn to the results for the allocation of proposed school closures in the bottom row of graphs. The one difference compared to the analysis of building permits is that we omit the lagged outcome as a control, for the simple reason that schools that were closed in the previous period cannot be subject to a proposal in the current period.

The results for proposed school closures show a negative effect from the presence of politicians' homes in the neighborhood. Across specifications and samples, the size of the estimates are around -0.3. This implies that a 10-percentage-point larger share of politicians in the governing majority rather than the opposition who live in the neighborhood will reduce the probability of a proposal to close a school in that neighborhood with 3 percentage points. When all control variables are included in the estimation, the point estimates are slightly smaller in size and the standard errors grow. But even in these demanding specifications, the p-values remain relatively small (at 0.055 for the full sample, 0.20 for power shifts, and 0.07 for elections with a close win margin).

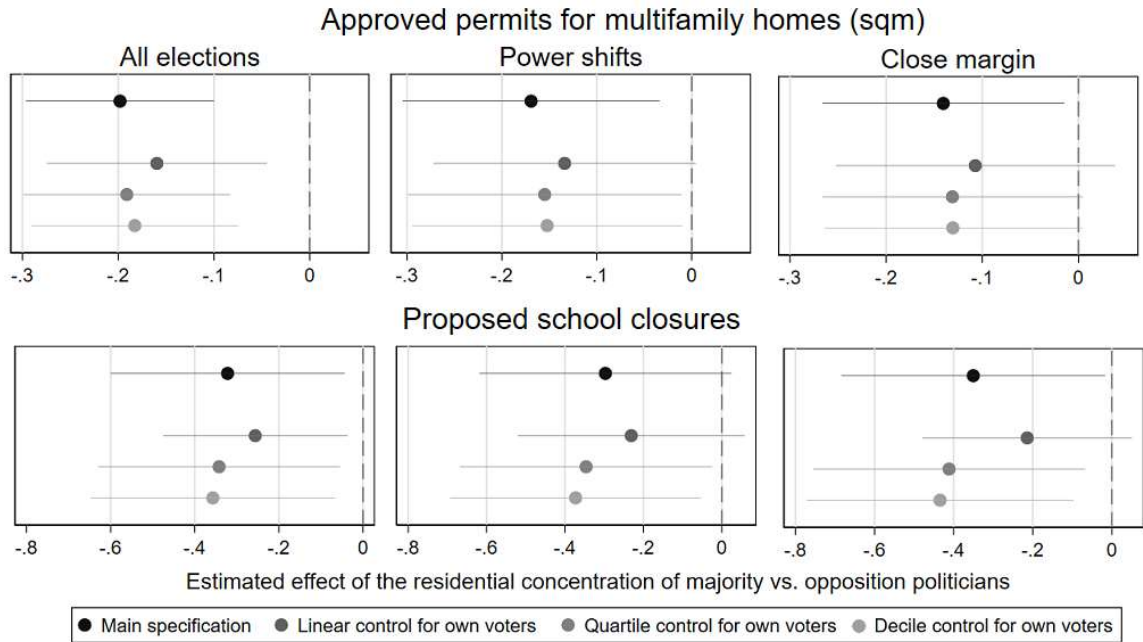
We interpret these results as confirmation of Hypothesis 2. For multifamily homes and school closures, the probability that these local public bads are placed in a neighborhood is reduced by the presence of politicians homes. The fact that we do not see any clear results for single family homes is not grounds for rejecting the hypothesis because these buildings are clearly less of a local public bad, as discussed above (see also Hankinson and Magazinnik, 2020).

So far, we have estimated the treatment effect at the neighborhood level. To better understand the magnitude of the estimated effects, we calculate the aggregated treatment effect from changing the governing majority in a typical municipality. The size of this aggregated impact depends on the size of the treatment effect, and how much the treatment variable would change across all of the municipality’s neighborhoods with the power shift. Appendix Section A.6 shows the exact calculation. We approximate that a shift in the governing majority leads to a re-allocation of 14% of the permits for new multifamily homes. For comparison, switching from a PR to majoritarian electoral system would generate an estimated 48% reduction in this type of building permit (Hankinson and Magazinnik, 2020). For school closures, our calculation shows that a switch in the governing majority leads to a 19-percentage-point drop in the likelihood of a proposed closure of schools in the neighborhoods where the new majority dominates.

The Appendix reports the results of our sensitivity analyses. Figure A12 shows that the result for multifamily homes is clearer in larger municipalities. The estimated effects are of a similar size, but less precise, in smaller municipalities. The negative effect on school closures is clearly concentrated to below-median-size municipalities. Figure A11 shows that the results are not sensitive to varying either of the thresholds that define close elections.

**Mechanisms: Political representation and favoritism.** Our theoretical framework specifies two possible reasons why fewer local public bads may be allocated to the neighborhoods of politicians – they favor their own neighborhoods, or they allocate fewer public bads to neighborhoods with more voters for their party. The result that politicians live in neighborhoods with more supporters of their own party also holds for party blocs. To test whether this pattern of co-residence explains the main result on local public bads, we add control variables for the residential concentration of voters to Equation 1. The intuition is that if co-residence with voters explains why fewer local public bads are allocated to majority politicians’ neighborhoods,

Figure 5: Main results with added controls for majority-party voters' concentration in neighborhoods



*Note:* The figure shows OLS estimates of Equation (1). Vertical lines indicate 95% confidence intervals. The linear control variable is defined as the difference in residential concentration between voters for the majority and the opposition. The quartile and decile controls are dummies for each municipality quartile or decile of this difference. Standard errors are clustered at the municipality level.

the estimates should approach zero when we include these controls.

We specify the controls for the concentration of voters in two ways. The first is a linear control variable for the difference in residential concentration between voters for the majority and the opposition. The second control is dummy variables for deciles and quartiles of that continuous variable, calculated for each municipality. We present the results of this analysis in Figure 5. We first show the bivariate estimates from our main analysis as a reference point and then add each of the vote controls.

Irrespective of the outcome variable, the sample, or how we control for the concentration of voters, the results in Figure 5 show that there is only a marginal reduction



in the estimates of the effect of the residential concentration of majority politicians. Thus, there is little evidence that the location of politicians' homes affects decisions about how to allocate local public goods and bads, because such decisions cater to voters for the politicians' parties for programmatic or clientelistic reasons. The finding that the concentration of voters for the majority relative to the opposition is only weakly related with the allocation of local public bads (not shown) further supports this conclusion. Thus, just like Carozzi and Repetto (2019), we do not find that parties benefit the precincts of their own voters when allocating public goods and bads.

By the process of elimination, our results suggest that the main result is likely explained by politicians favoring their home neighborhoods. This might mean that the large aggregated treatment effects calculated above are lower bounds. Favoritism is likely to be less prevalent in more competitive environments with close elections. In those contexts, the opposition might be able to place the most restrictions on the behavior of the majority, and bad behavior is more likely to result in losing votes (and thus power).

## 7 Conclusion

Where do politicians live, and does it matter? We find that local politicians live in affluent neighborhoods with relatively large shares of voters for their party. We also find that where they live matters, because neighborhoods in which more politically influential politicians reside have fewer multifamily homes and proposals to close schools, but the same level of construction of single-family homes.

Even if politicians tend to live in the same neighborhoods as their voters, this does not explain why they shield these neighborhoods from local public bads. Through the process of elimination, our results suggest that favoritism is a more likely mechanism. If this interpretation is correct, our results likely have negative implications,

because they indicate that politicians (either consciously or unconsciously) let personal considerations influence important decisions about construction and local public services. Such behavior might erode trust in the local political system, and may lead to sub-optimal (and potentially more costly) decisions.

