

Ethnic Enclaves and Elite Political Participation:

- Evidence from a Swedish Refugee Placement Program

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Abstract

In this paper, we leverage unique population registry data from Sweden to study whether immigrants' residence in ethnic enclaves enhances or hinders their propensity to run for political office. To do so, we utilize a refugee placement program that was in place in Sweden during the late 1980s and early 1990s, and which restricted refugees' opportunities to freely choose their place of residence. We present evidence that immigrants residing in ethnic enclaves stand for political office less often than others. This finding challenges the notion that residence in enclaves fosters immigrants' political incorporation by spurring the formation of social networks and group consciousness and facilitating political parties' mobilization efforts. Instead, our results suggest that residence in enclaves decreases immigrants' political incorporation. The findings have important implications for refugee resettlement policy as well as local immigrant integration efforts.

Introduction

Ethnic neighborhoods, or *enclaves*, feature heavily in debates about the civic and social integration of immigrants. Such enclaves are alternatively demonized and praised. On the one hand, critics argue that they foster parallel societies, isolating migrants from the social and civic lives of their host countries (Massey and Denton 1989; Duncan and Lieberman 1959; Gordon 1964). Others, however, contend that ethnic enclaves may spur the creation of dense social networks and foster group consciousness – ultimately strengthening immigrants’ civic and political engagement in their host countries (Laurence and Heath 2008; Bhatti and Hansen 2016; Portes and Bach 1985; Portes and Rumbaut 1990).

To date, studies relating immigrants’ residence in ethnic enclaves to their host-country political integration have yielded inconsistent results. Furthermore, extant studies focus largely on the relationship between residence in enclaves and voter turnout. Almost no existing work explores how neighborhood context influences individuals’ broader political interests, ambitions, and, in particular, their propensity to seek political office. Insofar as descriptive representation may inspire political participation among co-ethnics, further research on elite immigrant participation would constitute a critical step towards understanding how neighborhood context shapes patterns of immigrant mobilization (Gutmann and Thompson 2004; Karpowitz et al. 2012; Mansbridge 1999; Tate 1994; Bobo and Gilliam 1990; Lublin and Tate 1995).

In this paper, we leverage unique data from Sweden to study whether immigrants’ residence in ethnic enclaves enhances or hinders their propensity to run for political office. From 1985 to 1994, the Swedish government implemented a placement program known as “Whole of Sweden Program.” Through this program, the Swedish government sought to catalyze refugee settlement outside of the major cities, thereby spreading the cost of resettlement initiatives across municipal governments. Arriving refugees were assigned to

one of 277 Swedish municipalities with minimal consideration of their individual characteristics. We argue that the Swedish placement program gave rise to credible exogenous variation in immigrants' residence contexts that can be used to study the relationship between ethnic concentration and elite political participation.

The present study thus constitutes an important methodological contribution to the existing literature. To date, most works relating local context and immigrants' political participation has relied on data on immigrant population "stock." Such data does not account for the fact that immigrant minorities often self-select into ethnic neighborhoods (Borjas 1995; Cutler et al. 2008). Since immigrants' initial choice of locations may be influenced by factors correlated with political integration, such as language skills or the presence of ethnic and religious organizations, it is difficult to gauge the degree to which neighborhood context influences immigrants' political participation. Indeed, politicized immigrants may self-select into politicized areas. However, by exploiting the fact that refugee immigrants to Sweden did not get to choose where to locate during the years of the placement program we substantially reduce this sorting problem. In comparison with previous research in the field we are therefore in a much better position to identify the causal effect of interest.

Further, to date, efforts to analyze the emergence of minority candidates have been hampered by a lack of data on the ethnic origin of unsuccessful candidates for office (Lawless 2012; Lawless and Fox 2010). As a result, many studies focus on elected minority officials rather than minority candidates, complicating efforts to analyze when and why minority candidates emerge in the first place. Here, we make use of registry-based population micro-data encompassing all candidates and non-candidates for the six local elections between 1991 and 2010. This unique resource permits analysis of minority candidate emergence with a relatively high degree of statistical precision.

To preview our main findings, we find evidence that immigrants' likelihood of nomination for political office substantially decreases as the share of co-ethnics in their municipality of resettlement increases – suggesting that residence in ethnic enclaves decreases immigrants' likelihood to pursue leadership roles in local politics. The results thus suggest that residence in ethnic neighborhoods may hinder immigrants' political incorporation over the long run.

The rest of this article proceeds as follows. We first briefly review the relevant literature, and then provide the necessary background about Sweden's immigration history and the placement program. Thereafter we discuss data and methods and report the results from the empirical analyses. We conclude with a discussion of the main findings and their implications.

Literature Review

The existing literature on minority candidate emergence, including immigrant-origin minorities, is in its infancy. We know little about the factors that influence minorities' decisions to run for office and the role local context plays in such decisions. To date, the literature on minority candidate emergence has focused largely on African-Americans and Latinos in the U.S. A number of scholars have argued that the supply of minority candidates in the U.S. is highest in areas with relatively large co-ethnic populations (Branton 2009; Krebs 1999; Meier et al. 2005; Shah 2014). Branton (2009), for example, finds that higher Latino or African-American population shares in an electoral district correlate with greater numbers of minority candidates in primary elections. He further argues that minority candidates may expect their chance of winning to be higher in districts with more co-ethnics. As yet, however, it is unclear how such findings would translate to non-U.S. minorities or to proportional representation systems.

In the European context, extant work on immigrant representation emphasizes minority candidates' election prospects relative to natives rather than their decision to pursue political

office in the first place. Dancygier et al. (2015), for example, employ Swedish registry data to analyze immigrant candidates' likelihood of victory in local elections. They argue that immigrant candidates fare worse than natives as party gatekeepers tend to place them lower on party lists. They also find that immigrants' settlement in densely-populated areas with fewer available seats per person contributes to their underrepresentation. It remains unclear, however, how context influences immigrants' initial decisions to pursue political office.

In this paper, we contribute to the existing literature on candidate emergence by examining how residence in ethnic enclaves shapes immigrants' propensity to run for political office. Specifically, we test two competing theories derived from the literature on immigrant electoral participation.

On the one hand, classical assimilation theory posits that immigrants' residence in ethnic enclaves discourages their participation. By this logic, immigrants living in enclaves may have relatively limited access to information about politics or may turn their attention to the politics of their homeland rather than host-country politics (Cho et al. 2006). More generally, ethnic concentration may militate against political socialization, limiting immigrants' knowledge of how to participate and dampening their interest in host-country engagement (Huckfeldt 1986; Cho 1999; Cho et al. 2006). More specifically, immigrants living in ethnic enclaves may interact disproportionately with individuals from their home countries. They may also work in the "ethnic economy" or in industries employing many of their co-ethnics, impeding their access to information about host-country politics and policies (Piore 1979).

The residential and economic concentration of immigrants may also lower their incentives to learn host-country languages, further limiting their access to information about policies, parties, and candidates as well as information about how to participate in their host country's politics. As such, language and informational constraints may militate against migrants' political involvement. In addition, ethnic concentration may also depress immigrants' sense

of political efficacy. Recent studies from the U.S. suggest that Latino participation in politics is hindered by a sense that politics is the domain of individuals with stronger institutional knowledge and network ties (Reny and Shah forthcoming).

On the other hand, some maintain that ethnic concentration can increase the flow of information within immigrant groups, ultimately facilitating their mobilization (Laurence and Heath 2008; Maxwell 2013; Lipset et al 1954; Fieldhouse and Cutts 2008a). Schlichting et al. (1998) argue further that ethnic concentration facilitates the growth of minority political organizations. Ethnic organizations may facilitate political mobilization by enhancing immigrants' social trust (Fennema and Tillie 1999). Employing a more concrete example, Maxwell (2013) claims that ethnic concentration of South Asians in the U.K. facilitates intra-group mobilization, ultimately enhancing their descriptive representation.

