Voting Rights and Immigrant Incorporation: Evidence from Norway

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How do political rights influence immigrant integration? In this study, we demonstrate that the timing of voting rights extension plays a key role in fostering political incorporation. In Norway, non-citizens gain eligibility to vote in local elections after three years of residency. Drawing on individual-level registry data and a regression discontinuity design, we leverage the exogenous timing of elections relative to the start of residency periods to identify the effect of early access to political institutions. We find that immigrants who received early access were more likely to participate in subsequent elec-

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toral contests, with the strongest effects visible among immigrants from dictatorships and weak democracies. We also observe evidence consistent with spillover effects for other aspects of political engagement. These findings suggest that early access to voting rights influences subsequent trajectories of immigrant incorporation, in particular among immigrants from less developed states who may otherwise face high integration barriers.

KEY WORDS: Immigration, integration, turnout, regression discontinuity

How do states and societies successfully incorporate immigrant populations? Although policymakers have grappled with this issue for the better part of a century, the question has gained renewed importance in Europe in the wake of increasing flows of refugees and internal labor migrants. In 2015, for instance, EEA countries reported that 33.7 million foreign nationals resided within their borders, representing a 43% increase over 2005.[1]

While much attention has focused on policies that foster economic integration, governments have also engaged in extensive efforts to promote the political and civic incorporation of immigrants. Historically, political rights have been closely tied to citizenship. Yet in recognition of the large (and growing) number of non-naturalized citizens within Europe, policymakers have sought other means to channel immigrant sentiments and concerns through formal political institutions. Building on normative arguments that non-citizens are "members of a community of shared political fate"[2], and on the assumption that the exercise of political rights will accelerate engagement and adaptation with the host society,[3] a number of European states have extended voting rights to foreign residents in local elections. Currently, all European Union states allow EU citizens

[1] Eurostat Population Database. These figures understate the importance of the integration challenge in that they adjust for naturalization; in 2015 the EEA reported 55.9 million foreign-born residents.

[2] Earnest 2014:2.

[3] Munro 2008.

to vote in local elections, while 15 European states have extended some degree of local voting rights to third country nationals.[4]

Given the widespread adoption of foreign voting rights, it is surprising how little empirical evidence exists concerning whether rights extension promotes subsequent integration. In part, this can be attributed to the selection bias researchers face when seeking to identify the effect of non-compulsory rights: those individuals who take advantage of opportunities to express their preferences are precisely those who are most likely to exhibit positive integration outcomes. However, the evidence that does exist suggests that the consequence of rights extension goes beyond mere selection effects. For instance, Hainmueller, Hangartner and Pietrantuono[5] leverage a regression discontinuity related to citizenship referendums in Switzerland, in which ostensibly similar individuals were either granted or denied citizenship on the basis of a few votes. Studying long-term outcomes between the two groups, they find clear positive effects of naturalization on subsequent political participation.

Why might the extension of local voting rights similarly promote integration? First, granting voting rights to foreign residents provides local politicians with embedded incentives to seek immigrant votes. These efforts – regardless

[4] Eligibility conditions vary by state. For an overview, see Groenendijk 2008 and Toral 2015. The 15 countries are: Belgium, Denmark, Finland, Greece, Iceland, Ireland, Netherlands, Norway, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland (some cantons), UK.

[5] Hainmueller, Hangartner and Pietrantuono 2015

of their ultimate success in influencing turnout – should raise levels of political knowledge and engagement among foreign residents.[6] Second, and perhaps more importantly, the extension of rights may increase levels of trust and promote identification with the host society, providing foreign residents with incentives to avoid political exclusion by engaging with civil society and local governments.[7]

In this paper, we extend this perspective by arguing that the *timing* of rights extension plays a key role in shaping integration trajectories. Immigrants' level of civic and political engagement tends to be correlated with length of stay, likely due to the cumulative effect of exposure to host institutions,[8] or increasing social and economic capital.[9] However, regardless of the length of this socialization period, we hypothesize that formative early experiences will shape subsequent patterns of engagement in a path-dependent manner. The literature on habit formation and disruption finds that shocks to context may provide

[6] The extension of local voting rights may also generate a more favorable policy environment for immigrants, with respect to the content of service delivery, see Vernby 2013 and Ferwerda 2016.

[7] Munro 2008.

[8] White et al. 2008; Voicu and Comsa 2012; De Rooij 2012; Wass et al. 2015.

[9] Bueker 2005; White et al. 2006; Bevelander and Pendakur 2009. For a contrasting account, see Leal 2002, who finds that gaps in information and political interest explain variation in the political activity of non-citizens within the United States. Bevelander and Pendakur 2011 demonstrate an attitudinal component as well by linking participation to the pursuit of citizenship.

temporally-limited opportunities to set new modes of behavior.[10] Consistent with this process, an emerging body of research has suggested the presence of an 'integration window,' in which the likelihood of integration is shaped by initial experiences in the host country. For instance, as-if random delays in access to labor markets have been shown to depress long-term economic integration among asylum seekers in Switzerland.[11] Thus, if immigrants become habituated to exclusion from economic, political, or civic institutions, these patterns may persist.

We thus expect that early opportunities to participate in the political process will influence immigrants' subsequent level of engagement. In addition to creating a sense of active inclusion,[12] research in other contexts has suggested that initial political engagement is particularly habit-forming.[13] For instance, Meredith[14] finds that California voters who were born six weeks too late to be eligible to vote in the 2000 US presidential election were less likely to vote in the subsequent presidential election, compared to voters who were born six weeks prior to the eligibility date.[15]

[10] Wood et al. 2005; Lally et al. 2010.
[11] Hainmueller et al. 2016.
[12] Burchardt et al 2002; Bevelander and Pendakur 2011.
[13] E.g. Meredith 2009; Bedolla and Michelson 2012; Coppock and Green 2016; DeKadt 2015.
[14] Meredith 2009.
[15] In Norway, citizens become eligible to vote in the calendar year that they turn 18. As a result, the degree to which habituation influences voting tendencies among natives remains unknown. However, it is worth noting that an RD analysis of turnout in Denmark and Finland

While our empirical approach is similar to Meredith,[16] our design focuses on immigrant residency periods rather than age, and estimates the effect of early access on downstream integration outcomes. Examining the Norwegian context, which grants local voting rights to foreigners after three years of continuous residence, we draw on validated voting records and registry data for the complete population of eligible foreign residents in 27 of the largest Norwegian municipalities. Leveraging the exogenous timing of elections relative to the exact date of immigrants' arrival in Norway, we use a regression discontinuity design to identify the effect of early access to voting rights. Our findings suggest that immigrants who were just barely eligible for the 2011 elections (by a matter of days) participated at higher rates in the 2015 elections than their counterparts who missed the eligibility cutoff and had to wait an additional four years to vote in local elections. The positive results of early access are not strictly limited to electoral participation: while we find no evidence that early access to voting rights improves economic integration, we find suggestive evidence for spillover effects when assessing other aspects of integration such as continuing education, group membership, and political engagement.[17]

finds negligible or negative effects of past eligibility (Bhatti et al. 2016), which suggests that habituation effects among citizens may be limited in settings with automatic registration and high turnout.

[16] Meredith 2009.

[17] Given that we identify the effect of early access, these results may be viewed as a lower-bound for the effect of extending voting rights to immigrants, broadly writ.

Immigrants arrive in Norway with vastly different experiences and motivations to integrate. As a result, it is reasonable to expect heterogeneity in the effect of early eligibility across national origin. In particular, we evaluate whether prior experiences with democratic institutions moderate the relationship between voting rights and subsequent integration. Theoretical and empirical research suggests that these varying life experiences matter: individuals systematically differ in their "democratic capital", which may affect political preferences and modes of behavior.[18] Moreover, nationality of origin has been demonstrated to play an important role in moderating immigrants' likelihood of acquiring citizenship and voting, even after accounting for socio-economic factors,[19] because immigrants may initially "transfer" prior knowledge and modes of behavior to new contexts.[20]

While the consensus is that immigrants from non-democracies will, on average, initially participate at a lower rates in elections,[21] we should also expect these individuals to be more sensitive to the timing of rights extension. Whereas migrants from strong democracies are familiar with democratic institutions and

[18] Persson and Tabellini 2006; Fuchs-Schündeln and Schündeln 2015.

[19] Ramakrishnan and Espenshade 2001; Bueker 2005; Bird et al. 2010; Strijbis 2014; Ruedin 2016.

[20] Black 1987; White et al. 2008.

[21] Pikkov 2011; De Rooij 2012; Wass et al. 2015. Immigrants with strong ethnic networks and kinship ties are often cited as an exception, see e.g. Fennema and Tillie 1999. For a disputing account, see Strömblad and Edman 2010.

may have been habituated into voting or abstaining early in the lifecourse,[22] immigrants from dictatorships and weak democracies encounter a greater institutional and contextual shock that may promote subsequent behavioral change.[23] Our empirical results support this conditional relationship: we find that early access to voting rights sharply improves political participation by on average 8 percentage points among foreign residents originating from dictatorships or weak democracies. In contrast, early access appears to have a limited effect for immigrants with prior exposure to democratic institutions, who may already be habituated towards voting or abstaining.[24]

These findings have important policy implications. When contrasted with lowering naturalization barriers, extending local voting rights to non-citizens may provide a potentially less radical tool to improve immigrants' engagement with the host society. Although many European countries have recognized this possibility by extending local voting rights, eligibility criteria and the timing of rights extension vary extensively across states. Our findings suggest that policymakers seeking to improve the integration of foreign residents within these states should consider lowering the eligibility threshold. In particular, our re-

[22] Voicu and Comsa 2012.

[23] If escaping political repression, they may also place a higher value on the exercise of democratic rights.

