Heterogeneous Naturalization Effects of Migrant Destination Dual Citizenship Reform: Quasi-Experimental Evidence from Europe

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Abstract

Does dual citizenship acceptance increase immigrants' propensity to naturalize? We exploit exogenous variation in citizenship legislation in 200 migrant origin countries to identify the effect of destination country policy reforms. We test our identification strategy in two West European migration destinations with contrasting reforms: a canonical liberal reform in Sweden (2001) and an atypical restrictive reversal in the Netherlands (1997). We apply a staggered difference-in-differences model employing microlevel administrative register data on complete migrant populations. We find that the reform effects are remarkably similar in effect size and heterogeneity, with liberalizing reform increasing naturalization rates with 5.9 percent and restrictive change decreasing rates with 5.3 percent. The largest effect is visible three years after the reform and concentrated among immigrants from EU and other highly developed countries. Our quasi-experimental evidence informs naturalization scholarship as well public debate on the social and political integration of immigrants.

Dual citizenship restrictions are one of the key barriers to immigrants' naturalization and thus to their economic (Hainmueller, Hangartner, and Ward (2019); Peters, Schmeets, and Vink (2019)), social (Peters (2020); Leclerc, Vink, and Schmeets (2021)) and political integration (Hainmueller, Hangartner, and Pietrantuono (2015)) in destination contexts. While the occurrence of such restrictions has diminished (Alarian and Goodman (2017); Vink et al. (2019)), 51 percent of states worldwide still partially or fully restricts dual citizenship policies (Vink, Van der Baaren, et al. (2021)). Dual citizenship remains controversial in various European states such as Austria, the Netherlands, and Germany, where the new coalition government announced dual citizenship liberalization in late 2021 as part of a major overhaul of its migration and citizenship laws.

While the relevance of dual citizenship acceptance for immigrant naturalization has been widely studied, few empirical strategies allow for a causal interpretation of the effect of dual citizenship reforms. Existing studies typically rely on a comparison of cohorts who are subject to different policy conditions (Labussière and Vink (2020)) or of behavior in time periods before and after the policy change (e.g. Jones-Correa (2001)). Such approaches risk conflating policy effects with coinciding cohort or period effects, respectively. Other studies identify the effect of dual citizenship reform by utilizing legislative changes in selected origin countries to analyse naturalization behavior

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in a single destination country across periods (eg Latin American migrants in the US, Mazzolari (2009)), but do not facilitate identifying heterogeneous origin effects or speak to debates on dual citizenship restrictions in a destination context. Besides solidifying and generalizing the empirical evidence for the effect of destination country dual citizenship reform on immigrant naturalization, we contribute with this letter to a better understanding of the heterogeneous effects of such reforms.

Hence the question we ask in this letter: how do liberalizing and restricting dual citizenship reforms in destination country naturalization requirements affect immigrant naturalization rates, and to whom do these reforms matter most?

We develop an original quasi-experimental approach that exploits exogenous variation in origin country dual citizenship regulation to identify the treatment effect of destination country policy change on migrants' naturalization behavior. To account for contemporaneous changes in origin country legislation, which affect the timing of treatment, we apply difference-in-differences (DiD) for staggered treatment (Callaway and Sant'Anna (2021)). We test our DiD approach in two West European migration destinations that experienced contrasting policy changes: a canonical liberalizing change in Sweden (2001) and an atypical restrictive reversal in the Netherlands (1997). We link individual-level administrative data on complete migration populations to macro data on origin country citizenship law provisions on the consequences of naturalization abroad (Vink, De Groot, and Luk (2015)).

Our letter adds to a growing number of studies that employ (quasi-)random treatment assignment to analyse the effects of institutional context such as discretionary decision procedures (Hainmueller, Hangartner, and Pietrantuono (2015)), fee waivers (Yasenov et al. (2019)), information nudges (Hotard et al. (2019)) or access to voting rights (Slotwinski, Stutzer, and Bevelander (2020)) on immigrant naturalization behavior.

Heterogeneous naturalization effects of dual citizenship

States have traditionally attempted to avoid the occurrence of dual citizenship to prevent ambiguous claims of diplomatic protection and divided loyalties in case of conflict (Spiro (1997): 1415), by demanding renunciation from individuals who voluntarily acquire another citizenship, or by withdrawing citizenship upon naturalization abroad. Citizenship acquisition is more likely if migrants are able to retain their original citizenship upon naturalization (Peters, Vink, and Schmeets (2016)). This is explained by a cost-benefit calculation, in which having to renounce any other citizenship as a requirement to naturalize abroad constitutes a considerable cost (Yang (1994): 458). Immigrants are more likely to naturalize if the added value of acquiring destination country citizenship outweighs the costs of doing so. There are two sides to this argument, relating to costs and (relative) benefits. With regards to costs of naturalization, besides fees and investments necessary to navigate and overcome all requirements, there are practical and psychological costs to giving up the origin citizenship (Mazzolari (2009): 173). Practical concerns relate to being able to travel back visa-free to the origin country, maintaining or inheriting property and keeping the right to vote in origin country elections. Psychologically, giving up the entitlement to a passport for yourself and your children may conflict with continued identification as a national of your country of birth.

But to whom does holding on to the origin country citizenship matter most in predicting the likelihood to acquire destination country citizenship? Drawing on economic theories of citizenship ascension (DeVoretz and Irastorza (2017)), we expect that the relevance of dual citizenship acceptance is conditioned by the relative value of the original citizenship vis-à-vis the citizenship of the destination

country (Vink, Tegunimataka, et al. (2021)). Hence, the symbolical costs of giving up one’s origin citizenship may matter to all, but whether they pose an obstacle to naturalization -we argue- is determined by the variable ‘citizenship premium’ within a world system that implies global inequality of opportunity and migration (Milanovic (2016)).

We use EU membership and level of human development as proxies of the relative value of origin country citizenship vis-à-vis the citizenship of the Netherlands and Sweden. Hence we expect that the effect of the removal (SE) or re-imposition (NL) of dual citizenship restrictions will be more pronounced among migrants who already have near equal residence status to citizens due to the EU free movement regime, and those migrants from highly developed origin countries who hold comparable global mobility rights. By contrast, for migrants from less developed non-EU countries of origin, the benefits of acquiring Swedish or Dutch citizenship will outweigh the costs of having to give up the (globally) less valuable origin citizenship.

Research design

Two contrasting reforms

Both the Netherlands and Sweden have historically held restrictive views on dual citizenship and moved towards liberalization in recent decades. Sweden fully liberalized its dual citizenship policy in July 2001, while the Netherlands did so already in the early 1990s, halfheartedly, and with a subsequent reversal in June 1997. In this paper, we study the effect of the canonical abolishment of the renunciation requirement in Sweden and the atypical reintroduction of the renunciation requirement in the Netherlands (including a new provision that foreigners with a Dutch citizen partner are not required to renounce their other citizenship, further details in [SM2. Sample selection](#)).

