

# Bound by Borders: Voter Mobilization through Social Networks

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## Abstract

A vast and growing quantitative literature considers how social networks shape political mobilization but the degree to which turnout decisions are strategic remains ambiguous. Unlike previous studies, we establish personal links between voters and candidates and exploit discontinuous incentives to mobilize across district boundaries to estimate causal effects. Considering three types of networks—families, co-workers, and immigrant communities—we show that a group member’s candidacy acts as a mobilizational impulse that propagates through the group’s network. In family networks, some of this impulse is non-strategic, surviving past district boundaries. However, the bulk of family mobilization is bound by the candidate’s district boundary, as is the entirety of the mobilizational effects in the other networks.

*Keywords: Political participation, Social networks, Electoral geography*

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# 1. Introduction

Political parties can leverage social networks to boost voter turnout (Shachar and Nalebuff, 1999). They can, for example, make appeals through networks; orchestrate pressure to increase the social cost of *not* voting (Dellavigna et al., 2016; Gerber, Green and Larimer, 2008); and choose candidates with an eye to their ability to mobilize the voters with whom they are connected.

Existing studies focus on candidates’ mobilizational incentives (would effort make the difference between winning and losing?) and the characteristics of the networks they seek to activate (how strong are the links?)—while limiting attention to networks embedded within single electoral districts. For example, experimental studies examine the propagation of mobilizational messages from spouse to spouse (Nickerson, 2008) and friend to friend (Bond et al., 2012); survey-based analyses explore propagation within villages (Cruz, 2019; Eubank et al., 2021); observational studies consider propagation through electoral districts (Cox, Rosenbluth and Thies, 1998); and studies based on administrative data examine propagation from spouse to spouse (Dahlgaard et al., 2022) and neighbor to neighbor (Finan, Seira and Simpser, 2021). Because these studies focus on single districts, they cannot examine how mobilization and turnout change when district boundaries are crossed—which is our focus here.<sup>1</sup>

Theories of turnout can be divided into those that emphasize strategic mobilization by candidates and parties; and those that stress individual voters’ characteristics. Strategic mobilization theories naturally imply that mobilizers will target those who can vote in the specific election in which they are interested; and will thus be concerned with voters’ geo-location inside or outside of electoral district boundaries. In contrast, prominent alternative theories downplay mobilization and focus instead on (a) consumption values such as “citizen duty” (Riker and Ordeshook, 1968), (b) individual resources and

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<sup>1</sup>A related literature uses fine-grained geo-coded data to study the importance of geography in determining the location of local public goods and bads (Carozzi and Repetto, 2019; Folke et al., forthcoming; Harjunen, Saarimaa and Tukiainen, 2023). These papers also focus on single electoral districts and thus do not examine how mobilization in social networks changes when district boundaries are crossed.

expressive values (Brady, Verba and Schlozman, 1995), and (c) altruism (Fowler, 2006).<sup>2</sup> Under these theories, voting is a largely non-strategic act, and—as we explain below—electoral boundaries should play a much smaller role than they do in models of strategic mobilization.

Our aim in this paper is to empirically explore whether and how much turnout is shaped by electoral boundaries. In particular, we examine the effects of within-network candidacies on turnout in several different social networks; and the extent to which these effects change at district boundaries. Do effects decline sharply, consistent with mobilization being an important determinant of turnout; or do they decline gently or insignificantly, consistent with turnout being driven mostly by individual resources and decisions? The stronger the boundary effect, the more that parties should take into account the overlap between potential candidates’ social networks and their electoral districts; and we explore this issue, too.

The empirical setting for our analysis is Norway, which affords panel data on the turnout of a large sample of urban Norwegians. Our unique data allow us to observe these voters’ connections to the universe of local-level political candidates (approximately 60,000 per year) over two election periods. We consider three types of social networks—families, co-workers, and immigrant-occupation groups—and estimate the extent to which the candidacy of a group member acts like a mobilizational impulse which propagates through the group’s network.<sup>3</sup> Our research design mitigates several problems noted in the literature on peer effects (Bramoullé, Djebbari and Fortin, 2020). For example, neither self-selection into networks nor endogenous change of network structures over time are significant problems for the static networks we study. We deal with common external causes of turnout via fine-grained local unit-time fixed effects.

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<sup>2</sup>In a review of the literature, Smets and van Ham (2013, p.345) conclude that the “jury is still out on what the foundations of micro-level turnout are”.

<sup>3</sup>Several scholars have used comparable administrative data from Norway to examine the empirical relevance of different types of social networks. For example, Dahl, Kostøl and Mogstad (2014) document the existence of ‘family welfare cultures’, where parents’ involvement in disability insurance influences their children’s future participation. Markussen and Røed (2015) document how social insurance claims spread among neighbors and former schoolmates. Additionally, Bratsberg et al. (2021) find that the initial neighborhood that refugees are placed is highly predictive of future electoral participation.

We find that the mobilizational boost from having a network member running for office is about two to four percentage points. The boost is stronger in narrow networks (e.g., close family members), falls moderately with increasing geographical distance, but falls sharply to zero when social networks cross district boundaries. This suggests that candidates seek to win seats and therefore mobilize only those in their network(s) who can actually vote for them.

We also provide two kinds of evidence that political parties select immigrant candidates for their mobilizational prowess. First, in Section 6 we document a “Jackie and Jill effect” (Anzia and Berry, 2011): immigrant candidates face voter bias and it appears that they can secure list spots only if they can mobilize enough new voters to compensate for the loss of biased voters. Consistent with this view, we find that immigrant candidates generate substantially larger turnout boosts among their social networks (here, we explore in particular their families) than do native candidates; and this effect is larger in parties whose members view immigrants less favorably. Second, in Section 7 we offer some correlational evidence that immigrants with more *electorally efficient* occupational networks—with higher percentages residing in the same electoral district as the potential candidate—are more likely to become candidates.

## 2. Mobilizing social networks across boundaries

If voters care only about which candidate wins, then equilibrium turnout will be near zero in large electorates, since the probability of a single vote being pivotal is negligible (Palfrey and Rosenthal, 1985). To explain why turnout is well above zero, scholars have sorted into two broad schools, one arguing that turnout results from individual decisions, another focusing on strategic mobilization.

These schools make differing predictions about how electoral boundaries shape turnout. Strategic mobilizers should naturally target voters who can actually vote for them. Thus, any turnout effects due to candidates mobilizing their social networks should stop at the

border, where their mobilizational incentives discontinuously decline.

In contrast, theories of turnout that focus on individuals sometimes predict little or no border effects. For example, (1) instrumental voters would not generate a border drop-off because the difference between having literally zero chance of affecting the outcome (for out-of-district voters) and virtually zero chance (for in-district voters) is negligible; (2) citizen-duty voters would not generate a border drop-off because they vote based on a generalized sense of duty which should not vary discontinuously at any particular border; (3) genetic predispositions to participate (Fowler and Dawes, 2008) do not vary discontinuously at borders; and (4) individuals' resource endowments (Brady, Verba and Schlozman, 1995) do not vary discontinuously at borders (even if they do, our individual fixed effects adjust for these).

What if voters turn out simply because they enjoy voting for a candidate with whom they have social ties? This act-contingent utility would drop discontinuously at the candidate's electoral border.<sup>4</sup> Thus, if enough voters turn out as an act of consumption, then a border drop-off could arise in the absence of active candidate mobilization.