[24] Unlike existing studies in the American context, our study focuses on voters from all age cohorts.

sults show that early access increases engagement for immigrants from non-democratic backgrounds, a group who integrate at a slower pace and face substantial integration challenges.

INSTITUTIONAL SETTING

At first glance, the extension of local — rather than national — voting rights may seem inconsequential.[25] Yet in Norway, as in many other European states, local governments play an important role in shaping policy. Although Norway has a unitary system of government, in which all constitutional power rests in the national parliament (Stortinget), significant powers have been granted to the municipal level, particularly in the area of social provision. Core services and benefits of the Norwegian welfare state (such as social assistance payments, elementary and middle schools, local health care, eldercare and children's welfare services) are in large part determined and administered by municipal governments, using revenues financed by local income and property taxes, as well as transfers from the central government. In 2012, these expenditures accounted for 14.8% of National GDP (Eurostat).

Local governments are controlled by partisan municipal councils, democratically elected by the residents of each municipality. Local elections are held

[25] See Gonzalez-Ferrer and Morales 2013.

every four years, in September, two years after the last parliamentary elections. Although parliamentary elections receive a greater level of interest, media coverage, and voter turnout, the level of decentralization in Norway ensures that local elections are nonetheless a major event in Norwegian politics. The election campaign is widely covered in the media, and voter turnout remains high: in 2015, for instance, 60 percent of all eligible voters participated in local elections.

Eligibility rules for foreign nationals in Norway are among the most liberal in Europe, as local voting rights are granted to all foreign nationals after a brief period of residence. On the basis of reciprocal agreements, citizens of Nordic countries gain voting rights after less than three months of residence. More importantly, since 1983, all other foreign nationals with at least three years of continuous legal residence have had the right to vote in local elections. Although local voting rights for third-country nationals have been extended in other contexts, the Norwegian government has followed the Nordic tradition of lowering eligibility barriers and actively promoting foreign voter participation. Voter registration is automatic, and individuals receive a letter in the mail a month before the elections informing them of their rights and the location of their polling place.

In the 2015 Norwegian local elections, 309,593 foreign nationals possessed voting rights, constituting 7.7 percent of the electorate. Despite efforts to en-

courage voting amount foreign nationals, immigrant turnout remained substantially lower than that of the native population. In the 27 municipalities we study in this paper, foreign nationals voted at a rate of 28 percent, whereas 65 percent of those with Norwegian citizenship participated.[26] When we limit the population of foreign nationals to those with non-Nordic citizenship (as we do in the main analysis), turnout among foreign voters declines to 25 percent.

DATA

Our data is provided by the Norwegian government, drawing on two distinct sources. First, we leverage data from a new program implemented in 27 of Norway's largest municipalities that recorded whether an individual participated in the 2015 local elections.[27] This dataset includes individual-level, validated records of voter participation for the complete population of foreign and native voters in these municipalities. In total, these data cover 42 percent of the electorate, and 49 percent of eligible third-country nationals (108,744 unique cases,

[26] Foreign turnout was quite stable across the elections we study in this paper (see table **??** for turnout by immigrant groups). In 2011, turnout for newly eligible foreign voters was 28 percent, representing a slight decrease from 2007 (29 percent). Studies suggest that immigrant turnout was largely unaffected by the Breivik terror attacks on July 22, 2011, except among young voters (see Bergh and Ødegaard 2013). If we examine the change in turnout from 2007-2011 by immigrant groups, there is no clear pattern of a general increase in turnout among non-democracies or predominantly Muslim countries, with the exception of Somalis. The subsequent analysis is robust to excluding this group.

[27] See Online Appendix for details.

after excluding Nordic citizens).[28] The majority of eligible foreign voters are from Europe. In the sample we rely on below, Poland (23 percent), Germany (6 percent), and Lithuania (6 percent) are the largest European immigrant groups, while the largest non-Western groups are from Somalia (3 percent), The Philippines (2 percent), and Iraq (2 percent).

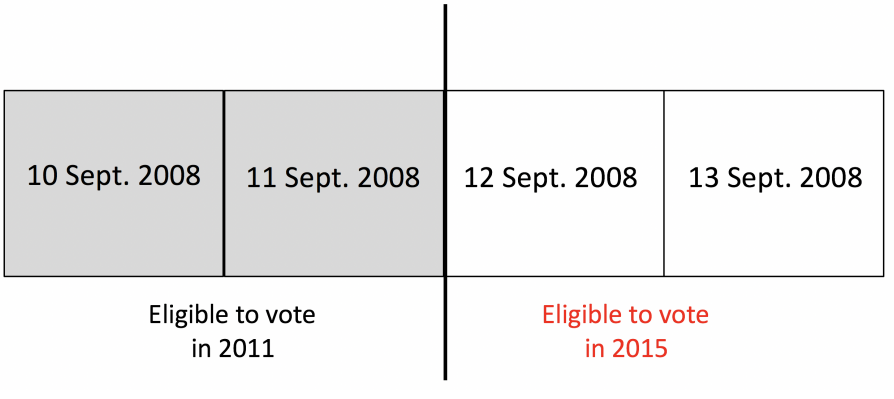
Using unique identifiers, we match voting records with official registry data provided by the Norwegian office for central statistics. This data includes basic demographic and background characteristics for each individual, including age, gender, marital status, and national origin, which we leverage to evaluate the validity of the regression discontinuity design. The administrative data also includes information on labor market participation, social assistance, union membership, and continuing education, which we use as alternate dependent variables for our analysis. Finally, the administrative data includes the precise date that the individual began their residency period in Norway. These records are used by the national electoral authorities when determining individual-level eligibility, and as a result, they provide a highly accurate measure of whether an individual was eligible to vote in previous electoral contests.

[28] Given that Nordic citizens are eligible at a different date, including them would violate the assumptions of the regression discontinuity design. We subsequently use these observations as a placebo test.

EMPIRICAL STRATEGY

Immigrants coming to Norway have the right to vote in local elections after three consecutive years of residency. The 2011 local elections took place on September 12, implying that immigrants had to arrive in Norway on or before September 11, 2008 in order to to be eligible to vote. Election authorities have confirmed by personal communication that this rule was strictly enforced: those who arrived on September 12, 2008 had to wait an additional four years before they gained the right to vote in local elections. Figure 1 illustrates the rule. As we outline next, this institutional rule makes it possible to estimate the causal effect of early access to voting rights on subsequent political participation.

Fig. 1: Eligibility is determined by date of arrival



We assume that each immigrant's decision to vote in the 2015 election has two potential outcomes; $Y_i(1)$ which is the observed decision to vote if the individual was eligible to vote in the 2011 election, and $Y_i(0)$ which is the observed decision if not eligible to vote in the 2011 election. The causal effect of eligibility to vote in 2011, τ_i, is the difference between these two potential outcomes: $\tau_i = Y_i(1) - Y_i(0)$. Unfortunately, we can only observe one potential outcome for each immigrant i. If the immigrant arrived on or before Sept 11, 2008, we observe $Y_i(1)$, if s/he arrived after Sept 11, 2008, we observe $Y_i(0)$.

The key empirical problem is to obtain plausible counterfactuals for each scenario. To do so, we exploit the strict eligibility rule and leverage a regression discontinuity (RD). Specifically, we estimate whether immigrants who barely missed the cutoff for eligibility (by a matter of days) participated at different rates in the 2015 elections when compared to similar immigrants who arrived a few days earlier, and subsequently had early access to voting rights. By focusing on individuals close to this arbitrary cutoff, we can generate plausible counterfactuals: $E[Y_i(1)|X_i = c]$ for those arriving at or prior to Sept 11, 2008 and eligible to vote, and $E[Y_i(0)|X_i = c]$ for those arriving after Sept 11, 2008. Provided that the two groups of immigrants are similar, the distance between the means at the cutoff will provide a reasonable estimate of the effect of early voting rights on participation in the 2015 local elections.

Whether $\hat{\tau}$ provides an unbiased estimate of τ, the average effect of early eligibility, hinges on the ability of individuals to influence their position with respect to the cutoff. In our case, some migrant groups need visa approval to enter Norway, but other groups are able to influence the date in which they arrive in Norway. However, despite this degree of individual agency, it is highly implausible that the decision to migrate was based on eligibility to vote in the next round of local elections. Most likely, prospective immigrants had no knowledge of the eligibility date when they arrived in Norway, which makes our RD approach similar to an experiment in which immigrants are as-if-randomly assigned into treatment and control groups. However, this assumption only holds close to the eligibility date, since changes in immigration flows might imply that the characteristics of immigrants – and hence the average potential outcomes – deviate as we move away from the cutoff date.[29]

Accordingly, we estimate τ_i using a local polynomial, which we fit to narrow bandwidths around the cutoff date. Given that the data includes the precise day on which an individual registered in Norway, our design permits these bandwidths to be highly granular. Nevertheless, the selection of the optimal bandwidth remains important: while the precision of the treatment estimate increases with the size of the bandwidth, the bias at larger bandwidths might increase. We

[29] We describe the characteristics of immigrants who arrived in close proximity to the registration cutoff in Online Appendix Table A-3.

