

The effect of exposure to ethnic minorities on ethnic preferences

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Abstract

We investigate the effect of exposure to ethnic minorities on the majority's preferences with regard to that group using individual-level panel data from the Netherlands. The data combine ethnic preferences with administrative data on refugee facilities. The study period is marked by a sudden inflow of refugees in some neighborhoods. We find that individuals living close to refugee facilities developed a more positive attitude toward ethnic minorities and became less inclined to support anti-immigration parties. Preferences of individuals living farther away remained unchanged. An investigation of channels suggests that the local effect is due to contact between residents and refugees.

JEL Codes: J15, R23, D91, C23

Keywords: attitudes toward ethnic diversity, voting, refugees, panel data, immigration, discrimination, prejudice, intergroup contact

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1 Introduction

Migration and demographic trends are leading to an increased presence of ethnic minorities in Western countries (de la Rica, Glitz, and Ortega, 2015; Hanson and McIntosh, 2016). Yet ethnic minorities encounter discrimination, which not only brings about unequal opportunities but also economic inefficiencies (Becker, 1957).¹ In the literature there has been an ongoing debate concerning the effects of exposure to ethnic minorities on the majority's preferences toward them, and this has so far generated mixed evidence.² The current paper uses novel individual-level panel data that shed new light on the debate. The data combine measures of attitudes toward ethnic diversity and political preferences with administrative information on the residential locations of respondents and refugees in the Netherlands.³ The study period is 2011 to 2016, and is characterized by a large, sudden influx of refugees during what is often referred to as the *European refugee crisis*.

We find that individuals who experienced an influx of refugees in their neighborhood developed a more positive attitude toward ethnic diversity and became less inclined to vote for the far-right party with its anti-immigration agenda as compared to individuals not exposed to refugees.⁴ The results are based on a sample that includes both individuals living in neighborhoods without any refugees and individuals living in neighborhoods that experienced an influx of refugees between 2014 and 2016. The neighborhoods of the latter group did not have any refugee facilities before 2014. Causal interpretation rests on the assumption that if the refugee crisis had not taken place, ethnic preferences of the two groups would have remained unchanged from previous years. This assumption is supported by statistical testing of the difference in (trends in) attitudes and political preferences between the two groups prior to the refugee influx. The estimation results are stable across a wide range of robustness checks.

To gain insight into the mechanisms driving the results, we first consider the role of contact by exploiting variation in proximity to refugee facilities and duration of exposure. We find

¹For example, ethnic minorities face discrimination in the labor market (Bertrand and Mullainathan, 2004; Bartoš, Bauer, Chytilová, and Matějka, 2016; Charles and Guryan, 2008; Heath, Liebig, and Simon, 2013; Hedegaard and Tyran, 2018), in education (Alan, Duysak, Kubilay, and Mumcu, 2021), in the legal system (Shayo and Zussman, 2011; Anwar, Bayer, and Hjalmarsson, 2012), in commerce (List, 2004; Ayres, Banaji, and Jolls, 2015), and in reciprocation of trust (Cettolin and Suetens, 2018), and are the subject of negative inaccurate stereotypes (Alesina, Miano, and Stantcheva, 2018).

²Preferences are more likely to develop in favor of ethnic diversity if exposure to minorities leads to positive interethnic contact (Allport, 1954) or to the acquisition of information that overcomes inaccurate stereotypes. An opposite effect may occur if exposure induces fears for a change in cultural norms (Card, Dustmann, and Preston, 2012), for a decay of the welfare state (Dustmann and Preston, 2007), or for an increase in labor market competition (Ortega and Polavieja, 2012). We discuss the mixed empirical evidence later in this section.

³The share of citizens in the Netherlands born or with at least one parent born in a non-Western country was equal to 13.4 percent in 2019. About 70 percent of the citizens with a non-Western background were born or have at least one parent born in a predominantly Muslim country (Statistics Netherlands, 2021). Migrant integration patterns in the Netherlands are similar to those in many other European countries; the unemployment rate, the risk of poverty or social exclusion, and the job overqualification rate among citizens in the Netherlands born in a non-Western country is several orders of magnitude higher than among native-born (Eurostat, 2021).

⁴A neighborhood in the Netherlands roughly corresponds to a large census tract in the US (between 2,500 and 9,000 residents).

that effects are highly localized; the presence of refugees outside one's direct neighborhood has essentially no effect on attitudes toward ethnic diversity and political preferences. We also find that the local effects are particularly strong if exposure lasts longer than six months. Close proximity and sufficiently long exposure are thus crucial in obtaining a positive effect, and both factors are associated with an increased likelihood of contact with ethnic minorities, either in the form of casual encounters or longer-lasting personal interaction. We consider alternative mechanisms, including improved local employment opportunities, increased confidence in the government in neighborhoods hosting refugees (e.g. due to increased public spending), and changes in preferences on matters unrelated to ethnic diversity, and find that none of these are able to explain the results.

The nature of the data makes it possible to rule out possible selection effects that could harm our identification. In particular, we find no evidence for *white flight*: very few individuals move to another neighborhood, and there is no indication that individuals move out of a neighborhood due to an influx of refugees.⁵ Furthermore, there is no selective attrition in our sample; the likelihood of answering questions on ethnic preferences does not depend on one's exposure to refugees. Another unique feature of our data is that they allow us to study whether effects of exposure depend on individual characteristics. A particularly noteworthy finding is that the positive effect of local exposure to refugees mostly stems from a change among individuals who were relatively right-leaning before the refugees arrived in their neighborhood; these individuals develop a more positive attitude toward ethnic diversity and turn away from the far-right party due to the exposure, whereas preferences of left-leaning individuals are largely unchanged.