These two countries are uniquely placed to compare the effect of these changes in the renunciation requirement for three reasons. First, the citizenship policies in both countries are highly comparable in these years with a maximum residence requirement of five years and no formalized language or civic integration tests (which were introduced in the Netherlands only in 2003). Procedures in both countries leave little room for administrative discretion and provide a high degree of certainty about the outcome of their application to migrants who decide to acquire citizenship. Second, both countries are advanced economies where citizenship comes with access to highly developed welfare regimes and mobility within the European Union. Third, comparable and detailed micro-level administrative data allow us to compare the before-after differences around the reform years in the two countries.

Identification strategy

Our identification strategy is based on a comparative analysis of naturalization rates, before and after the dual citizenship reforms in Sweden and the Netherlands, among migrants who were affected by, respectively, the removal or the reintroduction of a renunciation requirement (treatment group), compared to those who are not affected by these changes (control group and placebo groups). Whether an immigrant is affected by dual citizenship reform in the destination country depends, crucially, on the type of citizenship legislation in the origin country.

Figure 1 visualizes our treatment identification strategy. The intuition of our approach is that only those migrants are affected by a destination country policy change who do not automatically lose their origin citizenship upon naturalization as a result of home state legislation, but who are able to

renounce their citizenship. Only for these migrants does it matter whether destination countries, such as the Netherlands and Sweden, require them to renounce their origin country citizenship upon naturalization.

By contrast, migrants who automatically lose their origin citizenship due to the legislation of their origin country cannot hold dual citizenship, irrespective of the destination country regulation (control group). We thus compare naturalization rates among the treated group with the control group, before and after the implementation of the policy reform, to estimate the effect of change in the destination country renunciation requirement on naturalization propensity. Because migrants do not have individual control over the origin country citizenship policy, they cannot self-select into either the treatment or control group; hence we consider treatment allocation exogenous.

Migrants from a minority of countries globally fall into a third category, as they neither lose their origin citizenship nor can renounce it; as this group always ends up with dual citizenship upon naturalization, they are not affected by destination country policy reform (placebo group). Those migrants that do not lose and can renounce the origin citizenship, but are not required to do so due to destination country exemptions (as in the Netherlands, for migrants with a citizen partner) also serve as a placebo group.

Figure 1: Identification strategy: treatment group allocation

Data

We draw on administrative register data from Statistics Sweden and Statistics Netherlands. These data include all legally registered individuals in these countries over time (see [Data availability statement](#)).

The research population consists of all foreign-born residents registered at a municipality between

1998 and 2004 in Sweden and between 1994 and 2000 in the Netherlands, i.e. six years surrounding the dual citizenship reform in both countries. We only include foreign-born individuals both of whose parents were born abroad and select only migrants who are 18 years or older at the moment of migration and who resided less than 40 years in the host country. We only observe migrants once they are eligible for naturalization (see [SM2. Sample selection](#)). As in both countries the renunciation requirement during the restrictive observation periods does not apply to migrants who have refugee status or are entitled to subsidiary protection, in line with international law, we exclude those migrants with a high probability to be exempted on asylum grounds (see [SM1. Operationalization](#)). In Sweden, the full sample consists of 441,737 migrants (2,454,809 observations). In the Netherlands, due to the partner exemption to the reintroduced renunciation requirement, we restrict the main analyses to migrants without a citizen partner ($N = 120,338$ individuals, 701,667 observations); we employ a sample based on migrants with a citizen partner in a placebo analysis ($N = 129,589$ individuals, 716,651 observations).

We subsequently link the individual-level register data to origin country-level information on dual citizenship acceptance (Vink, De Groot, and Luk (2015)). This dataset charts the rules of 200 countries over time with regard to the loss or renunciation of citizenship after a citizen voluntarily acquires the citizenship of another state. See sample details in [\[SM3. Summary statistics\]](#).

Method

We apply a DiD strategy to identify the effect of policy change on naturalization rates among immigrants, by comparing rates among treatment groups, in the years before and after the respective reforms. By exploiting exogenous variation in origin country dual citizenship regulation, the comparison of naturalization rates among the control and treatment group, before and after the allocation of treatment, yields the average treatment effect on the treated (ATT). We interpret the ATT as the average difference in the naturalization rate between the control and treatment group as a result of the removal or reintroduction of the renunciation requirement, all else constant.

Since dual citizenship reforms do not affect all migrant groups at the same time, as also origin country policies change over time, our empirical strategy needs to deal with such staggered treatment. This problem has been overlooked so far in the literature on immigrant naturalization. In particular, while the destination country reform takes place in a single year, the timing of origin country policy change varies between migrants from different countries. Since treatment depends on the policy conditions in both the origin and host country, some migrants receive delayed treatment.

To incorporate staggered treatment in a robust manner we draw on the DiD model developed by Callaway and Sant’Anna (2021), which generalizes the canonical two-way fixed effects model by estimating the group-time average treatment effect reflecting the average treatment effect for group g at time t . The “groups” in our case represent units who, due to the citizenship policies of their origin countries, were first treated in a given year. In Sweden this varies from 2001 (when the renunciation requirement was abolished) to all subsequent years (2002-2004). In the Netherlands, there is only one group (1997) because there are no origin countries which abolished their ‘cannot renounce’ provisions afterwards (which would cause migrants who were not affected by the restriction initially to be affected later).

Under this specification, the conventional DID design can be expressed as:

$$ATT(g, t) = E[Y_t(1) - Y_t(0) \mid G_g = 1] \quad (1)$$

where the average treatment effect ATT for each group-time combination (g, t) , is the difference in the naturalization rate as a result of the respective policy reform between migrants from treatment groups g and never treated migrants at every time t . The $ATT(g, t)$ allows for the estimation of heterogeneous treatment effects, i.e. treatment effects that increase or decrease over time, or treatment effects that differ based on adoption time. The effect on the overall ATT is weighted as a function of the time that each unit spends in the pre-period and post-period (details in [SM5. Notes on method](#)).

Results

The canonical case: liberal reform in Sweden (2001)

Figure 2 displays the ATT of the abolishment of the renunciation requirement in Sweden on percentage point changes in naturalization rates. The left panel plots the dynamic effect to test the parallel trends assumption; the right panel plots the overall treatment effect. The trend reveals no differences between the control and treatment group prior to the policy change, with confidence intervals that generally cover zero. From first year after policy change (t_1), relative naturalization rates start to diverge, ranging from an increase among the treatment group of 3.8 percentage points one year after treatment to 10.6 percentage points 3 years after treatment. Overall, the post-treatment ATT amounts to 5.9 percentage points. This effect is driven by migrants treated in 2001 and 2002 ([SM7. Group-time effects](#)). Note that one year prior to treatment (t_{-1}) the relative naturalization rate is lower among the treatment than the control group (by 1.05 percentage points), suggesting a modest anticipation effect: migrants postponing naturalization in order to benefit from the upcoming liberalization.

Figure 2: The average treatment effect on the treated (ATT) of the abolishment of the renunciation requirement in Sweden in 2001 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants). Left panel: dynamic (pseudo-) ATT estimated with varying base period (changes from period $t-1$ to t), for total sample. Right panel: overall ATT for total sample, sub-samples by origin region and placebo treatment group of immigrants from origin countries that do not allow voluntary renunciation of citizenship. Detailed output in [SM6. Model summaries](#).

