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Democratic Involvement and Immigrants' Compliance with the Law*

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Abstract

Many people are concerned about societal cohesion in the face of higher numbers of foreigners migrating to Western democracies. The challenge for the future is to find and adopt institutions that foster integration. We investigate how the right to vote in local elections affects immigrants' compliance with the law. In our study for Denmark, we exploit an institutional regulation that grants foreigners local voting rights after three years of stay. Relying on register data, we find causal evidence that the first possibility to vote considerably reduces the number of legal offenses of non-Western male immigrants in the time after elections.

Keywords: migration, voting rights, immigrant integration, crime, RDD

JEL classifications: D02, K42, J15

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Introduction

Migration flows challenge many societies around the world (Collier, 2013; Castles et al., 2014). There are currently major concerns about the risks connected with masses of poorly integrated migrants. While culturally diverse societies afford many opportunities for the receiving countries in terms of labor force and new ideas that foster creativity and economic growth (Giovanni et al., 2015; Page, 2007; Leung et al., 2008; Borjas, 2014), their citizens worry about a weakening of social cohesion, community cooperation and, particularly, migrants' compliance with the law. According to the European Social Survey of 2014, worries about immigrants increasing the crime rate were more prevalent than worries about immigrants taking away jobs, even before the great inflow of refugees in 2015.¹ In migration policy, the challenges spur political discussions about optimal migration quotas and regulations of the labor and housing market, trading off the opportunities and threats of immigration. We complement this perspective and emphasize the potential role played by political institutions in the consequences of migration for receiving countries. In particular, democratic participation possibilities may affect integration into the economy and society, allowing to secure the welfare gains that ensue from a multi-cultural society.²

In our contribution, we assess the effect of the first possibility to participate in local elections on immigrants' compliance with the law. We exploit that in Denmark non-citizens are granted the right to vote after three years of stay. Theoretically, non-citizens' compliance with host societies' norms and laws is expected to depend on whether these individuals feel they are respected, treated with dignity, and perceive some personal control (aspects that Lane (1988) termed the procedural goods of democracy). In previous research, political preferences have been shown to depend on individual experience with democracy (Fuchs-Schündeln and Schündeln, 2015) and, in particular, with democ-

¹Figure A1 in the Supplementary Materials shows the distribution of the responses.

²So far, public debates and empirical research on the political incorporation of immigrants has focused on access to citizenship (Bevelander and Spång, 2015). However, there is also a long theoretical debate about the voting rights for foreigners (Munro, 2008; Seidle, 2015). While there are some contributions which assess the effect of non-citizen suffrage on policy outcomes (see, e.g., Vernby, 2013), there is no evidence about the effect of suffrage on immigrants' integration.

racy which provides inclusive and participatory political institutions fostering cooperation (Acemoglu and Robinson, 2012) and civic virtue (Frey, 1997). We therefore hypothesize that the possibility to participate in municipal and regional elections has a positive effect on migrants' norm compliance. As an indicator of immigrants' cooperation and compliance with the law, we concentrate on legal offenses. While there are certainly other important aspects of the multifaceted construct of integration, an individual's criminal record of severe offenses as well as petty offenses renders a key outcome measure. It has the capacity to capture potentially rapid motivational and behavioral reactions, and the requisite information is collected in administrative registers. Further, it constitutes an outcome that is obviously of high interest in the political discourse. In our application, individuals' criminal behavior is thus a prominent policy outcome as well as an attractive revealed preference measure of norms and norm compliance.³

While there is already an extensive economic literature on the relationship between immigration and economic and more specifically labor market outcomes (see, e.g., Borjas, 2014; Card, 2001), research linking immigration and crime is still developing (for a survey of this literature see Bell and Machin, 2013). Empirical evidence on whether immigration in general affects crime rates is mixed with most studies reporting no effect or, if anything, an increase in property crimes. Few studies are concerned with the determinants of criminal or norm compliant behavior of immigrants, and predominantly their legal status is emphasized (see, e.g., Baker, 2015; Pinotti, 2017). According to Pinotti (2017), institutional legalization of undocumented immigrants in Italy reduced the crime rate for property offenses by 55 percent.

We exploit an institutional setting in Denmark, where immigrants are allowed to vote in regional and municipal elections after three years' stay (and are automatically enrolled). Drawing on administrative data, we estimate the causal effect of the possibility to vote for the first time in these elections on the number of convictions for legal offenses in the two years after elections. Specifically, we apply a regression discontinuity design (RDD)

³A similar argument is pursued by Fisman and Miguel (2007) who use parking violations of diplomats in Manhattan to measure how cultural norms and enforcement affect norm compliance.

to compare the number of offenses between migrants who arrived just slightly more than three years before the next election and were thus just allowed to vote (treatment group) with migrants who arrived slightly later and were just not allowed to vote (control group). In the main analysis, we find that the first opportunity to vote on average reduces the number of convictions of non-Western immigrants (i.e., immigrants from Turkey, Iraq, the Philippines, Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia-Herzegovina) in the two years after the election by roughly 60%. There are large differences across groups, with the effects being largest for employed men. We perform several supplementary tests to support the causal claim of our results. First, to validate that the two groups are comparable, we test whether the delinquency of the same individuals already differed during their first two years of stay, and thus before the treatment. We do not find systematic differences. In a second validation test, we check for an election effect at the three-year duration threshold for placebo election dates. We do not find any evidence for reactions at these placebo-dates. Finally, we check for election effects on EU-citizens, who are not exposed to the treatment assignment scheme. They hold the right to vote in local elections from the beginning of their stay. Again, we find no systematic differences. These findings underpin the causal interpretation of our results and thus that the reduction in the number of convictions is causally driven by the right to vote in local elections.

Institutional setting

In 1981, Denmark introduced the right to vote in local elections, i.e., municipal and regional elections, for non-citizens after three years of uninterrupted legal residence. This arrangement complements an immigration policy that is comparatively restrictive regarding the admittance of foreigners, requirements for access to permanent residence as well as citizenship. The residence duration requirement for the grant of voting rights only holds for non-EU foreigners. EU-citizens and citizens of Scandinavian countries have the right to vote in municipal and regional elections right from the beginning of their stay.

Municipal and regional elections take place in late November every four years, and eligible individuals automatically receive a polling card in the weeks preceding the election. According to a study of the 2001 local elections, foreigners make extensive use of their voting right. About 47% of immigrants with non-Western origins participate. The prime explanation for this high rate of involvement is seen in the fact that the electoral system combines proportional representation with the possibility for preferential voting (Togeby, 2011). While Denmark still has a relatively homogeneous population, the proportion of inhabitants of foreign origin has increased substantially over the past few decades. In 2016, 4.3% (4.7%) of the male (female) population were foreign citizens from non-EU countries (excluding immigrants from Norway and Iceland, both of which are able to vote upon migration to Denmark). Other foreign male and female citizens (from the EU-28, Norway, and Iceland) represent 4.1% and 3.3%, respectively. These shares have increased by more than 50% in less than 15 years (Statistics Denmark, 2016).

Data

For our analysis, we draw on administrative data providing detailed information about foreigners' registration dates when taking up residence in Denmark.⁴ This date defines their eligibility to vote in local elections. Within the Danish foreign population, we primarily focus on people who immigrated within a time frame ranging between two years and seven months and three years and five months prior to the local and regional elections in 1997, 2001 and 2009.⁵ Using a unique encrypted identifier for each individual, we retrieve detailed background information from various registers, including information on convictions collected in the registers on criminal convictions in Statistics Denmark.

⁴Note that this date is not a choice variable. For people with a residence permit issued before arrival, the registration date is the day they receive a CPR number and thus the day of actual migration. For those who receive the permit after arrival, the date is the day of acquisition of the residence permit.

⁵The concrete election dates are November 18, 1997, November 20, 2001, and November 17, 2009. We exclude data for the elections in 2005, as this was the first vote within the new regional and municipal boundaries after a significant municipality reform, which also entailed changes in the administrative units' duties. Local and regional representatives were elected in 2005 within the new boundaries. However, they did not come into office before 2007 (see, e.g., Rasmussen, 2005; Kjaer and Klemmensen, 2015). This constitutes a special election situation that might not be comparable to other ones, especially for immigrants. We thus concentrate on elections that were held in a regular manner.

All violations of the penal code and other special laws (including the Danish Road Traffic Act) for which individuals have been found guilty by a court or by prosecution, are registered and available through Statistics Denmark. Accordingly, the register covers a broad range of offenses from petty offenses related to the traffic law (e.g. drunk driving) up to severe offenses like violent crimes and sexual offenses. In case that a person is convicted for several crimes in an incidence, only the main one is registered. Entries are individual-specific and thus can be linked to other administrative registers. In the analyses, we concentrate on convictions leading to a prison sentence, probation, or a fine. Fines represent the main type of convictions, especially among convictions against the Danish Road Traffic Act (Statistics Denmark, 2016). Fines are also the type of sanction used for the least severe offenses. Within offenses against the Danish Road Traffic Act, only fines above DKK 1,000 (app. USD 140) - before 2001 - and above DKK 1,500 (app. USD 210) - between 2001 and 2011 - are registered. Over the study period, the vast majority of fines registered against the Danish Road Traffic Act sanction speed infractions (Statistics Denmark, 2017). In our analyses, we use the actual date when the offense was committed, and not the date when the offense was sanctioned. Further, we only use entries for which the individual was eventually sanctioned and exclude acquittals or dismissals.

Empirical strategy

Regression discontinuity design

Figure 1 visually presents our identification strategy. In order to measure the causal effect of voting rights on compliance with the law, we compare people like person X, who arrived slightly more than three years before election day with persons like Y, who came just a little while later. While people like X receive a polling card and are entitled to vote, their fellows like Y, who arrive only slightly later, do not. As it is unlikely that individuals choose their day of arrival in Denmark strategically on the basis of the next election date, this assignment rule for the right to vote generates local randomization around the threshold of three years' stay by election day. As both groups have lived for

practically the same amount of time in Denmark, they should only differ in their exposure to the possibility to vote. Therefore, the causal effect of the treatment can be estimated by comparing the outcomes - in our application the compliance with the law - of both groups around the threshold. This is the basic idea of a regression discontinuity design (RDD), which was originally proposed in the 1960s' (Thistlethwaite and Campbell, 1960). RDDs are a highly appealing approach for the social sciences with which to draw causal evidence in settings where randomized control trials are not feasible.⁶ In a basic RDD structure, the treatment is assigned if the value of an assignment or running variable exceeds a specific value. The assignment variable in our application is the duration of stay of individuals on election day, the threshold being 3 years, or 1,095 days.⁷

In the econometric analysis, we apply a sharp RDD. The treatment effect estimate τ applies at the threshold value c of three years of stay on election day and identifies the local average treatment effect (LATE). In particular, τ can be estimated by the following limits (with m being some function of the assignment variable).

$$\hat{\tau}(c) = \hat{m}_+(c) - \hat{m}_-(c).$$

Thus τ amounts to the estimation of the difference in the limits of the outcome of interest at the threshold value when approaching it from the right (+) and from the left (-). This is implemented estimating local linear regressions (LLR) separately from both sides of the threshold. This strategy allows us to flexibly control for an underlying relationship between the dependent variable and the assignment variable (Porter, 2003; Hahn et al., 2001; Lee and Lemieux, 2010). We use a triangular kernel, which features advantageous

⁶For more information on RDD, see, for example, Hahn et al. (2001) or Lee and Lemieux (2010).

⁷In our setting, the effect of the possibility to vote can be identified independently of whether immigrants lose or gain access to special programs after three years (e.g., the introduction program as part of the formal integration period or social assistance transfers). With the duration of stay as the assignment variable, being treated with any of these programs changes day by day around the threshold of three years. Consequently, one is only able to separate treatment and control groups for one day. At the threshold, any measured effect would need to be the result of one additional day on or off the program. This is not plausible. Indeed, a placebo tests at the threshold of a three years' stay in Denmark, for placebo election dates do not reveal any effect (see Tables B11, B12 and Tables B13, B14 in the Supplementary Materials).

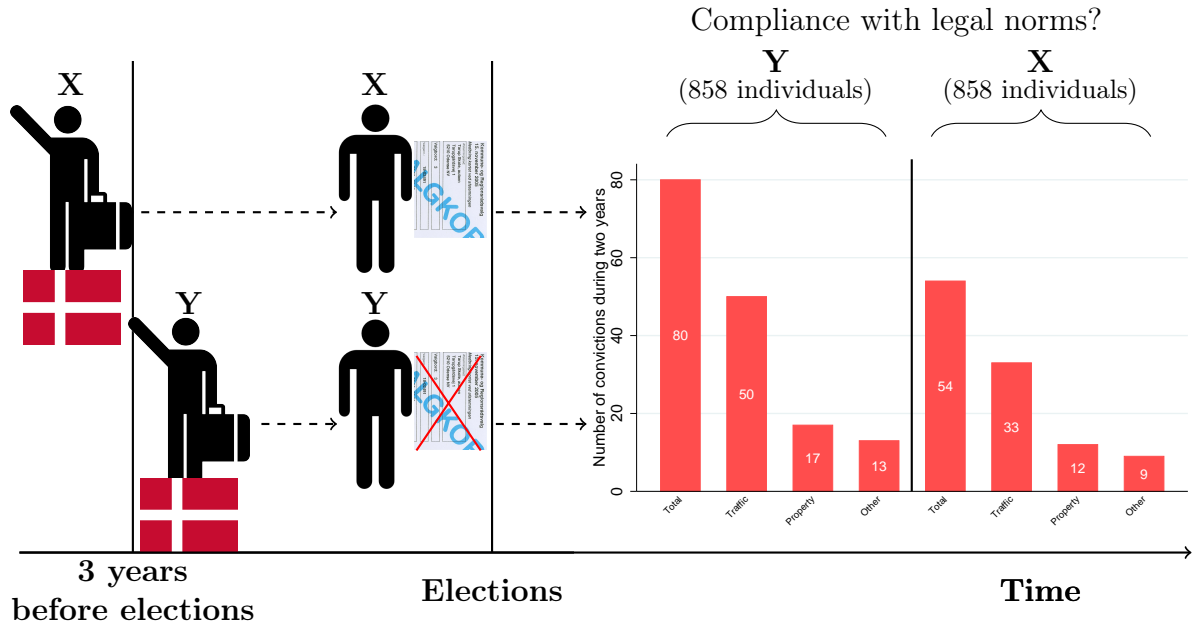


Figure 1: Identification strategy. Immigrants are allowed to vote in local elections after three years of residence in Denmark. People like immigrant X receive the polling card before the next election and form the treatment group. In contrast, people like immigrant Y do not receive it and form the control group. We are interested in whether the opportunity to vote leads to systematic differences in the number of their legal norm violations. The bar chart on the right shows the number of convictions within our main estimation sample of non-Western immigrants between November of the election year and October two years later. The left-hand side depicts the number of convictions for 858 Y individuals (the control group) and the right-hand side depicts the number of convictions for 858 X individuals (the treatment group). We use individuals within a range of the assignment variable of ± 120 days.

properties for estimates at boundary points (Fan and Gijbels, 1996).⁸ The discontinuity, i.e., the estimate for the LATE, is then determined by the difference between the constant terms of the two local linear regressions. It is a local estimate of the treatment effect, meaning that the effect of the possibility to vote is evaluated for individuals who spent three years in the host country. Any extrapolation to a granting of voting rights after fewer years, e.g., two, or more years, e.g., six or eight, should be undertaken with caution.⁹

⁸Note that we use the same bandwidths for all samples, as this makes comparisons between effect estimates across samples easier. In order to ensure that our results are not sensitive to a specific choice of bandwidth, we report results for several bandwidths. The optimal bandwidth (according to, e.g., Imbens and Kalyanaraman, 2012; Calonico et al., 2014, 2016) would be between 85 and 180 days depending on the sample choice for non-Western immigrants and the method. We use 120 and 150 days in the main analysis and report estimates for the bandwidth of 90 days in the Supplementary Materials. In our application, we use the Rdrobust implementation in Stata (Calonico et al., 2016). We additionally validate whether our estimates are sensitive to the choice of the polynomial order or the kernel. The results of this test are provided in the Supplementary Materials.