Ethnic enclaves may also increase immigrants' earnings, lowering the financial and opportunity costs of participation. In the U.S. context, Portes and his collaborators have argued that new immigrants profit from working within ethnic economies, as otherwise their unfamiliarity with the English language and U.S. customs might relegate them to low-wage jobs with low upward mobility (Portes 1981; Portes and Bach 1985; Portes and Jensen 1989; Wilson and Portes 1980). Working in ethnic enclaves might thus provide some immigrants with higher wages and more flexible schedules than others, facilitating their political participation (Verba and Nie 1972).

Moreover, political parties may also be especially likely to mobilize immigrants residing in ethnic enclaves. In such areas, the payoffs of outreach to immigrant communities is higher and the costs of mobilization are lower than in regions where immigrant populations are dispersed (Ramakrishnan 2005). Others, however, have noted that parties do not necessarily mobilize immigrants in areas of high concentration, and that their tendency to do so may depend on electoral institutions (Leighley 2001; Bilodeau 2009).

Finally, some have claimed that ethnic concentration can increase immigrants' sense of efficacy, the feeling that they can influence political outcomes (Leighley 2001). Likewise, ethnic concentration may also spur participation by engendering group consciousness (Bilodeau 2009). As ethnic enclaves increase in size, Leighley (2001) finds, more immigrants participate as they come to perceive that engagement benefits them and their group.

To date, most empirical work on the relationship between immigrants' residence in enclaves and political participation has focused on voter turnout. These studies have yielded mixed results.

On the one hand, numerous scholars have found a positive correlation between minority concentration and voter turnout. In the U.S., Matthews and Prothro (1963) found that African-American turnout is higher in counties with relatively high African-American population shares. Likewise, Ramakrishnan and Espenshade (2001) found that ethnic concentration correlates positively with voter turnout among Asian- and Latino-Americans. Similar results have also been reported for countries outside the US. Fieldhouse and Cutts (2008a, 2008b), for instance, found a positive correlation between ethnic concentration and voter turnout in the U.K, and Bhatti and Hansen (2016) presented similar results for Denmark.

Conversely, however, a number of scholars have found a negative relationship between ethnic concentration and turnout. Cho (1999) and Cho et al. (2006) found that turnout among Asian-Americans is relatively low among individuals living in ethnic enclaves. Likewise, Levitt and Olson (1996), in a study of six immigrant communities in New York City, found evidence of a negative relationship between concentration and voter turnout.

In summary, existing empirical work on the effects of ethnic enclavization on immigrants' political participation points in different directions. One important reason for this may be the difficulty of accounting for the self-sorting of immigrants into ethnic enclaves. That is, the

immigrants that choose to settle in enclaves may be different from those settling outside these areas. Unfortunately, many of these differences may relate to factors such as personality type or innate ability, which are difficult to observe, measure and control for.

In this study we attempt to overcome this problem by studying a refugee placement program that was in place in Sweden during the late 1980s and early 1990s. As we will describe in more detail in the next section, an important feature of this program was that refugees did not get to choose their initial location in Sweden, which considerably lessens the problem of self-sorting when studying the effects of residing in an ethnic enclave. In comparison with previous research in the field we are therefore in a better position to identify the causal effect of interest.

However, the present study also extends previous research in another important regard. To date, the extant literature emphasizes the relationship between turnout and residence in enclaves. Perhaps due to data limitations, few scholars have empirically analyzed how immigrants' residence in enclaves influences their propensity to stand for political office. To remedy this, our study utilizes unique registry-based population data to examine how living among co-ethnics affects immigrants' probability of running for political office.

Immigration to Sweden and the Refugee Placement Policy

Sweden's immigrant population has increased substantially during the postwar decades. In 1960, Sweden's foreign-born population share stood at 4.0% and by 2015 it had reached 17.0%. In 1990, around the starting date of our study, 9.2% of the population was born abroad. This trajectory is very similar to that of other European countries. For instance, the overall mean foreign-born share across 15 Western European countries and the U.S. in 2013 was 16.0% (the On-line Statistical Database of OECD).

Sweden's history of migration also resembles the experiences of other European countries. In the 1950s and 1960s the inflow of immigrants to Sweden was to a large extent made up of labor migrants. These came primarily from Finland, but migrants from Central and Southern Europe and Turkey were recruited to the manufacturing sector as well (Lundh and Ohlsson 1999; Nilsson 2004).

In the aftermath of the oil crisis and increasing unemployment in the early 1970s, demand for foreign labor dwindled. Since then, the share of labor migrants has decreased and refugees and family reunification migrants have dominated immigrant inflows. This pattern accelerated in the late 1980s and the early 1990s following a massive increase in immigration. As a consequence, the source country composition of the immigrant population has changed dramatically. Among the catalysts for this development were the military coup in Chile in 1973, the 1979 Iranian revolution, the persecution of the democratic movement in Poland during the early 1980s, the Balkan wars in the 1990s, the wars in Iraq and, most recently, the civil wars in Somalia and Syria (Dancygier et al. 2015).

Another feature shared by most Western countries is the concentration of immigrants in certain regions and neighborhoods (Åslund et al. 2011). Large cities such as Stockholm, Gothenburg and Malmö host around half of the foreign-born population but only around a third of the total. Moreover, within these regions immigrants are overrepresented in particular areas, especially suburbs.

In response to mounting complaints from the cities most affected by the rise in immigration, the formal framework regulating the reception of refugee immigrants underwent a drastic change in the mid 1980s.¹ The reform was implemented in 1985 and the Swedish

¹ This section is mainly based on Edin et al. (2003), Åslund and Fredriksson (2009), Borevi and Myrberg (2010), Åslund et al. (2011), and Grönqvist et al. (2012).

Immigration Board was given the responsibility of assigning all newly-arrived asylum seekers to an initial municipality of residence.

The main goal of this reform was to speed up the integration process by distributing asylum seekers across a larger group of municipalities possessing above-average educational, labor market, and housing opportunities for them. At the start of the reform, the Board signed contracts with around 60 receiving municipalities. However, due to the large increase in the number of asylum seeker in the late 1980s, the Immigration Board decided that all Swedish municipalities should partake as receivers. In 1989, the system included 277 out of Sweden's 284 municipalities. The explicit goal was that the number of refugees placed in each municipality should amount to at least 2.9 per thousand inhabitants. At the same time, the factors that initially were meant to guide the assignment of refugees to municipalities – educational and labor market opportunities – were abandoned. Instead, available public housing became the main governing principle.

The placement policy was formally in effect between 1985 and 1994, but the implementation was strictest between 1987 and 1991. Therefore, our analysis will focus on refugees arriving during the 1987-1991 period. A number of previous works have used the placement reform to study the effects of ethnic enclaves on immigrant economic success (Edin et al. 2003), peer effects on immigrant student achievement and welfare dependence (Åslund and Fredriksson 2009; Åslund et al. 2011) and the relationship between income inequality and health (Grönqvist et al. 2012). In this study, we will leverage the placement reform to test if and how ethnic enclavization influences individuals' propensity to run for public office.

Our identification strategy rests on the assumption that the settlement policy provides an exogenous source of variation in contextual characteristics. More concretely, an individuals'

presence in an ethnic enclave must not be linked to unobserved factors that might influence his or her propensity to run for political office.

The studies mentioned above thoroughly substantiate these claims. The essence of the argument rests on three facts. First of all, the policy was comprehensive. During the five-year period between 1987 and 1991, the Immigration Board assigned a large majority – around 90% – of the refugees to different municipalities.

