The Context of Voting: Does Neighborhood Ethnic Diversity Affect Turnout?

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With continuing immigration to Western countries, an important question concerns how these demographic changes impact natives' propensity to vote. In particular, the literature debates whether exposure to ethnic others in local contexts generates conflict that mobilizes individuals to vote (mobilization theory), diminishes social cohesion that in turn makes voters likely to withdraw from voting (marginalization theory), or does not impact turnout at all. This study is one of the first to investigate the question using individual-level longitudinal data, which adds substantially to the causal leverage of the analysis. In particular, we use a panel dataset with validated turnout data for 1.9 million Danish voters combined with detailed data regarding the ethnic composition of individuals' proximate residential neighborhood. The results suggest that increasing shares of immigrants does not affect natives' propensity to vote to any substantive extent, irrespective of how the size of the neighborhood is specified. This finding cannot be ascribed to lack of statistical power. Hereby, the study provides an important contribution to the existing knowledge regarding the democratic consequences of continuing immigration and increasing ethnic diversity.

How does exposure to ethnic others affect individuals' propensity to vote? Since voting is foundational to democratic societies, and high turnout rates across socioeconomic divisions are oftentimes considered indicators of strong democracies and healthy civil societies, the relevance of this question is self-evident. Yet, as ethnic diversity continues to increase in Western societies, and issues concerning immigration dominate political debates, the topic seems ever more important.

Reflecting the societal relevance of the issue, scholars have for decades debated the relationship between turnout and the demographic composition of individuals' residential context (e.g., Enos 2010, 2016; Fieldhouse and Cutts

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2008a, 2008b; Hill 1994; Hill and Leighley 1999; Key 1949; Leighley and Vedlitz 1999; Zimmer 1976). In particular, two opposing mechanisms have been suggested: Proponents of marginalization theory predict that exposure to ethnic others decreases natives' propensity to vote by diminishing social cohesion. This could lower democratic quality by generating biases in representation. Proponents of mobilization theory, on the other hand, expect that exposure to ethnic others enhances feelings of threat and interethnic competition, which increases individuals' propensity to vote in order to maximize the political power of their own ethnic group. Accordingly, though high turnout is generally considered positive, it could also be an indicator of greater tensions and divisions within civil society.

Empirical investigations have provided ambiguous results: Some find that individuals' propensity to vote is higher in areas with larger shares of ethnic others (Bhatti and Hansen forthcoming; Enos 2016; Giles and Buckner 1993; Key 1949; Zimmer 1976); others find the opposite (Barber and Imai 2014; Cho, Gimpel, and Dyck 2006; Hill and Leighley 1999); and still others find no relationship at all (Voss 1996; see Geys 2006 for a review). One reason for these inconsistent findings may be related to substantial methodological challenges. For one thing, unobservable factors may impact both individuals' choice of residence and their propensity to vote, which implies that correlations based on cross-sectional observational data can be biased estimates of the causal effects of contextual factors on turnout (Barber and Imai 2014). Moreover, determining the appropriate size of the context and measuring the demographic composition within this area is notoriously difficult (Fotheringham and Wong 1991; Wong et al. 2012; Wong 2009). And finally, individuals' self-reported electoral turnout is consistently found to be subject to social desirability bias, highlighting the need for better measures of individuals' turnout (Bernstein, Chadha, and Montjoy 2001; Karp and Brockington 2005).

This paper investigates the effect of exposure to ethnic others on natives' propensity to vote using panel data for about 1.9 million voters' turnout at two Danish elections (in 2009 and 2013, respectively). Considering the challenges that have limited previous research, the analyses have two substantial advantages: First, the longitudinal setup increases causal leverage considerably compared to cross-sectional designs. Second, high measurement validity is facilitated by data from the public registers providing objective reports on individuals' turnout (Elklit et al. 2000, 2005) and exceptionally fine-grained data on the ethnic composition of individuals' proximate neighborhood. Across various model specifications, we consistently find that exposure to non-Western immigrants does not to any substantive extent affect natives' likelihood of voting. In light of recent studies (Enos 2016; Giles and Buckner 1993) suggesting that in segregated areas individuals' propensity to vote increases with increasing levels of ethnic others, our findings are surprising and tentatively suggest that effects from highly segregated areas are exceptional compared to effects in areas in which ethnic lines are less conflictual. Thus, the study contributes to our knowledge on how immigration and increasing ethnic diversity is likely to affect a central aspect of democratic societies—voting propensities.

Ethnic Neighborhood Composition and Voting

Starting with Key (1949), the mobilization perspective has gained prominence within the literature on how individuals' voting behavior is affected by the ethnic composition of their neighborhood. Specifically, Key demonstrated that whites living in counties with higher shares of African Americans were more likely to vote than whites living in counties with relatively fewer African Americans. The pattern has been explained with reference to group threat theory, positing that individuals feel threatened when exposed to out-groups (Blalock 1967; Tajfel and Turner 1979). Following this line of thought, out-group exposure enhances individuals' wish to maximize the political power of their own ethnic group (Blalock 1967; Enos 2016). In fact, it has been argued that voting may reinforce in-group loyalties and ethnic conflict, because voting inherently involves opposing interests and confrontations regarding the distribution of resources (Uslaner and Brown 2005).

Recent nuances to the threat theory have been provided by studies suggesting that the effect of interethnic exposure depends on the salience of the ethnic distinctions (Enos 2010; Hopkins 2010, 2011). For instance, Hopkins (2010, 2011) shows that increasing ethnic diversity only produces anti-immigration attitudes when immigration is politicized by salient national rhetoric. Along the same lines, Enos (2016) suggests that the mobilization effect related to increasing neighborhood exposure may be particularly pronounced when the ethnic groups are socially very segregated rather than integrated, because the segregation indicates that group interests are also very different. Enos demonstrates that whites' turnout decreased when Chicago ghetto areas were demolished, presumably because of a diminished perception of threat, but it is possible that the effect is markedly different in areas that are less socially divided along ethnic lines.

Yet other scholars argue that ethnic differences do not by themselves serve to enhance turnout; rather, the effect is mediated by politicians' campaigning efforts (Cho, Gimpel, and Dyck 2006). Specifically, politicians who seek to mobilize voters along ethnic lines have an incentive to focus campaigns in areas with a large share of potential voters (in-group members), which in turn is likely to increase turnout among this group (Cho, Gimpel, and Dyck 2006). Consequently, individuals living in areas with large shares of out-group members are less likely to be targets of politicians' campaign activities and thus less likely to vote.