Using data from a lab-in-the-field experiment, we show that exposure to refugees in one's neighborhood has a qualitatively similar effect on *revealed* preferences with regard to ethnic minorities. The experiment was designed to elicit an incentivized measure of ethnic discrimination and was carried out on a subset of individuals from the same panel used in the main estimations. It consisted of three waves: before, during and right after the refugee crisis (see Cettolin and Suetens, 2018, for a detailed analysis of the first wave). We find that exposure to refugees in one's neighborhood has a positive effect on the discrimination measure (but the effect is not statistically significant). In particular, exposure tends to increase the inclination of people with a majority background to reciprocate the trust originating from a person with a minority background relative to the trust originating from another majority member.

The current study is related to Bursztyn, Chaney, Hassan, and Rao (2021) who investigated the effect of long-term exposure to Arab-Muslims in the United States on the majority's attitudes and behavior towards them and their political preferences. Given that Europe and the US have different migration and societal backgrounds, results may very well differ. Moreover, the nature of the data and type of effect that can be identified differ between the two stud-

⁵The context is thus quite different from that in, for example, Card, Mas, and Rothstein (2008) and Boustan (2010), who provide evidence of white flight in certain locations in the United States.

ies. We use a combination of cross-sectional and individual-level variation in local exposure to immigrants to estimate immediate effects, whereas Bursztyn et al. (2021) use cross-sectional variation in historical migration patterns to establish long-run effects on current preferences.⁶ Despite these differences, the general finding is similar: exposure to minorities improves attitudes and behavior toward them and shifts political preferences away from parties with a conservative stance on immigration and multiculturalism.

Our paper is also related to studies that investigate the effect of exposure to ethnic minorities on political preferences. Identification strategies, samples, and time periods vary across studies, and the results are mixed (see Cools et al., 2021). A number of studies finds that an increased presence of ethnic minorities increases the electoral success of Far Right or anti-immigrant parties (see, for example, Barone et al., 2016; Bratti et al., 2020; Campo et al., 2020; Dinas et al., 2019; Dustmann et al., 2019; Halla et al., 2017; Edo et al., 2019; Tabellini, 2020; Vasilakis, 2018) and decreases support for redistribution (Dahlberg, Edmark, and Lundqvist, 2012).⁷ Other studies instead report that the popularity of anti-immigrant parties shrank in areas with larger minority populations (Billings, Chyn, and Haggag, 2021; Calderon, Fouka, and Tabellini, 2020; Schneider-Strawczynski, 2021; Steinmayr, 2021; Vertier, Viskanic, and Gamalerio, 2020), even decades after their first arrival (Schindler and Westcott, 2021). Others yet find no effect or find that votes for the Far Right *and* the Far Left increase due to (perceived) increased exposure (Colussi, Isphording, and Pestel, 2021; Schaub, Gereke, and Baldassarri, 2021).

Our paper complements this literature in two ways. First, the granular and longitudinal nature of our data enables a detailed analysis of the effect of exposure to ethnic minorities that cannot be performed using aggregate electoral data and that contributes to an understanding of the mechanisms underlying the effect. Specifically, we can study the role of proximity to a refugee facility and of the duration of exposure (and find that both are important moderating variables); we can study heterogeneous effects and thus identify subgroups in the population for whom the effects are particularly strong; we can closely study compositional effects (due to, for example, white flight). Second, we show that attitudes and behavior toward ethnic minorities are a key channel through which exposure influences political preferences (like Bursztyn et al., 2021). Voting data alone—while important—do not make it possible to identify this channel. For example, in the context of the European refugee crisis, increased support for Far Right parties may be driven by anti-EU sentiment based on a dissatisfaction with the European Union’s response to the crisis rather than being the result of a negative attitude toward ethnic diversity *per se*.

Finally, the current paper is related to the strand of the literature that focuses on the effect of personal contact with minorities on the majority’s attitudes and behavior (inspired by Allport, 1954). Some of these studies exploit random natural variation in exposure to minor-

⁶See also Billings et al. (2021) on the long run effects of exposure to ethnic diversity in the USA. The authors find that early life contact with ethnic minorities decreases the likelihood of registering as a Republican by almost 9%.

⁷Also related is Freddi (2021) finding that exposure to refugees induces people to avoid information that may encourage welcoming them.

ity (class)roommates during an educational program (Boisjoly et al., 2006; Carrell et al., 2019; Corno et al., 2019; Merlino et al., 2019).⁸ Others use field experiments carried out on soldiers, members of sports teams or students (Finseraas, Hanson, Johnsen, Kotsadam, and Torsvik, 2019; Finseraas and Kotsadam, 2017; Lowe, 2021; Mousa, 2020; Scacco and Warren, 2018). A common finding is that contact with peers that have a minority background leads to a more positive attitude toward minorities among the majority.⁹ The current study complements this literature by focusing on the effect of contact with minorities on the preferences of a general-population sample, in a context where the contact is most likely not as close. In the light of the findings by Hangartner, Dinas, Marbach, Matakos, and Xefferis (2019) who show that exposure to refugees on Greek islands has strengthened anti-minority sentiment among local residents, it is not all that clear whether the positive effects maintain. The remainder of the paper is organized as follows: Section 2 describes the Dutch refugee policy and the country's political context. The data and the empirical strategy are presented in Section 3 and 4, respectively. Section 5 presents the main results, Section 6 discusses possible mechanisms, and Section 7 describes the results of our lab-in-the-field experiment. Section 8 concludes.