⁹Moreover, the interpretation of the effect depends on the definition of the treatment. We define the treatment as the first opportunity to vote, thus all individuals on the right-hand side of the threshold are treated, and we identify the LATE. If we, however, were interested in the effect of voting in itself,

Sampling

We pre-process our data and apply a matching technique to the left and the right of the threshold based on observable pre-determined characteristics of the individuals. This allows us to base our analysis on a balanced symmetrically composed sample at each point around the threshold.¹⁰ Given that we use the immigration date as the assignment variable and pool the data for several election years and nationalities, migration waves that occur at different times, and rather sporadically, might lead to an imbalanced sample composition around the threshold. This might happen with regard to, for example, nationalities and years, even though the migration date is locally randomized. By using the pre-processing strategy, we ensure that we compare the same number of individuals of the same nationalities and in the same years around the threshold; i.e., we form comparable groups. As the precise manipulation of the assignment variable is unlikely in our setting where the registration date is not a choice variable, this correction should be unproblematic. The alternative would be a scenario in which an individual is willing to prepone his immigration in expectation of obtaining the right to vote one day earlier. However, we do not observe that the number of individuals narrowly overshooting the threshold is noticeably high in the raw sample of non-Western immigrants (see Figures B1 and B2 in the Supplementary Materials). Further, the McCrary (2008) test does not reject the null of no discontinuity in the distribution of the assignment variable.¹¹ We are consequently not concerned that the local randomization assumption is invalidated but that any imbalance could be due to the pooling of the data of several nationalities and several years around the threshold.¹² Therefore, the pre-processing decreases model dependence and increases confidence in

we would only identify an intention-to-treat version of the effect, as we do not observe whether someone actually casts a vote or not.

¹⁰Similar approaches are applied in experimental settings to enhance precision and efficiency (Keele et al., 2010) and in RDD settings to exploit geographic boundaries (Keele et al., 2015).

¹¹Using the raw estimation sample, a bandwidth of 90 days, and a polynomial of first order, the point estimate for the discontinuity in the distribution is 0.380 with a p-value of 0.704.

¹²The histograms in Figures B3 to B5 depict the composition of the raw sample with respect to nationalities for single years. They show that different nationalities from different years potentially drive the sample at different points around the threshold. The matching correction is thus potentially important. In Section B.1 in the Supplementary Materials, we discuss some scenarios of circumstances which would lead to biased estimates and thus potentially to false conclusions.

causal estimates by ensuring that any measured effect will not be due to imbalances in the sample (Ho et al., 2007).

In Section B.1 in the Supplementary Materials, we present additional details about the composition of the sample and the balance before and after the pre-processing. In the pre-processing, we only consider exact matches within time bands of five days of the assignment variable (separately for each election year) and for the variables gender, and nationality. This provides us with a symmetric sample composition with regard to nationality, year, and gender on both sides of the threshold.¹³ We use exact and Mahalanobis distance matching procedures as they are to be preferred to propensity score matching (King and Nielsen, 2015). After pre-processing, standard estimation techniques can be used (Iacus et al., 2015) and still a LATE is estimated if the local randomization assumption holds (Imai et al., 2008).

The baseline sample is restricted to individuals who are observed for six years after immigration and who are at least 21 years old at the time of the election. Thus, they were at least 18 years old when they arrived in Denmark. Further, we restrict our sample to nationalities that are subject to the three year assignment rule and that show a relatively stable inflow into the country, such that we observe them in every election year. Consequently, the final main estimation sample consists of non-Western immigrants from Turkey (TR), Iraq (IQ), the Philippines (PH), Pakistan (PK), China (CN), Ukraine (UA), Iran (IR), Sri Lanka (LK), Morocco (MA) and Bosnia-Herzegovina (BA). Turkish migrants, who first went as guest workers to Denmark in the late 1960s, form the largest ethnic minority group in the country.

Figure 2 shows the composition of our (pre-processed) estimation sample of non-Western immigrants. The symmetric composition within bins of 5 days on both sides of the threshold for different nationalities is clearly visible. In addition, we compose a placebo

¹³Technically, we use the Mahapick routine in Stata (Kantor, 2012) and apply exact matching over 5-day bands on both sides of the threshold, the election year, sex, and nationality and use Mahalanobis matching for the age of individuals. The latter approach allows us to control for characteristics that are related to the outcome without strict parametric assumptions. If an individual has several matching counterparts, we randomly choose one of the candidates. As we only keep unique matches (1 to 1 matching), there is an equal number of observations on both sides of the threshold in the basic sample.

sample of immigrants from EU countries (Germany, the United Kingdom, Sweden, the Netherlands, France, and Italy), who are eligible to vote from the beginning of their stay. For them, no effect should be observed at the threshold if there is no other intervention that applies the same threshold for assignment.

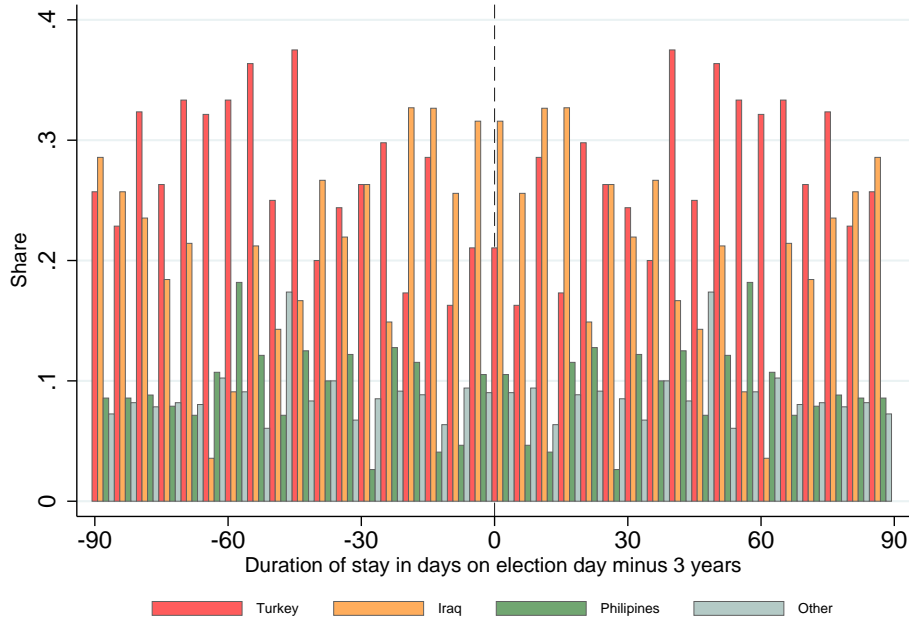


Figure 2: Composition of the estimation sample of non-Western immigrants. The histogram shows the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia Herzegovina. The histogram is based on the pre-processed sample.

Assignment and dependent variable

In our application, we define the assignment variable such that the threshold value is zero. Thus, it is the duration of stay of each individual at the date of the respective election minus 3 years, or 1,095 days, respectively. The main dependent variable is the number of offenses within the period of two years following the vote for which an individual was eventually convicted. The exact time range starts at the beginning of November in the year of the election and closes at the end of October two years later. The dependent variable thus measures the number of convictions for treated and untreated individuals within the same time range. The bar graph on the right of Figure 1 depicts the absolute number of convictions in total as well as for different categories of offenses for samples

to both sides of the threshold, each containing 858 individuals. These are individuals from the non-Western sample with a range of the assignment variable of ± 120 days. The left-hand side (marked with Y) shows the bars for individuals in the control group (with no voting rights), and the right-hand side (marked with X) shows those for individuals in the treatment group (with the right to vote). The graph provides a first glimpse of the direction of the effect, suggesting a reduction in the number of offenses for the treatment group. However, this comparison involves a wider range than the limit at the threshold and does not control for the relationship between the duration of stay and the number of offenses. The appropriate estimates are provided in the next section.

Results

The empirical results are presented in two steps. First, we show the findings for the group of non-Western immigrants. Second, we report the results of several supplementary tests in support of the causal claim in our analysis.¹⁴

Table 1 presents the main results for the effect of the first possibility to participate in local elections on non-Western immigrants' compliance with the law. For the full sample, columns I and II show a decline (τ) in the number of convictions of about 0.07 (within two years) for the treated group vis-à-vis the control group. This effect is statistically significant for the bandwidth of 150 days and stays rather stable in size when the bandwidth is reduced to 120 days. The effect is sizable when compared to the point estimate of the LLR just below the threshold (\hat{m}_-) of 0.12 convictions for the control group; it amounts to about 60%. When studying men and women separately in columns III to VI, it is revealed that the main effect is due to a decline in men's delinquency. Within the male sample, employed men (columns VII and VIII) respond to involvement in the democratic process to a greater extent than non-employed men (columns IX and X). The grouping is

¹⁴ More detailed results, in particular descriptive statistics, further estimates for the main sample of non-Western immigrants, estimates using the raw sample of non-Western immigrants, estimates using an alternative dependent variable, and estimates for immigrants from EU countries are presented in the Supplementary Materials.

Table 1: The effect of voting rights on legal norm violations of non-Western immigrants in Denmark

Dependent variable: Number of convictions										
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
Sample	all		female		male		male employed		male non-employed	
Effect τ	-0.0628** (0.0309)	-0.0697** (0.0283)	-0.0126 (0.0229)	-0.0130 (0.0203)	-0.145** (0.0690)	-0.159** (0.0631)	-0.222** (0.108)	-0.218** (0.0981)	-0.0349 (0.0689)	-0.0683 (0.0642)
\hat{m}_-	0.118	0.120	0.0416	0.0409	0.242	0.248	0.344	0.332	0.100	0.127
Bandwidth	120	150	120	150	120	150	120	150	120	150
N left	858	1,040	505	610	353	430	206	249	147	181
N right	858	1,040	505	610	353	430	207	250	146	180

Notes: Local linear sharp regression discontinuity estimates for two bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

thereby defined according to employment status in the year of the election and thus by a pre-determined characteristic, which is not influenced by the treatment.¹⁵ Figure 3 shows the graphical representation of the identified effect for employed men in a RDD graph, delineating the two local linear smooths from both sides of the threshold. It reveals a clear negative discontinuity at the threshold date, i.e., individuals who narrowly had the opportunity to vote (the assignment variable being positive) are systematically less often convicted than their counterparts who did not yet have the right to vote (the assignment variable being negative).¹⁶ Overall, a parallel downward shift of the relationship is observed at the threshold value. The graph suggests a positive relationship between the number of convictions and the time spent in the country. Such a positive trend in the average number of convictions with the duration of stay is also observed in samples of foreigners in general (see Figure B9 for 6 exemplary years in the Supplementary Materials). This observation indicates that controlling for the relationship between the assignment and the outcome variable is relevant to reduce any related bias in the estimated effects.¹⁷

¹⁵The category of employed persons comprises individuals who are occupied in a broad sense; i.e., employees, self-employed, co-working spouses and full-time university students. The non-employed are individuals who are unemployed or retired and individuals out of the labor force. This categorization is meant to capture not only the employment status, but also the opportunity to interact with locals.

¹⁶Please note that we find similar effects using the homogeneous group of Turkish immigrants forming the largest group in our sample and allowing for a separate analysis. The results of this exercise are available upon request.

¹⁷The analyzed setting, however, does not allow us to draw a conclusion on what causally underlies this correlation, and this is also not the goal.

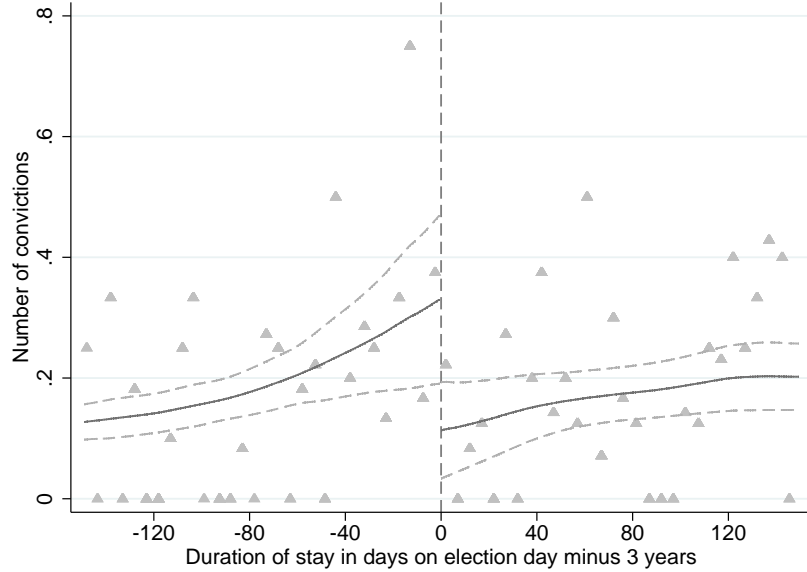


Figure 3: The effect of voting rights on convictions for employed non-Western male immigrants in Denmark. Regression discontinuity graph based on a local linear smooth, applied separately to both sides of the threshold, using a bandwidth of 150 days and a triangular kernel (column VII of Table 1). The dashed lines represent the 90% confidence intervals of the smooth, and the gray dots represent the binned means of the dependent variable (binwidth 5 days).