While a number of studies support mobilization theory by finding that higher shares of ethnic minorities correlate with higher turnout (Enos 2016; Key 1949; Zimmer 1976), other investigations find the opposite, namely that the propensity to vote is lower in ethnically diverse areas (Barber and Imai 2014; Cho, Gimpel, and Dyck 2006; Hill and Leighley 1999; see Geys 2006 for a review). In explaining this pattern, researchers often emphasize a process of withdrawal and marginalization (Barber and Imai 2014; Geys 2006). In particular, scholars argue that exposure to ethnic others decreases social connectedness and community cohesion, which in turn makes individuals withdraw from social and political participation (Geys 2006; Putnam 2007). The former part of this argument is

supported by studies from both Western Europe and the United States demonstrating that social trust is lower among individuals exposed to higher shares of people of foreign ethnic origin in their proximate neighborhood (Dinesen and Sønderskov 2015; Putnam 2007). The latter part dates back to arguments presented by Almond and Verba (1963), who argued that political participation in Britain and the United States is high as a result of a strong civil culture characterized by social cohesion and connectedness. Relatedly, scholars have recently turned to re-emphasizing voting as a social action related to feelings of social pressure and responsibility (Bond et al. 2012; Fowler 2005; Rolfe 2012; Sinclair 2012), which presumably decreases with lower social cohesion (Eagles and Erfle 1989; Parry and Moyser 1984).

Additionally, turnout may decrease in response to a tendency for individuals to withdraw from situations that involve conflict (Barber and Imai 2014). Conflict avoidance propensity has been documented in several studies, and related investigations have documented that individuals are more likely to vote when they engage in social environments with people with whom they agree politically (Campbell 2006 [cited in Barber and Imai 2014]; McClurg 2006; Mutz 2006 [cited in Barber and Imai 2014]; see, however, Klofstad, Sokhey, and McClurg 2013 for a critical discussion). Accordingly, if individuals take greater ethnic diversity as an indication of larger conflicts of interest, they might withdraw from political activities, including voting, in order to avoid being confronted with opposing political viewpoints (Barber and Imai 2014).

Thus, while mobilization theory argues that intergroup exposure generates conflict, which in turn inspires individuals to turn out, marginalization theory predicts that individuals feel less socially connected and committed when exposed to greater ethnic diversity, and as a consequence withdraw from political participation, including voting. Empirical investigations of the issue have not provided consistent evidence in favor of either theory. This, however, may be related to several severe challenges for the study of context effects on turnout. In the following, we elaborate on these challenges, and describe how the methodology and data that we apply accommodate some of the concerns.

Research Design

Scholars studying the effect of neighborhood exposure to ethnic others on individuals' propensity to vote face severe methodological challenges related to (1) establishing causality and (2) reducing measurement error. The problem related to causal inference arises as certain (typically) unobserved factors (e.g., personality traits and early life imprecisions such as factors related to individuals' upbringing) are likely to affect both where individuals reside and their likelihood of voting (Barber and Imai 2014). Accordingly, a correlation may not reflect an effect of context on turnout, but rather confounding factors. The challenge related to measurement error is particularly salient with regard to specifying the relevant contextual unit within which exposure effects occur. This challenge is particularly pronounced in light of extensive research showing that the use of inappropriate units can substantially bias results (for further details,

see Fotheringham and Wong 1991; Wong et al. 2012; and Wong 2009 on the Modifiable Areal Unit Problem). A recent study of neighborhood ethnic composition on trust demonstrates that the exposure effect is present only in individuals' very proximate residential area in which exposure to other residents is almost unavoidable (Dinesen and Sønderskov 2015). While, of course, individuals' daily experiences and observations are not confined to their very proximate neighborhood, it is less certain that they personally observe relevant demographic developments within their larger residential neighborhood. As a consequence, using the ethnic composition of larger areas (such as counties or municipalities) as a proxy of neighborhood exposure is problematic, because it is likely to be related to greater measurement error.

In this study, we are able to substantially reduce concerns regarding both confounding and measurement error by using a panel dataset with detailed geographical information. Specifically, our empirical investigations have two major advantages. First, the panel setup facilitates more precise causal estimates, because it allows us to control for all time-invariant individual heterogeneity (e.g., psychological variables and social upbringing) by examining how *changes* in individuals' turnout respond to *changes* in the ethnic composition of their local context (specifically, we perform first difference regression analyses). Considering that most previous studies, and to our knowledge all previous European studies, have been based on cross-sectional designs, this advantage brought about by the panel data is non-trivial.

Second, we are able to reduce measurement error regarding natives' exposure to ethnic minorities, because we have fine-grained information regarding the demographic composition of exceptionally small spatial units (100 * 100 meters) within which voters reside (the average number of relevant citizens in such a context is just 76). Accordingly, we do not have to rely on large administrative groupings such as electoral districts when measuring local exposure to immigrants, though we also test the robustness of our results with more aggregate context measures (contexts of 1 * 1 km and 10 * 10 km).

Ethnic Diversity in a Danish Context

The history of immigration to Denmark resembles that of other Nordic countries and other Western European countries like Germany without a strong colonial history. Historically, these countries have been relatively homogeneous and have had low levels of immigration. In the 1960s immigrants from particularly Turkey, Pakistan, and the former Yugoslavia settled in Denmark as migrant workers. Later, refugees from Vietnam, Iraq, Iran, the former Yugoslavia, and recently Syria have accounted for most of the non-Western immigration. Comparatively, Denmark is still a homogeneous society, though it has become less so in the recent years. In 2008, 9.2 percent of the population was of non-native descent, which increased to 12.3 percent in 2016. Individuals of non-Western descent comprised 6.2 percent of the population in 2008, increasing to 8.0 percent in 2016, with Turks constituting the largest group (around 1 percent of the population) (Statistics Denmark 2016).

Sixty percent of all immigrants live in social housing, compared to 20 percent of native Danes, and immigrants live 10 times more often in multi-ethnic neighborhoods. Even though non-native Danes tend to live in multi-ethnic neighborhoods (Andersen 2010; Andersen et al. 2016), the neighborhoods are not very segregated when compared to racial segregation in the United States. Rather, segregation is more similar to other Western European countries (Drever 2004). Even so, a negative relationship between high-share immigrants in neighborhoods and social trust has previously been shown in the country (Dinesen and Sønderskov 2015).