2 Background

2.1 Refugees in the Netherlands

Ethnic minorities in the context of our study are refugees who sought asylum in the Netherlands during the European refugee crisis.¹⁰ Upon arrival, refugees registered at a central reception center in the Netherlands. They normally stayed there for two weeks before being assigned to one of the many refugee accommodation facilities in the country. The Dutch Central Body for Sheltering Asylum Seekers (COA) is responsible for the intake and supervision of refugees, and it assists those who have been granted asylum in finding a facility.¹¹

The refugee facilities are typically located in or near residential areas. By law, a facility must be within 500m of public transportation and within 3km of schools and shops. During the first six months while waiting for a decision on their status, refugees are not allowed to work for pay but may do voluntary work. After six months, they can work for pay for up to twenty-four weeks per year. They receive weekly pocket money for groceries and other necessities if they do not have the means to provide for themselves, and are insured for basic health care. Any

⁸See also Albrecht and Smerdon (2022) for a study on the effect of an unexpected inflow of refugees in a rural town in Australia on interethnic trust and attitudes toward refugee settlement, and Rao (2019) for a study on the effect of exposure to poor classmates in Indian schools on discrimination against poor students by rich ones.

⁹An exception is Elwert, Keller, and Kotsadam (2020) who find no effect of interethnic contact on ethnic discrimination in a large-scale field experiment.

¹⁰For the sake of brevity, throughout the paper we use 'refugees' as a synonym to 'asylum seekers' in the legal sense of the word.

¹¹A detailed description of the asylum process in the Netherlands can be found at <https://www.coa.nl/> and <https://ind.nl/en/asylum/Pages/Asylum-seeker.aspx>.

Figure 1: Refugee facilities in the Netherlands



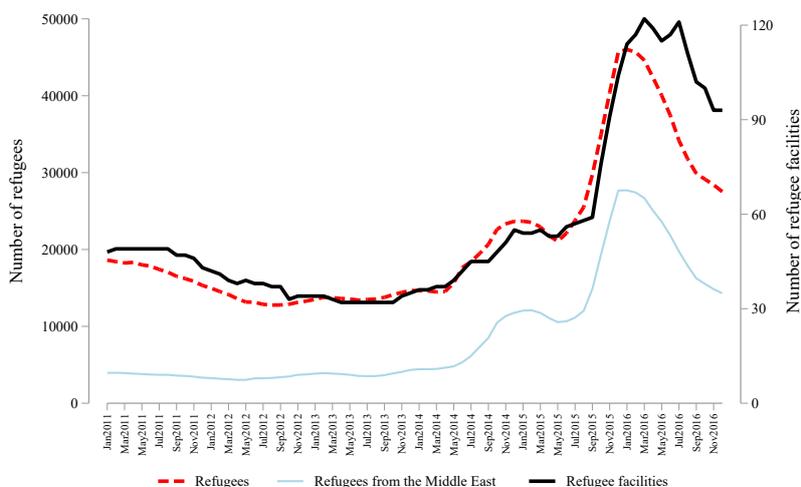
Notes: From top left in clockwise order: accommodations in a former bungalow park; a play room for children; a Dutch class; refugees share traditional food with locals; a refugee is taught how to ride a bike; a computer learning room.

money they earn is deducted from the weekly allowance. Refugees learn about behavioral and cultural aspects and norms in the Netherlands, attend a course to learn Dutch, and children go to local schools. Photos of a typical facility and of its activities are shown in Figure 1.

The total number of facilities varies at any given point in time depending on need. Figure 2 shows that at the beginning of 2011, less than 20,000 refugees were waiting for a decision on their status in one of 48 facilities in the country. During 2012 and 2013, the number of refugees decreased to less than 15,000 who were housed in 35 facilities. In mid-2014, the impact of the Syrian civil war increased the number of facilities to about 50. September 2015 saw a surge in the number of refugees and marked the beginning of the European refugee crisis. As can be seen in Figure 2, the number of refugees living in facilities in the Netherlands eventually tripled relative to the beginning of 2014, mostly due to the inflow from the Middle East. At the peak of the crisis, the COA operated around 120 facilities; Figure 3 shows the geographic distribution of the facilities over time.

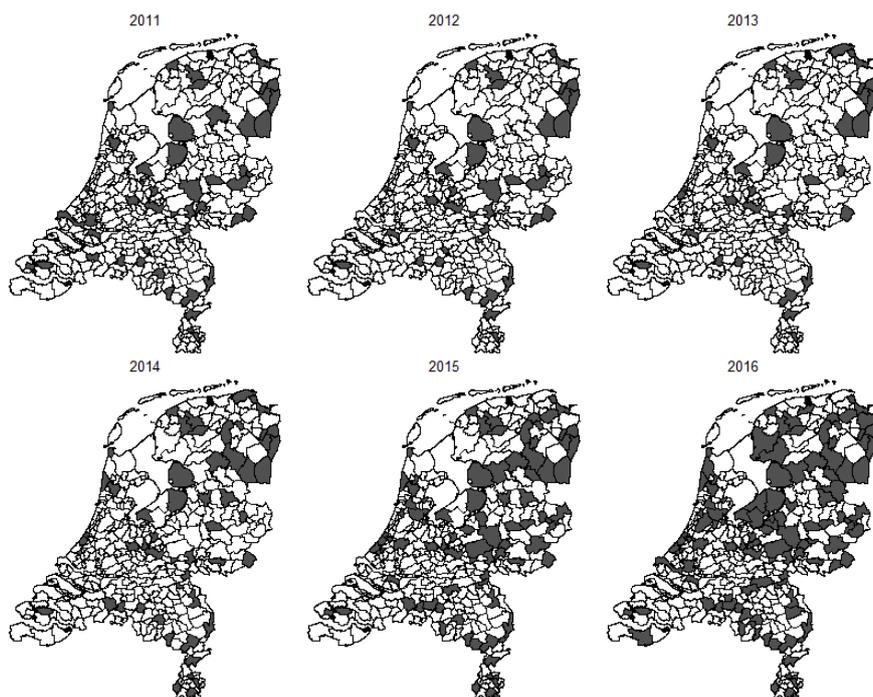
If the number of incoming refugees exceeds the capacity of existing facilities, then the COA opens facilities in new locations in cooperation with the municipalities. If there is excess capacity, then the COA can decide to close facilities down. According to Dutch law, the COA must endeavor to distribute refugees throughout the country, in order to spread the burden and fa-

