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// MICHAELA SLOTWINSKI, ALOIS STUTZER,
AND PIETER BEVELANDER

From Participants to Citizens? Democratic Voting Rights and Naturalization Behavior

From participants to citizens? Democratic voting rights and naturalization behavior *

Michaela Slotwinski[†] Alois Stutzer[‡] Pieter Bevelander[§]

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Abstract

We study the causal effect of the possibility to vote on foreigners' propensity to naturalize, a key indicator of successful integration. Based on Swedish administrative data and an institutional setting producing a quasi-random assignment of the eligibility to vote, we find that the overall effect depends on the composition of the migrant population. For immigrants from places with poor living conditions, we observe that the experience of non-citizen voting rights substantially increases their propensity to naturalize. In contrast, for those coming from places with a high standard of living, the same experience reduces it. Both reactions clearly reveal that individuals assign a positive value to formal democratic participation rights. While the behavior of the former group is likely dominated by the motivational force inherent in the possibility to participate, the behavior of the latter group reflects the devaluation of formal citizenship if it is decoupled from democratic rights.

Keywords: citizenship, migration, naturalization, value of voting, voting rights

JEL classifications: D02, J15, K37

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[†]University of Basel, Faculty of Business and Economics, Peter Merian-Weg 6, 4002 Basel, Switzerland. ZEW - Centre for European Economic Research, L7,1, 68161 Mannheim, Germany. Phone: +41 (0)61 207 33 75, email: michaela.slotwinski@unibas.ch. (Corresponding author)

[‡]University of Basel, Faculty of Business and Economics, Peter Merian-Weg 6, 4002 Basel, Switzerland. Phone: +41 (0)61 207 33 61, email: alois.stutzer@unibas.ch.

[§]MIM, Malmö University, 205 06 Malmö, Sweden. Email: pieter.bevelander@mau.se

1 Introduction

Non-citizen voting rights (NCVR) are a key aspect in the current debate about the development of democratic institutions in an era of mass migration.¹ While an increasing number of countries allows for some mode of formal non-citizen political participation (see, e.g., Ferris et al., 2019; Munro, 2008), NCVR remain a controversial proposal in the public and academic discourse.² In this context, causal evidence on the potential consequences is essential in informing the political process. Theories of democracy emphasize different values individuals attach to the right to vote, most importantly some instrumental values related to the expected impact on policy outcomes, an expressive value derived from voting according to one's self-image, and a procedural value from experiencing agency or self-determination. This latter procedural aspect is, moreover, crucial to the building up of intrinsic motivation or civic virtue, affecting people's voluntary contribution to public goods in society.³

Empirically, the strength of these forces and the value of democratic participation rights is still not well understood. This is not limited to, but especially applies to immigrants. There are two main views of the impact of NCVR: The dominant one follows the idea of 'integration through participation', emphasizing the motivational force of inclusive or participatory democracy that is unleashed with NCVR (for a review of the related arguments, see, e.g., Munro, 2008). A complementary view points is the devaluation of formal citizenship if voting rights are decoupled from it, which would weaken incentives to integrate (see, e.g., Bevelander and Spång, 2015; Huddleston and Vink, 2015; Munro, 2008; Pickus, 1998; Schuck, 1989). While playing a role in many aspects of integration, the two forces are of primary relevance when it comes to the willingness of immigrants to take over the full set of rights and obligations that come with naturalization. Naturalization is, firstly, the key institution regulating access to the demos.

¹According to the latest OECD data (2016/2017), the non-citizen population amounts to, for example, 9.29% in the UK (2017), 6.60% in France (2016), 12.15% in Germany (2017), 47.20% in Luxembourg (2017), and 24.01% in Switzerland (2017).

²According to Beckman (2006) non-citizens are still likely the largest disenfranchised group, apart from children. Recent contributions analyzing the enfranchisement of non-citizens include, for example, Pedroza (2019) or Hayduk and Coll (2018) building on a rich literature of qualitative single country studies as well as comparative case studies that provide in-depth analyses on the discourse and conditions when parliaments engage in liberalizing or retracting NCVR (see, e.g., Benhabib 2004, Hayduk 2006, Howard 2010, Soysal 1994 and the contributions in the special issues on "Who Decides? Democracy, Power and the Local Franchise in Cities of Immigration" of the *Journal of International Migration and Integration* in 2015, and "Voting Rights in the Age of Globalization" of the journal *Democratization* in the same year edited by Caramani and Grotz 2015). Quantitative cross-sectional studies exploring the determinants of the introduction of NCVR are undertaken in Toral (2015), Earnest (2015) and Stutzer and Slotwinski (2019).

³We cannot do justice to the rich literature behind these core ideas referring to the value of democracy, which trace back to ancient thinkers. Some prominent contributions include the account of instrumental voting in Downs (1957) and the extended models capturing psychological benefits of voting in Riker and Ordeshook (1968) and Brennan and Lomasky (1997). The motivational aspect of democratic participation possibilities is emphasized, for example, in Pateman (1970), Lane (1988) and Frey (1997) or more recently in concepts of inclusive democracy by Acemoglu and Robinson (2012).

Secondly, it is seen as most important indicator of migrants' successful integration as well as an important trigger and driver of migrants' further inclusion in the host country (see, e.g., Hainmueller et al., 2017; Munro, 2008).

In this paper, we want to learn about the impact and the value of formal political rights by studying the consequences of experiencing NCVR for the naturalization behavior of immigrants in Sweden. The Swedish institutional setting generates quasi-experimental variation in the eligibility to vote, which we exploit by drawing on administrative data on the whole foreign population between 1998 and 2015. In Sweden non-EU immigrants are eligible to vote after having resided in the country for at least three years at the date when the election takes place.⁴ Individuals who have immigrated just before the threshold immigration date are thus eligible to vote (forming the treatment group) and individuals who have immigrated just several days later, after the threshold date, are not eligible to vote (forming the control group). If immigrants were not to choose their immigration date strategically with respect to this rule (an assumption that we validate later), this institutional setting generates a quasi-random assignment to the right to vote narrowly around the threshold (see, e.g., Cattaneo et al., 2015; Hahn et al., 2001; Imbens and Lemieux, 2008). The two groups around the threshold have spent almost the same amount of time in the country, are exposed to the same policies and electoral outcomes, and just happened to arrive some days apart. It can thus be assumed that if not for the local and regional elections three years after their arrival, the conditions determining these people's naturalization decision would have evolved in a similar way. As a consequence, comparing their naturalization behavior over time allows capturing the causal effect of experiencing the invitation to participate and the possibility of exercising formal democratic participation rights (as enrolment to the electoral register is automatic).

Besides the sharp assignment rule of voting eligibility described above, Sweden is exceptionally well suited to investigating the effect of NCVR on naturalization for two more reasons. First, Sweden has a comparatively high electoral turnout among immigrants (Hutcheson and Bevelander, 2018). Second, Sweden has a very liberal naturalization law which allows for naturalization after three to five years of stay. The latter aspect guarantees that the control group has no possibility to exert their political rights before being eligible to naturalize.

The two theoretical views introduced above relate to two hypotheses offering contrary predictions as to how NCVR would impact naturalization decisions. On the one hand, there is the *intrinsic motivation hypothesis*. The right to vote strengthens immigrants' involvement in the host country and provides them with a feeling of belonging and being treated with respect and dignity, thereby increasing perceptions of control and efficacy, finally motivating them to become formal members

⁴EU citizens and Norwegians are allowed to vote in the local and regional elections from the beginning of their stay in Sweden.

of the citizenry. On the other hand, there is the *disincentive hypothesis*. It is based on the idea of citizenship as a bundle of rights or benefits and obligations (see, e.g., Marshall, 1965). The benefits typically involve political rights, but also protection, permanent permission to stay in the country, full access to the social security system, or a new passport, which might impact visa restrictions. The costs refer to any mandatory military or civil service, or sometimes the requirement to give up one's original citizenship. If individuals assign a positive value to political participation rights, granting them before naturalization effectively removes one attractive part of that bundle. As a consequence, the remaining bundle of rights and obligations that can be achieved by naturalization, at the margin, becomes less attractive. In sum, NCVR offer rights without the formal obligations of citizenship and thus might reduce the incentive to naturalize. Due to these opposing forces, from a theoretical perspective, the net effect of NCVR on the willingness to naturalize is ambiguous. It is an empirical question whether one channel outweighs the other, or whether they just net each other out. However, and importantly, any observed reaction in this setting, be it positive or negative, would indicate that individuals assign a positive value to political participation rights. If the reaction is positive, immigrants experience a *procedural* value from the possibility to formally participate in the democratic process that *motivates* them to the extent that any disincentive is outweighed. If, in contrast, the reaction is negative upon learning of the right to vote, it indicates a lower bound estimate for the valuation of the formal democratic participation rights that is underestimated to the extent that these people also experience a positive motivation effect.

We empirically study the net effect for three groups of immigrants who likely differ in the relative value they assign to the experience of the possibility to vote in Sweden. In order to approximate the relative weight of the components of citizenship for individuals, we draw on information about the standard of living in their country of origin based on its score in the Human Development Index (HDI).⁵ Regarding the indicator of development, we have on one end of the spectrum immigrants from less developed countries (approximated by a low score on the HDI). They often migrate due to an adverse economic and political situation in their country of origin and many of them enter the host country as refugees. We consider individuals in this first group as 'push migrants', for whom the safety aspects of Swedish citizenship are most salient and who are potentially most affected by experiencing the procedural value of being invited to participate in a fair and free electoral process. At the other end of the spectrum, there are immigrants from countries with a high standard of living (approximated by a high score on the HDI). Many of them resettle primarily for employment reasons. These 'pull migrants', forming the third group, have likely lived somewhere with a relatively well functioning democratic process. For them, there is in general less to gain from Swedish citizenship, and the experience of NCVR might mainly

⁵For reasons of data anonymization, we do not have information on the absolute value of the HDI for each individual. However, we observe the origin countries' position in the distribution of HDI scores.

stimulate their awareness that they have a core right even without becoming Swedish citizens. For them, the valuation of the democratic participation right is most likely primarily reflected in a disincentive effect. In between, there is the second group of immigrants from countries with an intermediate level of development and score on the HDI. For them, both push and pull factors likely affected their migration, and experiencing the possibility to vote might be about learning to belong to or be part of the community but also about learning that one has a say in the democratic process even without naturalization. In sum, we would thus hypothesize that the incentive channel gains (and the motivation channel loses) weight the more an immigrant is 'pulled' instead of 'pushed' to the host country.