It is somewhat surprising to find these results in Sweden, which typically ranks as having one of the lowest levels of corruption in the world (TI, 2020). Yet such international corruption indices usually apply to the national level. At the local level, 40% of Swedes agreed with the statement that "local politicians often use their political positions to benefit themselves or people close to them" (Bergh et al., 2013). As in other countries, the local level has less media presence and fewer bureaucratic personnel to monitor political decisions than the national level. However, local decisions often greatly affect people's lives.

More broadly, our results suggest that neighborhood traits might be an important aspect of political representation. Prior research has concluded that socio-economically disadvantaged groups are descriptively under-represented in politics (Dancygier et al., 2020; Carnes, 2020). Our results extend this literature to demonstrate that these groups are also less likely to have any politicians as neighbors. The *descriptive* under-representation of less affluent individuals thus further decreases their *substantive* representation. Conversely, politicians may not only under-represent the preferences of low-income citizens because they personally earn a high income, but also because they live in neighborhoods with more socio-economically advantaged people (Page et al., 2013; Gilens, 2012; Broockman and Skovron, 2018).

Unfortunately, our research design cannot determine whether politicians' skewing of the allocation of local public goods increases inequality. Our margin of causal identification is only between having homes of politicians with different degrees of power, not between having more or fewer homes of politicians. Future studies could find other empirical designs to target this margin, such as a regression discontinuity design for being elected or not. They could also study a wider array of local public

goods and bads, such as the spatial allocation of local transportation networks, cultural buildings, parks, recycling stations or local security infrastructure.

Finally, several types of policies apply to the problems studied in this paper. Rules to ban politicians from taking part in decisions that affect their own interests are obviously important, as is strict enforcement of these rules. Reforms to electoral systems affect where politicians live, for example by spreading them out across single-member districts. This might lead to a more equitable distribution of local public bads, but could also lead to fewer of them being implemented, despite being viewed as beneficial at the aggregate level (Hankinson and Magazinnik, 2020). Less dramatic policies could target parties' internal nomination practices. In addition, most local parties in Sweden voluntarily print neighborhood names next to candidate names on their ballot; making this mandatory would give all voters the opportunity to consider geographic representation when they chose between parties and candidates.

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# Appendix to "Politicians' Neighborhoods: Where do they Live and does it Matter?"

## A.1 Data

We use population-wide administrative register data compiled and de-identified by Statistics Sweden. The data sources come from several different government agencies, such as the Tax Authorities, the Election Agency, the Employment Office, and the Agency for Education. Given that all citizens and individuals with a residence permit in Sweden have a unique ID number, it is easy to merge information from multiple sources. Since we follow elected politicians, we focus on three elections – 2002, 2006, and 2010. We drop the smallest municipality, which has only one precinct.

We add data from six additional sources to the individual-level data. Municipal-level election results are merged at the electoral precinct level from 2002 to 2010. The data includes the number of eligible voters, the number of people who voted, and the election result for each precinct. Because Sweden does not have voter registration but takes this information directly from the population register, eligible voters amount to all permanent residents 18+ who have a home in a given precinct.

The property register divides the country into plots of land, i.e. properties, and lists information about the buildings located on these plots for the period 1998–2014. The register contains information about the plot size which buildings stand on, the size of each property, the number of housing units on each plot, each building's floor area, and the type of building (multifamily or single-family home). The building permit register contains approved building permits in all municipalities for the period 1998–2014. Each permit details the intended neighborhood of construction, the month and year of approval, the number of housing units, whether the building(s) are single-family or multifamily homes, and the total floor space. A single permit often incorporates several buildings located on the same plot of land. The property and building permit register includes information about precincts and can therefore be directly merged at the precinct level with our individual-level data.

To calculate house prices in the neighborhood, we add data from the Swedish network of real estate agents (Mäklarstatistik AB) 2005–2014. Brokers in this network report data on their individual sales to a joint database, which comprises approximately 80% of all real estate transactions in the country in a given year. For each sale we know the location, size of the property and final price. The data also indicates whether a sale belongs to a newly constructed building. We drop these sales from the measurement since we are interested in capturing the economic geography of the neighborhood rather than the direct impact on the housing market from the new residential construction (i.e. our dependent variable). Information on home ownership comes from the database GeoSweden 1998–2014, which contains full population

register information about whether a person lives in a housing unit that is owned or rented. Note that some individuals living in an owned house might sublet it from someone else. Statistics Sweden collects and anonymizes the register information in this database, which is hosted by the Institute for Housing and Urban Research at Uppsala University. For house prices, home ownership, and school closures we lack information about precincts, and geocoded shape files are not available for all election years. To add the data we instead aggregate the individual information to 500\*500-meter geocoded squares before merging them into the grid in our dataset. We assign 500 × 500-meter squares to precincts if all properties in the square belong to the precinct (80–90% of the squares). The remaining squares, which straddle several precincts, are assigned to the precinct to which the largest fraction of buildings belong.

Table A1 summarizes the final data set.

## A.2 Definition of variables

*Top income quartile* is based on the national distribution of disposable income, which is the net annual sum of labor earnings (from jobs or business ownership) and income from government programs or transfers (e.g. unemployment benefits or pension).

*Tertiary educated* includes people with any kind of post-high school education. Universities and schools update information on new, completed course credits each year in the administrative data, and for immigrants, information is collected by the Employment Office and via targeted surveys by Statistics Sweden.

*Born in the OECD* is a dummy variable for people born in an OECD country, including Sweden. Immigrants' country of birth is recorded by the authorities as part of the immigration process.

*Home ownership* is based on register information about whether a person lives in a housing unit that is owned rather than rented. Note that some individuals living in an owned house might sublet it from someone else.

*Age 65+ years* is the concentration of people aged 65+ in the neighborhood.

*Children 7–15 years* is the concentration of children of primary school age.

*Voted in election* is the concentration of people who turned out to vote in the municipal election. Eligible voters who did not vote is the non-focal group in this study.

*Existing stock of multifamily homes* and *Existing stock of single-family homes* rely on property register data that captures the type and floor area of each existing building in the neighborhood. We sum up the existing floor space in square meters in each neighborhood and for each type of building (Table A1 reports the levels of these

variables). In the main analysis, this variable is used to calculate the concentration index, using the adult population as the non-focal group.

*Floor area per capita* is the sum of the floor area in each neighborhood divided by the number of adult inhabitants, based on property register data. The unit of measurement of this variable is the number of square meters of living space per adult in the neighborhood. The variable is normalized to the deviation from the municipality mean.

*Population density* sums the area of all plots of land in each neighborhood and divides it by the number of adult inhabitants; the unit of measurement is square meters per adult inhabitant. The variable comes from the property register and the variable is normalized to the deviation from the municipality mean.

*Housing prices* are measured as the average log selling price per floor square meter in each neighborhood during the election period. Before calculating the variable, we exclude all sales of new buildings from the data. The variable is normalized to the deviation from the municipality mean.

*Children per school* is the number of schools in the precinct divided by the number of children 7–15 years old. This variable captures average school size. The variable is normalized to the deviation from the municipality mean.