Figure 2: Number of refugees and refugee facilities



Notes: Number of asylum seekers residing in refugee centers (left axis) and number of refugee facilities (right axis) in the Netherlands between January 2011 and December 2016.

Figure 3: Geographical distribution of refugees in the Netherlands



Notes: The distribution of refugee facilities across municipalities in the Netherlands from 2011 to 2016.

cilitate integration. Once a deal is made between the COA and a municipality regarding the hosting of refugees, a contract is signed that specifies the arrangements (exact location, maximum capacity, opening and closing date, etc.). Between 2014 and 2016 almost all facilities were set up in existing buildings (former schools, old-age homes, vacation homes, prisons, etc.), or in temporary prefab units. The buildings are usually owned by the municipalities, although sometimes they are privately owned.

2.2 The political landscape in the Netherlands

The Netherlands is both a parliamentary democracy and a constitutional monarchy, such that the King and the ministers together make up the government. Numerous parties compete in parliamentary elections and no single party has ever secured an overall majority of votes. As a result, two or more parties must form a coalition government. Parliamentary elections are usually held every four years.¹² Table 1 provides an overview of the election outcomes in 2010, 2012 and 2017 for parties that won at least one seat. The last column of the table provides an indicator of the party's position on immigration and integration based on data collected by the Manifesto Project, an acclaimed research and data collection project in comparative politics (see Burst et al., 2020). By using a unified approach based on content analysis of political platforms, the project measures political parties' positions on a range of economic, social and other issues and how much weight they receive in the platforms. The index we report is based on the frequencies of positive and negative statements regarding immigration and multiculturalism in the parties' platforms in March 2017. The larger the index, the more positive a party's platform is on these issues. The statements refer to the extent to which parties view immigration as a threat, the extent of their support for cultural diversity and plurality, or to whether they are in favor of providing opportunities to integrate versus inducing full assimilation.

The largest parties are the Party for Freedom (PVV) with an explicitly anti-immigrant agenda, and the People's Party for Freedom and Democracy (VVD), a conservative-liberal party. PVV actively seeks to halt immigration, especially that from non-Western countries. In fact, it has become increasingly radical on this issue during the past 10 years, and is now one of Europe's most extreme anti-immigrant and anti-Muslim parties. VVD's position on immigration and ethnic diversity has been consistently uncompromising: asylum seekers are welcome up to a certain limit, but illegal immigrants need to be fined or imprisoned; social security benefits should be fully available only to Dutch nationals; and immigrants are expected to integrate at their own expense on their way to becoming citizens. It also advocates a European policy of tight border controls.

As can be seen in Table 1, the Center and Left of the political spectrum are much more fragmented than the Right. The largest party in the Center is the Christian Democratic Appeal (CDA), whose stance on immigration and diversity partially echoes that of VVD. According to their program, refugees can be better assisted in their home countries, and those who are already in the Netherlands need to eventually be returned to their home country. Immigrants need to integrate, learn Dutch and be willing to give up their nationality in order to become Dutch citizens. Compared to VVD, CDA is more willing to invest in small-scale facilities for asylum seekers. Other (smaller) parties with a stance on immigration and diversity similar to that of CDA include the Reformed Political Party (SGP) and 50PLUS (50+).

The historically largest party on the Left is the social-democratic Labor Party (PvdA). On

¹²The Dutch Parliament consists of two chambers: the Senate and the House of Representatives. Only the members of the latter are directly elected by Dutch voters. It is these elections that we refer to as parliamentary elections.

Table 1: Election outcomes and party policy on immigration

Political party	Vote share (in %)			Manifesto index
	2010	2012	2017	
Party of Freedom (PVV)	15.4	10.1	13.1	-40
Forum for Democracy (FvD)	-	-	1.8	-6.275
People's Party for Freedom and Democracy (VVD)	20.5	26.6	21.3	-5.405
Reformed Political Party (SGP)	1.7	2.1	2.1	-2.899
Christian Democratic Appeal (CDA)	13.6	8.5	12.4	-2.544
50Plus	-	1.9	3.1	-2.424
Socialist Party (SP)	9.8	9.7	9.1	-0.351
Party for the Animals (PvdA)	1.3	1.9	3.2	-0.14
Christian Union (CU)	3.2	3.1	3.4	0.836
Green Left (GL)	6.7	2.3	9.1	1.235
Democrats'66 (D66)	6.9	8.0	12.2	1.943
Labour Party (PvdA)	19.6	24.8	5.7	3.607
THINK (DENK)	-	-	2.1	6.407