In the empirical analysis, we compare how the probability of naturalizing for non-EU citizens in the control (just missing eligibility to vote in the last election) and treatment (just eligible to vote in the last election) group evolves in the up-to-four-year period after the election. For immigrants from less developed countries, we find that the treatment group is systematically more likely to naturalize than the control group. In contrast, for immigrants from highly developed countries the probability of naturalizing in the treatment group is systematically lower than in the control group. There is no systematic difference between the control and the treatment group for immigrants from countries with an intermediate level of development. We thus find that both mechanisms referring to the intrinsic motivation as well as the disincentive effect are at work. As hypothesized, the motivation channel seems more important for individuals we frame as 'push' migrants, while the disincentive channel seems to have more weight for those we describe as 'pull' migrants. This shows that any net effect of the introduction of NCVR on naturalization rates strongly depends on the background of the migrant population, and is by no means necessarily positive or negative.⁶

We provide several robustness checks in our endeavor to validate that our results are indeed causally driven by the experience of the possibility to vote and to exclude possible alternative explanations. First, we validate that the threshold rule of eligibility is indeed applied and that our definition of the treatment and control group actually discriminates between those eligible and not eligible to vote. This is possible because we have access to administrative data on who was eligible to vote in the respective elections. Second, we validate that there is no selection in the timing of immigration with regard to the subsequent elections. We do not observe that there are particularly more or fewer arrivals in the days before than after the eligibility threshold. Third, we make use of EU citizens, who are eligible to vote right from the beginning of their stay in Sweden, as a quasi-placebo sample. We do not observe diverging naturalization behavior

⁶Our results capture the average effect of the first exposure to the possibility to vote in the host country. Our estimates could in principle be a lower bound, if what matters is the act of voting and not everyone turns out. However, it could as well be driven by the experience of the possibility and inclusion per se. Our data does not allow us to distinguish between these two mechanisms.

between the control and treatment group in this sample. Fourth, we study immigrants around placebo election dates, i.e., in non-election years. This helps us to exclude that the divergence we observe between treatment and control groups in the main sample is driven by, for example, the, on average, longer duration of stay in the treatment group or any other factors that affect the groups on either side of the threshold differently. Fifth, we present conventional regression discontinuity estimates for the probability of being naturalized in the fourth year after the election (or after 7 years of stay in the country), which confirm our findings.

Our analysis can specifically be related to two strands of literature. First, it contributes to the emerging literature on the effects of NCVR on immigrant behavior as well as on policy outcomes. In an early study, Vernby (2013) shows that the introduction of NCVR in Sweden in the 1970s increased spending on education as well as social and family services in municipalities where non-citizens made up a non-negligible share of the electorate. The effect is understood to work via the electoral demand of the newly enfranchised non-citizens. In a similar vein, Ferwerda (2020) finds a substantial increase in local social spending in response to the introduction of NCVR in Belgium and Switzerland in the 2000s. The timing of the reaction, however, suggests that the transfers are increased to mobilize immigrant voters. In their study for Norway, Ferwerda et al. (2020) document that immigrants who were exposed to voting rights early are more likely to turn out for the following election. This effect is driven by immigrants from countries with a weak democratic culture (or a lower level of development). In the institutional context of Denmark, Slotwinski et al. (2017) find that the exposure to voting rights temporarily reduces criminal convictions of immigrants from less developed countries. To these previous studies, we contribute the first rigorous evaluation of the causal impact of the possibility to participate in the political process on an important integration outcome, i.e., the decision to naturalize.⁷ Second, and from a broader perspective, our results contribute to the literature on individuals' valuation of formal political participation rights motivated inter alia by the idea of a participation paradox of the rational voter. While conceptually demanding, different approaches have been proposed to get at specific benefits of the right to vote and its actual use. In a series of experiments, researchers have been working towards capturing the value of voting rights by trying to estimate the willingness to pay for the possibility to vote. Examples are Dittmann et al. 2014 for the instrumental value, Tyran and Wagner 2019 for the expressive value and Güth and Weck-Hannemann 1997, or Tontrup

⁷There has been one previous attempt to exploit the Swedish setting to evaluate the effect of the possibility to vote on naturalization, namely by Engdahl et al. (2020). The authors conclude that the early voting opportunity is not effective and that it does not improve political integration, as measured by naturalization and subsequent voting. However, the data they use does not seem to allow them to identify the immigration date precisely. They provide no evidence that the applied threshold date effectively separates the treatment and control groups (as they probably lack the official eligibility information). Moreover, they seem not to have access to information about the reason for migration (which is important for the sample specification, as we show below). This is probably the reason why they fail to exclude students from their sample, which likely leads to the imbalance in their RDD estimates and invalidates their design.

and Morton 2016, for some total consumption value. Based on reports of people’s satisfaction with life, a direct valuation in terms of individual well-being has also been attempted for the use of voting rights (see, e.g., Weitz-Shapiro and Winters 2011) and democratic participation rights per se (Frey et al., 2004; Stutzer and Frey, 2006).

Our findings strongly suggest that individuals indeed care about political rights and assign a positive value to democratic participation possibilities per se, independent of outcomes. This implies a procedural value for voting rights and indicates that there is nothing very paradoxical about people turning out to vote.

The remainder of the paper is structured as follows. Section 2 describes the institutional setting in Sweden. Section 3 introduces the data and Section 4 introduces the applied empirical approach. Sections 5 and 6 present the main empirical findings as well as several robustness checks. Section 7 offers some concluding remarks.

2 Setting

Our analysis studies the causal effect of the possibility to vote on the propensity to naturalize within the institutional setting of Sweden. Specifically, we make use of Swedish electoral law, which offers a sharp assignment rule of local and regional voting rights for non-citizens after three years of stay in the country. We visualize the assignment rule in Figure 1. Non-citizens from non-EU countries are granted the right to vote in local and regional elections after three years of stay in the country.⁸ As a consequence, individuals like X, who arrived slightly more than three years before the next election, are eligible to vote, while individuals like Y, who arrived slightly later (their duration of stay being just under 3 years at the next election date), are not. Narrowly around the eligibility threshold, i.e., the date three years before the election date, this rule produces a treatment and a control group which have spent an approximately similar amount of time in the country, are exposed to similar policies and electoral outcomes. There should not be, and we see no indication for, a systematic selection of people regarding the timing of their immigration dates with respect to the next election date. As a consequence this assignment rule renders two comparable groups but for the difference in their political participation possibility after three years.⁹ Or to put it differently, this mechanism introduces a quasi-randomization into the treatment (the possibility to vote), which we can exploit to learn about the effect of experiencing the possibility to vote on individuals’ naturalization behavior. We can track individual outcomes over time to see whether the naturalization behavior of people like X in the treated group evolves differently compared to that of people like Y in the control

⁸EU-citizens and citizens of Nordic countries, on the other hand, are eligible to vote from the beginning of their stay.

⁹We validate this assumption in Section 3.

group.

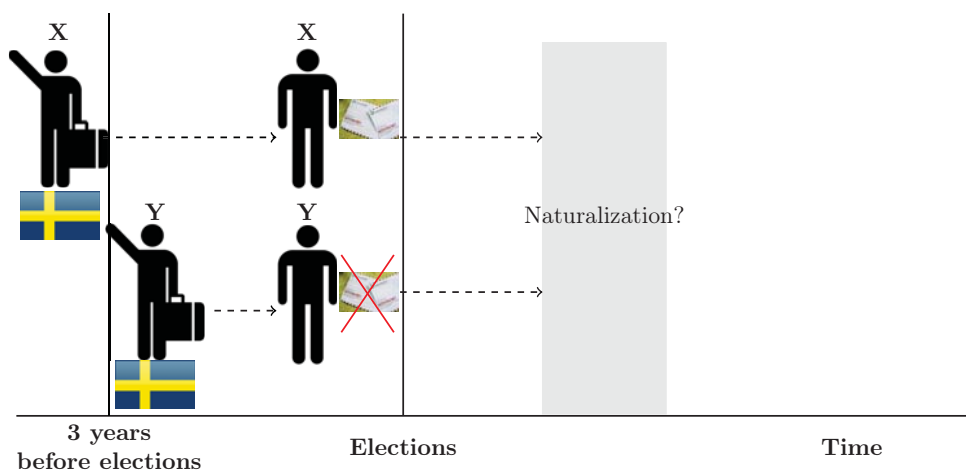


Figure 1: Identification strategy

Notes: Immigrants from non-EU countries are eligible to vote in local and regional elections after three years of residence in Sweden. People like X, immigrating before the respective threshold date three years before the next election, receive the polling card before the next election and form the treatment group. In contrast, people like Y, immigrating after the respective threshold date, are not eligible to vote, do not receive a polling card, and form the control group. We follow both groups over time, comparing whether the probability of naturalizing evolves systematically differently in the group that was treated compared to the control group.

According to the electoral law of 1997 (and the update in 2005), residents in the electoral register automatically receive a polling card in the weeks before the election.¹⁰ We can thus assume that eligible voters are informed about their participation possibilities once they are eligible.¹¹ As stated above, immigrants in Sweden make extensive use of their right to vote, also in comparison with non-citizens in other European countries. According to Hutchesson and Bevelander (2018), the average turnout of non-citizens was about 35.2 % (and 72 % for naturalized citizens) in the 2014 local elections.

A second appealing institutional characteristic making Sweden especially suited for our study is the comparatively short waiting period until immigrants become eligible to apply for naturalization. While this period is often long, for instance, 12 years in Switzerland, 9 years in Denmark, or 7 years in Norway, immigrants in Sweden become eligible after a maximum of five years. In the general case people must have resided in Sweden (with a corresponding permit) for a continuous period of 5 years. There are three main exceptions to this rule. Stateless individuals

¹⁰Polling cards are typically sent to eligible voters about 18 days before the election. The letter contains information on how and where the eligible person can vote. It also contains a link to the Swedish Electoral agency's website, where more information about the voting procedure can be found in about 40 different languages. It does not contain any moral statements or nudges that could be seen as additional treatment.