Second, immigrant preferences did not play any decisive role in the assignment process. Although refugees could apply for residence in a preferred city or area, it was up to local Immigration Board officers to decide on the placement of individual refugees. The explicit goal of the placement policy was to reduce the time between receiving a residence permit and placement in a municipality and the main obstacle in this process was lack of housing. The housing market was very tight in the late 1980s, especially in the larger cities. Thus, housing vacancies would have had to coincide with immigrants' receipt of residence permits for their residential preferences to be realized. The very low probability of these two events happening concurrently ensured that little, if any, weight was given to immigrant preferences in the assignment process.

Third, there was no interaction between local Immigration Board officers and individual refugees in the placement process. Any selection by local officers was driven solely by observable characteristics, particularly immigrants' language, formal education and family size. Against this backdrop, we argue that immigrants' municipal assignment was exogenous with respect to unobserved individual factors, conditional on these observed characteristics.

Data and Methods

We obtained all data used in the empirical analysis from various administrative registers held by Statistics Sweden (see the Appendix for a more detailed description of the data and variables). As previously mentioned, we focus on the refugees that immigrated between 1987

and 1991. Unfortunately, refugee immigrants cannot be identified directly from the data. Instead we will follow the approach of Åslund and Fredriksson (2009). As a general rule, Åslund and Fredriksson include immigrants from countries outside of Western Europe that were not members of the OECD as of 1985. The only exception from this rule is that they include Turkey, an important source of asylum seekers during the period of interest. Given that individuals who immigrated as family members were not affected by the placement policy, we exclude all individuals belonging to a household with an adult already living in the country. Like Åslund and Fredriksson (2009), we also restrict the analysis to individuals aged 18-55 at the time of immigration. Imposing these sample restrictions we are left with a sample of 49,871 immigrants that we follow in the six general elections held in 1991, 1994, 1998, 2002, 2006, and 2010.

We will use nomination to municipal parliament as our dependent variable. During the study period, the number of municipalities increased from 284 to 290 and all individuals above the age of 18 that have lived in Sweden for at least three years are eligible to stand as a candidate for the municipal parliament.

Municipal elections in Sweden operate by a party-list system, where local nomination committees largely control who gets nominated and how candidates are ranked on the list. Party gatekeepers are thus highly influential in deciding who is on the list and on what position. Though voters may, since 1998, cast preference votes for specific candidates, the list position still nearly exclusively determines winning. (see Dancygier et al. 2015 for a more detailed description).

Previous research has shown that immigrant groups are still severely politically underrepresented in Sweden. Studying the probability of being nominated and elected to municipality councils Dancygier et al. found that in 2010, which is the last year included in

this study, natives were 2 times as likely to be elected and 1.7 times as likely to be nominated to political office compared to immigrants (2015, tables 2 and A5).²

In this study, we will focus on the probability of nomination. The main reason for this is methodological. Even though we have access to population data, our sample only includes 123 individuals that were elected to political office at least once during the study period, which severely hampers statistical power. The number of nominated individuals is 795, and therefore we can get somewhat better precision in the estimates when studying nomination rather than election. But the choice of dependent variable can also be defended on theoretical grounds. First, the fact that more than 99 percent of all eligible individuals does not appear on a party list during a given election reflects the vital role of candidate selection. Put simply, the real hurdle in Swedish politics is not getting elected, but instead getting one's name on the party list in the first place. Second, failure to get elected does not necessarily preclude individuals at the lower end of the party lists from reaching different political positions. Above all, non-elected candidates are commonly used to populate the many municipality boards and committees.³

Turning to the key independent variable, we rely on the logarithm of ethnic density to measure ethnic concentration for the bulk of this analysis. Ethnic density is simply the

² More precisely, Dancygier et al. (2015) showed that in 2010 0.2 percent of all eligible natives and 0.1 percent of all eligible immigrants were elected to political office. The corresponding numbers for the probability of being nominated were 0.8 and 0.5 percent respectively.

³ About 60 percent of the non-elected candidates in the 2006 and 2010 municipal elections were members of one or more board or committee in 2007 and 2011.

percent of the total population in a municipality that any given ethnic group comprises. The reasons for taking the log of this ratio are theoretical as well as methodological.

Theoretically, as argued by Bertrand et al. (2000), even if a small ethnic group was fully concentrated in a single municipality, the group would never constitute a large fraction of the population in that municipality. Yet because, individuals tend to self-segregate into social networks rather than match randomly, we could still imagine that members of a small ethnic community spend considerable time with their co-ethnics even if that group makes up only a very small proportion of the municipality's population. From a theoretical standpoint, using logarithmic shares is preferable since it prevents us from underweighting small ethnic groups in the analysis.

Methodologically, as shown by Gerdes (2011), it is usually preferable to work with logarithmic, rather than actual, shares when estimating fixed effects models. This is because, when using actual shares, the observations are implicitly weighted by the denominators used to calculate the shares, which implies scope for spurious correlation between the shares and the dependent variable. Moreover, the logarithmic transformation considerably reduces the skewness of the ethnic density variable.⁴

The ethnic groups used for calculating the ethnic density measures are based on country of birth. However, for reasons of confidentiality, the country of birth variable has been grouped into 27 distinct groups. For immigrants from significant sending countries (e.g., Iran, Iraq, and Turkey) the region code is that of the country, but for those from other countries the code also includes neighboring countries (the full classification together with a frequency table of the relative size of the different groups is provided in the Appendix). Although we would

⁴ It should also be noted that because our models include assigned municipality fixed effects, our measure is observationally equivalent to using the logged size of the ethnic group in a municipality as done by Edin et al. (2003).

have preferred to have access to actual country of birth for all immigrants, our robustness checks (presented below) suggest that the aggregation does not unduly affect our main findings.

As can be seen from the descriptive statistics presented in the first column of Table 1, only a small share of the individuals in our sample ended up as nominees on a party list in the 1991 to 2010 elections. On average, the probability of being nominated to municipal parliament in this group was 0.6 percent, whereas the probability of actually being elected was as low as 0.08 percent. These figures highlight the fact that large data sets are needed to analyze these questions.

Table 1. Descriptive Statistics

Variables	Full sample	Initial placement	
		Low density	High density
Age	43.10 (9.91)	42.73 (9.61)	43.48 (10.18)
Female	0.42 (0.49)	0.41 (0.49)	0.44 (0.50)
Married	0.56 (0.50)	0.55 (0.50)	0.56 (0.50)
Nr. of children under 16	0.95 (1.27)	0.97 (1.30)	0.93 (1.24)
Years of education	10.82 (2.72)	10.84 (2.75)	10.80 (2.69)
Ethnic density (log)	-5.82 (1.02)	-6.60 (0.73)	-5.04 (0.55)
Nominated (%)	0.60 (7.71)	0.73 (8.50)	0.47 (6.82)
Elected (%)	0.08 (2.70)	0.09 (2.97)	0.06 (2.49)
Age at immigration	30.38 (8.11)	30.02 (7.72)	30.75 (8.46)
Immigration year	1988.95 (1.37)	1988.90 (1.40)	1989.00 (1.33)
Observations	229,250	114,620	114,630

Note: The table reports means and standard deviations (within parentheses) for three different samples. Low density refers to the individuals for which ethnic density in the assigned municipality is below the median value, whereas high density refers to observations for which this value is above the median.

From the descriptive statistics we can also see that the individuals in our sample on average immigrated at the age of 30, had around of 11 years of schooling and were somewhat more

likely to be male than female. Taking the average across all years, municipalities and country groups, ethnic density in our sample is about 0.5 percent.