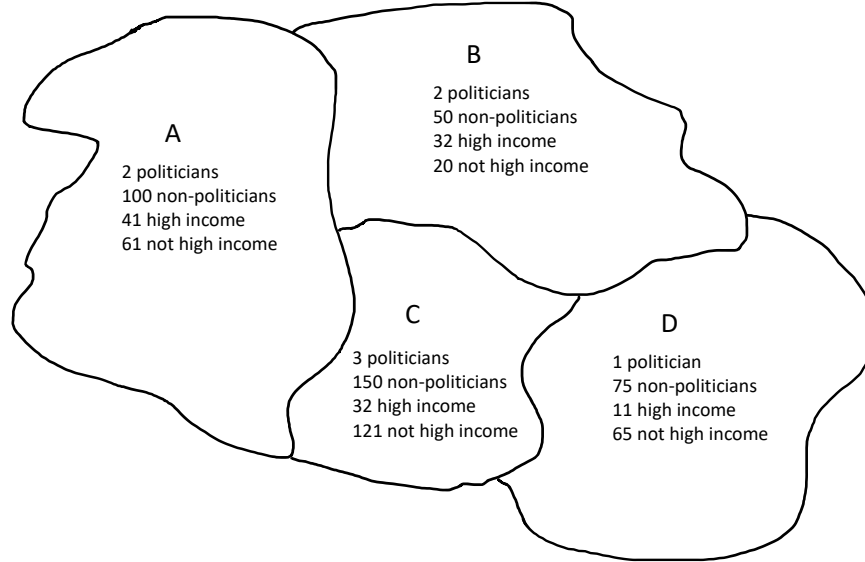
### A.3 Concentration measure

Figure A3.1 illustrates a municipality with four neighborhoods and two focal groups, politicians and high-income earners. For the sake of illustration, we have varied the population sizes more than the variation in our data (recall that precincts are of similar size). This fictitious municipality has 383 inhabitants, of whom eight are politicians and 116 are high-income earners.

To calculate the concentration of politicians in each neighborhood, we use the 375 non-politicians as the non-focal group. Neighborhood B has the highest concentration of politicians (25% of the politicians and 14% of the non-politicians); its concentration measure is 0.11 (see Table A3.1). This means that for the two shares to be equal, 11% of the politicians would have to move out of neighborhood B. The concentration of politicians is the lowest in neighborhood D, where 13% of the politicians and 20% of the non-politicians live, which gives it a concentration measure of -0.07. To make the shares equal, 7% of the municipality’s politicians would need to move into this neighborhood. Neighborhoods A and C also have negative concentrations of politicians (-0.02 and -0.02), meaning that an inflow of 2% of the municipality’s politicians would equalize the shares. Together, these four flows into and out of each neighborhood would equalize the share of politicians and non-politicians throughout the municipality.

Given that we can not divide a single politician between several neighborhoods, it is

Figure A3.1: Illustration of municipality with four neighborhoods



not possible to completely avoid segregation. However, if one politician moves from neighborhood B to D, the politicians and non-politicians would be more equally distributed, and the concentration would be reduced from 0.11 to 0.05.

To calculate the concentration of the 116 high-income earners in each neighborhood, we use the 267 non-high-income people as the non-focal group. The concentration of high-income earners is also largest in neighborhood B, where 28% of the high-income earners and 13% of the non-high-income earners live, which yields a concentration measure of 0.21. The concentration is lowest in neighborhood D, where 9% of the high-income earners and 24% of the non-high-income earners live; its concentration measure is -0.13. The concentration measure for high-income earners is 0.31, which means that 31% of the high-income earners would have to move across neighborhoods (including C and D) in order for high-income earners to be distributed like the non-high-income earners and end segregation.

Table A3.1: Illustration of the calculation of concentration measurements

Neighborhood	Concentration of municipal politicians			Concentration of high-income earners		
	Share of politicians (1)	Share of non-politicians (2)	Concentration (1)-(2)	Share of high-income citizens (3)	Share of non-high-income citizens (4)	Concentration (3)-(4)
A	$\frac{2}{8} = 0.25$	$\frac{100}{375} = 0.27$	-0.02	0.35	0.23	0.12
B	$\frac{2}{8} = 0.25$	$\frac{50}{375} = 0.14$	0.11	0.28	0.07	0.21
C	$\frac{3}{8} = 0.38$	$\frac{150}{375} = 0.4$	-0.02	0.28	0.45	-0.17
D	$\frac{1}{8} = 0.13$	$\frac{75}{375} = 0.2$	-0.07	0.09	0.24	-0.13

## A.4 Individualized neighborhoods

We use coordinates for the  $500 \times 500$  square meter of each person’s home to calculate new individualized neighborhoods by drawing concentric circles around each person. The method, called the  $k$ -nearest-neighbor approach, was originally developed by Östh (2014). This approach puts the home of each person at the center of their neighborhood and allows us to vary neighborhood size. The algorithm identifies the  $k$  number of nearest neighbors by expanding the concentric circle around the individual until  $k$  neighbors have been found. Since we have coordinate points that measure  $500 \times 500$  meters, we will not always get this exact number. For example, assume that we are looking for the  $k = 50$  nearest neighbors of a specific individual. If more than 50 persons live at the same  $500 \times 500$  coordinate point, all those individuals are included. Likewise, if there are fewer than 50 people in that area, the algorithm starts searching in the neighboring coordinate points. If the sum of all individuals on those two points equals or exceeds 50, all those individuals will be used to calculate the neighborhood’s characteristics. Since the algorithm does not take municipality borders into account, an individual’s closest neighbors may live in another municipality if they live close to the border.

Once the  $k$  nearest neighbors have been identified, we can measure the characteristics of each individual’s neighbors, such as the share of the  $k$  nearest neighbors who are socio-economically advantaged. This allows us to compare the traits of the  $k$  nearest neighbors for different types of individuals such as politicians and non-politicians.

Since the  $k$ -nearest-neighborhood boundaries are individual and not the same across all individuals (unlike precincts) we cannot calculate and correlate the concentration measures. Instead, the analysis compares the shares of socio-economically advantaged neighbors between politicians and non-politicians. To account for the fact that the share of socio-economic advantaged individuals varies across municipalities and years, we first calculate the average share ( $\bar{y}$ ) within each municipality  $m$  and year  $t$ ,

and then take the ratio of the politicians' share ( $x$ ) to the average share (i.e.  $\frac{x_{tm}}{\bar{y}_{tm}}$ ). If the ratio is above 1, the politicians are more likely to have socio-economically advantaged neighbors, and vice versa if it is below 1. Unlike the concentration measure in the main analysis, this ratio is not independent of the relative size of the advantaged group, which makes differences in magnitudes of the ratios across groups less interpretable.

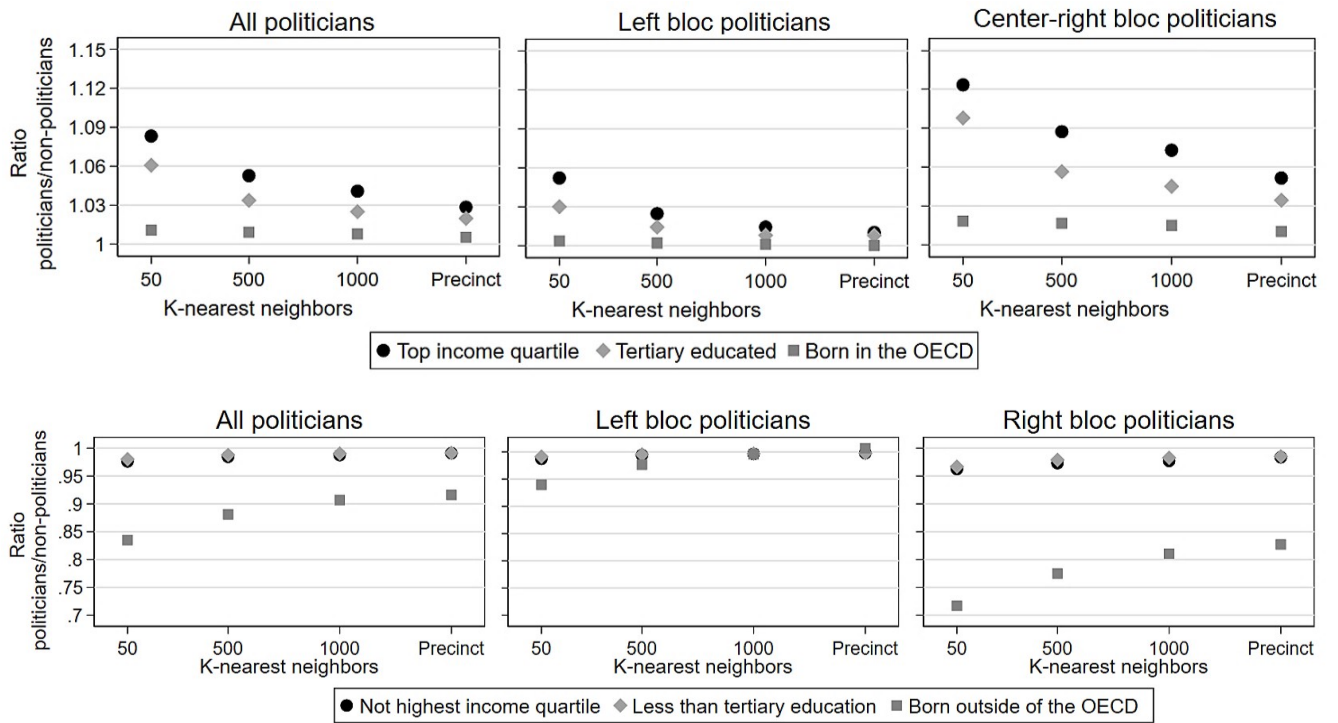
We present the results in Figure A4.1, starting with the ratios for all politicians in the left-most figure, left-bloc politicians in the middle and right-bloc politicians to the right. We calculate the ratios using three neighborhood sizes,  $k = 50, 500, 1000$  nearest neighbors. As a reference point, we also calculate the ratios using the precinct boundaries to define neighborhoods. Since we do not have home ownership connected to our main data at the individual level, we exclude it from the  $k$ -nearest-neighbor analysis.