In two additional analyses, we study which offenses drive the overall effect and re-estimate the discontinuity in the number of convictions separately for traffic, property, and other offenses. It is revealed that the overall reaction is mainly driven by the traffic offenses of employed men. When concentrating on *petty offenses* with the alternative dependent variable being the number of an individuals' fines, we find very similar results. This is reassuring, as traffic offenses make up 60% of the convictions in the sample and result from the kind of misbehavior that is probably most amenable to civic motivation.¹⁸ Overall, the results for the sample of non-Western immigrants provide causal evidence that is consistent with the hypothesis that democratic involvement fosters cooperation and norm compliance in societies.

For an interpretation, the result for the different sub-samples may need to be put in perspective with specific features of our setting. First, note that employed men are more often convicted for offenses than non-employed men to begin with (whereby the largest fraction of convictions is due to violations of traffic law). It seems intuitive that individuals

¹⁸Detailed results are presented in Tables B7 and B8 and Section B.4 in the Supplementary Materials.

who are frequently exposed to traffic rules, for instance car owners who drive to work, have more scope and more occasions to react, especially as behavior leading to traffic offenses can be changed relatively easily. The effect might also be stronger for employed men as they have more opportunities to interact with locals, reinforcing perceptions of belonging after the elections (Christ et al., 2014). Over and above this, employed male immigrants might be particularly sensitive, as they have been observed to make relatively more use of the right to vote (Togebly, 2011). It is not surprising that we do not observe a clear reaction in the number of offenses for women. Women exhibit crime rates that are considerably lower than the ones of men - or even close to zero. Women might well react along a different margin though. Accordingly, most of the economic literature about immigrants' criminal behavior concentrates on males' delinquent behavior (see, e.g., Pinotti, 2017; Bell et al., 2013).

In order to understand the temporal pattern of the overall effect, we explore how long after the election the treatment effect can be observed. We re-estimate the discontinuity for men based on the same sample as before. This time, however, the dependent variable captures the sum of convictions per individual within 6-month time windows. The resulting estimates are presented in Figure 4. While there are no systematic discontinuities in the three years before the elections, we find systematic discontinuities in the first and the third time window after the election, while the point estimate in the second time window is negative but not statistically significant at conventional levels. This finding suggests that the identified treatment effect for legal norm violations holds for at least 1.5 years. This does not exclude that there are more far-reaching and more enduring reactions. However, effects on norm compliance might level out due to social interaction between treated and untreated immigrants (Christ et al., 2014). Untreated immigrants might further become more norm-compliant as the remaining time required before applying for permanent residence in Denmark elapses¹⁹, and also other policies might impact the integration path in the long term after the elections. Thus, due to the specific institutional setting and

¹⁹Immigrants can apply for permanent residence in Denmark, on the basis of requirements which include an inconspicuous criminal record, after a stay of 6 years. They can submit their application several months before reaching this threshold.

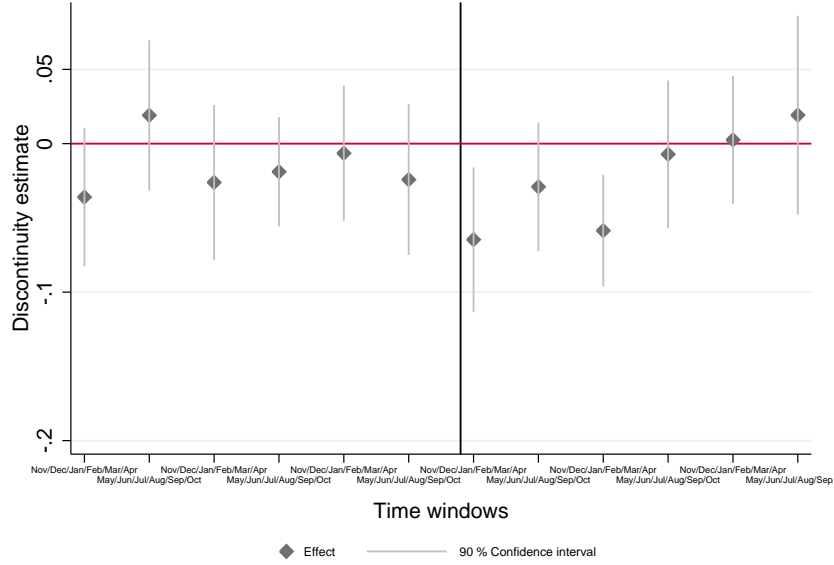


Figure 4: Time structure of the effect of voting rights on convictions. This graph shows the discontinuity estimates for the sample of male non-Western immigrants using the total of convictions per individual in 6-month time windows as the dependent variable and a bandwidth of 150 days. The black solid line indicates the start of the treatment period in November of the year of the election. The estimation results can be found in Table B6 in the Supplementary Materials.

the identification strategy applied, the contribution of democratic participation rights, or specifically the first possibility to participate in local elections, to long-term behavioral reactions cannot be isolated.

Robustness

Finally, we perform several robustness checks in order to underpin the causal interpretation of our results.

First, we re-estimate the main effects using the raw sample of non-Western immigrants. The results of this exercise can be found in Section B.3 in the Supplementary Materials. We find that our main results for men are sustained in this raw sample. As argued before, we give priority to the results based on the pre-processed sample as any effect cannot be due to imbalances in the sample composition.

Second, we check whether our main results are sensitive to the choice of the polynomial order or the choice of the kernel in the local polynomial estimation. Results are reported in Table B9 in the Supplementary Materials. We do not observe any specific sensitivity.

Third, we apply the same design to analyze the number of convictions of the same non-Western employed male immigrants during their first two years of stay. This allows us to test whether individuals have already differed systematically before the treatment, and thus the elections. As the estimates for the time structure have already suggested, we find no systematic difference in the number of convictions before treatment. In an additional supplementary test, we construct a sample of non-Western immigrants around the so-called placebo election dates²⁰ and test for an effect at the three-year duration threshold relative to these dates. We do not observe any systematic reactions.²¹ In a fourth step, we apply the design to immigrants from EU countries, who are eligible to vote in local elections right from the beginning of their stay in Denmark. We do not find any significant difference in the number of convictions during the two years after the elections between the two groups around the threshold of three years. The main results for the placebo tests are reported in Table 2. They support the causal interpretation of our finding of a negative effect of the possibility to vote on the number of convictions of non-Western foreigners after three years' stay in Denmark.

Fifth, to exclude that our results are driven by a single election year, we repeat our main estimates dropping one year in each round. Our sample is too small to estimate the effects for single years. However, if the results were driven by one single year, this procedure should reveal such a sensitivity. The findings for this exercise are reported in Table B10 in the Supplementary Materials. We observe the results to be stable to the dropping of single election years.

²⁰We set the placebo dates to dates in November of three non-election years. The dates are November 19, 1999, November 20, 2003, November 15, 2007.

²¹More detailed estimation results can be found in Tables B11 and B12 in the Supplementary Materials. We also repeat estimates for a second set of placebo dates in April of the election years. These estimates are reported in Tables B13 and B14, and we do not find any reactions at the respective placebo-dates either.

Table 2: Placebo tests for the effect of voting rights on legal norm violations of immigrants in Denmark

Dependent variable: Number of convictions											
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	
Sample	non-Western male employed		all		EU male		male employed		Placebo-dates male employed		
Time	before elections				after elections				after elections		
Effect τ	-0.0570 (0.0683)	-0.0549 (0.0611)	-0.0203 (0.0419)	-0.0186 (0.0381)	-0.0361 (0.0674)	-0.0318 (0.0601)	0.0149 (0.0693)	0.0143 (0.0626)	-0.0125 (0.0975)	-0.000478 (0.0884)	
\hat{m}_-	0.122	0.122	0.0798	0.0735	0.127	0.102	0.109	0.0850	0.191	0.171	
Bandwidth	120	150	120	150	120	150	120	150	120	150	
N left	206	249	558	677	328	400	259	316	216	256	
N right	207	250	558	677	328	400	246	299	208	238	

Notes: Local linear sharp regression discontinuity estimates for two bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Columns I and II report the discontinuity estimates for the main sample of non-Western immigrants for convictions during the first two years of stay as dependent variable. Columns III to VIII report the estimates for a sample of EU-citizens. Columns IX and X report the estimates at the placebo-dates for non-Western employed male immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

In a last check, we apply an alternative strategy to exploit the quasi-randomization narrowly around the threshold to see whether the possibility to vote affects the number of offenses of immigrants. We use the panel structure of the data and compose two groups - one treatment and one control - to estimate a model comparing the evolution of the number of convictions between these groups taking individual fixed effects into account and controlling for the duration of stay in Denmark. We restrict the control group to those individuals with a value of the assignment variable between -45 and -1 and the treatment group to those with a value between 0 and 45 . People in the two groups are rather comparable in their characteristics as well as their duration of stay in the country. While, this specification ignores that only individuals just at the threshold are approximately randomized, it should at least render a lower bound of the true effect. The dependent variable captures the number of convictions per individual in 12 month bands, always between November and October in the subsequent year. The results based on the pre-processed and the raw sample are reported in Tables B22 and B23 in the Supplementary Materials. We find the results to be very much in line with our main RDD estimates supporting our main conclusion. In particular, the number of offenses of employed men

is reduced in the first year after the election by 0.12 in the pre-processed and by 0.09 in the raw sample.

Summing up, so far we have found robust empirical evidence that the first possibility to participate in local elections reduces the number of convictions for non-Western immigrants in Denmark. Moreover, we think that the behavioral reaction is due to a change in people's motivation rather than outside forces. First, any effect that we observe cannot be explained by a change in the costs of unlawful behavior that are captured in traditional economic models of crime (Becker, 1968; Ehrlich, 1973). In our application, immigrants on both sides of the threshold legally reside in Denmark before and after any elections take place. Individuals on both sides thus bear the same expected costs for criminal activities. Neither their employment prospects nor the enforcement and punishment change at the threshold. Specifically, we do not consider it conceivable that the prosecution procedures of police forces change. Why should they be aware and care whether an immigrant they stop has been in the country for narrowly more or less than three years before the last elections had taken place? The same is true for the judges handling some of the cases. We therefore argue that the change in behavior we observe can only be ascribed to a motivation that is intrinsic to these individuals and is not produced by external factors that change the cost benefit structure of offenses.

Concluding remarks

There are gloomy prospects regarding the integration of non-Western ethnic minorities in Western countries today. Some commentators are concerned about the emergence of parallel societies, while others refer to a clash of civilizations. However, the outcome will not be purely a matter of fate, but will to some extent depend on the institutions and initiatives that receiving countries adopt to foster integration. Integration is a process based on the idea that immigrants form a common identity with the people residing in their host society, involving immigrants and natives alike. We hypothesize that power sharing in the democratic process is an institutional means to foster this process. The

grant of local voting rights might provide people with an improved sense of self as they become able to participate in politics in their host society. Access to the democratic process thus could help build intrinsic motivation in terms of civic virtues and motivate immigrants to follow the norms and laws in the host country.

From a broader perspective, our research also contributes to a better understanding of whether individuals value democratic participation opportunities irrespective of their outcome (for an introduction to procedural and outcome utility, see e.g., Frey et al. (2004) and Stutzer and Frey (2006)). This is normally very hard to investigate empirically, as there are few situations where there are comparable groups who face the same consequences, but where one group has the right to participate and the other does not. In the investigated setting both groups, those who were eligible to vote and those who were not, face the same election outcomes and their preferences should be equally represented. Any finding that individuals who obtain the possibility to participate increase their norm compliance suggests a change in their motivation and a valuation of democratic involvement per se.

Our evidence for Denmark indicates that convictions for legal norm violations are substantially lower after immigrants have had the possibility to vote in local elections. This result applies especially to non-Western employed male immigrants. The number of their convictions during the two years after the elections is, on average, strongly reduced compared to their non-eligible peers. The identifiable effects materialize primarily during the first one and a half years after the elections. For the full sample, the effect amounts to a reduction of roughly 60% in the number of convictions within the two years after the elections. This effect is sizable and comparable in magnitude to the one for the legalization of undocumented immigrants in the study of Pinotti (2017) for Italy. While our estimates are based on a different class of offenses and a different population, this underpins the potential of inclusive (political) institutions for social cohesion. We identify the causal effect of the actual possibility to engage in the electoral process making the democratic participation right particularly salient and leaving people with an experience. This effect might well be a lower bound as immigrants interact with one another and learn that all

residents eventually obtain the right to vote. The available setting does not allow us to pin down what exactly mediates the observed effect. It might be the mere possibility to vote, the act of voting and participating, the possible ensuing contact with locals or the feeling of being heard. Future research has to further elaborate the features of democracy allowing for immigration and cohesive societies.

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For Online Publication: Supplementary Materials

A Appendix

Supplementary analysis based on data from the European Social Survey

According to the European Social Survey of 2014, a representative survey in 15 European countries, 58.9% of the respondents report concerns about crime and 31.8% about job losses (assuming neutrality at the midpoint of the scale) as a consequence of immigration. Figure A1 shows the full distribution of responses.