Analyses of impact heterogeneity (right panel), confirm the expectation that the abolishment of the renunciation requirement in Sweden matters particularly to migrants from EU or other highly

developed countries of origin. This is consistent with the notion that the relevance of dual citizenship acceptance is conditioned by the relative value of the original citizenship vis-à-vis the citizenship of the destination country, and in line with earlier research comparing naturalization behavior during liberal and restrictive time periods (Vink, Tegunimataka, et al. (2021)). Effect sizes range from 7.2 and 7.1 percentage points for migrants from EU and other highly develop countries respectively to 4.7 percentage points for migrants from low to medium developed countries of origin (see dynamic effects in [SM8. Heterogeneous effects](#)).

When replacing the treatment group with the placebo group – who cannot renounce the origin citizenship and thus should not be affected by the dual citizenship liberalization – there is no longer a significant treatment effect. This confirms that the difference in the relative naturalization rate between the control and treatment group is driven by the change in institutional context.

We perform several checks to confirm the robustness of these findings. First, the inclusion of time-invariant covariates (gender, education, age at migration) does not alter the main results, indicating that our identification strategy successfully leverages the exogenous treatment of origin country legislation. Second, restricting our sample to migrants who remain in Sweden once they are eligible to naturalize results a marginally higher treatment effect, suggesting that we provide a conservative estimate that is robust to selective out-migration. Third, we find evidence of anticipatory behavior reflected in lower-than-expected naturalization rates in the year before treatment; taking into account 1 year anticipation lowers the ATT from 5.9 percent points to 4.7 percent points ([SM9. Robustness checks](#), Figure S9).

The atypical case: liberalization reversed in the Netherlands (1997)

Our analysis of the atypical restriction of dual citizenship policy in the Netherlands reveals a mirror image of the canonical Swedish case. The dynamic trend in Figure 3 (left panel) shows negligible differences in the relative naturalization rate between the control and treatment group prior to treatment, and a divergence afterwards. As expected, the ATT is negative, ranging from 3.6 percentage points the year after treatment to 12.1 percentage points three years after treatment. The aggregated ATT (right panel) amounts to 5.3 percentage points, which is remarkably similar in effect size to the Swedish case.

Figure 3: The average treatment effect on the treated (ATT) of the reintroduction of the renunciation requirement in the Netherlands in 1997 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants) who do not have a Dutch citizen partner in any observation year. Left panel: dynamic (pseudo-) ATT estimated with varying base period (changes from period $t-1$ to t), for total sample. Right panel: overall ATT for total sample and sub-samples by origin region; placebo analysis based on sample with immigrants who have a Dutch citizen partner in each observation year, with placebo treatment group those immigrants who do not lose, but can renounce citizenship of origin country. Detailed output in [SM6. Model summaries](#).

The same is true for the heterogeneity analyses, which confirm a stronger effect among migrants for whom the relative cost of renunciation is higher. Among migrants from EU and other highly developed countries, the post-treatment difference between the control and treatment group is 8.1 and 7.2 percentage points respectively, compared to 4.3 percentage points among migrants from low and medium developed countries of origin (see dynamic effects in [SM8. Heterogeneous effects](#)).

When substituting the treatment group for the placebo group – migrants with a Dutch partner who are exempted from the renunciation requirement – the negative ATT disappears, suggesting that the main analysis captures the relevance of the restrictive reversal of dual citizenship policy in the Netherlands. Note that the pre-reform naturalization behavior among the ‘cannot renounce’ group violates the parallel trends assumption and, in contrast with Sweden, is not a suitable placebo group here ([SM6. Model summaries](#)). We attribute this to a positive spillover effect of the liberal 1992-1997 (pre-reform) policy in the Netherlands, which encouraged higher naturalisation even among those that strictly were not subject to the renunciation requirement (as as they could not renounce their origin citizenship); by contrast, migrants from origin countries whose naturalization still came with the penalty of losing the origin citizenship (control group) were much less affected by this spillover effect. This finding suggests that placebo group identification requires attention to spillover effects of dual citizenship policies; this should be further explored in other contexts.

Given the institutional context, in the Netherlands we can only estimate the main reform effect for the subpopulation of immigrants without a Dutch citizen partner. However, a heterogeneity analysis of the reform effect by citizen status of the partner in Sweden indicates that dual citizenship reform affects migrants without a citizen partner (6.1 percentage points) more strongly than those with a citizen partner (5.3 percentage points, [SM6. Model summaries](#)), reflecting lower naturalization propensity among migrants without a citizen partner (Helgertz and Bevelander (2017); Peters, Vink, and Schmeets (2016)). These results remain stable when including time-invariant covariates, excluding right-censoring and allowing for one year anticipation ([SM9. Robustness checks](#), Fig S10).

Discussion

Our analysis of the relevance of dual citizenship acceptance for immigrant naturalization shows that both the abolishment of the renunciation requirement in Sweden and its reintroduction in the Netherlands are associated with substantial increases, respectively decreases, in the naturalization rate. Effect sizes are remarkably similar, with an overall increase in the naturalization rate of 5.8 percentage points in Sweden and decrease of 5.3 percentage points in the Netherlands. Consistent with our hypothesis that the perceived cost of renunciation is higher for migrants whose original citizenship is relatively valuable, dual citizenship liberalization affects the naturalization rate of migrants from EU and other highly developed countries more than their counterparts from low and medium developed countries. The treatment effect disappears in placebo analyses in which the treatment group is replaced by migrants who are exempted from the renunciation requirement.

These findings contribute to our understanding of the relevance of dual citizenship legislation for immigrant naturalization in four ways. First, we developed a novel quasi-experimental DiD approach that leverages exogenous variation in origin country dual citizenship legislation. In contrast to strategies that compare cohorts subject to different policy conditions or periods before and after a policy change, our approach is less sensitive to period or cohort confounders. By allowing for parameters disaggregated by group and time, we avoid bias associated with variation in treatment timing and duration (i.e. staggered treatment).

Second, we provide a better understanding of the heterogeneous effect of such reforms by confirming that the perceived cost of renunciation is conditioned by the relative value of the destination country citizenship vis-à-vis the original citizenship. Moreover, by accounting for staggered treatment our approach is more transparent about potential impact heterogeneity among those who have been subject to a policy condition for a shorter or longer period of time.

Third, the external validity of our identification strategy is increased by applying the design not only to a common liberalizing case but also in an atypical restricting context. Even if the global diffusion of dual citizenship acceptance (Vink et al. (2019)) makes such restrictions empirically rare, theoretically we expect -and confirm- that the heterogeneous effects of dual citizenship reform should apply equally under both types of reform, just in opposite directions. Hence, adding the atypical restrictive Dutch case to the typical liberal Swedish case allows generalizing our argument about the heterogeneous effects of dual citizenship reform.