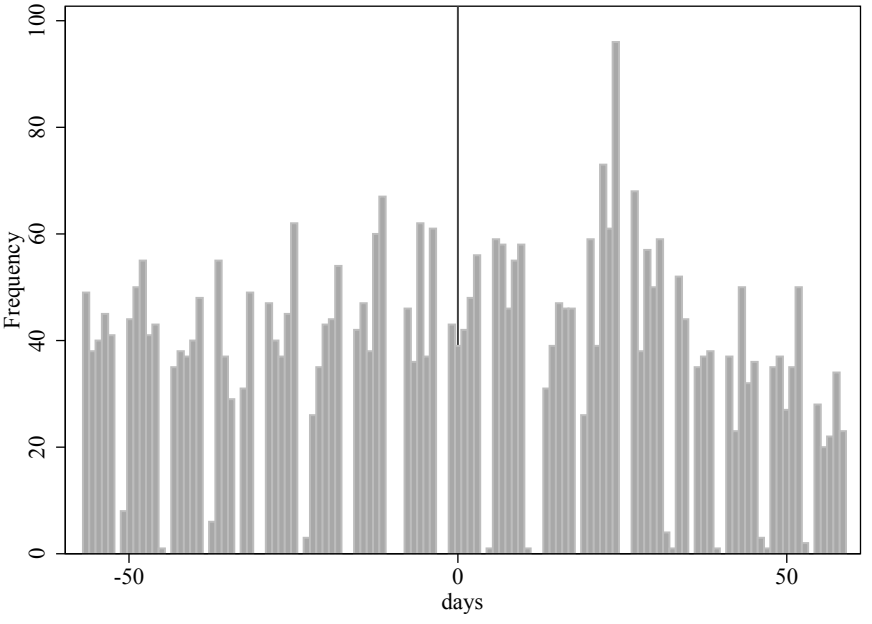
follow recent recommendations and developments in RDD methodology and select the optimal bandwidth using a data-driven, non-parametric approach.[30] In our main analyses, we rely on the Calonico, Cattaneo, and Titiunik[31] approach to select the bandwidth, but reports results using an alternative approach. Given recent advice on the dangers of over-fitting when using more than two polynomials[32] we rely on linear or quadratic specifications using a triangular kernel.

RESULTS

Before presenting the estimates, we first verify the RD design. Figure 2 plots the daily number of immigrants registered in Norway around the cut-off date of September 11, 2008. The figure demonstrates that immigrants are usually registered on weekdays (September 11, 2008 fell a Thursday) and that the daily number of registrations varies between 40 and 60 individuals.[33] Relatively few foreigners registered on September 11 (39), but the same is true for September 10 (42) and September 12 (43). There is a spike in number of immigrants on September 9 and September 8, but similar upticks can be observed for most weeks, reflecting the fact that immigrants are likely to be registered on Mondays

[30] Skovron and Titiunik 2015.
[31] See Calonico et al. 2014a; 2014b.
[32] Gelman and Imbens 2014; Skovron and Titiunik 2015.
[33] All conclusions below remain if we exclude the few observations registered on Saturdays and Sundays.

Fig. 2: Frequency of immigrants around September 11, 2008



and Tuesdays. We argue that this distribution suggests that immigrants did not sort around the eligibility date. Our interpretation is strengthened by Cattaneo et al.'s formal density test,[34] which provides no evidence of a discontinuity in the timing of arrival (p=.32).

Next we run the analysis on a set of pre-determined covariates. If immigrants are as-if randomly assigned to either side of the eligibility date, we should not observe any discontinuities with respect to background characteristics: immi-

[34] Cattaneo et al. 2016.

grants who were barely eligible should resemble immigrants who were ineligible to vote in the 2011 elections. In Table 1, we report estimates from the RD design, using optimal bandwidths. Additionally, in the Appendix we graph each covariate using first and second order polynomials (Figure A-7) and assess alternate bandwidths.[35] In addition to exploring discontinuities for age, gender, civic status, and regional background, we rely on information on voter turnout in different immigrant groups in the 2007 election to calculate the expected probability of turnout for each immigrant.[36] Using these probabilities, we test for imbalance across the threshold. We find no evidence that those individuals who received early eligibility to vote were drawn disproportionately from nationalities with high participation rates. We also assess balance for all large nationality groups within the sample (Table A-15). Across each specification, we find no significant discontinuities, suggesting that immigrants on either side of the arbitrary cutoff are statistically similar.[37]

[35] We follow Skovron and Titiunik 2015: 28 and use different bandwidths for each predetermined outcome because the unknown functional form might differ across variables. We present estimates using optimal bandwidth obtained in the subsequent analysis of turnout as a robustness check (Table A-2).

[36] Statistics Norway. For the 20 largest nationalities, this probability is country-specific; for the remainder of those in the sample, it is region-specific.

[37] Given that immigrant flows vary over time, we restrict the initial observation window to one year.

TABLE 1 *RD on pre-determined covariates*

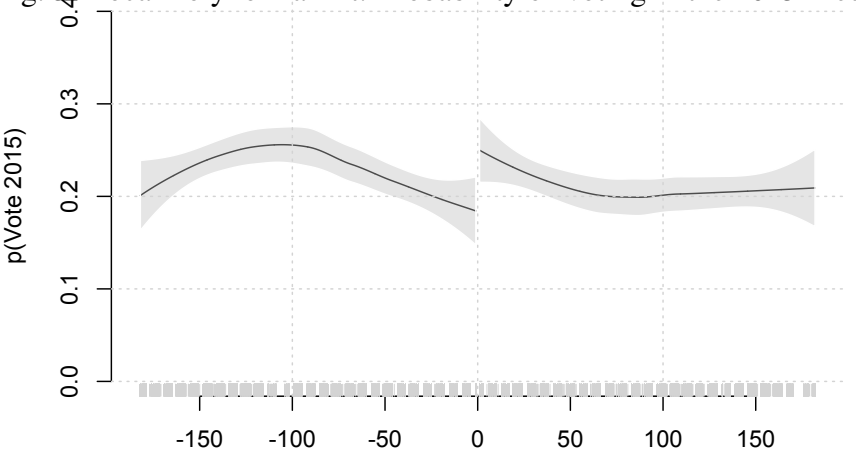
Covariate	Bandwidth (Days)	Treatment coefficient	SE	p-val	Effective N
Age	45	0.290	0.721	.71	3032
Male	52	0.015	0.036	.67	3498
Unmarried	65	-0.020	0.031	.51	4158
European country	34	0.012	0.041	.61	2439
East European country	43	-0.021	0.039	.63	2915
African country	38	0.010	0.019	.59	2653
Asian country	62	0.027	0.028	.35	3929
Expected turnout	45	-0.006	0.007	.45	3032

Local polynomial.

Did Early Eligibility Influence Electoral Participation?

Given that individuals are similar on either side of the threshold, we can plausibly identify whether early access to voting rights generated differences in subsequent political participation. Figure 2 fits a flexible local polynomial to either side of the cutoff, using a half-year window. A clear discontinuity in 2015 electoral participation is visible between individuals who barely missed eligibility for the 2011 elections (left hand side of plot), and those who were eligible to vote (right-hand side of plot). Given the balance in background characteristics between immigrants close to the cut-off (Table 1), this discontinuity indicates the effect of early eligibility on subsequent behavior.

Fig. 3: Local Polynomial Fit: Probability of Voting in the 2015 Election



Loess, shaded regions indicate 95% confidence intervals. n=11,068

In Table 2, we formally estimate the degree to which early access to voting rights affected turnout in the 2015 local elections, using the RD approach. Given the linear trend of the data close to the threshold, we use single order polynomials (see Appendix Figure A-2). We first present results using a half-year cutoff on either side of the discontinuity, and then switch to optimal bandwidths.[38] Using the latter approach, the local linear estimate suggests that early access to voting rights increased subsequent participation by 5.9 percentage points, plus or minus 5.6 percentage points. However, when we switch from the mean squared error (MSE) criteria to the Imbens-Kalyanaraman coverage error rate (CER)

[38] See Appendix Figure A-4: the results are robust to multiple bandwidths.

criteria, which utilizes narrower bandwidths, the treatment estimate drops to 3.9 percentage points, and the effect is less precisely estimated. Thus, while the data suggest a positive effect of eligibility to vote on subsequent electoral participation, the variance is likely too large to allow us to conclude that there is a global effect of early access on subsequent electoral participation for all categories of foreign residents.

TABLE 2 *RD on the probability of voting in the 2015 election (All Immigrants)*

Criteria	Bandwidth (Days)	Treatment coefficient	SE	p-val	Effective N
1 Year Window	183	0.043**	0.016	.01	11068
MSE	63	0.059**	0.028	.03	4092
CER	40	0.039	0.035	.27	2740

Local polynomial. ** p<0.05.

Early Access and Prior Exposure to Democracy

Immigrants in our sample are from very different backgrounds, which leads us to expect heterogeneity in response to early voting rights. Given that the focus of our analysis is on political behavior, we are particularly interested in the degree to which immigrants may already be habituated into modes of democratic participation. Although some immigrants within our sample arrived in Norway

from dictatorships, war-torn states, or fragile democracies, others migrated from countries characterized by political institutions similar to Norway. According to our argument, immigrants with less prior exposure to democratic institutions should be more responsive to the treatment, given that these groups encounter a greater contextual shock that may promote subsequent behavioral change.

We use two approaches to measure foreign residents' prior experiences with democracy.[39] The first is a 'cultural' approach. Drawing on data on each immigrant's nationality, we leverage an indicator provided by the Economist Intelligence Unit (EIU) to measure the strength of democratic culture in each origin country in 2007 (the year prior to arrival). Using this indicator, we split the sample according to whether individuals arrived in Norway from countries with weak and strong democratic cultures, respectively.[40] Second, we adopt an institutional approach based on a dichotomous coding of democracy within each origin country. We utilize Boix, Miller, and Rosato's 'Political Regimes Dateset' to measure the duration of democracy for each country year.[41] Using this classification of democratic years, we follow Fuchs-Schündeln and Schündeln's[42]

[39] We analyze alternative approaches in the Online Appendix.

[40] The EIU Democratic Political Culture indicator is derived expert coding and survey results. The final score is standardized and ranges from 0-10. In the main analysis we use a cutoff of 6.5 to distinguish between weak and strong democratic cultures. In the Appendix, we provide results for alternate cutoff points.

[41] Boix et al. 2013.