Notes: Only political parties that attained at least one seat in the parliament are included. The Manifesto index is equal to the sum of positively phrased items related to multiculturalism and the arrival of new immigrants (per607, per602.2) less the sum of scores on negatively phrased items (per608, per601.2).

the issue of immigration, PvdA maintains a fairly rigid policy with respect to economic immigrants, but favors a softer asylum policy, including special provisions for the most vulnerable, especially children, and financial support to facilitate integration. PvdA advocated a European solution to the refugee crisis and supported a deal between the EU and Turkey in March 2016, which provided financial incentives to Turkey to absorb asylum seekers and prevent them from continuing on to Europe. After five years as part of the coalition government, support for PvdA fell dramatically in 2017, while Democrats 66 (D66) and GreenLeft (GL) both gained votes. D66, and even more so GL, have adopted a more welcoming approach to refugees relative to PvdA.

The country has been governed since 2010 by a coalition headed by VVD, which gained the majority of the votes in 2010, 2012 and 2017 (and 2021). Mark Rutte, the VVD leader, has served as prime minister during this period. The first coalition government in 2010 included CDA and PVV. The coalition dissolved after PVV withdrew its support in 2012, and, following new elections PvdA entered the coalition. After the 2017 elections, VVD formed a coalition government with CDA, D66 and CU.

3 The data

3.1 Data sources

We use data from three sources and merge the data based on the year and the neighborhood (by means of the four-digit postal code). The first dataset, provided by the COA, includes the opening and closing dates of refugee facilities, the precise locations of the facilities, and the monthly number of asylum seekers in each facility during the period January 2011 to December 2016.

The second data source is the Longitudinal Internet Studies for the Social Sciences (LISS),

consisting of panel data of Dutch citizens created for research purposes. The panel is managed by CentERdata, a research institute located on the campus of Tilburg University. This dataset consists of about 7000 individuals in 4500 households and is based on a true probability sample of households drawn from the population register of Statistics Netherlands.¹³ Panel members complete online questionnaires, which take about 15 to 30 minutes, and they are paid for each completed questionnaire. We use the LISS Core Survey on Politics and Values to obtain data on attitudes toward ethnic diversity and political preferences (see Section 3.2). Between 2011 and 2016 the survey was run each year in December except in 2014, when it was not administered. From the LISS we also use individual-level background variables such as neighborhood of residence.¹⁴

The third data source is Statistics Netherlands (CBS), which publishes annual sociodemographic data on municipalities and neighborhoods. Specifically, we use population data by postcode (“BevolkingPerPostcode”) and additional data on neighborhoods and quarters (“Toelichting Wijk- en Buurtkaart”), which are only available for 2014, 2015, and 2016.

Table 2 provides descriptive statistics for a range of neighborhood characteristics for the Netherlands as a whole, the LISS sample, and the sample that we use in our main estimations. The statistics are based on the period 2011-2013, prior to the large influx of refugees.¹⁵ The table shows that neighborhoods in the LISS sample and our sample have similar characteristics to those in the Netherlands, apart from having a larger population and covering a smaller area on average.

3.2 Dependent variables

Our first dependent variable measures attitudes toward ethnic diversity in society. The variable is based on the extent to which individuals agree with statements related to immigration and ethnic diversity in society that are included in the LISS Core Survey on Politics and Values. The survey contains eight statements that respondents rate on a five-point Likert scale. We selected those (six) statements reflecting normative views (see Table 3).¹⁶ After coding negatively framed questions in reverse, we average the ratings to obtain a single standardized

¹³The households in the panel are recruited as follows: first, they are sent a letter and a brochure explaining the nature of the panel study. Subsequently, they are either contacted by phone or visited at home. Households that have no internet connection are provided with the necessary technology in order to participate by means of their television. Respondents’ willingness to participate in the panel is generally high: in total, 48% of the sample contacted in the first step agrees to participate. See <https://www.lissdata.nl> for detailed information about the panel.

¹⁴To safeguard the privacy of the respondents, CentERdata made the complete dataset available to us for analysis via remote access. The LISS data are publicly available in a format that does not contain information regarding the respondents’ location of residence.

¹⁵Notice that the difference between our sample and the full LISS sample is primarily due to the imbalanced nature of the panel data and the selection of individuals who did not move between 2014 and 2016 (see Section 3.3 for further details).

¹⁶We do not include the following two statements because they are rather descriptive in nature: (a) It is difficult for a foreigner to be accepted in the Netherlands while retaining his/her own culture; and (b) People of foreign origin or descent are not accepted in the Netherlands.