Fourth, whereas designs employing (quasi-)random treatment assignment are increasingly common in the citizenship acquisition literature, such studies frequently draw on unique circumstances for identification (e.g. discretionary decision procedures or specific interventions). Their conclusions are often difficult to generalize, as strategies cannot be easily replicated in other contexts. In contrast, our exogenous treatment variation leverages information that is readily available and universally applicable: origin country dual citizenship legislation. Our study thus provides a methodological blueprint to analyze other European countries that experienced similar liberal reforms, such as Finland (2003), Denmark (2015) or Norway (2020), or might experience these in the near future (Germany), as well as restrictions such as Croatia (2001), Estonia (1992) or the Czech Republic (1993).

Supplementary Materials

To view supplementary material for this article, please see [Supplementary materials](#).

Data availability statement

Administrative register data for the analyses in this paper were made available by Statistics Netherlands and Statistics Sweden. Use of these data is covered by data use agreements that do not allow us to disclose the individual-level data. Information about access to microdata of Statistics Netherlands is available here: <https://www.cbs.nl/en-gb/onze-diensten/customised-services-microdata/microdata-conducting-your-own-research>. Information about access to microdata of Statistics Sweden is available here: <https://www.scb.se/en/services/guidance-for-researchers-and-universities/>.

Replication material to reproduce our analyses once access to the data has been granted will be made available upon publication of the paper on the Dataverse of the corresponding author: [].

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Conflict of interest

The authors declare no ethical issues or conflicts of interest in this research.

Ethical standards

The authors affirm that this article adheres to the [APSA’s Principles and Guidance on Human Subject Research](#).

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Supplementary materials

Supplementary materials for paper ‘Heterogeneous Naturalization Effects of Migrant Destination
Dual Citizenship Reform: Quasi-Experimental Evidence from Europe’.

SM1. Operationalization

Notes on operationalization of main variables:

- *Observation year*: the administrative data made available to us by Statistics Sweden and Statistics Netherlands (see [Data availability statement](#)) contain annual observations with one observation per year per individual migrant.

In Sweden, annual observations refer to December 31 of each year. Each year in the Swedish data is thus treated as the same year (e.g. 2000 is based on data from December 31, 2000, and so forth).

In the Netherlands, observations refer to the first of January of each year, hence reflecting the situation in the previous calendar year. In other words, if a migrant is registered as having naturalized on the first of January of a given year, we treat this migrant as having naturalized in the previous calendar year. As a result, in all the tables and figures, each year refers to the measurement of January 1 of the subsequent year (e.g. 1994 is based on data from January 1, 1995, and so forth).

- *Citizen partner*: we distinguish between migrants without (0) and with (1) a registered partner who is a citizen of the destination country. For the purpose of the sub-samples used in our difference-in-differences analysis, this measurement is time-invariant, i.e. we identify migrants without (0) and with (1) a registered partner who is a citizen of the destination country in all relevant observation years.

Note that the ‘total’ sample in Sweden contains no restriction by the citizen status of the partner (nor changes in citizenship status of the partner during the observation years), whereas in the Netherlands -due to the exemption to the re-introduced renunciation requirement in 1997- the ‘total’ sample includes only migrants who are not registered as having a citizen partner in any observation year. The sub-sample of those migrants with a citizen partner in all observations is used in the placebo analysis.

- *EU15+*: binary variable capturing whether (1) or not (0) a migrant is born in a country that is a member state of the European Union or one of the associated states with which the EU shared a free movement regime between 1994-2004: Belgium, Denmark France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, the United Kingdom (EU) + Austria, Finland, Sweden (1994: EFTA/EEA, from 1995: EU) + Iceland, Norway (1994: EFTA/EEA) + Liechtenstein (1995: EFTA/EEA).

- *Human Development Index (HDI)*, this is a three-dimensional measurement of the socioeconomic conditions in a country, based on indicators for health, knowledge and standards of living. The composite index provides a score from 0 to 1, where a higher score equals greater development (World Bank 2018). For the subgroup analyses, this score is divided into two categories along the boundaries set by the United Nations Development Programme (UNDP 2022). More specifically, the groups are coded as follows: ‘Very high/high’: ≥ 0.7 ; and ‘Medium/Low’: < 0.7 .

- *Treatment group*: information is derived from the MACIMIDE Global Expatriate Dual Citizenship Dataset (Vink, De Groot, and Luk (2015)), which provides detailed information on 200 countries in the world since 1960 regarding the involuntary loss and voluntary renunciation of citizenship after a citizen of a state voluntarily acquires the citizenship of another state. We use the categorical variable “dualcit_grouped” from this Dataset (see Codebook for details) and combine this information with data on country of birth from the migrant population identified in the administrative micro data from Sweden and the Netherlands.

To identify treatment group we follow the stepwise model illustrated in Figure 1, with the following adjustment due to the specific context of Sweden and the Netherlands. In line with Callaway and Sant’Anna (2021), treatment group allocation is determined in a time-invariant manner, to ensure stable treatment group comparison of naturalisation rates over time.

Sweden - treatment group identification

Control group: countries with `dualcit_grouped = 1`

Treatment groups

- *2001:* countries with `dualcit_grouped = 2` from 2001
- *2002:* countries with `dualcit_grouped = 2` only from 2002
 - Comment: This group includes France and Italy as citizens of these countries lost resp. French and Italian citizenship when acquiring Swedish citizenship until 30/06/2002, when Sweden denounced Chapter 1 of the 1963 Strasbourg Convention on Reduction of Cases of Multiple Nationality, Chapter 1 (which implies automatic loss of origin citizenship by national of State Party who acquires citizenship of another State Party).
- *2003:* countries with `dualcit_grouped = 2` only from 2003
- *2004:* countries with `dualcit_grouped = 2` only from 2004

Placebo group (cannot renounce): `dualcit_grouped = 3`

The Netherlands - treatment group identification

Control group: countries with `dualcit_grouped = 1`

- Comment: This group includes France and Italy as citizens of these countries lost resp. French and Italian citizenship when acquiring Dutch citizenship during the observation period 1994-2000 as a result of Chapter 1 of the 1963 Strasbourg Convention on Reduction of Cases of Multiple Nationality, Chapter 1 (which implies automatic loss of origin citizenship by national of State Party who acquires citizenship of another State Party).

Treatment group: countries with `dualcit_grouped = 2` + no Dutch spouse or registered partner in any observation year.

- Comment: we measure citizen partnership in a time invariant manner, hence excluding migrants from our sample who change citizen partner status over the observation period.

Placebo groups

- *(cannot renounce):* countries with `dualcit_grouped = 3`
- *(citizen partner):* countries with `dualcit_grouped = 2` + Dutch spouse or registered partner in all observation years.

A replication file will be made available upon publication of the paper, linking all origin countries with treatment group in both destination countries.

- *Asylum applicant:* dichotomous indicator for whether a migrant is registered as, or likely to be, an asylum applicant (1) or not (0).