Statistical Estimation

The dominant approach in previous research on the relationship between ethnic concentration and political participation has been to relate the political activity of various ethnic groups to the number of co-ethnics living in their neighborhood (Cho 1999, Cho et al. 2006, Fieldhouse and Cutts 2008a). However, a well-known problem with this approach is that the immigrants that choose to live in areas with many co-ethnics may be different from those that settle in more diverse neighborhoods. To the extent that immigrants' settlement choices are correlated with unobserved factors driving political participation, this will lead to bias in the estimate of the effect of ethnic concentration on political activity. Immigrants with limited Swedish proficiency, for example, may be more likely to cluster together and less likely to participate in politics. Given our lack of a clear understanding of the process underlying settlement choices, it is very difficult to make any informed guesses about either the magnitude or the sign of this bias.

In this study we will use the Swedish placement program discussed above to help mitigate bias. More specifically, we will follow the approach of Edin et al. (2003) and use the ethnic composition of the initial (assigned) municipality as an instrument for ethnic concentration in later years. Fundamentally, we are interested in gauging how the nature of refugees' communities in an election year influenced their propensity to run for office. Through instrumentation, we attempt to identify the effect of ethnic concentration on elite participation by isolating the part of contemporaneous ethnic concentration that was driven uniquely by concentration at the time of initial settlement. More concretely, this method identifies the effect of ethnic concentration on immigrants who remained in their initial municipality of

assignment through the year of each respective election. The first and second stages of the two-stage least-squares model are specified as follows:

$$E_{igmt} = \delta E_{gm0} + \mathbf{\Gamma}' \mathbf{X}_{it} + \eta_{igmt} \quad (1)$$

$$Y_{igmt} = \gamma \hat{E}_{igmt} + \mathbf{\beta}' \mathbf{X}_{it} + \epsilon_{igmt} \quad (2)$$

where i indexes individuals, m municipalities, g ethnic groups, and t time. In the first stage, we regress ethnic concentration in each of the election years (E_{gmt}) on ethnic concentration in the assigned municipality, measured at the time of placement (E_{gm0}). Then in the second stage, Y_{igmt} , which is a binary indicator for running for local office (1="nominated", 0="not nominated"), is regressed on the predicted scores from the first stage. Ethnic concentration constitutes the number of individual i 's co-ethnics relative to the total number of individuals in a municipality m .

As discussed above, there are some indications in previous research that Immigration Board officers might have taken easily-observed individual characteristics into account when deciding where to place the new immigrants. For this reason, we control for a number of individual level characteristics in our model (\mathbf{X}_{it}), such as gender, formal education at the time of placement, family size at the time of placement, and year of birth. To reduce the risk of omitted variable bias even further, the regressions also include a full set of country of origin fixed effects, immigration year fixed effects, and fixed effects for assigned municipality.

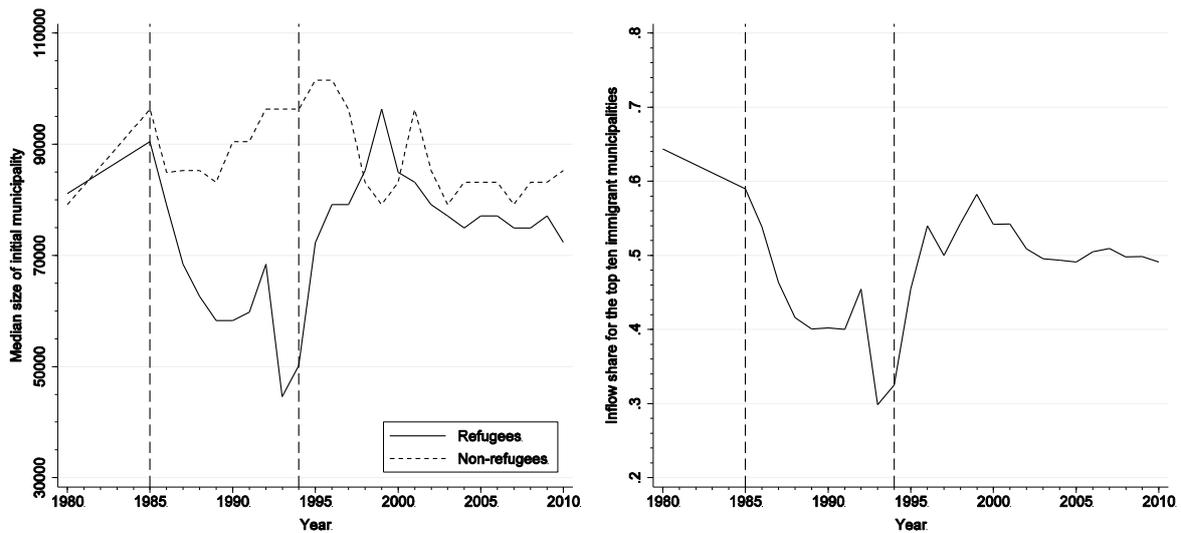
Somewhat simplified, the effect of ethnic concentration on political participation will be identified from comparing individuals from a particular country (group) that arrived to Sweden in the same year, but who were placed in municipalities with unequally large shares of residents from that country. Moreover, the fixed effects for arrival municipality will pick up the importance of all municipality characteristics that are constant for all immigrants assigned to a particular municipality, such as, for instance, its population size. It is important

to note, however, that we do not control for any municipal characteristics at later times since these latter factors may be endogenous due to systematic geographic mobility.

The identifying assumption underlying our empirical analysis is thus that the initial placement of immigrants, during the period in which the placement policy was in place, can be considered as exogenous conditional upon the covariates included in the model. The argument is thus not that refugee placement was completely random during the period of interest. It is, for instance, easily seen from columns 2 and 3 of Table 1 that the immigrants placed in municipalities with low ethnic density were not equal to those placed in municipalities with high ethnic density in all relevant respects. The assumption is instead that the Immigrant Board Officers had to decide the placement of the refugees on the basis of the observed individual characteristics included as controls in our model, such as age, gender, education and family situation. Unfortunately, it is very difficult to test the tenability of the conditional exogeneity assumption directly since it would require access to variables that are related to the outcome of interest (e.g., innate ability) and that were not available to (or considered by) the Immigrant Board Officer deciding on the placement.

However, as discussed above previous research on the topic has gathered a substantial amount of circumstantial evidence in support of the view that the use of the placement policy considerably reduces, if perhaps not totally eliminates, the problems of self-sorting typically plaguing studies of ethnic concentration (Edin et al. 2003; Åslund and Fredriksson 2009; Åslund et al. 2010; Grönqvist et al. 2012). The most convincing piece of evidence, in our view, is the major change in settlement patterns induced by the reform, which is illustrated in Figure 1.

Figure 1. The Effect of the Placement Policy on Immigrant Settlement.



Note: Data for the years 1981 to 1984 is missing and has been imputed by linear interpolation. Municipality size in 1985 has been used for all calculations. The top ten immigrant municipalities is the ten municipalities that had the largest share of inhabitants from refugee sending countries according to the Census conducted in 1985.

The leftmost graph of the figure shows how the (median) size of immigrants' initial municipality of residence has varied over time for refugee and non-refugee⁵ immigrants, respectively. Before the introduction of the placement policy the median municipality size was similar for both groups, but it decreased substantially for the refugee immigrants—which were the ones affected by the policy—when the policy was put in place. After the abolition of the policy in 1994, the settlement patterns of the two groups once again became more similar. This indicates that the policy had real consequences for the residential location of refugee immigrants.

This conclusion is further corroborated by the graph to the right in Figure 1. The solid line illustrates the share of newly arrived refugee immigrants that settled in the ten municipalities with the highest proportion of inhabitants from refugee sending countries in 1985. In the mid-1980s, these ten municipalities received more than 60 percent of all new refugee immigrants,

⁵ We exclude immigrants from Nordic countries from this group since the settlement patterns of these immigrants are very different from non-Nordic immigrants.

but this share more than halved during the time of the placement program. However, when the program ended this share quickly returned to its previous level.