As in other Western European countries, the issue of immigration has over the past decades been a highly political salient issue in Denmark (Dahlgaard, Hansen, and Pedersen 2014; Hansen and Andersen 2013; Hansen and Stubager 2016). The Danish People's Party has established themselves as an anti-immigrant voice in the political space, and many of the major traditional parties have moved in the direction of a more immigration-skeptical stance since the 1990s (Green-Pedersen and Krogstrup 2008). The party gained 7.4 percent of the vote in its first national election in 1998, 13.8 percent in 2007, and 21.1 percent in 2015.

In sum, Denmark can be characterized as a historically homogeneous country that has experienced medium to high levels of immigration and where immigration since the 1990s has been highly politicized. Thus, there is at least the potential for conflict and impact on connectedness (Dinesen and Sønderskov 2015), which can activate mechanisms within mobilization and marginalization theory.

Data and Measures

The analysis is based on a rich register panel dataset from the 2009 and 2013 Danish municipal and regional elections. Denmark is a decentralized welfare state, and the municipalities, which also levy taxes, spend about 25 percent of the entire GDP and half of the public expenditures (Hansen, Houlberg, and Pedersen 2014). The elections, which are held every fourth year in November, are considered important, and turnout in the 2009 and 2013 elections was 65.8 and 71.9 percent, respectively. Specifically, the data consist of official voter lists as well as public recordings of individual-level socio-economic data (these include information regarding, for example, employment status, marital status, income, residential location, and residential history). Data from the voter lists depend on the individual municipalities' participation in the present municipality election study, which was voluntary. For the 2009 election, 44 of the 98 municipalities submitted their voter lists to the study (see Bhatti and Hansen 2010 for more details about the data collection), while for 2013 all municipalities participated (see Bhatti et al. 2014). We did not find major differences between participating and non-participating municipalities in 2009. A logistic regression predicting municipal participation in 2009 using a range of municipal level variables was insignificant ($p = 0.19$; see table A1.1 in section 1 of the appendix), though we note that there is a tendency for participating municipalities to be larger in terms of inhabitants, for example, and to score lower on an index for

socio-economic expenditure needs. Thus, some caution should be taken when generalizing the results to all municipalities. The participating and non-participating groups experienced a similar change in turnout between 2009 and 2013 (5.8 percentage points and 5.9 percentage points, respectively). For the participating municipalities, we have data for all eligible individuals,[1] and a major advantage of our dataset is therefore the absence of individual level self-selection. In total, our sample consists of about 1.9 million voters of ethnic Danish origin, corresponding to about 44 percent of the entire eligible population in the 2009 Danish electorate.

To facilitate measures of individuals' local context, all municipalities were divided into 100 * 100 meter squares with (anonymized) codes connecting all voters to the specific square that they reside in on the two election days. By aggregating individuals within the neighborhood identifiers (the 100 * 100 meter squares), we can generate measures regarding various relevant contextual factors (i.e., share of [non-Western] immigrants as well as relevant control variables such as average income and educational level). In robustness tests, we experiment with alternative sizes of the contexts (e.g., Dinesen and Sønderskov 2015; Reardon et al. 2008). Besides eligible individuals, we also have access to socio-economic information for non-eligible individuals.

In the primary analyses, we exclude individuals who moved between the first and second wave (we define a move as having different residential addresses on January 1, 2010, and January 1, 2014), since otherwise unobserved contextual factors are not held constant, thereby furthering the risk of biased estimates (see Barber and Imai 2014 for a similar approach). Yet, in subsequent robustness analyses, we rerun the analyses with individuals who move.

Turnout is measured by a dummy variable that for each individual at each election denotes whether the individual voted (voting is coded as 1). All individuals eligible for voting are automatically registered as voters in Denmark, and the registration regarding whether an individual voted is carried out at the polling station when individuals retrieve their voting ballot. As the analysis uses the first difference estimator, the dependent variable is *change* in turnout, and is calculated by taking the 2013 turnout (0–1) and subtracting the 2009 turnout (0–1). It is therefore scaled from −1 to 1.[2]

We measure exposure to ethnic others by the share of individuals residing within the 100 * 100 meter area who are immigrants or first-generation descendants of non-Western origin (see section 2 in the appendix for a definition of non-Western origin). As Denmark has traditionally been very ethnically homogeneous, the share of (non-Western) immigrants correlates strongly with the share of ethnic others, and therefore is a useful operationalization. We focus on non-Western immigrants, because they are likely to differ in visible ways from natives. While this approach parallels that of other studies focusing partly or exclusively on visible minorities (Danckert, Dinesen, and Sønderskov 2016; Hjerm 2009; Schneider 2008; Stolle, Soroka, and Johnston 2008), we perform robustness tests with various alternative operationalizations that will be introduced later. Following Statistics Denmark, we classify individuals as native Danes if at least one parent was born in Denmark and holds Danish citizenship, irrespective of whether the individuals were born in

Denmark and/or hold Danish citizenship themselves. Individuals who do not meet these criteria are considered either immigrants (if they were born outside Denmark) or descendants (if their parents were born outside Denmark).

The first differencing decreases the variance in the main independent variable compared to a simple pooled regression, and most individuals only experience moderate changes in the share of non-Western immigrants in the proximate residential area. Thus, on average the share of voters with non-Western origin changed 2.6 percentage points (std. dev. = 5.3). Yet, some experienced substantially greater changes, and more than 10 percent of the natives experienced increases in the share of voters with non-Western origin of at least six percentage points. Accordingly, though the variance in the independent variable is generally moderate, it does vary between individuals, and as we will get back to later, allows for precise estimates. Yet one point of caution should be noted. It is not random who experiences an increase in non-Western immigrants in a local area. For instance, there is a weak tendency for high-density areas to experience a greater increase than others (Pearson = 0.05). However, in general, correlations between pre-treatment variables and the main independent variable are weak, and the results we present thus go beyond a very select group of Danes.

The first difference model used to analyze the panel data by definition controls for all time-invariant variables (e.g., personality and childhood socialization). To avoid that the estimates are confounded by time-varying factors, we include a number of contextual and individual-level controls. At the individual level, we control for changes in marital status, income, unemployment status, and residential stability (the number of days [logged] that individuals have lived at their Election Day address), as well as distance between home and polling station, which are all factors that typically affect turnout (e.g., Dyck and Gimpel 2005; Haspel and Knotts 2004; McNulty, Dowling, and Ariotti 2009; Smets and van Ham 2013).[3] At the contextual level, we control for changes in population density, average income, and unemployment rate, as well as average educational level, which previous research has found to be important for turnout (Cohen and Dawson 1993; Fieldhouse and Cutts 2008a; Geys 2006). We also include municipal fixed effects to separate local contextual effects from any possible contextual effects at the municipal level. Details regarding the measures as well as descriptive statistics are reported in sections 2 and 3 of the appendix.