In Sweden, this is based on register data whether a migrant is registered as an asylum applicant, which is available from migrant cohort 1986 onwards. Given that asylum seekers have to formally

apply for asylum, we assume that migrants for whom information on the migration motive is missing are not asylum migrants (and therefore coded as 0). In total, 737,639 observations in our sample are coded as asylum migrants. These migrants originate primarily from Bosnia and Herzegovina, Chile, Eritrea, Ethiopia, Lebanon, Iran, Iraq, Romania, Syria, Vietnam and former Yugoslavia.

In the Netherlands, register data on asylum applications is only available from migrant cohorts 1995 onwards. For older migrants, we draw on aggregate statistics on asylum applications in the Netherlands per origin country per cohort, made available by Statistics Netherlands. Aggregate statistics on asylum applications by origin country can only be determined from migration cohort 1987 onwards. Based on the proportion of asylum applications, we exclude a corresponding number of random observations from that country-cohort combination. For instance, according to Statistics Netherlands, 60.86 percent of migrants from Ghana in the year 1989 were asylum seekers. Therefore, of all Ghanaians who arrived in the Netherlands in 1989, 60.86 percent of the observations, selected at random, are excluded from the main analysis. In total, 152,573 observations are thus coded as asylum migrants. These migrants originate primarily from Afghanistan, Algeria, Ethiopia, Ghana, Iran, Iraq, Lebanon, the former Soviet Union, Syria and former Yugoslavia. For migrants from our sample from cohort 1995 and onwards, for which registered asylum application data is available, we observe a moderate to strong positive correlation between the registered and the derived status of being an asylum applicant (Pearson correlation coefficient = 0.632, $p = 0.000$).

SM2. Sample selection

Sweden

The research population consists of all foreign-born residents registered at a municipality between 1998 and 2004 in Sweden. We focus on cohorts who resided less than 40 years in the host country within the observation period, hence those who have migrated to Sweden from 1958 onwards (40 years before the first observation year 1998). We only include foreign-born individuals both of whose parents were born abroad and select only migrants who are 18 years or older at the moment of migration. We only observe migrants once they are eligible for naturalization.

Eligibility criteria

The requirements for naturalization in Sweden are stipulated in the Swedish Nationality Act (Lag om Svenskt medborgarskap). The most important requirements for naturalization are as follows: an applicant needs to

- be at least eighteen years of age;
- in possession of a non-temporary residence permit;
- have been domiciled on the territory of Sweden for a period of five years prior to the request for naturalization;
- has led, and can be expected to lead a respectable life.

While migrants normally become eligible for naturalization after 5 years of residence, there are exceptions for migrants who are the registered partner of a Swedish citizen for three years, and cohabited for the last two years. Moreover, the residence requirement is set to four years for stateless individuals and those who are considered refugees under Chapter 4, Section 1 of the Aliens Act (Utlänningslag), and to two years for Nordic citizens (Denmark, Finland, Iceland or Norway).

The moment of eligibility is determined in a stepwise manner according to the following criteria:

Figure S1: Sweden: main criteria for residence-based naturalization, 1998-2004

Pre-2001 renunciation requirement:

In Sweden, citizenship policy was oriented traditionally to avoiding instances of dual citizenship (Bernitz 2012: 4). The Swedish Citizenship Act of 1950 stipulates that a foreign national who acquires Swedish citizenship by naturalization is expected to renounce their original citizenship, unless she or he already automatically loses that citizenship, or cannot reasonably be expected to do so. However, the following group is exempted from this requirement:

- migrants who have refugee status or are asylum migrants who are entitled to subsidiary protection.

Discussions on gender equality in citizenship legislation, political representation of minorities and cultural pluralism in integration policy resulted in increasingly positive views towards liberalizing dual citizenship legislation, particularly among the Social Democrats. Throughout the 1980s exemptions to the renunciation requirement were instituted for those cases where renunciation would be very costly (Statens offentliga utredningar 1984, 11, 131ff. See also Departementsserien 1986, 6, 55: Dubbelt medborgarskap). Following several years of preceding discussion on full dual citizenship liberalization (see Szabo 1997: 54-79 and Bernitz 2012: 5 for an overview and discussion), under pressure from Swedish nationals living abroad who were also subject to dual citizenship restrictions (Spang 2007: 112), in July 2001 the renunciation requirement was abolished, allowing naturalizing migrants to retain their original citizenship if the host country would allow it (Sveriges Rikstag Parliament records 2000/01, 70).

Migrants who presumably have refugee status or are asylum migrants who are entitled to subsidiary protection are excluded from the main analysis (SM1. Operationalization). Note that Dutch migrants

with a Swedish partner, who would normally automatically lose their original citizenship when voluntarily acquiring another, could hold dual citizenship from 2001 onwards due to reciprocity of the partner exception in the Dutch citizenship law.

The Netherlands

The research population consists of all foreign-born residents registered at a municipality between 1994 and 2000 in the Netherlands. We focus on cohorts who resided less than 40 years in the host country within the observation period, hence those who have migrated to the Netherlands from 1954 onwards (40 years before the first observation year 1994). We only include foreign-born individuals both of whose parents were born abroad and select only migrants who are 18 years or older at the moment of migration. We only observe migrants once they are eligible for naturalization.

Eligibility criteria

The requirements for naturalization in the Netherlands are stipulated in the Dutch Nationality Act (Rijkswet op het Nederlanderschap). The most important requirements for naturalization are as follows: an applicant needs to

- be at least eighteen years of age;
- in possession of a non-temporary residence permit;
- have primary residence on the territory of the Netherlands for a period of five years prior to the request for naturalization;
- pose no danger to public order, good morals or security in the Netherlands;
- be considered sufficiently integrated into Dutch society based on his or her mastery of the Dutch language or – if residing in the Dutch Antilles or Aruba – the language commonly spoken on the island besides Dutch.

While migrants normally become eligible for naturalization after 5 years of residence, there are exceptions for migrants who are the registered partner of and cohabit with a Dutch citizen, or those who cohabit in the Netherlands with a Dutch citizen, for three years. Moreover, the residence requirement is set to three years for stateless individuals.

The moment of eligibility is determined in a stepwise manner according to the following criteria:

†Include years of cohabitation prior to the registered partnership.

Figure S2: The Netherlands: main criteria for residence-based naturalization, 1994-2000

Under current Dutch law, a foreign national who acquires Dutch citizenship by option declaration or naturalization in principle is expected to renounce her or his other citizenship(s), unless she or he already automatically loses that citizenship, or cannot reasonably be expected to do so; on the other hand, a Dutch citizen who voluntarily acquires another citizenship automatically loses Dutch citizenship, unless one of the exceptions indicated by the law applies. One of these exceptions is having a Dutch partner, which allows migrants to naturalize while keeping their original citizenship

We exclude migrants born in Indonesia or in Suriname before 1975 from the Dutch sample, as almost all these migrants are Dutch citizens by birth or where able to acquire Dutch citizenship on special facilitated grounds.

While migrants who are in a registered partnership with a Dutch citizen prior to migration to the Netherlands can be eligible earlier than three years after migration, Dutch register data does not include information to identify such instances.

