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Adriana Rocío Cardozo Silva[†], Christopher Prömel[‡]

Abstract

In this study, we contribute to the literature about the effects of improving access to citizenship on integration outcomes. Hereby, we exploit exogenous variation from two citizenship reforms in Germany to estimate the effects of residency requirements on perceived discrimination, which is strongly linked to individual well-being, sense of belonging, and migration desires and decisions. We find that reducing waiting times to become eligible for citizenship decreases perceptions of discrimination. However, heterogeneity analyses reveal that these effects appear to be mostly limited to men and immigrants from Eastern European countries. In addition to our main analysis, we exploit exogenous variation from EU enlargement to show that citizens from countries that became part of the EU report significantly less discrimination than non-EU immigrants.

Keywords: Perceived Discrimination, Legal Status, Naturalization, EU Enlargement

JEL: J15, J61, J68, J78, K37

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1. Introduction

As many industrialized societies face the prospects of demographic change with aging populations, increased shares of retirees, and, as a consequence, strained social security systems (Börsch-Supan et al., 2014), they increasingly rely on immigrants to fill open positions on the labor market. Yet after arrival, immigrants often struggle to fully participate in the economic, social, and political domains of their host country. Thereby, one often discussed factor holding them back are experiences and perceptions of discrimination.

Perceived discrimination, meaning the impression that one has been treated unfairly due to some personal characteristic or group membership (Kaiser and Major, 2006), has received little attention in economics to date. Instead, research has largely focused on studying the extent and impact of discrimination in the labor market and in social life using laboratory or field experiments (Riach and Rich, 2002; Neumark, 2018). Yet, there is an extensive literature on perceived discrimination in other disciplines like social psychology, ethnic studies, and public health, looking at its impact on various outcomes such as health, migration decisions, and trust (Pascoe and Richman, 2009; Röder and Mühlau, 2011; Di Saint Pierre et al., 2015).

In this study, we focus on the relationship between changes in access to citizenship and discrimination experienced by migrants. We do so in the context of Germany, a country with one of the oldest populations in the world, which is already substantially affected by the consequences of demographic change. Even though around a quarter of the people currently living in Germany are either foreign-born or the direct descendants of immigrants according to the German Federal Statistical Office, for a long time, the country, politically and culturally, has refused to be labeled as an "immigration country" or "*Einwanderungsland*" (Hell, 2005). Even as the country has increasingly opened up to immigration to tackle labor shortages, surveys indicate that the country's attractiveness to immigrants, particularly high-skilled ones, is not very high (Liebig and Ewald, 2023), as many of the newly arrived struggle to make German friends and feel left out (InterNations, 2023). In light of these dynamics, the German parliament has passed several reforms in recent years to raise Germany's attractiveness in the competition for global talent. Apart from making it easier for foreigners to come to Germany in the first place, a 2024 reform has also lowered residency requirements to acquire German citizenship.

As a number of researchers argue that improved access to citizenship can help to accelerate the integration of migrants (Hainmueller et al., 2017; Gathmann and Garbers, 2023), we want to analyze how reducing waiting periods for naturalization impacts perceived discrimination of immigrants in Germany, more specifically discrimination due to their ethnic background. Foreign nationals from non-EU countries face considerable legal and factual disadvantages on the

labor market in Germany due to not having a German passport, as they are precluded from entering certain jobs, are costlier to employ for firms due to administrative obligations, and may face statistical discrimination (Steinhardt, 2012). Moreover, they are less able to participate politically, may only enjoy restricted mobility (particularly when trying to travel abroad), and may encounter steeper barriers when trying to bring family members to Germany. We therefore test whether easing access to citizenship, which would help alleviate many of these legal disadvantages, leads to a decrease in feeling disadvantaged among immigrants.

To answer our research question, we use data from the German Socio-economic panel, an extensive longitudinal household survey which annually interviews over 25,000 individuals. Apart from providing information on respondents' nationalities, the dataset also asks first- and second-generation migrants about their experiences with discrimination due to their ethnic background.

For our main approach, we employ these data in the context of two reforms of German citizenship law in 1991 and 2000. These reforms provide us with exogenous variation in waiting times to become eligible for citizenship based on individuals' arrival year and age at arrival. We exploit this variation to estimate intent-to-treat (ITT) effects, similar to the approach in Gathmann and Keller (2018). More specifically, we estimate whether differences in the years required to reside in Germany in order to naturalize have an impact on perceived discrimination.

We find that, for the full sample, improved access to citizenship leads to a reduction in perceived discrimination. Estimates indicate that a reduction in waiting periods of seven years – which is the reduction brought about by the 2000 citizenship reform for most migrants – decreases experienced discrimination by 15 percent of the outcome mean. Moreover, we find that reducing waiting times particularly lowers experiences of frequent discrimination.

However, heterogeneity analyses uncover that the reforms did not affect all migrants the same way. Looking at region of origin, we find that a decrease in residency requirements of seven years reduces perceived discrimination among Eastern Europeans by around 30 percent of the outcome mean. In contrast, effects for Western and non-Europeans are insignificant. While Western migrants usually already enjoy many of the benefits that naturalization brings by being citizens of EU or EEA countries – and therefore may not see a reduction in perceived discrimination – , differences between Eastern European and non-European migrants are less clear. One possible explanation could be that the nature of discrimination experienced may differ between these groups, as non-European migrants are more likely to be the target of discrimination based on features like skin color or religion than Eastern Europeans (Booth et al., 2012). Therefore, naturalization may not bring the same benefits to non-Europeans as it provides only little cover against these types of discrimination.

Looking at the effects by gender, we find that a seven-year reduction in residency requirements reduces perceived discrimination among men by nearly 20 percent of the outcome mean. In contrast, perceptions of discrimination are unaffected for female migrants.

We test the robustness of our results by modifying our regression specification, extending or restricting our sample, and introducing additional covariates. Additionally, we test for selective panel attrition and outmigration. Overall, results remain robust to these checks.

As an alternative to our main approach, we evaluate the impact of an extension in rights and privileges for certain migrants by exploiting exogenous variation due to the three phases of EU expansion in 2004, 2007, and 2013. These events serve as quasi-natural experiments, as immigrants from EU accession countries in Germany started to benefit from additional rights and opportunities granted by EU law. Moreover, since these later waves of EU enlargement almost exclusively benefited citizens from Eastern European countries, it serves as an additional check of our previous results.

We estimate a staggered difference-in-differences model where we compare nationals from countries that became part of the EU with migrants from countries that have or had a plausible path to EU membership.[1] Even though the pool of treated individuals is small, the estimates still broadly corroborate our previous findings. We find that becoming an EU citizen reduces perceived discrimination by up to 20 percent of the outcome mean. Moreover, effects are particularly pronounced for men, which again is in line with our previous findings that men report less discrimination after naturalization than women. Effects remain significant and large even when we extend the control group.

As a last extension, we exploit variation in labor market access, which was not immediately granted to nationals of new EU countries, but took up to seven years for most. Estimating a similar staggered difference-in-differences model as before, we find broadly similar results as before. While the coefficients for the full sample are negative but insignificant, we find that granting equal access to the labor market significantly reduces perceived discrimination among men and frequent experiences of discrimination in the full sample.

Our study contributes to several strands of the literature. First, it contributes to the existent studies on the determinants of perceived discrimination. Even though it has been studied extensively in other disciplines like urban studies (Dill and Jirjahn, 2014), sociology (Diehl et al., 2021), ethnic studies (Yazdiha, 2019), and public health (Gil-González et al., 2013), the concept of perceived discrimination has thus far received only scant attention in economics. One notable

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[1]Hereby, we only include individuals without German nationality and whose nationality does not change over the observed time period.

exception is the recent study by Groeger et al. (2024), which examines perceived discrimination of Venezuelan immigrants in Peru.

Second, by studying perceived discrimination due to a person's ethnic background we add to the literature on ethnic and racial discrimination. The literature focusing on this type of discrimination is already very extensive (Riach and Rich, 2002; Rich, 2014; Bertrand and Duflo, 2017; Quillian et al., 2017; Neumark, 2018), studying not only its extent in various contexts and countries, but also examining whether the nature of discrimination is taste-based or statistical (Oreopoulos, 2011; Carlsson and Rooth, 2012; Zschirnt and Ruedin, 2016). However, because most of these studies rely on laboratory or field experiments, they tend to be relatively restrictive methodologically and often not fully representative. Studying perceived discrimination, on the other side, offers more flexibility, as it can be linked with survey and administrative data and examined both as an independent or dependent variable – provided that empirical researchers are aware of potential empirical pitfalls arising from endogeneity and selection.

Third, this study adds to the literature on the implications of legal status more broadly (Hall et al., 2010; Fasani, 2015; Mastrobuoni and Pinotti, 2015) and naturalization more specifically (Chiswick et al., 2009; Hainmueller et al., 2019; Govind, 2021; Gathmann and Garbers, 2023). While much research has examined the effects of naturalization on the labor market (Chiswick, 1978; Bratsberg et al., 2002; Devoretz and Pivnenko, 2005; Riphahn and Saif, 2019) and for social outcomes (Avitabile et al., 2013, 2014), by studying perceived discrimination, we add to this literature by focusing on a potentially intermediary factor, which helps explain social behaviors and the observed dynamics on the labor market.

The rest of this paper is structured as follows. In section 2, we describe perceived discrimination as a concept, lay out how and why it might be linked to legal status, and discuss its potential implications for other outcomes. Thereafter, we present our data and provide descriptive statistics in section 3. This is followed up by presenting the methodology and results of our main approach, where we exploit two German citizenship reforms, as well as robustness checks and heterogeneity analyses in section 4. In section 5, we provide an extension to our main results by studying the effects of EU enlargement. Section 6 concludes our study and discusses policy implications.

2. What is Perceived Discrimination?

2.1. Concept

The concept of perceived discrimination usually refers to self-reports of having been treated unfairly due to some personal characteristic or group membership (Kaiser and Major, 2006).

It captures whether individuals had any such experiences at all, but may also elicit how often people have faced such situations.[2] Perceived discrimination can be based on various personal characteristics. The most widely studied factors include gender, and – as in the case of this study – race or ethnic background (including related features like skin color, foreign names or accents). However, it can also extend to other aspects like age, religion, or sexual orientation (Almeida et al., 2009; Han and Richardson, 2015; Wu and Schimmele, 2021).

Importantly, perceived discrimination is not an objective or neutral measure of discrimination (Diehl et al., 2021), but depends on each affected individual; more specifically their experiences, how they interpret potentially discriminatory or otherwise negative situations, and how inclined they are to report them in an interview. In their study, Kaiser and Major (2006) describe how, in theory, perceived discrimination may under- or over-state actual discrimination. Under-reporting (also deemed minimization bias) may arise when affected individuals are not able to detect discrimination, e.g., because it is hidden or occurs in ambiguous circumstances, or when they deny its existence, e.g., to avoid psychological costs. In contrast, over-reporting (or vigilance bias) may result when individuals with a history of experiencing discrimination become more likely to attribute discrimination to ambiguous situations. Moreover, it may also occur if respondents blame negative events like job loss on discrimination to protect their self-worth. The authors do point out, however, that there is more evidence for under-reporting discrimination than over-reporting in the literature.

Figure 1 illustrates more broadly that discrimination and perceived discrimination are not necessarily directly linked. Rather, before being reported, potentially discriminatory events first have to be interpreted by each affected person, which determines whether individuals actually view events as discriminatory or not. How this interpretation actually plays out and which factors influence it has been the topic of many studies in social science research, particularly in the context of the so-called "integration paradox" (de Vroome et al., 2014; Steinmann, 2019; Schaeffer and Kas, 2023).[3] It describes the phenomenon often found in cross-sectional studies, whereby better integrated migrants appear to experience more discrimination than less well integrated migrants.[4] [5]

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[2]It can, however, also capture more general aspects, e.g., whether subjects believe themselves to be part of a discriminated group – thereby asking less about actual experienced discrimination and more about potential discrimination (Yazdiha, 2019).

[3]It is sometimes also called "skill paradox" (Dietz et al., 2015).

[4]This stands in contrast to the more conventional thinking along the assimilation theory, which posits that experiences of discrimination decline when migrants become better integrated (Gordon, 1964).

[5]There are several potential explanations for the integration paradox in the literature. First, as migrants become more integrated – with higher educational attainment, better language skills and more host-country specific knowledge – they may also become more able to discern discrimination, increasing reporting (Van Doorn et al., 2013; de Vroome et al., 2014). Second, better integration may make one more likely to ascribe ill intent to

Figure 1: Framework: Interpretation and Reporting of Discrimination
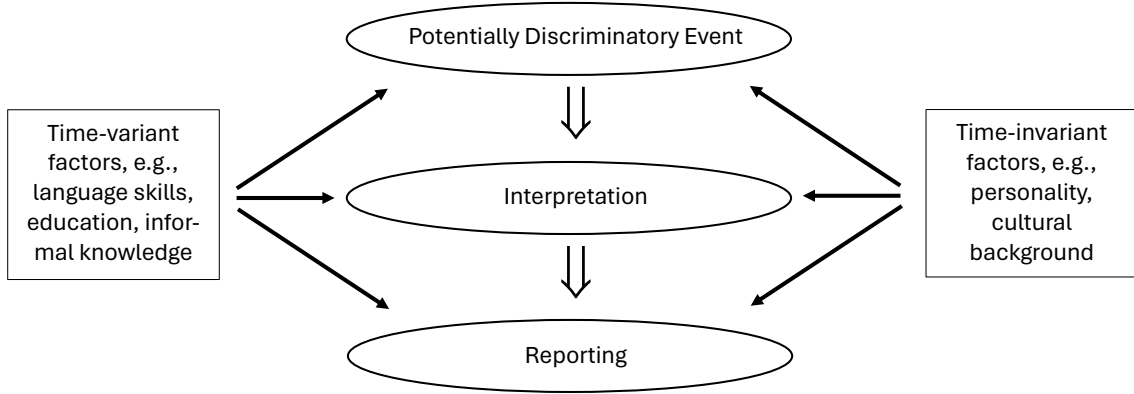
Note: This figure presents a simple graphical framework of the relation of experiences that were potentially discriminatory and the ensuing interpretation and reporting of these events in survey interviews. Own Illustration.

Hence, the interpretation of events depends on various time-variant characteristics like language skills, education, and host-country specific knowledge, but also time-constant factors like personality or cultural background. As Figure 1 implies, these time-variant and time-invariant factors affect not only the interpretation, but also the occurrence of potentially discriminatory events, as, e.g., people with darker skin or stronger accents may not only face more discrimination (Hersch, 2008), but may also interpret these situations differently (Gonlin, 2020). This illustrates that empirical researchers are faced with various problems of endogeneity when studying perceived discrimination.

However, studying perceived discrimination also has several advantages compared to other established approaches which examine discrimination more generally. While studies using field experiments (Bertrand and Mullainathan, 2004; Oreopoulos, 2011; Neumark, 2018) may give us a clearer idea of actual discrimination in general or in specific contexts, these approaches are generally not very flexible, being usually restricted to certain setups and circumstances. Moreover, while field experiments can to some extent help us understand the determinants of discrimination, both approaches usually can tell us only little about how discrimination affects other outcome variables, e.g., how it impacts well-being or labor market behavior in the long run. In contrast, using perceived discrimination as a variable that can be easily plugged into regressions as both an outcome or a determinant makes it very flexible. Furthermore, as individual and household surveys frequently capture this variable, a lot of data is already available and can be used in combination with many other control variables in empirical analyses.