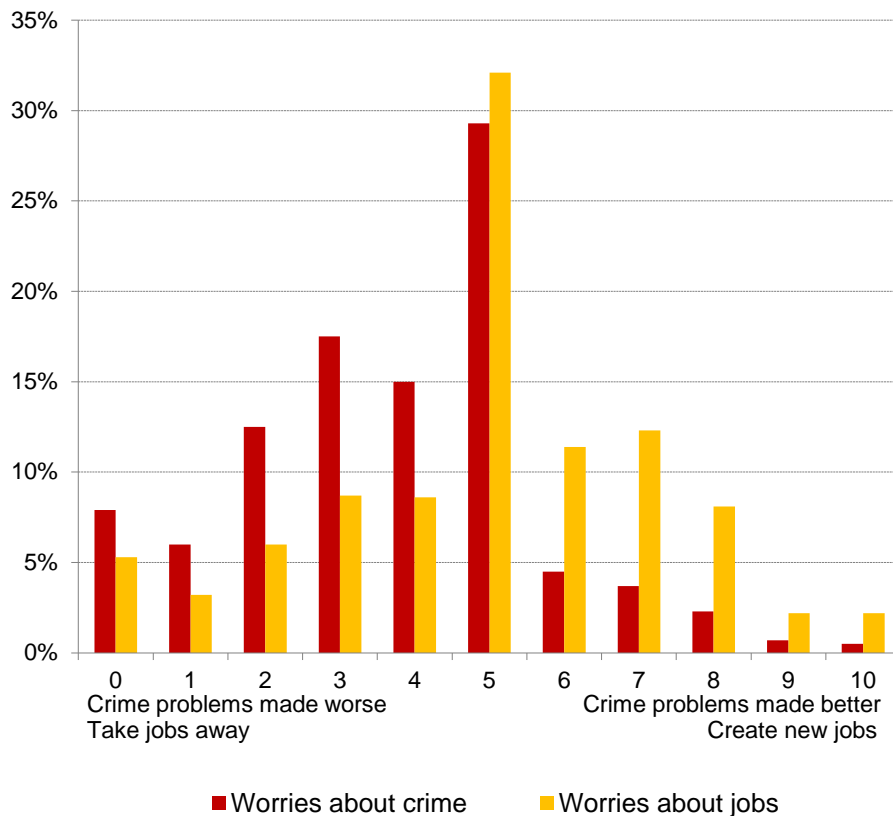


Figure A1: Concerns about immigrants in 15 European countries in 2014. Question D9 asks “Are [country]’s crime problems made worse or better by people coming to live here from other countries?” with responses on a scale from 0 “Crime problem made worse” to 10 “Crime problem made better”. Question D7 asks “Would you say that people who come to live here generally take jobs away from workers in [country], or generally help to create new jobs?” with responses on a scale from 0 “Take jobs away” to 10 “Create new jobs”. The sample includes respondents from Austria, Belgium, Czech Republic, Germany, Denmark, Estonia, Finland, France, Ireland, Netherlands, Norway, Poland, Sweden, Slovenia and Switzerland. The total number of respondents is 26,936 for the question on crime and 27,376 for the question on jobs.

Data source: European Social Survey 2014.

B Appendix

Supplementary analysis based on administrative data for Denmark

This section contains additional and more detailed information about our empirical analyses. It is presented in five steps. In a first step, we provide complementary estimates for the non-Western immigrant sample. For ease of comparison, the estimates from the main paper are repeated. In a second step, we repeat the basic estimates for the raw, non pre-processed, sample of non-Western immigrants. The corresponding results show, first, that we find a similar negative effect of the possibility to vote on the number of convictions for non-Western males and, second, that the effects are more precisely estimated in the pre-processed sample, although the number of observations is reduced. In a third step, we repeat our main estimates using a measure of petty offenses as an alternative dependent variable capturing norm compliance. We find the results to be very much in line with our main estimates. In fourth step, we re-run the estimates for the number of convictions on a sample of EU-citizens, who constitute an attractive placebo group. These immigrants are not subject to the treatment assignment scheme. Thus, any observed effect would be due to some co-occurring regulation. However, if the effect for the non-Western immigrants is due to the opportunity to vote, there should be no effect for the EU-citizens. In a final step, we report the results of an alternative estimation strategy to capture the effect of the possibility to vote on the number of offenses applying a panel estimation approach.

Table B1 reports descriptive statistics for our main dependent variable, i.e., the number of convictions within the two years after the election for different samples.

Table B1: Descriptive statistics for the number of convictions

	Non-Western				EU			
	Total	Traffic law	N	Max	Total	Traffic law	N	Max
All	0.0781 (0.3181)	0.0484 (0.2427)	1,716	3	0.0430 (0.2446)	0.0296 (0.1915)	1,116	3
Male	0.1512 (0.4437)	0.1076 (0.3569)	706	3	0.0595 (0.2836)	0.0396 (0.2103)	656	3
Female	0.0267 (0.1674)	0.0069 (0.0830)	1,010	2	0.0196 (0.1672)	0.0152 (0.1392)	460	2
Male employed	0.1792 (0.4744)	0.1453 (0.4217)	413	3	0.0634 (0.2745)	0.0495 (0.2347)	505	2
Male non-employed	0.1126 (0.3938)	0.0546 (0.2276)	293	3	0.0464 (0.3128)	0.0066 (0.0814)	151	3

Notes: Descriptive statistics for the main dependent variable, i.e., the number of convictions of immigrants between November in election years and the two following years. The mean values of the total number of convictions (Total) and the total number of traffic offenses (Traffic law) are reported for a bandwidth of 120 days around the threshold based on the pre-processed estimation samples. Standard deviations in parentheses. We additionally report the maximum number of offenses in our sample (Max). The minimum number is obsolete as it is always zero.

The sample compositions with respect to nationality within the bandwidth of 120 days is: Non-Western: 472 Turkey, 368 Iraq, 148 Philippines, 136 Pakistan, 136 Ukraine, 134 China, 112 Iran, 98 Sri Lanka, 68 Morocco, and 44 Bosnia and Herzegovina. EU: 438 German, 236 UK, 176 Sweden, 132 Netherlands, 72 France, and 62 Italy.

B.1 Detailed information on the analyses involving non-Western immigrants in Denmark

In this section, we first discuss challenges to a balanced sample and the reasoning behind our pre-processing strategy. Second, we report complementary results for the sample of non-Western immigrants while repeating the results from the main paper.

B.1.1 Sample balance

As we use the date of immigration as the assignment variable, migration waves that do not follow a stable pattern over time might bias our RDD estimates. Specifically, when pooling the data of several nationalities and for several years, differences in the composition of the estimation sample might lead to imbalances around the threshold, even though the migration date is locally randomized. Two scenarios exemplify the potential imbalance. First, an imbalance might occur because a wave of immigrants of a certain nationality A is observed just above the threshold by chance, while the inflow of people with nationality B is stable. This would lead to the observation that there are more individuals from country A above the threshold than there are from country B, such that B drives the sample below the threshold, while A drives the sample above it. Spurious treatment effects might then be estimated because immigrants from one of the countries might be more disadvantaged and commit more legal offenses. A balance test might then also indicate an imbalance in age, even though individuals had not manipulated their immigration date. This could occur because individuals from country A are younger on average than individuals from country B. A second reason for an imbalance might emerge if there are many individuals in some year X above the threshold. This would turn out to be problematic if, for example, the economic situation was tougher and the crime rate higher in year X than in some year Y. In the RDD, we would compare treated observations primarily from year X with relatively many observations from year Y in the control group. Such circumstances could again lead to systematic effect estimates where none exist, or to the opposite conclusion

that there is no effect when, in fact, the actual effect is only masked by such sample composition issues.

The histograms for individual years in Figures B3 to B5 suggest that different nationalities from different years indeed to some extent drive the sample at different points around the threshold in the *raw* sample. In order to see whether this might be an issue in our analysis, we perform standard balancing tests on our raw sample. We test for a discontinuity in the characteristics of our sample at the threshold value for a narrow bandwidth of 90 days (see Table B2). Indeed, we find that individuals' age features a discontinuity while gender, the hourly wage and the probability to be employed in the election year are continuous. Some years are slightly over- or under-represented. Furthermore, we find that individuals from Sri Lanka and Turkey are slightly over-represented above the threshold and those from Iraq slightly under-represented. This latter compositional difference could explain the slight discontinuity in age as Turks and individuals from Sri Lanka are, on average, younger than people from the other nationalities (see Table B3). The fact that there are randomly more of them above the threshold in the pooled data could thus produce the imbalance in age. It is therefore potentially important to apply a sample correction. With our pre-processing or matching strategy, we ensure that we are comparing the same number of individuals from the same countries and from the same years around the threshold, i.e., that we form comparable groups. The second part of Table B2 repeats the balancing test for the final pre-processed sample and shows that as expected all characteristics of the sample are nicely balanced after the pre-processing. Descriptive statistics for the number of convictions in the pre-processed estimation sample are provided in Table B1.

B.1.2 Local randomization

A challenge for our RDD strategy would be a precise manipulation of the assignment variable. However, this is unlikely as the registration date cannot be freely chosen unless an individual is willing to prepone his immigration in the hope of obtaining the right to vote one day earlier. This holds for individuals who apply for a residence permit before arrival. Those who receive their residence permit only after arrival in Denmark would

Table B2: Sample characteristics of non-Western immigrants before and after the pre-processing

Raw sample	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.	XIII.	XIV.	XV.	XVI.	XVII.
Variables	Age	P(female)	Wage/Hour	P(Emp)	P(1997)	P(2001)	P(2009)	P(LK)	P(IQ)	P(IR)	P(CN)	P(PK)	P(UA)	P(MA)	P(PH)	P(BA)	P(TR)
Effect τ	-2.106*** (0.766)	0.0421 (0.0365)	-16.49 (10.07)	-0.000742 (0.0356)	0.0998*** (0.0326)	-0.0659* (0.0369)	-0.0340 (0.0311)	0.0367* (0.0189)	-0.0912*** (0.0318)	-0.0243 (0.0217)	-0.0143 (0.0167)	-0.00473 (0.0197)	-0.0109 (0.0184)	0.0108 (0.0152)	0.0245 (0.0190)	0.0162 (0.0123)	0.0572* (0.0292)
\hat{m}_-	33.73	0.571	154.1	0.363	0.181	0.580	0.239	0.0431	0.329	0.109	0.0563	0.0868	0.0726	0.0343	0.0582	0.0240	0.186
h	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90
N left	1496	1496	514	1496	1496	1496	1496	1496	1496	1496	1496	1496	1496	1496	1496	1496	1496
N right	1712	1712	599	1712	1712	1712	1712	1712	1712	1712	1712	1712	1712	1712	1712	1712	1712
Pre-processed sample																	
Effect τ	0.330 (1.013)	-0.00734 (0.0523)	-8.750 (12.07)	-0.0397 (0.0527)	-0.00180 (0.0478)	-0.000960 (0.0542)	0.00276 (0.0450)	0.00123 (0.0272)	-0.00277 (0.0470)	0.00239 (0.0339)	0.00118 (0.0215)	-0.00143 (0.0265)	-6.10e-05 (0.0280)	-0.00108 (0.0198)	0.000547 (0.0295)	-0.000549 (0.0163)	0.000543 (0.0439)
\hat{m}_-	32.05	0.629	150.8	0.377	0.221	0.573	0.205	0.0536	0.311	0.114	0.0430	0.0691	0.0690	0.0299	0.0770	0.0301	0.203
h	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90	90
N left	668	668	244	668	668	668	668	668	668	668	668	668	668	668	668	668	668
N right	668	668	216	668	668	668	668	668	668	668	668	668	668	668	668	668	668

Notes: Local linear sharp regression discontinuity estimates for a bandwidth of 90 days using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the raw and the pre-processed sample of non-Western immigrants. *Age* stands for the age at the time of the vote, *P(female)* is an indicator for gender, *Wage/Hour* is the hourly wage, *P(emp)* is the probability to be employed in the election year, *P(year)* are indicators for the election year in the sample, and *P(nation)* are indicators for individuals' nationality.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B3: Mean age by nationality

Sample	Overall	LK	IQ	IR	CN	PK	AU	MA	PH	BA	TR
Mean(Age)	35.42	33.94	35.59	36.55	32.52	30.13	28.32	30.93	31.87	39.85	28.93

Note: Mean age of individuals within a range of ± 365 days of the assignment variable by nationality.

have to manipulate the date on which their residence permit is issued. This should not be possible. In fact, the histograms showing the number of observations from the raw sample of non-Western immigrants overall and by nationality do not indicate any manipulation. We do not observe that the number of individuals narrowly overshooting the threshold is noticeably high (see Figure B1 and Figure B2) neither does the McCrary (2008) test reject the null of no discontinuity in the distribution of the assignment variable.

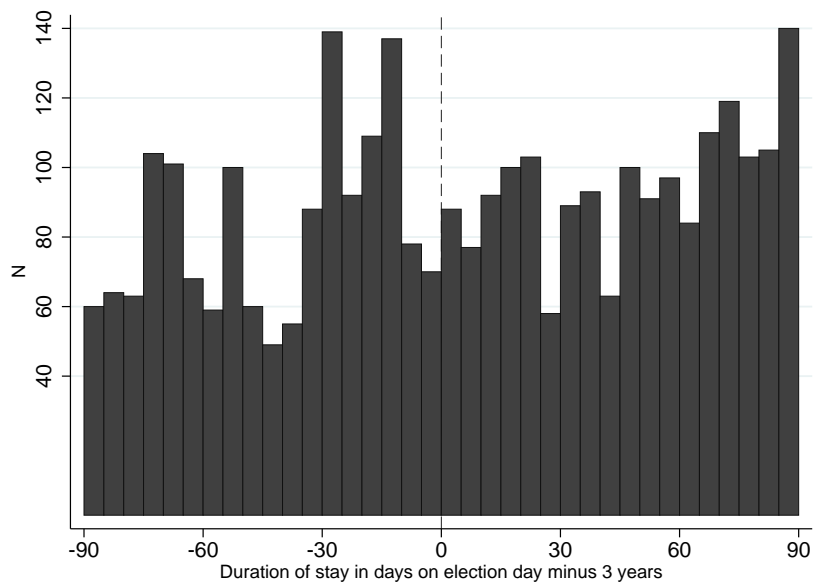


Figure B1: Histogram of the raw sample for non-Western immigrants around the threshold. The histogram shows the number of observations in the sample within bins of 5 days, separately to both sides of the threshold for the raw sample of non-Western immigrants.