The results in the top of Figure A4.1 confirm the results of our main analysis: politicians live in socio-economically advantaged neighborhoods. The more narrowly we define the neighborhood in terms of the number of nearest neighbors, the stronger this pattern becomes. Comparing the results across precincts and the  $k$  nearest neighborhoods, we see that the ratios are larger for the latter, even when we use neighborhoods based on the 1,000 nearest neighbors. This suggests that the fact that precincts include individuals living on the border of neighborhoods will underestimate the degree of segregation.

The difference between left- and right-wing politicians also holds up in the  $k$ -nearest-neighbor analysis. Politicians from the right bloc are much more likely to have socio-economically advantaged individuals among their closest neighbors than those in the left bloc. As mentioned above, it is hard to compare the magnitudes of these results across different affluence variables due to differences in their baseline levels. The ratios are smallest for those born in the OECD because this group makes up a much larger share of the politicians. To substantiate this point, we calculate the ratios after reverse coding the affluence variables, making the non-affluent group the focal group and vice versa. In this analysis, shown in the bottom of Figure A4.1, the relative sizes of the ratios across the affluence variables all drop below zero. However, the two groups that were furthest from 1 in the first graph are now closest to 1, while the ratio is furthest away for the group that was previously closest. This verifies that the relative size of the groups is a key explanation of the differences across groups in how far the ratios are from 1.

Overall, this analysis verifies that the results in Figure 1 are not created by the use of precincts to define neighborhoods. If anything, it leads us to underestimate the degree to which politicians live with socio-economically advantaged neighbors.

Figure A4.1: K-nearest neighbors



Note: Individualized neighborhoods are defined using the closest 50, 500, or 1,000 individuals.



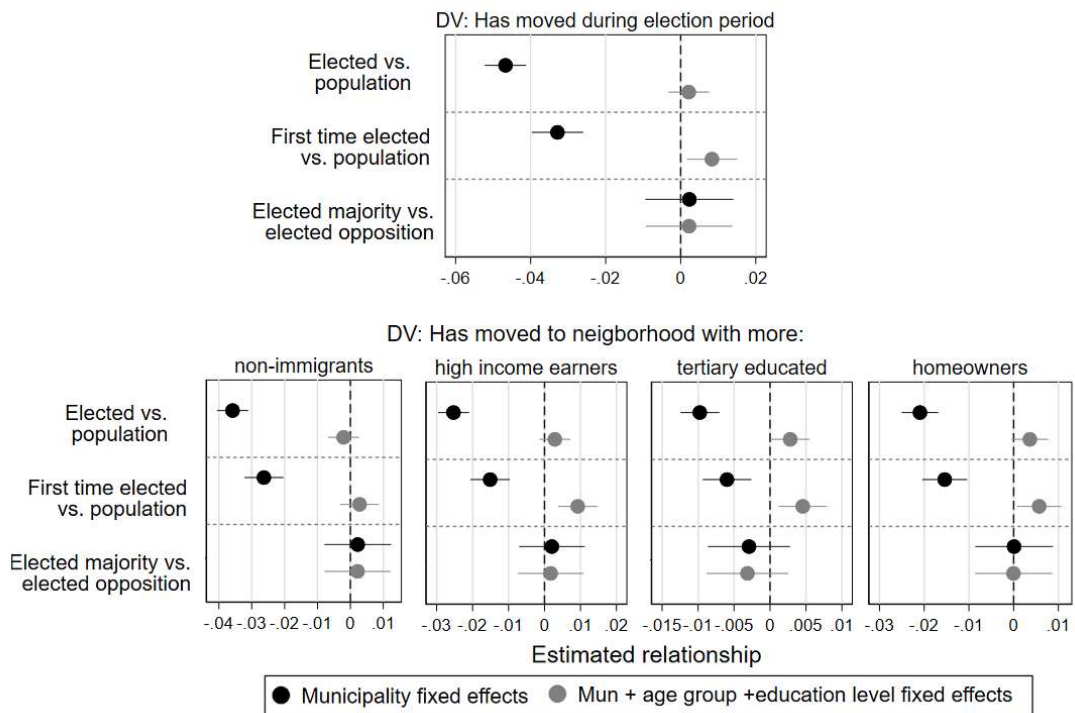
## A.5 Residential relocation

Because precinct boundaries change between elections, residential relocations are better measured using geocodes. Our yearly administrative data include identification codes for 500x500-meter geocoded squares of people’s homes. We create a dummy variable that takes the value 1 for persons whose home moves to a different geocoded square between election years, and 0 for non-movers and people who make very short moves that keep them within the square. We exclude observations for people who move to a different municipality.

We create additional variables to test whether moves take people to more (or less) socio-economically advantaged neighborhoods. We approximate which neighborhoods the pre- and post-move squares belong to using the same method that we use to merge geocoded data into the precinct grid (see section A.1). We then calculate the difference in the proportion of each of the four advantaged groups in the person’s neighborhood before and after the move. Because we are mainly interested in whether moves take people ”upward” in terms of neighborhood affluence, we create four binary indicators that take the value 1 for people who move to a neighborhood with a higher share of the group. These indicators are 0 for people who move ”downward,” or who do not move.

We use OLS regressions to estimate the differences in the probability of moving, and moving to a more affluent neighborhood, between the following groups: politicians compared to the adult population, freshmen councilors compared to the population, and majority versus opposition councilors. Regressions are run both bivariately (comparing the two groups by regressing moves on a binary indicator) and including fixed effects for life cycle patterns (fixed effects for 5-year age brackets and for seven education categories). The results are shown in Figure A5.1. It is apparent that elected politicians move less frequently than the overall population, but that this gap can be ascribed to differences in the distributions of age and education, i.e. life cycle effects. After holding age and education constant, the results show that first-time councilors are somewhat more likely to move compared to the population as a whole. The size of this difference is substantively small. An average of 21% of the population moves during the average election period, while the average among freshmen politicians is 1 percentage point higher. Politicians who are elected for the first time are also about 1 pp. more likely to move to a higher income area, 0.05 pp. more likely to move to an area with better-educated inhabitants, and 0.5 pp. more likely to move to an area with more homeowners. There is no difference in the probability of moving, or moving to a more advantaged area, between majority and minority politicians.

Figure A5.1: Analysis of moves to a new neighborhood between election years.