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negative events (Diehl et al., 2021). Third, higher-qualified migrants may be the target of more discrimination on the labor market than lower-qualified ones, as they compete for more exclusive and contested positions in firms (Dietz et al., 2015; Auer et al., 2019).

In this section, we want to briefly lay out how perceived discrimination may generally be dependent upon migrants' legal status, focusing in particular on the effects of naturalization.

Before doing so, it is important to mention that the legal treatment of migrants in Germany is highly dependent on one's nationality. On one side, migrants from (most) EU countries already enjoy very similar rights compared to natives due to EU law (Tridimas, 2006).[6] For these migrants, naturalization usually only gives very limited legal advantages. On the other side, when looking at non-EU countries, conditions and opportunities for migrants can vary a lot depending on whether home countries have bilateral agreements with Germany or not (Steinhardt, 2012). Moreover, the type of residence permit also has an impact. Migrants with temporary residence permits usually face more restrictions, particularly in terms of mobility and on the labor market, but also higher uncertainty about their staying prospects. However, even immigrants with permanent residency face *de facto* legal disadvantages compared to natives.[7]

First, there are considerable constraints for migrants on the labor market. While some jobs in the civil service are limited to German citizens, e.g., in the judicial system or in certain public administrative positions, many other jobs – such as doctors or lawyers – are highly regulated, and require certain qualifications to perform (Gathmann and Garbers, 2023).[8] Moreover, employment chances of immigrants may also be reduced as hiring and employing foreign workers can be more expensive for employers (e.g., due to additional administrative work) (Steinhardt, 2012) and because migrants may become the target of statistical discrimination as employers only have incomplete information and may infer worker productivity based on wrong generalizations (Phelps, 1972; Hainmueller et al., 2019).

Second, migrants – especially those from poorer non-European countries – are often restricted in their mobility. While mobility within the EU is possible for migrants with a residence permit, stays in other EU countries are usually limited for up to 90 days. In addition, international travel can often be complicated, especially for migrants with "weak" passports, i.e., passports with which visa-free travel is only allowed for a small number of countries. In contrast, people with a German passport can travel to more than 180 countries visa-free. In addition, chances of

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[6]Similar privileges are also available for citizens of other EEA countries, i.e., Iceland, Liechtenstein, and Norway, outside the EU. Swiss citizens also benefit from certain additional privileges, but the legal setting is more complicated.

[7]Unfortunately, we cannot look further into the consequences of having a temporary or permanent residence, as our dataset does not provide any information on that.

[8]Although migrants may already bring qualifications from their home countries, the recognition of certificates and degrees is usually very time-consuming, cumbersome, and not too rarely unsuccessful (Jacobsen, 2021; Sommer, 2021). Therefore, many immigrants are forced to pursue non-regulated jobs that usually are less well-paid or to go back to school to acquire the necessary certificates (Nikolov and Goodarzi, 2022).

acquiring visas are also considerably higher (Satzewich, 2015), which in part is due to very high levels of trust towards Germany internationally. Different studies have shown that enhanced mobility is often a prime motivator for migrants to naturalize (e.g. Birkvad, 2019). Acquiring a "strong" passport like the German one makes it easier to travel for touristic or professional purposes but also to travel to one's home country, e.g., to visit friends and family or to attend events like weddings or funerals.

Third, non-Germans may also be less able to participate socially and politically. Access to public services or other social welfare programs may be curtailed, and it can be more difficult for them to bring family members to Germany. Moreover, it is harder to partake in political activities like joining parties,[9] their freedom of assembly is restricted, and, of course, they have neither active nor passive voting rights.

Lastly, there may be further disadvantages, which have been observed in the literature. E.g., there are various studies showing that non-nationals get sentenced more harshly (e.g. Light, 2016), and they may also experience disadvantages in school or university (e.g. Glock and Krolak-Schwerdt, 2013).

All of these factors can contribute to a sense of disadvantage that migrants may experience in the host country. Even though the mentioned examples of factual discrimination may not be based on characteristics like ethnicity or race, individuals may still interpret them to be due to their own origins. They might believe that their experiences would have been different if they were born in a different country. Yet, to what extent one may feel discriminated because of these rules likely depends on each individual and their experiences and expectations.

These disadvantages could be alleviated by acquiring German citizenship. While this process can be fairly lengthy and takes effort, naturalization provides migrants with equal rights compared to natives. It enables them to fully participate socially and politically in Germany, makes international travel easier, and lowers barriers to the labor market. We would therefore expect that migrants who naturalize report less discrimination than those who do not. In extension, we would expect that improving access to citizenship would do the same.

Lastly, one aspect that deserves mention, but which is hard to assess, is to what extent acquiring citizenship may also affect individual perceptions. On one side, people who naturalize may feel greater attachment to and feel more welcome in the host country (Fick, 2016), which, e.g., could make them less inclined to report discrimination. On the other side, people's expectations may shift after naturalization, which might make people more sensitive towards potentially

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[9]While most parties allow non-citizens to join, this is not the case for some, e.g., the CDU, which just allows EU citizens to become a member.

discriminatory situations or other transgressions. While such a change in perceptions is possible, it is unclear how it would affect our results. While there are various studies on the determinants of naturalization (Zimmermann et al., 2009), including ones looking at the motivations of people through interviews (Birkvad, 2019), there is little existing research examining how the process of naturalizing itself changes individual perceptions.

2.3. Implications

Before we start looking more closely at the impact of legal status on perceived discrimination, it may be worthwhile to first take a step back and examine the potential impact perceived discrimination may have on other outcomes.

There have been a number of studies from various fields looking at the implications of perceived discrimination. First, there is a broad literature on its health effects, finding a detrimental impact of perceived discrimination on both physical and mental health (Pascoe and Richman, 2009; Schmitt et al., 2014; Szaflarski and Bauldry, 2019). Moreover, some studies have shown that experiences of discrimination are a strong driver of return intentions and actual outmigration (Di Saint Pierre et al., 2015; Kunuroglu et al., 2018; Yilmaz Sener, 2019). Further research has looked at the impact of perceived discrimination on other outcomes like national identification and ethnic identity (Martinovic and Verkuyten, 2012; De Vroome et al., 2014), political engagement (Fischer-Neumann, 2014), and trust in public institutions (Röder and Mühlau, 2011). These studies usually find negative effects on host country identification,[10] while the impacts on institutional trust and political interest are more nuanced, and depend on factors like ethnicity, ethnic identity, and whether migrants are born abroad or in Germany.

Accompanying the main estimations of this study, we add to the existing research by running a quantitative analysis on the implications of perceived discrimination using data described in section 3. This approach is explained in greater detail in Appendix B. We first run simple OLS regressions of various outcomes on perceived discrimination, while employing year and state of residence fixed effects, and controlling for a host of time-varying individual characteristics (listed in Table A.2). Results of these regressions indicate a strong link of perceived discrimination and staying intentions as well as observed migration. Figure 2 reveals that perceived discrimination is negatively related to wanting to stay in Germany long-term and shows that respondents with higher perceived discrimination are more likely to leave Germany and to drop out of the dataset.

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[10]An additional study by Prömel (2023) showed that perceived discrimination can also act as a mediator between migration inflows and migrants' host and home country attachment. Looking at the impact of refugee inflows on other migrants' identity outcomes, the study, among other things, shows that people who perceive discrimination feel less belonging to Germany in regions with higher refugee inmigration. The author argues that this is most likely due to increased concerns about nativist backlash.

Figure 2: Implications of Perceived Discrimination on Staying Intentions, Observed Migration, and Attrition
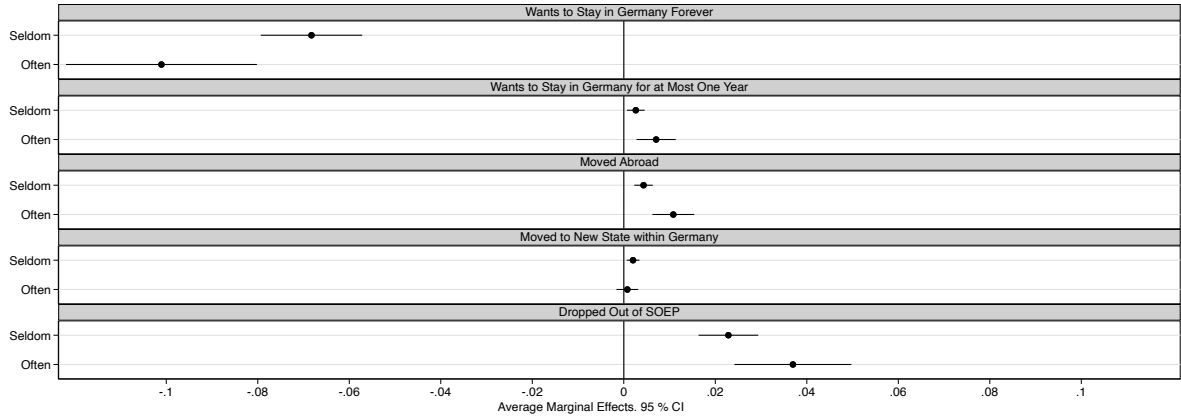
Note: This graph shows coefficients of OLS regressions (with 95% confidence intervals), with various outcomes regressed on the expressions of perceived discrimination. Outcome variables are binary. Regressions include all controls in Table A.2.

Effects are usually larger for respondents who report more discrimination. Hence, perceived discrimination appears to drive migrants out of Germany.

To examine the robustness of these relations, we extend our model to include individual fixed effects, which eliminate all time-constant heterogeneity – thereby largely accounting for people's personality and inclination to report perceived discrimination. This drastically reduces but not fully eliminates the potential bias in our estimations, as endogeneity due to omitted variable bias and, in some cases, reverse causality cannot be fully ruled out.

Overall, coefficients – while decreasing in size – remain significant and large (see columns (6)-(10) in Table B.1). Moreover, results also show that perceived discrimination is negatively linked with other outcomes including individual well-being, and mental health. Lastly, we also find significant effects on political preferences, with increases in political interest and a higher likelihood to prefer left-wing parties in Germany.

While we do not claim these results to be causal, our findings nevertheless appear remarkably robust to the inclusion of further control variables (Table B.2). Thus, our estimates show that perceived discrimination is negatively linked to migrants' well-being and mental health, and, moreover, may also be detrimental to the German economy, as migrants who report discrimination also seem more likely to leave Germany.

3. Data & Descriptives

For our analyses, we use data from the German socio-economic panel (SOEP, Goebel et al. 2019). This longitudinal household survey interviews over 25,000 respondents annually, capturing a wide range of social, economic, and demographic characteristics as well as attitudes and opinions. The dataset allows us not only to identify individuals with a migrant background but

Figure 3: Distribution of Perceived Discrimination By Origin Groups
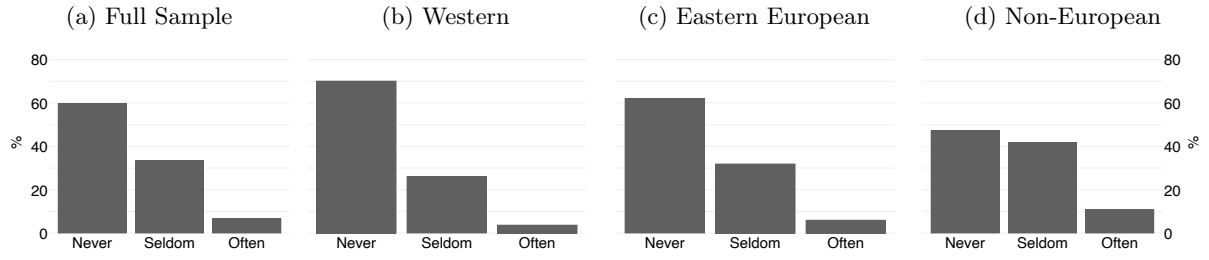
Note: The figure presents the distribution of response options regarding perceived discrimination averaged over the observed time period from 1996 to 2017 for the full sample (a) and sub-samples by region of origin (b-d).

also provides detailed information on people's country of birth and their nationality over time, allowing us to determine whether and when they acquire German citizenship. Moreover, it also asks respondents with a migrant background (1st or 2nd generation): "How often in the last two years have you felt discriminated against here in Germany because of your ethnic origins?".[11] Response options are "never", "seldom", and "frequently". With this exact phrasing, the question was surveyed annually from 1996 to 2011 and every two years between 2011 and 2017.[12] In total, the datasets consists of 57,954 observations, i.e., interviews in which respondents' perceived discrimination was elicited.[13] Table A.1 shows descriptive statistics of perceived discrimination over time.

Figure 3 shows the distribution of responses averaged over time for the full sample (a) and for subsets based on region of origin (b-d). Western migrants include respondents from Western Europe (e.g., France, Greece, Italy) or non-European "Western" countries (e.g., United States, Australia). Eastern Europeans include respondents from the former Warsaw Pact countries (e.g., Poland), post-Soviet nations (e.g., Russia, Kazakhstan), and from the Western Balkans (e.g., Serbia). Non-Europeans are respondents from non-European, non-Western states which includes migrants from Turkey as well as those from the MENA region, Latin America, East Asia, and (Sub-Saharan) Africa.[14] Table A.3 reveals which region encompasses which countries. In Figure 3 (a), we see that around 60 percent of respondents reported to have never experienced discrimination in the previous two years, while less than 10 percent said to have felt disadvantaged often. Thus, while it is unclear what constitutes notable discrimination in the eyes of respondents,

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[11]Hereby, we refer to the phrasing in the English questionnaires. In German, the term *Herkunft* is used, which is not necessarily congruent with ethnic origin, but can also describe more generally where someone comes from, referring to a location.

[12]In the following waves, the question on perceived discrimination was rephrased and response options were modified (frequently, sometimes, rarely, never). For consistency we therefore include only data until 2017.

[13]In this sample, we do not include the specific refugee sample in the SOEP (Brücker et al., 2017), which surveys refugees who came to Germany mostly between 2013 and 2016. Cardozo Silva et al. (2022) look at the perceived discrimination among refugees in Germany, showing that it increased during the coronavirus pandemic compared to the years before.

[14]Figure A.1 shows the distribution of perceived discrimination for smaller subsets of origin region and by gender.