Table 2: Neighborhood characteristics

	<i>The Netherlands</i> Mean (S.E.)	<i>LISS</i> Mean (S.E.)	<i>Sample</i> Mean (S.E.)
Number of inhabitants (unweighted)	4,145.792 (64.802)	6,408.214 (86.195)	6,624.704 (99.141)
Number of inhabitants (weighted)	8,229.849 (68.609)	8,999.458 (86.658)	9,230.025 (100.445)
Share of non-Western immigrants	0.116 (0.002)	0.125 (0.003)	0.125 (0.003)
Age distribution			
below 20 years	0.233 (0.001)	0.232 (0.001)	0.232 (0.001)
20-40 years	0.248 (0.001)	0.254 (0.002)	0.255 (0.002)
40-60 years	0.293 (0.001)	0.290 (0.001)	0.290 (0.001)
60-80 years	0.185 (0.001)	0.183 (0.001)	0.182 (0.001)
above 80 years	0.041 (0.000)	0.041 (0.001)	0.041 (0.001)
House value (thousands of euros)	229.399 (1.281)	225.638 (1.526)	226.362 (1.617)
Income per inhabitant (thousands of euros)	22.399 (0.066)	22.416 (0.084)	22.532 (0.093)
Proportion of inhabitants receiving benefit	0.069 (0.000)	0.069 (0.000)	0.068 (0.000)
Distance to a doctor (in km)	0.953 (0.012)	0.851 (0.011)	0.838 (0.012)
Distance to a supermarket (in km)	0.883 (0.011)	0.786 (0.010)	0.778 (0.011)
Distance to a school (in km)	0.653 (0.005)	0.620 (0.005)	0.617 (0.005)
Area (in km ²)	1.90 (0.043)	1.714 (0.050)	1.669 (0.053)
Population density			
very low	0.025 (0.002)	0.009 (0.002)	0.008 (0.002)
low	0.072 (0.004)	0.045 (0.004)	0.041 (0.005)
middle	0.172 (0.006)	0.150 (0.008)	0.147 (0.009)
high	0.322 (0.008)	0.343 (0.010)	0.333 (0.011)
very high	0.409 (0.008)	0.452 (0.011)	0.471 (0.012)
Vote share Far Right 2012	0.101 (0.001)	0.101 (0.001)	0.101 (0.001)
Clusters	4,033	2,236	1,757

Notes: Based on pre-exposure years (2011-2013). Statistics weighted by neighborhood population. Vote share Far Right 2012 refers to the parliamentary election outcome of the PVV in 2012.

index, which we refer to as the *attitude index*. The standardized scores range from -2.87 (the most negative attitude toward ethnic diversity) to 3.16 (the most positive).¹⁷ Within the index, the ratings of the statements are significantly correlated with each other, with correlation coefficients ranging between 0.210 and 0.554. Moreover, Cettolin and Suetens (2018) show that the index is significantly correlated with discrimination against ethnic minorities elicited in a lab-in-the-field experiment, suggesting that the index is also behaviorally relevant.

The second dependent variable measures political preferences, as reflected in voting intentions included in the LISS Core Survey on Politics and Values. In the survey, individuals are asked which political party they would vote for if a national election was called.¹⁸ We focus on the intention to vote for the Far Right, i.e. the PVV or the FvD. We do so for two reasons. First, the Far Right is the most relevant for our purposes since in contrast to the other parties almost the entire platforms are built around the themes of migration and multiculturalism. Second,

¹⁷In Section A of the appendix, we show that our main results are robust to redefining the attitude index using principal component analysis and polychoric correlation (see Table A.8 and A.9).

¹⁸A worry could be that individuals who are unwilling to answer this question have different political values than individuals who do not mind answering. In Section 4.4 we show this is not the case.

Table 3: Survey items related to attitudes toward ethnic diversity

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1. It is desirable for a society to consist of people from different cultures.
 2. It should be made easier to obtain asylum in the Netherlands.
 3. Legally residing foreigners should be entitled to the same social security system as Dutch citizens.
 4. There are too many people of foreign origin or descent in the Netherlands.
 5. It does not help a neighborhood if many people of foreign origin or descent move in.
 6. Some sectors of the economy can only continue to function because people of foreign origin or descent work in them.
-

Notes: Statements on ethnic diversity and immigration in the LISS Core Survey on Politics and Values. Respondents are asked to what extent they agree with each of these statements on a seven-point scale.

the Far Right has not been part of the national government in the Netherlands in our period of study, and can therefore not be held responsible by voters for particular policy choices of which the timing coincides with the European refugee crisis.

To gauge overall voting intentions, we also use a *political index*, which is a standardized version of the Manifesto index (c.f. Table 1) associated with the party individuals report to vote for. The advantage of this index is that it combines information for all the parties while taking into account their stance on ethnic diversity and multiculturalism and how much weight they attach to the issue. For example, a standardized score of -2.304 (0.904) implies an intention to vote for PVV (DENK), which is the party with the strongest anti-immigrant (pro-immigrant) agenda.¹⁹

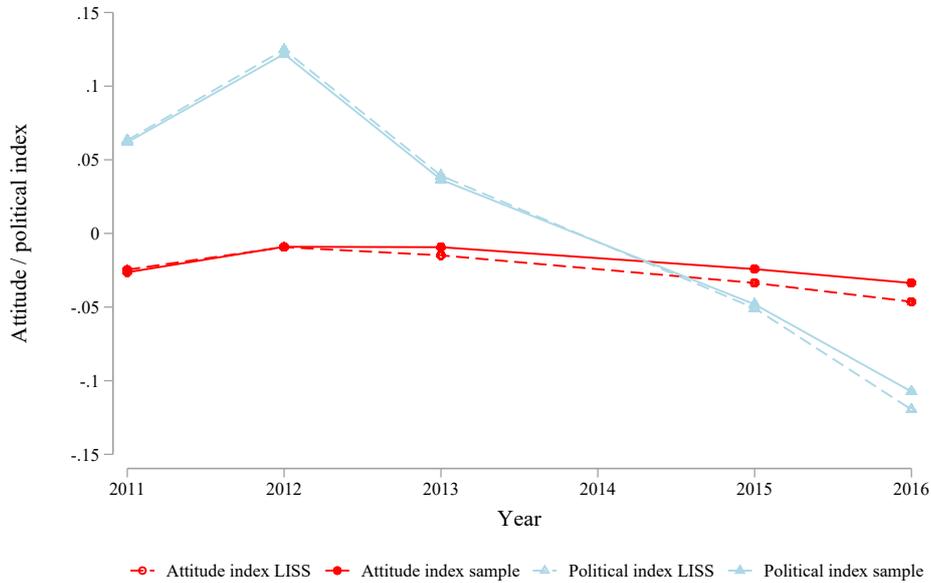
To which extent do self-reported political preferences match election outcomes? To answer this question, we look at correlations of self-reported and actual outcomes in parliamentary elections at the municipality level in years in which such elections were held (i.e., 2010, 2012 and 2017). Across these years, the correlation between self-reported and actual voting for the Far Right is 0.437 and the correlation between our political index and an index calculated using the actual voting outcomes is 0.463.²⁰ This shows that the self-reported data match the actual outcomes well, especially given that political preferences were measured up to six months prior to the actual elections.