*Note:* The figure shows estimates from regressing dummy variables for moving (top) or moving to neighborhoods with more advantaged citizens (bottom). Black markers show estimates from bivariate regressions and gray markers from specifications that include age and education fixed effects. Data is pooled cross-sections of three election years (2002, 2006, and 2010).

## A.6 Aggregated treatment effects

To get a sense of the importance of the geographic distribution of political power for the geographic allocation of local public bads, we calculate what would happen to the allocation of the local public bad if a municipality experienced a shift in power from the existing majority to the existing opposition.

A rough calculation would proceed in three steps. First, we need to calculate how the shift in power would change the treatment variable in each neighborhood. Second, we calculate how the change in in the treatment variable would affect the allocation of the local public bad to each neighborhood. Finally, we aggregate the changes in the allocation of the local public bad across all neighborhoods within a municipality. This final step tells us how much a power shift would geographically reallocate the local public bads within the municipality.

To illustrate, for an imaginary municipality X, we provide the key statistics in Table A6.1. The municipality has six neighborhoods – three "majority" neighborhoods (i.e. a higher concentration of majority politicians than opposition politicians) and three "opposition" neighborhoods. A power shift would cause opposition neighborhoods to become majority neighborhoods, and vice versa. Based on our main results, the neighborhoods turning from opposition to majority neighborhoods would get less of the local public bad, and the former majority neighborhoods would get more.

Step 1: In neighborhood A, the treatment variable is -0.1 (column 3). That is, the concentration of opposition politicians is 0.1 greater than the concentration of majority politicians. A power shift would cause the treatment to flip to 0.1; in other words, it would increase the treatment variable by 0.2 (column 4). The change in the treatment variable is calculated for each neighborhood.

Step 2: In our main results, the estimated treatment effect for the concentration of multifamily homes is around -0.15. Multiplying this estimate by the shift in treatment for neighborhood A demonstrates that it would receive a 0.03 lower share (i.e. 3% less; column 5) of the municipalities' permitted floor space.

Step 3: To determine how much the allocation would change at the municipality level, we aggregate the change across all neighborhoods. Since an increase in one neighborhood will cause a decrease in another, we take the sum of either all of the positive or all of the negative changes when calculating the total change. We find that a power shift would move 10.5% of the permitted floor space for multifamily homes in the municipality from a current majority neighborhood to a current opposition neighborhood (bottom of column 5).

In the last two columns we perform the same calculation for school closures, assigning a school to two-thirds of the neighborhoods, which mirrors the current distribution. Neighborhood A has an open school; precincts without an open school cannot be affected. Multiplying the change in the treatment variable by the treatment effect,

Table A6.1: Illustration of the calculation of aggregate effects when the political majority shifts

Neighborhood	Type	Treatment variable	Change in treatment	Change in share of multifamily homes	Open school	Change in probability of proposed school closure
A	Opposition	-0.1	0.2	-0.03	yes	-0.06
B	Majority	0.15	-0.3	0.045	yes	0.09
C	Opposition	-0.05	0.1	-0.015	no	0
D	Opposition	-0.2	0.4	-0.06	yes	-0.12
E	Majority	0.15	-0.3	0.045	yes	0.09
F	Majority	0.05	-0.1	0.015	no	0
Aggregate				0.105		0.18

which for school closures is estimated to be about 0.3, we find that the probability of a proposal to close the school in this neighborhood will be reduced by 0.06 (6%). The aggregate change in the probability of school closures is 0.18. In other words, the probability that a school will close in a current majority neighborhood increases by 18%, and decreases by the same amount in the current opposition neighborhoods.

We can then use these municipality-specific calculations to approximate the aggregate effect of a power shift on the average municipality.

## A.7 Tables

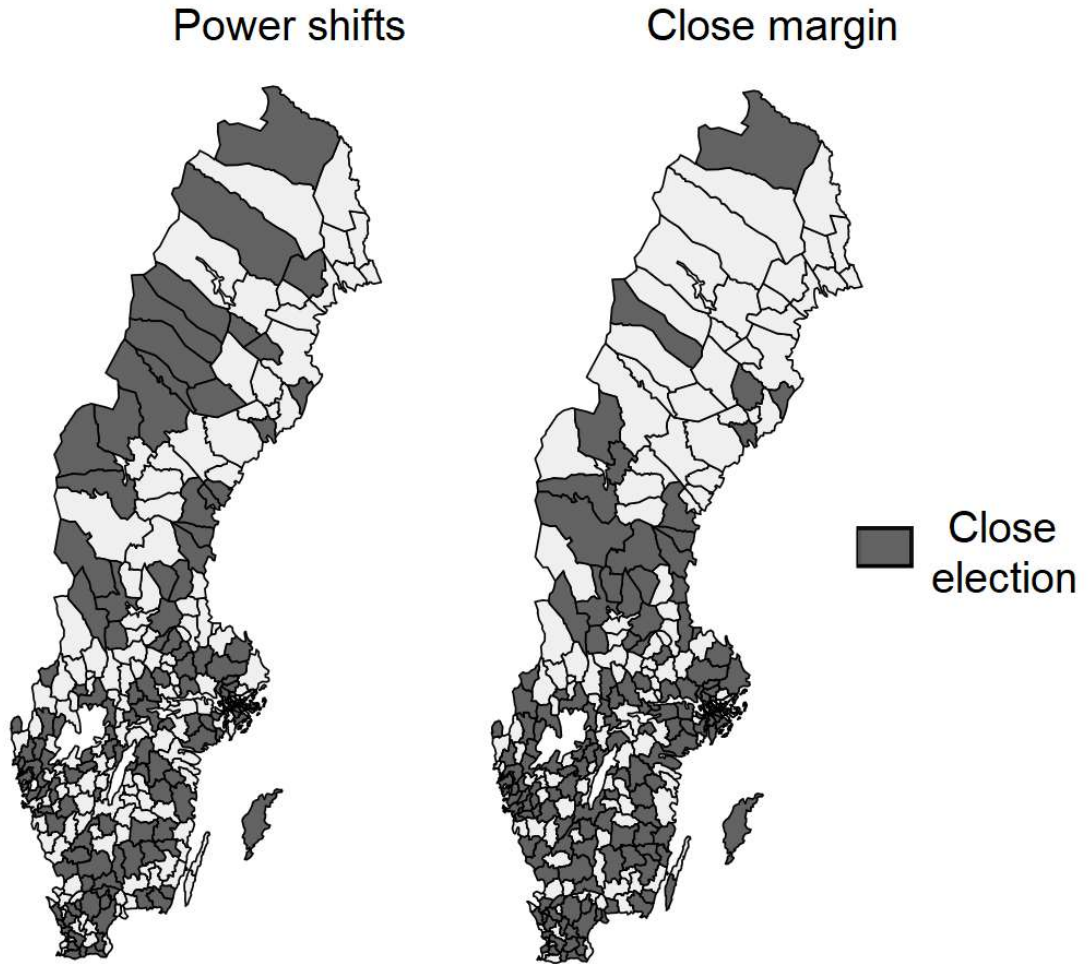
Table A1: Summary statistics

	mean	min	max	count
Population	1191.1	94	3091	17427
Top income quartile	249.7	6	1448	17427
Tertiary education	297.5	6	1781	17427
Born in OECD	1102.6	31	2590	17427
Home owners	613.0	0	6794	17427
Retired	295.8	0	1058	17427
Children age 7-15	166.3	0	859	17427
Voters	911.4	71	2094	17427
Multi family house square meeter	17462.8	0	824349.5	17375
Single family house square meeter	53191.3	0	369985	17375
Population density	7503.8	0	621158.5	17375
Floor area per capita	69.5	0	1161.7	17427
ln(Price per square meeter)	8.92	6.24	11.3	15835
Children per school	134.8	3.50	859	10454

*Note:* All observations correspond to one neighborhood-election year observation. Observations are weighted by the inverse of the number of precincts in the municipality.