Figure 4: Time Trend of Perceived Discrimination By Origin Groups

(a) Full Sample   (b) Western

(c) Eastern European   (d) Non-European
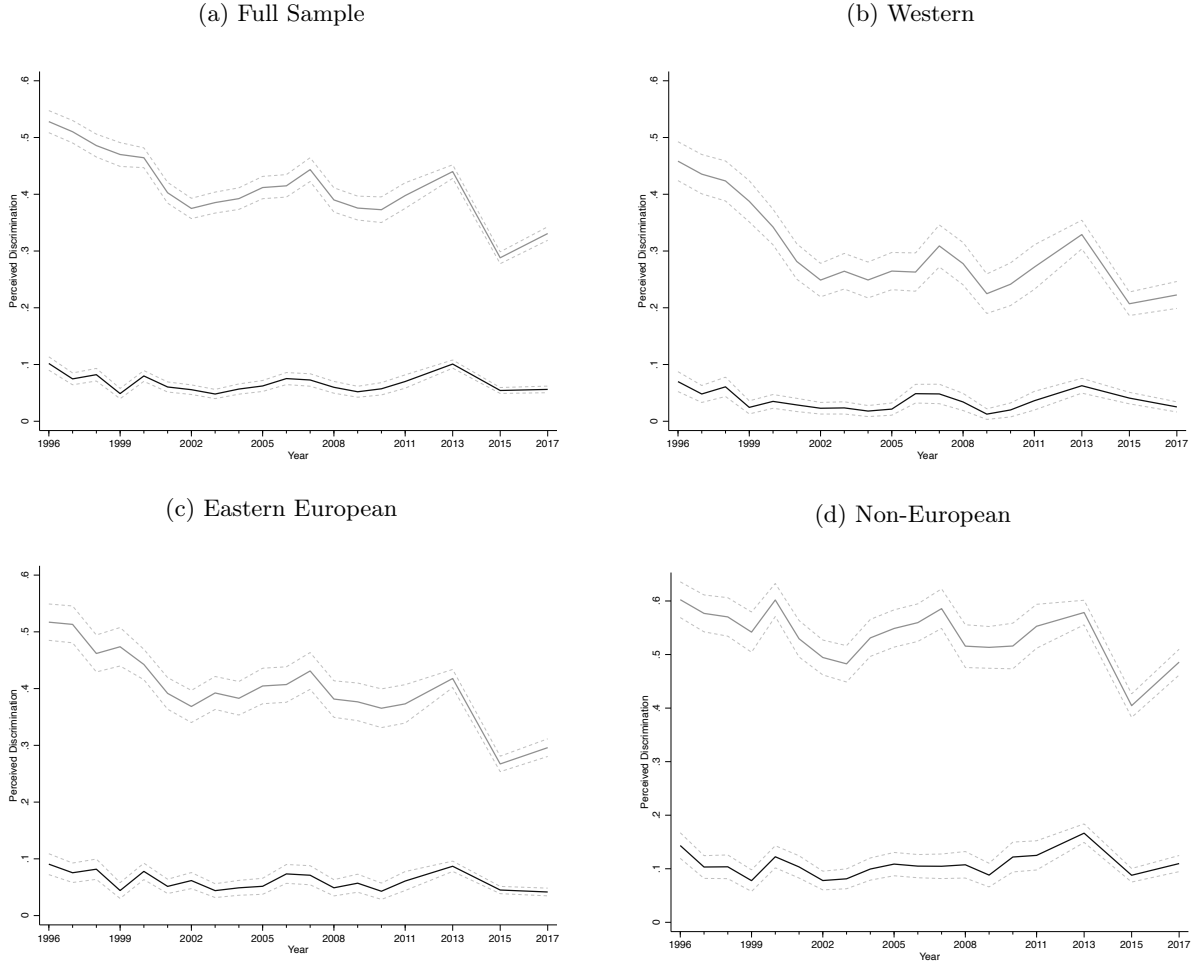
Note: This figure presents time trends of perceived discrimination including 95 percent confidence intervals for the full sample (a) and sub-samples by region of origin (b-d). Grey line: Has experienced discrimination at least sometimes. Black line: Has experienced discrimination often.

a clear majority of migrants in the sample reported to have faced no discrimination. However, there are strong differences between migrant groups. While Western migrants were the least likely to report discrimination, more than 50 percent of non-Europeans reported at least some discrimination with Eastern Europeans in-between.

Looking at perceived discrimination over time in Figure 4, it shows that reported discrimination has declined over the years for the sample as a whole, even though decreases in frequent discrimination are small. In 1996, a majority of respondents said that they had faced at least some discrimination, while in 2017, the share was around 35 percent. Looking at the subgroups, we see that there were considerable decreases in perceived discrimination over time among Western migrants and Eastern Europeans. In contrast, dynamics among non-Europeans were basically flat until 2013, with a sudden decrease in 2015 and an uptick in 2017.

Making use of the information on respondents' nationalities, we can see the share of migrants

Figure 5: Share of Migrants with German Citizenship By Origin Groups
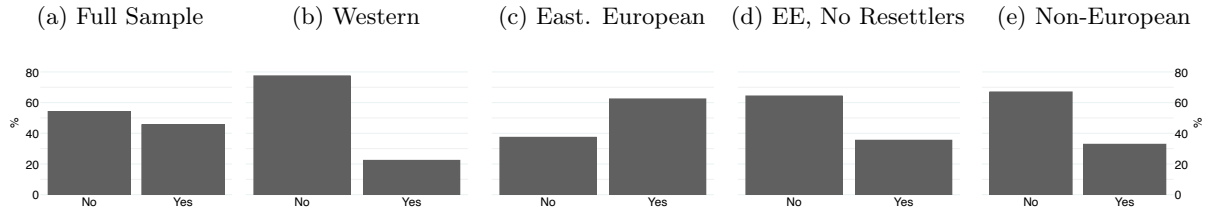
(a) Full Sample   (b) Western   (c) East. European   (d) EE, No Resettlers   (e) Non-European

Note: The figure displays the share of migrants with German citizenship averaged over the observed time period from 1996 to 2017 for the full sample (a) and sub-samples by region of origin (b-e).

with German citizenship in Figure 5.[15] While in the full sample, around 45 percent of migrants are German, there are considerable heterogeneities by origin group. Western migrants rarely hold German citizenship (around 20 percent) while the share is much larger for Eastern Europeans (around 60 percent). Even when we remove resettlers – a group of ethnic Germans who arrived in Germany particularly after the Fall of the Berlin Wall from Eastern European countries like Russia, Poland, or Romania – the share is at 35 percent. Non-Europeans are at over 30 percent.[16]

In addition to the variables on people's nationality and their perceived discrimination, the dataset also offers very broad and extensive information on individual and household characteristics, which we use as control variables. Apart from information on the exact dates of interviews and respondents' state of residence, the dataset includes variables on demographic (e.g., age, gender, region of origin), social (e.g., marriage status, number of children), educational (e.g., type of school degree), economic (e.g., labor income, employment status), and health characteristics. Descriptive statistics of these variables are provided in Table A.2 for the full sample and for selected samples constructed for the empirical analysis in the following sections.[17] In the full sample, a little more than half of respondents are women, the average age is just under 42 years and those respondents born abroad were living in Germany for on average about 20 years. Around a quarter of individuals are from Turkey and MENA countries and Western countries each, while around a third have roots in Eastern Europe and one eighth in the Western Balkans. Only minor shares are from Latin America, Sub-Saharan Africa, and East Asia. The vast majority of migrants live in Western German states with nearly half of them living in only two states, Northrhine-Westphalia and Baden-Württemberg. Less than three percent reside in the former East German states (outside of Berlin). More than 80 percent of migrants live in urban areas,

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[15]The sample used for these descriptive statistics is based on respondents between 1996 and 2017, who in at least one year responded to the question on perceived discrimination. The sample is larger than for the figures before because nationality was more often elicited than perceived discrimination.

[16]Figure A.3 shows the shares of naturalized migrants in our sample for smaller subsets of origin region and by gender.

[17]In order to not lose too many observations due to missing values of control variables, we recoded missing values as zero and included additional dummy variables into our regressions, which indicate whether values were missing.

two thirds are married, they have a little less than one child on average, more than 80 percent are either medium- or high-skilled, and nearly 60 percent have some kind of work (mostly full-time or part-time), less than ten percent are unemployed, and around eight percent attend schooling, training, or university.

4. Main Approach: Exploiting Variation from Citizenship Reforms

In this section, we make use of exogenous variation arising from two citizenship reforms in Germany to estimate the causal effect of residency requirements on perceived discrimination. This approach is similar to the one used in Gathmann and Keller (2018). As waiting periods are strongly linked to the likelihood of naturalizing, we are able to determine whether easing or restricting access to citizenship has an effect on perceived discrimination.

4.1. Citizenship Reforms

Our approach makes use of two different citizenship reforms, the first in 1991, and the second in 2000. Before 1991, citizenship in Germany was generally based on ancestry (*jus sanguinis*). This means that migrants without German ancestors had no entitlement to become German even if they had been living in Germany for many years, were without criminal conviction, and economically self-sufficient. Instead, citizenship could be granted through discretionary decisions by public authorities. However, applications could also be denied. This legal setting had the consequence that the total annual numbers of naturalizations were generally very low in Germany, not exceeding 20,000 per year (excluding ethnic Germans) before 1990 (see Gathmann and Keller, 2018).

This was changed with the passage of the Alien Act (*Ausländergesetz* (AuslG)), which was enacted on 1 January 1991. The reform established clear and explicit criteria to acquire German citizenship for non-ethnic Germans, removing discretionary leeway. Among other criteria,[18] the law established minimum waiting periods for migrants based on their age at arrival. Migrants who arrived in Germany, when they were seven years or younger, had to wait until they were 16 years old to acquire German citizenship. Those, who were between 8 and 14 years at arrival

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[18]There are several other criteria defined in the law: Migrants had to give up their previous citizenship upon naturalization. There were some exceptions to this: E.g., citizens from other EU countries or countries where renunciation of citizenship was not possible were allowed to keep their old citizenship. Moreover, they had to have no prior criminal convictions, could demonstrate their economic self-sufficiency (for older immigrants, i.e., those who arrived at age 15 or older) – meaning that they were able to provide for themselves and dependent family members without having to rely on welfare benefits or unemployment assistance –, had completed a minimum number of years of schooling in Germany (for younger immigrants), and declared their loyalty to the German constitution.

Table 1: Residency Requirements among Different Migrant Groups

Group	Age of arrival in Germany	Residency requirement for citizenship	Access to citizenship at age	% in the sample
Child immigrant	Ages 0–7	9–16 years (possibly longer for arrival cohorts 1975–82)	Age 16 (older for arrival cohorts 1975–82)	21.81
Younger immigrant	Ages 8–14	8 years (9–15 years for arrival cohorts 1975–82)	Ages 16–22 (older for arrival cohorts 1975–82)	25.35
Older immigrant	Ages 15–22	15 years (9–14 years for arrival cohorts 1986–91) 8 years (arrival cohorts 1992–2000)	Ages 30–38 (younger for arrival cohorts 1986–91) Ages 23–30 (arrival cohorts 1992–2000)	52.84

Note: Table from Gathmann & Keller (2018) which describes variation in waiting times by arrival cohort and age at arrival. Share in sample based on own calculations using SOEP data.

had to reside in Germany for eight years, while the residency requirement for older migrants (15 years or older) was 15 years.

These criteria were amended through the passage of the Citizenship Act (*Staatsangehörigkeits-gesetz* (StAG)), which was enacted on 1 January 2000. Apart from adding language requirements, the act reduced the residency requirements for migrants who were 15 or older at arrival to eight years, while keeping all other criteria in place.[19]

The two reforms led to variations in waiting times along two dimensions. First, the laws set up different waiting times by age at arrival. Migrants, who arrived in Germany when they were between zero and seven years old had to wait until they were 16 years old – or in other words, between nine and 16 years. Migrants who were between eight and 14 years old at arrival all had to wait eight years, while older immigrants had to wait 15 years (eight years since 2000). Second, migrants had different waiting times based on the timing of the reforms in combination with their arrival years. For instance, migrants who arrived in 1975 had to wait 16 years to naturalize regardless of age, as the reform was passed and enacted 16 years later, while waiting times were shorter for younger migrants of later cohorts. Moreover, there is additional variation because of the 2000 reform. Older immigrants (i.e., those who arrived at age 15 or older) who arrived in the years between 1986 and 1991 had to wait 9 to 14 years depending on the exact arrival date. This variation is summarized in Table 1.[20] It also shows that just over one half of our sample (which is described in more detail further down below in section 4.2) consists of older immigrants who arrived when they were 15 or older, while the rest were younger at arrival.

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[19]Moreover, the reform made it possible for children born in Germany to foreign parents to attain German citizenship if at least one parent was had been living legally in Germany for at least eight years and had a permanent residence for at least three years.

[20]The table is taken from the original paper by Gathmann and Keller (2018). The last column has different values than in the original table as the data source and the sample we use is different.

Figure 6: Distribution of Waiting Times
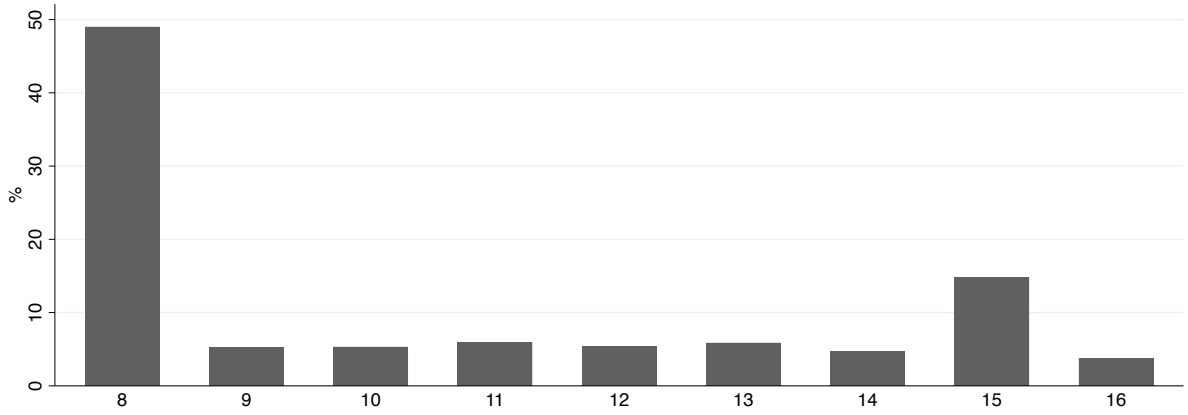
Note: Figure displays the distribution of how many years foreign citizens had to reside in Germany to be eligible to naturalize in our sample.

Figure 6 additionally presents the distribution of waiting times to be eligible to naturalize. While nearly 50 percent of the sample had to wait for only eight years, around 15 percent had to wait 15 years. The rest of the sample is spread relatively evenly among the other time periods.

4.2. Sample Selection and Empirical Methodology

For our estimation approach to work, we need to perform certain sample restrictions, which are similar to those performed in Gathmann and Keller (2018). First, we only study migrants who were born abroad and then arrived later in Germany – thereby excluding second-generation migrants. Second, we remove ethnic Germans from the sample, as there were different criteria for them to acquire German citizenship. Third, we only look at migrants who arrived in Germany between the years 1975 and 2009 – meaning those cohorts most affected by the reform – and who became eligible for citizenship between 1991 and 2017. The latter part implies that migrants who became German before 1991 are also excluded, as they were not affected by the reforms. Fourth, to have a more homogeneous and comparable sample, we also exclude migrants who were older than 22 at arrival. Fifth, we make sure that respondents resided in Germany for at least two years at the time of the interview.

For our estimations, we use the survey waves from 2002 until 2017. Although, as outlined above, perceived discrimination was already captured in the years before, we want to make sure that respondents were already fully affected by the reforms at the time of the interview, limiting our sample to interviews conducted in the year 2000 or later. Moreover, as the question asks about experiences of discrimination in the prior two years, we add two years to our cutoff.[21]

Overall, this leaves us with a sample of 2,120 migrants and a total of 10,687 observations

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[21]However, as a robustness check, we show results when we use different cutoff years.

for this period in our data set. However, as only 1,244 migrants answered the question about perceived discrimination at least once, the sample size for our main analysis is 6,758 observations.

