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## Love Thy (Elected) Neighbor? Residential Segregation, Political Representation and Local Public Goods

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**ABSTRACT**

We study the link between geographic political representation and geographic distribution of public goods *within* local jurisdictions using geo-coded data on politicians, the electorate and elementary school closures. We find that poorer neighborhoods are under-represented. Inequality in geographic representation matters as the probability of school closure halves from about 20% to 10% when a candidate close to the school is randomly elected due to ties in personal vote counts. High-income residents react to school closures by moving away from their neighborhood. Taken together, residential sorting leads to inequality in representation and public goods across neighborhoods, which further reinforces residential segregation.

JEL Classification: C21, D72, H75, R23

Keywords: Geographic representation, random elections, residential segregation, school closure

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# 1 Introduction

There is a growing body of evidence showing the harmful effects of residential segregation. For example, a number of recent papers have documented that childhood neighborhood context has a causal effect on various adulthood outcomes (see e.g. Damm and Dustmann 2014, Chetty et al. 2016, Chyn 2018, Chyn and Haggag 2019, Nakamura et al. 2019, Deutscher 2020 and Laliberté 2020) and intergenerational mobility (Chetty and Hendren 2018 and Chetty et al. 2020). At the same time, the mechanisms through which these neighborhood or place effects manifest themselves have remained elusive. Are only peer effects at work or can neighborhood effects be explained also by differences in local public goods across neighborhoods? Moreover, if there is geographic inequality in public good provision, what is behind this unequal provision?

This paper concentrates on the political underpinnings of the provision of local public goods across neighborhoods. More precisely, we study the link between geographic political representation and geographic distribution of public goods *within* local jurisdictions using Finnish data spanning three municipal council terms from 2005 to 2017. The analysis is facilitated by detailed geo-coded data on the residential location of all municipal election candidates, the electorate and local public goods. The electoral context for our analysis is an at-large open-list proportional representation (PR) system.<sup>1</sup> As opposed to a ward system, when the elections are held at-large there is no guarantee that all neighborhoods are equally represented or that politicians have incentives to cater to local voters.

We start our analysis by describing the geographic representativeness of municipal councils by comparing the residential distribution of local politicians and the electorate across small neighborhoods. This analysis reveals that neighborhoods with poorer and less-educated electorates are under-represented relative to their share of municipal population, both at the extensive and in-

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<sup>1</sup>According to the Database of Political Institutions (Scartascini et al. 2018), 94 out of 147 democracies employ PR, and about fourth of these use open-list procedures.

tensive margins. We also document that candidates get systematically more votes from the polling districts they themselves live in compared to other polling districts and that the additional votes increase with the length of the candidates' residential spell in the district and with incumbency status.<sup>2</sup> This means that local politicians have a strong local support base, which incentives them to cater to voters in their neighborhoods even though the elections are held at-large.

In the second part of the analysis, we ask whether the unequal geographic representation that we document translates into unequal geographic distribution of local public goods. We focus our analysis on closures of elementary schools as they have a well-defined geographic location and are a prime example of a local service where proximity is an important factor as young children need to attend the school on a daily basis. Moreover, the number of elementary schools in Finland diminished by a third during the time period of our analysis providing us with spatial and temporal variation in local service availability.<sup>3</sup> To make sure we can interpret our results as causal, we use election outcomes decided by a lottery, which take place when there is a tie in personal votes within a party for the last seat of the party list. We find that randomly increasing the representation of a local school in the municipal council, which makes decisions on school closures, decreases the probability of school closure during the election term. The effect is large as in our lottery sample the probability of closing down a school during the election term roughly halves from about 20% to 10% when a candidate close to the school is randomly elected (as opposed to a candidate near another school).

Finally, we examine what happens to the neighborhoods when a local school is closed. Using difference-in-differences (DID) methods, we find evidence of neighborhood re-sorting. High-income residents of the neighborhoods

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<sup>2</sup>Finnish municipalities are divided into multiple polling districts. The polling districts are only used to allocate voters to polling stations and for vote counting purposes.

<sup>3</sup>Similar waves of school closures have taken place, for example, across the US (Engberg et al. 2012 and Brummet 2014), Denmark (Humlum and Smith 2015 and Beuchert et al. 2018), the Netherlands (De Haan et al. 2016) and Sweden (Taghizadeh 2020).

experiencing a closure of a relatively large school ”vote with their feet” by moving away from these neighborhoods, thus reinforcing residential segregation.

We contribute to several strands of literature both in economics and political science. First, we examine political representation and its effects from a new angle of geographic representation at a very local level.<sup>4</sup> This is important not only because of the possible effects on the distribution of public goods and funds more generally, but also because interacting with one’s neighbors may affect politicians’ beliefs and preferences. To the extent that politicians are exposed to systematically more affluent and educated neighbors than the electorate in general as we document, their perceptions about public opinion may be biased (e.g. Broockman and Skovron 2018). Descriptive evidence on the within-jurisdiction geographic representation is still very limited as most candidate level data sets analyzed in the literature lack the geographic detail of our data.

Related to this, we speak to the literature on the widely recognised phenomenon of ”friends-and-neighbors” voting.<sup>5</sup> Our results are consistent with the idea that politicians have a core support base in their neighborhood and that lasting neighborhood ties are important for cultivating personal votes. We document this phenomenon in elections held at-large where voters can cast votes to individual candidates to express their preferences for locals due to open-lists (see also Carey and Shugart 1995, Shugart et al. 2005 and Saarimaa and Tukiainen 2016).

Second, our results contribute to the literature on distributive politics. Several studies show convincingly that representation in a legislative body matters for the geographic distribution of centralized spending (e.g., Ansolabehere et al. 2002, Knight 2008, Berry et al. 2010, Dragu and Rodden 2011, Brollo and Nannicini 2012, Albouy 2013, Hodler and Raschky 2014

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<sup>4</sup>A large body of empirical work has established a causal link between legislative representation of various groups and policies preferred by those groups. See, for example, Pande (2003) and Beach et al. (2019) on minorities, Chattopadhyay and Duflo (2004) on gender or Hyytinen et al. (2018a) on occupation.

<sup>5</sup>E.g., Campbell et al. (2019) provide a list of papers documenting this in various countries and electoral contexts.



and Fiva and Halse 2016). However, as these studies focus on national or regional level spending, we still have an incomplete understanding of how distributive politics operate within local jurisdictions. This is a major gap in our knowledge as many important tasks have been delegated to local governments worldwide.<sup>6</sup> For example in Finland, municipalities are responsible for elementary schooling, primary health care, land use and zoning policies, public transportation and other such policies that influence the daily lives of the citizens. To our knowledge, we are the first to identify a causal link running from geographic representation to policy outcomes within local jurisdictions.<sup>7</sup> Taken as a whole, our findings suggest that the models of distributive politics are relevant also at the very local level.

Furthermore, prior research on geographic representation and spending mostly concerns ward-based systems where politicians have obvious electoral incentives to cater to their local ward. It is less clear that these incentives exist in at-large PR systems (e.g., Carey and Shugart 1995, Persson and Tabellini 2004, Trebbi et al. 2008, Trounstein 2010, Abbott and Magazinnik 2020 and Fiva et al. 2020). Our results indicate that the electoral incentives to cater to a local core support that arise in election systems with personal votes, such as the Finnish open-list PR system, are enough to connect geographic representation and public good provision.

Finally, our results speak to the literature on residential sorting and neighborhood effects. Recently this literature has focused on how a neighborhood's private amenities respond endogenously to its socio-economic makeup and how this reinforces residential sorting (e.g., Diamond 2016, Couture and Handbury 2020, Couture et al. 2020 and Almagro and Dominguez-Iino 2020). We high-

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<sup>6</sup>Cox (2009) highlights this mismatch between theoretical (e.g., Dixit and Londregan 1996) and empirical work on distributive politics. Specifically, district level studies are not informative about how resources are distributed across different groups of voters.

<sup>7</sup>Perhaps the closest studies to ours are Beach et al. (2019) and Carozzi and Repetto (2019) who analyze local level close elections in the US and Spain, respectively. However, neither of these studies have information on the residential location of the politicians themselves, and thus, are unable to link geographic representation and policy outcomes as we do.

light how residential sorting together with the local political system leads to inequality in representation and in publicly provided amenities across neighborhoods and how this may also reinforce residential sorting. Moreover, the link between residential segregation and the provision of public goods across neighborhoods may be an important mechanism behind neighborhood effects, as suggested recently by Fogli and Guerrieri (2019) and Laliberté (2020).