The summary statistics of the dependent variables, including means, standard deviations and sample sizes, can be found in Table A.1 in the appendix. Figure 4 shows the evolution over time of the attitude and political indexes. After increasing in 2011 and 2012, both indexes trended downward during the sample period. The attitude index fluctuates to a lesser extent than the political index, which is possibly due to the political index reflecting voters' general preferences, including those on issues unrelated to immigration and multiculturalism.

¹⁹In 2016, the voting intentions of half of the respondents were elicited using a slightly different question, which asked them to state the likelihood (in percent) of them voting for each party. Our results are robust to including recoded answers to the reformulated question (see Section 5.2). In combination with the fact that questions on voting intentions are typically answered by fewer individuals, a smaller number of observations is obtained than in the case of the attitude index.

²⁰The correlations are calculated based on data from municipalities for which we have at least 10 respondents in a given year.

Figure 4: Evolution of the attitude index and the political index



Notes: The attitude index and the political index for the LISS panel (solid lines) and the estimation sample (dashed lines) in the period 2011-2016.

Political preferences are, in addition, also influenced by the (un)popularity of specific political candidates.

3.3 Estimation sample

Our sample consists of annual data for the period 2011-2016 (excluding 2014). To match the monthly refugee data with the annual data on individuals' preferences, we collapse the information on refugees into annual variables. The sample is composed of individuals born in the Netherlands living in municipalities that did not host refugees prior to 2014. It consists of two groups of individuals. The first group of individuals lives in neighborhoods that hosted refugees in 2014, 2015 or 2016 (the *Treatment* group, $N = 203$). The second group of individuals lives in neighborhoods not hosting refugees throughout the entire sample period (the *Control* group, $N = 5,227$).²¹ Neighborhoods correspond to four-digit postal codes, and on average represent an area of about 1.7 km² (about 0.66 square miles) with a population of roughly 9000 residents. Individuals in *Treatment* (*Control*) are spread across 47 (1,710) different neighborhoods and *Control*.²²

We focus on individuals who did not move to another neighborhood or municipality between 2014 and 2016, i.e. during the years of the refugee crisis. This guarantees that individuals

²¹These are the numbers of individuals for the attitude regressions. The corresponding numbers for the regressions on political preferences are 165 and 4,339, respectively.

²²In the regressions on political preferences, the numbers are 46 and 1,601, respectively.

Table 4: Descriptive statistics for individuals in *Treatment*

	2014	2015	2016	Overall
Median exposure in months	3	2	8	6
Mean exposure in months	4	5.4	9.4	7.7
Median number of refugees in neighborhood	303.0	318.2	214.0	228.3
Mean number of refugees in neighborhood	261.9	390.4	263.3	301.6
Median share of refugees in neighborhood	0.026	0.040	0.025	0.026
Mean share of refugees in neighborhood	0.041	0.054	0.043	0.046

Notes: The number of refugees in year t in a particular neighborhood is calculated by taking the mean/median of the number of refugees accommodated in a neighborhood across months in which there is at least one refugee present in the neighborhood. The number of months of exposure refers to the cumulative number of months in which refugees were present in the neighborhood. The first four cells for 2014 are empty because the LISS Core Survey on Politics and Values was not carried out in 2014.

are coded correctly as exposed or unexposed and that in a given year all individuals living in the same neighborhood are exposed to the same number of refugees for the same length of time.²³ Notice that very few people in the Netherlands moved to another neighborhood in these three years (see Section 4.4.3 for an elaborate discussion). Moreover, an intention-to-treat analysis that includes individuals who move yields statistically significant effects that are only slightly smaller than those obtained in our main analysis. This is exactly what can be expected if there is a true effect of exposure (see Section 5.2 and Table A.16 in the appendix for details).

3.4 Descriptive statistics

Table 4 presents descriptive statistics for the individuals in the *Treatment* group. On average, they were exposed for 8.3 months (median of 6 months), and the average length of exposure increased from a bit less than 6 months in 2015 to more than 10 months in 2016. Conditional on having a refugee facility, the average neighborhood hosted 288 refugees (median of 254). The average number of refugees hosted in a neighborhood declined substantially from 2015 to 2016, which is in line with the fact that the facilities that were opened at the peak of the crisis (in the Netherlands, first half of 2016) tended to be smaller in size and hosted fewer refugees.