¹¹There is no evidence as to whether individuals know about this possibility before. If they were informed, and were to internalize this knowledge from the beginning of their stay, any effect we measure would be a lower bound of the actual effect.

and refugees can apply after only four years, and those who have lived with a Swedish partner for at least two years can apply after only three years. There are no formal requirements with respect to integration in Sweden. There is neither a language nor an integration test, nor are there any economic requirements. This liberal regime leads to a rather high naturalization rate. Depending on their region of birth, between 50 and 90 percent of all non-EU immigrants have acquired Swedish citizenship after ten to fifteen years in the country (Helgertz and Bevelander, 2017). An important resulting fact for our empirical strategy is that immigrants in the treatment and control groups become eligible for naturalization before those in the control group acquire the possibility to vote in the subsequent election.

3 Data

We draw on comprehensive Swedish administrative data for the whole population with a migration background between 1997 and 2015.¹² In the following, we describe the samples that we define for our analyses, address the assumption of local randomization, and introduce the complementary data characterizing immigrants' country of origin in terms of living standards.

Sample definition

For our main analysis, we compose an election-year sample of individuals with origin in non-EU countries immigrating in the ± 10 days around the date three years before an election. Elections in Sweden take place every four years on the second Sunday in September. Our data window thus allows us to exploit 3 elections for which we observe migrant outcomes in the first three years of stay and the four years after the election. These were the local and regional elections in 2002, 2006, and 2010 that took place on: 15 September 2002, 17 September 2006, and 19 September 2010. The corresponding immigration dates marking the eligibility threshold, are: 14 September 1999, 16 September 2003, and 18 September 2007. For the validation, we similarly compose a non-election-year sample based on placebo-election dates. These are dates at which the elections would have been held if the years were election years, i.e., on the second Sunday in September of the respective non-election year.¹³

Moreover, we compose an additional quasi-placebo sample of individuals who are not subject to the three-year assignment rule, i.e., individuals who are EU citizens or citizens of Nordic

¹²Specifically, we rely on the STATIV data set, which is a longitudinal database for integration studies. This is proprietary data of the Swedish Statistical Office and can thus not be publicly archived. Interested researchers can, however, apply for access.

¹³This procedure leaves us with the following placebo-election dates (and the respective immigration date thresholds in brackets): 17 September 2000 (16 September 1997), 16 September 2001 (15 September 1998), 14 September 2003 (13 September 2000), 19 September 2004 (18 September 2001), 18 September 2005 (17 September 2002), 16 September 2007 (15 September 2004), 21 September 2008 (20 September 2005), 20 September 2009 (1 September 2006), and 18 September 2011 (17 September 2008).

countries. They are eligible to vote right from the beginning of their stay, and thus form an attractive placebo group to test whether there are other factors, not related to the exposure to voting rights, that could explain any differential development in naturalization probabilities. Within these samples, we define two groups, the treatment group (with subjects immigrating up to 10 days before the real or placebo eligibility threshold) and the control group (with subjects arriving up to 10 days after the real or placebo eligibility threshold).

Throughout, the estimation samples include individuals whose nationality is known, who are above the age of 21 in the election year, and who have one immigration date, i.e., do not emigrate and immigrate again, as otherwise the eligibility is hard to determine.¹⁴ We follow individuals during the first 7 years of their stay in the country, i.e., starting in the first year of stay until 4 years after the election. We are mainly interested in individuals' naturalization behavior. Our dependent variable thus is an indicator set to one if an individual is naturalized (holds Swedish citizenship) in a given year.¹⁵

Local randomization

The identification of the causal effect in our setting is based on the assumption that there is local randomization around the threshold, or to put it differently, that individuals do not target their immigration date to Sweden just to obtain the possibility to vote earlier or later (see, e.g., Lee and Lemieux, 2010). Practically speaking, this is difficult to accomplish even if the immigration is planned far ahead, because the immigration administration determines when the residency permit is issued. For refugees, it is virtually impossible, to target the immigration date which for them will turn out to be the date their status is accepted. If, however, individuals were somehow to select the date, this should be clearly visible as excess mass in the distribution of immigration dates around the date that marks the eligibility threshold (McCrary, 2008). Figure 2 shows the distribution of immigration dates in the 90 days around the eligibility date in election years for the overall sample and the sample of non-EU immigrants. We do not observe that the number of immigrants is disproportionally higher or lower just before the threshold (just below zero in Figure 2). This supports the assumption that there is no particular targeted selection with respect to this date.¹⁶

Given this quasi-random assignment, the individuals just to the right and the left of the threshold should, on average, be comparable in their observable and unobservable characteristics. However,

¹⁴The relevant date is actually the date when immigrants become a formal part of the population (residents), i.e., are registered in the population register. For many this might coincide with the actual date they enter the country; but for some, not. Asylum seekers, for example, although living in Sweden for some time are not included before their status is accepted and they receive a residence permit.

¹⁵This definition is based on the variable `medbland`.

¹⁶The distribution features dates with a low number of immigrations, which can be simply explained by the fact that we show counts for two consecutive days and that there are few entries on weekends (when the administration does not work).

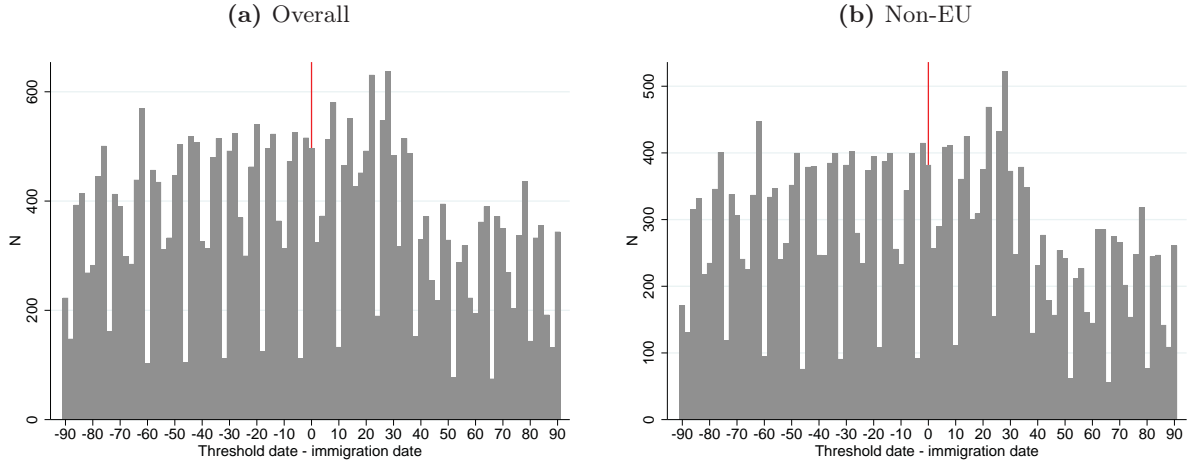


Figure 2: Histograms of immigration dates around the eligibility threshold in election years in bins of two days in the ± 90 days around the threshold

Notes: The y-axis shows the number of immigrations, and the x-axis the days centered at the threshold date, i.e., the election threshold immigration date minus the immigration date. It marks the distance in days from the eligibility threshold. Consequently, individuals with positive values are treated (eligible to vote) and individuals with negative values are not. The left hand side visualizes the histogram for the overall election-year sample. The right hand side for the election-year sample restricted to non-EU immigrants.

in Sweden the elections are always held in the middle of September, which also marks the beginning of the semester at universities. As Sweden has many international students, this constitutes a considerable number of immigrants arriving just before the start of the semester and who potentially are not comparable to the average migrant. Indeed, when plotting the distributions for students (i.e., individuals whose reason for immigration is study) and non-students, we see a fairly large wave of students arriving before the threshold date (just to the right of the threshold in Figure A1 in the Appendix).¹⁷ In order to not confound our estimates, we exclude students from our estimation sample. Leaving them in the sample would lead us to compare a group in which students are strongly over-represented with a group of 'typical' immigrants who immigrate during the year.

The exclusion of students from the estimation sample is also indicated by the balancing tests. When comparing individuals' characteristics (age, gender, and whether there are school age children in the household) in a 10-day window around the threshold, we indeed find that it is imbalanced in the *overall* sample. When comparing the average probability of being a student when entering the country, we observe a systematically higher probability in the treatment group above the threshold. Furthermore, the treatment group in the overall sample is, on average, younger, exhibits a lower female share, and has a lower probability of having children of school age

¹⁷The same does not apply to immigrants with children of school age, as the school year typically starts in mid August, which does not coincide with the election date.

in the household. These differences are consistent with the idea that students are over-represented in this group. However, in the sample which excludes students, individuals' characteristics are balanced. The corresponding statistical tests are reported in Table A2 in the Appendix. They furthermore reveal that the same differences between the overall and the non-student sample are observed in election as well as non-election years. This strongly suggests that the imbalance is indeed driven by the start of the semester in September, and not by any selection with respect to the election date

Country characterization

In order to distinguish between 'pull' and 'push' migrants, we separate the sample with respect to the development status of the country of origin in the year of immigration. The status of development is approximated using the human development index (HDI), combining information about life expectancy, basic education, and per capita income.¹⁸ Due to confidentiality regulations of the data provider, we have to group the countries of origin by their position in the distribution of HDI scores. We draw on the decile a country is located in a given immigration year using all the available HDI data between 1996 and 2009, i.e., for all relevant immigration years. The higher the assigned decile, the higher the country ranks in the distribution of the HDI in a given year. We define three groups, assuming that the degree to which migrants are 'pushed' instead of 'pulled' decreases with the HDI score:

- Low HDI: Defined as countries of origin up to the 3rd decile of the distribution with relatively poor living conditions.
- Medium HDI: Defined as countries of origin in the 4th decile of the distribution with medium high living conditions. This group happens to include a particularly large fraction of our sample.
- High HDI: Defined as countries of origin in and above the 5th decile of the distribution with very good living conditions.

This categorization might, of course, capture living conditions beyond the dimensions included in the HDI that are strongly correlated.¹⁹ It might, for example, also reflect countries' political stability, democratization and level of corruption. Overall, it seems an attractive way to approximate immigrants' background, their motivation for migration, and thus the valuation of different rights and obligations provided by Swedish citizenship.