The paper proceeds as follows. In the next section, we introduce the Finnish local election system, the school system and municipal finances. In Section 3, we describe the data. In Section 4, we analyze the geographic representation, the effects of representation on school closures, and the effects of school closures on residential segregation. Finally, we conclude and discuss policy implications and interesting avenues for future research.

## 2 Institutional background

### 2.1 Municipal elections

Finland has a two-tier system of government consisting of the central government and municipalities. Municipal councils are the main seat of power in the municipal decision-making and the councils make the decisions on school closures. Municipal elections are held simultaneously in all municipalities and the council term lasts for four years. Our data span three council terms: 2005-2008 (elections held in October 2004), 2009-2012 (elections held in October 2008) and 2013-2017 (elections held in October 2012).<sup>8</sup> During our analysis period, the council size was a step function of population and varied between 13 and 85, the median being 27. At the same time, median municipality size was 7262 and ranged from 773 to 603,968. Each municipality has only one electoral district and no geographic quotas are in place.

The seat allocation is based on proportional representation (PR), using the open-list D'Hondt election rule. The elections are held at-large so that voters

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<sup>8</sup>From 2017 onward the start of council term was moved forward from January to June. The 2017 elections were held in April 2017.

can vote for any candidate in the municipality. In the elections, each voter casts a single vote to a single individual candidate and they cannot vote for a party without specifying a candidate. Moreover, the list order in the ballot is alphabetical so parties cannot use list order to signal their preferences. These rules mean that voters (as opposed to parties) decide which candidates are eventually elected from a given list, because the number of votes that a candidate gets determines the candidate's rank on her party's list. The total number of votes over the candidates of a given party list determines the votes for each party and this determines how many seats each party gets.

An important feature of this election system is that in many cases, there is an exact tie in the number of votes at the margin where the last available seat (or seats) for a given party list is allocated. For example, it is possible that a party gets  $k$  seats in the council and that the  $k^{th}$  and  $(k + 1)^{th}$  ranked candidates of the party receive exactly the same number of votes. The Finnish law dictates that in this case, the winner of the marginal seat has to be decided randomly. Typically, the seat is allocated by drawing a ticket (name) from a hat. We make use of these randomly decided election outcomes in our empirical analysis.<sup>9</sup>

## 2.2 Elementary school system

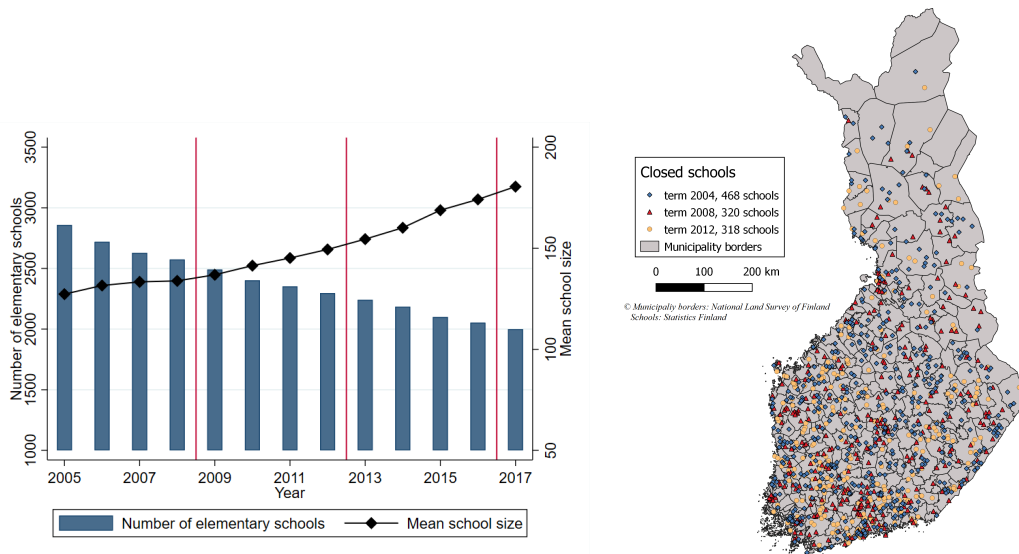
Municipalities are responsible for elementary education, which consists of a nine-year compulsory school starting in the year the child turns seven. Most elementary schools are public schools and they are free of charge. In most municipalities, school intake is catchment area based so that each address in a municipality is assigned to a catchment area of at least one school and children living within the catchment area of a particular school are guaranteed a place in that school. With this institutional setup, most children attend the school closest to where they live.

The number of elementary schools has declined and their mean size has increased substantially in Finland during the time period of our analysis as can

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<sup>9</sup>See Hyytinen et al. 2018b for more details on these randomly decided election outcomes.

be seen from the left panel of Figure 1. The closures are related to declining enrollment, mostly due to migration from rural to urban areas. The migration has been driven especially by younger households so that the number of school-aged children has declined in many municipalities and remote neighborhoods. At the same time, school closures have taken place all over Finland as is evident from the right panel of Figure 1. In more urban areas, school closures have taken place in search for economies of scale.



**Figure 1:** Number of elementary schools and geography of school closures.

Notes: On the left panel the blue bars depict the number of elementary schools during each year (left axis), the black line the mean size of schools (right axis) and the red vertical lines the election timing. The right panel depicts a map of school closures by election term.

## 2.3 Municipal finances

Finnish municipalities are multi-task jurisdictions. In addition to elementary schools, they are responsible for providing health and social care and other usual local public goods, such as public transport and waste management.

The most important revenue source is the flat municipal income tax which the municipalities can set freely. There is also a property tax, but importantly property tax revenue is not ear-marked for financing schools or in other ways to benefit the neighborhoods from which the taxes are collected, as is the case

with US school districts. A central government grant system, consisting of 20% of total revenue, is used to equalize local cost and revenue disparities. For more details on municipal tasks and finances, see e.g. Hyytinen et al. (2018a).

## 3 Data

### 3.1 Data sources

Our data come from several sources. Each data set comes with coordinate information so that we are able to combine them using GIS tools.

**Candidates.** The first source is the election data provided by the Ministry of Justice containing candidate-level information on the candidates' age, gender, party affiliation, the number of votes they received, their election outcomes, and incumbency status. We have linked these data to Statistics Finland's data on the candidates' education, occupation, and socioeconomic status. Finally, we obtained coordinates for the candidates' places of residence from the Digital and Population Data Services Agency.<sup>10</sup> Thus, we have candidate level election data with a rich set of background characteristics and their exact residential location for elections held from 2004 to 2012.

Most municipalities are divided into multiple polling districts and our election data also contain information on the number of votes received by the candidate from each of the polling districts in the municipality. Each polling district has a specific polling station where its residents go to vote. Importantly, polling districts have no other role in the elections as they are held at-large. Together with information in which polling district candidates live in, we can examine whether candidates receive more votes from their home polling district compared to other districts.<sup>11</sup>

**Candidates' neighborhoods.** Our data on the characteristics of the

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<sup>10</sup>Out of the 115,540 candidates in these three elections, 668 did not have coordinate data.

<sup>11</sup>We can match candidates residence to a polling district because each building has a polling district code, which matches the code in the election results data.

electorate in candidates' neighborhoods come from Statistics Finland's Grid and Zipcode Databases. The data contain information on age, education and income of the residents in 250 m x 250 m sized grids and zipcodes covering the whole of Finland. In the analysis, we will aggregate the grids to square kilometer level. Unfortunately, we are unable to match the polling districts to these neighborhood measures as we do not have geo-coded data, such as shapefiles, for the polling district.<sup>12</sup>

**Elementary schools.** Our school data also comes from Statistics Finland and span from 2004 to 2017. The data include annual information on the number pupils in different grades, the year the school was closed and coordinates for the school building.

### 3.2 Assigning candidates to schools

Since we have coordinate information for all schools and candidates, we can assign the closest school for each candidate at the time of the election. With this distance based approach, we assume that the candidate "represents" the school that is closest to them.<sup>13</sup>

Figure 2 illustrates our strategy for one election in one municipality with three schools. The schools are marked with a circle, a cross and a star (large symbols). The red circle means that this school was closed during the election term. The smaller symbols refer to the candidates' places of residence in the municipality. In the figure, a particular school is closest to the candidate when the candidate's symbol matches the school's symbol. Hollow symbols mean that the candidate was not elected while the filled symbols indicate elected candidates.