We then estimate the following model, which is in line with the one by Gathmann and Keller (2018):

$$Y_{iabt} = \beta Wait_{ab} + \lambda D(B_b) + \mu D(Coh_a) + \nu_t + \gamma_1 YSM_{at}$$
$$+ \gamma_2 YSM_{at}^2 + \pi_1 Age_{bt} + \pi_2 Age_{bt}^2 + \delta' X_{it} + \epsilon_{iabt} \tag{1}$$

The outcome variable Y_{iabt} is perceived discrimination of migrant i, who was born in year b, arrived in Germany in year a, and was interviewed in year t.[22] The main explanatory variable is $Wait_{ab}$, the years a person has to wait until becoming eligible to acquire German citizenship, which depends on the year of arrival and the year of birth. As the relationship between waiting periods and perceived discrimination might be influenced by various other factors, we include a wide set of controls. First, we include year of birth fixed effects $D(B_b)$ and cohort of arrival fixed effects $D(Coh_a)$ to control for potential differences in the likelihood and inclination to report discrimination among different birth and arrival cohorts. We also include year fixed effects ν_t to control for macro changes affecting all migrants, which may change respondents' likelihood to report discrimination. Moreover, we include years since arrival (YSM_{at}) and age (Age_{bt}) and their quadratic terms in the regression to control for the effects of assimilation and aging. Lastly, the model also includes several further control variables (X_{it}), namely gender, region of origin dummies,[23] state fixed effects and state-specific time trends. Thereby, we are able to control for differences in terms of gender – as men and women may be differently affected – and origin – as respondents from different origin countries or regions may face and process discrimination differently. Moreover, state fixed effects and state-specific time trends capture differences by state of residence and changes over time within these states. Lastly, standard errors are clustered by age times year of arrival.

4.3. Main Results

The results of estimating equation 1 can be seen in Table 2. In column (1), we first test whether waiting periods affect respondents' probability to naturalize – which essentially serves

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[22]In our main estimations, we make use of the full range of response options, treating the outcome as if it was cardinally scaled, but we also provide results where we use binary dependent variables.

[23]For that, we construct several dummy variables for the following regions: Latin American, East Asia, Africa, Turkey and the MENA region, Western countries, Eastern Europe (excl. Western Balkans countries), and Western Balkans countries. For the categorization, we use various information based on the following characteristics: country of origin, first and second nationality, past nationality, and the country of origin and nationality of the respondents' parents. Hereby, we allow respondents to have multiple regions of origins: E.g., a respondent with a French father and a Polish mother would be classified as both Western and Eastern European. We additionally create dummies indicating whether a respondent is a recognized refugee, and whether they come from a country that is part of the EU at the time of the interview.

Table 2: ITT Effects of Citizenship Reforms: Main

	Naturalized	Perceived Discrimination		
	(1)	(2)	(3)	(4)
Residency Req.	-0.0167***	0.0113*	0.0055	0.0057**
	(0.0030)	(0.0059)	(0.0046)	(0.0024)
Cohort of Arrival FE	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes
Mean	0.306	0.535	0.453	0.083
N	10687	6758	6758	6758

Note: The table reports results estimating equation 1. Dependent variable is: Column (1): Whether respondent naturalized (0/1). (2): Perceived discrimination as a continuous variable. (3): PD as a binary variable, with cutoff between "Never" and "Seldom". (4): PD as a binary variable, with cutoff between "Seldom" and "Frequent". Standard errors are clustered by age times year of arrival. * $p < .10$, ** $p < .05$, *** $p < .01$

as a first stage. Therefore, instead of using perceived discrimination our dependent variable is whether a person is naturalized in year t.[24] The estimate in column (1) indicates that increasing one's waiting time by one year decreases the person's likelihood to naturalize by 1.67 percentage points.[25] This means that a reduction in residency requirements of seven years – the reduction brought about by the 2000 reform – increases one's probability to naturalize by around 11.7 percentage points.

Column (2), (3), and (4) display the reduced-form estimates of residency requirements on perceived discrimination. In column (2), we make use of the whole spectrum of response options in our outcome variable and treat it as if it is was cardinally scaled. In the other two columns, we construct binary dependent variables, where we split the outcome: In column (3), the split is by whether respondents have experienced discrimination sometimes or frequently (=1) or never (=0). In column (4), the split is between whether they have experienced discrimination frequently (=1) or sometimes or never (=0).

The coefficient in column (2) is positive at 0.0113 and (weakly) significant, indicating that each additional year a person has to wait longer increases perceived discrimination by 2.11 percent of the mean. Phrased differently, reducing the waiting period by seven years decreases perceived discrimination by nearly 15 percent of the mean ($0.0113 \cdot 7 \div 0.531 = 0.149$).

Looking at the estimates with binary outcome variables, it seems that reducing residency requirements lowers the likelihood of reporting frequent but not infrequent discrimination as only

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[24]The samples are different because perceived discrimination was not surveyed in 2012, 2014, 2016, and 2017.

[25]This is a little larger than the effect in Gathmann and Keller (2018), who estimate a coefficient of 0.013. One reason could be that Gathmann and Keller (2018) only look at results until 2010, while our sample includes years until 2017, so that effects have more time to realize.

the coefficient in column (4) is significant. The coefficient indicates that reducing the waiting period by seven years decreases frequent perceived discrimination by around 4 percentage points. Thus, it seems that lowering residency requirements has a dampening effect on strong perceptions of discrimination.

4.4. Robustness Checks

To evaluate the robustness of our main findings, we perform a number of additional checks. In all tables, results are provided for both the continuous treatment variable (always Panel A) and the two binary outcome variables (Panels B and C).

First, we make various alterations to our regression model. In Table A.4 (in the appendix), we modify the control variables used in our main model with column (1) showing the baseline estimation results from Table 2. In column (2), instead of using year of arrival and year of birth fixed effects, we assign respondents to larger groups with five-year intervals for year of arrival and year of birth.[26] In column (3), we use country of origin dummies as originally employed in the study by Gathmann and Keller (2018). In column (4) we only use an East-West dummy instead of state fixed effects, while in column (5), we include state fixed effects, but treat the former East German states as if they were just one state – because the number of migrants in East Germany is very small (around 3 percent in our sample).[27] Lastly, in column (6), we use state-year fixed effects instead of linear trends. Across specifications and in all three panels, our main coefficient remains fairly stable, never deviating strongly from the baseline estimate in terms of size or significance. Then, in Table A.5, we test whether removing or adding (up to fourth order) polynomials for age and years since arrival change our results. In all specifications, results remain very robust to these modifications.

Second, we include additional control variables to evaluate whether our estimates are sensitive to the inclusion of potential confounders. In Table A.6 (in the appendix), various individual and regional characteristics are added. Column (1), again, shows baseline coefficients. Then, we separately add information on respondents' educational attainment (2), whether they live in an urban area (3), marriage status and number of children (4), language proficiency (5), personal labor income and labor market status (6), and health status (7), and state GDP per capita and unemployment rate (8). Column (9) shows results with all additional controls. Estimates in all three panels show that the coefficient of interest changes only little when introducing these additional control variables. Thereafter, we evaluate whether results change after including

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[26]E.g., a person born in 1972 is put into the bracket of those born between 1971 and 1975.

[27]In both cases, we adjust the state-specific time trends. In (4), we include an interaction of the East-West dummy and year, while in (5), we use state-specific time trends where we treat all former East German states as one state.

parental characteristics in our regression in Table A.7. More specifically, we look at the mother's and father's highest school diploma, their vocational education, and their job status when the respondent was 15 years old. Even though some of the characteristics may have changed after arrival in Germany, they may still give us a good overview of the socio-economic background of the respondent at arrival. Looking at the results, we see that our results remain robust to the inclusion of parental characteristics.

Third, we test for selective panel attrition and outmigration. Results could be biased if differences in residency requirements also impact respondents' likelihood to leave the panel or Germany. For our tests, we use basically the same approach as before, estimating equation 1, but we use a different outcome variable. In Table A.8, in columns (1) to (3), the outcome variable is whether a respondent has left the panel, while in columns (4) to (6), it is whether they have left Germany. In columns (1) and (4), our main explanatory variable is whether a person eventually becomes German. In columns (2) and (5), it is whether a person is eligible for naturalization, and in columns (3) and (6), it is the residency requirement. Coefficients are negative and significant in (1) and (4), indicating that people who naturalize at some point are less likely to leave the panel and to move abroad, which is expected. However, more importantly, results are insignificant in all other columns, indicating that our main approach is likely not affected by selective panel attrition or outmigration.

Lastly, we check whether our results are sensitive to our sample selection. In a first step, we evaluate whether our results are affected by our choice of the cutoff year. As described previously, our estimations include the survey waves starting in 2002, as we want respondents to be affected by both reforms and to account for the phrasing of the survey question on perceived discrimination. Estimates in Table A.9 in the appendix reveal that this indeed changes estimates a little bit, but these differences confirm our expectations. In Panel A, when the cutoff is moved to 2000 (column 2) or 2001 (column 3), the coefficients become smaller and less significant which is in line with the notion that the reform may not have fully affected respondents yet. In contrast, the estimate in column (4) is very similar compared to the baseline, but slightly less precise because of the reduced sample. Looking at Panels B and C, results remain in line with previous results. Thereafter, we make further changes to our sample in Table A.10. First, in column (2), we test whether results are affected by the nationality of respondents' parents. Children of at least one German parent who was born in Germany are automatically eligible for German citizenship. Unfortunately, our dataset gives us only limited information on the nationality of respondents' parents. Therefore we proxy nationality with country of birth. While this approach is rather imprecise, we see that results remain very similar. In columns (3) and

(4), we check whether results differ when we exclude respondents with a German spouse, as this also gives people easier access to German citizenship. Because German spouses are not always easily identifiable in the dataset, we use two different approaches. First, in column (3), we identify and then exclude respondents who were married to a German at any point between 1990 and 2015.[28] Then, in column (4), we remove individuals who were married to a German before they were eligible to naturalize, i.e., before they fulfilled their residency requirements. In both cases, coefficients become larger and more significant, indicating that our baseline estimates may under-estimate the effects of waiting periods. In the following two columns, we test whether results might be impacted by selective in-migration due to the reforms in 1991 and 2000. In column (5), we drop all respondents who moved to Germany in 2000 or later. In column (6), we drop all who arrived after 1990. Again, results are a little larger than baseline estimates but similar in significance. Lastly, to check for differences in experiences in East and West Germany, we remove all respondents residing in East German states. Results in column (7) are virtually the same as in column (1).

Hence, our results appear to be fairly robust to changes in the regression model, additional confounders, possible misgivings about selective panel attrition, and the sample selection.

4.5. Heterogeneity Analysis

In this section, we check for potential heterogeneities by gender and region of origin. Therefore, we conduct subsample regressions in Table 3. The results of the first stage are displayed in Panel A. Panels B to D report the estimates for the three specifications of perceived discrimination.

Looking at effects by gender, we can see that estimates in Panel A are fairly similar, indicating that waiting periods affect both men and women similarly in terms of their naturalization decisions. However, results in Panel B show that the coefficient for the effect of residency requirements is positive and significant only for men. Going further, when we look at the binary outcomes in Panel C and D, results reveal that waiting periods only have a significant effect on reports of frequent discrimination for men. The estimate suggests that for every year a man had to wait less to naturalize, perceived discrimination is lowered it by 1.2 percentage points. In contrast, the coefficient for women is always insignificant regardless of which outcome variable we use. This result is interesting insofar as Gathmann and Keller (2018) found that that quicker access to citizenship mostly benefited women and not men on the labor market. Thus, if we were to assume that our effects were substantially driven by improvements on the labor

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[28]We use 2015 instead of 2017 as the cutoff as people need to be married for at least two years to apply for citizenship.

Table 3: ITT Effects of Citizenship Reforms: Heterogeneity

	(1) Men	(2) Women	(3) West	(4) Eastern Europe (incl. Balkan)	(5) Non-Western Non-Europe
Panel A: Naturalization Decision					
Residency Req.	-0.0131***	-0.0116***	-0.0086**	-0.0194***	-0.0146***
	(0.0044)	(0.0039)	(0.0034)	(0.0048)	(0.0047)
Mean	0.325	0.292	0.049	0.330	0.372
N	4641	6046	1825	4222	4999
Panel B: Continuous Dependent Variable					
Residency Req.	0.0183**	0.0045	0.0137	0.0214**	0.0030
	(0.0089)	(0.0079)	(0.0134)	(0.0107)	(0.0094)
Mean	0.563	0.514	0.328	0.484	0.656
Panel C: Binary Outcome with Cutoff between No PD and Some PD					
Residency Req.	0.0061	0.0041	0.0114	0.0160**	-0.0034
	(0.0068)	(0.0062)	(0.0105)	(0.0081)	(0.0067)
Mean	0.471	0.438	0.282	0.409	0.547
Panel D: Binary Outcome with Cutoff between Some PD and Frequent PD					
Residency Req.	0.0122***	0.0004	0.0023	0.0054	0.0064
	(0.0040)	(0.0035)	(0.0055)	(0.0044)	(0.0047)
Mean	0.092	0.076	0.046	0.074	0.109
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N (Panel B – D)	2960	3798	1244	2470	3267

Note: The table reports results estimating equation 1 but splitting the sample by gender or region of origin. In Panel A, the outcome is whether a respondent is naturalized (yes = 1, no = 0). In Panels B – D, the outcome is perceived discrimination. The outcome variable is continuous in Panel B, binary in Panels C and D. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

market, our results would be in contrast to that. An alternative explanation could be that men may benefit more from other privileges gained through citizenship like enhanced mobility and international travel, more political and social rights, or less discrimination in other areas like education. Moreover, women and men may also occupy different spaces in everyday life and may therefore be differently affected by discrimination. E.g., mothers, especially those who are not working, may be more involved in matters like childcare where nationality plays less of a role. Another reason for our results could be that some women could interpret the survey question on perceived discrimination differently, and may also report discrimination based on gender – especially if ethnic and gender discrimination intersects. Therefore, if women include sexist discrimination in their reports, then the effects of naturalization on perceived discrimination would

probably be reduced.

Going further, when we differentiate migrants based on their origins, we can see that the reforms had differing effects. First, looking at the effects on naturalization decisions, we see strong differences between groups. While a reduction of waiting times of seven years increases the likelihood to naturalize only by six percentage points among Westerners, it is raised by more than ten and twelve percentage points among Non-Western and Eastern European migrants, respectively. Second, in column (3) we can see that results are positive but always insignificant for Western migrants across specifications. Third, we see a large and significant coefficient when we only look at Eastern European migrants in Panel B, which indicates that this group reacts the most to differences in waiting periods.[29] More precisely, when we use binary outcome variables in Panel C and D, results indicate that easing access reduces less frequent perceptions of discrimination among Eastern Europeans by around 2 percentage points for every year they have to wait less. Fourth, the estimate for non-Western, non-European migrants is not only small but also always insignificant in all specifications, which means that migrants in this group on average do not report less discrimination when barriers to naturalization are reduced.

Thus, we see substantial differences by region of origin, with a decline in perceived discrimination among Eastern Europeans. Possible reasons for that are rather straightforward: As laid out in section 2.2, naturalization removes most legal and factual forms of discrimination that foreigners may encounter in Germany, guaranteeing unrestricted access to the labor market, full mobility within EU countries, improved opportunities for international travel, and granting further rights and privileges, e.g., making it possible to fully participate in the democratic processes in Germany. Moreover, it may also make statistical discrimination less likely, as acquiring citizenship may be interpreted as a strong signal of ability and commitment to stay in Germany by employers. We would expect that all these are benefits that Eastern Europeans would enjoy after naturalizing, particularly those from non-EU countries, which may reduce feelings of exclusion and discrimination. However, why would the other groups not experience the same effects?

Western migrants have only limited benefits from the additional legal privileges of naturalization, as this group is mostly composed of EU and EEA migrants who already enjoy most of them. E.g., there are very few additional labor market benefits EU migrants have from naturalization, EU law already guarantees unrestricted mobility (Tridimas, 2006), and most EU countries have very strong passports, making international travel easy. Therefore, it is not surprising that the

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[29]Distinguishing between Eastern Europeans from the Western Balkans and outside, we run separate regressions, which are not shown for brevity. We find that the effects are largely driven by the former group. However, these results could be due to sampling, as many Eastern Europeans were only included in the SOEP starting in 2013, i.e., after many of them were already EU citizens. In contrast, the sample size of migrants from the Western Balkans was already quite large.

impact on naturalization decisions are so low and changes in terms perceived discrimination are not significant for this group.