¹⁸The country level data is freely available at <http://hdr.undp.org/en/data>.

¹⁹Table A1 in the Appendix presents the distribution of HDI scores in our main non-EU estimation sample. As becomes evident the 4th decile happens to include a large fraction of our sample. Therefore, the separation around the 4th decile seems to be the most reasonable way to form 3 groups.

4 Empirical strategy

Based on our setting offering a local randomization of the possibility to vote around the voting eligibility threshold and a bandwidth choice of ± 10 days around this threshold, we obtain a treatment and control group with individuals who have spent almost a similar amount of time in the country. Within this narrow window of immigration, the two groups should be comparable in all respects except their exposure to the possibility to vote. We want to test whether the probability of deciding to become naturalized develops systematically differently between the two groups in the time after elections. For this, we choose a difference-in-differences (DID) type of specification. It allows us to directly analyze the time pattern of the development of any effect. Specifically, we compare the difference in the probability of being naturalized between the treatment and control group in the first, second, third and fourth year after the election, to the difference between the two groups in the election year. We thus implicitly assume that the two groups will evolve in parallel, if not for the election experience in the treatment group. As the two groups are identical up to the election year with regard to their formal involvement in the democratic process, and as naturalization is in principle only possible after at least two to three years (and only for specific groups), the naturalization rates up to the election year are expected to be low (or zero) and not different between the two groups. This is why a comparison in the first years of stay makes no sense.

The main DID estimation is specified as follows:

$$y_{i,t} = \beta_0 cons + \beta_1 Treat + \sum_{j=1}^4 \delta_j I(t = j) \times Treat + \beta_2 X_i + \mu + \epsilon_{i,t} \quad (1)$$

In this, $Treat$ is the indicator set to one for the treatment group (eligible to vote). t indicates the time normalized at the election year (i.e., $t = 0$). $I(\dots)$ are indicator variables set to one if the respective condition holds (indicating the periods after the election). X is a vector of individual-level controls including gender and the distance to the threshold date in days. The latter variable takes account of the fact that the individuals in the treatment group have stayed for a slightly longer time in the country than the individuals in the control group, i.e., by approximately ten days (thus even without elections, the former group might, on average, show a slightly higher naturalization rate at a given point in time as the eligibility threshold for naturalization is reached slightly earlier). μ is a set of immigration year times duration of stay (in years) fixed effects flexibly controlling for different time patterns in naturalization behavior depending on the year of immigration.²⁰ ϵ is an idiosyncratic error term. As we observe the same individuals over time, we cluster the standard errors at the individual level to account for the potential serial

²⁰Note that these fixed effects absorb the level effects ($I(\dots)$) of the control group.

correlation. The coefficients of interest are the δ s. They indicate whether the probability of naturalization in the treatment group diverges systematically from that of the individuals in the control group in the one to four years after the election. We present the analysis based on DID type estimations in Sections 5 and 6.2.

As noted, in principle, the treatment group has an, on average, slightly longer duration of stay in the country. As a consequence, one could thus be concerned that the differentials captured by the specification in Equation 1 might partly be driven by this difference. We address this issue in two ways. First, we use a very narrow window around the voting eligibility threshold of only 10 days. The individuals in the two groups become eligible for naturalization almost at the same time (the maximal difference is 20 days). Second, we validate the robustness of our results with respect to this concern by including an additional control group in our design. We exploit individuals who cross the threshold of 3 years in non-election years as an additional control group. This additional control group would control for the fact that the group with a slightly longer duration of stay is always more likely to be naturalized at a given point in time. This approach is described in Section D of the Appendix in more detail, and the results are discussed in Section 6.2. It yields very similar results.

An alternative way to exploit the local randomization around the eligibility threshold, which however does not allow for visualizing the time pattern as nicely, is a classic regression discontinuity design (RDD) (see, e.g., Lee and Lemieux, 2010). We present the results of an RDD analysis of the probability of being naturalized after 7 years of stay in Section 6.4.

5 Main results

We present our main results in three steps. In a first step, we validate that the threshold rule for the assignment of the eligibility to vote is indeed applied and that our defined threshold date is able to separate the treatment from the control group. In a second step, we show graphical evidence on the evolution of the naturalization rates in the two groups over time. In a third step, we provide DID estimates.

5.1 Treatment take up

The assignment of voting eligibility after three years of stay is an administrative rule and therefore likely to hold and to be applied. We still validate that we can precisely separate the treatment and control group based on the available information on the country of origin, the date of immigration, and the defined threshold date.

Table 1 presents the average probability of being eligible to vote (or to have received a polling

Table 1: Eligibility to vote in the treatment and control group

Probability of being eligible to vote	Control group	Treatment group	Δ	t-value	p(value)	N_c	N_t
EU citizen	1.00	1.00	0.00	-	-	465	448
Non-EU citizen	0.00	0.99	-0.99	-342.60	0.00	1,326	1,170

Notes: Average probability of being eligible to participate in the elections in the election-year sample and in the year of the election ($t = 0$). The eligibility is calculated separately for the treatment and control group, and the EU and non-Eu sample in the 10 days around the eligibility threshold. N_c stands for the number of observations in the control group and N_t for the number of observations in the treatment group, respectively.

card before the election) for our main sample of individuals immigrating in the 10 days before and after the threshold date three years before the next elections, and separately for EU and non-EU citizens. It clearly reveals that eligibility does not change at the threshold for the quasi-placebo sample of EU citizens (who are eligible to vote right after the beginning of their stay). For non-EU citizens, however, there is a sharp difference in the probability of being eligible to vote between the two groups. For individuals in the control group it is zero and for those in the treatment group it jumps to almost 100 percent. This validates that the assignment rule is strictly applied and that our data, and the defined threshold date, is able to identify and to cleanly separate the treatment group from the control group.

5.2 Graphical evidence

Figure 3 presents raw averages of the probability of being naturalized over time and separately for the individuals narrowly eligible to vote (treatment group) and individuals narrowly ineligible to vote (control group) across the three defined country groups. In all three groups, the probability of holding Swedish citizenship is almost zero for the first three years of stay. This is easily explained by the requirements of the Swedish naturalization law described above. However, thereafter the patterns evolve rather differently, depending on the group the origin country belongs to.

Figure 3 a) depicts the evolution of the likelihood of holding Swedish citizenship for the treatment and control groups among immigrants from low HDI countries. While the two groups evolve more or less in parallel in the first years, the treatment group shows a clearly stronger increase in the probability of naturalizing in the second year after the election, compared to the control group. This is equivalent to the fifth year after arrival in Sweden and thus one year after immigrants are eligible to initiate the naturalization process (in the case that they immigrated as refugees). Figure 3 b) shows the corresponding evolution of the likelihood of holding Swedish citizenship

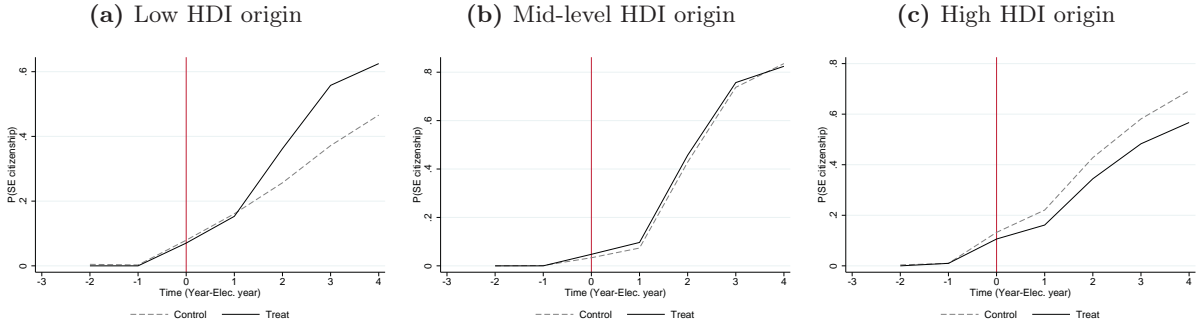


Figure 3: Evolution of the possibility of voting on the probability of being naturalized across groups of countries of origin in Sweden

Notes: Raw averages of the dependent variable (Swedish citizenship) in the non-EU election-year sample, separately for the treatment group with the possibility to vote (dark gray line) in $t = 0$ and the control group not allowed to vote (light gray line) in $t = 0$. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold.

among individuals originating in countries with a mid-level HDI. We do not observe any particular deviation in the probability of holding Swedish citizenship between the treatment and the control group. Figure 3 c) presents the corresponding numbers for individuals originating in high HDI countries. In this group, we observe that individuals in the treatment group show a lower probability of naturalizing than those in the control group 1 to 2 years following the election. Overall, this descriptive evidence already shows a rather clear and differentiated pattern. It suggests that experiencing the possibility to vote in local and regional elections in Sweden has a positive effect on the probability of naturalizing for immigrants from low HDI countries, no particular net-effect for those from mid-level HDI countries, and a negative effect for those from high HDI countries. These different reactions would most likely remain hidden if the differences in immigrants' background were not considered.