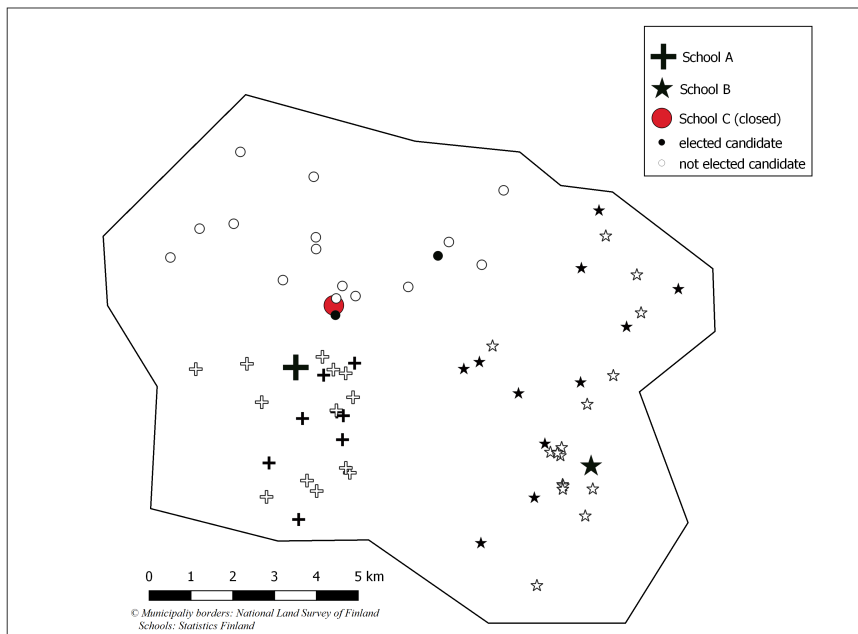
In this municipal election, there were in total 64 candidates of which 21

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<sup>12</sup>Polling districts are typically somewhat larger geographically than zipcodes. For example, in 2012, there were in total 2264 polling district and 3036 zipcodes. Figure A1 illustrates this for one municipality we were able to obtain polling districts in GIS format.

<sup>13</sup>An alternative approach would be to use the schools' catchment areas. However, we do not have data on the catchment areas for all municipalities for the time period of our analysis.

were elected. The seat shares of the schools are 10%, 33% and 57%, respectively, and the school with the lowest seat share is the one that was closed.



**Figure 2:** Assigning candidates to schools.

Notes: The figure illustrates how we assign candidates to the closest school to them in one municipality with three schools.

## 4 Results

### 4.1 Geographic representation and local support base

We start by examining geographic representation within municipalities. We first ask, whether the extent of representation depends on the socio-economic makeup of the neighborhood. In this analysis, a neighborhood refers to a square kilometer sized grid or a zipcode. The grids are much smaller units than the zipcodes with mean populations of 66 and 1830, respectively. After that, we analyze whether candidates receive more votes from their own neighborhoods compared to other neighborhoods. In this analysis, a neighborhood refers to a polling district, which is the smallest geographic unit we have in

the election data. In most municipalities, polling districts are geographically similar in size to zipcodes. In this analysis we use data from the 2012 elections.

To answer the first question, we run simple OLS regression models where we regress measures of neighborhood level representation on the income and education levels of the neighborhood’s residents while controlling for the neighborhood’s population share:

$$Representation_{im} = \beta * Popshare_{im} + \delta * Sosecon_{im} + \gamma_m + u_{im}, \quad (1)$$

where  $Rerepresentation_{im}$  refers to either the prevalence of candidates or councilors in neighborhood  $i$  (grid or zipcode) in municipality  $m$ . We use four measures: the candidate and councilor shares in the neighborhood, whether the neighborhood has any councilors and the election rate from the neighborhood, i.e. the share of the candidates running from the neighborhood that get elected. We control for the neighborhood’s share of municipal population ( $Popshare_{im}$ ) and municipality fixed effects ( $\gamma_m$ ).  $Sosecon_{im}$  is either the income or education level of the neighborhood’s residents.

The results are presented in Table 1 where we report 16 separate regression results in total.<sup>14</sup> Panel A reports the results for the grids and Panel B reports the results for the zipcodes. We highlight three findings. First, there seems to be no robust pattern with respect to neighborhood candidate share (models (1),(5), (9) and (13)). Second, the neighborhoods with higher average incomes and more educated electorate have more representatives both in the extensive and intensive margins. This is true at both neighborhood scales. Finally, also candidates’ electoral success is better in these higher income and better educated neighborhoods. The associations are also quantitatively substantial. For example, one standard deviation increase in average income is associated with 5.2%-point increase (from 40%) in the propensity to have at least one councilor.

The key takeaway from Table 1 is that neighborhoods with more affluent and educated electorates are more strongly represented in municipal councils.

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<sup>14</sup>Mean income and education levels of a neighborhood are highly correlated, which is why we do not include them in the same regressions.



It also seems that the geographic differences in candidacy are not the main explanation for these differences in representation, but rather how candidates perform in the elections.

**Table 1:** Political representation and socio-economic structure of neighborhoods.

Outcome:	Candidate share	Councilors (0/1)	Councilor share	Councilors/ candidates
Panel A: Square km grid				
	(1)	(2)	(3)	(4)
Mean income	0.0001 (0.0001)	0.052*** (0.016)	0.001*** (0.0003)	0.018*** (0.007)
R-squared	0.816	0.264	0.556	0.190
	(5)	(6)	(7)	(8)
Share highly educated	0.010** (0.003)	1.054*** (0.103)	0.036*** (0.005)	0.417*** (0.098)
R-squared	0.816	0.269	0.558	0.192
Outcome mean	0.021	0.401	0.019	0.238
N	5733	5733	5733	4172
Panel B: Zipcode				
	(9)	(10)	(11)	(12)
Mean income	-0.002** (0.001)	0.048*** (0.015)	0.003** (0.001)	0.012* (0.006)
R-squared	0.973	0.186	0.936	0.257
	(13)	(14)	(15)	(16)
Share highly educated	0.008 (0.013)	1.670*** (0.342)	0.062** (0.019)	0.068 (0.162)
R-squared	0.972	0.168	0.936	0.296
Outcome mean	0.108	0.791	0.108	0.307
N	2653	2653	2653	2653
Municipality FE	yes	yes	yes	yes

Notes: The table presents results from regressions where the unit of observation is either a square kilometer grid or a zipcode. Grids with less than ten persons over 18 are omitted due to data confidentiality. All the models include the neighborhood's share of municipal population as a control. Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

These results can be driven by a number of different mechanisms that are not mutually exclusive. First, it is well documented that there are differences in turnout between different socio-economic groups (e.g. Lindgren et al. 2019, Lahtinen et al. 2019, Akee et al. 2020 and Cantoni and Pons 2020). Due to residential segregation, neighborhoods with more affluent and educated neighborhoods have higher turnout. If all voters vote for local candidates at roughly similar rates, the results we observe could be simply due to neighborhood differences in turnout. Second, at a given level of turnout, voters in more affluent and educated neighborhoods may be more likely to vote for a local candidate. This could be because they have higher valuation for local public goods or that they have a better understanding of the link between voting locally and local public goods. Third, voters may simply vote for high-quality candidates regardless of geography. If education and income level are cues for candidate quality, then neighborhoods with higher incomes and education get more votes simply because candidates from these neighborhoods are perceived to be of high quality by all voters. Finally, it could be that candidates campaign differently in different types of neighborhoods or that different quality candidates campaign in different ways.