Consequently, there is clear evidence that the placement policy considerably reduced the room for refugee immigrants to settle in the municipality of their own choosing. Given that the case officers deciding on the placement of newly arrived immigrants did not get to meet with the immigrants being placed it also appear very unlikely that placement should correlate with any important unobserved individual characteristics. We therefore find the conditional exogeneity assumption plausible.

Although the conditional exogeneity assumption is sufficient to obtain a causal estimate of the effect of initial ethnic concentration on political activity, the two-stage least-squares procedure, discussed above, requires us to invoke an additional assumption. That is, we need to assume that the effect of initial ethnic density on candidacy is fully transmitted through current ethnic density (this is the so-called exclusion restriction). Although it could not be completely ruled out that initial ethnic density could have a direct and long-lasting effect on political engagement, we believe it reasonable to assume that most of the effect is mediated by current ethnic density. In line with the seminal study of Edin et al. (2003), we will therefore proceed with the instrumental variable approach in the following empirical analysis.

Empirical Results

Table 2 displays the results of the basic specification where the outcome of interest is whether an individual was nominated for municipal parliament in the six elections held between 1991 and 2010. For most parts of the analysis we will make use of a linear probability model to obtain our estimates. The reason for this is that the instrumental variables approach becomes much more involved when applied to non-linear models such as logit or probit. However, we will also examine the results from an instrumental variables probit model as a robustness check.

Table 2. Relationship between political candidacy and ethnic density.

	OLS	IV	OLS	IV
Ethnic density	-0.124** (0.052)	-0.462** (0.170)	-0.095* (0.054)	-0.395** (0.170)
Female			-0.188*** (0.053)	-0.172*** (0.045)
Married			0.067 (0.062)	0.064 (0.061)
Education			0.106*** (0.019)	0.103*** (0.014)
Children			0.030 (0.021)	0.041* (0.023)
First stage coef.		0.263*** (0.022)		0.261*** (0.022)
Observations	229,250	229,250	229,250	229,250

Note: All models include fixed effects for election year, year of birth, country of origin, and year of immigration. In addition the OLS models include municipality fixed effects and the IV models fixed effects for assigned municipality. Clustered standard errors are shown in parentheses. ***/**/* indicate significance at the 1/5/10% level.

Columns (1) and (3) of Table 2 report standard OLS estimates where ethnic density in an election year is treated as exogenous. Columns (2) and (4), on the other hand, provide the two-stage least-squares results where we use ethnic density for each immigrant's initial placement as an instrument for ethnic density in the election years.⁶

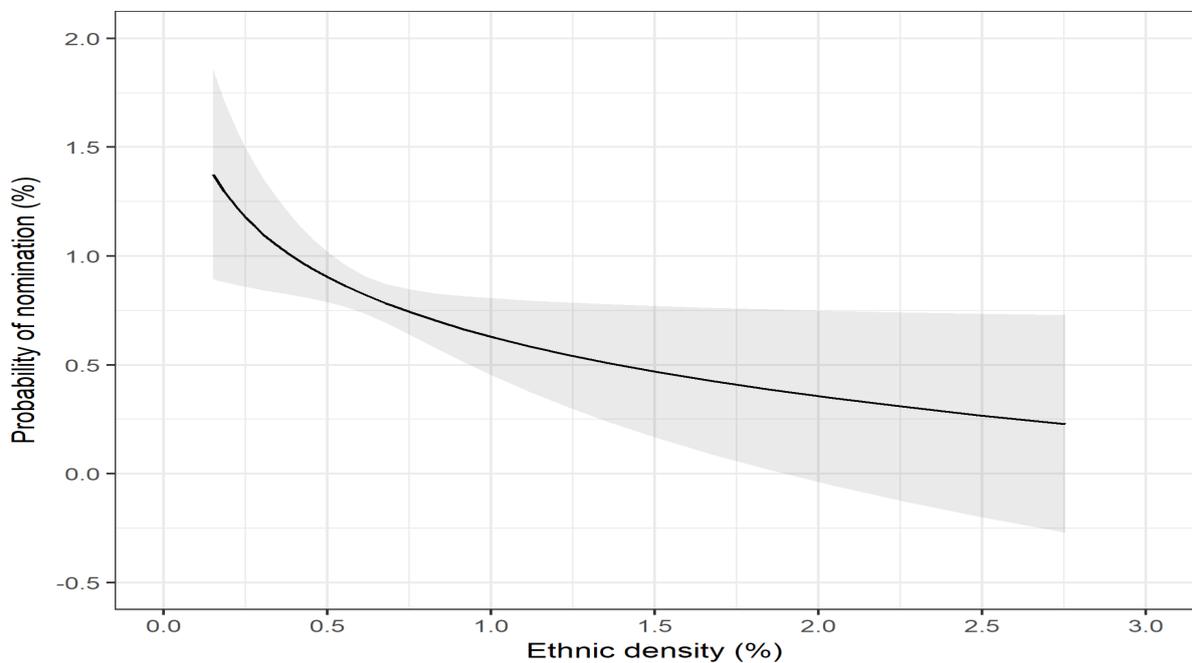
To judge from the OLS estimate an increase in ethnic density by one log unit is associated with a 0.1 percentage point lower probability of running for local office in a given election. However, the coefficient is no longer statistically significant at the .05 level once we control for individual characteristics such as education and gender.

The instrumental variables (IV) estimates, however, suggest that ethnic density has a stronger and statistically significant, negative effect on political candidacy. Increasing (log)

⁶ In all IV models standard errors are clustered on assigned municipality to account for correlations in error terms across individuals. Correspondingly, in the OLS models the standard errors are clustered on the municipality of residence.

ethnic density by one unit is estimated to decrease the likelihood of running for office by 0.38 percentage points. Alternatively, if we increase ethnic density from the 25th to 75th percentile the probability of nomination is reduced by 0.42 percentage points (based on the results in Column 4). Given that the baseline probability of nomination in this group is as low as 0.6 percent this is indeed a substantively important effect.

Figure 2. Predicted probability of nomination by ethnic density



To get an even better sense of the magnitude of this effect, Figure 2 illustrates the relationship between the predicted probability of nomination and the actual, rather than the logarithmic, ethnic share (this graph is based on the results in column 4 of Table 2). The minimum and maximum values on the x-axis correspond to the 5th and 95th percentiles of the ethnic density variable. As can be seen the probability of nomination decreases substantially as ethnic density increases. To judge from these results an immigrant living in a municipality where ethnic concentration corresponds to the 5th percentile is more than 5 times as likely to

stand for political office compared to a similar individual living in a municipality where ethnic density is at the 95th percentile.

Another important issue concerns the strength of the instrument used in the analysis. As can be seen from the first-stage coefficients reported at the bottom of Table 2 the instrument does indeed have strong predictive power. An increase in (log) ethnic density in the assigned municipality by one unit increases (log) ethnic density in later years by 0.26 units, and the effect is highly statistically significant (the F-statistic of the first stage is 143). Concretely, this means that sufficiently many immigrants remained in place over time for ethnic concentration at assignment to affect ethnic concentration at later points in time.⁷

According to these results the standard OLS approach, used in most previous research on the topic, seems to suffer from sorting bias. The instrumental variable estimates are four times as large as the OLS estimates. Admittedly, part of this difference could be due to the effect that the instrumental variables approach gives us the local average treatment effect (LATE) for the compliers of the placement policy, rather than the average treatment effect (ATE). Yet, given the relatively small differences in observed characteristics between the immigrants staying in their assigned municipality (the compliers) and those moving to another municipality (see Table A3 in the Appendix) we do not find it likely that the difference between the LATE and the ATE is sufficiently large to account for the large change in coefficients when going from OLS to IV. In our view, it therefore appears likely that the effect of ethnic concentration in the OLS model is positively biased.