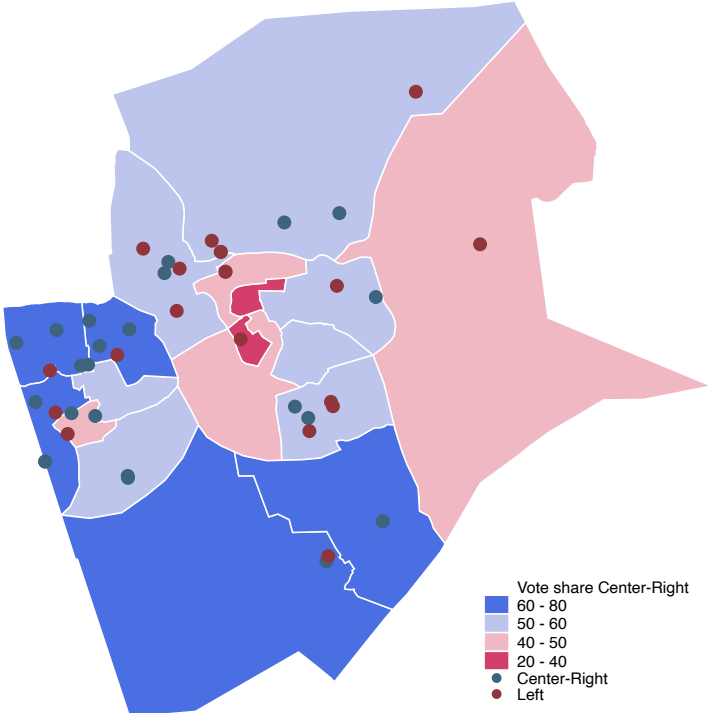
## A.8 Figures

Figure A1: Municipalities with close elections in 2010.



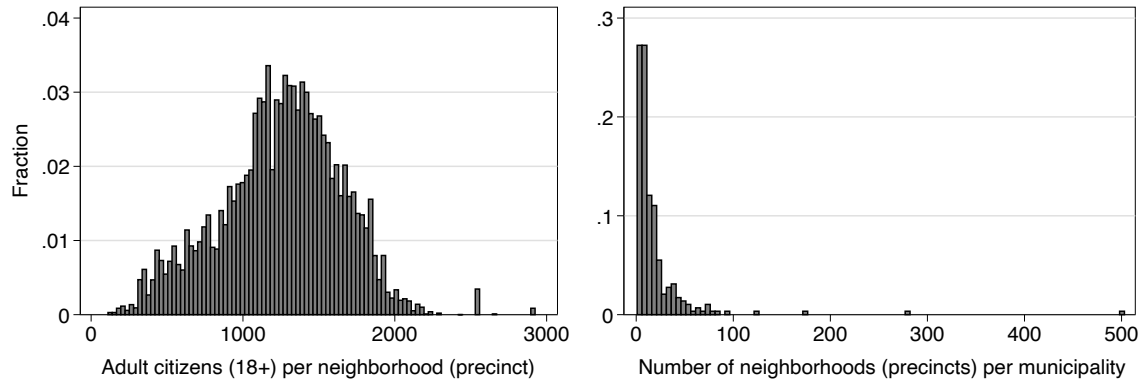
*Note:* The map shows which municipalities had close elections in 2010 according to both definitions.

Figure A2: Placement of municipal councilors' homes in Partille municipality.



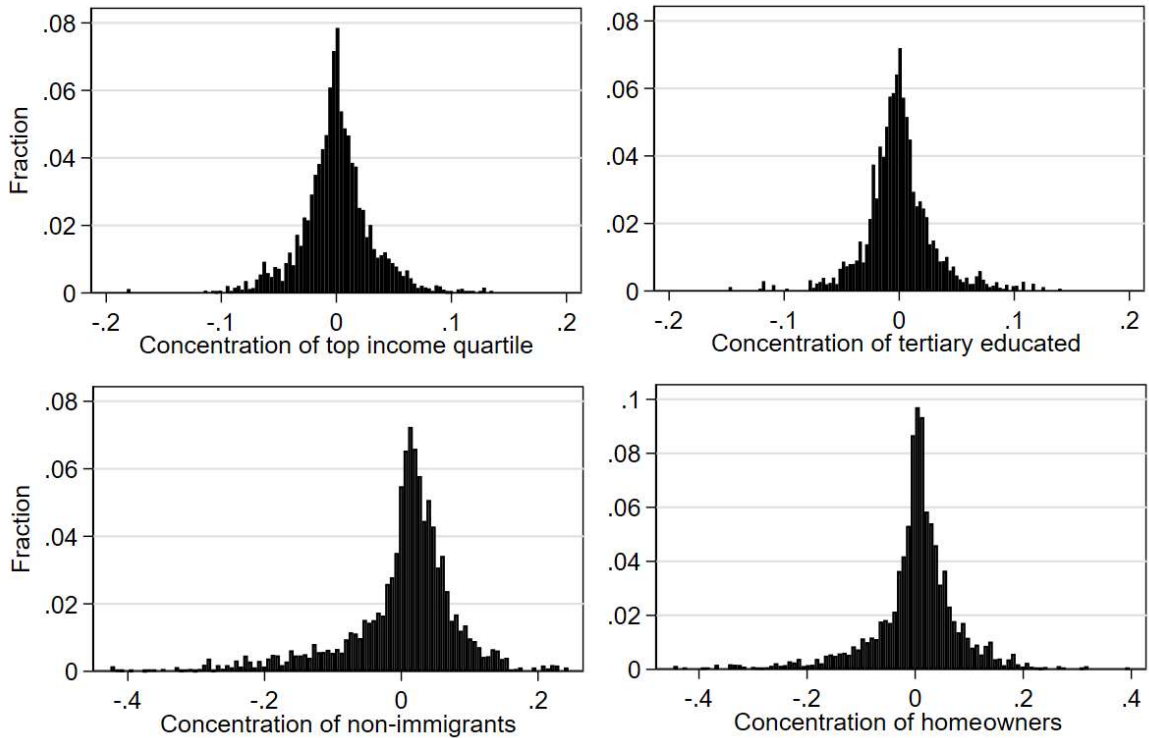
*Note:* The map shows the location of politicians from the left and center-right blocs following the 2018 election. Borders show precincts, and the shaded colors denote the left (red)–right (blue) balance of votes in the 2018 election. Vote shares are calculated based on the total number of votes going to either political bloc, and thereby exclude votes for local parties and the Sweden Democrats. Home addresses were found via Google searches, as the data used in the analysis only shows the home precinct.

Figure A3: Size of neighborhoods (left) and neighborhoods per municipality (right)



*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010. We dropped the smallest municipality, which had only one precinct, thus our sample has a minimum of 2 precincts and a maximum of 500 (Stockholm).