Results

To estimate the effects of neighborhood exposure to non-Western immigrants on turnout among natives, we perform first difference regression models, estimated by OLS with cluster robust standard errors.[4] The findings are reported in table 1; the model shows the predicted effect of a 100 percent increase in the share of non-Western immigrants within the very proximate residential area (100 * 100 meter). In other words, because changes in non-Western immigrants are measured in shares, its coefficient denotes the estimated effect from the largest possible change in neighborhood composition, that is, going from a zero share of non-Western immigrants to a neighborhood comprised only of non-Western

Table 1. First Difference Linear Regression Analysis Predicting Natives' Propensity to Vote (stayers only)

	(1) 100*100 m
Change in share of non-Western immigrants in context	−0.00900 (0.00826)
Individual-level controls	
Change in personal income billion DKK	−0.0299 (0.0937)
Change in unemployment status	0.0358*** (0.00177)
Change in marital status	0.0445*** (0.00189)
Change in residential stability (number of days logged)	0.0357*** (0.000679)
Change in distance to polling station (10,000 meters)	−0.00393 (0.00201)
*Context level (100*100 m) controls*	
Change in density (no. of 1,000 individuals) in context	−0.00650 (0.0347)
Change in average income (billion DKK) in context	−0.284 (0.542)
Change in unemployment rate in context	0.00664 (0.00461)
Change in educational level (share with high school degree) in context	0.00202 (0.00424)
Municipal-level fixed effects included	Yes
Constant	−0.0483*** (0.00305)
N	1,295,890

Note: Cluster-robust standard errors (clustered within 100*100 meter areas) in parentheses; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

immigrants. Note that as the first difference estimator analyzes changes in the independent variables in relation to changes in the dependent variable, effects of time-invariant variables are not estimated (they partial out, that is, they are indirectly controlled).

Model 1 shows a very weak negative correlation between the share of non-Western immigrants within the proximate neighborhood and individuals' propensity to vote, and the effect is insignificant ($p = 0.28$). The confidence intervals are so narrow that we with reasonable confidence can rule out even moderate effects (95 % CI = [−0.025 ; 0.007]). Even when considering the lower bound of the 95 percent confidence interval around the point estimate, a substantial

increase in the share of non-Western immigrants of 10 percentage points—which only about 5 percent of the sample experiences—is predicted to decrease individuals' propensity to vote with merely 0.25 percentage points. This corresponds to about one-twentieth of the effect of getting married (documented also by, e.g., Wolfinger and Wolfinger 2008). The maximum possible change from zero percent to 100 percent non-Western immigrants would only yield an effect of −2.5 percentage points. The upper bound of the confidence interval corresponds to a miniscule 0.07 percentage point increase in turnout if the share of non-Western immigrants increased 10 percentage points and 0.7 percentage points when going from the theoretical minimum to the theoretical maximum of non-Westerners. Thus, contradicting the expectations of the threat and mobilization theory as well as the marginalization theory, increasing shares of non-Western immigrants does not appear to impact individuals' propensity to vote.

As a supplement to the confidence interval interpretation above, we can illustrate the lack of substantial effects by conducting two one-sided tests of equivalence (Hoenig and Heisey 2001; Rainey 2014). The idea here is to define an absolute effect size (m) that constitutes a minimum meaningful effect and then test whether values below $-m$ and above $+m$ can be rejected. This approach of course involves some element of judgment about what constitutes a meaningful effect. One reasonable number for m in the present case could be 0.1, corresponding to a 1 percentage point increase in turnout for a 10 percentage point increase in the share of non-Western immigrants. This would for instance be equivalent to a modest 2.5 percentage point effect of going from the minimum to the 95th percentile in the 2009 data. As a comparison, Enos (2016) finds a 13.4 percentage point effect of demolishing projects in Chicago. If we use the proposed m in the model in table 1, the p-value associated with an effect smaller than $-m$ or greater than $+m$ is 0.0000 and we can thus confidently reject substantively important effects.

Corresponding to previous studies, table 1 also shows that residential stability increases individuals' propensity to vote, and that increases in the costs of voting, such as longer distance to the polling station, tend to have a small negative effect ($p = 0.05$) on voter turnout (Dyck and Gimpel 2005; Haspel and Knotts 2004; Highton 2000; McNulty, Dowling, and Ariotti 2009). Interestingly, becoming unemployed increases an individual's turnout. This may be because job loss acts as a mobilizing event, increases interactions with the government, and generates more free time to follow politics (Incantalupo 2012, 3–4).

Robustness Tests

Alternative size of neighborhoods

In robustness tests we repeat the main analysis with alternative definitions of individuals' proximate neighborhood; specifically, we measure contextual variables within 1 * 1 km and 10 * 10 kilometers squares (results are reported in table A4.1 in section 4 of the appendix). The analyses show that parallel results are obtained if the share of non-Western immigrants is measured within squares

of 1 * 1 km (see model 1 in table A4.1). The point estimate is almost identical to the 100 * 100 m squares. Due to the smaller number of neighborhoods, the uncertainty is higher than in the 100 * 100 m case, but we can still reject the possibility of major effects (95% CI = [−0.052; 0.038], *p*-value for equivalence test = 0.00). The effect estimate is also negative and insignificant with regard to the 10 *10 km squares (see model 2 in table A4.1), but the confidence intervals are so large that our model is not very informative (95% CI = [−0.321; 0.202], *p*-value for equivalence test = 0.38). In sum, the findings do not appear to be a construct of the particular geographic unit; rather, the conclusion is similar when using alternative context specifications.

Alternative Measures of Interethnic Exposure

Our primary measure of exposure to ethnic others is the share of non-Western immigrants and descendants within the proximate neighborhood. This operationalization rests on the assumption that non-Western immigrants constitute a more salient and typically more visually distinct out-group than Western immigrants. Yet, in subsequent tests, we repeat the analyses with a measure denoting the share of immigrants, irrespective of whether they are of Western or non-Western origin. However, the results are parallel to the ones reported in table 1 (regression estimates are reported in table A5.1, model 1, in section 5 of the appendix) and hereby further testify to the conclusion that local residential ethnic composition does not affect natives' propensity to vote.