[42] Fuchs-Schündeln and Schündeln 2015.

motivation and derive an individual-level measure of democratic exposure in 2008. To avoid arbitrary cutoffs, we rely on a binary measure that indicates whether an individual was born and raised in a stable democracy, or whether they experienced periods of dictatorship prior to emigrating to Norway.

Table 3 presents RD estimates for each subsample.[43] The results suggest that immigrants' level of democratic capital matters: early eligibility sharply increases subsequent participation rates for individuals with low exposure to stable democracy. For instance, the results suggest that immigrants from countries with weak democratic cultures were 8.0 percentage points, plus or minus 4.4 percentage points, more likely to vote in the 2015 elections when compared to similar immigrants who missed the 2011 eligibility cutoff. These results are consistent with those obtained from the institutional approach, which suggests that individuals who experienced periods of non-democracy were 6.8 percentage points more likely to vote following early access to political institutions. Importantly, null results are obtained for individuals who migrated from strong democratic cultures or were raised in a democracy, suggesting that the positive effect of early eligibility may be conditional on prior exposure to democratic

[43] See Table A-5 for balance tests for each subset, Figure A-5 for the distribution of the data, and Figure A-6 for point estimates obtained from alternate bandwidths. Note that subsetting the sample does not violate the assumptions of the design, given that the eligibility date is plausibly exogenous for each sub-group of migrants.

norms and institutions.[44] This conditional relationship is visible in Figure 4, which reveals sharp discontinuities for the group of migrants with low exposure to democracy, and no discontinuities for foreign residents originating from developed democracies.

Given that immigrants likely differ along other dimensions correlated with regime type, the research design does not permit us to establish that prior exposure to democracy is the only factor shaping divergent responses to voting rights extension.[45] However, the effect heterogeneity does not appear to be driven by observable confounders. Similar results are obtained when examining discontinuities in residuals after partialling out pre-treatment background characteristics. Moreover, as Appendix Table A-7 demonstrates, the findings remain consistent when the analysis is restricted to young immigrants within each subsample.[46]

[44] The imprecise estimates in the democratic sample imply that differences in treatment effects across the subsamples are not significant at conventional levels. However, the point estimates in the democratic sample are effectively zero (see Online Appendix for consistent results using alternative specifications. When using a more inclusive definition of democratic culture, the difference is significant at conventional levels). The null findings in the democratic sample are consistent with no habituation effects among citizens in Scandinavian countries, e.g. Bhatti et al. 2016.

[45] From a policy perspective, this is non-problematic. When assessing the effectiveness of interventions, collinearity implies that policymakers can reasonably expect individuals from less developed countries to differ on a variety of background characteristics.

[46] Given the established importance of voting early in the lifecourse, differences in age distributions could potentially explain the result. However, ages within each subsample are quite similar. The group from stable democracies is only slightly older (mean: 40 vs. 38), while in the democratic capital analysis the mean is equivalent across both samples (39).

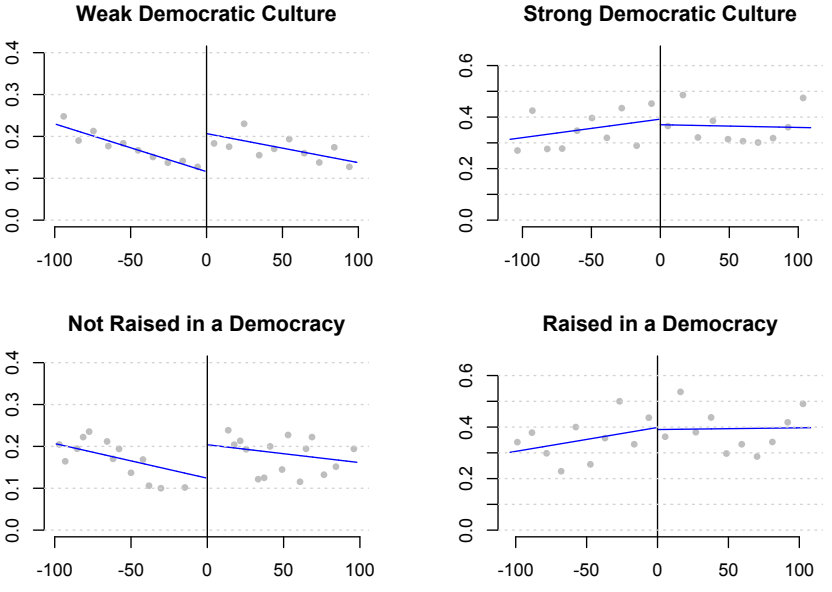
TABLE 3 *RD on the probability of voting in the 2015 election (Subset Analysis)*

Polynomial order	Criteria	Bandwidth	Treatment coefficient	SE	p-val	Effective N
			Weak democratic culture			
1	MSE	98	0.080***	0.022	.00	4656
2	MSE	126	0.071**	0.029	.02	5797
1	CER	60	0.070**	0.029	.02	2943
			Strong democratic culture			
1	MSE	99	0.004	0.054	.94	1407
2	MSE	135	0.009	0.067	.90	1913
1	CER	71	0.013	0.066	.85	966
			Not Born in a Democracy			
1	MSE	99	0.079***	0.022	.00	4879
2	MSE	123	0.067***	0.029	.02	6067
1	CER	61	0.066**	0.029	.02	3085
			Born in a Democracy			
1	MSE	109	0.005	0.066	.94	897
2	MSE	145	-0.013	0.082	.88	1235
1	CER	73	-0.000	0.080	.99	621

Local polynomial. *** p<0.01, ** p<0.05, * p<0.1.

The results are also robust to a variety of additional tests. When performing a placebo evaluation that alters the timing of cutoff eligibility, no significant treatment effects can be detected (Table A-8), suggesting that the findings do not represent a false positive. Parallel results are obtained for a placebo estimating

Fig. 4: RD on the probability of voting in the 2015 election (Subset Analysis)



First order polynomials, with binned means.

the treatment effect for a group unaffected by the cutoff, namely immigrants from the Nordic countries who receive voting rights three months after arrival (Table A-8). Finally, the results remain similar when using alternate measures of democratic exposure (Table A-11), or when further subsetting the data. As seen in Table A-12, for instance, the largest treatment effects are visible for those immigrants arriving from dictatorships.

DISCUSSION

What explains the positive relationship between early access to voting rights and subsequent political engagement? Evaluating the evidence, we argue that the results are consistent with a process of habit disruption and formation. Upon exposure to new institutions and norms, immigrants are likely to establish new modes of interaction with the state and society.[47] Yet this period is temporally limited: as immigrants become accustomed to life in the host society, the opportunity for habit disruption narrows, and initial modes of behavior are likely to persist in a path-dependent manner.

Before evaluating further implications of this argument, we focus on an alternative explanation and assess whether the gap in political participation follows from practical advantages possessed by early voters. For instance, it is possible that immigrants who were eligible for the 2011 elections received additional information or experienced higher levels of mobilization than their counterparts who missed the 2011 cutoff. But while gaps in information or mobilization have been found to influence long-term patterns of political participation in other contexts,[48] this mechanism is unlikely to be driving the results in the Norwegian case. Voter registration is automatic and immigrants receive a letter in the

[47] White et al. 2014; Voicu and Comsa 2014; Voicu and Serban 2012.
[48] De Rooji 2012.

mail indicating the location of their polling place prior to every election.[49] It is perhaps more plausible that new immigrants or those from non-democratic states were selectively mobilized by local governments and unions. Given the availability of registry data indicating eligibility, it is possible that local actors focused mobilization efforts on specific types of immigrants as an informational shortcut.[50] This mechanism would most directly affect turnout in the first election in our sample (2011), but if outreach is targeted rather than broad, this differential may have persisted over time.

Although direct data on local mobilization efforts does not exist, we assess this hypothesis using administrative data. First, we evaluate whether individuals received social assistant payments from local governments. A discontinuity will partially reflect (lack of) labor market integration. However, social assistance is means-tested and indexed at the local level, implying that local politicians have some discretion to influence levels and access to social assistance transfers. If immigrants were systematically mobilized by the delivery of local transfers and

[49] Immigrants who were eligible in 2011 received two such letters (one in 2011, and one in 2015). A recent field experiment finds no effect on turnout from different versions of these letters, suggesting that differences in letter exposure is unlikely to explain the results. The results are available from the authors upon request.

[50] A potential explanation for the large treatment effect among individuals from weak democracies is that these individuals felt compelled to vote. However, turnout among this group was quite low, and some argue that concerns about ballot secrecy might depress turnout among immigrants from non-democracies. Consistent with this argument, experimental evidence shows that a letter informing immigrant voters about the high level of ballot secrecy has a positive effect on turnout (see Bergh et al. 2016).

services, we should observe a discontinuity in benefit recipiency. Second, we evaluate whether a discontinuity exists in union membership. Although union membership is similarly conditional on employment, unions play a mobilizing role by providing political information to members. The results for both measures appear in Table A-9, measured prior to the 2011 election. No clear discontinuities are visible for union membership and benefit recipiency across the subsamples, suggesting that if mobilization was active, it was broad rather than targeted to specific types of immigrants.

If immigrants with early access to voting rights do not appear to have experienced discontinuities with respect to information or mobilization, the process driving increased engagement over time likely reflects individual-level habituation to democratic society. To further evaluate this possibility, we turn to several alternative measures of integration. We first assess two validated outcomes present in the administrative data: whether an immigrant was successful in obtaining employment, and whether they enrolled in a voluntary education program (both measured five years after arrival).[51] From an integration perspective, the latter indicator is particularly important: educational programs are likely to

[51] 2013 is the most recent available administrative dataset that covers these outcomes. Employed is defined as having earnings above 1 G. G ("Grunnbeløp") is a cut-off point used to calculate pension benefits.

substantially increase the speed of language acquisition and expand employment opportunities beyond sectors typically dominated by new immigrants.