⁷ Moreover, if we multiply the first-stage coefficient with the ethnic density coefficient we obtain an estimate of the so-called reduced form effect. That is, based on the estimates presented in Column 4 of Table 2 we can see that the reduced form effect of (log) ethnic density is $-.103 (-0.395 \cdot 0.261)$.

Robustness and Potential Causal Mechanisms

To judge from the results presented above, ethnic residential segregation seems to have a negative effect on immigrants' likelihood of standing for political office. Before drawing any firm conclusions, however, we need to examine the robustness of these findings. In Table 3 we therefore alternate the baseline specification in a number of different ways.

Table 3. Sensitivity Checks.

	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) IV-Probit	(5) 2SLS	(6) IV-Probit
Ethnic density	-0.382** (0.170)	-0.458** (0.196)	-0.785** (0.319)	-0.352** (0.173)	-0.095 (0.068)	-0.349** (0.151)
Female	-0.172*** (0.045)	-0.187*** (0.055)	-0.115 (0.075)	-0.217*** (0.053)	-0.164*** (0.045)	-0.214*** (0.051)
Married	0.059 (0.062)	0.085 (0.076)	0.047 (0.099)	0.058 (0.068)	0.055 (0.061)	0.055 (0.069)
Education	0.105*** (0.015)	0.122*** (0.014)	0.089*** (0.018)	0.112*** (0.011)	0.107*** (0.014)	0.113*** (0.011)
Children	0.036 (0.023)	0.050* (0.027)	0.023 (0.041)	0.045* (0.024)	0.040* (0.023)	0.042* (0.023)
Seats to voters	290.123*** (53.704)					
First stage	0.258*** (0.022)	0.244*** (0.020)	0.248*** (0.026)	0.258*** (0.024)	0.832*** (0.094)	0.821*** (0.104)
Observations	229,250	186,262	113,432	205,770	229,250	205,770

Note: All models include fixed effects for election year, year of birth, country of origin, year of immigration, and assigned municipality. Clustered standard errors are shown in parentheses. ***/**/* indicate significance at the 1/5/10% level. In models 1-4 Ethnic density has been log transformed, whereas the unlogged version of the variable is used in models 5-6. The probit models are estimated by the ivprobit command in Stata, and the coefficients for these models refer to average marginal effects.

In the IV models in Table 2 we controlled for assigned municipality but not for any election-year municipality characteristics (e.g., average education levels) since these latter variables may be endogenous due to sorting. In Column (1) of Table 3 we make a partial exception from this procedure and include the seats-to-voters ratio of the current municipality among the set of controls. By controlling for this variable we adjust for the strong and mechanically negative effect between the size of the electorate and the probability of running for office,

which is due to the fact that the size of local assemblies does not increase proportionally to the size of the electorate (Dancygier et al. 2015). However, as can be seen the ethnic density variable is hardly affected at all by the inclusion of the seats-to-voters variable.

In Column (2) we instead examine to what extent the results are unduly driven by immigration to the large urban areas by excluding the individuals that were placed in the three big cities Stockholm, Gothenburg, and Malmö from the analysis. Although this means that we lose almost a quarter of our original sample the substantive results remain very similar.

A potential drawback with the data being used is that immigrants from small source countries have been grouped together with immigrants from neighboring countries for reasons of confidentiality. To examine whether this poses a problem we have re-run the model including only immigrants from countries that we can identify uniquely. The results are presented in Column (3) and, if anything, the negative effect of ethnic density on political candidacy becomes even more pronounced when restricting the analysis to immigrants from individual source countries. According to these results, increasing ethnic density by one log unit can be expected to decrease the probability of nomination by as much as 0.8 percentage points (although the coefficient is not very precisely estimated).

Throughout this paper, we have employed a linear probability model, adjusting for heteroscedasticity in the error terms. We did so because instrumental variable estimation is much more involved, and requires additional strong model assumptions, when applied to non-linear models such as logit or probit. Nonetheless, in Column (4) of Table 3 we present the results from the instrumental variable probit model discussed by Newey (1987). An advantage with this model is that we can take the binary nature of our dependent variable into account. However, a disadvantage is that we now have to assume that initial ethnic density is

not only *a* valid instrument for ethnic density in later years, but that it is the *only* relevant instrument (Lewbel et al. 2012).

This said, it is reassuring to note that the substantive results of the probit model are very similar to those of the linear probability model. To increase comparability the coefficients for the probit model refer to average marginal effects, i.e., the change in the probability of a positive outcome (nomination) associated with a one unit increase in the independent variable of interest. As can be seen the marginal effect from the probit model closely mimics the linear probability effect reported in the last column of Table 2.

Finally, we previously argued that there are both theoretical and methodological reasons to log transform the ethnic density variable. Yet, it might nevertheless be interesting to examine what happens to the results if we use actual shares instead of log shares in the model. This we do in columns (5) and (6). Starting with the linear probability specification in Column (5) we see that the coefficient of the ethnic density variable remains negative, but it is no longer statistically significant at conventional levels.

However, when using the instrumental variable probit model the ethnic density variable increases in magnitude and regains statistical significance, as indicated by the results reported in Column (6). One likely reason for the greater difference between the probit and linear probability results in this case is that the probit model implicitly accounts for the decreasing marginal effect of ethnic share on the probability of nomination. These results could thus be taken support for our choice to use the logarithmic transformation of ethnic density in the main analysis.

Overall, our main finding appears to be robust to our modelling assumptions. Once we have adjusted for the sorting bias plaguing the standard OLS approach we thus find strong support for the view that immigrants living in ethnic enclaves stand a smaller chance of

obtaining political office. According to these results, ethnic segregation could therefore serve to aggravate the political underrepresentation of immigrants.

Mechanisms

Finally, a natural question to ask is what mechanisms underlie the observed negative relationship between residential segregation and political activity. Due to data constraints, a more in-depth analysis of the causal mechanisms at work is unfortunately outside the scope of the current study, but we will use the available register data to try to shed at least some light on this important issue.

In Table 4 we therefore present results from a simple mediation analysis in which we control for some potential mediators. Admittedly, the validity of this type of analysis rests on strong modeling assumptions (Imai et al. 2011), but we nevertheless consider it a first valuable step to better understand why residential segregation affects immigrants' likelihood of running for political office.

The first column of Table 4 reproduces the estimate from our preferred specification for reasons of comparison. In Column 2 we add controls for earnings, (current) education, and employment.⁸ The basic idea of this analysis is to examine the extent to which the effect of residential segregation on political candidacy is channeled through socio-economic status (Verba and Nie 1972). Residential segregation may have a negative influence on educational and employment opportunities, which could lower immigrants' likelihood of standing as a candidate (Sanders and Nee 1987; Chiswick and Miller 1985). Or, alternatively, immigrants living in enclaves may find work more easily than more isolated immigrants through co-

⁸ Previously we have controlled for education at the time of immigration, but now we also control for education at the time of the election.

ethnic social networks, or earn more than their counterparts (Portes 1981; Portes and Bach 1985; Portes and Jensen 1989; Wilson and Portes 1980).

As expected, the results suggest that earnings, education and employment are strongly and positively related to candidacy status. Moreover, the coefficient of the ethnic density variable decreases slightly (by about 7 percent) when controlling for socio-economic status, but given the size of the standard errors of the various coefficients this reduction should not be overinterpreted. This finding suggests that, even if residence in enclaves influences immigrants' socioeconomic status, such changes in status do not seem to mediate the relationship between residence in enclaves and elite political engagement to any larger extent.