Another concern relating to the operationalization of ethnic others is whether the measure should include both immigrants and descendants. Descendants comprise slightly more than one-quarter of non-Western immigrants in the 2013 sample. If descendants to a large extent behave like their Danish peers, they may not affect natives' voting behavior to the same extent as immigrants, who are more likely to, for example, speak Danish and maintain cultural norms from their country of origin. Along the same lines, recent immigrants presumably have assimilated less to the Danish culture and may therefore be more likely to affect natives' voting behavior (about 20 percent of the non-Western immigrants in the 2013 sample immigrated within the past five years). To address these concerns, we reran the analyses using two alternative measures of exposure to non-Western immigrants. The first alternative measure uses only immigrants (and not descendants) in the calculation of the measure of the neighborhood ethnic composition. The second alternative measure is further restricted to include only immigrants who came to Denmark relatively recently (five years prior to the elections). Yet, neither of these changes substantially alters the results (regression estimates are reported in table A5.1, models 2 and 3, in section 5 of the appendix).

Interactions

While we found no overall effects of the share of non-Western immigrants on voter turnout, a focus on the average effects could mask important effects for

certain segments of the population. We have no strong priors about such heterogeneous effects, but test two groups of interactions that could be relevant based on the existing theoretical literature.

First, within the literature it has been suggested that local or individual factors related to economic hardship may prime or enhance the perception that ethnic others constitute a threat and therefore act as a moderating factor (Citrin et al. 1997; Malhotra, Margalit, and Mo 2015; Scheve and Slaughter 2001). To inquire into this possibility, we conduct two additional tests. In the first we include an interaction between the neighborhood percentage of non-Western immigrants and the change in the neighborhood unemployment rate. In the second we add an interaction between the neighborhood percentage of non-Western immigrants and the change in individual-level unemployment. As is clear from table A6.1 (models 1–2), both interactions are insignificant. Accordingly, even when taking into account factors that are expected to enhance the perception that immigrants constitute a threat, we do not find any evidence in favor of the threat perspective.

Second, we examine whether the effect of non-Western immigrants depends on the initial share of non-Western immigrants in the neighborhood. This idea follows a logic proposed by Putnam (2007), who tentatively suggests that individuals may grow accustomed to exposure to ethnic others over time. In line with these expectations, Newman (2013) finds that increasing shares of immigrants only generates negative interethnic attitudes when the increases occur in neighborhoods that used to be very homogeneous, presumably because they are not used to interethnic exposure. However the alternative scenario—that negative effects of ethnic diversity may intensify with higher levels of ethnic diversity—is also plausible. For instance, in areas with higher shares of immigrants, the level of social ethnic segregation may be higher, causing new immigrants to become less well integrated among natives and in turn become more likely to affect natives' likelihood of voting. We find a small but significant ($p = 0.05$) interaction between the initial level of non-Western immigrants and the change in the share of non-Western immigrants (coefficients are reported in table A6.1, model 3, and are visualized in figure A6.1). For low initial levels of non-Western immigrants, the effects estimate is positive and statistically insignificant from zero. For high initial levels, the effect is negative and statistically different from zero. However, substantive effects are miniscule. The effect estimate is only significant for less than 5 percent of the sample (those living in neighborhoods with an initial share of non-Western immigrants of minimum 28 percent), and even here the average marginal effect is less than 2 percentage points.

Movers vs. Stayers

The primary analysis concerned only individuals who do not move. This parallels previous related research (Barber and Imai 2014) and is preferable considering that many contextual unobservable factors are not held constant when individuals move from one location to another. Yet, the effect of exposure to immigrants on individuals' propensity to vote may vary with (unobservable)

factors related to the choice of moving, and therefore we also probe the results obtained above by rerunning the analysis with movers rather than stayers. The results are reported in section 7 in the appendix. This analysis shows that increasing shares of non-Western immigrants exert a significant, negative effect on individuals' propensity to vote among individuals who move. Yet, though statistically significant, the negative effect is substantively negligible: Individuals experiencing a 25 percentage point increase in the share of non-Western immigrants (which occurred for only 5 percent of the moving population) are only 0.6 percentage points less likely to vote when considering the point estimate of the effect. In other words, moving to a substantially more ethnically diverse area makes 6 out of 1,000 natives refrain from voting, which corresponds to about one-seventh of the effect of changing marital status from married to not married or one-sixth of the effect of changing unemployment status. Thus, despite the statistical significance of the coefficient, in substantive terms the effect of changing ethnic composition when moving is largely irrelevant for individuals' propensity to vote.

Discussion and Conclusion

Widespread electoral participation across socioeconomic divisions and other societal fault lines is considered a core aspect of a healthy democracy. In light of high immigration rates in Western societies, it is therefore of obvious importance to what extent and in what way exposure to ethnic others impacts citizens' propensity to vote. Using a unique panel dataset with official turnout records for about 1.9 million Danish voters, this paper demonstrates that exposure to non-Western immigrants within citizens' proximate neighborhood does not have any substantive effects on their propensity to vote. This result is consistent across various context sizes and alternative specifications of the immigrant group. It is also important to notice that the results are not a result of low power, and even the upper and lower bounds of the confidence intervals imply only marginal effects.

As this is among the first studies to examine the issue using large-scale longitudinal data, which increases causal leverage considerably, the findings are an important contribution to our knowledge of out-group exposure effects on individuals' propensity to vote. Moreover, the implication of the findings is substantial: While contradicting both the mobilization/threat perspective and marginalization theory, the results provide evidence that comfort worries of detrimental consequences of increasing diversity, by showing that citizens' propensity to vote seems to be substantively unaffected by neighborhood exposure to immigrants and ethnic others more generally. When immigrants move into an area, overall neighborhood turnout may descriptively decrease due to the low turnout of immigrants, but the immigrants do not to any substantial extent affect the propensity to turn out among the natives living in the area.