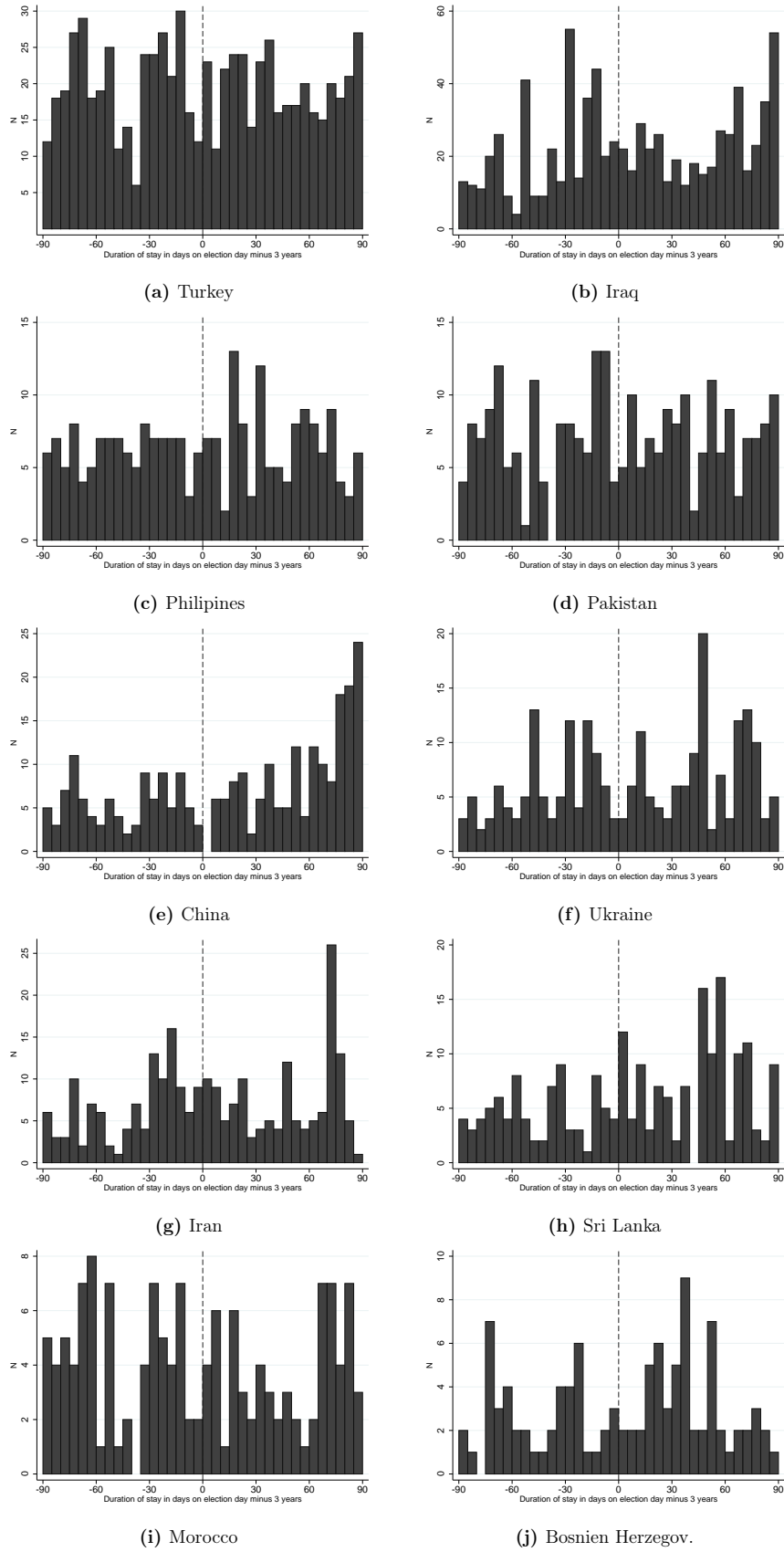
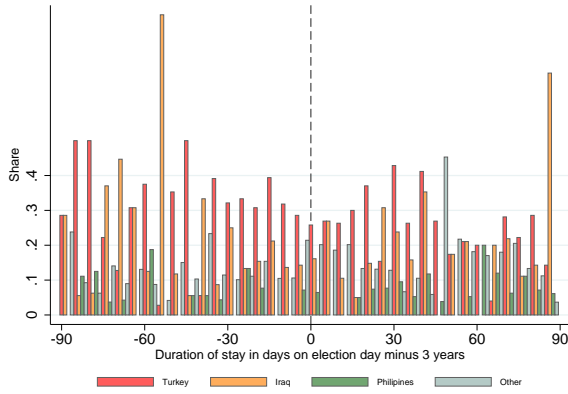
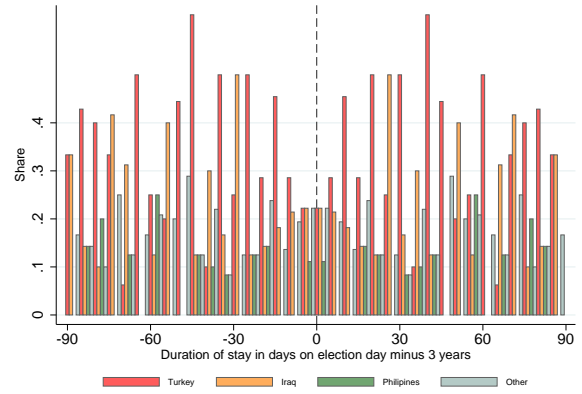


Figure B2: Composition of the sample for non-Western immigrants around the threshold by country of origin. The histograms show the number of observations by country of origin in the sample within bins of 5 days, separately to both sides of the threshold for the raw sample of non-Western immigrants.

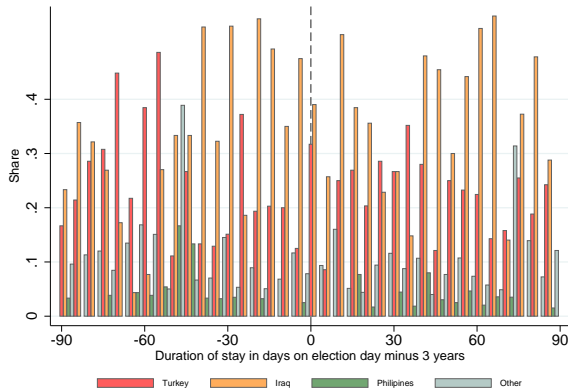


(a) all in 1997

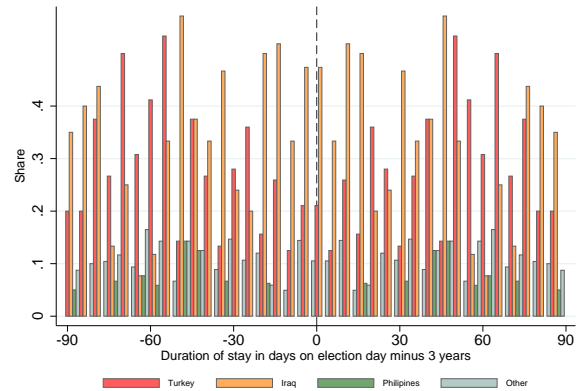


(b) pre-processed in 1997

Figure B3: Composition of the sample for non-Western immigrants in 1997. The histograms show the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia Herzegovina. The left-hand side shows the overall raw sample, while the right-hand side shows the pre-processed estimation sample of non-Western immigrants in 1997.

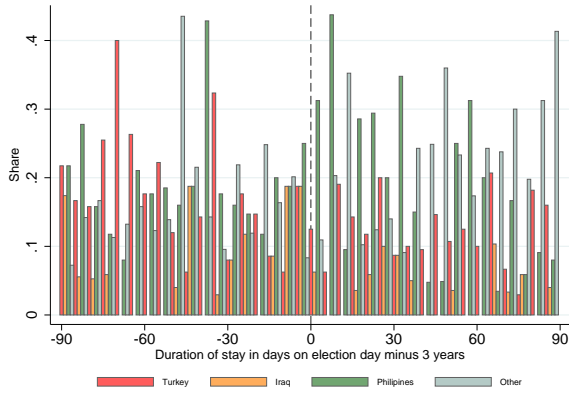


(a) all in 2001

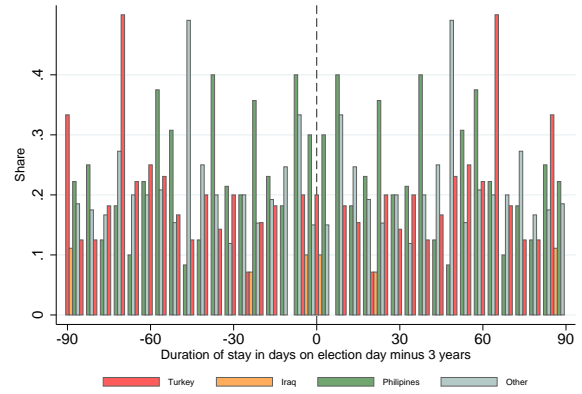


(b) pre-processed in 2001

Figure B4: Composition of the sample for non-Western immigrants in 2001. The histograms show the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia Herzegovina. The left-hand side shows the overall raw sample, while the right-hand side shows the pre-processed estimation sample of non-Western immigrants in 2001.

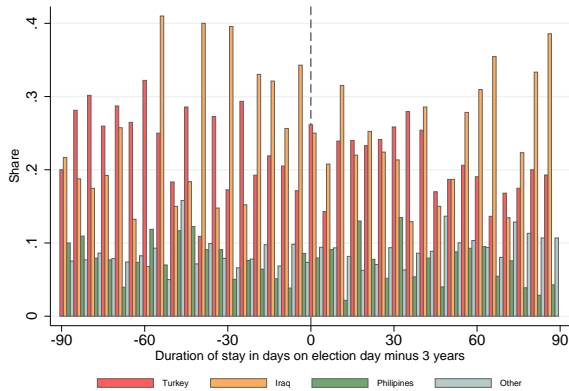


(a) all in 2009

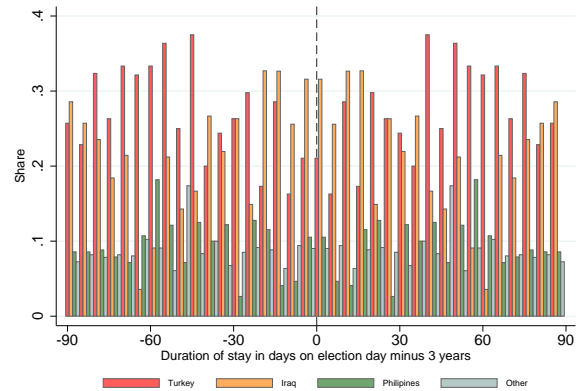


(b) pre-processed in 2009

Figure B5: Composition of the sample for non-Western immigrants in 2009. The histograms show the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia Herzegovina. The left-hand side shows the overall raw sample, while the right-hand side shows the pre-processed estimation sample of non-Western immigrants in 2009.



(a) all



(b) pre-processed

Figure B6: Composition of the sample for non-Western immigrants. The histograms show the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from Pakistan, China, Ukraine, Iran, Sri Lanka, Morocco, and Bosnia Herzegovina. The left-hand side shows the overall raw sample, while the right-hand side shows the pre-processed estimation sample of non-Western immigrants.

B.1.3 Further estimation results

Main analysis

We present a full set of estimation results for the causal effect of the first possibility to exercise democratic participation rights in Denmark on convictions. In addition to the results in the main text, findings for a bandwidth of 90 days are reported. Table B4 includes the findings for the full sample as well as men and women separately. Columns I to VI of Table B5 focus on employed and non-employed men, and columns VII to IX present the results of a placebo test on the number of convictions during individuals' first two years of stay, and thus before treatment. The results for specifications II and VIII in Table B5 are graphically presented in Figure B7.

Table B4: The effect of voting rights on legal norm violations of non-Western immigrants in Denmark

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Effect τ	-0.0618* (0.0348)	-0.0628** (0.0309)	-0.0697** (0.0283)	-0.0150 (0.0271)	-0.0126 (0.0229)	-0.0130 (0.0203)	-0.141* (0.0767)	-0.145** (0.0690)	-0.159** (0.0631)
\hat{m}_-	0.110	0.118	0.120	0.0436	0.0416	0.0409	0.222	0.242	0.248
Bandwidth	90	120	150	90	120	150	90	120	150
N left	668	858	1,040	395	505	610	273	353	430
N right	668	858	1,040	395	505	610	273	353	430

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Very similar results to the ones in the main analysis are observed if the estimates rely on a bandwidth of 90 days compared to those based on 120 or 150 days.

Effects over time

In order to obtain a better understanding of the temporal structure of the effect, we extend the presentation of the analysis for time windows in the main text. Remember that we undertake discontinuity estimates for time windows of 6 months. The first treatment month is the November of the election year in which individuals receive their polling cards

Table B5: The effect of voting rights on legal norm violations of non-Western employed and non-employed male immigrants in Denmark

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	male employed			male non-employed			male employed		
Time	after elections						before elections		
Effect τ	-0.253** (0.122)	-0.222** (0.108)	-0.218** (0.0981)	0.0117 (0.0741)	-0.0349 (0.0689)	-0.0683 (0.0642)	-0.0517 (0.0777)	-0.0570 (0.0683)	-0.0549 (0.0611)
\hat{m}_-	0.346	0.344	0.332	0.0592	0.100	0.127	0.123	0.122	0.122
Bandwidth	90	120	150	90	120	150	90	120	150
N left	156	206	249	117	147	181	156	206	249
N right	154	207	250	119	146	180	154	207	250

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of non-Western immigrants. Columns VII to IX report the discontinuity estimates for convictions during the first two years of stay.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

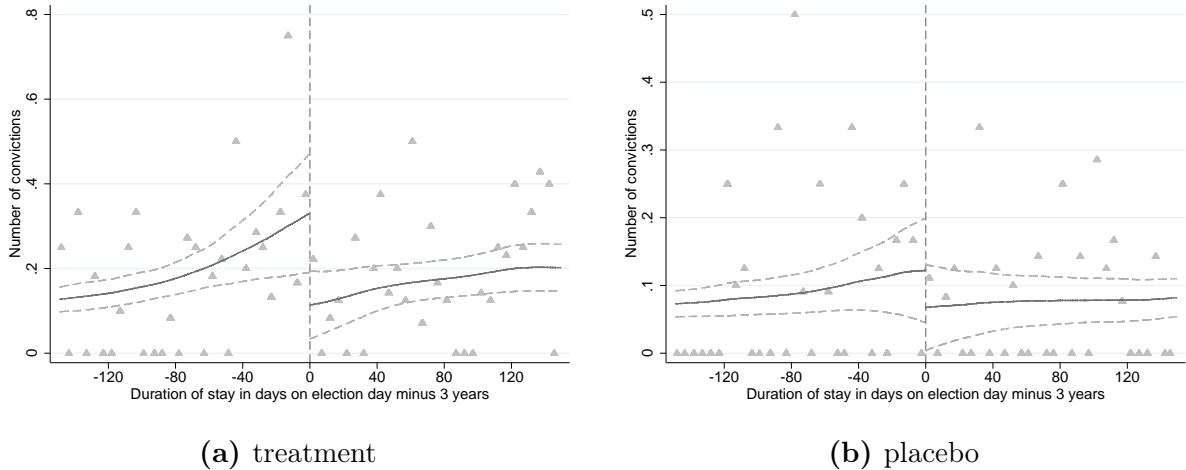


Figure B7: Regression discontinuity graph for the effect of the right to vote on convictions for non-Western immigrants in Denmark. Local linear smooth, applied separately on both sides of the threshold, using a bandwidth of 150 days and a triangular kernel based on the sample of employed men. Estimates for the treatment sample using the two years after the election are presented on the left-hand side, and estimates for the placebo sample using the outcomes of individuals in their first two years of stay are presented on the right-hand side (column II and column VIII of Table B5). The dashed lines represent the 90% confidence intervals of the smooth, and the gray dots represent the binned means of the dependent variable (binwidth 5 days).

and are entitled to participate in the elections. The result of this analysis is summarized in Figure B8. On the left-hand side, the discontinuity estimates for the time windows are presented. We find statistically significant negative discontinuities for the first and the

third time windows after treatment, while the point estimate in the second is negative but not significantly different from zero at conventional levels. The estimation results are reported in Table B6. The right-hand side of Figure B8 displays the point estimates for the respective discontinuity estimates to the left and the right of the threshold value. We see that both groups move in parallel before the elections, whereas after the elections the number of convictions for individuals just above the threshold falls during the first three time windows, while the convictions in the control group keep rising, before the groups move in parallel again.