In contrast, however, it is less clear why effects for non-European migrants are insignificant. Although this group is quite heterogeneous, including migrants from very different regions like Latin America, Sub-Saharan Africa, and East Asia, results are likely not due to missing statistical power, as the sample size is even larger than for Eastern Europeans and the point coefficient is much closer to zero. However, we would expect that acquiring German citizenship should substantially improve their legal status, perhaps even more than for other groups, e.g., because international travel was much harder with their previous passport or because they were more likely to be the target of statistical discrimination. So what could explain our results?

One potential explanation might be that the legal disadvantages of having a foreign citizenship in Germany are not as salient for non-European migrants. They may simply accept them as the "rules of the game" and would not consider them to be discriminatory. In contrast, discrimination based on one's ethnic origin – e.g., because of a different skin color, appearance, religion, accent or else – may be much more prominent and impactful for one's experience in Germany (Vernby and Dancygier, 2019). Non-Europeans are more likely to experience these forms of discrimination than Europeans (Booth et al., 2012) – in part because discrimination correlates with cultural and genetic distance (Spolaore and Wacziarg, 2016) – and naturalization might do little to dampen these forms of discrimination (Vernby and Dancygier, 2019).

5. Natural Experiment: EU Enlargement

In the previous section, we made use of two citizenship reforms in Germany to estimate the effect of residency requirements on perceived discrimination. Thus, we were only able to estimate the effect of easing access to citizenship, but not the direct effect of naturalizing. To get closer at the latter, we make use of a quasi-natural experiment in the form of EU expansion, which provides us with exogenous changes not in citizenship, but in legal status. The idea is that respondents from other EU countries may benefit from very similar rights and privileges compared to Germans. In extension, this implies that migrants in Germany would experience an improvement in legal status once their home country becomes part of the EU.

One important caveat, though, is that this change did not include full access to the German labor market at the time of EU accession. Rather, to protect their domestic labor markets, EU member states were allowed to restrict access to citizens of accession states for up to seven years, which Germany made use of (Kahanec, 2013). Therefore, in the following, we look at the effects of becoming an EU citizen and gaining full access to the German labor market separately.

First, we study the recent phases of EU enlargement.[30] We hereby exploit three waves of EU accessions after 1996:

- 1 May 2004: Cyprus, Czechia, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia.
- 1 January 2007: Bulgaria, and Romania.
- 1 July 2013: Croatia.

We make use of the timing of these institutional changes and estimate a kind of staggered difference-in-differences estimation, where we regress perceived discrimination on a dummy variable indicating whether a respondent is citizen of an EU country. Respondents are therefore treated once their home country joins the EU, with the treatment variable being zero beforehand.

For our estimations, we make certain sample restrictions. First, we restrict our sample to respondents with a stable non-German nationality – meaning that they did not experience a change in citizenship over the observed time period. Second, we exclude all respondents whose home country was already part of the EU in 1996, and we exclude nationals from Iceland, Liechtenstein, Norway, and Switzerland as member states of the European Free Trade Agreement (EFTA). Third, we only keep respondents from countries that either became part of the EU after 1996 or those whose home countries have or had a plausible path to EU membership, e.g., because they are candidate countries.[31] Lastly, the sampling of migrants in the SOEP data was substantially expanded in 2013, which considerably increased the number of respondents that could only be observed many years after EU expansion took place. Therefore, we only include migrants in our sample, that were interviewed both before and after their home country became part of the EU. Thus, respondents from countries like Poland or Hungary had to have been surveyed at least once before and after 1 May 2004, Romanians and Bulgarians before and after 1 January 2007, and Croatians before and after 1 July 2013. Respondents in the control group of EU candidate countries had to have been interviewed before 1 May 2004 and after 1 July 2013. The construction of these groups is summarized in Table A.11.

Unfortunately for us, the SOEP surveyed only few respondents from EU accession countries in earlier waves. This is evident in Table A.12, which shows that the yearly number of treated individuals starts out at only 10 observations in 2004, 25 in 2005 and 27 in 2006. In spite of the EU accession of Romania and Bulgaria, the number of treated individuals does not increase in

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[30]In extension, this serves as an additional test of our main results regarding Eastern Europeans as mostly Eastern Europeans countries joined the EU in recent years.

[31]Later, in a robustness check, we extend our sample to also include citizens from post-Soviet countries.

2007, indicating the low number of people surveyed from these countries. In the following years, the number becomes even smaller until 2015, after Croatia became part of the EU. Because of this sample composition, we only have very little variation to work with. We therefore caution that the results presented below may not be very robust, but rather complementary to our previous analyses.

With this caveat in mind, we use this sample to perform a staggered difference-in-differences estimation with the following regression equation:

$$Y_{it} = \beta_0 + \beta_1 T_{it} + \gamma X_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \tag{2}$$

Our dependent variable (Y_{it}) – which is perceived discrimination – of respondent i, interviewed in t, is regressed on the treatment variable T_{it}, which is a dummy variable indicating whether a respondent is a citizen of an EU country. In addition, we also include individual, state, and year fixed effects. Lastly, we introduce a vector of control variables X_{it} in the regression. At first, these are age, age squared, gender and educational attainment, which are as good as exogenous. Later on, we also include marriage status, number of children, language proficiency, labor income, employment status, health and disability status, and an urban residence dummy. Standard errors are clustered by individual.[32]

The main results are shown in Table 4 with Panel A showing estimates without further controls, Panel B including plausibly exogenous control variables, and Panel C employing all controls together. Columns (1) to (3) show results when we use the continuous outcome variable, where the full sample is used in column (1), and subsets by gender are employed in the following two columns. Columns (4) and (5) then show results for the full sample when we use binary outcomes.

In column (1), across specifications, estimates are significant and negative, indicating that perceived discrimination decreased for respondents after their home countries joined the EU. The coefficient in Panel A indicates that after becoming an EU citizen, perceived discrimination decreased by .105 or around 20 percent of the mean. As EU expansion after 1996 has almost exclusively affected Eastern European countries, these results can be seen as additional support of our previous findings which showed that easing access to citizenship reduces perceived discrimination among members of this group. Looking at effects by gender in columns (2) and (3), we see that effects for men are statistically significant, while coefficients for women are consid-

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[32]We are aware of the ongoing debate in the literature on two-way fixed effects regressions (De Chaisemartin and d'Haultfoeuille, 2020, 2023; Callaway et al., 2024). However, given the restricted nature of our data, it was not possible for us to use newer estimators like the one from Callaway and Sant'Anna (2021). We, however, again would recommend to interpret the results not by themselves but together with our findings in the earlier sections.

Table 4: EU Enlargement

	Continuous Outcome			Binary Outcome	
	(1)	(2)	(3)	(4)	(5)
	All	Men	Women	PD: Seldom	PD: Frequently
Panel A: Without Controls					
Treated	-0.105**	-0.167*	-0.072	-0.058	-0.046*
	(0.048)	(0.088)	(0.058)	(0.038)	(0.025)
Panel B: With Exog. Controls					
Treated	-0.119**	-0.178**	-0.095*	-0.066*	-0.053**
	(0.047)	(0.087)	(0.055)	(0.037)	(0.025)
Panel C: With All Controls					
Treated	-0.106**	-0.170*	-0.074	-0.064*	-0.042*
	(0.047)	(0.090)	(0.055)	(0.037)	(0.024)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Mean	0.526	0.559	0.503	0.438	0.088
Treated N	293	80	213	293	293
Control N	2747	1182	1565	2747	2747

Note: The table reports regression estimates of equation 2. Columns (1)-(3) use a continuous outcome variable, while columns (4) and (5) use binary outcomes with the cutoff between "Never" and "Seldom" and "Seldom" and "Frequently", respectively. Panel A displays results without additional controls, Panel B shows estimates with plausibly exogenous controls, and Panel C shows estimates with all controls (as described in the text). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

erably smaller and mostly insignificant, which also is in line with our findings in the previous section. Lastly, when we use binary outcome variables for the full sample in columns (4) and (5), coefficients are similar in significance and sign, indicating that joining the EU reduces both frequent and infrequent perceived discrimination.

In Table A.13 in the appendix we conduct the same regressions as before, but with an extended control group which also includes respondents from post-Soviet countries like Russia and Ukraine. Results are very similar in size and significance, confirming our previous results.

As mentioned before, EU accession was not immediately accompanied by full integration of labor markets. Rather, migrants from EU expansion countries had to wait for up to seven years to gain equal access to the German labor market. In the case of the three waves of EU expansions, this played out as following:

- 1 May 2011: Cyprus, Czechia, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia.
- 1 January 2014: Bulgaria, and Romania.
- 1 July 2015: Croatia.

In the following, we make use of this variation in labor market access to perform a similar estimation approach as before. In this case, the treatment group consists of individuals gaining full access to the German labor market, while the control group consists of migrants from EU candidate countries. Sample restrictions are very similar compared to before. The main difference

is that respondents in the treatment group had to have been interviewed before and after gaining full labor market access in Germany, while individuals in the control group had to have been interviewed before 1 May 2011 and after 1 July 2015. We then again estimate equation 2, but for these new treatment and control groups.

Results are displayed in Table A.14. While point estimates for the full sample are negative (column 1), they are insignificant, meaning that on aggregate, gaining access to the labor market does not have an effect on perceived discrimination. However, looking at the following columns, we see that there does appear to be a reduction in frequent perceived discrimination and reports of discrimination among men. These results indicate that lowering barriers to the labor market appears to reduce perceptions of discrimination in some instances and for some groups.

Thus, our results show that both becoming an EU citizen and receiving equal access to the labor market has a negative effect on perceived discrimination. These effects seem to be mostly limited to men and, in the case of labor market access, mostly reduce more frequent reports of discrimination. Overall, these results fit in nicely with the results in section 4. There, we found that mainly men experience reductions in perceived discrimination when barriers to naturalization (in the form of waiting periods) are lowered and that reductions mostly occur among individuals with frequent experiences of discrimination. Moreover, results are also similar in terms of origin country: While reducing residency requirements lowers perceived discrimination the most among Eastern Europeans, it is also Eastern Europeans who benefit the most from EU expansion, as all EU accession countries apart from Cyprus and Malta are located in Eastern Europe.

6. Conclusion

In this study, we examine the impact of reducing barriers to naturalization on migrants' perceived discrimination. Hereby, we use data from the German socio-economic panel in which perceived discrimination is elicited by asking respondents with a migrant background to what extent they have felt disadvantaged in the previous two years because of their ethnic origin. Before conducting our main analysis, we present evidence that perceived discrimination is, among other things, strongly and negatively related to individual well-being, mental health, and staying intentions, and positively related to the probability to actually leave Germany, and to drop out of the panel. Furthermore, we show that these relationships hold even after controlling for time-invariant individual heterogeneity.

To estimate the effects of easing access to citizenship, we exploit exogenous variation in residency requirements due to reforms of German citizenship law in 1991 and 2000. While our

main results show that reducing waiting times significantly reduces perceived discrimination for the whole sample, heterogeneity analyses uncover that effects are mostly limited to men and Eastern Europeans. In addition to our main approach, we perform an extension exploiting variation from EU enlargement, which (apart from Cyprus and Malta) only affected Eastern European countries. Results show that respondents whose home countries became part of the EU – leading to a change of those migrants' legal status – experienced a significant decline in perceived discrimination. Lastly, we also show that granting equal access to the labor market, which was not tied to EU membership but took up to seven years, also leads to reductions in perceived discrimination among men and in perceptions of frequent discrimination.

Our findings have numerous policy implications. First, our results indicate that (perceived) discrimination is not only detrimental to migrants directly affected, but also to the German economy, which increasingly relies on foreign workers. Second, we show that lowering barriers to citizenship leads not only to an increase in the likelihood to naturalize but also a decrease in perceived discrimination. As we argue, this reflects that acquiring citizenship grants migrants additional privileges and rights, among them more social and political participation, enhanced mobility, and lower barriers on the labor market. Observed effects, however, are not spread evenly across different migrant groups, but mainly seem to affect migrants from Eastern Europe. We presume that this is due to the ethnic and cultural distance between natives and migrants: As Eastern Europeans appear closer in terms of appearance, customs, and religion to natives, they may experience less hostility and racist encounters in everyday life than other non-European migrant groups (such as those from the MENA region, sub-Saharan Africa or East Asia). Instead issues like labor market access, extensions of temporary residence permits, issues on the housing market or family reunions – which may lead to a feeling of being disadvantaged – are probably more salient in their everyday lives. In contrast, although members of non-European migrant groups should possibly benefit from acquiring German citizenship the most, their experiences with discrimination are likely shaped much more by other factors (like skin color, religious clothing, accents) than by their nationality. Our findings would therefore suggest that improving access to German citizenship (such as through the recent German citizenship reform in 2024) will likely not lead to a substantial reduction in perceived discrimination among non-European migrant groups. Hence, other measures would be needed to address this issue among these migrants. Third, results regarding EU enlargement would suggest that decreases in perceived discrimination are not solely tied to citizenship, but may just as well be achieved through other improvements in legal status. This would suggest that policymakers could also pursue other measures to decrease perceived discrimination – e.g., by facilitating more social and political

participation or lowering barriers to the labor market.

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Appendix A. Additional Tables and Figures

Figure A.1: Distribution of Perceived Discrimination By Origin Groups and Gender
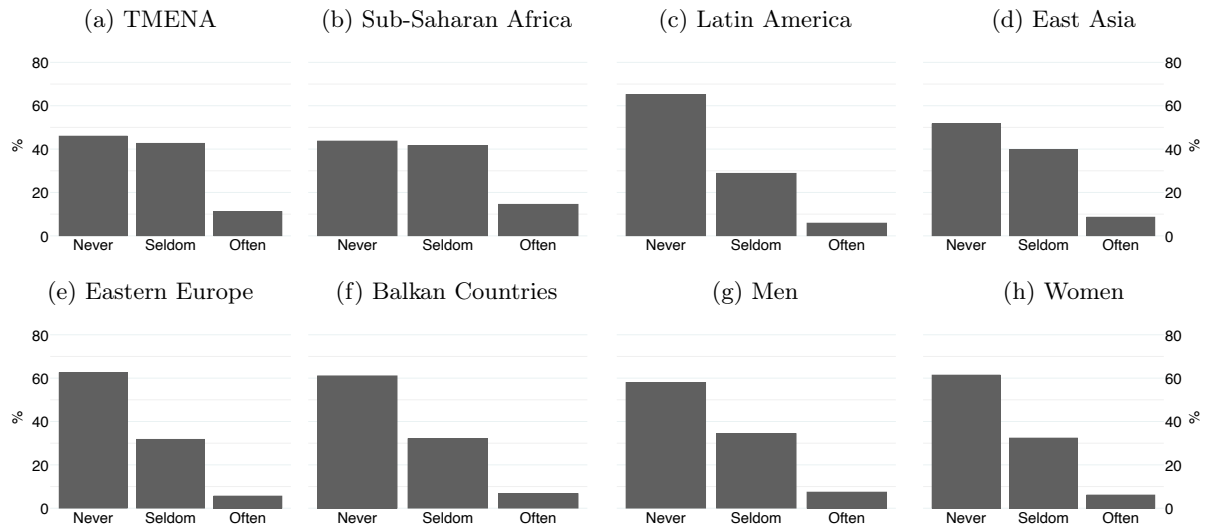
Note: The figure presents the distribution of response options regarding perceived discrimination averaged over the observed time period from 1996 to 2017 for various sub-samples by region of origin (a-f) and gender (g-h).