The renunciation requirement before and after 1997

In the Netherlands, the restrictive approach towards dual citizenship (Van Oers et al. 2013) already met with sustained criticism since the 1980s from immigrant groups and (centre-)left political parties, who argued that dual citizenship policies should be liberalized (van den Bedem 1993: 31-35). The Scientific Council for Government Policy advised late 1980s to allow migrants to retain their original citizenship upon naturalization to reinforce the legal position of immigrants through naturalization, thereby facilitating their participation and integration (Heijs 1995). The subsequent centre-left coalition thus decided, after considerable parliamentary debate, no longer to require naturalizing foreigners to renounce their original citizenship (Staatscourant 1992, no. 25; see also Kamerstuk 21 132, nr. 8, vergaderjaar 1989-1990, p. 43). However, the amendment of the Dutch Nationality Act

to abolish the renunciation requirement, which was introduced in 1993 (Wetsvoorstel 23 029 (R1461), encountered increasing opposition and failed to be approved by the Senate in 1996, resulting in its withdrawal. The renunciation requirement was thus reinstated in June 1997, albeit with a new provision that foreigners who were married to a Dutch citizen were not required to renounce their other citizenship (Staatscourant 1997, no. 128). This exception reflected the Second Protocol to the 1963 Strasbourg Convention, which was adopted in 1993 and had been signed by the Netherlands and ratified in 1996.

Hence, since 18 June 1997, foreign nationals who acquire Dutch citizenship by option declaration or naturalization are expected to renounce their other citizenship(s), unless they automatically lose that citizenship due to origin country legislation, or cannot reasonably be expected to do so. In addition, there are a number of exceptions to this requirement, including those relevant for our sample population of foreign-born person who arrive in the Netherlands as adult:

- migrants who have refugee status or are asylum migrants who are entitled to subsidiary protection.
- migrants with a Dutch partner

Migrants who presumably have refugee status or are asylum migrants who are entitled to subsidiary protection are excluded from the main analysis ([SM1. Operationalization](#)).

SM3. Summary statistics

Sweden

variable	category	percent
naturalised	yes	57.38
	no	42.61
treatment	control	25.44
	treatment2001	39.05
	treatment2002	1.80
	treatment2003	27.64
	treatment2004	0.28
	placebo	2.41
	post-treatment eligible	3.35
gender	male	45.39
	female	54.60
age at migration	18-30	65.16
	31-40	21.11
	41-50	7.58
	50+	6.13
minor children in the household	yes	34.09
	no	65.90
highest level of education	low	53.65
	middle	23.07
	high	14.80
	unknown	8.46
country of birth (grouped)	EU	47.35
	non-EU high HDI	28.33
	non-EU low/medium HDI	24.31

Table S1: Sweden: summary statistics (sample: total) | N(ind) = 441.737 | N(obs) = 2.454.809

Table S2: Sweden: top 10 origin countries, control group and treatment groups

country	obs	group	country	obs	group
Norway	120413	control	fmr Yugoslavia	183504	treat_2001
Denmark	102436	control	Poland	158131	treat_2001
Germany	78150	control	Turkey	99606	treat_2001
Chile	54467	control	Iran	78996	treat_2001
Iraq	41025	control	Greece	47094	treat_2001
Thailand	28803	control	UK	47017	treat_2001
Bosnia and Her	23361	control	Hungary	40087	treat_2001
China	22962	control	Lebanon	32805	treat_2001
Spain	19065	control	Syria	30340	treat_2001
India	18177	control	Romania	29025	treat_2001
Italy	19423	treat_2002	Finland	646560	treat_2003
France	13221	treat_2002	Philippines	19038	treat_2003
Netherlands	6479	treat_2002	Iceland	13179	treat_2003
Australia	4167	treat_2002	Rwanda	41	treat_2003
Belarus	361	treat_2002	Sao Tome & Prin	9	treat_2003
Congo	172	treat_2002	Bolivia	6021	treat_2004
			Mozambique	633	treat_2004
			Djibouti	92	treat_2004

Table S3: Sweden: top 10 origin countries, placebo group (no loss of citizenship, cannot renounce)

country	obs	group
Morocco	19683	placebo (cannot renounce)
Tunisia	12600	placebo (cannot renounce)
Uruguay	8564	placebo (cannot renounce)
Argentina	7256	placebo (cannot renounce)
Eritrea	2253	placebo (cannot renounce)
Mexico	2195	placebo (cannot renounce)
Ecuador	1509	placebo (cannot renounce)
Guatemala	655	placebo (cannot renounce)
Dom Repuglic	528	placebo (cannot renounce)
Panama	476	placebo (cannot renounce)

Netherlands

variable	category	percent
naturalised	yes	32.40
	no	67.59
treatment	control	21.40
	treatment1997	39.49
	placebo (cannot renounce)	29.68
	post-treatment eligible	9.41
gender	male	48.16
	female	51.83
age at migration	18-30	65.91
	31-40	25.02
	41-50	7.47
	50+	1.58
minor children in the household	yes	54.19
	no	45.80
highest level of education	low	33.47
	middle	8.02
	high	4.50
	unknown	53.99
country of birth (grouped)	EU	22.02
	non-EU high HDI	6.54
	non-EU low/medium HDI	71.43

Table S4: Netherlands: summary statistics I (sample: no citizen partner)

N(ind) = 120.338 | N(obs) = 701.667

variable	category	percent
naturalised	yes	65.97
	no	34.02
treatment	control	40.35
	placebo (citizen partner)	37.93
	placebo (cannot renounce)	10.57
	post-treatment eligible	11.13
gender	male	41.48
	female	58.51
age at migration	18-30	78.09
	31-40	17.54
	41-50	3.59
	50+	0.76
minor children in the household	yes	62.20
	no	37.79
highest level of education	low	23.00
	middle	13.49
	high	8.19
	unknown	55.30
country of birth (grouped)	EU	38.13
	non-EU high HDI	13.08
	non-EU low/medium HDI	48.77

Table S5: Netherlands: summary statistics II (sample: citizen partner)
N(ind) = 129.589 | N(obs) = 716.651

Table S6: The Netherlands: top 10 origin countries, control group and 1997 treatment group (immigrants without citizen partner)

country	obs	group	country	obs	group
Germany	49475	control	Turkey	169250	treat_1997
Belgium	19399	control	UK	20120	treat_1997
Spain	14811	control	fmr Yugoslavia	16518	treat_1997
Italy	13436	control	Portugal	11037	treat_1997
China	8973	control	Cape Verde	10494	treat_1997
France	5873	control	Greece	5785	treat_1997
Pakistan	5519	control	Vietnam	5083	treat_1997
India	4180	control	Egypt	4582	treat_1997
Ethiopia	3537	control	Poland	4167	treat_1997
Philippines	2843	control	US	3715	treat_1997

Table S7: The Netherlands: top 10 origin countries, control group and placebo treatment group (immigrants with citizen partner)

country	obs	group	country	obs	group
Germany	121169	control	Turkey	89570	placebo (citizen partner)
Belgium	48100	control	UK	38147	placebo (citizen partner)
China	24318	control	fmr Yugoslavia	23928	placebo (citizen partner)
Spain	18203	control	Cape Verde	14300	placebo (citizen partner)
Philippines	12730	control	Vietnam	13171	placebo (citizen partner)
Pakistan	9137	control	Poland	12905	placebo (citizen partner)
Austria	8509	control	US	8041	placebo (citizen partner)
Thailand	6680	control	Portugal	6617	placebo (citizen partner)
India	5381	control	Egypt	6537	placebo (citizen partner)
Denmark	3808	control	Greece	6048	placebo (citizen partner)

SM4. Descriptive trend

Figure S3: Unadjusted naturalization rates over time in the staggered treatment groups and control group of never treated (left panel) and the placebo group and control group (right panel), among foreign-born immigrants (excl asylum applicants) who are eligible to naturalize and have resided less than 40 years in Sweden. Placebo group consists of migrants from origin countries where citizenship is not lost upon naturalization abroad and cannot be renounced. Vertical line refers to the removal of the renunciation requirement in 2001.