While our data does not allow us to disentangle these mechanisms completely, we can shed more light on them using election results data at candidate-polling district level. These data allow us to compute for all candidates in the 2012 municipal elections the share of votes that the candidate received out of all the votes given in a particular polling district within a municipality. Using this vote share, we estimate the following regression model:

$$Votesh_{ip} = \beta_0 Own_p + \beta_1 Own_p * Residence_i + \beta_2 Own_p * Incumbent_i + \gamma_i + u_{ip}. \quad (2)$$

That is, we regress candidate  $i$ 's vote share out of all the votes given in polling district  $p$  ( $Votesh_{ip}$ ) on a dummy variable indicating the polling district where the candidate herself lives ( $Own$ ). In addition, we interact this dummy with the residence spell in the polling district ( $Residence$ ) and with incumbency status ( $Incumbent$ ). These can be seen as information cues for

voters. We rely on within candidate variation across the polling districts by including candidate fixed effects ( $\gamma_i$ ). This regression also sheds light on the electoral incentives that candidates have to cater to voters in their neighborhoods, as opposed to all voters in the municipality.

The results, presented in Table 2, indicate that candidates' vote share is roughly 1.5%-points higher in their own district compared to other districts in the municipality. Given that the mean vote share from all districts is on average 0.308%, the own district vote premium is indeed substantial. As the regressions include candidate fixed effects, we are perfectly controlling for the quality of the candidates as well as their campaigning ability and effort. This closes down the role of candidate characteristics and behavior in attracting votes overall, and thus, the above mechanism that arises from high-quality candidates getting more votes in general and sorting into particular neighborhoods. In column (2), we add the two interaction terms into the model. The results indicate that the own polling district vote premium is higher both for candidates who have resided in the district longer and for the incumbents. The findings are in line with the notion of "friends-and-neighbors" voting where politicians have a core support base in their neighborhood and that lasting neighborhood ties are important for cultivating personal votes (e.g., Shugart et al. 2005 and Jankowski 2016). One explanation for this could be that voters believe that candidates with longer residence spells are likely to stay in the neighborhood and continue to promote the interests of the residents as they are aligned with the candidates interests. On the other hand, to the extent that incumbency status is a measure of overall candidate quality, higher quality candidates seem to be able to make use of the local support base more efficiently.

Finally, in columns (3) and (4) we show that these results are not an artifact of the number of polling districts in a municipality. In these columns we have divided the data into two samples based on the median number of polling districts in all municipalities. The results in both samples are qualitatively similar, although there are some quantitative differences.

Taken as a whole the results in this section suggest that the poor and low-

educated neighborhoods are left behind in geographic representation. Moreover, given that voting behavior reflects strong preferences for local candidates, it is likely that demographic differences in voting behavior, such as variation in turnout, explain at least substantial part of these inequalities. Also, the results indicate that candidates have a large local vote premium, and thus, strong electoral incentives to cater to their local support base.

**Table 2:** Candidates' vote shares from polling district.

	All municipalities (1)	All municipalities (2)	#districts $\leq 4$ (3)	#districts $> 4$ (4)
Constant	0.237*** (0.004)	0.236*** (0.003)	0.730*** (0.043)	0.223*** (0.003)
Own district	1.464*** (0.087)	0.512*** (0.043)	0.936*** (0.107)	0.440*** (0.043)
Own district $\times$ residence spell		0.046*** (0.003)	0.023*** (0.004)	0.051*** (0.004)
Own district $\times$ incumbent		2.171*** (0.137)	1.646*** (0.151)	2.296*** (0.168)
Outcome mean	0.304	0.304	1.276	0.277
N	713,134	713,134	18,849	694,285
R-squared	0.39	0.43	0.56	0.40
Candidate FE	yes	yes	yes	yes

Notes: The table presents results from candidate-polling district level regressions where the dependent variable is the candidate's vote share (%) of the polling district. Only municipalities with more than one polling district are included. Columns (1) and (2) include all candidates from these municipalities. In columns (3) and (4), the data is divided into two samples based on the median number of polling district in all municipalities (4). Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 4.2 Effects of representation on school closure

We have shown that political representation is unequal in geographic terms and that candidates have a strong support base in their neighborhood. In this section, we examine whether this matters for policy outcomes, i.e. whether there is a causal channel running from geographic political representation to geographic policy outcomes. To achieve this, we ask whether electing a given candidate has an effect on the probability that the elementary school closest to that candidate is closed during the council term.

Our main model specification can be written as:

$$Closure_{it} = \alpha + \delta * Elected_{it} + \beta' X_{it} + u_{it}, \quad (3)$$

where  $Closure_{it}$  is a dummy variable that equals one if the school closest to candidate  $i$  was closed during council term  $t$ . The dummy variable  $Elected_{it}$  is equal to one if candidate  $i$  was elected into the municipal council for election term  $t$ .  $X_{it}$  include school and candidate level control variables and council term fixed effects, and  $u_{it}$  is the error term.

Our interest lies on the parameter  $\delta$ , which measures the effect of increasing the representation of the school in question by one councilor on the probability that the school is closed down. A simple OLS estimation of equation (3) would likely suffer from omitted variable bias as many other factors besides political representation affect school closures. Many of these factors are unobservable to us and likely correlated with the election status as the same economic, demographic and social factors influence both the demand for schools in neighborhoods and candidate selection and voting decisions.

To estimate  $\delta$  consistently we resort to randomly assigned election outcomes by including in our analysis sample only the candidates whose election status was decided by a lottery. This makes sure that  $Elected$  was randomly assigned. Moreover, we omit lotteries where all the involved candidates have the same closest school as these lotteries do not provide useful identifying variation (results are robust to including them).<sup>15</sup>

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<sup>15</sup>We include all municipalities with at least two schools in the estimations. In the

In order to increase power and to examine the robustness the results, we in a sense increase the bandwidth around the last seats by including also the candidates whose election status was decided by a margin of a single vote. This sample is constructed in the following way. For each party list, we define the pivotal number of votes as the average of the maximum number of votes among the non-elected candidates and the minimum number of votes among the elected candidates. The distance to getting elected for each candidate is the number of votes of the candidate minus the pivotal number of votes of her party list. We include in this alternative sample those candidates whose distance was less or equal to one. As long as these candidates were not able to precisely manipulate the number of votes they got and the within-party election threshold, the election status for these candidates is as-good-as-random.<sup>16</sup> This increases our sample size to 1540. Also for this larger sample, we use the same eligibility criteria that the candidates involved in the close elections and ties are assigned to different schools.

Balance tests reported in Table 3 verify that both the candidate and school level covariates are balanced across the control and treatment groups in both the lottery sample and the sample that combines the lottery and the one vote margin candidates. Importantly, also the distance to the closest school is balanced across the groups suggesting that the groups are comparable in geographic terms. The last two variables in Table 3 show that the treatment group has more representation both at the intensive and extensive margins than the control group. These also reveal that the average school seat share is roughly 20% and that almost all schools have at least some representation. This means that even though the candidates in these samples get the very last seats in their party, they typically increase the already sizable seat share of the schools, which may be quite important in terms of within council coalition elections between 2004 and 2012, 419 candidates had an eligible tie (different schools) within their party list for the last seat(s).

<sup>16</sup>Hyytinen et al. (2018b) verify this in an analysis where they compare the incumbency advantage effect obtained using the lotteries and a regression discontinuity design using these within party list close elections.



formation and bargaining related to school closures (see Baron and Ferejohn 1989).

**Table 3:** Covariate balance at candidate level.

Variable	Lottery (N = 419)		One vote margin (N = 1540)	
	Control mean	Difference	Control mean	Difference
Age	46.79	-0.109 (1.329)	47.98	-0.240 (0.674)
Male (0/1)	0.569	0.032 (0.046)	0.605	0.024 (0.022)
Children (0/1)	0.313	0.067 (0.048)	0.332	0.026 (0.025)
Number of children	0.725	0.111 (0.132)	0.725	0.041 (0.064)
Incumbent (0/1)	0.332	0.014 (0.042)	0.339	0.042* (0.023)
Student (0/1)	0.038	-0.009 (0.016)	0.026	-0.002 (0.008)
Unemployed (0/1)	0.043	-0.009 (0.018)	0.023	0.016* (0.009)
Entrepreneur (0/1)	0.156	0.065* (0.038)	0.192	0.025 (0.020)
High professional (0/1)	0.204	0.017 (0.045)	0.192	0.015 (0.020)
Distance to school (km)	3.334	-0.573 (0.525)	2.829	-0.110 (0.205)
Number of pupils in school	129.4	10.85 (10.49)	127.9	1.498 (6.928)
School seat share	0.202	0.021 (0.016)	0.172	0.033*** (0.010)
School has representation (0/1)	0.948	0.052*** (0.017)	0.950	0.050*** (0.009)

Notes: Each row in the table refers to a separate bivariate regression where the dependent variable is reported on the first column and the explanatory variable is election status (*Elected*). The control mean refers to the constant in these models and the difference refers to the coefficient on the explanatory variable *Elected*. Standard errors are presented in the parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4 reports results from OLS regressions using the lottery sample (Panel A) and the one vote margin sample (Panel B). According to the results using the lottery sample, there is a clear effect of representation on school closure. Column (1) presents the most parsimonious model for the lottery sample, from which we see that, on average, 20% of the schools of the lottery losers are closed down, whereas for the winners school closures happen only half as frequently. Moving to the right of Panel A, we see that adding school and candidate level controls and council term fixed effects has virtually no effect on the point estimates, which is consistent with the balance tests reported in Table 3.