The main problem with this line of argument is that it assumes voters will automatically learn who is running as a candidate. When turnout increases in a given social network subsequent to the candidacy of one of its members, the most plausible mechanism involves communication. At a minimum, the message must get out that someone in the network is a candidate. This crucial messaging begins, of course, with the candidates themselves, who choose when and how to announce their candidacies. Were a candidate to keep their candidacy secret, their social networks would not be activated on their behalf. For this reason, we view the turnout effect we document as produced by "mobilization." We cannot, however, parse the overall mobilization effect into components due to (i) announcement of candidacy and (ii) additional mobilizational efforts, such as asking for donations of money or campaign effort.

We also know from surveys that acquaintance with and direct contact by candidates

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<sup>4</sup>Of course, if voters enjoy voting for a within-network candidate's *team*, then again district boundaries will not matter as much.

are important mediators for voting decisions. In the 2015 Norwegian local elections, personal familiarity played a major role for 40% of respondents casting a personal vote (Panel A of Figure 1), suggesting candidates mobilized their “friends and neighbors.”<sup>5</sup> A separate survey conducted by Statistics Norway in 2015 showed that 34% of respondents considered “*family, friends, and co-workers*” to be important or very important for getting information about the election, while 19% reported that direct contacts with candidates were important or very important (Panel B of Figure 1).<sup>6</sup>

### 3. Empirical case: Norway 2015–2019

#### 3.1 *Elections and voter turnout*

Norway’s unitary state has three governmental tiers: central, regional and local. The local governments, which employ about 17% of the Norwegian work force, are multipurpose authorities responsible for welfare services like child care, compulsory schooling, and primary health care. The regional governments have more limited tasks, such as regional transportation, and employ 2% of the Norwegian work force.

Local and regional elections are held concurrently every fourth year in September. Norwegian citizens aged 18 or older by the end of the election year, and non-citizens with three years of consecutive residency, are eligible to vote. Voter registration is automatic, and individuals receive a letter in the mail about a month before the elections informing them of their rights and the closest polling place (Ferwerda, Finseraas and Bergh, 2020).

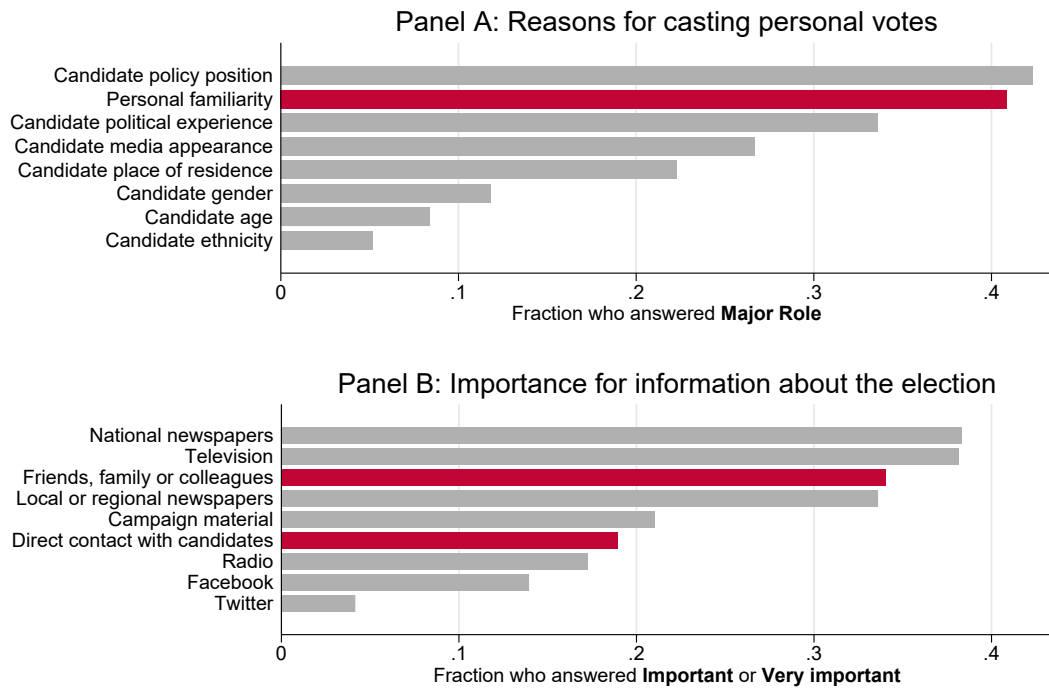
Local elections are decided by “flexible list systems” where both voters and parties affect candidate selection. Voters choose a party list and may opt to express preferences

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<sup>5</sup>Several studies – from Norway (e.g., Fiva, Halse and Smith, 2021) and other countries (see Górecki, Bartnicki and Alimowski, 2022, for a recent review) – have documented that candidates tend to receive more votes in their hometowns. Key (1949) famously refers to this as “friends and neighbors” voting.

<sup>6</sup>The purpose of this survey was to get a better understanding of the political behavior of immigrants. 18,181 people were invited to participate (12,856 with immigrant background; 5,325 without immigrant background). The response rate was 32% among individuals with immigrant background and 39% for individuals without immigrant background (<https://www.ssb.no/valg/artikler-og-publikasjoner/velgerundersokelsen-2015>).

Figure 1: Survey evidence on voting decisions



Notes: Panel A presents survey evidence of voters' reasons for casting personal votes. Reported are the fraction of survey respondents answering that they cast a personal vote because the reason given in the legend played a 'major role'. Alternative responses are 'don't know', 'no role', and 'some role'. Data from the 2015 Local Election Survey (Lokalvalgsundersøkelsen) ( $n=1,190$ ). The analysis is restricted to the 619 respondents who report that they cast a personal vote. Panel B presents survey evidence showing the importance of various factors for getting information about the election. Reported are the fraction of survey respondents answering 'important' or 'very important'. The alternative responses are 'not important', 'of little importance', and 'of some importance'. Data from the 2015 Election Survey (Velgerundersøkelsen) ( $n=6,275$ ).

for individual candidates by casting personal votes. Parties affect candidate selection by granting some candidates, listed on the top of the ballot in bold face, a “head start”. The advantage is so large that other candidates almost never receive enough personal votes to overtake a candidate with a head start (see Appendix C).

In Norway, local councilors typically hold other jobs concurrently. However, mayors (elected by the councilors) have full-time well-paid jobs that also serve as stepping stones to national politics (Cirone, Cox and Fiva, 2021).

### ***3.2 Candidate-level data combined with administrative voter turnout data***

Our candidate-level data set stem from Fiva, Sørensen and Vøllo (2021) and cover the universe of candidates running for local and regional office in the 2015 and 2019 elections. These data were originally collected by Fiva and Røhr (2018) for a study of the incumbency advantages in party-list systems, and include election outcomes, along with comprehensive background information, for every candidate. We restrict our analysis to those running for one of the nine main parties that dominate Norwegian politics.<sup>7</sup> 90% of these candidates run only for local office, 8% run for local and regional office, and 2% run for regional office only. We focus on candidates running for the local office only (92,767 candidate-year observations).