Figure S4: Unadjusted naturalization rates over time in the treatment group and control group of never treated (left panel) and the placebo group and control group (right panel), among foreign-born immigrants (excl asylum applicants) who are eligible to naturalize and have resided less than 40 years in the Netherlands. In the left panel both the treatment and control group consist of immigrants who do not have a Dutch citizen partner in any observation year. In the right panel both the placebo and control group consist of immigrants who have a Dutch citizen partner in each observation year, and the placebo group consists of migrants from origin countries where citizenship is not lost upon naturalization abroad but can be renounced. Vertical line refers to the reintroduction of the renunciation requirement in 1997.

SM5. Notes on method

We use migrant-level ('ID_number'), unbalanced panel data with annual observations to analyze the effect of a policy change ('treatment') on the outcome *naturalized* ('Y') using difference-in-differences (DiD) among those migrants from the treatment group. Due to exogenous variation (changes in origin country characteristics) some units are not treated immediately in the year the policy change goes into effect, but only in one of the subsequent years. Staggered treatment is problematic for a number of reasons (Callaway and Sant'Anna (2021)). Ordinary least squares regression would over-weight units with more variance in treatment status (in this case, being subject to a policy condition) when estimating the treatment effect, introducing selection bias if being treated early or late is non-random, or if treatment effects depend on treatment duration. Moreover, in the context of DiD models, the assessment of the parallel trend assumption is complicated when some individuals in the treatment group are not yet treated in the post-treatment period. To account for differences in treatment timings ('treat_group'), or staggered treatment, we use the "did" package by Callaway and Sant'Anna in R, applying the following settings:

```
library(did)

att_gt(yname = "Y", tname = "t", idname = "ID_number", gname = "treat_group",
data = dataset, panel = TRUE, allow_unbalanced_panel = TRUE)
```

In this approach, we start with the identification of causal parameters associated with the policy change disaggregated by group and time. These 'group-time average treatment effects' can be thought of as the average treatment effect for group g at time t . In this paper, group g corresponds to the year in which immigrants first receive treatment. In Sweden for example, migrants from Poland receive treatment in 2001, because Polish citizens do not automatically lose their original citizenship upon naturalization and Sweden no longer requires renunciation from this year onwards. Migrants from Finland however only receive treatment in 2003 because they automatically lost their original citizenship upon naturalization before then, regardless of the Swedish policy. Migrants from Poland and Finland are thus in the 2001 and 2003 group respectively. Time t corresponds to the observation year.

Formally, the estimation draws on weighted average of all possible 2x2 traditional DiD comparisons (i.e. DiD designs involving two discrete time periods – pre- and post-treatment – and two treatment groups – units receiving treatment or not). This number increases exponentially with the number of treatment groups, so to simplify, consider the scenario in which there are only two treatment groups besides the never-treated (u) [note: the Swedish case has four]: the early-treated e who receive treatment at time (t_e^*), and the late-treated l who receive treatment at time (t_l^*).

In this set-up, there are three time-periods: the early period which takes place prior to treatment reception for group e and group l (specified as T1 or $[0, t_e^* - 1]$; the intermediate period in which group e has received treatment but group l has not (specified as T2 or $[0, t_l^* - 1]$; the late period in which both group e and group l have received treatment (specified as T3 or $[t_l^*, T]$). This culminates in four possible 2x2 DiD comparisons which together make up the ATT.

The first two combinations are based on a comparison of group e or l respectively with the untreated (group u) in the periods where the relevant treatment group has received treatment (T2 + T3 for group e (equation 2) and T3 for group l (equation 3)).

$$Y_{eu}^{2x2} = (\bar{Y}_e^{T2+T3} - \bar{Y}_e^{T1}) - (\bar{Y}_u^{T2+T3} - \bar{Y}_u^{T1}) \quad (2)$$

$$Y_{lu}^{2x2} = (\bar{Y}_l^{T3} - \bar{Y}_l^{T1+T2}) - (\bar{Y}_u^{T3} - \bar{Y}_u^{T1+T2}) \quad (3)$$

The other two combinations are based on a comparison between groups that receive treatment at different timings. In the first case, we compare units in group e to units in group l before T3. In this scenario, group l serves as the control group to the treatment group e , whose treatment status varies (over T1 and T2), in contrast to the stable status over this period of time for group l .

$$Y_{el}^{2x2,e} = (\bar{Y}_e^{T2} - \bar{Y}_e^{T1}) - (\bar{Y}_l^{T2} - \bar{Y}_l^{T1}) \quad (4)$$

In the second case, we compare units in group l to units in group e after T1. In this scenario, group e serves as the control group to the treatment group l , whose treatment status varies (over T2 and T3), in contrast to the stable status over this period of time for group e .

$$Y_{el}^{2x2,l} = (\bar{Y}_l^{T3} - \bar{Y}_l^{T2}) - (\bar{Y}_e^{T3} - \bar{Y}_e^{T2}) \quad (5)$$

If the number of treatment groups is denoted as G , then the possible 2x2 DiD combinations equals $G^2 - G$ (comparing treated groups at various times) + G (comparing treatment groups with the never-treated), or G^2 . The ATT constitutes the weighted average of the outcomes of these 2x2 DiD equations, where the relevant weights depend on (1) the absolute size of the subsample in question, (2) the relative size of the treatment and control group, (3) the duration of treatment (based on timing of treatment), and (4) the magnitude of the treatment variance in each subsample.

This approach has a number of advantages. First, it avoids bias by accommodating variation in treatment timing. If we used a traditional two group, two periods DiD setup in this context, the parallel trend assumption is unlikely to hold given that some migrants are not yet treated in parts of the post-treatment period. Second, given that the overall ATT is weighted as a function of the time that each unit spends in the pre- and post-treatment period, failing to account for unequal treatment timing could result in an over- or underestimation of the treatment effect. Third, this approach is transparent about potential impact heterogeneity by group and/or time, whereas such information would be hidden in the traditional DiD setup where all treatment groups and post-treatment periods are aggregated in one ATT.