Table 4. Mediation Analysis

	(1)	(2)	(3)	(4)
Ethnic density	-0.395** (0.170)	-0.367** (0.169)	-0.351** (0.172)	-0.350** (0.172)
Earnings		0.009** (0.003)	0.009** (0.003)	0.008** (0.003)
Education		0.157*** (0.024)	0.155*** (0.024)	0.154*** (0.024)
Employed		0.200*** (0.058)	0.194*** (0.058)	0.149** (0.068)
Native partner			0.352** (0.152)	0.347** (0.152)
Share native colleagues				0.118 (0.113)
Observations	229,250	229,250	229,250	229,250

Note: All models include fixed effects for election year, year of birth, country of origin, year of immigration, and assigned municipality, as well as controls for gender, marital status, number of children, and education at the time of immigration. Clustered standard errors are shown in parentheses. ***/**/* indicate significance at the 1/5/10% level.

In columns 3 and 4 we instead examine whether the observed effect of ethnic concentration can be explained by the fact that immigrants surrounded by many individuals of similar ethnic background may be less likely to come into contact with native Swedes. Here, we test the classical assimilation hypothesis, which holds that such limited contact may militate against immigrants' political socialization and exposure to information about host-country

politics. In Column 3 we therefore add a dummy variable indicating whether an individual has a Swedish born partner.⁹ Although having a Swedish born partner is a strong predictor of running for office the coefficient of the ethnic density variable decreases only marginally when adding this control to the model. In Column 4 we also add the share of Swedish born colleagues¹⁰ to the model, but once again the estimated effect of ethnic concentration hardly changes at all as a result of this.

Although our mediators only account for a small fraction of the overall effect of ethnic concentration on political candidacy, these results are valuable in helping to rule out potential mechanisms. To judge from these results the observed effect is not, to any large extent, channeled through socio-economic status or through exposure to natives at home or in the workplace. These results challenge classical assimilation theory's assumptions that residence in enclaves militates against immigrants' political incorporation by limiting their contact with natives. Moreover, supposing that inter-marriage with Swedes and ethnically diverse workplaces enhance immigrants' proficiency in Swedish, these results cast some doubt on the hypothesis that our findings can be explained by the effect of residence in enclaves on language proficiency.

In order to better understand why ethnic concentration decreases the likelihood of immigrants to stand for office future research thus need to look elsewhere. For instance, ethnic concentration may depress immigrants' internal efficacy – their sense that they are capable of effectively participating in Swedish politics. In particular, immigrants residing in

⁹ The variable takes on the value of 1 for those having a Swedish born partner and 0 for those being single or having a foreign born partner.

¹⁰ This variable records the share of Swedish born individuals employed at the same workplace as the immigrant in question. For individuals without employment this share is set to 0.

enclaves may assume that political participation is the domain of natives possessing stronger local knowledge and network ties (Reny and Shah forthcoming). Concentration might also create pressures for immigrants to spend their time and resources on causes linked to the politics of their countries of origin rather than Swedish politics. Indeed, in order to determine why residence in enclaves depresses immigrants' elite political participation, further survey research may be necessary.

Conclusion

In this paper, we present evidence that immigrants residing in ethnic enclaves may pursue political leadership less often than others. This finding challenges the notion that residence in enclaves can foster immigrants' political incorporation by spurring the formation of social networks and group consciousness and facilitating political parties' mobilization efforts. Instead, our results suggest that residence in enclaves decreases immigrants' political incorporation. This finding has important implications for refugee resettlement policy as well as local immigrant integration efforts.

Our paper constitutes both a valuable substantive and methodological contribution to the existing literature. First, to our knowledge, this constitutes the first quantitative paper relating immigrants' residence in ethnic enclaves to their propensity to run for office. Methodologically speaking, Sweden's unique individual registry data on candidacy, encompassing both winning and losing candidates, enables us to analyze the determinants of immigrant elite participation in a hitherto unprecedented manner. Further, to date, most of this literature has relied on data on minority population "stock" without accounting for self-sorting into ethnic neighborhoods (Borjas 1995; Cutler et al. 2008). By leveraging the exogeneity of Sweden's historical refugee placement programs, our findings limit biases that sorting might otherwise engender.

Our analysis also provides some tentative insights into possible mechanisms linking residence in enclaves to decreased participation. In particular, we find little evidence that enclaves decrease participation by limiting immigrants' contact with Swedes and decreasing their incentives to learn Swedish. Further, this relationship does not appear to be mediated by enclaves' effects on immigrants' socioeconomic status. At the moment, data constraints do not allow us to gauge whether immigrants' residence in enclaves alters their participation by changing their attitudes towards host-country politics. That said, our findings suggest that research along this vein – and particularly research gauging whether residence in enclaves depresses immigrants' sense of political efficacy – may constitute a promising way forward.

Ultimately, this article represents only an early step towards a better understanding of the effect of context on minorities' elite participation. For one, the settlement program under study was limited to refugees. It is plausible that our results may thus not generalize to economic migrants or family reunification migrants with differing experiences of political socialization in their homelands and upon arrival in Sweden. In particular, some refugees may be less familiar with democratic politics than non-refugee immigrants (Portes and Mozo 1985). Future work might therefore examine the relationship between residence in enclaves and elite participation among economic and family reunification migrants.

Moreover, moving forward, scholars would do well to carry out further research exploring the mechanisms through which neighborhood context may influence minorities' propensity to run for office. In a context of rising nationalism and burgeoning anti-immigrant sentiment across developed democracies, further research along this vein may support efforts to improve minority representation and, ultimately, safeguard minorities' rights.

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Online Appendix

A. Data Availability

In this paper, we use individual level information obtained from various administrative registers. The data are stored on an encrypted server at Statistics Sweden and all our analysis have been conducted through a remote desktop application. We are under contractual obligation not to disseminate these data to other individuals. For interested researchers there are, however, two ways to get access to the administrative data used in this paper for replication purposes. The first possibility is to order the data directly from Statistics Sweden. Currently, Statistics Sweden requires that researchers obtain permission from a Swedish Ethical Review Board before data can be ordered (a description, in Swedish, of how to order data from Statistics Sweden is available at: http://www.scb.se/sv_/Vara-tjanster/Bestalla-mikrodata). We will also make available a complete list of the variables that we ordered from Statistics Sweden for this project.

The second possibility to replicate our analyses for interested researchers is to come to **[named removed to safeguard anonymity]** and reanalyze these data through the same remote server system that we used for our analyses. Any researcher interested in using this option needs to contact us before coming to Sweden so that we can arrange with Statistics Sweden that the researcher is temporarily added to our research team, which is required in order to get access to the remote server system.

B. Variables and Data Sources

Nominated – Equal to 1 if the individual ran for office at the municipal level in the six elections held in 1991, 1994, 1998, 2002, 2006, and 2010. Information is retrieved from the Register of Nominated and Elected Candidates held at Statistics Sweden.

Elected – Equal to 1 if the individual was elected for office at the municipal level in the six elections held in 1991, 1994, 1998, 2002, 2006, and 2010. Information is retrieved from the Register of Nominated and Elected Candidates held at Statistics Sweden.

Female – Equal to 1 if female. Information is retrieved from the Swedish Population Register.

Birth Year – Information is retrieved from the Swedish Population Register.

Immigration Year – Information is retrieved from the Swedish Population Register.