Figure A.2: Perceived Bitterness over Time by Gender
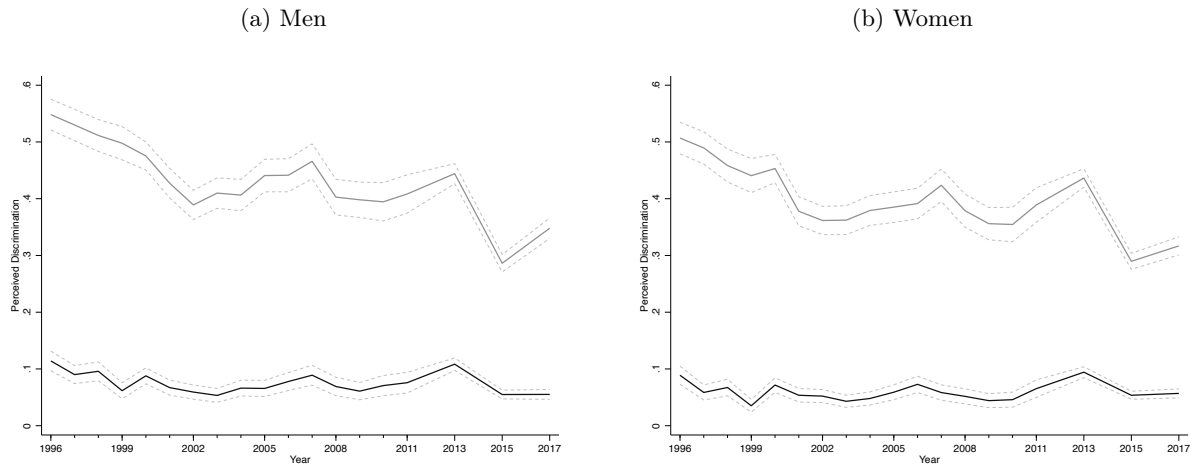
Note: Figure presents time trends of perceived discrimination including 95 percent confidence intervals for men (a) and women (b). Grey line: Has experienced discrimination at least sometimes. Black line: Has experienced discrimination often.

Figure A.3: Share of Migrants with German Citizenship By Origin Groups and Gender
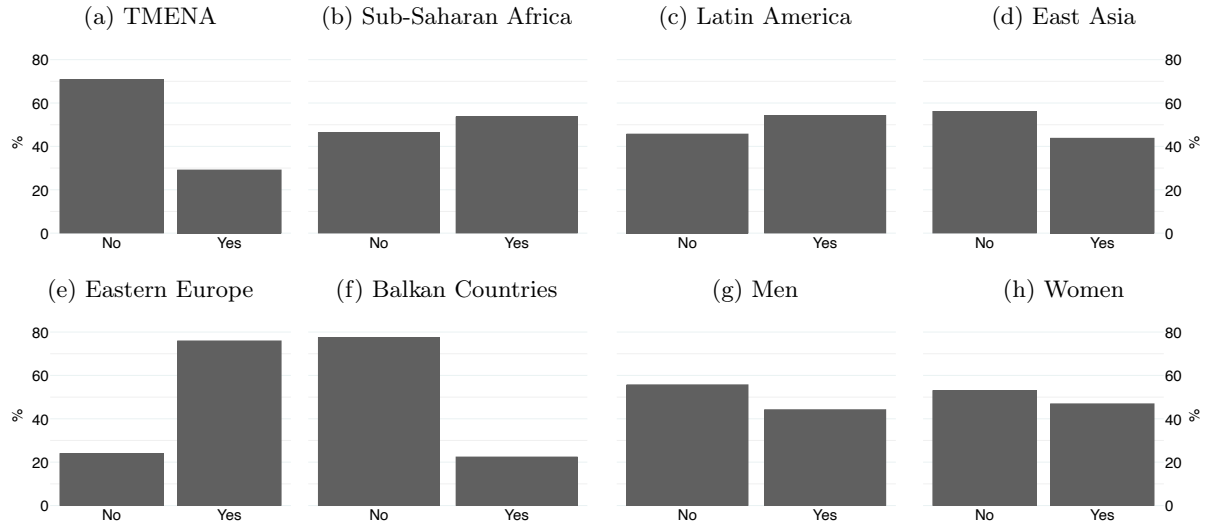
Note: The figure displays the share of migrants with German citizenship averaged over the observed time period from 1996 to 2017 for various sub-samples by region of origin (a-f) and gender (g-h).

Table A.1: Descriptive Statistics of Perceived Discrimination Variable

Year	Never	%	Seldom	%	Often	%	Total	%	Mean	SD
1996	1196	47.20	1080	42.62	258	10.18	2534	100.00	0.63	0.66
1997	1200	48.98	1067	43.55	183	7.47	2450	100.00	0.58	0.63
1998	1215	51.42	954	40.37	194	8.21	2363	100.00	0.57	0.64
1999	1151	52.99	915	42.13	106	4.88	2172	100.00	0.52	0.59
2000	1677	53.56	1204	38.45	250	7.98	3131	100.00	0.54	0.64
2001	1651	59.73	946	34.23	167	6.04	2764	100.00	0.46	0.61
2002	1763	62.50	901	31.94	157	5.57	2821	100.00	0.43	0.60
2003	1638	61.46	899	33.73	128	4.80	2665	100.00	0.43	0.58
2004	1541	60.76	851	33.56	144	5.68	2536	100.00	0.45	0.60
2005	1418	58.81	843	34.96	150	6.22	2411	100.00	0.47	0.61
2006	1392	58.51	808	33.96	179	7.52	2379	100.00	0.49	0.63
2007	1201	55.65	800	37.07	157	7.28	2158	100.00	0.52	0.63
2008	1223	61.00	662	33.02	120	5.99	2005	100.00	0.45	0.61
2009	1273	62.43	660	32.37	106	5.20	2039	100.00	0.43	0.59
2010	1107	62.72	557	31.56	101	5.72	1765	100.00	0.43	0.60
2011	1092	60.23	594	32.76	127	7.00	1813	100.00	0.47	0.62
2013	3733	56.00	2261	33.92	672	10.08	6666	100.00	0.54	0.67
2015	5169	71.19	1698	23.39	394	5.43	7261	100.00	0.34	0.58
2017	4029	66.92	1654	27.47	338	5.61	6021	100.00	0.39	0.59
Total	34669	59.82	19354	33.40	3931	6.78	57954	100.00	0.47	0.62

Note: The table reports response frequencies on the question about perceived discrimination including percentages, means, and standard deviations for each year. Frequencies are based on the full sample in the SOEP without further restrictions.

Table A.2: Descriptive Statistics of Dependent and Explanatory Variables

	Full Sample			Sample Citizenship Reform			Sample EU Enlargement		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Perceived Discrimination	57,954	0.470	0.620	6,758	0.535	0.643	3,040	0.526	0.653
Female	57,954	0.523	0.499	6,758	0.562	0.496	3,040	0.585	0.493
Age	57,953	41.672	15.093	6,758	34.069	9.024	3,040	44.174	13.940
Age sq.	57,953	1964.324	1391.028	6,758	1242.100	637.762	3,040	2145.572	1329.159
Year of Birth	57,954	1965.464	16.441	6,758	1976.101	9.944	3,040	1961.236	13.467
Year of Arrival	45,199	1986.881	13.684	6,758	1990.061	9.254	2,649	1980.866	10.810
Years since Arrival	45,199	20.140	11.745	6,758	20.109	8.290	2,649	24.467	11.612
Years since Arrival sq.	45,199	543.554	560.629	6,758	473.068	356.139	2,649	733.434	579.295
Latin America	57,954	0.012	0.110	6,758	0.011	0.102	3,040	0.000	0.000
East Asia	57,954	0.025	0.155	6,758	0.031	0.172	3,040	0.000	0.000
Sub-Saharan Africa	57,954	0.011	0.103	6,758	0.017	0.128	3,040	0.000	0.000
Turkey + MENA	57,954	0.253	0.434	6,758	0.427	0.495	3,040	0.524	0.500
Western Countries	57,954	0.261	0.439	6,758	0.184	0.388	3,040	0.011	0.104
Eastern Europe	57,954	0.326	0.469	6,758	0.197	0.398	3,040	0.134	0.341
Western Balkan Countries	57,954	0.134	0.341	6,758	0.171	0.376	3,040	0.363	0.481
EU Citizen	57,954	0.255	0.436	6,758	0.213	0.409	3,040	0.096	0.295
Refugee	56,728	0.062	0.241	6,725	0.151	0.358	3,040	0.042	0.200
Schleswig Holstein	57,954	0.023	0.150	6,758	0.026	0.158	3,040	0.023	0.151
Hamburg	57,954	0.015	0.120	6,758	0.019	0.137	3,040	0.041	0.199
Lower Saxony	57,954	0.092	0.288	6,758	0.090	0.287	3,040	0.045	0.207
Bremen	57,954	0.009	0.095	6,758	0.013	0.113	3,040	0.000	0.000
Northrhine-Westphalia	57,954	0.267	0.443	6,758	0.263	0.440	3,040	0.302	0.459
Hesse	57,954	0.098	0.297	6,758	0.099	0.298	3,040	0.099	0.299
Rhineland Palatinate	57,954	0.058	0.233	6,758	0.059	0.235	3,040	0.044	0.206
Baden-Württemberg	57,954	0.214	0.410	6,758	0.216	0.412	3,040	0.230	0.421
Bavaria	57,954	0.153	0.360	6,758	0.141	0.348	3,040	0.188	0.390
Saarland	57,954	0.016	0.126	6,758	0.015	0.121	3,040	0.003	0.051
Berlin	57,954	0.029	0.168	6,758	0.031	0.174	3,040	0.015	0.123
Brandenburg	57,954	0.007	0.083	6,758	0.009	0.097	3,040	0.000	0.000
Mecklenburg Western Pomerania	57,954	0.002	0.045	6,758	0.003	0.057	3,040	0.000	0.000
Saxony	57,954	0.007	0.084	6,758	0.005	0.072	3,040	0.010	0.099
Saxony-Anhalt	57,954	0.005	0.070	6,758	0.007	0.083	3,040	0.000	0.000
Thuringia	57,954	0.005	0.073	6,758	0.004	0.059	3,040	0.000	0.000
Urban Residence	57,954	0.820	0.384	6,758	0.845	0.362	3,040	0.880	0.325
Married	57,735	0.666	0.472	6,737	0.659	0.474	3,037	0.805	0.396
Number of Children	57,896	0.930	1.165	6,750	1.467	1.300	3,038	1.025	1.226
Medium or High Language Skills	37,426	0.831	0.375	5,266	0.888	0.316	2,046	0.762	0.426
Medium-Skilled	56,654	0.477	0.499	6,625	0.437	0.496	2,951	0.588	0.492
High-Skilled	56,654	0.329	0.470	6,625	0.297	0.457	2,951	0.137	0.343
In Education	57,952	0.076	0.266	6,758	0.094	0.292	3,040	0.015	0.123
In Work	57,952	0.589	0.492	6,758	0.616	0.486	3,040	0.560	0.496
Unemployed	57,952	0.092	0.289	6,758	0.092	0.290	3,040	0.100	0.300
Log Real Labor Income	34,435	7.475	0.845	4,213	7.351	0.895	1,659	7.459	0.771
Poor Health	57,884	0.167	0.373	6,752	0.122	0.327	3,035	0.217	0.412
Disabled	57,787	0.075	0.263	6,750	0.036	0.187	3,033	0.084	0.277

Note: The table reports means and standard deviations of perceived discrimination and explanatory variables used as additional control variables in regressions of sections 4 and 5, and Appendix B. Descriptive statistics are provided for the full sample in the SOEP, and samples constructed for the regression analyses in sections 4 and 5.

Table A.3: Categorization of Countries into Regions of Origin

Region	Countries
Western	Australia, Austria, Belgium, Benelux, Canada, Cyprus, Denmark, Finland, France, Great Britain, Greece, Ireland, Israel, Italy, Luxembourg, Monaco, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, USA
Eastern Europe	Armenia, Azerbaijan, Belarus, Bulgaria, Chechnya, Czechia, Estonia, Georgia, Hungary, Kabardino-Balkaria, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Moldova, Poland, Romania, Russia, Slovakia, Tajikistan, Turkmenistan, Ukraine, Uzbekistan
Balkan Countries	Albania, Bosnia and Herzegovina, Former Yugoslavia/Serbia and Montenegro, Kosovo, Kosovo/Albania, Croatia, (North) Macedonia, Montenegro, Serbia, Slovenia
Turkey, Middle East, North Africa	Algeria, Egypt, Iraq, Iran, Jordan, Kurdistan, Kuwait, Lebanon, Libya, Morocco, Palestine, Saudi-Arabia, Stateless, Syria, Tunisia, Turkey, United Arab Emirates, Yemen
Africa (excl. North Africa)	Africa, Angola, Benin, Botswana, Burkina Faso, Cameroon, Chad, Congo, Djibouti, Eritrea, Ethiopia, Gambia, Ghana, Guinea, Ivory Coast, Kenya, Lesotho, Liberia, Madagascar, Mali, Mauritius, Mozambique, Namibia, Niger, Nigeria, Rwanda, Senegal, Seychelles, Sierra Leone, Somalia, South Africa, Sudan, Tanzania, Togo, Uganda, Zambia, Zimbabwe
Latin America	Argentina, Bahamas, Bolivia, Brazil, Chile, Colombia, Costa Rica, Cuba, Dominican Republic, Ecuador, El Salvador, Guyana, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Paraguay, Peru, St. Lucia, Suriname, Trinidad and Tobago, Uruguay, Venezuela
East Asia (incl. Oceania)	Afghanistan, Bangladesh, Cambodia, China, Hong Kong, India, Indonesia, Japan, Korea, Laos, Malaysia, Maldives, Mongolia, Myanmar, Nepal, Pakistan, Philippines, Samoa, Singapore, Sri Lanka, Taiwan, Thailand, Vietnam

Note: The table displays which countries were categorized as belonging to which regions. Country names are directly taken from the SOEP dataset. Therein, some of the entries do not represent actual countries, but either regions within countries (e.g., Chechnya) or broader regions capturing several countries (e.g., Benelux, Kosovo/Albania).