5.3 Estimates

Table 2 presents the estimates based on the DID model in Equation 1 for the non-EU immigrant election-year sample. The results are very much in line with the graphical evidence. We observe a systematic reduction in the probability of being naturalized for the individuals with an origin in low HDI countries. The difference between the treatment and control group starts increasing in the first year after the election. In the fourth year, i.e., after about 7 years of stay in Sweden, individuals in the treatment group are about 17 percentage points more likely to be naturalized compared with those in the control group. This amounts to a 36-percent difference when considered in relation to a 47-percent fraction of Swedish citizens in the control group in the fourth year after the election (indicated by $Mean_{Control}(t + 4)$ in the table). In the sample of immigrants from mid-level HDI countries, we do not measure a systematic divergence in the probability of

Table 2: Effect of the possibility to vote in local and regional elections on the probability of holding Swedish citizenship

	(1)	(2)	(3)
	Low HDI	Mid-level HDI	High HDI
<i>Treat</i>	-0.043 (0.052)	-0.007 (0.027)	-0.040 (0.059)
Treat x $I(t + 1)$	0.012 (0.022)	0.008 (0.012)	-0.032 (0.021)
Treat x $I(t + 2)$	0.115*** (0.035)	0.005 (0.027)	-0.045 (0.038)
Treat x $I(t + 3)$	0.200*** (0.040)	-0.006 (0.025)	-0.062 (0.043)
Treat x $I(t + 4)$	0.173*** (0.042)	-0.028 (0.023)	-0.099** (0.043)
Immigration year x duration of stay FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
$Mean_{Control}(t + 4)$	0.47	0.84	0.69
N	2,976	6,333	2,726
N clust	623	1,300	583
R2	0.164	0.448	0.189

Notes: Estimation results of a linear probability model in the non-EU election-year sample. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold and is based on the estimation model outlined in Equation 1. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t + 4)$ reports the average naturalization probability of the respective control group in the fourth year after the election. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

holding Swedish citizenship between the treatment and control groups. Finally, there is some evidence of a systematic reduction in the probability of being naturalized for individuals who experienced the possibility to vote in the sample of high HDI countries in the fourth year after the election (or after about 7 years in the country). People in the treatment group are about 10 percentage points less likely to be naturalized when compared to those in the control group. Putting this in perspective with a naturalization rate of 69 percent in the control group, the difference amounts to about 14 percent. When splitting the estimation samples by individuals' gender, we find that the effect in the low HDI group is present for both men and women. The effect in the high HDI sample seems, however, to be primarily driven by men. The corresponding estimation results can be found in Table A3 in the Appendix.

These results lead us to conclude that immigrants indeed care about the right to vote. The net effect on naturalization behavior, however, is likely to depend on the composition of the migrant population. In our data we observe that the motivation effect seems to be stronger for immigrants originating in countries with poor living conditions, i.e., individuals who were able to vote are more likely to naturalize to quite a large extent. For immigrants originating in countries with very high living standards, however, we observe that the incentive mechanism seems to outweigh. In this group, individuals who were eligible to vote tend to be less likely to naturalize. The fact that we see no response in the group of individuals originating in countries with medium living conditions, does not necessarily mean that they do not care about the right to vote. It could well be that the two opposing effects just net each other out.

Both, the negative and the positive responses in the probability of naturalizing imply that individuals care about formal political rights. Both groups are subject to the same election result and any resulting policy change. They only differ in their exposure to the possibility to vote. Their response, thus indicates a recognition and valuation of the right to vote.

The following section provides corroborating evidence that our findings are indeed driven by the exposure to the opportunity to vote and that we arrive at very similar conclusions, when applying other specifications or using other bandwidths around the eligibility threshold.

6 Validation

In this section, we perform several checks to validate that our results are robust and indeed causally driven by exposure to the possibility to vote. First, we analyze whether a similar response is observed in the group of EU citizens, who are eligible to vote right from the beginning of their stay. Second, we present estimates including placebo election years offering us an additional control strategy to validate that our results are not driven by a general difference between the treatment and control group. Third, we show results for additional DID specifications with alternative bandwidths around the eligibility threshold. Fourth, we present conventional RDD estimates at the eligibility threshold.

6.1 EU citizens as a quasi-placebo group

The Swedish institutional setting allows us to test for responses in a quasi-placebo group. EU citizens are not subject to the three-year eligibility rule, but allowed to vote in local and regional elections right from the beginning of their stay in Sweden. Consequently, when applying our empirical design, both groups immigrating around the eligibility threshold date get the opportunity to vote in the respective elections. However, for individuals in both the EU and the non-EU

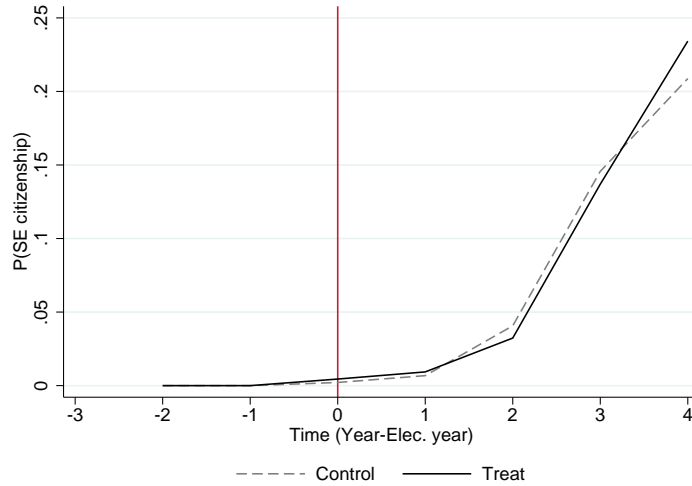


Figure 4: Evolution of the probability of being naturalized in the quasi-placebo group of EU citizens
Notes: Raw averages of the dependent variable (Swedish citizenship) in the EU sample (quasi-placebo group) for election years, separately for the quasi-treatment group and the quasi-control group. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold.

samples it is the first election event since they immigrated to Sweden. If the different naturalization behavior between the treatment and control group that we observe in the non-EU sample was not driven by the differential exposure to the right to vote, but by something else that happens in election years to individuals with a slightly longer duration of stay, we should observe a similar response in the EU sample. If, however, what we observe was driven by exposure to the possibility to vote, we would not expect to see any response in the EU sample, as both groups are equally treated.

The descriptive evidence is presented in Figure 4. The probabilities of being naturalized in the two groups, treatment and control in the EU sample, evolve roughly in parallel and we do not observe any particular divergence after the election, i.e., in the third year of stay. This finding is confirmed by the estimates presented in Table A4 in Appendix C. The estimated differentials are not only not statistically significant but also close to zero for the full sample as well as for males and females separately.

6.2 Non-election years as additional control group

In our setting, we do not have a clear pre-treatment period, as the naturalization rate is very low before three years of stay. One resulting concern might be that, what we capture is driven by the fact that the two groups differ slightly when it comes to their duration of stay. The individuals in the treatment group always have a slightly longer duration of stay than those in

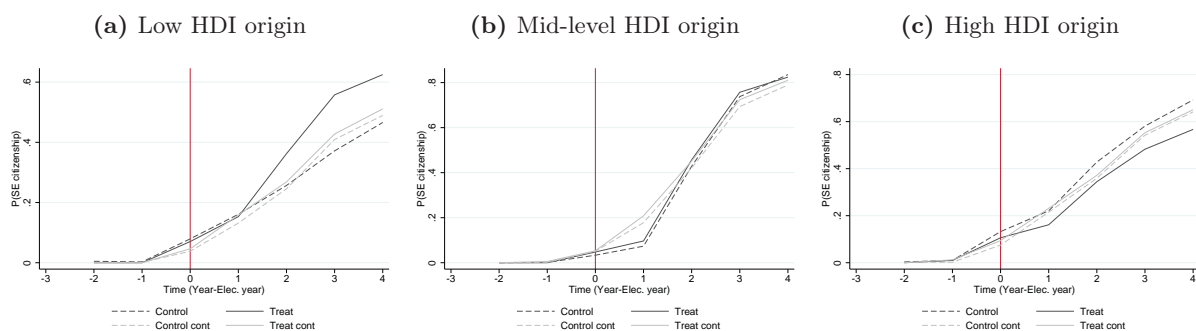


Figure 5: Evolution of the likelihood of being naturalized in the election-year and the non-election-year (control) sample

Notes: Raw averages of the dependent variable (holding Swedish citizenship) in the election-year sample (the dark gray lines) and the non-election-year sample (the light gray lines) for non-EU citizens. Averages are displayed separately for the treatment group (solid lines, *Treat* in the election-year sample and *Treat cont* in the non-election-year sample) and the control group (dashed lines, *Control* in the election-year sample and *Control cont* in the non-election-year sample). The samples are restricted to individuals immigrating within 10 days around the eligibility threshold.

the control groups, which amounts to a maximum of twenty days. The differential development in naturalization behavior we document thus might in part be driven by the fact that the treatment group is always eligible to naturalize slightly earlier. If, therefore, in the sample of immigrants from low HDI countries people in the treatment group are more likely to naturalize, part of the effect we capture might be driven by this earlier access to the naturalization process.²¹

We already try to approach this concern by using quite a narrow bandwidth around the eligibility threshold of only ± 10 days and by including the duration of stay, i.e., the assignment variable as a linear control.

Another way to approach this concern is to study whether the deviation between the treatment and the control groups differs in the election-year sample and in a sample of individuals immigrating around thresholds calculated on the basis of placebo election dates in non-election years. The empirical approach is described in more detail in Appendix D. In a nutshell, we now analyze whether the control and treatment groups deviate more from each other in the election-year sample than similar groups in the non-election-year sample. If the group immigrating before the threshold date was more likely to naturalize in general, this strategy would control for the baseline differential. Importantly, this strategy also accounts for periodical patterns of immigration (like any unbalanced unobserved characteristics), which might affect immigrants' naturalization behavior differentially.

Figure 5 presents graphical evidence of how the probability of holding Swedish citizenship evolves in the treatment and control groups in the election and non-election-year samples. Descriptively,

²¹The described bias would lead to an underestimation of the effect in the high HDI sample, respectively.

the treatment and control groups seem to evolve very much in parallel in the non-election-year sample over the entire period of analysis. Similarly, across samples, the probability of being naturalized moves in close parallel up to the third or fourth year in the country (Year - Election Year = 0 or 1). However, in the election-year sample, the probabilities of being naturalized after elections in the treatment and control groups start to deviate from each other (and from those in the non-election-year sample) among immigrants from low and high HDI countries. The estimation results confirm the earlier finding of a positive impact in the low HDI sample and a negative one in the high HDI sample (see Table A6 in Appendix D).²²

The two additional tests using EU citizens as quasi-placebo groups and non-election samples as additional control groups indicate that our findings are indeed driven by differential exposure to the possibility to vote in the sample of non-EU citizens.