Married – Equal to 1 if married at the time of arrival in Sweden. Information is retrieved from the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Native Partner – Equal to 1 if married to or cohabiting with a swede at the time of each of the six elections held between 1991 and 2010. Information is retrieved from the 1991-2010 waves of the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Years of Schooling – Educational attainment at the time of arrival in Sweden or, if information is missing, at the first available year according to the three-digit Swedish standard classification of education (SUN 2000). Following the manual for classifying educational programmes in OECD countries (ISCED-97), we assigned the following years of schooling to each category: (old) primary school (7); (new) compulsory school (9); (old) junior secondary education (9.5); high school (10-12 depending on the program); short university (13); longer university (14-16 depending on the program); short postgraduate (17); long post-graduate (19). The information on educational attainment is retrieved from the 1990-2010 waves of the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Years of Schooling at Election Years – Educational attainment at the time of each of the six elections held between 1991 and 2010 according to the three-digit Swedish standard classification of education (SUN 2000). Following the manual for classifying educational programmes in OECD countries (ISCED-97), we assigned the following years of schooling to each category: (old) primary school (7); (new) compulsory school (9); (old) junior secondary education (9.5); high school (10-12 depending on the program); short university (13); longer university (14-16 depending on the program); short postgraduate (17); long post-graduate (19). The information on educational attainment is retrieved from the 1991-2010 waves of the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Labor Income – Individual monthly labor income at the time of each of the six elections held between 1991 and 2010 (in 1,000 SEK). The variable is retrieved from the 1991-2010 waves

of the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Children – Number of children at the time of arrival in Sweden. Information is retrieved from the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Assigned Municipality – Code for the municipality to which the refugee was assigned after receiving a residence permit. Information is retrieved from the Swedish Population Register.

Ethnic Density – The share (in percentage points) of the total population in the assigned municipality belonging to the same country group of origin (see below) as the refugee at the time of arrival. Annual information on the size of the stock of co-ethnics is only available from 1990 and onwards. For the years 1990-1994 we use information from the Swedish Population Register, and the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym). For the years 1987-1989 we instead use information on the size of the stock of co-ethnics from the census in 1985 in combination with information on the inflow of immigrants in the years 1986-1989 from the Swedish Population Register. Thus, in order to estimate the annual stock of co-ethnics across municipalities for the early years we need to rely on the assumption that immigrants residing in Sweden at the time of the census in 1985 did not move or die between 1985 and 1990.

Share Native Colleagues – the share of Swedish born individuals employed at the same plant as the immigrant at the time of each of the six elections held between 1991 and 2010. For individuals without employment this share is set to 0. Information is retrieved from the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Seats-to-Voters Ratio – The ratio of the number of municipality council seats to the number of eligible voters in the municipality within which the individual resides at the time of each of the six elections between 1991 and 2010. The data is retrieved from the Swedish Election Authority (www.val.se).

Country of Origin – Country of birth. Information is retrieved from the Swedish Population Register. For reasons of confidentiality, the country of birth variable has been grouped into 27 distinct groups as described in the table below. For immigrants from significant sending

countries (e.g., Iran, Iraq, and Turkey) the region code is that of the country, but for those from other countries the code also includes neighboring countries.

C. Additional Tables

As described in the main text, the information on country of birth has been aggregated into 27 groups for confidentiality reasons. Table A1 provides information on the country groups.

Table A1: The classification of country groups

Code	Country of origin
26	Finland
27	Denmark
28	Norway, Iceland
29	Bosnia-Herzegovina,
30	Yugoslavia, Croatia, Macedonia, Slovenia
31	Poland
32	Ireland, Great Britain
33	Germany, West Germany, East Germany
34	Greece, Italy, Malta, Monaco, Portugal, San Marino, Spain, Vatican City
35	Estonia, Latvia, Lithuania
36	Albania, Armenia, Azerbaijan, Bulgaria, Georgia, Kazakhstan, Kyrgyzstan, Moldavia, Romania, Russia, Soviet Union, Tajikistan, Turkmenistan, Ukraine, Uzbekistan, Belarus
37	Czech Republic, Slovakia, Czechoslovakia, Hungary
38	Andorra, Belgium, France, Liechtenstein, Luxembourg, Netherlands, Switzerland, Austria
39	Canada, USA

40	Antigua, Bahamas, Barbados, Belize, Costa Rica, Cuba, Dominica, the Dominican Republic, El Salvador, Grenada, Guatemala, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, S:t Lucia, St. Vincent, St. Kitts-Nevis
41	Chile
42	Bolivia, Brazil, Colombia, Ecuador, Guayana, Paraguay, Peru, Surinam, Uruguay, Venezuela
43	Djibouti, Eritrea, Ethiopia, Somalia, Sudan
44	Algeria, Bahrain, Cyprus, Egypt, French protectorate in Morocco, United Arab Emirates, Gaza Strip, Israel, Yemen, Jordan, Kuwait, Lebanon, Libya, Morocco, Palestine, Qatar, Saudi Arabia, South Yemen, Syria, Tunisia
45	Angola, United Arab Republic, Benin, Botswana, Burkina Faso, Burundi, Central African Republic, Comoros, Equatorial Guinea, Ivory Coast, Gabon, Ghana, Guinea, Guinea-Bissau, Cameroon, Cape Verde, Kenya, Democratic Republic of the Congo, Lesotho, Liberia, Madagascar, Malawi, Mauretania, Mauritius, Mozambique, Namibia, Niger, Nigeria, Rwanda, Sao Tomé and Príncipe, Senegal, Seychelles, Sierra Leone, Swaziland, South Africa, Tanzania, Chad, Togo, Uganda, Zaire, Zambia, Zanzibar, Zimbabwe
46	Iran
47	Iraq
48	Turkey
49	Hong Kong, Japan, China, Taiwan, North Korea, South Korea
50	Burma, Philippines, Indonesia, Laos, Federation of Malaya, Malaysia, Singapore,

	Thailand, Vietnam
51	Afghanistan, Bangladesh, Bhutan, Brunei, India, Democratic Kampuchea, Maldives, Mongolia, Nepal, Oman, Pakistan, Sikkim, Sri Lanka
52	Australia, Fiji, Kiribati, Micronesia, Nauru, Palau, Papua New Guinea, Solomon Islands, Tonga, Vanuatu, Samoan Islands
53	Unknown

Table A2 then provide information on the number of individuals in our main sample that are born in each of these country groups. As described in the main text our main sample includes 49,874 unique individuals.

Table A2: Individuals by country groups in the main sample

Country group	Individuals	Share
30	2044	4.10
31	1692	3.39
35	153	0.31
36	3554	7.13
37	1146	2.30
40	761	1.53
41	5085	10.20
42	1188	2.38
43	5309	10.64
44	7044	14.12
45	1040	2.09
46	11886	23.83
47	3552	7.12
48	1640	3.29
50	2430	4.87
51	1350	2.71
<i>Total</i>	49874	100

Note: The frequencies refer to the individuals included in the main estimation sample.

In Table A3 we provide separate descriptive statistics for the movers and stayers in our sample for the year 2010. In this table all individuals who remain in their municipality of assignment in 2010 are classified as stayers, whereas all those who live in a different municipality in 2010 is classified as movers. Although there are some differences between the two groups, most of the differences appear rather marginal compared to the standard deviations of these variables (but gender is a notable exception).

Table A3: Descriptive statistics for Movers and Stayers in 2010

	All	Stayers	Movers
Age	51.27	52.27	50.70
	(8.11)	(8.59)	(7.77)
Female	0.43	0.49	0.40
	(0.50)	(0.50)	(0.49)
Married	0.57	0.58	0.56
	(0.50)	(0.49)	(0.50)
Nr. of children under 16	0.98	0.99	0.97
	(1.29)	(1.26)	(1.31)
Years of education	10.81	10.67	10.89
	(2.73)	(2.77)	(2.70)
Ethnic density (log)	-5.81	-5.55	-5.95
	(1.01)	(0.97)	(1.01)
Nominated (%)	0.80	0.79	0.80
	(8.88)	(8.84)	(8.91)
Elected (%)	0.13	0.13	0.14
	(3.64)	(3.59)	(3.67)
Age at immigration	30.38	31.35	29.83
	(8.01)	(8.46)	(7.69)
Immigration year	1989.11	1989.08	1989.13
	(1.37)	(1.38)	(1.37)
Observations	42865	15480	27385

Note: All data in this table refers to the year 2010 and movers are those for who the municipality of residence in 2010 is not the same as the municipality of placement.