We use administrative registers to construct a balanced panel of 1,400,562 voters in the 2015 and 2019 elections, constituting about 34% of the Norwegian vote-eligible population. Our main outcome of interest, turnout, is collected from the *Electronic Election Administration System* which was implemented by 27 out of 428 municipalities in 2015. In these districts, voters were electronically registered upon their arrival at the polling stations, forming the basis of our data. We excluded two municipalities due to a reform which altered their borders between 2015 and 2019. While candidacies may well affect not just whether, but also for whom, people voted, we lack data on this and so

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<sup>7</sup>Ordered along the left-right dimensions, the nine main parties are: Red Party, Socialist Left Party, Labour Party, Center Party, Green Party, Liberal Party, Christian Democrats, Conservative Party, and Progress Party. The non-main parties include party-independent lists and minor parties that tend to get limited electoral support.



cannot study it.

Appendix Table A.1 shows that the 25 municipalities in our main sample – which includes the four largest cities in Norway – have a higher share of immigrants and somewhat lower voter turnout (about 58%).<sup>8</sup> The 2015 data have been previously used by Ferwerda, Finseraas and Bergh (2020), who study how immigrants’ early access to political institutions affects turnout in subsequent elections, and Bratsberg et al. (2021), who study how refugees’ initial neighborhood affects their future political participation. Geys and Sørensen (2022) use 2013-2019 panel data to study how public sector employment affects voter turnout.

Norway is divided into approximately 14,000 “basic statistical units” (BSU’s), which are nested within electoral districts (municipalities). These units vary in size, from just a few city blocks to several square kilometers in rural areas. Each BSU is constructed to cover homogenous areas in terms of demography, nature and infrastructure. An illustrative map of BSU’s in Oslo (the capital) is shown in Appendix Figure A.2.

Our administrative data comprise information obtained from the National Population Register. This includes the BSU in which each voter and candidate resides, along with unique IDs for family relations and immigration status for the entire Norwegian population. We incorporate a comprehensive distance matrix that covers the fastest driving distances (in kilometers) between all BSUs in the country (Sand et al., 2022). Additionally, we possess information on income, employment and occupation, which originate from tax records and official payroll reports that every Norwegian firm is required to file on a monthly basis. Further details about sample construction are discussed in Appendix B.

### **3.3 *Social networks***

We consider three types of social networks—families, co-workers, and immigrant communities. We face a trade-off in choosing how broad the network definitions should be;

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<sup>8</sup>Appendix Figure A.1 illustrates our sample using maps of Norway.

a broad definition is useful for statistical precision but the network ties are probably weaker. A narrow definition may have lower statistical precision but the network ties are probably stronger. For each of these three types of network, we therefore create one narrow and one broad category, with the latter subsuming the former. All social networks are assumed to be static and defined as they exist in 2015.<sup>9</sup> This section provides a brief description of each network (see Appendix B for details).

## Families

Political candidates are matched to family members in *close family networks*, defined as any parent, sibling or child, or in *extended family networks*, which also include grandparents, aunts, uncles, cousins, nieces, nephews, grandsons, and granddaughters. We cannot accommodate spouses or co-habitants, as we are specifically looking for cases of geographic variation between voters and politicians.

On average, a close (extended) family network has five (fifteen) members (Appendix Table A.2; Appendix Figure A.3). Among voters and politicians who belong to the same close family network and live in the same municipality, 23% reside within the same BSU (Appendix Figure A.4) (presumably many belong to the same household).

## Co-workers

As mentioned above, most candidates also hold regular jobs outside of politics. In a study using Swedish data, Aggeborn and Andersson (2022) find that workplace networks matter for individuals' decision to run for office. We match candidates to their co-workers using payroll reports from Norwegian employers (*A-melding*), restricting our sample to small and medium establishments, thereby excluding "super" firms where social connections are likely to be weaker. Even with this restriction, we retain over 97% of registered establishments (63% of employees). Co-workers are defined at either the broader establishment or the narrower establishment-age group (younger than 35, 35-50,

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<sup>9</sup>Violations of this assumption mean that some ties between candidates and people in their networks may no longer exist (e.g, a person switching jobs). In general, this should weaken any results we find.

over 50) level. We believe the latter to be a plausible proxy for factions within workplaces but also consider splits by firm size in the appendix. Each co-worker network contains around three (six) voters on average at the establishment-age group (establishment) level (Appendix Table A.2).

## Immigrants

We define first-generation immigrants as people born outside of Scandinavia to non-Scandinavian parents.<sup>10</sup> The five largest immigrant groups in our voters sample are from Poland (10.5%), Pakistan (6.0%), Somalia (5.2%), Iraq (5.2%) and Iran (3.9%). Among political candidates in 2015, the top five groups were from Germany (10.3%), Iran (5.6%), the Netherlands (4.8%), Poland (4.1%) and Bosnia-Herzegovina (4.0%). Our data may not enable us to explicitly observe the common platforms where immigrants interact. As a reasonable proxy for these individuals' true social networks, we pair candidates and voters who share the same country of birth and held the same profession in 2015.

To classify occupations, we use the standard four-level classification of Norwegian occupations (*STYRK-08*). We use three-digit occupation codes (e.g., “231 University and higher education teachers”) to define the narrow category, and two-digit codes (e.g., “23 Teaching professionals”) to define the broad category.<sup>11</sup> The three most common three-digit occupations among immigrant voters are *Domestic, hotel and office cleaners and helpers* (8.6%), *Personal care workers in health services* (8.2%) and *Shop salespersons* (5.2%). On average, there are 14 (29) voters per network using the three-digit (two-digit) definition (Appendix Table A.2; Appendix Figure A.3). Compared to politicians in the other network types, immigrant candidates tend to be more educated, but have less political experience and are less likely to be granted a “head start” by their party (Appendix Table A.3).

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<sup>10</sup>We disregard Swedish and Danish immigrants, who are culturally and historically similar to native Norwegians.

<sup>11</sup>The fraction of immigrants with politicians in their network is 40% and 53%, for three-digit or two-digit occupation codes, respectively. We do not define immigrant networks at the birthcountry level because then almost all immigrants (98.47%) have at least one politician in their network. We explore this further in Section 7.

## 4. Empirical specification

### 4.1 *Baseline model*

To study voter mobilization in networks, we estimate the following linear probability model:

$$Turnout_{ibt} = \alpha_{ib} + \lambda_t + \beta AnyDistrict_{it} + \gamma SameDistrict_{it} + \varepsilon_{ibt}. \quad (1)$$

$Turnout_{ibt}$  is an indicator variable turned on if individual  $i$ , residing in BSU  $b$ , at time  $t$  turns out to vote.  $AnyDistrict_{it}$  is an indicator variable turned on if  $i$  has a network member running for office at time  $t$ .  $SameDistrict_{it}$  is an indicator variable turned on if  $i$  has a network member running for office in  $i$ 's election district at time  $t$ .<sup>12</sup>  $\beta$  captures any network-wide effect on members' propensity to turn out (that does not depend on co-residence), while  $\gamma$  captures the additional effect of co-residence. We expect district boundaries to affect the propagation of mobilization within networks, i.e.,  $\gamma > 0$ .

By including individual-BSU fixed effects ( $\alpha_{ib}$ ) in Equation (1), we ensure that inference is drawn from individuals who do not move across BSUs but do experience a change in their social network over time (i.e. a network member entering or exiting politics). We also include time fixed effect ( $\lambda_t$ ) and allow for arbitrary correlation within BSUs ( $n = 3,705$ ) by clustering the error term  $\varepsilon_{it}$  at this level.