6.3 Different bandwidths for the estimation sample

In a further robustness check, we repeat our estimates for samples covering different time windows around the eligibility threshold. The wider the bandwidth, the larger the difference in the duration of stay at any given point in time, and the less comparable the two groups probably are. We still want to study how sensitive our estimates are and re-estimate the specification defined in Equation 2 for a bandwidth of 30 and five days around the eligibility threshold. The results are presented in Table A7 in the Appendix. The basic conclusion is sustained. We see positive effects of the possibility to vote on the probability of being naturalized for immigrants from low HDI and negative effects for those from high HDI countries for both bandwidths, whereby the size of the estimated effects is larger for the smaller bandwidths, when the groups are most likely to be comparable.

6.4 Conventional RDD estimates

An alternative empirical approach to studying the causal effect of the possibility to vote on naturalization behavior in the Swedish institutional setting is the RDD. The basic idea is to exploit the local randomization of the treatment assignment by comparing the outcomes of treated and untreated individuals just at the threshold value, and thus, for individuals who virtually arrived at the same time, as they are assumed most comparable. The conventional RDD analysis estimates the discontinuity in the outcome of interest at the threshold value and at a given point in time after the treatment (see, e.g., Porter (2003), Imbens and Lemieux (2008) and Lee and Lemieux (2010) for a detailed introduction to RDD).

²²Figure A2 and Table A6 in the Appendix present the corresponding estimation results for the quasi-placebo group of EU citizens. As before, we do not find any evidence for a divergence of the treatment and control group in this sample.

While in our main analysis we choose to estimate a simple linear model, which allows presenting the time structure of the effect, in this section, we concentrate on conventional RDD estimates for the probability of being naturalized by the fourth year after the election ($t = 4$). Table 3 presents the discontinuity estimates for the election-year sample. The estimates and bandwidth choices follow (Calonico et al., 2016). As expected, we do not observe any discontinuity for our quasi-placebo sample of EU citizens in column (1). Both groups around the threshold are eligible to vote and the probability of being naturalized should therefore not differ if any effect was due to the experience of the possibility to vote. For the non-EU citizens from low HDI countries in column (2), as before, we observe a positive treatment effect. The probability of being naturalized features a positive discontinuity at the threshold, i.e., individuals in the treatment group are systematically more likely to naturalize. The effect is comparable across the three estimates of the discontinuity based on slightly different estimates and standard errors. It is sizable and statistically significant. The probability of being naturalized after seven years in the country, i.e., four years after the election, is 17 to 18 percentage points higher. Within the group of immigrants from high HDI countries in column (4), as before, we observe a clear negative treatment effect. The probability of being naturalized after seven years is about 16 to 18 percentage points lower for those who experienced and learned about the possibility to vote. We do not measure any discontinuity among immigrants from countries with a mid-level HDI origin in column (3). These results are very much in line with our prior findings.

Table A8 in Appendix E presents corresponding discontinuity estimates for the non-election-year sample around the placebo threshold dates. We do not find any systematic discontinuity around the thresholds derived from the pseudo election dates in either sample. The estimated discontinuities are not only not statistically significant at conventional levels but also close to zero. Overall, these results corroborate our main findings presented in the DID analysis.

7 Conclusion

Access to the formal democratic participation process – or to elections, for short – is an important right that comes with formal citizenship. Its value is likely reflected in non-citizens’ investments to become naturalized and to acquire formal citizenship. However, as citizenship comes with other rights and benefits, such as protection, permanent permission to stay in the country, full access to the social security system, or a new passport that might impact visa restrictions, evidence from revealed preferences mostly shows its valuation as a mixed bag. In our analysis, we therefore study the reverse, i.e., the potential loss or gain of the attractiveness of naturalization if voting rights are obtained automatically by law without formal citizenship, a setting that is offered in the context of NCVR. The decoupling of voting rights and formal citizenship relates to a topical

Table 3: Effect of the possibility to vote on the probability of being naturalized based on RDD estimates

Sample	(1)	(2)	(3)	(4)
	EU quasi-placebo	Non-EU Low HDI	Non-EU Mid-level HDI	Non-EU High HDI
Conventional	0.028 (0.044)	0.167*** (0.058)	-0.015 (0.032)	-0.163*** (0.057)
Bias-corrected	0.034 (0.044)	0.183*** (0.058)	-0.026 (0.032)	-0.182*** (0.057)
Robust	0.034 (0.053)	0.183*** (0.067)	-0.026 (0.037)	-0.182*** (0.067)
Observations l	863	682	1,741	700
Observations r	967	465	1,234	666
Bandwidth l	24	25	25	28
Bandwidth r	24	25	25	28

Notes: The probability of being naturalized is measured after 7 years of stay in the country for individuals immigrating around the eligibility threshold covered in the election-year sample. RDD estimates are based on local linear estimates using a triangular kernel with the automatic rdrobust bandwidth choice (Calonico et al., 2016). Standard errors are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

debate about the reform of democratic constitutions in countries with a large population of non-citizen residents. Some skeptics argue that citizenship would be devalued with NCVR, a position implicitly reflecting an outcome-oriented valuation of the right to vote. In contrast, proponents of NCVR emphasize presumed positive effects on immigrants' motivation to integrate and to finally become full members of the host country by accepting citizenship. The latter position builds on the procedural value of participation rights.

Our evidence can inform both debates, the general one about the valuation of democratic participation rights as well as the specific one about the immediate consequences of NCVR for naturalization. We refer to the institutional context of Sweden, where immigrants receive the right to vote in local and regional elections after three years of stay in the country. Specifically, we exploit the quasi-random assignment of the possibility to vote around the eligibility threshold to compare immigrants who have just stayed in Sweden long enough to get a polling card with those immigrants who arrived slightly later and were thus not eligible to participate.

The empirical analysis reveals a provocative heterogeneity in the causal effect of the possibility to vote on the propensity to naturalize. While immigrants from countries with a low level of development (low HDI) are more likely to naturalize after having experienced the possibility to vote, immigrants from highly developed countries (high HDI) treated in same way are less likely to naturalize. For the former individuals, the procedural value affecting the intrinsic motivation to integrate and naturalize thus outweighs the negative disincentive effect of formal citizenship

coming with fewer additional rights. For the latter individuals, the reverse seems to hold. Experiencing and learning that access to voting rights is offered even without formally becoming a citizen provides a disincentive stronger than any motivational force from the procedural value. While, due to the opposing forces, both behavioral reactions likely reflect lower bound estimates for the magnitude, the respective consequences on the probability of being naturalized four years after elections (or after seven years in the country) are sizeable and amount to an increase of about 36 percent for immigrants from low HDI countries and to a reduction of 14 percent for those from high HDI countries. At the very least, this shows that democratic participation rights are highly valued although the contribution of voting rights to the overall value of citizenship strongly depends on individuals' background.

The evidence presented does not readily lend itself to general policy conclusions in the context of immigrant integration. First, it highlights that the consequences of institutional integration measures like NCVR strongly depend on people's economic and cultural origin. Second, it demands for a more comprehensive understanding of the motivational consequences in the positive domain of stronger civic virtue as well as in the negative domain of a weaker incentive to naturalize. Given the importance of non-citizen suffrage at the frontiers of democratic reform, additional evidence will ideally shed light on additional aspects, such as motivational transfer to political engagement and community service, but also to norm compliance regarding, e.g., honest payment of taxes and forming an attachment to the host country.

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From participants to citizens?
Democratic voting rights and naturalization behavior

Michaela Slotwinski* Alois Stutzer† Pieter Bevelander‡

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Appendix

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* Michaela Slotwinski, University of Basel, Faculty of Business and Economics, Peter Merian-Weg 6, 4002 Basel, Switzerland. ZEW - Centre for European Economic Research, L7,1, 68161 Mannheim, Germany. Phone: +41 (0)61 207 33 75, email: michaela.slotwinski@unibas.ch. (Corresponding author)

† Alois Stutzer, University of Basel, Faculty of Business and Economics, Peter Merian-Weg 6, 4002 Basel, Switzerland. Phone: +41 (0)61 207 33 61, email: alois.stutzer@unibas.ch.

‡ Pieter Bevelander, MIM, Malmö University, 205 06 Malmö, Sweden. Email: pieter.bevelander@mau.se.

A Immigration of students to Sweden around the eligibility threshold

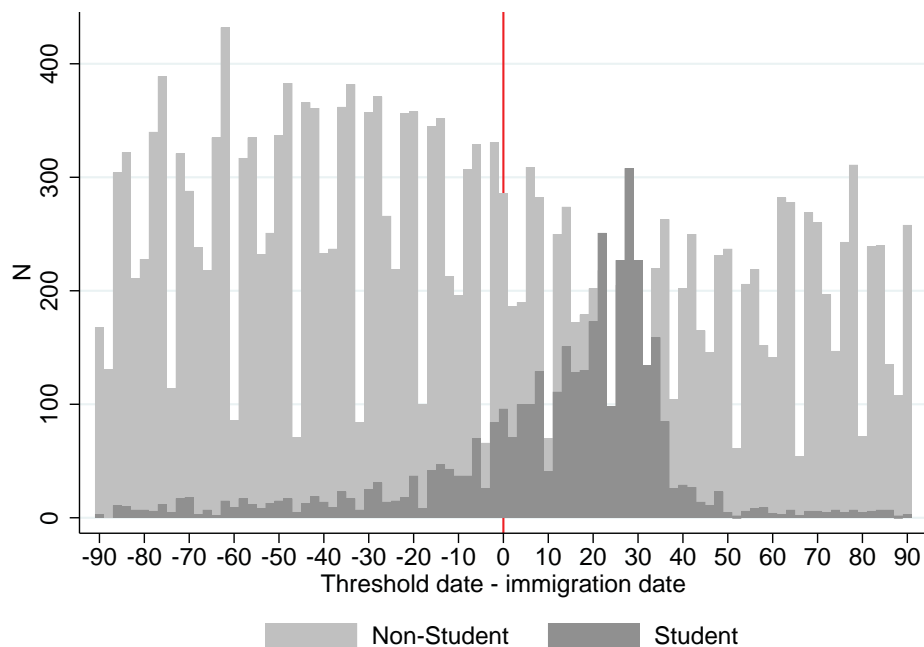


Figure A1: Histogram of the immigration of non-EU citizens around the eligibility threshold for election years

Notes: The data is presented in bins of two days in the ± 90 days around the threshold. The bars indicate the distribution of the absolute number for non-students in light gray and students in dark gray. On the x-axis, the days are centered at the threshold date, i.e., the election threshold immigration date minus the immigration date. It marks the distance in days from the eligibility threshold. Consequently, individuals with positive values are eligible to vote and treated and individuals with negative values are not.