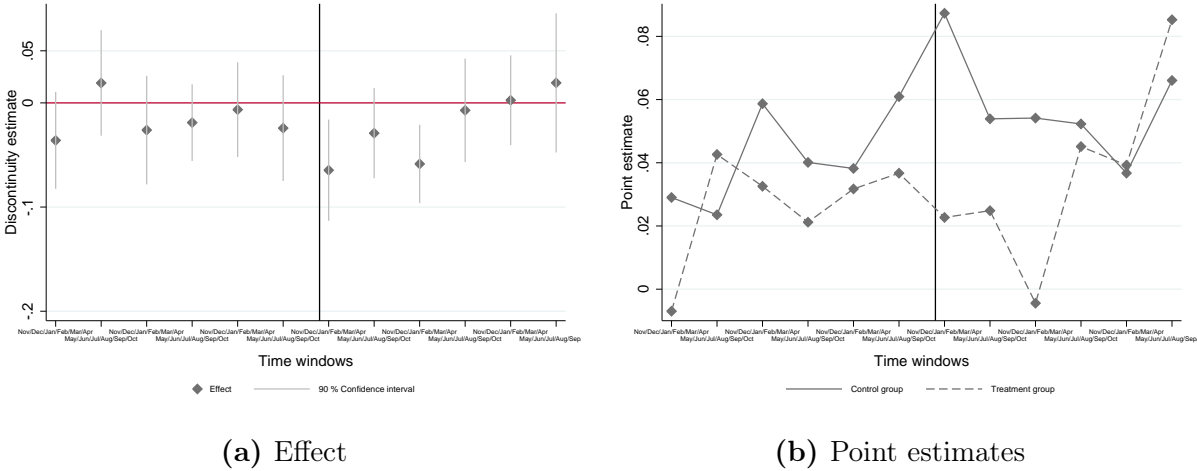


Figure B8: Time structure of the effect of voting rights on convictions. The graph on the left-hand side shows the discontinuity estimates for the sample of non-Western male immigrants, using the total of convictions per individual in 6-month time windows as the dependent variable and a bandwidth of 150 days. The graph on the right-hand side shows the point estimates of the constant term of the smooth at the threshold, coming from the left-hand side (control) and coming from the right-hand side (treated), also using a bandwidth of 150 days. The solid black line indicates the start of the treatment period in November of the election year. The estimation results can be found in Table B6.

Table B6: Time structure of the effect of voting rights on convictions

Dependent variable: Number of convictions within 6 months												
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
Time band	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct
Time	t-2	t-2	t-1	t-1	t	t	t+1	t+1	t+2	t+2	t+3	t+3
Effect τ	-0.0360 (0.0283)	0.0191 (0.0308)	-0.0261 (0.0317)	-0.0189 (0.0224)	-0.00648 (0.0277)	-0.0242 (0.0309)	-0.0646** (0.0295)	-0.0291 (0.0263)	-0.0586** (0.0228)	-0.00715 (0.0302)	0.00253 (0.0262)	0.0192 (0.0406)
\hat{m}_-	0.0290	0.0235	0.0587	0.0401	0.0382	0.0609	0.0873	0.0539	0.0542	0.0523	0.0367	0.0660
Bandwidth	150	150	150	150	150	150	150	150	150	150	150	150
N left	88	430	430	430	430	430	430	430	430	430	430	430
N right	317	430	430	430	430	430	430	430	430	430	430	430

Notes: Local linear sharp regression discontinuity estimates for a bandwidth of 150 days using the total of convictions per individual in 6-month time windows as the dependent variable and a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of male non-Western male immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Effects for different offense categories

Our dataset allows some analysis of different categories of offenses. The dominant category is convictions for violations of traffic laws. In comparison, property, sexual, violent and other kinds of offenses are relatively rare. As behavior leading to traffic offenses can be changed relatively more easily, we would expect that the overall effect is mainly due to a reduction in offenses in this category. Table B7 lists the estimates for men and three broad types of convictions. We find, as conjectured, that the effect seems to be driven by convictions for traffic offenses.

When separating the sample for employed and non-employed men in Table B8, we find that the effect for traffic offenses is mainly driven by employed men.

Table B7: The effect of voting rights on legal norm violations of non-Western male immigrants by type of offense

Dependent variable: Number of convictions				
	I.	II.	III.	IV.
Category	total offenses	traffic offenses	property offenses	other offenses
Effect τ	-0.159** (0.0631)	-0.137** (0.0587)	-0.00151 (0.0155)	-0.0209 (0.0183)
\hat{m}_-	0.248	0.213	0.00962	0.0255
Bandwidth	150	150	150	150
N left	430	430	430	430
N right	430	430	430	430

Notes: Local linear sharp regression discontinuity estimates for a bandwidth of 150 days using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of male non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B8: The effect of voting rights on legal norm violations of non-Western employed and non-employed male immigrants by type of offense

Dependent variable: Number of convictions								
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
Category	male employed				male non-employed			
	total offenses	traffic offenses	property offenses	other offenses	total offenses	traffic offenses	property offense	other offense
Effect τ	-0.218** (0.0981)	-0.207** (0.0926)	-0.00153 (0.0157)	-0.0104 (0.0277)	-0.0683 (0.0642)	-0.0316 (0.0530)	-0.00161 (0.0287)	-0.0351* (0.0211)
\hat{m}_-	0.332	0.304	0.00629	0.0221	0.127	0.0826	0.0143	0.0301
Bandwidth	150	150	150	150	150	150	150	150
N left	249	249	249	249	181	181	181	181
N right	250	250	250	250	180	180	180	180

Notes: Local linear sharp regression discontinuity estimates for a bandwidth of 150 days using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of employed and non-employed male non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Sensitivity with regard to alternative specification

To check whether our estimates are sensitive to the choice of the polynomial order or the kernel, we report the main estimates for employed male non-Western immigrants, once using a second order polynomial, and once using an uniform kernel. As shown in Table B9, we do not find the estimates to be particularly sensitive.

Table B9: Robustness with regard to the polynomial order and the kernel choice

Dependent variable: Number of convictions						
	I.	II.	III.	IV.	V.	VI.
Sample	male employed 2 nd order polynomial			male employed uniform kernel		
Effect τ	-0.268* (0.162)	-0.287* (0.147)	-0.254* (0.137)	-0.227** (0.111)	-0.213** (0.0998)	-0.198** (0.0912)
\hat{m}_-	0.344	0.355	0.364	0.357	0.331	0.303
Bandwidth	90	120	150	90	120	150
Polynomial	2	2	2	1	1	1
Kernel	tri	tri	tri	uni	uni	uni
N left	156	206	249	156	207	250
N right	154	207	250	156	210	253

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular (I-III) or uniform (IV-VI) kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of employed male non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Sensitivity with regard to single election years

To check whether our estimates are sensitive to single election years and thus to exclude that our main findings are driven by a single year we repeat our main estimates for non-Western males in each step dropping one election year from the sample. Our sample is too small to perform the estimates for single years, if they however were driven by one single year this procedure should reveal this sensitivity. The results for this exercise are reported in Table B10. We do not find that the results are sensitive to the dropping of single election years in significance or size.

Results for placebo election dates

Finally, we construct samples of non-Western immigrants around placebo election dates applying the same procedure as for the treatment sample above. First, we set the placebo dates to dates in November of non-election years. The dates are: November 18, 1999, November 20, 2003, and November 15, 2007. We re-estimate the main results for this sample around the chosen placebo dates and do not find any systematic reactions. The results for this placebo test are reported in Table B11 and Table B12.

Second, we repeat the estimates for another round of placebo election dates using dates in April of the election years, to ensure that the measured effect does not capture something

Table B10: Robustness with regard to single election years

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	excl. 1997			excl. 2001			excl. 2009		
Category	Total	Traffic	Fines	Total	Traffic	Fines	Total	Traffic	Fines
Effect τ	-0.170** (0.0751)	-0.146** (0.0715)	-0.157** (0.0709)	-0.190** (0.0944)	-0.178** (0.0882)	-0.192** (0.0876)	-0.130** (0.0657)	-0.103* (0.0601)	-0.121** (0.0569)
\hat{m}_-	0.270	0.232	0.238	0.271	0.244	0.249	0.211	0.173	0.166
h	150	150	150	150	150	150	150	150	150
N left	320	320	320	217	217	217	323	323	323
N right	320	320	320	217	217	217	323	323	323

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of male non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B11: The effect of voting rights on legal norm violations of non-Western immigrants in Denmark for placebo election dates in November of non-election years

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Effect τ	-0.0254 (0.0358)	-0.0139 (0.0305)	-0.00128 (0.0277)	-0.00385 (0.0209)	-0.00205 (0.0191)	0.00221 (0.0180)	-0.0624 (0.0862)	-0.0342 (0.0720)	-0.00763 (0.0647)
\hat{m}_-	0.0900	0.0824	0.0737	0.0215	0.0225	0.0224	0.200	0.177	0.154
Bandwidth	90	120	150	90	120	150	90	120	150
N left	714	978	1,146	433	596	701	281	382	445
N right	714	978	1,146	433	596	701	281	382	445

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using a pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

that is specific to non-Western immigrants in the election years. The dates are: April 18, 1997, April 20, 2001, and April 17, 2009. The results for these estimates are reported in Table B13 and Table B14.

The fact that we find no systematic reaction around the placebo dates strengthens the causal interpretation of our findings. We would not expect any effect if the documented main result is in fact due to the first possibility to participate in local elections.

Table B12: The effect of voting rights on legal norm violations of non-Western employed and non-employed male immigrants in Denmark for placebo election dates in November of non-election years

Dependent variable: Number of convictions						
	I.	II.	III.	IV.	V.	VI.
Sample	male employed			male non-employed		
Time	after placebo elections					
Effect τ	-0.0304 (0.114)	-0.0125 (0.0975)	-0.000478 (0.0884)	-0.107 (0.135)	-0.0622 (0.107)	-0.0157 (0.0950)
\hat{m}_-	0.209	0.191	0.171	0.193	0.161	0.133
Bandwidth	90	120	150	90	120	150
N left	164	216	256	117	166	189
N right	156	208	238	125	174	207

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using a pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B13: The effect of voting rights on legal norm violations of non-Western immigrants in Denmark for placebo election dates in April of election years

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Effect τ	-0.0135 (0.0302)	-0.0278 (0.0266)	-0.0363 (0.0254)	-0.00411 (0.0187)	-0.00915 (0.0192)	-0.0182 (0.0184)	-0.0285 (0.0707)	-0.0554 (0.0615)	-0.0629 (0.0588)
\hat{m}_-	0.0449	0.0604	0.0701	0.0147	0.0210	0.0263	0.0962	0.129	0.146
Bandwidth	90	120	150	90	120	150	90	120	150
N left	526	721	960	324	438	576	202	283	384
N right	526	721	960	324	438	576	202	283	384

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using a pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B14: The effect of voting rights on legal norm violations of non-Western employed and non-employed male immigrants in Denmark for placebo election dates in April of election years

Dependent variable: Number of convictions						
	I.	II.	III.	IV.	V.	VI.
Sample	male employed			male non-employed		
Time	after placebo elections					
Effect τ	-0.0725 (0.115)	-0.0800 (0.102)	-0.0746 (0.0972)	0.00891 (0.0880)	-0.0346 (0.0748)	-0.0547 (0.0719)
\hat{m}_-	0.169	0.177	0.170	0.0212	0.0765	0.122
Bandwidth	90	120	150	90	120	150
N left	107	155	210	95	128	174
N right	121	171	227	81	112	157

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using a pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

B.2 Additional graphs and tables

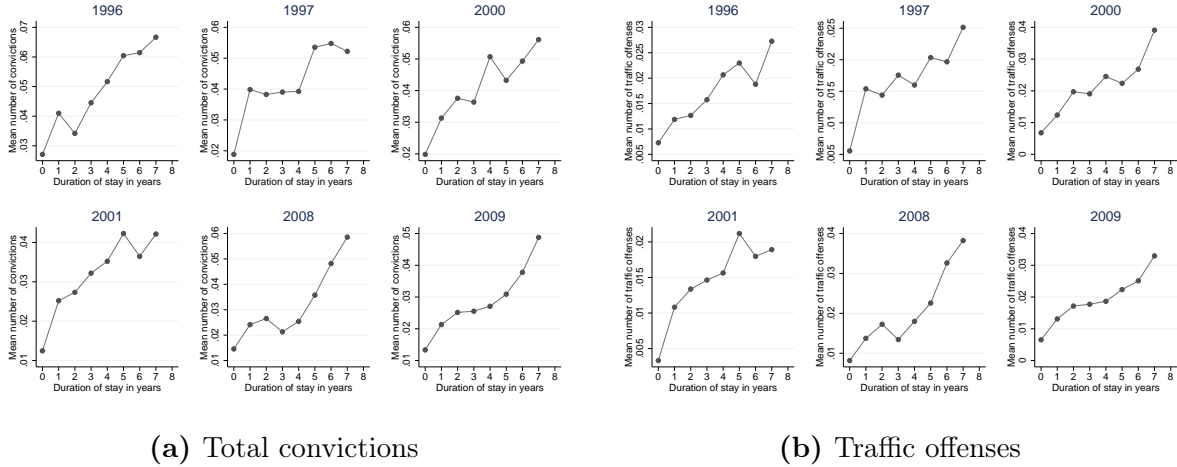


Figure B9: Mean yearly number of total convictions and traffic offenses for immigrants by their duration of stay in Denmark for six exemplary years.