### 4.2 *The discontinuity at the district boundary*

The baseline model (Equation (1)) distinguishes between candidates inside and outside the focal voter's district. A natural extension is to use the district boundary explicitly in our research design. Specifically, we measure the fastest driving distance in kilometers between the BSU of the candidate and the BSU of the network member (voter).<sup>13</sup> We

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<sup>12</sup>Candidacy is coded as 1 regardless of the number of connected politicians. Among nation-wide networks with at least one candidate, 94% (close families), 87% (age-establishment co-workers) and 44% (3-digit immigrants) are single-candidate networks.

<sup>13</sup>If a voter has multiple network members running for office inside the district boundary, we use distance to the geographically closest within-network candidate. If a voter has no network members

expect the mobilizational impulse to fall in distance within districts and to exhibit a sharp drop-off when the network crosses the candidate’s district boundary.

To fix ideas, consider the co-worker networks illustrated in Figure 2. At one extreme, candidate 1’s co-workers all reside in the same municipality (Oslo). At another extreme, all of candidate 3’s co-workers (in this case, just one person) reside outside the candidate’s home district. In-between, about half of candidate 2’s co-workers are in the same district. Our empirical design exploits this distributional feature by recognizing that politicians have discontinuous incentives to mobilize voters within and outside their own electoral districts. In Figure 2, candidates 1 and 2 may improve their election outcomes by mobilizing some or all of their connected voters. For candidate 3, however, we would expect the mobilization incentive to be negligible.

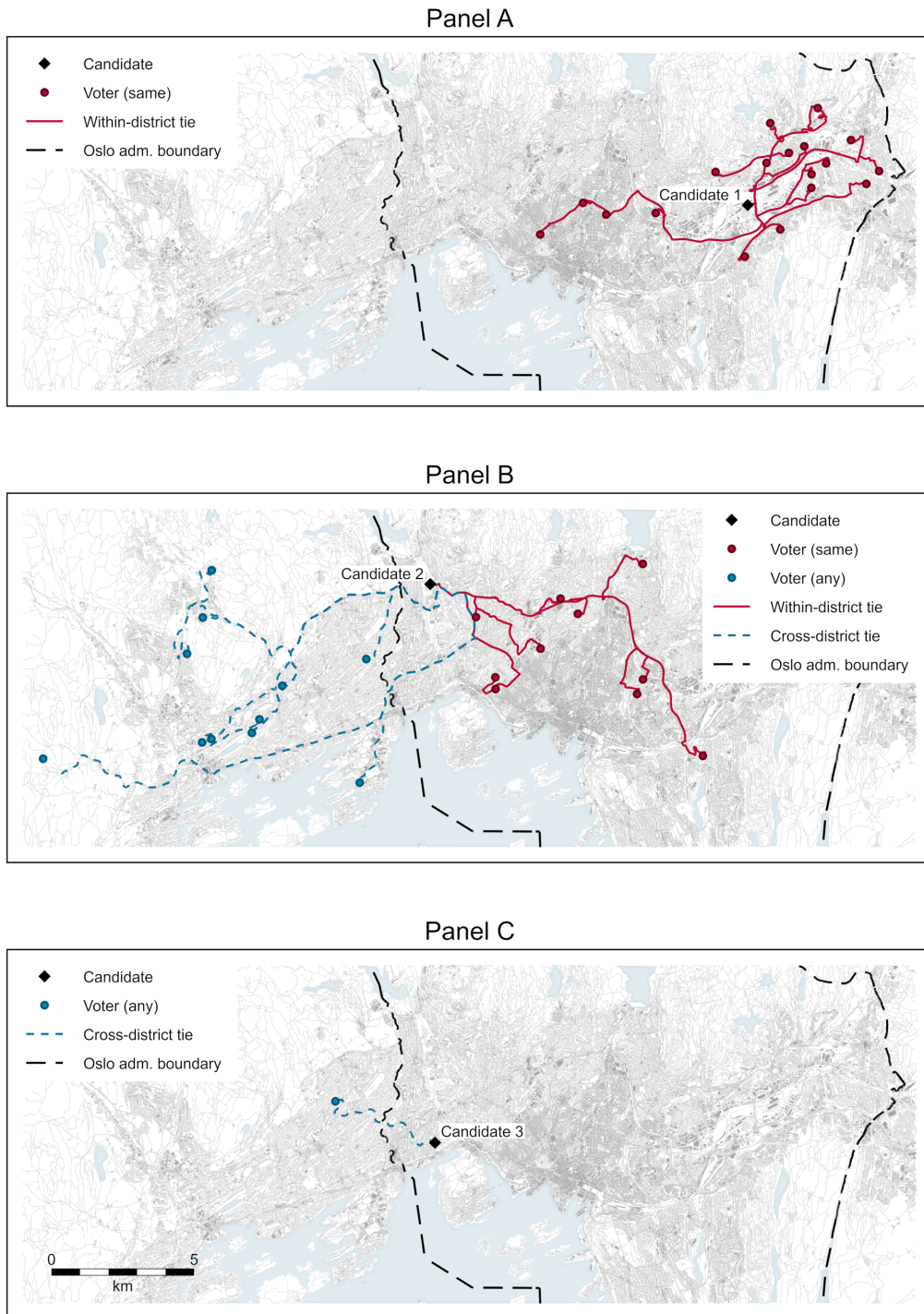
Our identification strategy is related to the geographic regression discontinuity design, where a geographic or administrative boundary splits units into treatment and control (Keele and Titiunik, 2015). Examples include Black (1999), who leveraged school district boundaries to estimate parents’ willingness to pay for good schools, and Huber and Arceneaux (2007), who compared same-state voters in different media markets to study the effects of advertising. In geographic regression discontinuity designs, units *equally close to the boundary* but on opposite sides of it are taken as valid counterfactuals for each other. We consider voters who are *equally close to the politician network member*, but on opposite sides of district boundaries, as valid counterfactuals for each other (after netting out  $\alpha_{ib}$  and  $\lambda_t$ ).<sup>14</sup>

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running for office inside the district boundary, we use distance to the geographically closest network member outside the district.

<sup>14</sup>Because networks are assumed to be static and defined as they exist in 2015, any time-invariant factors that potentially change at the border (such as the probability to belong to a particular network) are netted out by  $\alpha_{ib}$ . The only remaining inferential threat would be time-varying characteristics changing at the border which correlate with the treatment. We consider this unlikely but discuss the possibility in Section 5.4.

Figure 2: Network Appearance



*Notes: The figure shows the geospatial distribution of voters and politicians in three co-worker networks in our data (estbl. level). Black diamonds indicate the geographic locations of politicians, while red (blue) circles indicate the locations of voters in the same (different) district(s). Solid (dashed) lines illustrate the fastest driving route between politicians and each connected voter when both reside in the same (different) district(s). In this illustrative example, the within-district locations of each politician is randomized to preserve their anonymity, while we use the actual basic statistical unit of connected voters. Underlying map data: ©OpenStreetMap contributors. Data available under the Open Database License.*

## 5. Results

### 5.1 *The mobilization boost*

Table 1 provides estimation results from the baseline model (Equation (1)) for different definitions of the family (columns 1-2), co-worker (columns 3-4) and immigrant networks (columns 5-6).<sup>15</sup>

Column (1) shows that voters with a close family member running in *another* district from the one they live in increase their turnout rate by about 0.6 percentage points (from a baseline turnout level of 66.6 percent). This effect, which is statistically significant, might be driven by increased civic pride, belief in the legitimacy of the political process, and feelings of efficacy that affect family members regardless of where they reside.