B Descriptives and balancing tests

Table A1: Distribution of HDI scores in the non-EU election-year sample

Score	Number of observations	Percent of observations
1	294	11.78
2	152	6.09
3	177	7.09
4	1,294	51.84
5	247	9.90
6	119	4.77
7	81	3.25
8	7	0.28
9	46	1.84
10	79	3.17
Total	2,496	100.00

Notes: Distribution of HDI scores in the non-EU election-year sample excluding students in the election year and in the 10 days around the eligibility threshold (± 10 days).

Table A2: Comparison of characteristics of individuals immigrating around the eligibility threshold

Sample - Variable	Control mean	Treat mean	Δ	t-value	p(value)	N_c	N_t
Overall sample including students							
P(student)	0.16	0.25	-0.09	-13.07	0.00	7,061	8,239
Age	34.36	33.96	0.41	2.54	0.01	7,061	8,239
P(female)	0.47	0.44	0.03	4.21	0.00	7,061	8,239
P(children)	0.43	0.39	0.04	5.29	0.00	7,061	8,239
Overall sample excluding students							
Age	35.36	35.60	-0.24	-1.28	0.20	5,903	6,180
P(female)	0.51	0.50	0.01	0.88	0.38	5,903	6,180
P(children)	0.50	0.50	0.00	0.40	0.69	5,903	6,180
Election-year sample including students							
P(student)	0.16	0.25	-0.09	-7.51	0.00	2,127	2,157
Age	34.34	33.67	0.68	2.26	0.02	2,127	2,157
P(female)	0.44	0.41	0.03	1.88	0.06	2,127	2,157
P(children)	0.44	0.39	0.05	3.52	0.00	2,127	2,157
Election-year sample excluding students							
Age	35.37	35.24	0.13	0.36	0.72	1,791	1,618
P(female)	0.48	0.47	0.01	0.86	0.39	1,791	1,618
P(children)	0.52	0.50	0.02	0.97	0.33	1,791	1,618
Non-election-year sample including students							
P(student)	0.17	0.25	-0.08	-10.68	0.00	4,934	6,082
Age	34.37	34.06	0.31	1.64	0.10	4,934	6,082
P(female)	0.48	0.45	0.04	3.99	0.00	4,934	6,082
P(children)	0.43	0.39	0.04	3.98	0.00	4,934	6,082
Non-election-year sample excluding students							
Age	35.35	35.73	-0.38	-1.67	0.10	4,112	4,562
P(female)	0.52	0.52	0.01	0.76	0.45	4,112	4,562
P(children)	0.50	0.50	-0.00	-0.18	0.86	4,112	4,562
Election-year sample excluding students, non-EU							
Age	35.13	34.92	0.21	0.50	0.62	1,326	1,170
P(female)	0.48	0.46	0.01	0.74	0.46	1,326	1,170
P(children)	0.53	0.53	0.01	0.45	0.65	1,326	1,170

Notes: Descriptive statistics of predetermined individual characteristics, separately for the treatment and control group in the 10 days around the threshold (± 10 days). N_c and N_t describe the number of observations in the control group (c , to the left of the threshold) and the treatment group (t , to the right of the threshold), within a 10 day window. $P(students)$ captures whether an individual first entered the country as a student, Age measures individuals age in the year of the election, $P(female)$ indicates an individual's gender, and $P(children)$ is an indicator capturing whether there is at least one child of school age in the household.

C DID estimation results by gender

Table A3: Effect of the possibility to vote on the probability of being naturalized: Female and male non-EU citizens

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low	Mid-level	High	Low	Mid-level	High	Low	Mid-level	High
	HDI	HDI	HDI	HDI	HDI	HDI	HDI	HDI	HDI
				Women	Women	Women	Men	Men	Men
<i>Treat</i>	-0.043	-0.007	-0.040	-0.078	-0.001	0.003	-0.007	-0.014	-0.073
	(0.052)	(0.027)	(0.059)	(0.071)	(0.039)	(0.083)	(0.077)	(0.038)	(0.083)
<i>Treat</i> x <i>I</i> (<i>t</i> + 1)	0.012	0.008	-0.032	0.004	-0.007	-0.070**	0.021	0.016	0.006
	(0.022)	(0.012)	(0.021)	(0.031)	(0.020)	(0.031)	(0.032)	(0.014)	(0.029)
<i>Treat</i> x <i>I</i> (<i>t</i> + 2)	0.115***	0.005	-0.045	0.112**	0.007	-0.010	0.117**	0.010	-0.083
	(0.035)	(0.027)	(0.038)	(0.051)	(0.040)	(0.055)	(0.049)	(0.037)	(0.055)
<i>Treat</i> x <i>I</i> (<i>t</i> + 3)	0.200***	-0.006	-0.062	0.254***	-0.053	-0.020	0.150***	0.030	-0.106*
	(0.040)	(0.025)	(0.043)	(0.057)	(0.037)	(0.061)	(0.057)	(0.035)	(0.061)
<i>Treat</i> x <i>I</i> (<i>t</i> + 4)	0.173***	-0.028	-0.099**	0.212***	-0.073**	-0.041	0.137**	0.009	-0.154**
	(0.042)	(0.023)	(0.043)	(0.059)	(0.032)	(0.062)	(0.059)	(0.033)	(0.062)
Immigration year x duration of stay FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Mean</i> _{Control} (<i>t</i> + 4)	0.47	0.84	0.69	0.48	0.89	0.69	0.45	0.79	0.70
N	2,976	6,333	2,726	1,494	2,852	1,353	1,482	3,481	1,373
N clust	623	1,300	583	311	584	289	312	716	294
R2	0.164	0.448	0.189	0.1878	0.477	0.214	0.147	0.431	0.177

Notes: Estimation results of a linear probability model in the election-year sample of non-EU citizens. We present overall results, and separate results for women and men. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold and is based on the estimation model outlined in Equation 1. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t+4)$ reports the average naturalization probability of the respective control group in the fourth year after the election. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effects on the probability of being naturalized in the quasi-placebo group of EU citizens

	(1)	(2)	(3)
	All	Women	Men
<i>Treat</i>	0.004 (0.021)	-0.024 (0.031)	0.033 (0.027)
Treat x $I(t + 1)$	0.001 (0.004)	0.003 (0.009)	-0.000 (0.000)
Treat x $I(t + 2)$	-0.011 (0.012)	-0.003 (0.022)	-0.015 (0.013)
Treat x $I(t + 3)$	-0.009 (0.024)	-0.004 (0.036)	-0.014 (0.033)
Treat x $I(t + 5)$	0.024 (0.030)	0.034 (0.042)	0.011 (0.042)
Immigration year x duration of stay FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
$Mean_{Control}(t + 4)$	0.21	0.20	0.22
N	4,164	2,038	2,126
N clust	914	440	474
R2	0.117	0.095	0.147

Notes: Estimation results of a linear probability model in the election-year sample of EU citizens. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold for non-EU citizens and is based on the estimation model outlined in Equation 1. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t + 4)$ reports the average naturalization probability of the respective control group in the fourth year after the election. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Validation based on the combined sample for election and non-election years

Equation 2 shows the extended DID specification for the combined sample for election and non-election years.

$$y_{i,t} = \beta_0 cons + \beta_1 Treat + \beta_2 Election_Year + \sum_{j=1}^4 v_j I(t = j) \times Treat + \sum_{j=1}^4 \delta_j I(t = j) \times Treat \times Election_Year + \beta_3 X_i + \mu + \epsilon_{i,t} \quad (2)$$

Compared to Equation 1, it includes two additional groups. One that would have been part of the control group and one that would have been part of the treatment group, if there had been an election in a non-election year. However, as there was no election, the outcome for these groups is expected to evolve in parallel if our assumption of parallel trends is valid. To test the validity of our assumption and that our results are not driven by the slightly longer duration of stay in the treatment group, the DID specification is extended with an interaction for election years. The additional indicator *Election_Year* is set to one for individuals immigrating around the threshold date of election years. Consequently, the estimates of the v 's indicate whether the outcomes for the treatment and control group deviate from each other in the non-election-year sample. The coefficients of primary interest, the δ 's, capture whether the outcomes across groups diverge more in the election-year sample than in the non-election-year sample. This specification would thus control for any general divergence between the control and treatment group that is also present in non-election years.²³

²³Note that again the level effects are absorbed by the immigration year times duration of stay (in years) fixed effects allowing for a differential development of the naturalization rate over time depending on the year of immigration.

Table A5: Effect of the possibility to vote on the probability of being naturalized: Additional non-election year control sample of non-EU citizens