Table A.4: ITT Effects of Citizenship Reforms: Robustness I

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Continuous Dependent Variable						
Residency Req.	0.0113*	0.0116**	0.0103*	0.0130**	0.0121**	0.0112*
	(0.0059)	(0.0056)	(0.0059)	(0.0058)	(0.0059)	(0.0059)
Mean	0.535	0.535	0.535	0.535	0.535	0.535
Panel B: Binary Outcome with Cutoff between No PD and Some PD						
Residency Req.	0.0055	0.0066	0.0044	0.0069	0.0064	0.0052
	(0.0046)	(0.0043)	(0.0045)	(0.0046)	(0.0046)	(0.0046)
Mean	0.453	0.453	0.453	0.453	0.453	0.453
Panel C: Binary Outcome with Cutoff between Some PD and Frequent PD						
Residency Req.	0.0057**	0.0050**	0.0060**	0.0060**	0.0057**	0.0059**
	(0.0024)	(0.0023)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
Mean	0.083	0.083	0.083	0.083	0.083	0.083
Cohort of Arrival FE	Yes		Yes	Yes	Yes	Yes
Year of Birth FE	Yes		Yes	Yes	Yes	Yes
Grouped Cohort of Arrival		Yes				
Grouped Year of Birth		Yes				
Region of Origin Dummies	Yes	Yes		Yes	Yes	Yes
Region of Origin FE			Yes			
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes			Yes
East Dummy				Yes		
State FE w/ East as One					Yes	
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	
State-Year FE						Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	6758	6758	6758	6758	6758	6758

Note: The table shows robustness tests for estimated results of equation 1 by modifying parts of the regression specification. Column (1): Baseline. Column (2): Instead of using year of arrival and year of birth fixed effects, respondents are assigned to larger groups with five-year intervals for year of arrival and year of birth. Column (3): Instead of region dummies, we use country of origin dummies as originally employed in the study by Gathmann and Keller (2018). Column (4): Instead of state fixed effects, we include a East-West dummy variable. Column (5): We include state fixed effects, but treat the former East German states as if they were just one state. Column (6): We use state-year fixed effects instead of linear trends. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.5: ITT Effects of Citizenship Reforms: Robustness II

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Continuous Dependent Variable								
Residency Req.	0.0117**	0.0113*	0.0112*	0.0111*	0.0114*	0.0113*	0.0112*	0.0112*
	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)
Mean	0.535	0.535	0.535	0.535	0.535	0.535	0.535	0.535
Panel B: Binary Outcome with Cutoff between No PD and Some PD								
Residency Req.	0.0058	0.0055	0.0055	0.0055	0.0056	0.0055	0.0055	0.0053
	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)
Mean	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453
Panel C: Binary Outcome with Cutoff between Some PD and Frequent PD								
Residency Req.	0.0060**	0.0057**	0.0057**	0.0056**	0.0058**	0.0057**	0.0057**	0.0058**
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
Mean	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YSM Polynomial	Linear	Quadratic	Cubic	Quartic	Quadratic	Quadratic	Quadratic	Quadratic
Age Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Linear	Quadratic	Cubic	Quartic
N	6758	6758	6758	6758	6758	6758	6758	6758

Note: The table shows robustness tests for estimated results of equation 1 by removing or adding polynomials for years since arrival and age. Columns (1)-(4) use a first-order to fourth-order polynomial of years since arrival, and a quadratic polynomial for age. Columns (5)-(8) use a first-order to fourth-order polynomial for age, and a quadratic polynomial for years since arrival. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.6: ITT Effects of Citizenship Reforms: Robustness III

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Continuous Dependent Variable									
Residency Req.	0.0113*	0.0124**	0.0114*	0.0105*	0.0101*	0.0120**	0.0115*	0.0113*	0.0111*
	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0058)	(0.0059)	(0.0059)	(0.0059)
Mean	0.535	0.535	0.535	0.535	0.535	0.535	0.535	0.535	0.535
Panel B: Binary Outcome with Cutoff between No PD and Some PD									
Residency Req.	0.0055	0.0062	0.0056	0.0049	0.0048	0.0060	0.0058	0.0055	0.0055
	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0045)	(0.0046)	(0.0046)	(0.0046)
Mean	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453
Panel C: Binary Outcome with Cutoff between Some PD and Frequent PD									
Residency Req.	0.0057**	0.0062***	0.0058**	0.0055**	0.0053**	0.0060**	0.0057**	0.0057**	0.0056**
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
Mean	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educ. Attainment		Yes							Yes
Urban Residence			Yes						Yes
Social Contr. Var.				Yes					Yes
Language Contr. Var.					Yes				Yes
Labor Contr. Var.						Yes			Yes
Health Contr. Var.							Yes		Yes
State Economic Contr. Var.								Yes	Yes
N	6758	6758	6758	6758	6758	6758	6758	6758	6758

Note: The table shows robustness tests for estimated results of equation 1 by gradually including control variables: (1): Baseline. (2): Respondent is high-skilled (0/1 dummy), is medium skilled (0/1 dummy), is currently in education (0/1 dummy). (3): Respondent lives in urban residence (0/1 dummy). (4): Respondent is married (0/1 dummy), has at least one kid (0/1 dummy). (5): Respondent has at least medium language proficiency (0/1 dummy). (6): Log real labor market income, respondent is working (0/1 dummy), unemployed (0/1 dummy). (7): Self-assessed health status. (8): State GDP per capita and unemployment rates. (9): All controls at once. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.7: ITT Effects of Citizenship Reforms: Robustness IV

	(1)	(2)	(3)	(4)	(5)
Panel A: Continuous Dependent Variable					
Residency Req.	0.0113*	0.0116**	0.0115**	0.0118**	0.0120**
	(0.0059)	(0.0059)	(0.0059)	(0.0059)	(0.0059)
Mean	0.535	0.535	0.535	0.535	0.535
Panel B: Binary Outcome with Cutoff between No PD and Some PD					
Residency Req.	0.0055	0.0060	0.0057	0.0056	0.0060
	(0.0046)	(0.0046)	(0.0046)	(0.0046)	(0.0046)
Mean	0.453	0.453	0.453	0.453	0.453
Panel C: Binary Outcome with Cutoff between Some PD and Frequent PD					
Residency Req.	0.0057**	0.0057**	0.0058**	0.0063***	0.0060**
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
Mean	0.083	0.083	0.083	0.083	0.083
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Parental Schooling		Yes			Yes
Parental Voc. Education			Yes		Yes
Parental Job Status				Yes	Yes
N	6758	6758	6758	6758	6758

Note: The table shows robustness tests for estimated results of equation 1 by including parental control variables. Column (1): Baseline. Column (2): Includes dummy variables capturing school degrees of both mother and father of the respondent: no information (0/1), no school degree (0/1), *Abitur* degree or similar (0/1). Column (3): Includes dummy variables on the vocational education of both mother and father of the respondent: no information (0/1), no vocational education (0/1), university education or similar (0/1). Column (4): Includes dummy variables on the job status of both mother and father when respondent was 15 years old: no information (0/1), no job or not working (0/1), skilled or high-skilled position (0/1). Column (5): Includes all dummy variables used in columns (2) to (4) at the same time. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.8: ITT Effects of Citizenship Reforms: Panel Attrition

	Panel Attrition			Moved Abroad		
	(1)	(2)	(3)	(4)	(5)	(6)
Naturalized Eventually	-0.0431***			-0.0030*		
	(0.0075)			(0.0015)		
Eligible for Citizenship		0.0002			-0.0080	
		(0.0309)			(0.0111)	
Residency Req.			0.0007			-0.0008
			(0.0033)			(0.0009)
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean	0.161	0.166	0.166	0.010	0.010	0.010
N	14753	6758	6758	14753	6758	6758

Note: The table reports results when testing for selective outmigration and panel attrition. In columns (1)-(3), the outcome variable is whether a respondent dropped out of the panel. In columns (4)-(6), the outcome variable is whether a respondent moved abroad. In columns (1)+(4), the main explanatory variable is whether the respondent became German at some point (yes = 1, no = 0). In columns (2)+(5), the main explanatory variable is whether the respondent is eligible for citizenship (yes = 1, no = 0). In columns (3)+(6), the main explanatory variable is the number of years a person has to reside in Germany to become eligible for citizenship. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors are clustered by age times year of arrival. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.9: ITT Effects of Citizenship Reforms: Robustness V

	(1) Baseline: 2002	(2) 2000	(3) 2001	(4) 2003
Panel A: Continuous Dependent Variable				
Residency Req.	0.0113*	0.0098*	0.0095*	0.0109*
	(0.0059)	(0.0055)	(0.0057)	(0.0061)
Mean	0.535	0.539	0.537	0.536
Panel B: Binary Outcome: Cutoff b/w No PD/Some PD				
Residency Req.	0.0055	0.0044	0.0039	0.0048
	(0.0046)	(0.0042)	(0.0044)	(0.0047)
Mean	0.453	0.457	0.454	0.452
Panel C: Binary Outcome: Cutoff b/w Some PD/Frequent PD				
Residency Req.	0.0057**	0.0054**	0.0056**	0.0061**
	(0.0024)	(0.0023)	(0.0023)	(0.0025)
Mean	0.083	0.082	0.083	0.084
Cohort of Arrival FE	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	6758	7641	7205	6312

Note: The table shows robustness tests for estimated results of equation 1 by changing the selection of the first year of survey data included in regressions. Column (1): Baseline. Column (2): Includes survey years 2000 to 2017. Column (3): 2001 to 2017. Column (4): 2003 to 2017. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.10: ITT Effects of Citizenship Reforms: Robustness VI

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Excluding Respondents with/who					
		German	German	German	Arrived	Arrived	Live in
	Baseline	Parent	Spouse I	Spouse II	>=2000	>=1991	East
Panel A: Continuous Dependent Variable							
Residency Req.	0.0113*	0.0106*	0.0171**	0.0134**	0.0126*	0.0140*	0.0113*
	(0.0059)	(0.0060)	(0.0067)	(0.0061)	(0.0066)	(0.0079)	(0.0059)
Mean	0.535	0.534	0.542	0.526	0.526	0.511	0.534
Panel B: Binary Outcome with Cutoff between No PD and Some PD							
Residency Req.	0.0055	0.0055	0.0095*	0.0079*	0.0058	0.0051	0.0055
	(0.0046)	(0.0047)	(0.0053)	(0.0047)	(0.0052)	(0.0062)	(0.0046)
Mean	0.453	0.453	0.460	0.448	0.448	0.438	0.452
Panel C: Binary Outcome with Cutoff between Some PD and Frequent PD							
Residency Req.	0.0057**	0.0051**	0.0076***	0.0055**	0.0068***	0.0089***	0.0058**
	(0.0024)	(0.0024)	(0.0027)	(0.0025)	(0.0026)	(0.0033)	(0.0024)
Mean	0.083	0.082	0.081	0.079	0.078	0.073	0.082
Cohort of Arrival FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region of Origin Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6758	6601	4615	5605	5493	3369	6496

Note: The table shows robustness tests for estimated results of equation 1 by altering the sample selection: (1): Baseline. (2): Excludes respondents with at least one parent who was born in Germany, which proxies whether the parent is German. (3): Excludes respondents with a German spouse (version 1). Respondents are identified by checking whether they were married to a German at any point between 1990 and 2015. (4): Excludes respondents with a German spouse (version 2). Respondents are identified by checking whether they were married to a German before they were eligible to naturalize, i.e., before they fulfilled their residency requirements. (5): Excludes respondents who arrived in 2000 or later. (6): Excludes respondents who arrived in 1991 or later. (7): Removes respondents in East Germany. The outcome variable is continuous in Panel A, binary in Panels B and C. Regressions otherwise specified as in columns (2)-(4) of Table 2. Standard errors (in parentheses) are clustered at the age times arrival year level.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table A.11: EU Enlargement: Construction of Treatment and Control Groups

Region	Countries
Treatment Group	Cyprus, Czechia, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, Slovenia (starting in 2004), Bulgaria and Romania (starting in 2007), Croatia (starting in 2013).
Control Group 1	Albania, Bosnia & Hercegovina, Georgia, Kosovo, Kosovo-Albania, Macedonia, Moldova, Montenegro, Serbia, Turkey, Ukraine, Yugoslavia/Serbia & Montenegro.
Control Group 2	All countries in Control Group 1 + all other post-Soviet countries in the sample, in this case Armenia, Azerbaijan, Belarus, Kazakhstan, Kyrgyzstan, Russia, Tajikistan, Uzbekistan.

Note: The table displays which countries were categorized as belonging to the treatment and the control groups used for the estimations in Table 4. Country names are directly taken from the SOEP dataset.

Table A.12: EU Enlargement: Distribution of Treated Individuals over Time

	Not Treated	Treated	Total
1996	131	0	131
1997	132	0	132
1998	134	0	134
1999	138	0	138
2000	170	0	170
2001	174	0	174
2002	181	0	181
2003	187	0	187
2004	169	10	179
2005	146	25	171
2006	143	27	170
2007	143	25	168
2008	141	22	163
2009	138	20	158
2010	140	18	158
2011	148	15	163
2013	169	15	184
2015	90	73	163
2017	73	43	116
Total	2747	293	3040

Note: The table displays in which year how many respondents in our sample were affected by EU enlargement. Sample consists of treatment group and control group 1.

Table A.13: EU Enlargement: Control Group 2

	Cardinal Outcome			Binary Outcome	
	(1)	(2)	(3)	(4)	(5)
	All	Men	Women	PD: Seldom	PD: Frequently
Panel A: Without Controls					
Treated	-0.096**	-0.164*	-0.064	-0.052	-0.044*
	(0.048)	(0.087)	(0.058)	(0.038)	(0.025)
Panel B: With Exog. Controls					
Treated	-0.111**	-0.176**	-0.088	-0.060	-0.051**
	(0.047)	(0.086)	(0.054)	(0.037)	(0.025)
Panel C: With All Controls					
Treated	-0.103**	-0.168*	-0.069	-0.060*	-0.042*
	(0.047)	(0.089)	(0.055)	(0.036)	(0.024)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes
Mean	0.523	0.551	0.503	0.436	0.087
Treated N	293	80	213	293	293
Control N	2844	1215	1629	2844	2844

Note: The table reports regression estimates of equation 2 using an alternative control group which also includes respondents from post-Soviet nations. Columns (1)-(3) use a continuous outcome variable, while columns (4) and (5) use binary outcomes with the cutoff between "Never" and "Seldom" and "Seldom" and "Frequently", respectively. Panel A displays results without additional controls, Panel B shows estimates with plausibly exogenous controls, and Panel C shows estimates with all controls (as described in the text). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A.14: EU Enlargement: Full Labor Market Access

	Cardinal Outcome			Binary Outcome	
	(1)	(2)	(3)	(4)	(5)
	All	Men	Women	PD: Seldom	PD: Frequently
Panel A: Without Controls					
Treated	-0.085	-0.204*	-0.029	-0.017	-0.068**
	(0.056)	(0.104)	(0.066)	(0.041)	(0.032)
Mean	0.502	0.552	0.467	0.429	0.073
Treated N	309	110	199	309	309
Control N	2378	1023	1355	2378	2378
Panel B: With Exog. Controls					
Treated	-0.081	-0.206**	-0.023	-0.015	-0.065**
	(0.055)	(0.103)	(0.065)	(0.041)	(0.032)
Mean	0.499	0.545	0.467	0.427	0.072
Treated N	309	110	199	309	309
Control N	2527	1063	1464	2527	2527
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes

Note: The table reports regression estimates of equation 2, but making use of variation in labor market access instead of EU accession. Columns (1)-(3) use a continuous outcome variable, while columns (4) and (5) use binary outcomes with the cutoff between "Never" and "Seldom" and "Seldom" and "Frequently", respectively. Panel A displays results without additional controls, Panel B shows estimates with plausibly exogenous controls, and Panel C shows estimates with all controls (as described in the text). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix B. Implications of Perceived Discrimination

As outlined in section 2.3, we add to the literature on the potential implications of perceived discrimination by looking at its effect on a number of outcome variables.