In Panel B, we present the results including the candidates where the last seat(s) was decided with at most a one vote margin. In this case, the effect of representation is slightly smaller. However, the differences in the point estimates in Panel A and B are not statistically significantly different from each other at conventional significance levels. Moreover, the baseline closure rate for the close elections losers is lower in this sample (column (5)) making the relative effect more comparable to the lottery sample. Again the point estimates remain the same when adding controls and council term fixed effects.<sup>17</sup>

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<sup>17</sup>Roughly 20% of the candidates moved during the council term. We re-estimated the models using only those candidates who did not move during the council term (stayers). The results from this robustness check are very similar to those presented in Table 4. These additional results are reported in Table A1 in the Appendix. Furthermore, we estimated separate models for municipalities with populations below and above the country median. The effect is larger in the sample of small municipalities. One explanation for this finding could be that council size is also smaller in these municipalities, and thus, the increase in a school's seat share from one additional candidate is larger. These results are presented in Table A2 in the Appendix.

**Table 4:** Effect of representation on school closure

<i>Panel A: Lottery</i>	(1)	(2)	(3)	(4)
Constant	0.204*** (0.030)	0.282*** (0.038)	0.329*** (0.100)	0.305*** (0.099)
Elected	-0.108*** (0.035)	-0.101*** (0.035)	-0.099*** (0.035)	-0.097*** (0.035)
N	419	419	419	419
R-squared	0.023	0.064	0.096	0.108
<i>Panel B: One vote margin</i>	(5)	(6)	(7)	(8)
Constant	0.186*** (0.016)	0.260*** (0.022)	0.207*** (0.053)	0.213*** (0.054)
Elected	-0.062*** (0.020)	-0.061*** (0.020)	-0.065*** (0.020)	-0.064*** (0.020)
N	1540	1540	1540	1540
R-squared	0.007	0.045	0.063	0.064
P-value for effect difference	0.122	0.172	0.247	0.258
School controls	No	Yes	Yes	Yes
Candidate controls	No	No	Yes	Yes
Election term FE	No	No	No	Yes

Notes: The table presents results from linear probability models where the outcome is school closure (0/1). The school controls include the number of pupils in school. The candidate controls include age, sex, children in the family or not, incumbency and occupation status. The  $p$ -value is for the test testing the statistical significance of the difference of the coefficients for Elected in the two samples with the same model specification. Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

The results so far provide clear revealed preference evidence that local politicians want to prevent school closures in their neighborhoods. To understand more deeply why, we examine whether the effect is heterogeneous with respect to candidates' characteristics and incentives towards the local electorate. In particular, we are interested in whether the effects are larger when the school closure is likely to affect the politicians' own families. This will inform us whether their actions are motivated by personal gains or whether they are driven by electoral gains.

First, we examine heterogeneity in the effect based on whether the candidates have children (12 years old or younger) who currently attend or are likely to attend the local school in the near future. Roughly 35% of the lottery sample candidates have young children. The second measure is the size of the school, which at least indirectly measures the number of voters who are affected by the closure of the school. The bigger the closed school, the more parents there are that may express their disappointment at the ballot box. School size may also be correlated with school quality as larger schools are able provide a larger menu of subjects.

Table A3 in the Appendix presents the results for these interactions both for the lottery and the one vote margin samples. Interestingly, all of the interaction terms are statistically insignificant. Therefore, we do not find evidence suggesting that personal non-electoral gains motivate the councilors' decisions regarding the school closures.

We also examine heterogeneity with respect to how inconvenient or costly in terms of school commute the school closure is for the candidates and the local electorate. For the candidates, we measure this cost as the difference between the candidate's own distance to the currently closest school (measured before potential closure) and to the second closest school, which is the one the candidate's children would most likely transfer to in case of school closure. For the electorate, the distance measure is simply the distance between the school buildings of the school in question and the closest school to it. Of course, these two measures are highly correlated.

We hypothesize that the travel costs are nonlinear because, for example,

after a certain threshold the child is unable to go to school alone. We divide the sample based on the median (p50) and 75th percentile (p75). From Table A4 in the Appendix we see that all of coefficients for the interaction terms with the distance measures are negative, but in all but one case statistically insignificant. The evidence is therefore only suggestive that geographic representation matters more when the policy is more important.

Taken together, these results suggest that local politicians want to prevent school closures in their neighborhood to cater to local voters rather than to obtain personal non-electoral gains.

### 4.3 School closures and residential re-sorting

In this section, we examine residential sorting near closed schools using a staggered DID (event study) design for all school closures that took place in 2013, 2014 and 2015. We concentrate on these closures because our neighborhood data is available from 2010 to 2018 and we want to have enough pre- and post-treatment observations for testing pre-treatment trends and to have enough time for residential re-sorting to take place.

In this analysis, we use zipcode level panel data using only those zipcodes that have or had just one school during the analysis period. We make this choice because our neighborhood measures are at zipcode level and it is unclear what would happen in zipcodes with more than one school. When there is only one school, the treatment is more clear cut. Furthermore, we use nearest-neighbour matching based on school size and zipcode level population of the last pre-treatment year.<sup>18</sup> This increases the comparability of the treatment and the control groups both in terms of school size and number of people affected by the closures.

Using the matched data set, we estimate the following event study specifications:

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<sup>18</sup>We match two control units for each treated unit. The results are robust to using one or three control units (not reported).

$$y_{kt} = \sum_{\tau=-4}^3 \delta_{\tau} D_{\tau,kt} + \gamma_t + \alpha_k + u_{kt}. \quad (4)$$

where  $y_{st}$  is the outcome of interest for zipcode  $k$  at year  $t$ . The dummy variable,  $D_{\tau,st}$ , indicates the year relative to year of closure of school  $s$ . The negative values of  $\tau$  indicate the pre-closure years and the positive values indicate the post-closure periods. At  $\tau = 0$ , the school no longer exists. The specification includes zipcode and year fixed effects and we cluster the standard errors at the zipcode level.

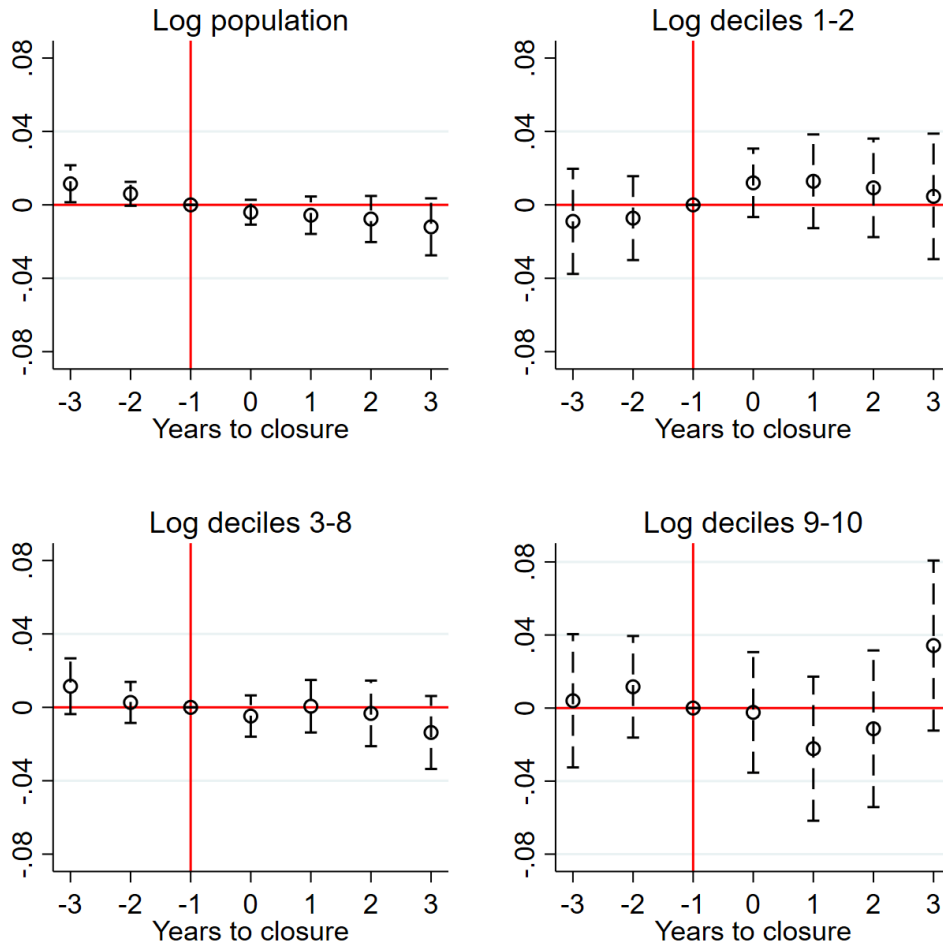
The outcome we are interested in is the prevalence of different income groups in the zipcodes. In particular, we want to understand whether the high-income families move away from the neighbourhood as local public services deteriorate. We make use of the Zipcode Database by Statistics Finland where the adult population is divided into three groups based on income deciles constructed annually at the national level: low-income (deciles 1 and 2), middle-income (deciles 3 through 8) and high-income (deciles 9 and 10).