Table 1: Results - Baseline Networks Analyses

	Family		Co-workers		Immigrants	
	(1) Close	(2) Extended	(3) Age-estbl.	(4) Estbl.	(5) 3-digit	(6) 2-digit
No candidate in network	ref.	ref.	ref.	ref.	ref.	ref.
Any District	0.006 (0.003)	0.002 (0.002)	-0.001 (0.003)	-0.003 (0.002)	-0.004 (0.004)	-0.004 (0.004)
Same District	0.026 (0.005)	0.015 (0.004)	0.014 (0.005)	0.010 (0.004)	0.045 (0.012)	0.036 (0.010)
Observations	2,801,126	2,801,126	1,087,562	1,087,562	239,810	239,810
Clusters	3,733	3,733	3,702	3,702	3,535	3,535
Mean turnout (%)	66.56	66.56	66.50	66.50	41.19	41.19

*Notes: Each column represents a separate regression based on Equation (1), where the dependent variable is turnout for voter  $i$  in BSU  $b$  at time  $t$ . The sample is trimmed in columns (3)-(4) and (5)-(6) to only consists of individuals who belong to a network under the indicated category. Not reported, but also included in all models, are individual-BSU fixed effects and year fixed effects. Standard errors are clustered on the basic statistical unit level and reported in parenthesis.*

<sup>15</sup>Clustering at the election district level ( $n = 25$ ) gives similar standard errors as in Table 1. As an alternative way to assess our statistical inference, we re-estimate our baseline model after randomizing who is running for office (keeping the social networks constant). This placebo exercise, which we repeat 100 times for each type of network, yields a distribution of point estimates which are centered at zero (Appendix Figure A.5). Importantly, the actual point estimates from Table 1 lie well outside the placebo distributions for all network types.

The mobilization effect is, however, about five times as large for family members co-residing in the municipality where the candidate runs for office. We estimate a mobilizational boost of an additional 2.6 percentage points. The cross-district drop-off in the mobilizational impact of having a family member as a candidate—from 3.2 to 0.6 percentage points—reflects the fact that the candidate has a larger incentive to lobby family members who can vote for them, as we hypothesized in Section 4.1 ( $\gamma > 0$ ).

Relative to a baseline turnout rate at 66.6%, a mobilization boost of 3.2 percentage points implies that 9% of non-voting close family members are mobilized to vote by a new within-family candidacy. This effect is particularly significant, given that (i) our outcome variable specifically focuses on turnout and not on party shifts or personal votes, (ii) multiple networks exist, and (iii) our observations of these networks are not perfect. We discuss challenges to this interpretation in Section 5.4.

When using the broader family network (column (2)), we find that both the out-of-district boost and the additional within-district boost are smaller. This is as expected since ties between close family members are stronger than among extended family members.<sup>16</sup>

Columns (3)-(6) show that social networks are also important for turnout among co-workers and co-occupational immigrant populations. For both networks, our estimates are somewhat larger for the narrow (age-establishment) than the broad (establishment) definitions of the network. We estimate a mobilizational boost of 1.4 percentage points for co-workers from the same age group (from a baseline turnout of 66.5%).<sup>17</sup>

For co-occupational immigrants, we estimate the largest mobilizational boost (4.5 percentage points from a baseline of 41.2%); we comment on why this is larger than in other networks in Section 6.<sup>18</sup> There are no statistically significant effects of having net-

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<sup>16</sup>Appendix Table A.4 show that the strongest mobilizational boost come from children and parents running for office. All family categories display positive point estimates, except cousins, where the estimate is negative but statistically indistinguishable from zero.

<sup>17</sup>The co-worker network effects decline with network size (Appendix Table A.5) suggesting that social ties are stronger in smaller workplaces.

<sup>18</sup>Appendix Table A.6 shows that the within-district mobilizational boost is primarily driven by co-occupational immigrant networks where members have ties to Africa and Asia.



work members outside the district boundary for co-workers or co-occupational immigrant networks.

In Appendix D, we estimate heterogeneous mobilization effects depending on candidates' electoral viability. We find that having a network member running in *another* district boosts a voter's turnout negligibly, irrespective of candidate viability. The within-district mobilization effect is, however, increasing in candidate viability. For example, we estimate that a strong candidate in a co-worker-age-group increases network members' probability of voting by six percentage points, while a hopeless candidate in the same co-worker-age-group only increases network members' turnout rate by one percentage point. The relationships between candidate viability and voter mobilization are similar, albeit more muted, for family and co-occupational immigrant networks.

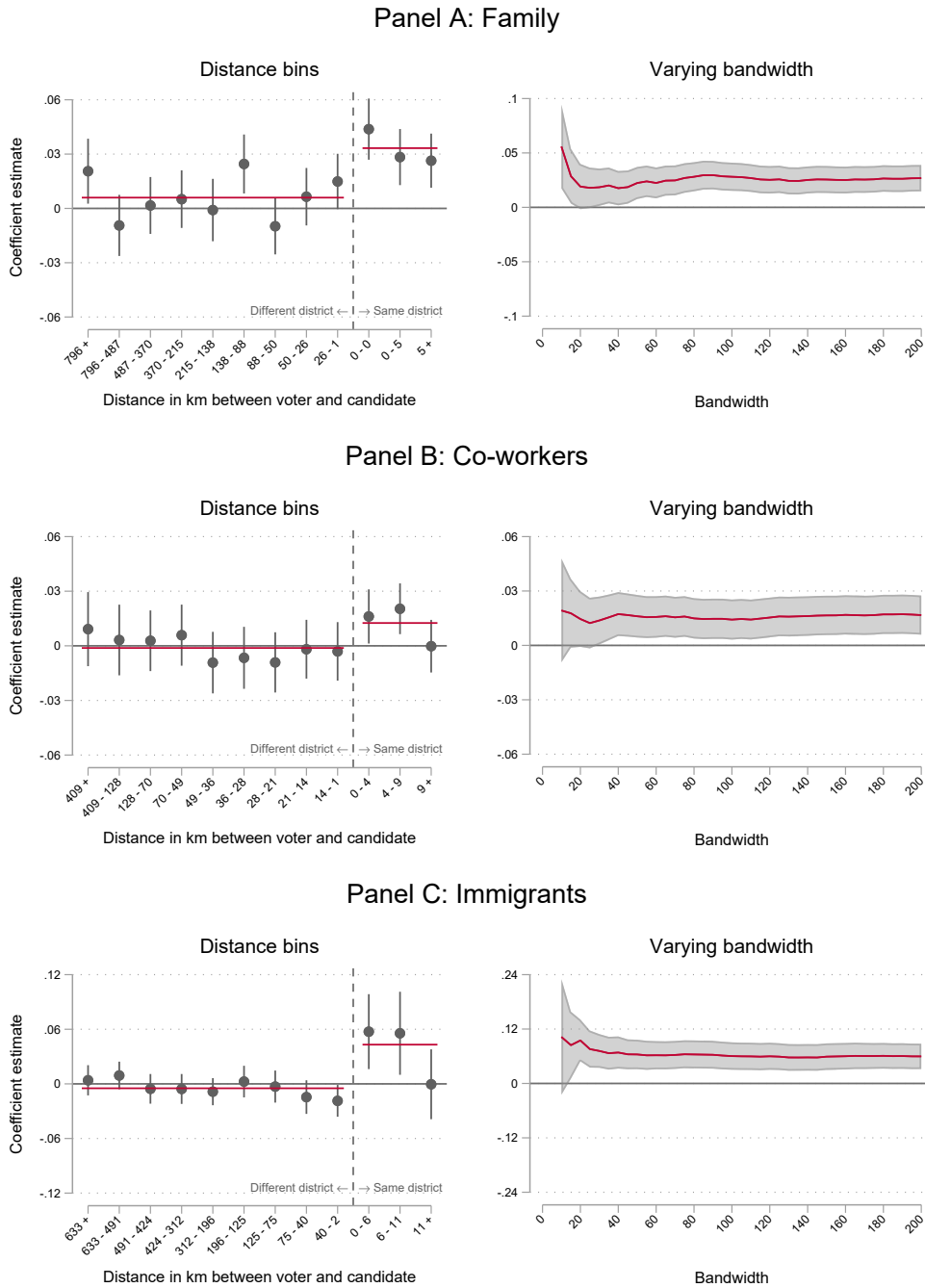
The effect that within-network candidacies have on turnout can be parsed into two components: new turnout for the candidate's list; and new turnout for other lists. Since political parties will seek to nominate candidates who will increase their vote shares, most of the turnout effect we find should be due to candidates mobilizing new voters for their own party rather than due to their candidacy "back-firing" and mobilizing new voters for other parties.