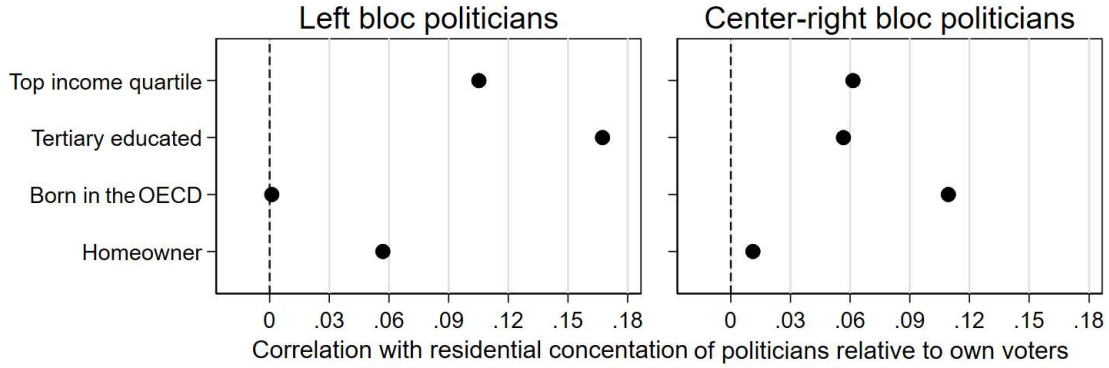
Figure A4: Distributions of concentration indices for socio-economic variables



*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

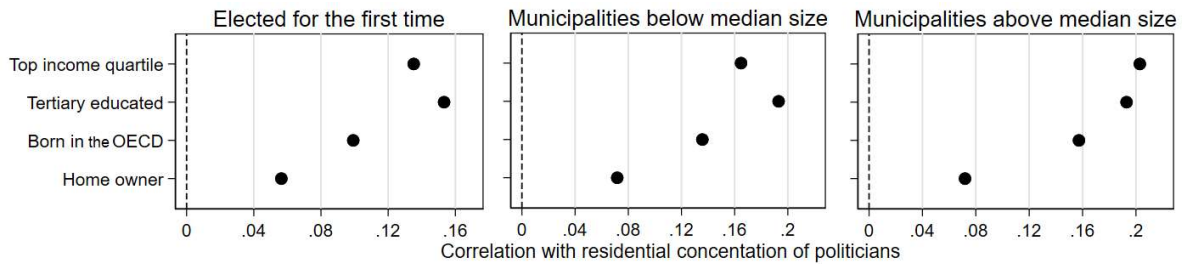


Figure A5: Residential pattern of politicians compared to voters for their own political bloc.



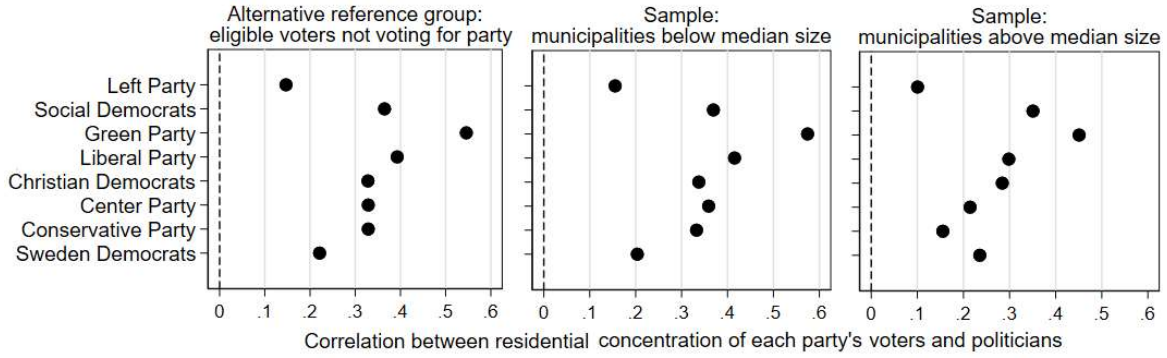
*Note:* The figure shows the correlation coefficient for the concentration of politicians across neighborhoods within municipalities relative to voters for their own political bloc of parties, and the concentration of citizen types across those same neighborhoods. Section 5 explains the calculation of the concentration measure. Observations are weighted by the inverse of the number of precincts in the municipality (N=17,427).

Figure A6: Sensitivity checks for first-time elected and sample split by median municipality size



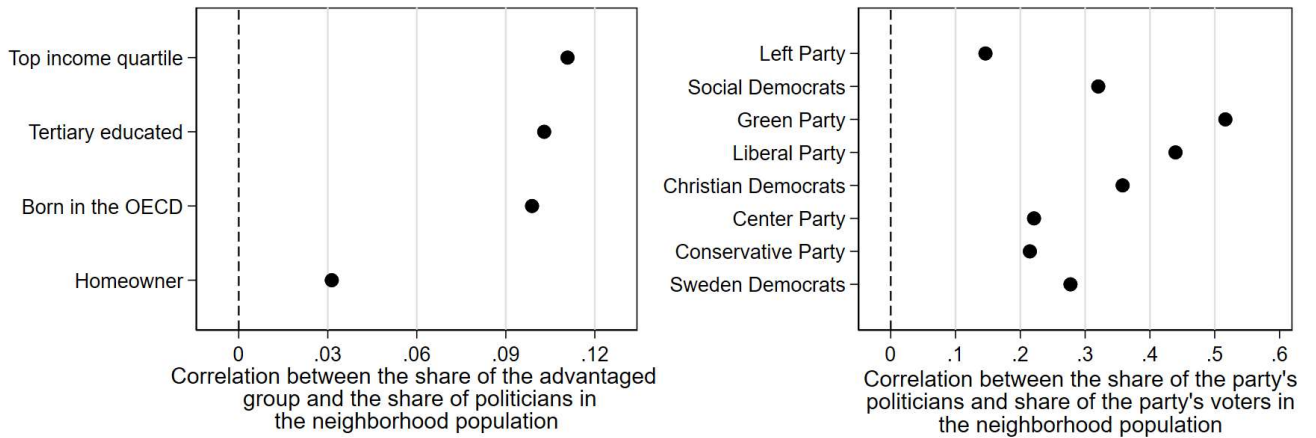
*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A7: Sensitivity checks by political party and sample split by median municipality size



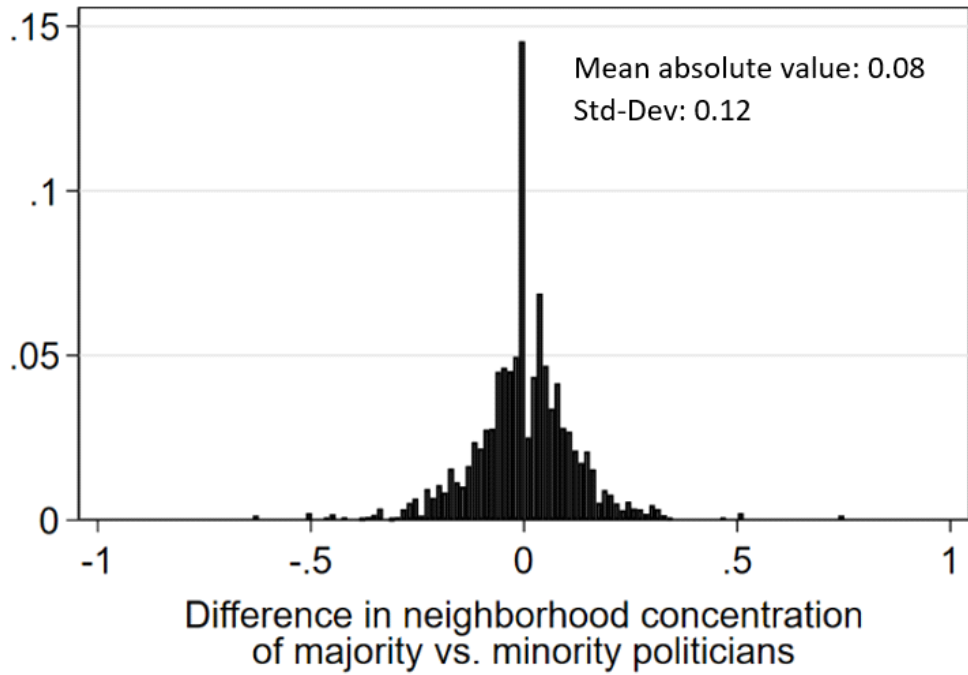
Note: Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A8: Sensitivity check for neighborhood population shares instead of concentration measures