At the same time, the results are noticeable in light of recent studies indicating that turnout increases in ethnically segregated areas. Thus, Enos (2016) finds that the demolition of Chicago ghetto areas reduced whites' turnout substantively,

presumably because the conflict over resources and power was less salient when the black minority group diminished markedly in size. As pointed out by Enos, the substantively larger turnout responding to the propinquity of black minorities may be related to a high level of segregation serving to increase the saliency of ethnic differences. A cautious interpretation, therefore, is that the dynamics related to living in ghetto areas in which ethnic groups are highly segregated and related to hostility and fear may not be representative for the mechanisms and effects playing out generally when ethnic diversity increases. The results are in this respect particularly interesting, as immigration in Denmark is politicized and therefore Denmark is by no means a least likely case in terms of mobilization due to fear of ethnic others. Alternatively, the difference in the results may be due to the different minority context in the United States and Europe. Specifically, majority-minority relations in the United States are often seen as a matter of race, whereas in Europe to a larger extent it is focused on country of origin. Clearly, further research along these lines is needed to fully understand these potential mechanisms.

Additionally, future research may shed light on long-term effects of increasing ethnic diversity. Specifically, while our study shows that there is no substantial effect of changes occurring over a period of four years, the effects may differ over very long periods of time. Individual-level panel data for longer periods do not exist in the literature, but in the future these data may become available, which could add even further to our knowledge of the consequences of increasing ethnic diversity.

Notes

1. In 2009, there were no missing districts in 39 municipalities. In Rudersdal, one district was missing. In Copenhagen, one table (voters are assigned randomly to tables) within one district was missing. In Aarhus, six districts were missing. In all cases, the municipalities had lost the ballots. Esbjerg participated only with those districts with digital voting lists. In one municipality, Odense, the electronic registration in district four broke down for a couple of hours during Election Day. This resulted in 1,160 voters being erroneously coded as non-voters. In all five municipalities, no self-selection was involved, and the missing districts/tables/votes should therefore not pose an issue for the analysis. In 2013, all 98 municipalities participated. Again, some lists were lost in a few municipalities (this time Lyngby-Taarbæk, Greve, Kalundborg, Slagelse, Esbjerg, Aarhus, and Viborg), but again without self-selection. In total, we had access to 98.93 percent of the eligible individuals.
2. Note that in a two-wave panel like ours, this operationalization of the dependent variable in the first difference model gives results that are entirely parallel to the fixed effects estimator.
3. We did not include a measure regarding individuals' educational level, as there is very limited variation within the four years of study. Including it does not change the conclusions.
4. Angrist and Pischke (2009, 94–96) argue that the use of OLS regression is legitimate with binary dependent variables, which eases interpretation substantially compared to logistic regression analyses. Yet, we reran the analysis using fixed effects logistic regression, which gives substantively similar results.

Appendix

Section 1. Participation in the Study

Table A1.1. Logistic Regression Predicting Whether a Municipality Participated in the Study (i.e., participated in 2009)

	(1)
Log (inhabitants)	0.268 (0.570)
Area in sq km	−0.00112 (0.000937)
Socio-econ index	−2.417 (1.507)
Taxation foundation (1000 DKK)	−0.0214 (0.0164)
Share with higher education	4.519 (5.691)
Share non-Western inhabitants	3.409 (7.675)
Municipal turnout in 2009 (share)	−9.246 (8.623)
Constant	8.159 (11.85)
N	98
Prop > chi^2	0.19

Note: Logit coefficients. Standard errors in parentheses. * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. Data from Ministry of Social Affairs and the Interior (2016); Statistics Denmark (2016); Thomsen (2016). The socio-economic index is a standard index from Ministry of Social Affairs and the Interior (2016) where higher values imply a high expenditure need.

Section 2. Measures

Table A2.1. Measures Used in the Main Analyses (dependent and main independent variable)

Turnout	Denotes whether an individual voted (= 1) or did not vote (= 0). The measure is based on official records from the polling stations.
Share of non-Western immigrants	The share of individuals in a context who are immigrants and descendants of immigrants with non-Western origin. For the definition of immigrants and descendants, we rely on the practice employed by Statistics Denmark: Individuals are classified as native Danes if at least one parent was born in Denmark and holds Danish citizenship, irrespective of whether the individuals were born in Denmark and/or hold Danish citizenship themselves. Individuals

(*Continued*)

Table A2.1. ***continued***

	who do not meet these criteria are considered either immigrants (if they were born outside Denmark) or descendants (if their parents were born outside Denmark). For immigrants and descendants, the country of origin is defined as the mother's country of birth; the father's country of birth is considered to be the country of origin if it is not possible to identify the mother. If none of the parents can be identified, the country of origin is classified according to the immigrant's own statements. Immigrants and descendants are considered non-Western if they do not originate from the EU-15, Iceland, Norway, Switzerland, the European micro-states, North America, Australia, or New Zealand. The terms "first-generation immigrants" and "second-generation immigrants" are often used for immigrants born outside Denmark and descendants born in Denmark, respectively. In the remainder of the present paper, the term "immigrant" refers to both immigrants, refugees, and descendants. The influence of the respondent him-/herself was subtracted from the measure.
Individual-level control variables	
Personal income	Yearly pretax income in billion Danish crowns. The measure is based on 2013 prices.
Unemployment	Unemployed individuals in the week of the election are coded 1, others 0.
Marital status	Denotes the individuals' marital status on January 1, just after the election. Married (+ separated) individuals and individuals in a civil union are coded 1, others 0.
Residential stability	The number of days (logged) that individuals have lived at the address for which they are registered on Election Day.
Distance to the polling station	The distance to the polling station (measured in 10,000 meters) from the individual's residential address.
Contextual-level control variables	
Population density	Number of individuals residing within the context. The influence of the respondent him-/herself was subtracted.
Income level	Average yearly pretax income after taxes, measured in billion Danish crowns (2013 prices), of the individuals who reside within the context. The influence of the respondent him-/herself was subtracted.
Unemployment rate	Share of individuals residing within the context who are unemployed. The influence of the respondent him-/herself was subtracted.
Educational level	The share of individuals residing within the context who have completed high school. The influence of the respondent him-/herself was subtracted.

Note: As we subtract the individual him-/herself in the context-level measures, individuals drop out of the analysis if they are the only adult individual in a context.

Section 3. Descriptive Statistics for the Main Analysis

Table A3.1. Descriptive Statistics for Main Analysis (table 1, model 1)

	Mean	SD	*N*
Change in turnout	0.060	0.435	1,295,890
Change in share of non-Western immigrants in context (100*100 m)	0.012	0.058	1,295,890
Change in personal income billion DKK	0.000	0.001	1,295,890
Change in unemployment status*	0.995	0.243	1,295,890
Change in marital status*	1.004	0.251	1,295,890
Change in residential stability (number of days logged)	0.541	0.736	1,295,890
Change in distance to polling station (10,000 meters)	0.013	0.246	1,295,890
Change in density (no. of 1,000 individuals) in context (100×100 m)	0.006	0.019	1,295,890
Change in average income (billion DKK) in context (100×100 m)	0.000	0.001	1,295,890
Change in unemployment rate in context (100*100 m)	0.000	0.094	1,295,890
Change in educational level (share w/high school degree) in context	0.024	0.106	1,295,890

* = rescaled so 1 denotes no change.