## 5.2 *The border drop-off*

Panel A of Figure 3 shows how the mobilizational impact varies with distance between the voter and the candidate in his/her close family member network (the bins on each side include the same number of observations). Consider first the left side of the threshold in the plot to the left in Panel A, which captures effects for candidates living in a different district as the voter (and the horizontal red line correspond to the estimate of  $\beta$  reported in Column (1) of Panel A in Table 1). There is no indication that distance matters for turnout; even if network members reside within walking distance of each other (but in different districts) the confidence intervals overlap with zero.

Estimates to the right of the threshold capture effects for candidates living in the same

Figure 3: Effects over Distance and across District Boundaries



Notes: This figure displays how the mobilizational impact depends on distance in kilometers between voters' and candidates' basic statistical units (BSU). In each panel, the left plot reports coefficient estimates and 95 percent confidence intervals for observations belonging in each distance bin. The red lines denote the average mobilization impacts on the left and right side of the threshold. The number of observations per bin are constant on each side. The right plots in each panel reports our main coefficient estimates from Equation (1) but excludes from identification all observations whose distance falls outside the indicated bandwidth (i.e., the red line shows the difference between the lines in Panel A as we zoom closer to the threshold). If a person has multiple candidates in his/her network we use the geographically closest candidate to measure distance. For all networks, we use the narrow definition ('close', 'age-establishment', and '3-digit'). A small fraction of the sample is omitted from each analysis due to missing distance. Standard errors are clustered on the BSU level.

district as the voter. We find that estimates are largest (above 4 percentage points) when network members reside in the same geographical unit but remain around 2.5 percentage points further away. The difference between the two horizontal red lines in Figure 3 corresponds to the estimated  $\gamma$  from Column (1) of Panel A in Table 1.

In the plot to the right in Panel A, we investigate how the average border effect (i.e., the difference between the red lines in the left-most plot) varies as we zoom closer to the threshold. As we move to the left, only individuals whose network distance is smaller are used for identification. We find that the estimated  $\gamma$  is stable across bandwidths but increases slightly when the bandwidth becomes very small, in line with the results from the left-most plot. We believe this mitigates concerns about endogenous political entry; if candidates were chosen based on unobserved trends in the political engagement of their social networks, then we would have seen “mobilization” both inside and just outside district borders.

Panel B and C of Figure 3 performs an identical exercise for the narrow definition of co-worker and co-occupational immigrant networks. The results are similar to those for families but with less statistical precision to the right of the threshold (because of network and sample size).<sup>19</sup>

### 5.3 *Two-step network effects*

In Table 2, we investigate whether mobilized voters in politicians’ social networks go on to mobilize *additional* voters in their own social networks. Column (1) shows that turnout rates go up by 0.6 percentage points among the close family members of a person who has a close co-worker running for office when they all reside in the same district. Column (2) shows corresponding estimates when the mobilization impulse goes in the opposite direction, from family to co-worker networks. In this specification the two-step mobilization estimate is also positive (0.3 percentage points) but not statistically significant. In column (3), we pool the two-step mobilization effects to improve statistical

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<sup>19</sup>Appendix Figure A.6 provides corresponding results using the broad network definitions.

precision. We find a statistically significant pooled effect of 0.5 percentage points. If the typical family member was connected to at least 25 persons as strongly as they were to their close co-workers, then the overall turnout boost via secondary mobilization would exceed the primary boost by a factor of four, in line with existing studies (e.g., Fowler, 2005; Bond et al., 2012).

Table 2: Mobilization Effects in Two-step Networks

	Co-workers and families			Immigrants and families		
	(1) Candidate → co-wkr. → family	(2) Candidate → fam. → co-worker	(3) Pooled	(4) Candidate → imm. → family	(5) Candidate → fam. → immigrant	(6) Pooled
No candidate in network	ref.	ref.	ref.	ref.	ref.	ref.
Any District	0.001 (0.002)	-0.003 (0.002)	-0.001 (0.001)	-0.002 (0.006)	0.006 (0.004)	0.003 (0.004)
Same District	0.006 (0.004)	0.003 (0.004)	0.005 (0.003)	0.015 (0.013)	0.018 (0.011)	0.019 (0.008)
Observations	2,801,126	2,801,126	2,801,126	2,801,126	2,801,126	2,801,126
Clusters	3,733	3,733	3,733	3,733	3,733	3,733
Mean turnout (%)	66.56	66.56	66.56	66.56	66.56	66.56

*Notes: Each column represents a separate regression based on a variant of Equation (1) that estimates mobilization effects from multiple networks in the same model. The dependent variable is turnout for voter  $i$  in BSU  $b$  at time  $t$ . The variables of interest indicate if the voter is two steps away from a candidate (e.g., the politician is a co-worker of a close family member, as in column (1)). All three network members (voter, mediator, candidate) must reside in the same district in order for Same District to indicate. First-order effects from the involved networks are also included in all models. Columns (3) and (6) consider pooled models where the mobilization impulse is allowed to be mediated by either of the networks in the preceding columns. All network categories use the narrow definitions (close, age-estbl., 3-digits). Not reported, but also included in all models, are individual-BSU fixed effects and year fixed effects. Standard errors are clustered on the basic statistical unit level and reported in parenthesis.*

Models 4-6 provide similar analyses of mobilization propagating from narrow immigrant-occupation to close family networks, vice versa, and pooling the two directions. As can be seen, we find a statistically significant pooled effect of 1.9 percentage points.