We also examine the heterogeneity of sorting with respect to school size. School size captures two important aspects. First, the larger the school, the more people in the neighborhood are affected by the closure (due to our matching procedure, both in absolute and relative terms), and second, school size is correlated with school quality. As we are analyzing schools that are quite small, it could be, for example, that different age cohorts have to share a classroom and the teacher. Larger schools are also able to provide a larger variety of subjects. We use a cutoff of 90 pupils in the school to divide the sample into small and large schools. With 90 pupils, we would expect the class size at each grade to be 15 pupils. This cutoff is somewhat *ad hoc* and we analyse robustness with respect to this choice.

The event study plots (point estimates and 95% confidence intervals for  $\delta_{\tau}$ 's) based on equation (4) are presented in Figure 3 for the small schools (under 90 pupils) and in Figure 4 for the large schools (at least 90 pupils). Starting from Figure 3, there is a slight downward trend in the total population in the zipcodes with a school closure both pre- and post-treatment. This is

not surprising given that diminishing enrollment is one of the main reasons for school closures. However, individual point estimates are not statistically significant after the closure. This slight downward trend is visible with the middle-class residents (deciles 3 through 8). There seems to be no systematic pattern in the development of low- (deciles 1 and 2) and high-income groups (deciles 9 and 10), although these estimates are quite noisy. It seems that the small drop in total population is driven by the middle-income group (deciles 3 through 8). In sum, there seems to be no clear pattern of residential re-sorting after school closures when the schools are small.





**Figure 3:** Effect of school closures on residential sorting, small schools.

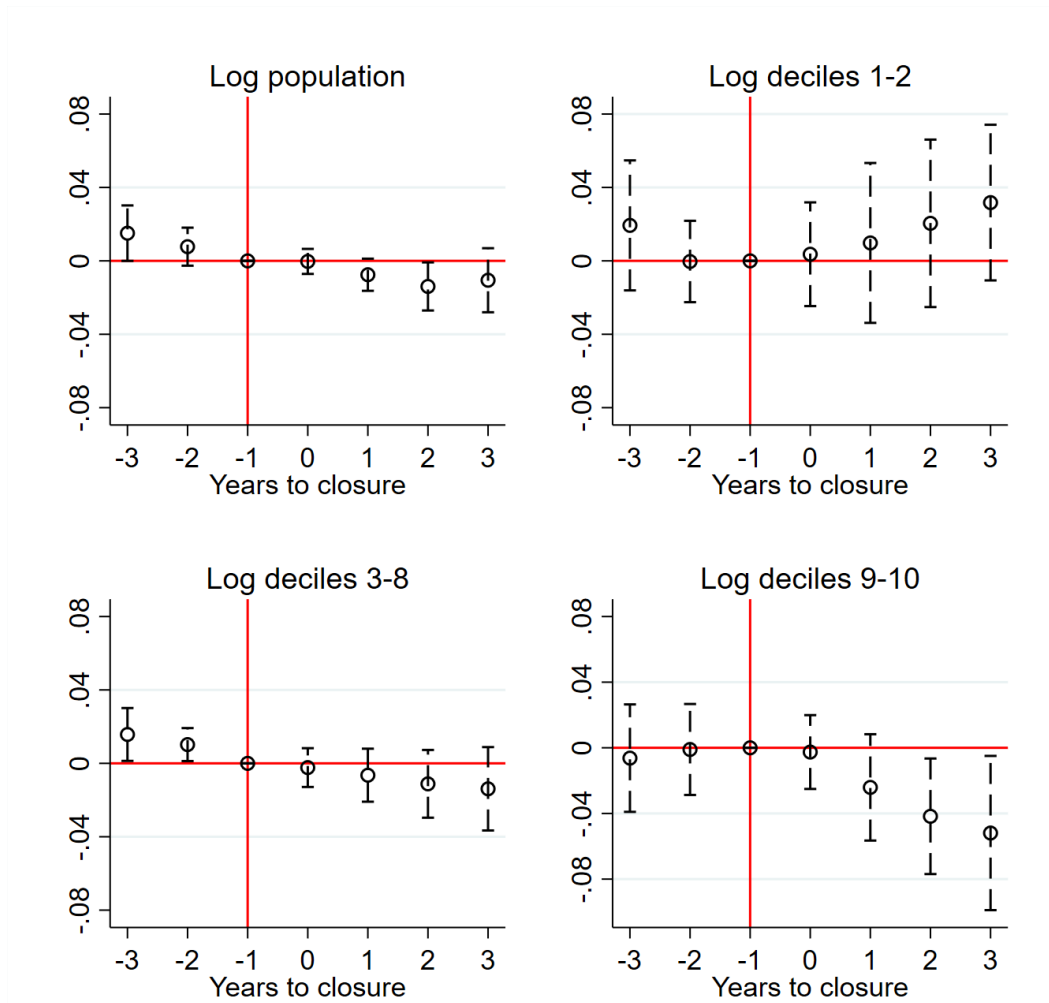
Notes: The Figures plot the coefficient estimates and 95% confidence intervals from event study regressions using zipcode level panel data. Only zipcodes with one school are included in the analysis ( $N = 2338$ ). The outcomes are the logs of total population and the number of neighborhood residents in the national level income deciles (1 and 2, 3 through 8 and 9 and 10). The schools in the sample had under 90 pupils prior to closure. Event time is the year relative to the year of school closure. School closures included in the analysis took place in 2013, 2014 and 2015. The omitted period is -1. The regressions include year and zipcode fixed effects. Standard errors used for confidence intervals are clustered at the zipcode level.

Results are different for the sample of larger schools. According to Figure 4, there again seems to be a downward trend in total population in pre- and

post-treatment periods that is driven by the middle-class (deciles 3 through 8). The low-income group (deciles 1 and 2) remains quite stable, but there is a decline in the number of high-income residents after closure and this decline is also statistically significant. This group also exhibits a clean common pre-trend three years prior to closure. The decline in high-income residents appears to be accompanied by a slight increase in the number of low-income residents, although the point estimates are not statistically significant for this group. Thus, when the closed school is relatively large and possibly of higher quality, high-income residents vote with their feet meaning that school closures reinforce residential segregation.<sup>19</sup>

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<sup>19</sup>In Figure A2, we report event study plots for different large school cutoffs. The results are very similar to those in Figure 4.



**Figure 4:** Effect of school closures on residential sorting, large schools.

Notes: The Figures plot the coefficient estimates and 95% confidence intervals from event study regressions using zipcode level panel data. Only zipcodes with one school are included in the analysis ( $N = 667$ ). The outcomes are the logs of total population and the number of neighborhood residents in the national level income deciles (1 and 2, 3 through 8 and 9 and 10). The schools in the sample had at least 90 pupils prior to closure date. Event time is the year relative to the year of school closure. School closures included in the analysis took place in 2013, 2014 and 2015. The omitted period is -1. The regressions include year and zipcode fixed effects. Standard errors used for confidence intervals are clustered at the zipcode level.