As seen in Table 4, early access to voting rights appeared to have no discernable effect on labor market behavior. However, immigrants from non-democratic contexts were substantially more likely to be enrolled in continuing education if they were eligible to vote in the 2011 elections (+ 5.8%). Although the data does not permit a direct assessment of the causal relationship that underlies this pattern, it suggests increased motivation to integrate among immigrants who may otherwise face barriers to success within Norwegian society.

Given that administrative registries only cover a subset of relevant outcomes, we next evaluate survey data to assess other dimensions of engagement. This approach has its limits: representative surveys only include a small number of immigrants and tend to ask the year rather than the precise date of arrival in Norway. Nevertheless, two recent surveys – Innbyggerundersøkelsen ("The Citizen Survey") from 2013 and 2015 – permit a a coarse analysis of spillover effects to other aspects of political and civic integration. In these surveys, immigrants were asked the number of years they have lived in Norway, allowing us to construct two cohorts based on reported time of arrival. We define the treatment group as recently eligible immigrants, which includes those arriving in 2007, as well as those arriving in 2008 that report being eligible to vote in 2011. By

TABLE 4 *RD on administrative outcomes measured in 2013*

Outcome	Bandwidth	Treatment coefficient	SE	p-val	Effective N
Weak democratic culture					
Employment	115	-0.001	0.023	.96	7528
Continuing Education	93	0.058***	0.017	.00	6303
Strong democratic culture					
Employment	157	0.043	0.038	.27	2792
Continuing Education	108	0.003	0.028	.90	1868
Not Born in a Democracy					
Employment	113	-0.005	0.023	.85	7607
Continuing Education	88	0.067***	0.017	.00	6139
Born in a Democracy					
Employment	111	0.043	0.055	.43	1113
Continuing Education	103	-0.015	0.036	.67	1052

Local polynomial. *** p<0.01, ** p<0.05, * p<0.1. This sample is based on the total population of immigrants (arriving around the cut-off date in 2008) who lived in Norway in the beginning of 2013. This sample is larger than the one used in the analysis of turnout because of out-migration between January 2013 and September 2015, and because of the eligibility criteria of continued residency in the 2015 sample.

contrast, the control group refers to those arriving in 2009 and those arriving in 2008 that report not being eligible.

We regress the treatment group indicator on five outcomes. Three outcomes measure aspects of political behavior: *Political interest*, whether the respondent

has *contacted a local politician* and whether the respondent has attempted to *influence* a decision of the local government. The fourth outcome is general *political trust* in elected Norwegian officials, while the final outcome, *civic participation*, counts the number of civic associations the respondent has joined (re-scaled between 0 and 1).

Table 5 presents the regression results. Although the coarse data on time of arrival and small sample sizes imply that the findings should be viewed with caution, the results are suggestive of weak spillover effects. The difference between the treatment and control group is imprecisely estimated for several dimensions, but statistically significant for attempts to influence the local municipal council, trust in politicians, and the civic participation index.[52] When viewed in tandem with the higher rates of political participation observed among this group, these findings are consistent with the argument that initial habits of engagement with the host society (or lack thereof) may persist.

CONCLUSION

Faced with expanding foreign resident populations, European policymakers have increasingly sought means to promote immigrant integration. This process has

[52] The results are similar when excluding the total years of stay, given that temporal trends are quite flat. See Appendix Table A-17. The findings also remain consistent when omitting covariates.

TABLE 5 *Survey Evidence: Political and Social Integration (OLS)*

	Political interest	Contacted local politician	Influence municipal council	Political trust	Civic participation
Early Access	0.079	0.022	0.066*	0.145**	0.046**
	(0.075)	(0.031)	(0.036)	(0.075)	(0.021)
Observations	564	567	554	567	571
Controls	Yes	Yes	Yes	Yes	Yes
Survey Dummy	Yes	Yes	Yes	Yes	Yes
Length of Stay	Yes	Yes	Yes	Yes	Yes
Mean outcome	.41	.05	.07	.47	.11
SD outcome	.49	.21	.26	.50	.12

Robust standard errors in parentheses. All regressions include controls for age, gender, level of education, year of arrival, and a survey-year dummy. For similar results when examining 2008 arrivals only, see Table A-10. ** p<0.05, * p<0.1. Respondents are asked to express their *political interest* and *political trust* on a scale from -3 to 3. We code positive answers as 1, zero otherwise. *civic participation*: Respondents are asked about membership in resident associations, sports associations, music associations, trade unions, political associations, religious associations, and "other associations". To ease interpretation we re-scaled between 0 (member of none) and 1 (member of all types of associations).

resulted in the widespread extension of social and civic rights to migrants,[53] and more recently, the extension of local voting rights to foreign resident popula-

[53] Sainsbury 2012.

tions. Although this trend is increasingly prevalent and enshrined in EU law, the effects of extending the franchise to foreign voters nevertheless remain unclear. Does the extension of local voting rights foster immigrant incorporation? And to what extent does the timing and conditions attached to voting rights influence subsequent integration trajectories?

This paper builds upon a growing literature that emphasizes that early access to institutions is consequential. Leveraging a natural experiment related to the exogenous timing of elections relative to immigrant arrival dates, we have demonstrated a causal relationship between early access to local voting rights and subsequent patterns of political engagement. Immigrants who received voting rights in 2011 participated at substantially higher rates in the next round of elections, relative to similar immigrants who missed the eligibility cutoff. These effects appear to be broad rather than narrow, with spillovers to several alternate integration indicators. By demonstrating a clear connection between early access and subsequent engagement, these results thus nuance existing findings that immigrants' political integration is shaped by socioeconomics, length of stay, and citizenship regimes.

However, our results suggest important heterogeneity in the effect of early access to institutions. Individuals from unstable democracies or dictatorships experience clear, positive effects. In contrast, those migrants with prior exposure to stable democracy do not appear to adjust their political participation in

response to early access. While one might interpret this conditional relationship as evidence that early access to voting does not unambiguously improve outcomes, it is important to keep in mind that those immigrants who clearly benefit from early access – namely, those hailing from war-torn or weak states – are precisely those who often face the highest barriers to successful integration. As a result, a positive interpretation of our results is that the early extension of political rights provides a useful policy tool to improve vulnerable groups' engagement and interaction with the host society. Given that all immigrants within our sample eventually receive voting rights, these treatment effects likely represent a lower bound on the integration effect of extending voting rights to foreign populations.

These findings are subject to some limitations. First, while our data demonstrates that immigrants have higher rates of turnout in local elections seven years after arrival, the time horizon is necessarily limited by the recency of the data. We expect political participation to persist, but future studies are needed to assess truly long-term effects. Second, our register-based measures cover a subset of outcomes under the rubric of integration. It is possible that engagement with local democracy stimulates other aspects of integration, such as social networks and identification with the host society. Our study suggests a useful foundation for subsequent analysis linking eligibility data with larger, targeted surveys of the immigrant population.

Despite these limitations, our findings have direct policy implications. When seeking to incorporate immigrants, policymakers should be cognizant of the fact that opportunities to integrate may be temporally limited. If foreign residents face delays in accessing institutions, this exclusion may persist, stymying subsequent efforts to incorporate these groups. Thus, while the question of whether to extend political rights to migrants remains a normative and practical question, our empirical results suggest that countries that choose to extend such rights should do so as expediently as possible.

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Online Appendix
"Voting Rights and Immigrant Incorporation: Evidence from Norway" (BJPS)

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Online Appendix

The 27 municipalities within the sample are (ordered by population size, from large to small): Oslo, Bergen, Trondheim, Stavanger, Bærum, Fredrikstad, Drammen, Sandnes, Sarpsborg, Asker, Skien, Skedsmo, Bodø, Sandefjord, Larvik, Tønsberg, Karmøy, Porsgrunn, Haugesund, Ålesund, Mandal, Vefsn, Hammerfest, Re, Tynset, Radøy, and Bremanger. As seen in the table below, the immigrants in these 27 municipalities had on average higher earnings and much higher employment levels in 2013 than immigrants residing elsewhere in Norway. These differences partly reflect labor market differences (there are differences in the same direction if we compare native Norwegians), but the main reason is that the cities attract a much higher number of labor immigrants.

Table A-1: Characteristics of immigrants born before 1994 which arrived in Norway in 2008. Outcomes are measured in 2013.

	Municipality included in our sample	Rest of Norway
Employed	.63	.39
Total earnings (NOK)	255106	141019
University level education	.33	.18
Age	36.49	37.24

Employed is defined as having earnings above 1 G. G (grunnbeløp) is a cut-off point used to calculate pension benefits. The number is adjusted by the Norwegian Storting each year. In 2013 it was 85245 NOK.

Table A-2: RD on pre-determined covariates using the optimal bandwidth from the voting analysis

Covariate	Bandwidth (Days)	Treatment coefficient	SE	p-value
Age	63	-0.476	0.611	.44
Male	63	0.015	0.033	.46
Unmarried	63	-0.022	0.031	.49
European country	63	-0.033	0.031	.29
East European country	63	-0.040	0.033	.23
African country	63	0.018	0.015	.22
Asian country	63	0.027	0.028	.34
Expected turnout	63	-0.005	0.006	.40

Local polynomial (single order).