To do so, we again employ data from the German socio-economic panel, which is described in section 3. Hereby, we do not perform any sample restrictions apart from omitting the refugee sample (Brücker et al., 2017). Therefore, the respective sample for each of the following regressions is determined by the number of observations for which we have information on our main explanatory variable (perceived discrimination) and the respective outcome variable. In our analysis, we look at the following outcomes: (1) Life satisfaction (scaled from 1 to 10); (2) mental health (based on the mental component score); (3) concerns about xenophobia (with available response options: "not concerned at all", "somewhat concerned", "very concerned"); (4) whether respondent is feeling German (scaled from 0 ("not at all") to 4 ("completely")); (5) perceived connection to home country (scaled from 0 ("not at all") to 4 ("very strong")); (6) intention to stay forever in Germany; (7) intention to stay in Germany for at most one year; (8) whether respondent has moved abroad; (9) whether respondent has moved to another state within Germany; (10) whether respondent has dropped out of the SOEP; (11) political interest (scaled from 0 ("completely disinterested") to 3 ("very interested")); (12) whether respondent has a preference for a political party in Germany; (13) whether respondent has a preference for a left-wing party in Germany, namely SPD, Bündnis 90/Die Grünen, Die Linke or Piratenpartei; (14) whether respondent has a preference for a center-right party in Germany, namely CDU, CSU or FDP. Hereby, outcomes (1) to (5) and outcome (11) are treated as continuous variables while all other outcomes are binary (0 = no; 1 = yes).

We use these outcomes as dependent variables in separate regressions, in which two of the three response options to the question on perceived discrimination are used as the main explanatory variables, namely, whether discrimination has occurred "frequently" or "seldom", with "never" being the base category. Hereby, we use the following regression model (with standard errors clustered by person):

$$y_{it} = \alpha_0 + \alpha_1 Seldom_{it} + \alpha_2 Frequently_{it} + \beta Z_{it} + S_{it} + \tau_t + \rho_i + \epsilon_{it}. \tag{B.1}$$

In this model, the respective outcome variable y_{it} for individual i in year of interview t is regressed on the two dummy variables $Seldom_{it}$ and $Frequently_{it}$, which indicate whether the respondent has rarely or frequently experienced discrimination in the past two years. (If respondents did not experience discrimination, both dummy variables are equal to zero.) Additionally, we include state of residence (S_{it}) and year fixed effects (τ_t), and control for a number of de-

mographic, social, educational, language, labor market, and health characteristics (Z_{it}). Adding these controls should already to a large extent account for time-varying factors that could affect the relation between perceived discrimination and the respective outcome. In particular, we are able to control for the general effects of integration and assimilation, which may allow migrants to better identify discrimination (Diehl et al., 2021). Moreover, we also add individual fixed effects (ρ_i) to the regression in a separate step. Doing so has the meaningful benefit of allowing us to estimate within-individual effects, or in other words: they eliminate all observable and unobservable time-constant heterogeneity. This means that we are able to account for differences in personality between individuals, which may influence how likely they are to attribute discrimination to certain situations and how willing they are to report discrimination – at least to the extent that these factors are time-constant.

Nevertheless, we caution against interpreting our estimates as causal as we cannot rule out that there is some remaining omitted variation coming from unobserved and time-variant factors, which may influence both our outcomes and perceived discrimination. Furthermore, for many of the outcome variables, we also cannot rule out reverse causality (or at least simultaneity), as, e.g., changes in life satisfaction could potentially lead to changes in how respondents report discrimination.

The results of our estimations can be found in Table B.1, with Panel A (Panel B) showing results without (with) individual fixed effects. Generally, we find very striking relationships between perceived discrimination and most of the examined outcomes, with significant effects for both frequent and infrequent discrimination. Thereby, the effects appear particularly large for the former, even after accounting for time-invariant heterogeneity. E.g., facing frequent discrimination is associated with strong decreases in life satisfaction (column 1) and mental health (2). Rather unsurprisingly, there is also a very strong positive relationship with concerns about xenophobia (3). Estimates also indicate that respondents who report discrimination appear to de-identify with Germany (4) and feel stronger attachment to their home countries (5). In addition, there is not only an effect on one's staying intentions, but also observed migration. Respondents are less likely to want to stay in Germany forever (6), more likely to want to leave Germany within one year (7), and, they are also more likely to actually leave Germany after the respective interview (8). Respondents who encountered discrimination were 0.6 percentage points more likely to move abroad, which – considering that the mean in the sample is only around one percent – is a noteworthy effect. Looking at movement between states within Germany, effects do mostly not reach significance (9), with only a significant effect for those reporting rare discrimination in the OLS estimations. However, the mean value of 0.4 percent is very low, which

makes us suspect that many respondents who move residence inside of Germany may simply drop out of the panel. We therefore estimate the effects on panel attrition in (10). Hereby, we do find a highly significant increase in panel attrition for those who rarely faced discrimination, while estimates for frequent faced discrimination are also positive but only weakly significant. These findings suggest that respondents who face infrequent discrimination may try to alleviate that by moving to another location within Germany while those who feel discriminated more often rather leave the country altogether. Estimates in the last four columns reveal that perceived discrimination may also go along with changes in political preferences. More specifically, we see positive coefficients for political interest (11) but also a higher preference for any political party (12). This increased preference for a party appears to benefit left-wing parties (13), while support for moderately conservative parties goes down (14). This is not too surprising, as left-wing parties are usually perceived as being more supportive of the causes of immigrants.

To test the robustness of our estimates and to reduce omitted variable bias, we modify our fixed effects regressions to include additional control variables. Results are shown in Table B.2. In Panel A, we include month and day of week fixed effects to account for potential seasonality and weekend effects in our estimates. In Panel B, regressions include state GDP per capita and unemployment rates, as changes in the local labor markets may affect the examined relation. In Panel C, we test whether results may be driven by tragic events experienced by respondents, namely the deaths of relatives or close friends. In Panel D, we examine whether effects could be caused by changes in worries and satisfactions more generally. Hereby, we include measures that should be completely unrelated to perceived discrimination and the relation in question, namely worries about the economy in general, satisfaction with one's own health, and satisfactions with housework. Panel E shows results when all control variables are inserted together in the regression equation. Overall, our main estimates remain remarkably stable.

Table B.1: Effects of Perceived Discrimination on Several Outcomes

	(1) Life Satisf.	(2) Mental Health	(3) Worry Xenophobia	(4) Feel German	(5) Connect Home C.	(6) Stay Forever	(7) Leave Soon	(8) Moved Abroad	(9) Moved to New State	(10) Left SOEP	(11) Polit. Interest	(12) Party Pref.	(13) Left-Wing	(14) Conserv.
Panel A: OLS Results														
Seldom	-0.3543***	-2.9547***	0.2114***	-0.2227***	0.1340***	-0.0683***	0.0026***	0.0043***	0.0020***	0.0229***	0.0259**	-0.0101*	0.0134***	-0.0244***
	(0.0197)	(0.2284)	(0.0082)	(0.0198)	(0.0188)	(0.0057)	(0.0010)	(0.0010)	(0.0007)	(0.0033)	(0.0105)	(0.0056)	(0.0048)	(0.0037)
Often	-0.7864***	-5.5288***	0.4719***	-0.4252***	0.1372***	-0.1010***	0.0071***	0.0108***	0.0008	0.0370***	0.1150***	0.0235**	0.0461***	-0.0267***
	(0.0443)	(0.5244)	(0.0151)	(0.0382)	(0.0379)	(0.0106)	(0.0022)	(0.0023)	(0.0012)	(0.0065)	(0.0220)	(0.0105)	(0.0091)	(0.0058)
Panel B: FE Results														
Seldom	-0.1412***	-0.7293***	0.1181***	-0.0609***	0.0542**	-0.0215***	0.0006	0.0016	0.0015	0.0107***	0.0203***	-0.0006	0.0059	-0.0062**
	(0.0167)	(0.2489)	(0.0078)	(0.0233)	(0.0226)	(0.0043)	(0.0011)	(0.0013)	(0.0009)	(0.0036)	(0.0076)	(0.0045)	(0.0036)	(0.0029)
Often	-0.4119***	-2.5673***	0.2843***	-0.2843***	0.1938***	-0.0795***	0.0076***	0.0060**	-0.0006	0.0130*	0.0169	0.0190**	0.0180**	-0.0009
	(0.0381)	(0.5454)	(0.0164)	(0.0492)	(0.0467)	(0.0088)	(0.0025)	(0.0026)	(0.0015)	(0.0072)	(0.0156)	(0.0088)	(0.0077)	(0.0053)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477

Note: The table reports results when regressing various outcomes on perceived discrimination and additional control variables (listed in Table A.2) and state and year fixed effects. Panel A shows estimates for OLS regressions, while Panel B shows results after additionally including individual fixed effects. Examined outcomes are: (1) Life satisfaction (scaled from 1 to 10); (2) Mental health (based on the mental component score); (3) Concerns about xenophobia (response options: "not concerned at all" (1), "somewhat concerned" (2), "very concerned" (3)); (4) Attachment to Germany (both scaled from 0 ("not at all") to 4 ("completely"/"very strong")); (6) Intention to stay forever in Germany (either yes (1) or no (0)); (7) Intention to stay in Germany for at most one year (either yes (1) or no (0)); (8) Respondent has moved abroad (either yes (1) or no (0)); (9) Respondent has moved to another state within Germany (either yes (1) or no (0)); (10) Respondent has dropped out of the SOEP (either yes (1) or no (0)); (11) Political interest (scaled from 0 ("completely disinterested") to 3 ("very interested"); (12) Preference for a political party in Germany (either yes (1) or no (0)); (13) Preference for a left-wing party in Germany, namely SPD, Bündnis 90/Die Grünen, Die Linke or Piratenpartei (either yes (1) or no (0)); (14) Preference for a center-right party in Germany, namely CDU, CSU or FDP (either yes (1) or no (0)). Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B.2: Effects of Perceived Discrimination on Several Outcomes: Robustness Checks

	(1) Life Satisf.	(2) Mental Health	(3) Worry Xenophobia	(4) Feel German	(5) Connect Home C.	(6) Stay Forever	(7) Leave Soon	(8) Moved Abroad	(9) Moved to New State	(10) Left SOEP	(11) Polit. Interest	(12) Party Pref.	(13) Left-Wing	(14) Conserv.
Panel A: Month and Weekday FE														
Seldom	-0.1411***	-0.7394***	0.1182***	-0.0630***	0.0532**	-0.0213***	0.0006	0.0015	0.0015	0.0103***	0.0204***	-0.0004	0.0060	-0.0060**
	(0.0167)	(0.2485)	(0.0078)	(0.0232)	(0.0227)	(0.0043)	(0.0011)	(0.0013)	(0.0009)	(0.0036)	(0.0076)	(0.0045)	(0.0036)	(0.0029)
Often	-0.4110***	-2.5947***	0.2848***	-0.2848***	0.1926***	-0.0795***	0.0077***	0.0060**	-0.0006	0.0123*	0.0169	0.0193**	0.0182**	-0.0007
	(0.0381)	(0.5430)	(0.0164)	(0.0490)	(0.0468)	(0.0088)	(0.0025)	(0.0026)	(0.0015)	(0.0072)	(0.0156)	(0.0088)	(0.0077)	(0.0053)
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477
Panel B: State GDP/Capita and Unemployment Rate														
Seldom	-0.1412***	-0.7310***	0.1179***	-0.0561***	0.0517**	-0.0217***	0.0007	0.0016	0.0014	0.0107***	0.0201***	-0.0004	0.0061*	-0.0062**
	(0.0167)	(0.2490)	(0.0078)	(0.0232)	(0.0226)	(0.0043)	(0.0011)	(0.0013)	(0.0009)	(0.0036)	(0.0076)	(0.0045)	(0.0036)	(0.0029)
Often	-0.4123***	-2.5754***	0.2839***	-0.2766***	0.1886***	-0.0798***	0.0077***	0.0061**	-0.0006	0.0130*	0.0166	0.0192**	0.0183***	-0.0010
	(0.0381)	(0.5453)	(0.0164)	(0.0496)	(0.0468)	(0.0088)	(0.0025)	(0.0026)	(0.0015)	(0.0072)	(0.0156)	(0.0088)	(0.0077)	(0.0053)
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477
Panel C: Death of Relative or Close Friend														
Seldom	-0.1418***	-0.7382***	0.1182***	-0.0622***	0.0548**	-0.0215***	0.0006	0.0016	0.0015	0.0107***	0.0202***	-0.0006	0.0059	-0.0062**
	(0.0167)	(0.2490)	(0.0078)	(0.0233)	(0.0226)	(0.0043)	(0.0011)	(0.0012)	(0.0009)	(0.0036)	(0.0076)	(0.0045)	(0.0036)	(0.0029)
Often	-0.4130***	-2.5513***	0.2843***	-0.2841***	0.1945***	-0.0795***	0.0077***	0.0060**	-0.0005	0.0129*	0.0168	0.0189**	0.0179***	-0.0009
	(0.0381)	(0.5422)	(0.0164)	(0.0492)	(0.0468)	(0.0088)	(0.0024)	(0.0026)	(0.0015)	(0.0072)	(0.0155)	(0.0088)	(0.0077)	(0.0053)
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477
Panel D: Unrelated Worries & Satisfactions														
Seldom	-0.1147***	-0.5876**	0.1148***	-0.0585***	0.0521**	-0.0204***	0.0007	0.0016	0.0015*	0.0106***	0.0182**	-0.0010	0.0057	-0.0065**
	(0.0162)	(0.2485)	(0.0076)	(0.0233)	(0.0226)	(0.0043)	(0.0011)	(0.0013)	(0.0009)	(0.0036)	(0.0075)	(0.0045)	(0.0036)	(0.0029)
Often	-0.3672***	-2.3033***	0.2789***	-0.2829***	0.1902***	-0.0770***	0.0077***	0.0063**	-0.0005	0.0142**	0.0106	0.0168*	0.0168**	-0.0018
	(0.0371)	(0.5272)	(0.0161)	(0.0495)	(0.0467)	(0.0088)	(0.0024)	(0.0026)	(0.0015)	(0.0072)	(0.0155)	(0.0088)	(0.0077)	(0.0053)
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477
Panel E: All Confounders at Once														
Seldom	-0.1150***	-0.6131**	0.1148***	-0.0571***	0.0494**	-0.0203***	0.0007	0.0016	0.0015	0.0103***	0.0179**	-0.0008	0.0059	-0.0063**
	(0.0162)	(0.2486)	(0.0076)	(0.0231)	(0.0226)	(0.0043)	(0.0011)	(0.0013)	(0.0009)	(0.0036)	(0.0075)	(0.0045)	(0.0036)	(0.0029)
Often	-0.3678***	-2.3385***	0.2789***	-0.2753***	0.1844***	-0.0774***	0.0078***	0.0064**	-0.0006	0.0137*	0.0102	0.0174**	0.0172**	-0.0017
	(0.0371)	(0.5229)	(0.0161)	(0.0495)	(0.0469)	(0.0088)	(0.0025)	(0.0026)	(0.0015)	(0.0072)	(0.0155)	(0.0088)	(0.0077)	(0.0053)
Mean	7.379	51.071	0.932	2.308	2.326	0.799	0.008	0.010	0.004	0.135	0.965	0.270	0.141	0.122
N	57389	11127	57401	16212	16258	56531	56297	57954	57954	57954	57768	57477	57477	57477
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports robustness checks for the fixed effects estimations from Table B.1, Panel B, in which various outcomes (described in the table notes of Table B.1) are regressed on perceived discrimination, additional control variables (listed in Table A.2) and state and year fixed effects. Panel A: Estimations include month and day of week fixed effects. Panel B: Estimations include state GDP per capita and unemployment rates as controls. Panel C: Estimations controls for death of relatives or close friends. Panel D: Estimations include worries and satisfactions that should be unrelated to perceived discrimination, namely worries about the economy in general, satisfaction with own health, and satisfaction with housework. Panel E: Uses all control variables from Panels A-D combined.
Standard errors (in parentheses) are clustered at the person level. * $p < .10$, ** $p < .05$, *** $p < .01$