## 5 Conclusions

We have studied the link between geographic political representation and geographic distribution of local public goods within local jurisdictions using detailed geo-coded micro data spanning three municipal election terms. We have produced four novel findings. First, neighborhoods with poorer and less-educated electorates are under-represented relative to their share of municipal population. Second, candidates have a strong local support base, and thus, strong electoral incentives to cater to voters in their neighborhood even though the elections are held at-large. Third, based on randomly decided election outcomes, geographic representation has a causal effect on the geographic distribution of local public goods. Finally, deterioration of local public goods, in our case through school closures, seems to lead to residential re-sorting with respect to income of the residents. We conclude with some thoughts on the external validity and policy implications of our findings and highlight important avenues for future research.

It is important to note some limitations of our findings in terms of external validity. Although open-list PR systems are prevalent worldwide, the details in the systems and the tasks assigned to local jurisdictions may influence the link between geographic representation and local public good provision in nuanced ways. Moreover, our causal results come from a subset of candidates who were involved in close races for the last party seat(s). Concentrating on these marginal candidates facilitates causal inference, but provides us with a local effect that may not capture more general effects across a wider distribution of politicians. On the one hand, the candidates who occupy, in a sense, the last seat of the party may have less power to influence the council decisions. On the other hand, these marginal candidates are electorally vulnerable, and thus, may have stronger electoral incentives to cater to local voters compared to electorally safer candidates.

The important policy question that our results raise is how can we make political representation geographically more balanced. We consider three factors: voter mobilization, election system reforms and housing policies that

aim to affect residential sorting.

First, if under-representation of poorer and less-educated neighborhoods is mainly due to lower turnout by the socio-economic groups in these neighborhoods (e.g., Lindgren et al. 2019, Akee et al. 2020, Hall and Yoder 2020 and Yoder 2020), policies designed to mobilize voters may be important. However, evidence on the effectiveness of interventions mobilising nonvoters (so-called get-out-the-vote) is not encouraging in this respect. For example, Enos et al. (2014) summarize the findings from 27 experimental mobilization interventions. They find that on average, these mobilization strategies actually widen disparities in participation and representation by mobilizing well-represented citizens more than the under-represented. On the other hand, increase in turnout and representation of disadvantaged groups due to major reforms typically result in large shifts in policies (e.g., Husted and Kenny 1997, Cascio and Washington 2014 and Fujiwara 2015). Whether mobilization interventions in countries that already have a broad franchise can be designed more effectively to alleviate the geographic differences in participation should remain high in the research agenda.

Second, when it comes to comparing election systems in terms of balanced geographic representation, perhaps the most interesting comparisons are between at-large and ward systems, open- and closed-lists and district magnitudes in at-large systems. At a first glance, a ward system seems to offer clear benefits as it guarantees that all neighborhoods get at least some representation. Recent evidence also indicates that moving from at-large to a ward system indeed improves minority representation (e.g. Abott and Magazinnik 2020). However, the comparison between at-large and ward systems involves subtle trade-offs. For example, Mast (2020) shows that the ward system worsens the so-called not-in-my-neighborhood problems and suppresses local housing supply. Moreover, the effects of moving to a ward system on geographic representation crucially depends on districting as the large literature on optimal districting and gerrymandering indicates (e.g., Coate and Knight 2007, Trebbi et al. 2008 and Gul and Pesendorfer 2010). Moving to wards in a PR system may also have drastic effects on the number of parties due to

Duverger’s law (Duverger 1959). In addition, according to Beath et al. (2016), at-large systems tend to select higher quality candidates compared to ward systems. Analyzing these important trade-offs should also prove a fruitful avenue for future research.

Conducting elections at-large and moving to closed-lists would transfer power from the voters to the parties because in addition to compiling the party lists, parties’ would also rank the candidates and ultimately decide which candidates are elected. It is unclear what would happen to the geographic composition of party lists and candidate rankings if, for example, Finland would move to closed-lists (e.g., Latner and McGann 2005, André and Depauw 2018 and Fiva et al. 2020). Increasing district magnitude or council size, on the other hand, would increase the proportionality of the election system (e.g., Taagepera and Shugart 1989 and Cox 1997). This would probably lead to fewer neighbourhoods having no representation, at a given level of turnout. Comparative evidence on geographic representation from closed- and open-list systems and elections with different district magnitudes would help to resolve these issues.

As the underlying cause of unequal geographic representation is residential segregation, it is important to also consider policies directly aimed at decreasing it. These programs can be either place-based social mixing policies where the main tool is the location of public housing across neighborhoods, or tenant-based programs where housing vouchers would somehow be conditioned on neighborhood characteristics (e.g., Collinson and Ganong 2018 and Davis et al. 2020). At the same time, the details of the program matter because the results of social mixing policies from different countries are quite mixed and the programs tend to be expensive (e.g., Eerola and Saarimaa 2018, Verdugo and Toma 2018, Diamond and McQuade 2019 and Bergman et al. 2019). Future research should focus on gaining a better understanding of the relative merits of place- and tenant-based housing programs in creating socially mixed neighborhoods.

Finally, our results show that the effects of school closures are more far-reaching than policy-makers may have thought. Their indirect effects on

neighbourhood segregation and geographic balance of political representation constitute an important consideration when deciding on school closures or the geography of local public good provision more generally.

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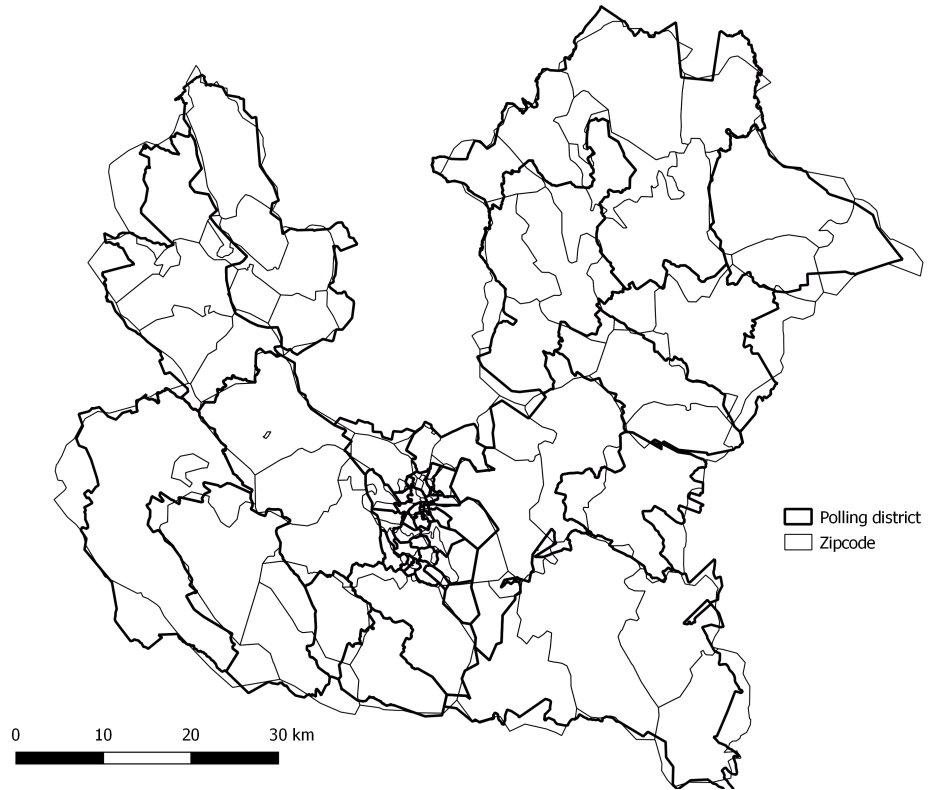
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## Appendix: Additional figures and tables