Table A-3: Descriptive statistics for outcomes in tables 1-6

	Eff. N	Mean	Std. Dev.
Tables 1-2.			
Vote	4,092	.22	.41
Male	3,498	.54	.50
Age	3,032	38	9
Unmarried	4,203	.31	.46
European country	2,439	.65	.48
East Eur. country	2,915	.49	.50
Asian country	3,929	.21	.41
African country	2,653	.06	.23
Expected turnout	3,032	.29	.09
Weak democratic culture, tables 3-5.			
Vote	4,656	.17	.37
Social assistance	6,538	.10	.30
Union member	9,990	.12	.32
Employment	7,528	.62	.49
Continuing education	6,303	.11	.31
Strong democratic culture, tables 3-5.			
Vote	1,407	.36	.48
Social assistance	964	.06	.24
Union member	1,907	.13	.33
Employment	2,792	.64	.48
Continuing education	1,868	.10	.30
Not born in democracy, tables 3-5.			
Vote	4879	.17	.38
Social assistance	6,325	.09	.29
Union member	7,970	.12	.32
Employment	7,607	.62	.49
Continuing education	6,139	.11	.31
Born in democracy, tables 3-5.			
Vote	897	.37	.48
Social assistance	1,088	.06	.23
Union member	1,659	.10	.30
Employment	1,113	.65	.47
Continuing education	1,052	.10	.30
Survey data, Table 6.			
Political interest	564	.41	.49
Contacted local politician	567	.05	.21
Influence municipal council	554	.07	.26
Political trust	567	.47	.50
Civic participation	571	.11	.12

Table A-4: Voter turnout for selected immigrant countries, 2003-2015.

	2003	2007	2011	2015
Total turnout	59	62	65	60
Foreign nationals (all)	34	36	32	29
Western nationals	39	42	33	28
Non western nationals	25	30	30	28
Large sending countries				
Afghanistan	-	32	35	32
Bosnia-Herzegovina	20	18	18	15
France	45	45	46	50
Germany	51	48	39	40
Iraq	19	23	23	27
Iran	23	24	-	30
Netherlands	47	53	56	-
Pakistan	40	36	44	33
Poland	25	23	8	7
Russia	20	27	26	21
Serbia and Montenegro	17	16	16	-
Somalia	23	36	51	48
Thailand	23	31	33	33
Turkey	24	22	23	32
United Kingdom	40	41	46	43
United States	46	45	46	42

Source: Election statistics, Statistics Norway. The sample size for each election is between 200-250 for each country group.

Figure A-1: RD on pre-determined covariates, first order polynomials. Optimal bandwidths (CCT)

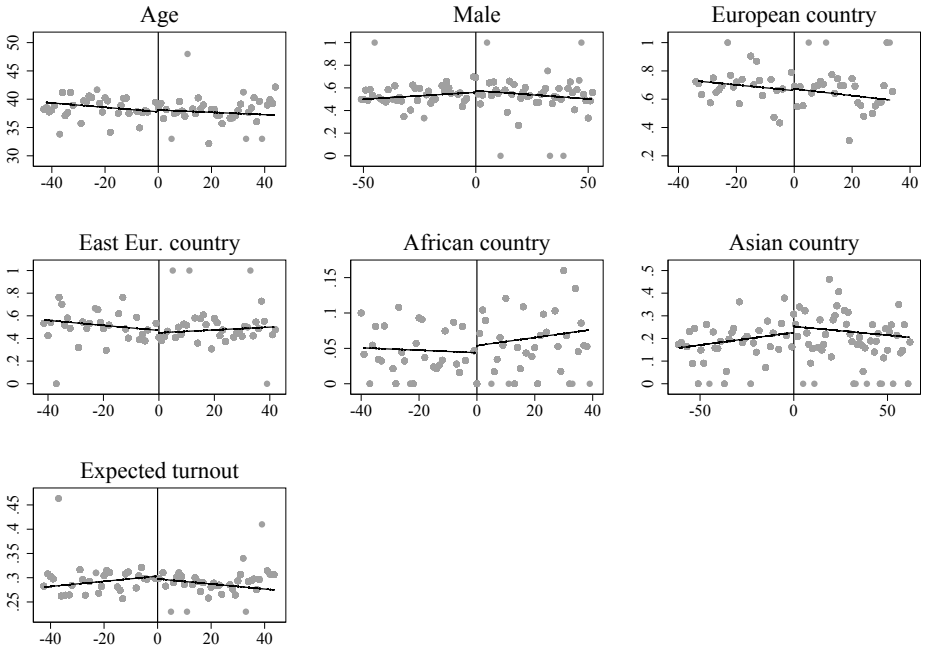


Figure A-2: RD on pre-determined covariates, second order polynomials. Optimal bandwidths (CCT)

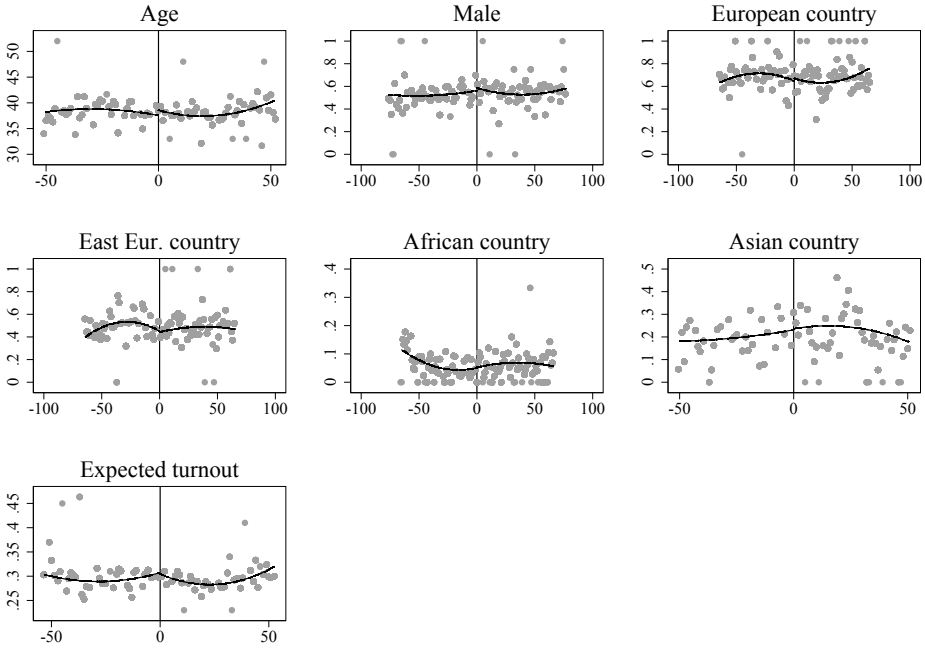
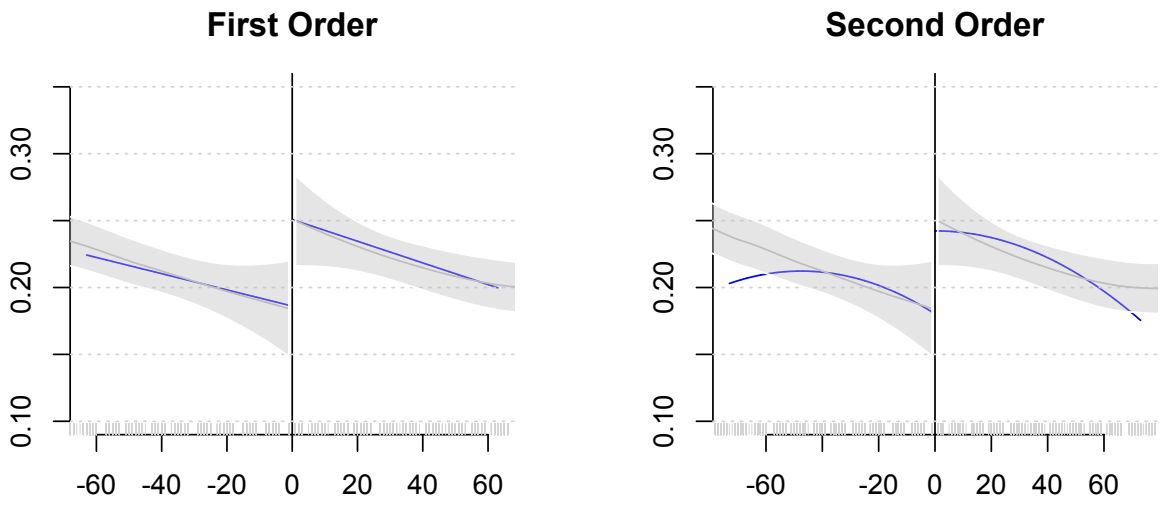
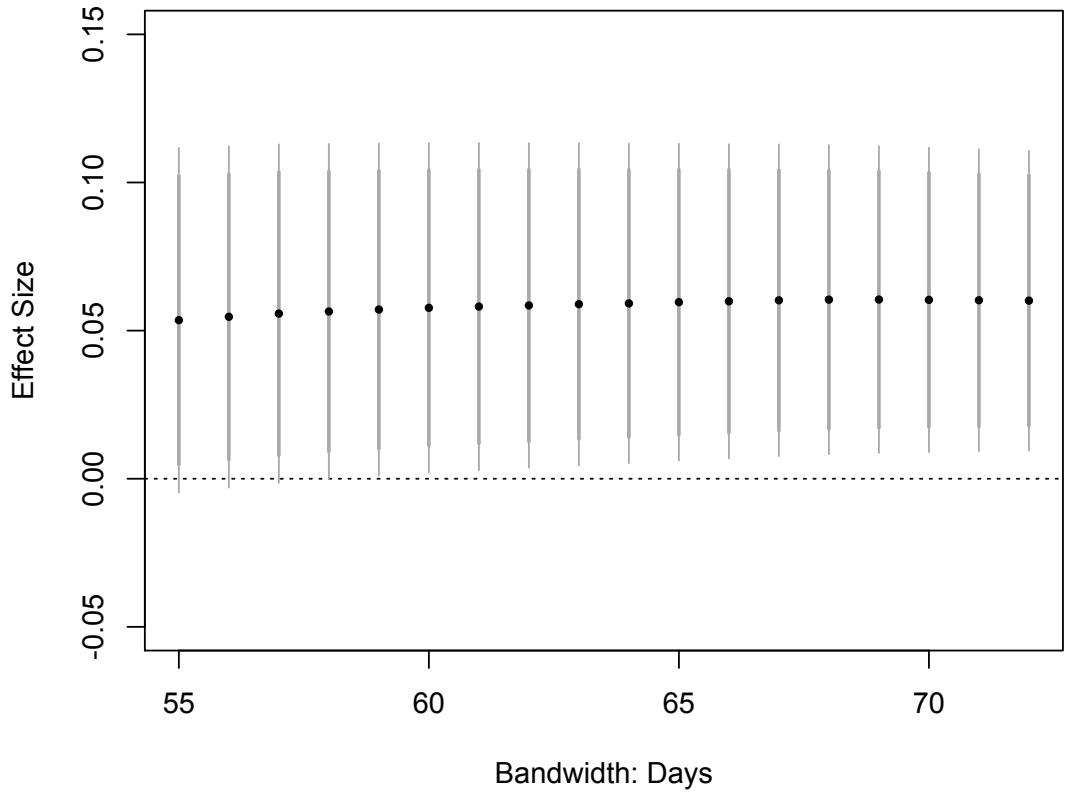