B.3 Detailed information on the analyses involving non-Western immigrants in Denmark using the raw sample

In this section, we report estimation results using the raw (not pre-processed) sample of non-Western immigrants. The respective sample composition is depicted in Figure B6a. Please keep in mind that in this sample the number of individuals to the right and the left of the threshold needs not to be balanced with regard to nationality, sex, and election year.

We find no systematic effect for the full raw sample (in contrast to the main analysis based on the pre-processed data). However, the main results for male immigrants suggest an effect in the same direction and of comparable size to the main analysis. We nonetheless concentrate on the pre-processed sample in our main analysis, as we want to minimize concerns about the sample composition.

Table B15: The effect of voting rights on legal norm violations of non-Western immigrants in Denmark - Raw sample

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Effect τ	-0.0202 (0.0237)	-0.0234 (0.0206)	-0.0215 (0.0190)	0.0303 (0.0245)	0.0231 (0.0208)	0.0152 (0.0188)	-0.0796* (0.0439)	-0.0776** (0.0388)	-0.0649* (0.0361)
\hat{m}_-	0.0796	0.0913	0.0922	0.0283	0.0334	0.0374	0.148	0.169	0.167
Bandwidth	90	120	150	90	120	150	90	120	150
N left	1,497	2,023	2,615	848	1,131	1,430	649	892	1,185
N right	1,712	2,256	2,745	924	1,205	1,461	788	1,051	1,284

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the raw sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B16: The effect of voting rights on legal norm violations of non-Western employed and non-employed male immigrants in Denmark - Raw sample

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	male employed			male non-employed			male employed		
Time	after elections						before elections		
Effect τ	-0.156** (0.0683)	-0.145** (0.0599)	-0.131** (0.0547)	0.00784 (0.0526)	0.00549 (0.0469)	0.0154 (0.0461)	-0.0342 (0.0525)	-0.0148 (0.0475)	0.00715 (0.0440)
\hat{m}_-	0.235	0.244	0.232	0.0476	0.0778	0.0880	0.124	0.119	0.117
Bandwidth	90	120	150	90	120	150	90	120	150
N left	359	515	657	290	377	528	358	514	656
N right	431	559	679	357	492	605	431	559	679

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the raw sample of non-Western immigrants. Columns VII to IX report the discontinuity estimates for convictions during the first two years of stay.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

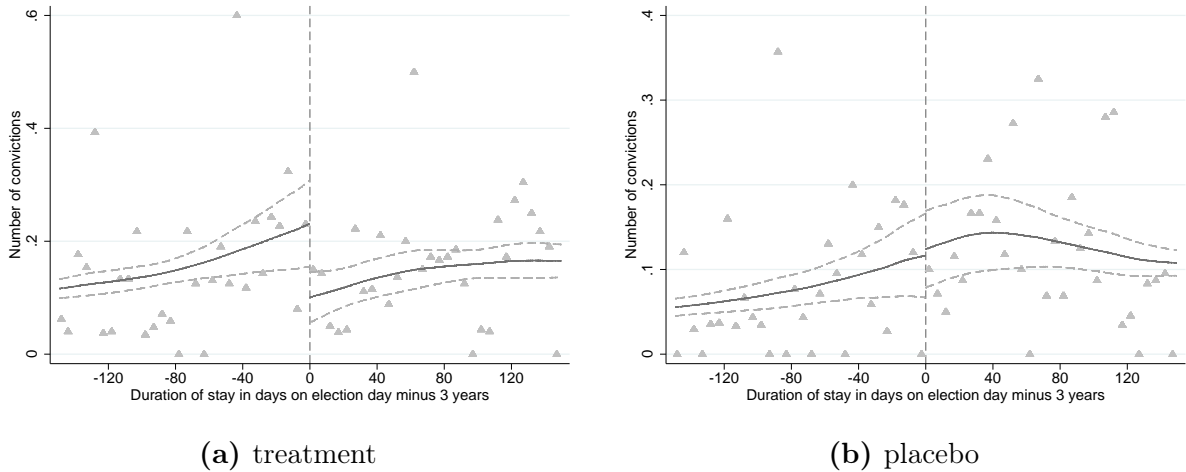


Figure B10: Regression discontinuity graph for the effect of the right to vote on convictions for non-Western immigrants in Denmark. Local linear smooth, applied separately on both sides of the threshold, using a bandwidth of 150 days and a triangular kernel based on the sample of employed men in the raw sample. Estimates for the treatment sample using the two years after the election are presented on the left-hand side, and estimates for the placebo sample using the outcomes of individuals in their first two years of stay are presented on the right-hand side (column II and column VIII of Table B16). The dashed lines represent the 90% confidence intervals of the smooth, and the gray dots represent the binned means of the dependent variable (binwidth 5 days).

B.4 Analysis of an alternative measure of *petty offenses* involving non-Western immigrants in Denmark

The results presented above indicate that the main effect is driven by traffic offenses, which could be regarded as *petty offense*. An alternative way to measure this kind of offenses directly would be to consider the offenses which are eventually sanctioned with a fine, instead of a more severe sanction such as a driving ban or a prison sentence.

To support the idea that our effect is driven by petty offenses, we repeat our analysis of the pre-processed sample using the number of an individuals' fines in the two years after the election as the dependent variable.

As the tables and graphs in this section show, when using this alternative dependent variable, we find results that are very much in line with the findings in the main analysis. There is a systematic reduction in the number of fines for individuals who have been eligible to vote. The effect seems, once again, to be mainly driven by males. And it primarily holds in the first 1.5 years after the elections.

Table B17: The effect of voting rights on the number of fines of non-Western immigrants in Denmark

Dependent variable: Number of fines									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Conventional	-0.0566* (0.0332)	-0.0598** (0.0291)	-0.0660** (0.0264)	-0.0140 (0.0271)	-0.0112 (0.0228)	-0.0117 (0.0202)	-0.129* (0.0717)	-0.139** (0.0638)	-0.152*** (0.0578)
\hat{m}_-	0.0991	0.106	0.108	0.0436	0.0429	0.0430	0.194	0.210	0.213
Bandwidth	90	120	150	90	120	150	90	120	150
N left	668	858	1,040	395	505	610	273	353	430
N right	668	858	1,040	395	505	610	273	353	430

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B18: The effect of voting rights on the number of fines of non-Western employed and non-employed male immigrants in Denmark

Dependent variable: Number of fines									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	male employed			male non-employed			male employed		
Time	after elections						before elections		
Effect τ	-0.225** (0.113)	-0.218** (0.0996)	-0.222** (0.0896)	0.00607 (0.0709)	-0.0249 (0.0636)	-0.0453 (0.0580)	-0.0564 (0.0776)	-0.0468 (0.0681)	-0.0421 (0.0608)
\hat{m}_-	0.284	0.295	0.296	0.0694	0.0888	0.0947	0.128	0.118	0.114
Bandwidth	90	120	150	90	120	150	90	120	150
N left	156	206	249	117	147	181	156	206	249
N right	154	207	250	119	146	180	154	207	250

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of non-Western immigrants. Columns VII to IX report the discontinuity estimates for the number of fines during the first two years of stay.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

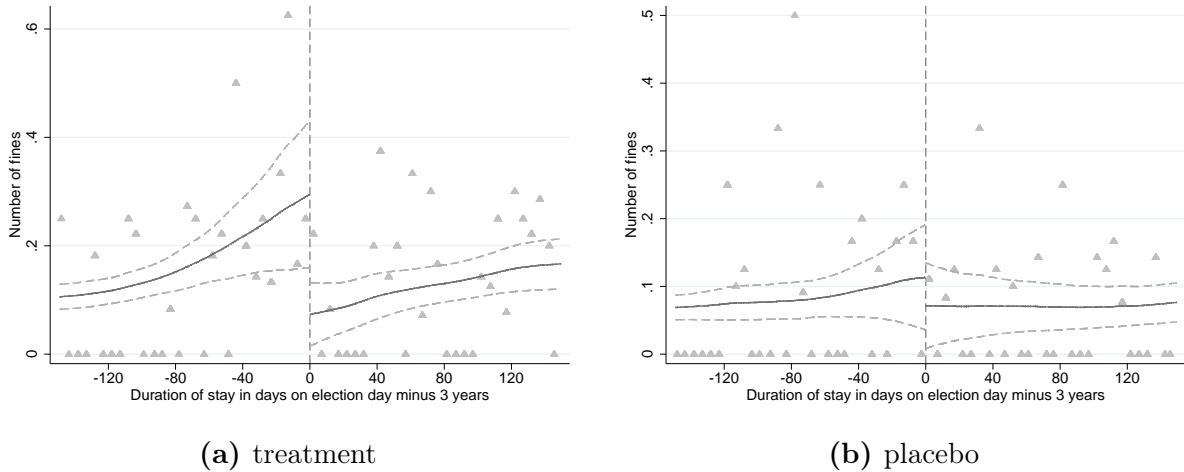


Figure B11: Regression discontinuity graph for the effect of the right to vote on the number of fines for non-Western immigrants in Denmark. Local linear smooth, applied separately on both sides of the threshold, using a bandwidth of 150 days and a triangular kernel based on the sample of employed men. Estimates for the treatment sample using the two years after the election are presented on the left-hand side, and estimates for the placebo sample using the outcomes of individuals in their first two years of stay are presented on the right-hand side (column II and column VIII of Table B18). The dashed lines represent the 90% confidence intervals of the smooth, and the gray dots represent the binned means of the dependent variable (binwidth 5 days).

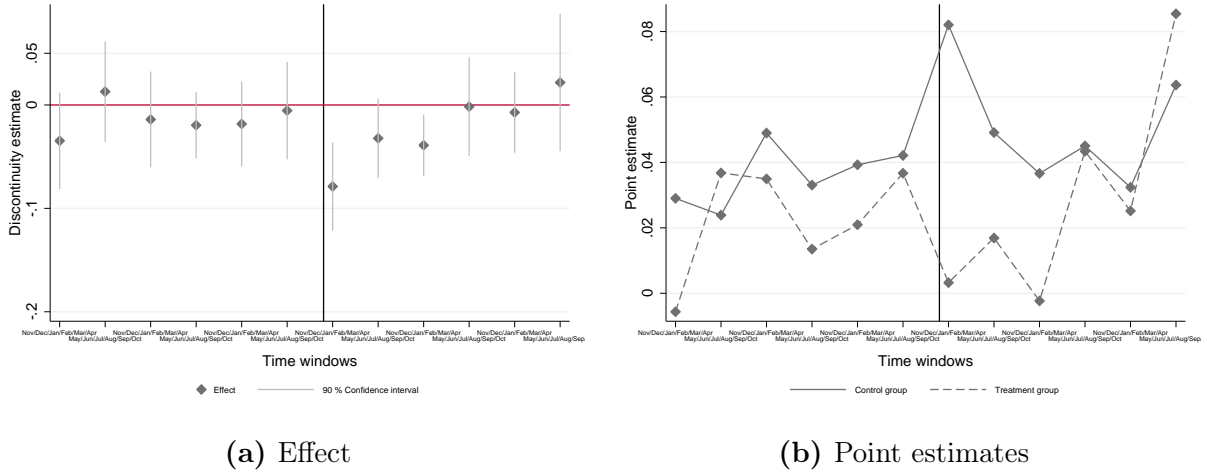


Figure B12: Time structure of the effect of voting rights on the number of fines. The graph on the left-hand side shows the discontinuity estimates for the sample of non-Western male immigrants, using the total of fines per individual in 6-month time windows as the dependent variable and a bandwidth of 150 days. The graph on the right-hand side shows the point estimates of the constant term of the smooth at the threshold, coming from the left-hand side (control) and coming from the right-hand side (treated), also using a bandwidth of 150 days. The solid black line indicates the start of the treatment period in November of the election year. The estimation results can be found in Table B19.

Table B19: Time structure of the effect of voting rights on the number of fines

Dependent variable: Number of fines within 6 months												
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
Time band	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct	Nov-Apr	May-Oct
Time	t-2		t-1		t		t+1		t+2		t+3	
Effect τ	-0.0347 (0.0283)	0.0129 (0.0295)	-0.0140 (0.0282)	-0.0196 (0.0194)	-0.0183 (0.0249)	-0.00542 (0.0287)	-0.0788*** (0.0260)	-0.0322 (0.0234)	-0.0390** (0.0181)	-0.00161 (0.0290)	-0.00721 (0.0237)	0.0218 (0.0404)
\hat{m}_-	0.0290	0.0235	0.0587	0.0401	0.0382	0.0609	0.0873	0.0539	0.0542	0.0523	0.0367	0.0660
Bandwidth	0.0290	0.0239	0.0490	0.0331	0.0393	0.0421	0.0820	0.0491	0.0366	0.0450	0.0324	0.0637
N left	88	430	430	430	430	430	430	430	430	430	430	430
N right	317	430	430	430	430	430	430	430	430	430	430	430

Notes: Local linear sharp regression discontinuity estimates for a bandwidth of 150 days using the total of fines per individual in 6-month time windows as the dependent variable and a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of non-Western male immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

B.5 Detailed information on the analyses involving immigrants from EU countries in Denmark

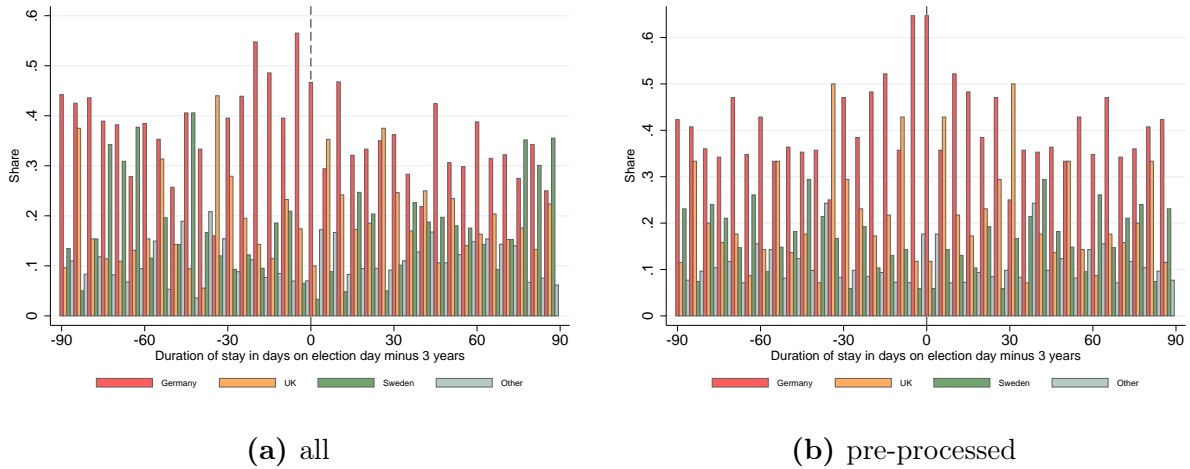


Figure B13: Composition of the sample for EU immigrants. The histograms show the share of observations for nationalities in the sample within bins of 5 days, separately to both sides of the threshold. For reasons of readability, the category ‘Other’ comprises observations of individuals from the Netherlands, France, and Italy. The left-hand side shows the overall raw sample, while the right-hand side shows the pre-processed estimation sample of EU-citizens.