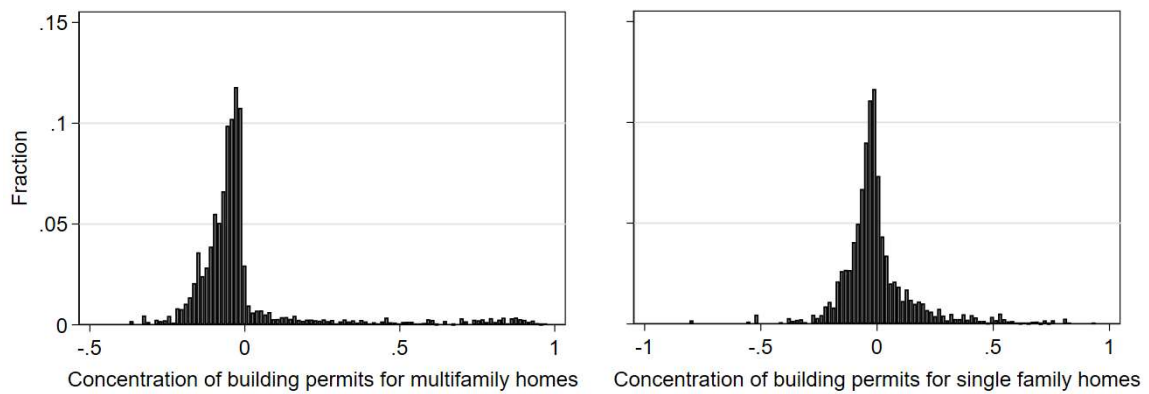
Note: Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A9: Distribution of the treatment variable



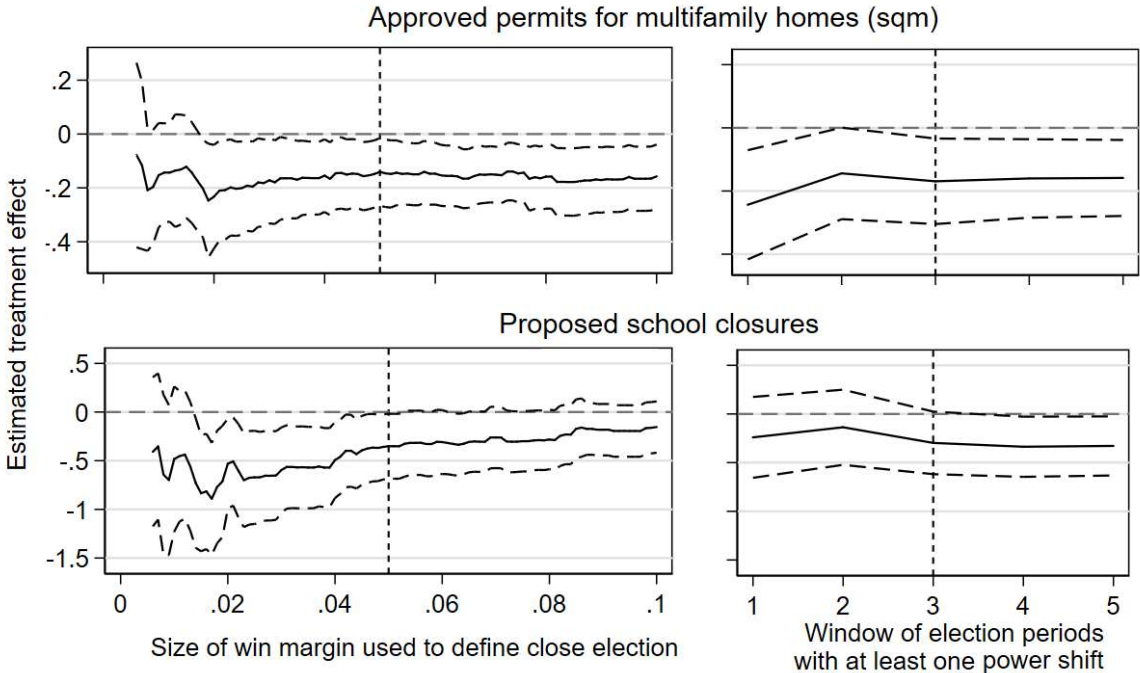
*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A10: Distribution of dependent variables for building permits



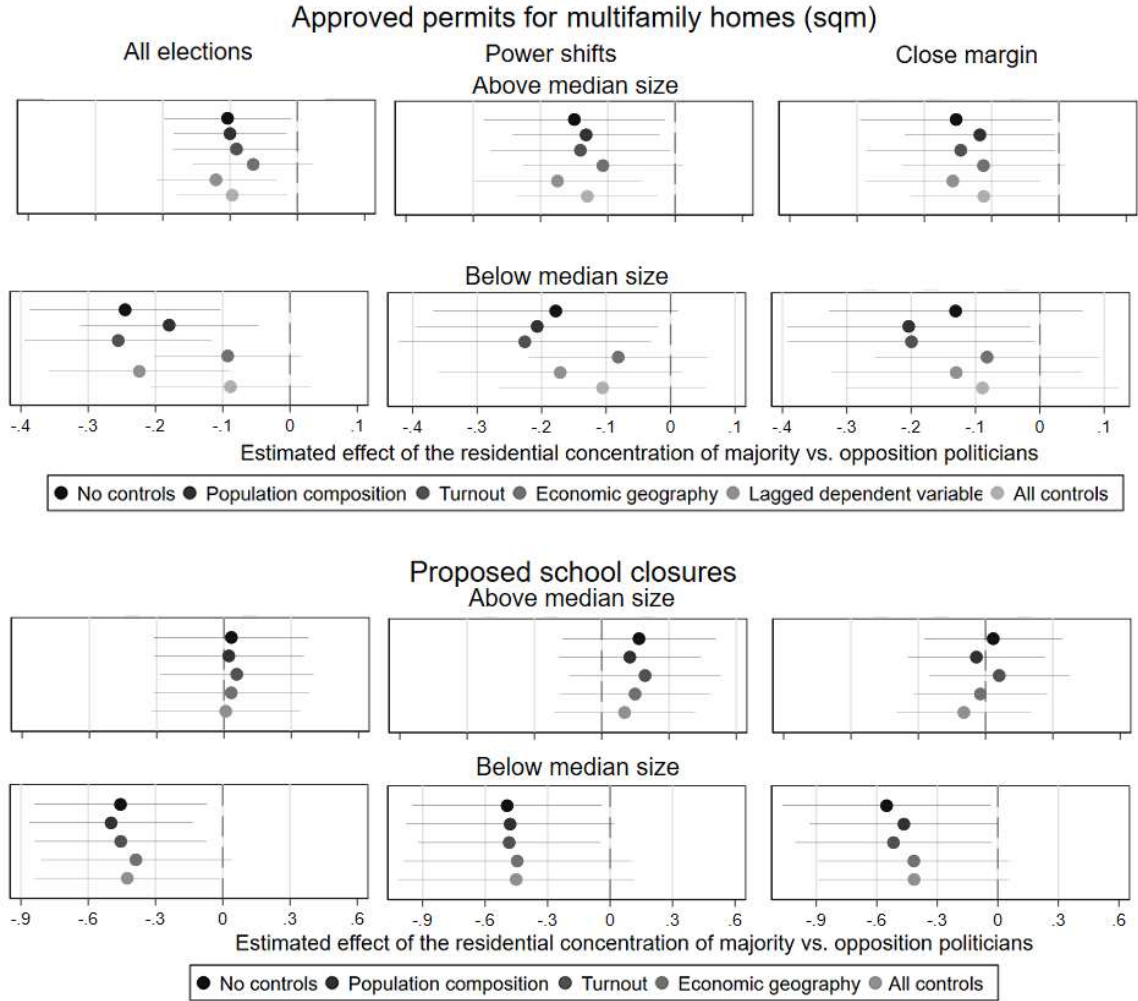
*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A11: Sensitivity check for alternative definitions of close elections



Note: Data is pooled cross-sections of three election years, 2002, 2006, and 2010.

Figure A12: Sensitivity check by municipality size – above or below median population



*Note:* Data is pooled cross-sections of three election years, 2002, 2006, and 2010.