In both of these analyses, we again find a border dropoff. There is no evidence of two-step mobilization effects when the candidate resides in a different district from either their primary or secondary network member.

## 5.4 *Internal validity*

It is widely recognized that “in ... observational studies, the self-selection of people into peer groups can make the measurement of peer effects extremely difficult” (Sacerdote, 2014, p. 235). For example, Christakis and Fowler’s (2007; 2008) finding that health outcomes (obesity, quitting smoking) propagate through networks of friends has been challenged by Cohen-Cole and Fletcher (2008), who show that even non-transmissible traits appear to propagate through friends’ networks, using Christakis and Fowler’s method.

Our research design mitigates such concerns. First, we study *static* networks. Thus, several threats arising from endogenous change in networks do not afflict our analysis. Second, individuals do not choose their families or immigrant groups; and their choice of workplace and occupation is more constrained than their choice of friends. Families do share nature (genes) and nurture (upbringing), and so do immigrant groups (genes, culture). But our individual-BSU fixed effects ( $\alpha_{ib}$  in Equation (1)) control for the direct effect on turnout of these factors.

What about local variables that boost turnout among all network members residing in the same neighborhood? We can address that concern by replacing our year fixed effects ( $\lambda_t$  in Equation (1)) with BSU-year fixed effects ( $\lambda_{bt}$ ).<sup>20</sup> Appendix Table A.7 shows that this leaves our results mostly unaltered.

Finally, the internal validity of our analysis could be compromised if parties allocate list positions to people whose networks are becoming more politically engaged over time. However, if candidates’ networks were trending upward in political engagement, then we should see “mobilization” both inside and just outside district borders, contrary to what we actually find.

In Appendix Figures A.7-A.10, we consider four time-varying outcomes: income (measured in constant USD 1000s), education (high or low), marital status (married or not), and charity donations (yes or no). Using the approach depicted in Figure 3, we estimate

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<sup>20</sup>This follows the approach to controlling for environmental confounding via area fixed effects (Cohen-Cole and Fletcher, 2008).

border effects for these variables. The results of these placebo checks reveal no consistent treatment effects, thereby further reinforcing the validity of our empirical strategy.

### 5.5 *External validity*

Because candidates choose to seek list spots and parties choose to accept them, our results do not provide evidence that, were one to randomly assign list spots to the general population, similar mobilizational impacts could be expected. If parties award list spots to candidates they believe can mobilize more latent party supporters, then the within-network mobilizational boosts we identify will reflect the largest mobilizational boosts the party can discover among its supporters. Thus, our results may provide evidence on the upper tail of the mobilizational impacts that one could expect.<sup>21</sup>

Would our Norwegian results generalize to other settings? The mechanism we argue produces the border drop-off is a combination of (M1) strong strategic mobilization effects; and (M2) weak non-strategic effects.

We think strategic mobilization effects are likely to be strong in any electoral system that (i) divides the electorate into geographically defined districts; (ii) converts votes into seats exclusively within those districts; and (iii) does not make voting mandatory. Re (ii), we would expect border drop-offs in systems using upper tiers to be less sharp, since then parties would clearly wish their candidates to mobilize more broadly than just their own district. Even in system in which all voters are converted to seats within districts, candidates' incentives to mobilize decline as elections become less close and consequential, so that the expected border drop-off would also decline. Re (iii), we would expect negligible candidacy effects—and thus negligible border drop-offs—where voting is mandatory.

It is harder to generalize about when non-strategic effects will be weak or, alterna-

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<sup>21</sup>Of course, most parties place many people in unwinnable positions on their lists, and many of these may be selected for their loyalty or past service to the party, rather than their mobilizational ability. Moreover, if we were able to directly observe network connections, the mobilization boosts in our co-worker and immigrant networks (which are both proxies that may contain some rather weak ties) might be larger.

tively, when they will be strong enough to wash out border effects. It is likely that the nature of societal organization would be important in this regard.

## 6. Comparing immigrant and native candidates

We have seen, in Section 5, that the immigrant co-occupational boost is substantially larger than the family and workplace boosts. One plausible reason for this is that immigrants have less information and lower baseline turnout rates than natives. For example, in a canvassing experiment in France, Pons and Liegey (2018) find larger impacts of visits on immigrants than the native population, and present evidence suggesting that immigrants' lower baseline level of information about the elections drive the heterogeneous impact.

Another plausible reason for the large size of the immigrant co-occupational boost is a “Jackie and Jill effect” (Anzia and Berry, 2011). To explain, suppose that party gatekeepers accept immigrant candidates only if they believe those candidates can mobilize enough new immigrant voters to compensate for the expected vote loss among natives. In this case, immigrant candidates should generate larger turnout boosts in their social networks than native Norwegians; and that turnout gap should be larger in parties whose voters harbor greater anti-immigrant biases.

We explore this first by estimating family turnout effects separately for immigrant and native families. Columns (1) and (2) in Table 3 reproduce the results from the first two columns in Table 1, except that the sample is restricted to voters who were born in Norway. Columns (3) and (4) explicitly considers immigrant families. Immigrants generate much larger turnout increases among their family members than do native candidates.

Moreover, Appendix Figure A.11 documents that immigrant candidates' mobilizational boost grows progressively stronger the less favorable party supporters are toward increased immigrant participation.<sup>22</sup> This aligns with the notion that party gatekeepers

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<sup>22</sup>Appendix Figure A.12 shows that our measure of attitudes toward immigrants correlate with party bloc (left-right) and the proportion of immigrant candidates on party lists.

Table 3: Native versus Immigrant Families

	Natives		Immigrants	
	(1) Close	(2) Extended	(3) Close	(4) Extended
No candidate in network	ref.	ref.	ref.	ref.
Any District	0.006 (0.003)	0.002 (0.002)	0.023 (0.028)	0.027 (0.026)
Same District	0.021 (0.005)	0.012 (0.004)	0.139 (0.043)	0.127 (0.041)
Observations	2,301,710	2,301,710	408,566	408,566
Clusters	3,723	3,723	3,601	3,601
Mean turnout (%)	71.59	71.59	39.39	39.39

*Notes: Each column represents a separate regression based on Equation (1), where the dependent variable is turnout for voter  $i$  in BSU  $b$  at time  $t$ . The sample in columns (1) and (2) consist of voters who were born in Norway, while the sample in columns (3) and (4) considers all first-generation immigrants (as defined in section 3.3). Not reported, but also included in all models, are individual-BSU fixed effects and year fixed effects. Standard errors are clustered on the basic statistical unit level and reported in parenthesis.*

strategically allocate list spots to immigrants whom they believe will induce a compensatory increase in voter turnout within the immigrant community (proxied here by family members).

## 7. The political consequences of border drop-offs

Many scholars have noted that groups whose members are distributed inefficiently across electoral districts may have difficulty converting their votes into seats (e.g., Rodden, 2019; Taylor and Johnston, 1979). Section 5.2 documented one mechanism that worsens votes-to-seats conversion: candidates' inability to use their social networks to mobilize people who can actually vote for them.

In Appendix Table A.8, we provide evidence on the average *electoral efficiency* of candidates' networks that is, the average share of network members who reside in the same district. We find that electoral efficiencies vary widely across different networks, suggesting that groups may have mobilization (dis)advantages based simply on the distribution



of their members relative to district boundaries. In the rest of this section, we consider whether network efficiency helps to explain where immigrants become candidates.