**Figure A1:** Polling districts and zipcodes in the municipality of Kuopio in 2019.

**Table A1:** Effects of representation on school closure (close elections, stayers).

<i>Panel A: Lottery</i>	(1)	(2)	(3)	(4)
Constant	0.200*** (0.031)	0.286*** (0.041)	0.348*** (0.108)	0.326*** (0.109)
Elected	-0.091** (0.037)	-0.085** (0.037)	-0.080** (0.037)	-0.080** (0.038)
N	379	379	379	379
R-squared	0.016	0.063	0.115	0.122
<i>Panel B: One vote margin</i>	(5)	(6)	(7)	(8)
Constant	0.188*** (0.018)	0.263*** (0.023)	0.199*** (0.060)	0.207*** (0.061)
Elected	-0.060*** (0.021)	-0.060*** (0.021)	-0.062*** (0.021)	-0.062*** (0.021)
N	1397	1397	1397	1397
R-squared	0.007	0.044	0.066	0.068
School controls	No	Yes	Yes	Yes
Candidate controls	No	No	Yes	Yes
Election term FE	No	No	No	Yes

Notes: The table presents results from linear probability models where the outcome is school closure (0/1). The school controls include the number of pupils in school. The candidate controls include age, sex, children in the family or not, incumbency and occupation status. Standard errors are presented in parentheses and are clustered at the municipality level.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



**Table A2:** Effect heterogeneity with respect to municipality size.

	Lottery		One vote margin	
	Small	Large	Small	Large
	municipalities	municipalities	municipalities	municipalities
	(1)	(2)	(3)	(4)
Constant	0.508*** (0.161)	0.022 (0.085)	0.329*** (0.087)	0.082 (0.064)
Elected	-0.159*** (0.058)	-0.042 (0.043)	-0.093*** (0.032)	-0.043* (0.023)
N	211	208	771	769
R-squared	0.159	0.172	0.076	0.044

The table presents results from linear probability models where the outcome is school closure (0/1). The models include election term fixed effects and the following controls. The school controls include the number of pupils in school. The candidate controls include age, sex, children in the family or not, incumbency and occupation status. Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A3:** Effect heterogeneity with respect to candidate background and school size.

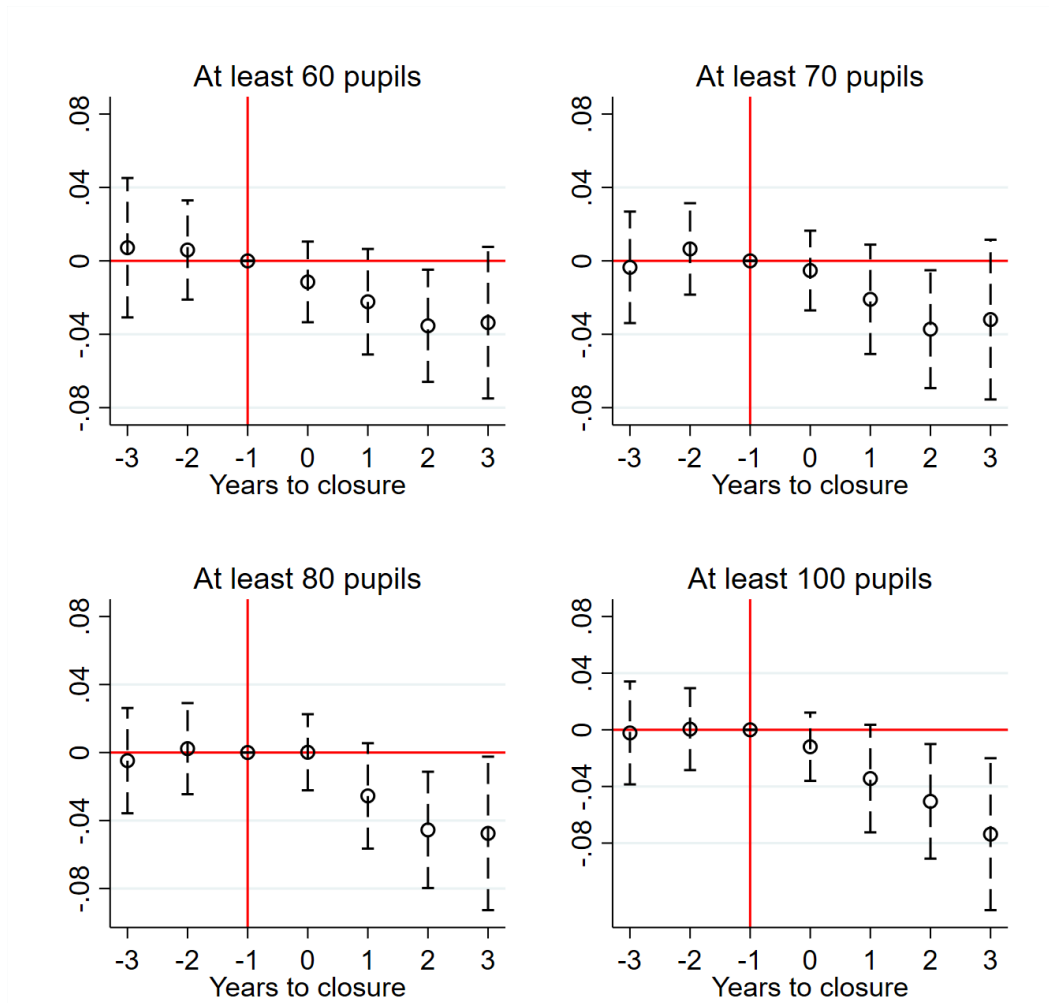
	Lottery		One vote margin	
	Candidate has children	School size	Candidate has children	School size
	(1)	(2)	(3)	(4)
Constant	0.305*** (0.099)	0.314*** (0.104)	0.203*** (0.054)	0.224*** (0.057)
Elected	-0.098** (0.042)	-0.115* (0.058)	-0.046** (0.023)	-0.085** (0.033)
Elected $\times$ heterogeneity	0.002 (0.073)	0.0001 (0.0002)	-0.055 (0.039)	0.0002 (0.0002)
N	419	419	1540	1540
R-squared	0.108	0.109	0.066	0.065

The table presents results from linear probability models where the outcome is school closure (0/1). The models include election term fixed effects and the following controls. The school controls include the number of pupils in school. The candidate controls include age, sex, children in the family or not, incumbency and occupation status. Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A4:** Effect heterogeneity with respect to distance measures.

	Candidate distance (p50)	Candidate distance (p75)	Voter distance (p50)	Voter distance (p75)
<i>Lottery</i>	(1)	(2)	(3)	(4)
Constant	0.276*** (0.097)	0.297*** (0.096)	0.246** (0.098)	0.242*** (0.090)
Elected	-0.078* (0.046)	-0.072* (0.042)	-0.061 (0.045)	-0.037 (0.039)
Elected × heterogeneity	-0.042 (0.069)	-0.104 (0.085)	-0.069 (0.069)	-0.226*** (0.083)
N	419	419	419	419
R-squared	0.110	0.113	0.116	0.134
<i>One vote margin</i>	(5)	(6)	(7)	(8)
Constant	0.212*** (0.058)	0.211*** (0.056)	0.192*** (0.056)	0.205*** (0.054)
Elected	-0.066** (0.027)	-0.060*** (0.022)	-0.047* (0.025)	-0.044** (0.022)
Elected × heterogeneity	0.004 (0.040)	-0.016 (0.044)	-0.036 (0.040)	-0.081 (0.050)
N	1540	1540	1540	1540
R-squared	0.064	0.064	0.066	0.067

The table presents results from linear probability models where the outcome is school closure (0/1). The models include election term fixed effects and the following controls. The school controls include the number of pupils in school. The candidate controls include age, sex, children in the family or not, incumbency and occupation status. Standard errors are presented in parentheses and are clustered at the municipality level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



**Figure A2:** Sensitivity of effect of school closures on residential sorting with respect to school size cutoff.

Notes: The Figures plot the coefficient estimates and 95% confidence intervals from event study regressions using zipcode level panel data. Only zipcodes with one school are included in the analysis. The sample sizes are 862 for top-left, 776 for top-right, 701 for bottom-left and 515 for bottom-right, respectively. The outcomes are the logs of total population and the number of neighborhood residents in the national level income deciles (1 and 2, 3 through 8 and 9 and 10). The schools in the sample had at least 60, 70, 80 or 100 pupils prior to closure date. Event time is the year relative to the year of school closure. School closures included in the analysis took place in 2013, 2014 and 2015. The omitted period is -1. The regressions include year and zipcode fixed effects. Standard errors used for confidence intervals are clustered at the zipcode level.

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