In a final set of RDD analyses, we run a placebo test for immigrants from six EU countries that exhibit a rather stable immigration pattern over time; i.e., Germany, the Netherlands, Sweden, the United Kingdom, France, and Italy. Immigrants from these countries are allowed to vote in local elections right away when they take up residence in Denmark. The placebo test is performed using the exact same specification and time window as in the main analyses (for immigrants who are subject to the treatment assignment after some duration of stay). Figure B13 shows the composition of the raw and the pre-processed sample around the threshold. The largest group is made up of immigrants from Germany followed by people from the UK. The results of the sharp regression discontinuity analyses are reported in Tables B20 and B21. For all the specifications, no systematic difference around the threshold is estimated. This clearly indicates that there is no factor affecting convictions that kicks in after three years of stay that is somehow related to the election dates. Figure B14 graphically presents this point for the results in specifications II and VIII in Table B21.

Table B20: Placebo test for legal norm violations of EU immigrants at the threshold value

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	all			female			male		
Effect τ	-0.0179 (0.0454)	-0.0203 (0.0419)	-0.0186 (0.0381)	0.00976 (0.0319)	0.00117 (0.0355)	-8.30e-05 (0.0351)	-0.0391 (0.0757)	-0.0361 (0.0674)	-0.0318 (0.0601)
\hat{m}_-	0.0848	0.0798	0.0735	0.0258	0.0325	0.0308	0.127	0.127	0.102
Bandwidth	90	120	150	90	120	150	90	120	150
N left	412	558	677	169	230	277	243	328	400
N right	412	558	677	169	230	277	243	328	400

Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of EU-citizens.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

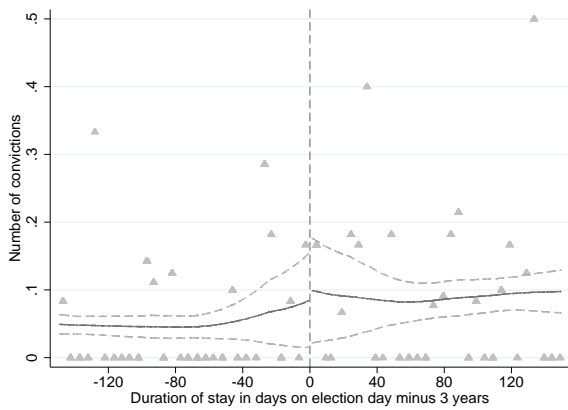
Table B21: Placebo test for legal norm violations of EU employed and non-employed male immigrants at the threshold value

Dependent variable: Number of convictions									
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.
Sample	male employed			male non-employed			male employed		
Time	after elections						before elections		
Effect τ	0.00817 (0.0772)	0.0149 (0.0693)	0.0143 (0.0626)	-0.193 (0.171)	-0.198 (0.155)	-0.172 (0.138)	0.0585 (0.0495)	0.0556 (0.0468)	0.0513 (0.0438)
\hat{m}_-	0.109	0.109	0.0850	0.181	0.175	0.154	0.0211	0.0227	0.0257
Bandwidth	90	120	150	90	120	150	90	120	150
N left	193	259	316	50	69	84	193	259	316
N right	179	246	299	64	82	101	179	246	299

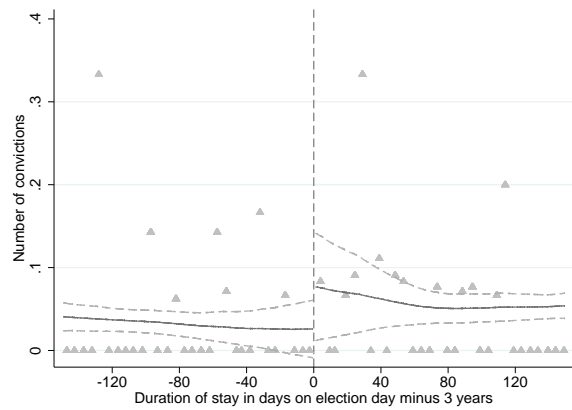
Notes: Local linear sharp regression discontinuity estimates for three bandwidths using a triangular kernel. Standard errors in parentheses. \hat{m}_- stands for the point estimate of the LLR smooth at the threshold value approaching it from the left. Estimates are performed using the pre-processed sample of EU-citizens. Columns VII to IX report the discontinuity estimates for convictions during the first two years of stay.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

The observations that treated and untreated individuals do not differ before the elections, that there is no systematic reaction at placebo election dates, and that EU-citizens whose treatment status does not differ to the left and the right of the threshold do not react at the three-year threshold all substantiate the causal claim of our results. The opportunity to participate in local elections reduces the likelihood that non-Western immigrants might violate legal norms in their host-country Denmark.



(a) after elections



(b) before elections

Figure B14: Regression discontinuity graph for the placebo test for EU nationals in Denmark. Local linear smooth, applied separately on both sides of the threshold, using a bandwidth of 150 days and a triangular kernel based on the sample of men. Estimates for the treatment sample using the two years after the election are presented on the left-hand side, and estimates for the placebo sample using the outcomes of individuals in their first two years of stay are presented on the right-hand side (column II and column VIII of Table B21). The dashed lines represent the 90% confidence intervals of the smooth, and the gray dots represent the binned means of the dependent variable (binwidth 5 days).

B.6 Alternative specification to measure the effect of the first possibility to vote on non-Western immigrants' number of offenses and fines

In this section, we report the results of an alternative strategy to exploit the quasi-randomization narrowly around the threshold to see whether the possibility to vote affects the number of offenses of immigrants. We use the panel structure of the data to compose two groups - one treated and one not - to estimate a fixed effects model comparing the evolution of the number of convictions between those groups. Moreover, we control for the duration of stay in Denmark. In the control group are individuals with a value of the assignment variable between -45 and -1 . In the treatment group those with a value of the assignment variable between 0 and 45 . Individuals in the two groups are rather comparable in their characteristics and their duration of stay in the country. While this specification ignores that only individuals just at the threshold are approximately randomized, it should at least render a lower bound of the true effect. Moreover, this approach has the advantage that it allows us to control for all time invariant characteristics of individuals in our sample. The dependent variable captures the number of convictions per individual in 12 month bands, always between November and October in the subsequent year. In the year just before elections (Year0 in the following tables), men (and employed men respectively) in the pre-processed sample, on average, have 0.080 convictions in total, 0.056 traffic convictions and 0.063 fines (0.073 conviction in total, 0.061 traffic convictions and 0.061 fines). Please note that there is a large overlap between the latter two categories. In the first year after the elections (Year1), the corresponding means are 0.094 convictions in total, 0.073 traffic convictions and 0.073 fines for men and 0.115 convictions in total, 0.085 traffic convictions and 0.085 fines for employed men. The results using the pre-processed and the raw sample are reported in Tables B22 and B23. We find the results to be very much in line with our main RDD estimates supporting our main conclusion. There is no statistically significant difference in the level of convictions before the intervention

(Year0), but a decline in the first year (Year1) after male immigrants in the treatment group had the possibility to participate in the elections.

Table B22: Alternative parametric specification for the pre-processed sample of non-Western immigrants

Dependent variable: Number of convictions between November and October in the following year						
	I.	II.	III.	IV.	V.	VI.
Category	Total		Traffic		Fines	
Sample	Male	Male emp.	Male	Male emp.	Male	Male emp.
Year0	-0.0325 (0.0498)	-0.0234 (0.0568)	-0.00838 (0.0358)	0.0117 (0.0470)	-0.0395 (0.0431)	-0.0250 (0.0532)
Year1	-0.0305 (0.0774)	0.0407 (0.0955)	0.0213 (0.0552)	0.0643 (0.0750)	-0.0212 (0.0680)	0.0273 (0.0935)
Year2	-0.105 (0.0955)	-0.0580 (0.118)	-0.0469 (0.0645)	0.0123 (0.0855)	-0.108 (0.0831)	-0.0599 (0.114)
Year3	-0.138 (0.124)	-0.110 (0.161)	-0.0451 (0.0833)	-0.0281 (0.116)	-0.118 (0.111)	-0.101 (0.157)
Treat*Year0	-0.0147 (0.0388)	-0.0116 (0.0474)	-0.000870 (0.0315)	-0.0223 (0.0427)	-0.00334 (0.0353)	-0.0213 (0.0433)
Treat*Year1	-0.0705 (0.0453)	-0.117* (0.0596)	-0.0776** (0.0380)	-0.117** (0.0527)	-0.0940** (0.0397)	-0.141** (0.0548)
Treat*Year2	-0.0286 (0.0401)	0.0158 (0.0586)	-0.0149 (0.0349)	-0.00658 (0.0558)	-0.0173 (0.0357)	0.00609 (0.0558)
Treat*Year3	0.0269 (0.0481)	0.0517 (0.0642)	0.0127 (0.0427)	0.0167 (0.0568)	0.00336 (0.0459)	0.0177 (0.0620)
Duration	0.0399 (0.0256)	0.0289 (0.0332)	0.0262 (0.0176)	0.0172 (0.0245)	0.0377 (0.0231)	0.0291 (0.0326)
Constant	0.000759 (0.0396)	0.0153 (0.0531)	-0.0132 (0.0282)	0.00848 (0.0410)	-0.00801 (0.0364)	0.00963 (0.0525)
Observations	1,627	940	1,627	940	1,627	940
R-squared	0.009	0.018	0.018	0.020	0.011	0.017
No. of clusters	287	165	287	165	287	165
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Parametric fixed effects model comparing individuals within ± 45 days around the threshold in the time after the elections. *Treat* is an indicator for the treatment group, i.e., those narrowly allowed to vote with a positive value of the assignment variable and *Year*. are indicators for the years after the election starting with 0 in the election year up to 3 which is the third year after the election. The indicator *Year1* thus estimates the effect in the 12 months after the election. *Duration* captures the duration of stay in Denmark in years. Robust standard errors are reported in parentheses and are clustered at the individual level. Estimates are performed using the pre-processed sample of male non-Western immigrants. Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.

Table B23: Alternative parametric specification for the raw sample of non-Western immigrants

Dependent variable: Number of convictions between November and October in the following year						
	I.	II.	III.	IV.	V.	VI.
Category	Total		Traffic		Fines	
Sample	Male	Male emp.	Male	Male emp.	Male	Male emp.
Year0	-0.0420 (0.0284)	-0.0398 (0.0368)	-0.0243 (0.0216)	-0.0372 (0.0318)	-0.0394 (0.0253)	-0.0505 (0.0351)
Year1	-0.0584 (0.0434)	-0.0250 (0.0599)	-0.0226 (0.0329)	-0.0201 (0.0500)	-0.0402 (0.0395)	-0.0336 (0.0586)
Year2	-0.0920* (0.0549)	-0.0715 (0.0746)	-0.0497 (0.0411)	-0.0491 (0.0611)	-0.0810 (0.0496)	-0.0831 (0.0736)
Year3	-0.117 (0.0711)	-0.113 (0.100)	-0.0738 (0.0518)	-0.0985 (0.0784)	-0.0875 (0.0653)	-0.102 (0.0985)
Treat*Year0	-0.0237 (0.0224)	-0.0449 (0.0285)	-0.0202 (0.0178)	-0.0354 (0.0252)	-0.00599 (0.0198)	-0.0192 (0.0259)
Treat*Year1	-0.0438* (0.0253)	-0.0910** (0.0371)	-0.0459** (0.0210)	-0.0763** (0.0330)	-0.0434* (0.0224)	-0.0757** (0.0338)
Treat*Year2	-0.0263 (0.0255)	-0.0394 (0.0351)	-0.0198 (0.0218)	-0.0349 (0.0337)	-0.0117 (0.0217)	-0.0138 (0.0327)
Treat*Year3	-0.00866 (0.0281)	-0.0292 (0.0378)	0.000641 (0.0242)	-0.0352 (0.0332)	-0.0112 (0.0258)	-0.0292 (0.0364)
Duration	0.0336** (0.0145)	0.0312 (0.0204)	0.0242** (0.0108)	0.0290* (0.0160)	0.0265** (0.0134)	0.0291 (0.0200)
Constant	0.0117 (0.0225)	0.0243 (0.0320)	-0.00322 (0.0168)	0.00366 (0.0249)	0.00787 (0.0211)	0.0165 (0.0312)
Observations	3,936	2,213	3,936	2,213	3,936	2,213
R-squared	0.004	0.007	0.007	0.009	0.004	0.006
No. of clusters	693	389	693	389	693	389
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Parametric fixed effects model comparing individuals within ± 45 days around the threshold in the time after the elections. *Treat* is an indicator for the treatment group, i.e., those narrowly allowed to vote with a positive value of the assignment variable and *Year.* are indicators for the years after the election starting with 0 in the election year up to 3 which is the third year after the election. The indicator *Year1* thus estimates the effect in the 12 months after the election. *Duration* captures the duration of stay in Denmark in years. Robust standard errors are reported in parentheses and are clustered at the individual level. Estimates are performed using the raw sample of male non-Western immigrants.

Significance levels: * $.05 < p < .1$, ** $.01 < p < .05$, *** $p < .01$.