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low	Mid-level	High	Low	Mid-level	High	Low	Mid-level	High
	HDI	HDI	HDI	HDI	HDI	HDI	HDI	HDI	HDI
				Women	Women	Women	Men	Men	Men
<i>Treat</i>	-0.059** (0.025)	-0.000 (0.016)	-0.014 (0.027)	-0.064* (0.034)	-0.011 (0.023)	-0.012 (0.038)	-0.060* (0.036)	0.008 (0.024)	-0.011 (0.038)
<i>Treat</i> x <i>I</i> (<i>t</i> + 1)	0.023 (0.016)	0.023 (0.014)	0.007 (0.017)	0.018 (0.022)	0.014 (0.019)	-0.002 (0.025)	0.035 (0.022)	0.035* (0.020)	0.016 (0.024)
<i>Treat</i> x <i>I</i> (<i>t</i> + 2)	0.012 (0.021)	0.028 (0.018)	0.011 (0.021)	0.010 (0.029)	0.035 (0.025)	0.014 (0.031)	0.017 (0.030)	0.022 (0.027)	0.004 (0.030)
<i>Treat</i> x <i>I</i> (<i>t</i> + 3)	0.017 (0.024)	0.012 (0.018)	0.015 (0.024)	0.031 (0.033)	0.008 (0.024)	0.035 (0.034)	0.004 (0.037)	0.018 (0.026)	-0.009 (0.034)
<i>Treat</i> x <i>I</i> (<i>t</i> + 4)	0.020 (0.025)	0.010 (0.016)	0.016 (0.024)	0.046 (0.034)	0.000 (0.022)	0.040 (0.033)	-0.005 (0.039)	0.024 (0.024)	-0.016 (0.035)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 1)	-0.030 (0.034)	-0.007 (0.022)	-0.058 (0.038)	-0.046 (0.047)	0.007 (0.033)	-0.088 (0.054)	-0.024 (0.050)	-0.027 (0.030)	-0.025 (0.055)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 2)	0.083* (0.044)	-0.015 (0.034)	-0.075 (0.047)	0.070 (0.062)	-0.000 (0.048)	-0.044 (0.067)	0.090 (0.063)	-0.020 (0.047)	-0.102 (0.068)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 3)	0.164*** (0.048)	-0.010 (0.030)	-0.097* (0.049)	0.191*** (0.067)	-0.034 (0.043)	-0.075 (0.070)	0.137** (0.069)	0.005 (0.042)	-0.113 (0.071)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 4)	0.133*** (0.049)	-0.030 (0.027)	-0.134*** (0.048)	0.134** (0.068)	-0.046 (0.036)	-0.101 (0.067)	0.132* (0.071)	-0.023 (0.039)	-0.153** (0.069)
Immigration year x duration of stay FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Mean_{Control}(t + 4)$	0.47	0.84	0.69	0.48	0.89	0.69	0.45	0.79	0.70
N	11,335	19,530	11,922	6,080	9,963	6,074	5,255	9,567	5,848
N clust	2,373	4,006	2,580	1,260	2,031	1,311	1,113	1,975	1,269
R2	0.165	0.397	0.205	0.180	0.411	0.230	0.155	0.392	0.190

Notes: Estimation results of a linear probability model in the sample of non-EU citizens exploiting the non-election-year sample as a control sample. We present overall results, and separate results for women and men. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold and is based on the estimation model outlined in Equation 2. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t + 4)$ presents the average naturalization probability of the respective control group in the fourth year after the election in the election-year sample. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

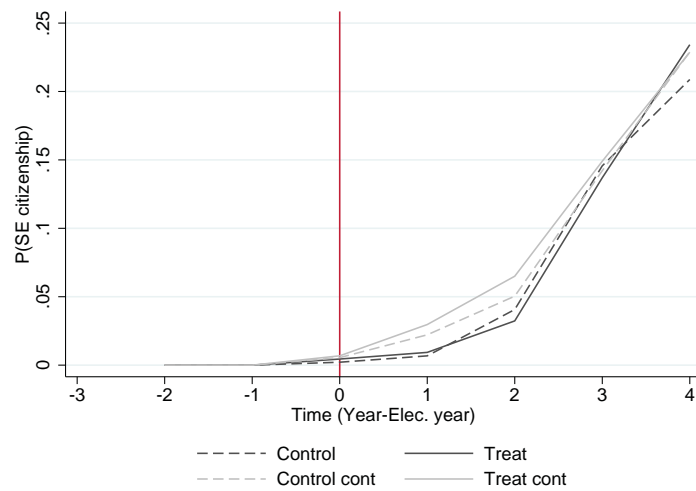


Figure A2: Evolution of the probability of being naturalized in the quasi-placebo group of EU citizens in the election and the non-election-year sample

Notes: Raw averages of the dependent variable (Swedish citizenship) in the election-year sample (the dark gray lines) and the non-election-year sample (the light gray lines) for EU citizens. Averages are displayed separately for the treatment group (solid lines, *Treat* in the election-year sample and *Treat cont* in the non-election-year sample), the control group (dashed lines, *Control* in the election-year sample and *Control cont* in the non-election-year sample). The sample is restricted to individuals immigrating within 10 days around the eligibility threshold. The darker lines indicate the average probabilities of being naturalized for the two groups of the election-year sample, and the lighter lines the corresponding probabilities for the non-election-year sample.

Table A6: Effects on the probability of being naturalized in the quasi-placebo group of EU citizens combining election and non-election-year samples

	(1)	(2)	(3)
	All	Women	Men
<i>Treat</i>	-0.004 (0.012)	-0.008 (0.019)	-0.001 (0.014)
<i>Treat</i> x <i>I</i> (<i>t</i> + 1)	0.005 (0.006)	0.013 (0.010)	-0.001 (0.007)
<i>Treat</i> x <i>I</i> (<i>t</i> + 2)	0.013 (0.010)	0.018 (0.015)	0.009 (0.012)
<i>Treat</i> x <i>I</i> (<i>t</i> + 3)	0.004 (0.016)	0.010 (0.023)	-0.002 (0.021)
<i>Treat</i> x <i>I</i> (<i>t</i> + 4)	-0.007 (0.019)	-0.004 (0.028)	-0.010 (0.026)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 1)	-0.004 (0.009)	-0.007 (0.017)	-0.003 (0.008)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 2)	-0.024 (0.017)	-0.018 (0.028)	-0.028 (0.018)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 3)	-0.013 (0.029)	-0.011 (0.044)	-0.016 (0.040)
<i>Treat</i> x <i>Elec.Y.</i> x <i>I</i> (<i>t</i> + 4)	0.031 (0.035)	0.042 (0.051)	0.018 (0.049)
Immigration year x duration of stay FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
$Mean_{Control}(t + 4)$	0.21	0.20	0.22
N	14,574	7,233	7,341
N clust	3,175	1,556	1,619
R2	0.110	0.103	0.120

Notes: Estimation results of a linear probability model in the sample of EU citizens, exploiting the non-election-year sample as an additional control sample. We present overall results, and separate results for women and men. The sample is restricted to individuals immigrating within 10 days around the eligibility threshold for non-EU citizens and is based on the estimation model outlined in Equation 2. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t+4)$ presents the average naturalization probability of the respective control group in the fourth year after the election in the election-year sample. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of the possibility to vote on the probability of being naturalized based on combined election and non-election-year sample of non-EU citizens: Alternative bandwidth choices

	(1)	(2)	(3)	(4)	(5)	(6)
	Low	Mid-level	High	Low	Mid-level	High
	HDI	HDI	HDI	HDI	HDI	HDI
Bandwidth	30 days	30 days	30 days	5 days	5 days	5 days
<i>Treat</i>	-0.009 (0.015)	0.006 (0.010)	0.003 (0.016)	-0.046 (0.030)	0.017 (0.021)	0.015 (0.035)
Treat x $I(t + 1)$	0.016* (0.010)	0.011 (0.008)	0.012 (0.010)	0.004 (0.024)	0.030 (0.021)	0.021 (0.027)
Treat x $I(t + 2)$	0.034*** (0.013)	0.018* (0.011)	0.012 (0.013)	-0.014 (0.031)	0.035 (0.028)	0.054 (0.034)
Treat x $I(t + 3)$	0.017 (0.015)	0.004 (0.010)	0.009 (0.014)	-0.022 (0.037)	0.006 (0.026)	0.031 (0.037)
Treat x $I(t + 4)$	0.006 (0.015)	-0.006 (0.010)	0.013 (0.014)	-0.033 (0.038)	-0.016 (0.023)	-0.007 (0.037)
Treat x <i>Elec.Y.</i> x $I(t + 1)$	-0.012 (0.021)	0.006 (0.013)	-0.056*** (0.021)	0.003 (0.051)	-0.047 (0.032)	-0.136** (0.056)
Treat x <i>Elec.Y.</i> x $I(t + 2)$	0.008 (0.028)	0.011 (0.020)	-0.074*** (0.027)	0.080 (0.064)	-0.035 (0.049)	-0.124* (0.070)
Treat x <i>Elec.Y.</i> x $I(t + 3)$	0.062** (0.030)	-0.014 (0.018)	-0.070** (0.028)	0.212*** (0.070)	-0.018 (0.043)	-0.082 (0.073)
Treat x <i>Elec.Y.</i> x $I(t + 4)$	0.081*** (0.031)	-0.008 (0.016)	-0.064** (0.028)	0.222*** (0.072)	0.003 (0.039)	-0.136* (0.070)
Immigration year x duration of stay FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$Mean_{Control}(t + 4)$	0.51	0.83	0.65	0.44	0.82	0.72
N	29,589	57,084	34,077	5,349	9,462	5,458
N clust	6,231	11,693	7,406	1,122	1,943	1,185
R2	0.158	0.397	0.198	0.165	0.409	0.234

Notes: Estimation results of a linear probability model in the sample of non-EU citizens exploiting the non-election-year sample as an additional control group. The sample in columns (1) to (3) is restricted to individuals immigrating within a 30-day window around the eligibility threshold, and in columns (4) to (6), to individuals immigrating within a 5-day window around the eligibility threshold. The estimates follow the model outlined in Equation 2. Standard errors are clustered at the individual level and are reported in parentheses. $Mean_{Control}(t + 4)$ presents the average naturalization probability of the respective control group in the fourth year after the election in the election-year sample. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Supplementary RDD estimation results

Table A8: Effects on the probability of being naturalized: RDD estimates for the non-election-year samples

Sample	(1)	(2)	(3)	(4)
	EU quasi-placebo	Non-EU Low HDI	Non-EU Mid-level HDI	Non-EU High HDI
Conventional	-0.017 (0.025)	-0.003 (0.031)	0.010 (0.020)	-0.013 (0.034)
Bias-corrected	-0.022 (0.025)	-0.004 (0.031)	0.005 (0.020)	-0.027 (0.034)
Robust	-0.022 (0.030)	-0.004 (0.037)	0.005 (0.023)	-0.027 (0.039)
Observations l	2,582	2,342	3,994	1,844
Observations r	2,878	2,121	3,464	1,813
Bandwidth l	29	34	30	23
Bandwidth r	29	34	30	23

Notes: The probability of being naturalized is measured after 7 years of stay in the country for individuals immigrating around the eligibility threshold covered in the non-election-year sample. RDD estimates are based on local linear estimates and a triangular kernel with the automatic rdrobust bandwidth choice (Calonico et al., 2016). Standard errors are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



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**ZEW – Leibniz-Zentrum für Europäische
Wirtschaftsforschung GmbH Mannheim**

ZEW – Leibniz Centre for European
Economic Research

L 7,1 · 68161 Mannheim · Germany

Phone +49 621 1235-01

info@zew.de · zew.de

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