Section 4. Robustness Test with Alternative Sizes of Neighborhoods

Table A4.1. First Difference Linear Regression Analysis Predicting Natives' Propensity to Vote (stayers only)

	(1) 1*1 km	(2) 10*10 km
Change in share of non-Western immigrants in context	−0.00687 (0.0230)	−0.0591 (0.133)
Individual-level controls		
Change in personal income billion DKK	−0.0418 (0.105)	−0.0458 (0.108)
Change in unemployment status	0.0357*** (0.00180)	0.0356*** (0.00280)
Change in marital status	0.0442*** (0.00195)	0.0441*** (0.00368)
Change in residential stability (number of days logged)	0.0360*** (0.000789)	0.0359*** (0.00164)

(*Continued*)

Table A4.1. ***continued***

	(1) 1*1 km	(2) 10*10 km
Change in distance to polling station (10,000 meters)	−0.00450 (0.00327)	−0.00436 (0.00458)
*Context level (1*1 km/10*10 km) controls*		
Change in density (no. of 1,000 individuals) in context	−0.00294 (0.00170)	−0.000138 (0.000162)
Change in average income (billion DKK) in context	−1.801 (1.744)	−207.6* (97.75)
Change in unemployment rate in context	−0.00893 (0.0183)	0.0679 (0.145)
Change in educ. level (share with high school degree) in context	−0.0136 (0.0148)	−0.0782 (0.0535)
Municipal-level fixed effects included	Yes	Yes
Constant	−0.0433*** (0.00394)	−0.0312* (0.0139)
N	1,313,084	1,313,369

Note: Cluster-robust standard errors (clustered within 1*1 km areas in model 1 and clustered within 10*10 kilometer areas in model 2) in parentheses; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table A4.2. Descriptive Statistics for 1 * 1 Kilometer Analysis (table A4.1, model 1)

	Mean	SD	N
Change in turnout	0.060	0.435	1,313,084
Change in share of non-Western immigrants in context (1*1 km)	0.015	0.028	1,313,084
Change in personal income billion DKK	0.000	0.001	1,313,084
Change in unemployment status*	0.994	0.243	1,313,084
Change in marital status*	1.003	0.253	1,313,084
Change in residential stability (number of days logged)	0.539	0.735	1,313,084
Change in distance to polling station (10,000 meters)	0.013	0.247	1,313,084
Change in density (no. of 1,000 individuals) in context (1*1 km)	0.382	0.790	1,313,084
Change in average income (billion DKK) in context (1*1 km)	0.000	0.000	1,313,084
Change in unemployment rate in context (1*1 km)	0.001	0.030	1,313,084
Change in educational level (share w high school degree) in context	0.026	0.040	1,313,084

* = rescaled so 1 denotes no change.

Table A4.3. Descriptive Statistics for 10 * 10 Kilometer Analysis (table A4.1, model 2)

	Mean	SD	*N*
Change in turnout	0.060	0.435	1,313,369
Change in share of non-Western immigrants in context (10*10 km)	0.016	0.014	1,313,369
Change in personal income billion DKK	0.000	0.001	1,313,369
Change in unemployment status*	0.994	0.243	1,313,369
Change in marital status*	1.003	0.253	1,313,369
Change in residential stability (number of days logged)	0.539	0.735	1,313,369
Change in distance to polling station (10,000 meters)	0.013	0.247	1,313,369
Change in density (no. of 1,000 individuals) in context (10*10 km)	26.422	42.566	1,313,369
Change in average income (billion DKK) in context (10*10 km)	0.000	0.000	1,313,369
Change in unemployment rate in context (10*10 km)	0.002	0.008	1,313,369
Change in educational level (share w high school degree) in context	0.024	0.022	1,313,369

* = rescaled so 1 denotes no change.

Section 5. Robustness Tests with Alternative Measures of Exposure to Immigrants

Table A5.1. First Difference Linear Regression Analysis Predicting Natives' Propensity to Vote

	(1) 100*100 m	(2) 100*100 m	(3) 100*100 m
Change in share of immigrants	−0.0123 (0.00730)	–	–
Change in share of non-Westerners excluding descendants	–	−0.00730 (0.00995)	–
Change in share of new non-Western immigrants	–	–	0.0110 (0.0129)
Individual-level controls			
Change in personal income billion DKK	−0.0296 (0.0933)	−0.0296 (0.0936)	−0.0287 (0.0939)
Change in unemployment status	0.0358*** (0.00177)	0.0358*** (0.00177)	0.0358*** (0.00177)
Change in marital status	0.0445*** (0.00189)	0.0446*** (0.00189)	0.0446*** (0.00189)
Change in residential stability (number of days logged)	0.0357*** (0.000679)	0.0357*** (0.000679)	0.0357*** (0.000679)

(*Continued*)

Table A5.1. ***continued***

	(1) 100*100 m	(2) 100*100 m	(3) 100*100 m
Change in distance to polling station (10,000 meters)	−0.00393* (0.00201)	−0.00392 (0.00201)	−0.00391 (0.00201)
*Context level (100*100 m) controls*			
Change in density (no. of 1,000 individuals) in context	−0.00435 (0.0347)	−0.00842 (0.0345)	−0.0112 (0.0346)
Change in average income (billion DKK) in context	−0.290 (0.547)	−0.280 (0.540)	−0.261 (0.528)
Change in unemployment rate in context	0.00670 (0.00461)	0.00653 (0.00461)	0.00645 (0.00461)
Change in educ. level (share w/high school deg) in context	0.00217 (0.00424)	0.00202 (0.00424)	0.00192 (0.00424)
Municipal-level fixed effects included	Yes	Yes	Yes
Constant	−0.0483*** (0.00305)	−0.0484*** (0.00305)	−0.0485*** (0.00305)
N	1,295,894	1,295,890	1,295,890

Note: Cluster-robust standard errors (clustered within 100*100 meter) in parentheses;
* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Section 6. Robustness Tests with Interactions