By specifying separate treatment groups for each period in which individuals first receive treatment, this provides average treatment effects across all group-time combinations. To acquire average effects across different lengths of exposure (but irrespective of treatment timing) or across different groups (but irrespective of duration since treatment), we apply the following settings (where ‘att_gt_output’ is an att_gt object):

```
aggte(att_gt_output, type = "dynamic")
aggte(att_gt_output, type = "group")
```

Due to exogenous treatment allocation, a comparison of the control and treatment group should not be biased by compositional confounders. To assess whether that is the case, we perform the main analysis whilst controlling for either gender, highest level of education and age at migration, using these settings (where ‘control’ is the control variable):

```
att_gt(yname = "Y", tname = "t", idname = "ID_number", gname = "treat_group",
data = dataset, panel = TRUE, allow_unbalanced_panel = TRUE, xformula =
\~control)
```

If s denotes time since treatment, group compositions can differ across s when using an unbalanced panel (as is the case in our study). The price one pays for balancing the panel is reduced external validity and potential selection bias due to non-random right- and left-censoring; hence we do not balance the panel but have to consider the implications of these censored units. In our design, right-censoring arises due to outmigration and left-censoring due to eligibility for naturalization. The institutional conditions migrants face upon eligibility (in this case, a renunciation requirement or not) are difficult to anticipate and thus exogenous. However, the decision to leave the destination country is endogenous and likely associated with the propensity to naturalize. To explore potential selection bias associated with non-random right-censoring, we perform a robustness analysis in which we only analyze migrants who remain in the data for the entire observation window by applying the following settings (where ‘dataset_no_right_censoring’ is a sample including only migrants who, from their first observation onwards, remain in the dataset for the duration of the observation window):

```
att_gt(yname = "Y", tname = "t", idname = "ID_number", gname = "treat_group",
data = dataset_no_right_censoring, panel = TRUE, allow_unbalanced_panel
= TRUE)
```

Migrants may anticipate policy changes, which could affect their untreated outcomes (and by extension the ATT). To assess the extent to which this is the case, we perform a separate analysis where potential anticipatory behavior is explicitly modelled (by relaxing the default “no anticipation” assumption ([Callaway and Sant’Anna 2021: 204](#))) as follows:

```
att_gt(yname = "Y", tname = "t", idname = "ID_number", gname = "treat_group",
data = dataset, panel = TRUE, allow_unbalanced_panel = TRUE, anticipation
= 1)
```

SM6. Model summaries

country	model	estimate	std.error	units	obs	p.val.par.trends
SE	total	0.059	0.003	430494	2390664	0.999
SE	EU	0.072	0.002	206098	1162449	0.986
SE	non-EU (high HDI)	0.071	0.003	120287	677684	0.921
SE	non-EU (low-medium HDI)	0.047	0.003	104236	550531	0.976
SE	placebo (cannot renounce)	0.014	0.009	11826	64145	0.999
SE	wh citizen partner	0.061	0.001	209131	1114572	0.990
SE	w citizen partner	0.053	0.002	172330	971677	0.925

Table S8: ATT of the abolishment of the renunciation requirement in Sweden in 2001 on naturalization rates among foreign-born immigrants (excl asylum applicants), sub-samples by origin region of country of birth and by citizen status of partner. Placebo analysis based on total sample, with placebo treatment group those immigrants from origin countries that do not allow voluntary renunciation of citizenship. Estimates based on difference-in-differences specification for staggered treatment, using did package by Callaway and Sant’Anna (2022). All models with control group = never treated, estimation method = doubly robust, no anticipation, no covariates. Final column includes p-values for test of violation parallel trends assumption.

country	model	estimate	std.error	units	obs	p.val.par.trends
NL	total	-0.053	0.002	81208	464963	0.842
NL	EU	-0.081	0.003	27952	154519	0.758
NL	non-EU (high HDI)	-0.072	0.008	8031	44260	0.991
NL	non-EU (low-medium HDI)	-0.043	0.004	45225	266184	0.955
NL	placebo (citizen partner)	0.009	0.002	110854	619761	0.835
NL	placebo (cannot renounce)	0.054	0.002	67299	386921	0.002

Table S9: ATT of the reintroduction of the renunciation requirement in the Netherlands in 1997 in change of naturalization rates among foreign-born immigrants (excl asylum applicants) who do not have a Dutch citizen partner in any observation year, sub-samples by origin region of country of birth. Placebo (citizen partner) based on sample of immigrants who have a Dutch citizen partner in each observation year, with placebo treatment group those immigrants who do not lose, but can renounce citizenship of origin country; Placebo (cannot renounce) based on sample of immigrants who do not have a Dutch citizen partner in any observation year, with placebo treatment group those immigrants from origin countries that do not allow voluntary renunciation of citizenship. Estimates based on difference-in-differences specification for staggered treatment, using did package by Callaway and Sant’Anna (2022). All models with control group = never treated, estimation method = doubly robust, no anticipation, no covariates. Final column includes p-values for test of violation parallel trends assumption.

SM7. Group-time effects

Figure S5: The average treatment effect on the treated (ATT) of the abolishment of the renunciation requirement in Sweden in 2001 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants), by staggered treatment group.

Figure S6: The average treatment effect on the treated (ATT) of the reintroduction of the renunciation requirement in the Netherlands in 1997 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants) without a citizen partner, by treatment group.

SM8. Heterogeneous effects

Figure S7: The average treatment effect on the treated (ATT) of the abolishment of the renunciation requirement in Sweden in 2001 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants). Dynamic (pseudo-) ATT estimated with varying base period (changes from period $t-1$ to t), for total sample. Placebo analysis based on total sample, with placebo treatment group those immigrants from origin countries that do not allow voluntary renunciation of citizenship. Detailed output in [S6. Model summaries].

Figure S8: The average treatment effect on the treated (ATT) of the reintroduction of the renunciation requirement in the Netherlands in 1997 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants) who do not have a Dutch citizen partner in any observation year. Dynamic (pseudo-) ATT estimated with varying base period (changes from period $t-1$ to t), for total sample. Placebo analysis based on immigrants who have a Dutch citizen partner in each observation year, with placebo treatment group those immigrants who do not lose, but can renounce citizenship of origin country. Detailed output in [S6. Model summaries].

SM9. Robustness checks

Figure S9: The average treatment effect on the treated (ATT) of the abolishment of the renunciation requirement in Sweden in 2001 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants). First model reports overall ATT from main model; models 2-4 include selected time-invariant controls; model 5 based on sample excluding migrants who emigrate from Sweden before the end of the observation period; model 6 sets anticipation at 1 year before first treatment year.

Figure S10: The average treatment effect on the treated (ATT) of the re-introduction of the renunciation requirement in the Netherlands in 1997 in percentage point change of naturalization rates among foreign-born immigrants (excl asylum applicants) who do not have a Dutch citizen partner in any observation year. First model reports overall ATT from main model; models 2-4 include selected time-invariant controls; model 5 based on sample excluding migrants who emigrate from the Netherlands before the end of the observation period; model 6 sets anticipation at 1 year before first treatment year.