In Table 4 we present regression results where the dependent variable is the share (in percent) of a group’s total candidacies at time  $t$  (across all municipalities) that occurred in municipality  $m$ . We control for birthcountry fixed effects and either a linear, quadratic, cubic or quartic polynomial of the share of each group’s population in each municipality. The regressor of interest is the maximum available birthcountry-occupation efficiency. In other words, in municipality  $m$ , we examine each occupation group from each immigrant group, compute the birthcountry-occupation electoral efficiency, and record the maximum (*maximum efficiency*).<sup>23</sup> Unlike in Section 5 (where we needed to observe turnout), these analyses use the full population of immigrants.

Table 4: Effect of Maximum Efficiency on Candidacy

	(1)	(2)	(3)	(4)	(5)
Maximum efficiency (std.)	0.365 (0.056)	0.191 (0.052)	0.174 (0.043)	0.123 (0.042)	0.119 (0.042)
Population share polynomial	-	Linear	Quadratic	Cubic	Quartic
Observations	22,321	22,321	22,321	22,321	22,321
Clusters	47	47	47	47	47
Mean dependent variable	0.40 %	0.40 %	0.40 %	0.40 %	0.40 %

*Notes: Each column represents a separate regression of the share (in percent) of a group’s total candidacies (across all municipalities) that occurred in municipality  $m$  on the maximum available birthcountry-occupation efficiency. The unit of observation is birthcountry-municipality-years. Occupations are defined at the 2-digit level. The sample is restricted to immigrant-occupation groups with ten or more individuals (per year) and countries with a (nationwide) population of more than 1000. Starting in column (2), we include a polynomial which controls for the share of each group’s population in each municipality. Country of birth fixed effects are included in all specifications. Standard errors are clustered on the birthcountry level and reported in parenthesis.*

We focus on the maximum efficiency because only about 1% of birthcountry groups have more than one candidate running in a given municipality. Thus, one would expect the most efficient occupational subgroup in each municipality to be the most likely to

<sup>23</sup>Appendix Table A.9 provide evidence that candidates were not systematically mobilizing their entire co-resident immigrant communities (as defined by birthcountry alone). They were, however, successfully mobilizing co-residents who shared both their birthcountry and occupation. This is why we focus our analyses on this level.

secure a list spot. For interpretive convenience, we standardize maximum efficiency to have mean zero and standard deviation one.

Flexibly controlling for the percent of the group’s population in each municipality and birthcountry fixed effects, we find that maximum efficiency is positively and significantly associated with candidacy. Substantively, increasing the maximum available efficiency by one standard deviation increases the expected share of candidacies by between 0.1 and 0.2 percentage points, when including population controls (columns (2)-(5)).<sup>24</sup> This corresponds to 25% – 50% of the mean of the dependent variable.

Our results resonate with Cruz, Labonne and Querubin’s (2017) finding that candidates for public office in the Philippines are disproportionately drawn from families with higher network centrality. Possible mechanisms include immigrants with more efficient occupational networks being more likely to seek candidacies; and parties seeking to list someone from a particular immigrant group preferring persons with more efficient birthcountry-occupation networks.

Of course, someone might make a good candidate by virtue of other networks they can mobilize—e.g., through their church or former university classmates. At this point, we have little ability to identify each candidate’s full portfolio of networks. So, occupational network efficiency may correlate with other networks’ efficiency. Future work will have to deal with this and other forms of omitted variable bias. That said, the correlation we report suggests that the first step toward converting a group’s votes into seats—converting its votes into candidacies—depends in a plausible way on how its members are distributed across relevant electoral districts (in this case, municipalities).

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<sup>24</sup>We exclude from the sample immigrant-occupation groups with less than ten individuals and countries with a (nationwide) population of less than 1000. Appendix Figure A.13 shows that these results are robust to a range of population restrictions.

## 8. Conclusion

In this paper, we exploit high-resolution administrative data from Norway to explore how electoral geography affects mobilization through social networks. For families, coworkers, and birthcountry-occupational groups, we show that the candidacy of a group member acts like a mobilizational impulse that propagates through the group’s network. The effects are substantial, corresponding to a 2-4 percentage point increase in turnout. Effects increase as the strength of social ties increase—for example, they are larger in smaller business establishments than in bigger ones. Effects also increase when candidates’ incentives to mobilize increase—in particular, viable candidates mobilize more voters than do hopeless ones.

The political parties appear to select immigrant candidates on the basis of their mobilizational ability. Immigrant candidates generate larger turnout boosts in their families than do natives; and this effect grows in proportion to anti-immigrant attitudes among the party’s members. Moreover, parties are more likely to select immigrants whose co-occupational networks are electorally more efficient (with more members residing within the potential candidate’s electoral district). While we cannot directly observe candidates’ mobilizational efforts, our results, as well as survey data, are consistent with candidates actively mobilizing their social networks and being selected for that ability.

The electoral impact of social networks is likely larger than our estimates suggest. First, within-network candidacies will plausibly affect not just turnout but also vote choice. Second, there are many primary networks beyond the three we can observe with our data. Third, secondary mobilization will magnify primary-network turnout effects (as previous work and our two-step analysis show).

More novel than the results described above, our work also illuminates how electoral district boundaries shape mobilizational impulses. Previous research has focused on local networks (e.g., spouses, neighbors) contained within single districts. The networks we study often spread beyond individual districts, allowing us to show that mobilization is

bound by borders. Within district borders, mobilizational impulses decline moderately with distance. However, the impulse falls off dramatically as soon as the social network crosses the candidate’s district boundary. To our knowledge, our paper is the first to provide quantitative assessments of such border effects.

The sharpness of the border drop-off, combined with the general importance of mobilization through social networks, suggest that electoral geography has more complex effects than previously thought. For example, formal models of gerrymandering typically take the parties’ objective to be sorting individuals with fixed partisan preferences (and turnout propensities) across districts to optimize how votes translate into seats from the party’s perspective (e.g., Owen and Grofman, 1988). Yet, to the extent that elections hinge on mobilizing supporters, the gerrymanderer’s objective should be to sort entire social networks efficiently across districts. More generally, the electoral success of any given group will depend not just on how its members are distributed geographically but also on the distribution of their social networks.

Our work also suggests a broader issue in network studies. Most businesses have “service areas,” some with fairly sharp borders (e.g., TV stations), others with fuzzy borders defined by travel times and competition. Any ad campaign seeking to orchestrate word-of-mouth support for a business would need to consider the overlap between their primary contacts’ social networks and their service area.

## **9. Supplementary Material**

The supplementary material for this article can be found at [TO BE INSERTED]

## **10. Data availability statement**

The data utilized in this study are sourced from Norwegian administrative registers and provided under loan from Statistics Norway, making them legally restricted from further

sharing. However, researchers interested in accessing these data can apply to Statistics Norway, using <https://www.ssb.no/en/data-til-forskning/utlan-av-data-til-forskere/soknad-offentlig-myndighet>. The statistical code used for our empirical analyses are deposited at the Harvard Dataverse: <https://doi.org/10.7910/DVN/KENHFH> (Cox, Fiva and King, 2024). Programs listed under section 1 of the file MASTER.do include the names of all files obtained from Statistics Norway.

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## **13. Competing interest**

None.

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