Table A6.1. First Difference Linear Regression Analysis Predicting Natives' Propensity to Vote (stayers only)

	(1) 100*100 m	(2) 100*100 m	(3) 100*100 m
Change in share of non-Western immigrants in context (MAIN)	−0.00903 (0.00826)	0.0309 (0.0306)	0.00288 (0.0102)
MAIN * Change in unemployment rate in field	0.00680 (0.0625)	–	–
MAIN * Change in unemployment status	–	−0.0401 (0.0297)	–
MAIN * lagged share of Non-Western immigrants in context	–	–	−0.0769* (0.0389)
Individual-level controls			
Change in personal income billion DKK	−0.0299 (0.0937)	−0.0299 (0.0937)	−0.0298 (0.0942)
Change in unemployment status	0.0358*** (0.00177)	0.0364*** (0.00182)	0.0358*** (0.00177)
Change in marital status	0.0445*** (0.00189)	0.0445*** (0.00189)	0.0445*** (0.00189)

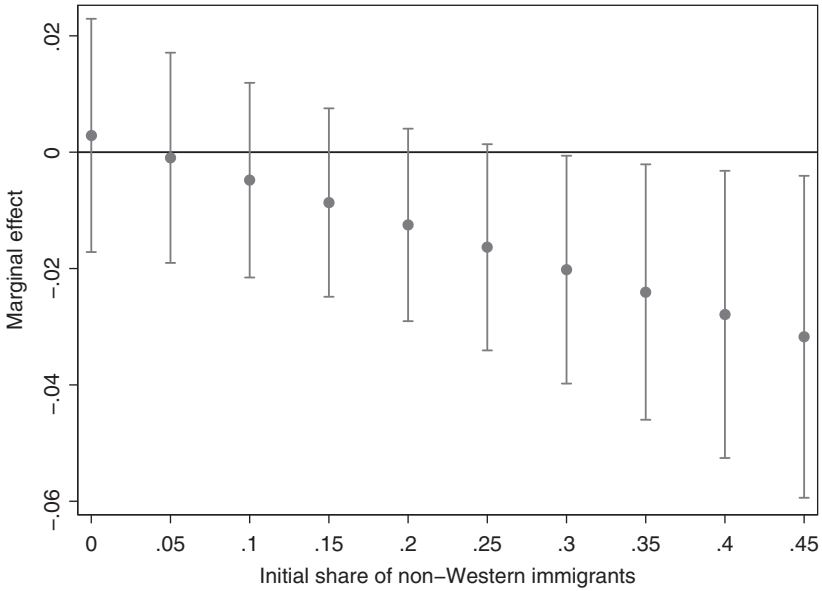
(*Continued*)

Table A6.1. ***continued***

	(1) 100*100 m	(2) 100*100 m	(3) 100*100 m
Change in residential stability (number of days logged)	0.0357*** (0.000679)	0.0357*** (0.000679)	0.0357*** (0.000679)
Change in distance to polling station (10,000 meters)	−0.00393 (0.00201)	−0.00393 (0.00201)	−0.00392 (0.00201)
*Context level (100*100 m) controls*			
Change in density (no. of 1,000 individuals) in context	−0.00649 (0.0347)	−0.00638 (0.0347)	−0.00569 (0.0346)
Change in average income (billion DKK) in context	−0.284 (0.542)	−0.284 (0.543)	−0.280 (0.542)
Change in unemployment rate in context	0.00659 (0.00463)	0.00662 (0.00461)	0.00663 (0.00461)
Change in educ. level (share w/high school deg) in context	0.00202 (0.00424)	0.00201 (0.00424)	0.00191 (0.00424)
Municipal-level fixed effects included	Yes	Yes	Yes
Constant	−0.0484*** (0.00305)	−0.0489*** (0.00308)	−0.0484*** (0.00305)
N	1,295,890	1,295,890	1,295,890

Note: Cluster-robust standard errors (clustered within 100*100 meter) in parentheses; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$. In model (3) we do not include the main effect for "lagged share of Non-Western immigrants in context," so the model corresponds to an identical fixed effects model. Including the main affect yields the same conclusion.

Figure A6.1. Illustration of interaction of the effect of model 3, table A6.1



Note: The figure illustrates the marginal effect of a 100 percentage point increase in share of non-Western immigrants on individuals' propensity to vote depending on the initial share of non-Western immigrants. Vertical lines indicate 95 percent confidence intervals.

Section 7. Robustness Tests to Movers Instead of Stayers

Table A7.1. First Difference Linear Regression Analysis Predicting Natives' Propensity to Vote (stayers only)

	(1) 100*100 m
Change in share of non-Western immigrants in context	−0.0232*** (0.00579)
Individual-level controls	
Change in personal income billion DKK	−11.61** (3.766)
Change in unemployment status	0.0347*** (0.00219)
Change in marital status	0.0406*** (0.00201)
Change in residential stability (number of days logged)	0.0285*** (0.000421)
Change in distance to polling station (10,000 meters)	−0.00735*** (0.00223)
*Context level (100*100 m) controls*	
Change in density (no. of 1,000 individuals) in context	−0.0505*** (0.00756)
Change in average income (billion DKK) in context	39.89*** (6.670)
Change in unemployment rate in context	−0.0143* (0.00706)
Change in educational level (share with high school degree) in context)	−0.00637 (0.00331)
Municipal-level fixed effects included	Yes
Constant	0.0353*** (0.00364)
N	587,715

Note: Cluster-robust standard errors (clustered within 100*100 meter areas) in parentheses; * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$.

Table A7.2. Descriptive Statistics for the Analysis of Movers (table A7.1)

	Mean	SD	*N*
Change in turnout	0.112	0.545	587,715
Change in share of non-Western immigrants in context (100×100 m)	0.010	0.145	587,715
Change in personal income billion DKK	0.000	0.000	587,715
Change in unemployment status*	1.009	0.336	587,715
Change in marital status*	1.034	0.392	587,715
Change in residential stability (number of days logged)	−0.718	1.890	587,715
Change in distance to polling station (10,000 meters)	0.012	0.382	587,715
Change in density (no. of 1,000 individuals) in context (100×100 m)	0.000	0.123	587,715
Change in average income (billion DKK) in context (100×100 m)	0.000	0.000	587,715
Change in unemployment rate in context (100×100 m)	0.000	0.116	587,715
Change in educational level (share w/high school degree) in context	0.011	0.254	587,715

* = rescaled so 1 denotes no change.

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