Figure A-3: RD on the probability of voting in the 2015 election (All Immigrants)



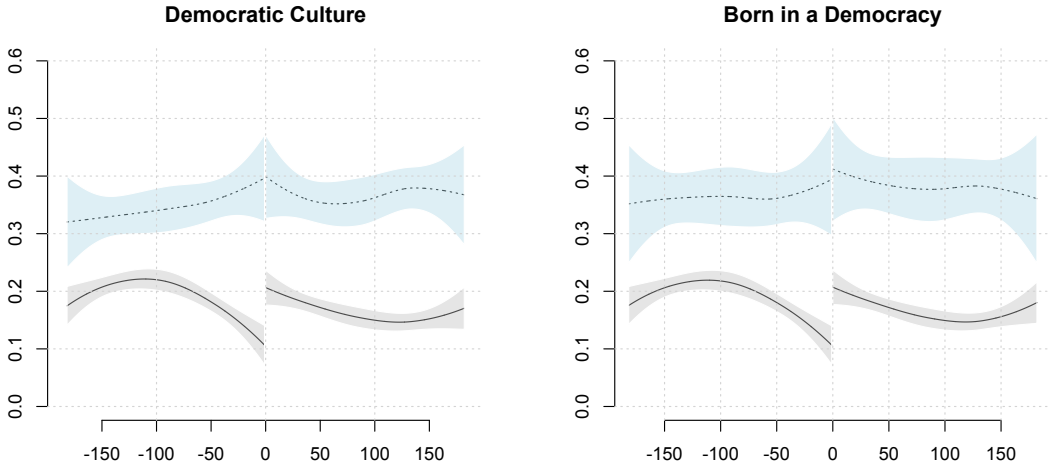
Shaded regions represent loess fits; blue lines indicate first and second order polynomials fit with MSE Optimal Bandwidths.

Figure A-4: RD on the probability of voting in the 2015 election (Multiple bandwidths)



Thick lines: 90% confidence intervals. Thin lines: 95% confidence intervals. The optimal bandwidth according to the CCT algorithm is 63.

Figure A-5: Loess fits: discontinuities for democratic subsets



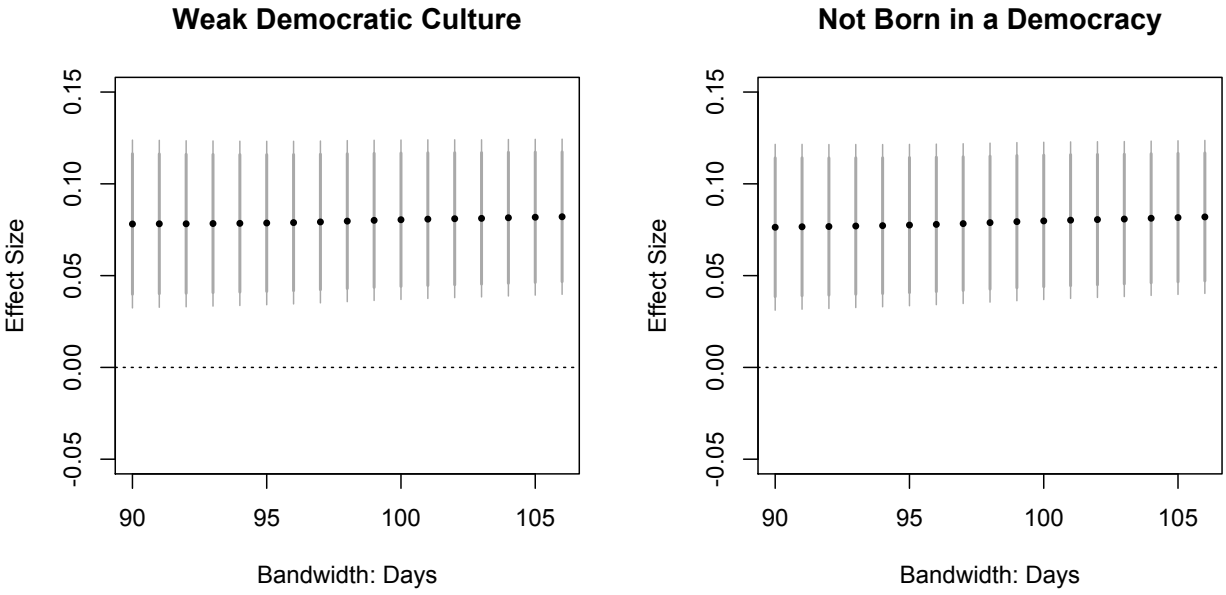
Note that due to changing immigrant flows over time, the expected level of turnout is not stable across the full distribution. Immigrant background characteristics are only balanced in close proximity to the eligible date.

Table A-5: RD on pre-determined covariates - Subset Analysis

Covariate	Bandwidth	Treatment coefficient	SE	p-val
Weak democratic culture				
Male	99	0.019	0.032	.55
Age	111	-0.541	0.549	.32
Unmarried	89	-0.020	0.030	.51
Not Born in a Democracy				
Male	97	- 0.001	0.030	.96
Age	104	-0.806	0.536	.13
Unmarried	84	-0.026	0.030	.38

Local polynomial (single order). Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). We exclude the nation of origin dummies given that we subset directly on national characteristics.

Figure A-6: Subset Results (Multiple bandwidths)



Thick lines: 90% confidence intervals. Thin lines: 95% confidence intervals.

Table A-6: Subset Results with Covariates

Polynomial order	Criteria	Bandwidth	Treatment coefficient	SE	p-val
		Weak democratic culture			
1	MSE	97	0.079***	0.022	.00
2	MSE	124	0.070**	0.029	.02
1	CER	60	0.072**	0.029	.01
		Not Born in a Democracy			
1	MSE	99	0.078***	0.021	.00
2	MSE	129	0.081**	0.028	.02
1	CER	61	0.067**	0.028	.02

Second order local polynomials. Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). Covariates include age, gender, and marital status. *** p<0.01, ** p<0.05, * p<0.1.

Table A-7: Subset Results when sample is restricted to young immigrants

	Bandwidth	Treatment coefficient	SE	p-val
Weak democratic culture	89	0.092***	0.031	.00
Strong democratic culture	94	0.007	0.082	.93
Not Born in a Democracy	87	0.100***	0.032	.00
Born in a Democracy	135	-0.009	0.080	.91

Local polynomials. Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). Young is defined as below the mean age of immigrants (38 years of age). *** p<0.01, ** p<0.05, * p<0.1.

Table A-8: Placebo Tests

Placebo Cutoff	Bandwidth	Treatment coefficient	SE	p-val
Panel A: Weak democratic culture				
Right side placebo	74	0.001	0.042	.97
Left side placebo	49	-0.058	0.037	.12
Panel B: Not Born in a Democracy				
Right side placebo	58	0.007	0.027	.81
Left side placebo	44	-0.049	0.039	.21
Panel C: Nordic immigrants				
Sept 11, 2008 placebo	90	-0.015	0.073	.83

Local polynomial (single order). Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). In Panels A and B we follow Imbens and Lemieux (2008: 632) closely and conduct placebo cut-off analyses at both sides of the cut-off. In the right (left) side cut-off analysis we include only observations from the right (left) side of the cut-off to avoid including the true discontinuity in the analysis. The fake cut-off is the median value at each side, which ensures that we maximize the power of the test. In Panel C we estimate the treatment effect for Nordic immigrants. This is a placebo analysis because Nordic citizens were not affected by the cutoff.

Table A-9: Mobilization: RD on alternate outcomes

Outcome	Bandwidth	Treatment coefficient	SE	p-val	Effective N
Weak democratic culture					
Social assistance	98	-0.019	0.015	.23	6538
Union member	149	0.015	0.013	.25	9990
Strong democratic culture					
Social assistance	51	0.033	0.025	.18	964
Union member	111	0.025	0.032	.44	1907
Not Born in a Democracy					
Social assistance	91	-0.011	0.015	.48	6325
Union member	119	0.007	0.015	.61	7970
Born in a Democracy					
Social assistance	108	-0.033	0.027	.22	1088
Union member	158	0.035	0.032	.27	1659

Local polynomial. Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1. This sample is based on the total population of immigrants (arriving around the cut-off date in 2008) who lived in Norway in the beginning of 2013. This sample is larger than the one used in the analysis of turnout because of out-migration between January 2013 and September 2015, and because of the eligibility criteria of continued residency in the 2015 sample.

Table A-10: Survey Evidence: Political and Social Integration, 2008 Arrivals (OLS)

	Political interest	Contacted local politician	Influence municipal council	Political trust	Civic participation
Early Access	0.099	0.024	0.061	0.132*	0.045**
	(0.079)	(0.031)	(0.037)	(0.078)	(0.021)
Observations	180	181	176	180	182
Controls	Yes	Yes	Yes	Yes	Yes
Survey Dummy	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses. All regressions include controls for age, gender, level of education, and a survey-year dummy. *** p<0.01, ** p<0.05, * p<0.1.

Alternate Measures of Democratic Exposure

We use Varieties of Democracy's "electoral regime index" (Coppedge et al. 2016) to classify country years as being electoral democracies in each year. Using this classification of democratic years, we follow Fuchs-Schündeln and Schündeln (2015) closely and derive an individual level measure of democratic capital in 2008. This stock variable is the accumulated years of democracy over ones' lifetime, but where previous years of democratic experience depreciates by two percent each year.

Table A-11: RD on the probability of voting in the 2015 election

Polynomial order	Criteria	Bandwidth	Treatment coefficient	p-val
Low level of democratic capital				
1	MSE	67	.091*	.06
2	MSE	77	.073	.27
1	CER	67	.091*	.06
High level of democratic capital				
1	MSE	56	.021	.69
2	MSE	84	.020	.72
1	CER	37	.018	.83

Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1.

Examining the last 30 years, we use the Boix-Miller-Rosato (2013) dichotomous dataset to code countries as dictatorships (0 years of democracy), stable democracies (30 years of democracy), or new democracies (>0, <30 years of democracy).

Table A-12: RD on the probability of voting in the 2015 election.

Polynomial order	Criteria	Bandwidth	Treatment coefficient	SE	p-val
Dictatorships					
1	MSE	81	0.109	0.067	.11
2	MSE	112	0.153*	0.086	.08
1	CER	70	0.200*	0.111	.07
New Democracies and Dictatorships					
1	MSE	99	0.077***	0.022	.00
2	MSE	123	0.063**	0.030	.03
1	CER	61	0.065**	0.029	.02
Stable Democracies					
1	MSE	56	0.023	0.054	.66
2	MSE	84	0.037	0.065	.57
1	CER	37	0.039	0.065	.54

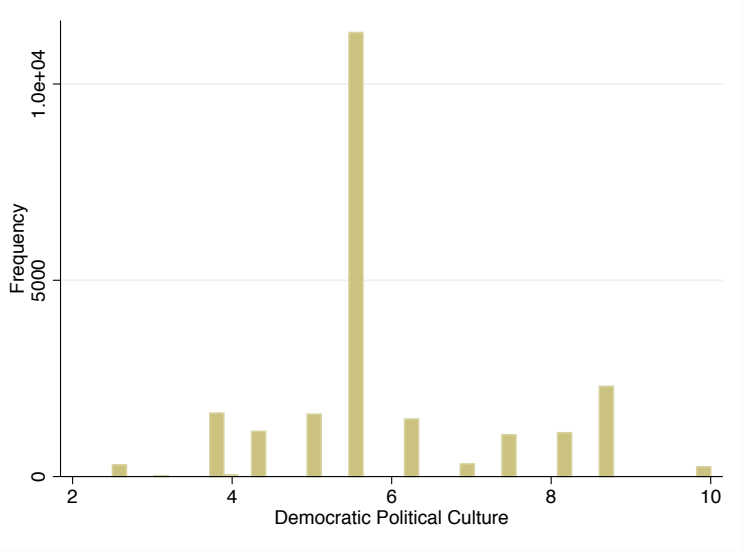
Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1.

Alternate Cutoffs for EIU Index

We use a cutoff of '6.5' on the EIU Democratic Culture Index. This classifies the following origin countries within our sample as having a weak democratic culture:

Afghanistan, Albania, Algeria, Angola, Argentina, Armenia, Azerbaijan, Bangladesh, Belarus, Bhutan, Bolivia, Bosnia and Herzegovina, Brazil, Bulgaria, Burkina Faso, Burundi, Cambodia, Cameroon, Chile, China, Colombia, Congo, Congo, Democratic Republic, Cote d'Ivoire, Croatia, Cuba, Cyprus, Djibouti, Dominican Republic, Ecuador, El Salvador, Eritrea, Ethiopia, Fiji, Gambia, Georgia, Ghana, Guatemala, Guinea, Honduras, India, Indonesia, Iran, Iraq, Jordan, Kazakhstan, Kenya, Kosovo, Kyrgyzstan, Laos, Latvia, Lebanon, Liberia, Libya, Lithuania, Macedonia, Malawi, Mexico, Moldova, Mongolia, Montenegro, Morocco, Myanmar, Nepal, Nicaragua, Niger, Nigeria, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Romania, Russia, Rwanda, Senegal, Serbia, Sierra Leone, Slovakia, Somalia, Sudan, Suriname, Tajikistan, Tanzania, Thailand, Togo, Trinidad and Tobago, Turkey, Turkmenistan, Uganda, Ukraine, Uzbekistan, Venezuela, Vietnam, Yemen, Zambia, Zimbabwe

Figure A-7: Distribution of EIU Scores



Moving the cutoff to a more inclusive definition of democractic culture (6) or more exclusive (7) does not affect the results.

Table A-13: EIU Cutoff of 6

Polynomial order	Criteria	Bandwidth	Treatment coefficient	SE	p-val
		Weak democratic culture			
1	MSE	100	0.079***	0.023	.00
2	MSE	180	0.063*	0.032	.05
1	CER	61	0.068**	0.030	.03
		Strong democratic culture			
1	MSE	110	-0.027	0.044	.54
2	MSE	128	-0.012	0.060	.84
1	CER	71	-0.010	0.055	.86

Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1.

Table A-14: EIU Cutoff of 7

Polynomial order	Criteria	Bandwidth	Treatment coefficient	SE	p-val
		Weak democratic culture			
1	MSE	102	0.079***	0.022	.00
2	MSE	119	0.059*	0.030	.05
1	CER	62	0.066**	0.028	.02
		Strong democratic culture			
1	MSE	93	0.000	0.058	.99
2	MSE	129	0.016	0.071	.82
1	CER	71	0.015	0.070	.84

Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1.

Nationality Balance

Immigrants within our sample arrived from 153 origin countries. Although the sample sizes are too small to test balance for a majority of these nationalities, in the following table we test balance for nationalities with at least 100 immigrants within the sample. The patterns are inconsistent with clustered arrivals around the eligibility threshold.

Table A-15: Balance by national origin (optimal bandwidths)

Country	estimate	se	p-val
Afghanistan	-0.006	0.016	0.72
Brazil	-0.008	0.008	0.32
Bulgaria	-0.002	0.014	0.89
China	0.021	0.028	0.45
Eritrea	-0.018	0.017	0.31
France	0.005	0.016	0.89
Germany	-0.034	0.028	0.23
Great Britain	0.038	0.021	0.08
India	0.024	0.023	0.31
Iran	0.021	0.013	0.11
Iraq	0.024	0.020	0.22
Latvia	0.004	0.012	0.77
Lithuania	-0.008	0.029	0.79
Netherlands	0.005	0.009	0.54
Pakistan	-0.010	0.017	0.55
Philippines	0.015	0.024	0.54
Poland	-0.077	0.051	0.13
Romania	0.021	0.020	0.31
Russia	0.017	0.019	0.39
Slovakia	-0.010	0.010	0.37
Somalia	0.004	0.007	0.56
Thailand	-0.034	0.018	0.05
Turkey	0.011	0.011	0.30
USA	-0.010	0.007	0.13

Table A-16: RD on the probability of voting in the 2015 election (Only nationalities with > 100 immigrants in the sample)

Criteria	Bandwidth (Days)	Treatment coefficient	SE	p-val	Effective N
1 Year Window	183	0.030*	0.017	.09	8648
MSE	73	0.063**	0.027	.02	3709
CER	46	0.070*	0.035	.05	2449

Local polynomial. Optimal bandwidths selected according to Calonico, Cattaneo, and Titiunik (2014). *** p<0.01, ** p<0.05, * p<0.1.

Survey Evidence: Length of Stay

The specification for the Citizen Survey includes a linear trend for the length of time within Norway. However, in the results that follow, we demonstrate that the number of years spent in the country does not predict an increase in the level of engagement, given that engagement trends are fairly flat across years of arrival. Given heterogeneity in voting eligibility, we restrict our analysis to either the treatment or control group. We report several different windows for each group. Observations in 2008 are separated into treatment and control on the basis of self-reported eligibility.

Table A-17: Effect of Additional Year in Norway

	Political interest	Contacted local politician	Influence municipal council	Political trust	Civic participation
Treated					
2007-2008	-0.054	-0.003	0.011	-0.113	-0.028
	(0.069)	(0.031)	(0.039)	(0.069)	(0.018)
2006-2008	-0.038	-0.030**	-0.022	-0.002	-0.004
	(0.037)	(0.012)	(0.020)	(0.037)	(0.011)
2005-2008	0.019	0.022	-0.005	-0.018	-0.008
	(0.024)	(0.014)	(0.013)	(0.024)	(0.006)
Control					
2008-2009	-0.025	-0.014	-0.030	-0.118*	-0.000
	(0.063)	(0.023)	(0.024)	(0.062)	(0.015)
2008-2010	-0.006	-0.017	-0.016	-0.037	0.006
	(0.030)	(0.013)	(0.013)	(0.030)	(0.006)
2008-2011	-0.013	-0.010	0.001	-0.014	-0.000
	(0.018)	(0.008)	(0.008)	(0.018)	(0.004)

Robust standard errors in parentheses. Coefficients represent the estimated effect of one additional year within Norway. All regressions include controls for age, gender, level of education, and a survey-year dummy. *** p<0.01, ** p<0.05, * p<0.1.

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