We next provide the results of a series of balancing tests based on neighborhoods or individuals included in our main estimation sample. Panel A of Table 5 compares a variety of neighborhood characteristics between *Control* and *Treatment*. The statistics are based on the period 2011-2013, prior to the influx of refugees. As can be seen in the table, the average population size in *Treatment* is substantially larger than that in *Control*. This is in line with COA's mandate to match the distribution of refugees to that of the population, resulting in more populous

²³Consider the following fictitious example: person A moved from neighborhood x in year t to neighborhood y in year $t + 1$; person B always lived in neighborhood y ; refugees arrived in neighborhood y in year $t + 1$. If A moved to neighborhood y several months after the refugees arrived, then including A in the treatment group could bias the results because A had a less intense exposure experience than person B. Including A in the control group (according to their pre-treatment location, as is done in an intention-to-treat analysis) would also give a biased estimate of exposure because A actually *was* exposed.

Table 5: Neighborhood and individual characteristics

	<i>Control</i> Mean (S.E.)	<i>Treatment</i> Mean (S.E.)	<i>N</i>
<i>A. Neighborhood characteristics</i>			
Number of inhabitants (unweighted)	6,581.550 (100.524)	8,012.138 (578.788)***	1,757
Number of inhabitants (weighted)	9,185.959 (102.220)	10,387.687 (533.811)**	1,757
Share of non-Western immigrants	0.125 (0.003)	0.142 (0.017)	1,757
House value (thousands of euros)	226.754 (1.647)	216.147 (8.562)	1,720
Income by inhabitant (thousands of euros)	22.529 (0.095)	22.626 (0.492)	1,726
Proportion of inhabitants receiving benefit	0.068 (0.000)	0.072 (0.002)	1,726
Distance to a doctor (in km)	0.841 (0.012)	0.762 (0.062)	1,729
Distance to a supermarket (in km)	0.782 (0.011)	0.659 (0.057)**	1,729
Distance to a school (in km)	0.617 (0.005)	0.623 (0.028)	1,725
Area (in km ²)	1.681 (0.054)	1.364 (0.279)	1,729
Population density			
very low	0.008 (0.002)	0.000 (0.011)	1,729
low	0.042 (0.005)	0.014 (0.025)	1,729
middle	0.149 (0.009)	0.096 (0.045)	1,729
high	0.332 (0.012)	0.381 (0.060)	1,729
very high	0.470 (0.012)	0.509 (0.063)	1,729
Vote share Far Right 2012	0.101 (0.001)	0.097 (0.006)	1,729
<i>B. Individual characteristics</i>			
Attitude index	-0.008 (0.014)	-0.049 (0.064)	4,675
Political index	0.057 (0.015)	0.144 (0.068)	3,723
Far Right	0.132 (0.005)	0.095 (0.024)	3,723
Voter turnout	0.901 (0.004)	0.927 (0.020)	4,143
Male	0.463 (0.007)	0.472 (0.039)	4,678
Age	50.100 (0.260)	52.815 (1.370)*	4,678
Number of children	0.850 (0.017)	0.739 (0.091)	4,678
Education			
primary school	0.096 (0.004)	0.116 (0.025)	4,676
junior high school	0.252 (0.006)	0.281 (0.035)	4,676
high school	0.111 (0.005)	0.133 (0.026)	4,676
junior college	0.231 (0.006)	0.181 (0.030)	4,676
college	0.232 (0.006)	0.184 (0.030)	4,676
university	0.079 (0.004)	0.106 (0.024)	4,676
Marital status			
married	0.582 (0.007)	0.569 (0.039)	4,678
separated	0.005 (0.001)	0.000 (0.000)	4,678
divorced	0.085 (0.004)	0.043 (0.016)*	4,678
widow(er)	0.053 (0.003)	0.081 (0.022)	4,678
never married	0.276 (0.007)	0.306 (0.036)	4,678

Notes: The table shows averages and standard errors based on pre-exposure years (2011-2013) across neighborhoods (panel A, weighted by neighborhood population) and across individuals (panel B). *N* denotes number of neighborhoods in panel A and number of individuals in panel B. Vote share Far Right 2012 refers to the parliamentary election outcome of the PVV in 2012. Statistical significance of difference between *Control* and *Treatment*: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

neighborhoods hosting more refugees. Neighborhoods that hosted refugees are also slightly closer to a supermarket, and are characterized by a somewhat higher population density (although statistically the difference is not significant). In the empirical analysis we control for

these differences between neighborhoods by including individual-level fixed effects.²⁴ Two other noteworthy observations are that refugees are not located in remote areas and that neighborhoods in *Treatment* are not peculiar as compared to neighborhoods in the Netherlands in general (see Table 2).

Panel B of Table 5 provides the statistics for a number of individual-level variables for the *Control* and *Treatment* group in the period 2011-2013. Individuals in *Treatment* tend to be slightly older and are somewhat more often divorced. These differences do not invalidate our identification strategy; in the estimations we control for possible time-invariant differences between both groups by including individual-level fixed effects. Importantly, none of the dependent variables differs significantly at the 5% level between the two groups of individuals.

4 Empirical strategy

4.1 Estimation strategy

Our estimation strategy is built around the panel structure of the dataset. By including individual fixed effects in the regressions we control for stable unobservable differences in ethnic preferences between individuals in *Control* and *Treatment*. The regression results can thus be interpreted as relating changes in preferences to changes in exposure. Equation 1 represents our main regression specification, where subscript *int* refers to individual *i* living in neighborhood *n* in year *t*:

$$Y_{int} = \alpha_i + \tau_t + \beta_1 Exposed\ close_{nt} + \gamma X_{int} + e_{int}. \quad (1)$$

Y_{int} corresponds to one of the three dependent variables discussed in Section 3.2 (attitude index, political index, or a binary variable referring to the intention to vote for the Far Right), α_i are individual fixed effects and τ_t are year fixed effects. $Exposed\ close_{nt}$, our main independent variable, is a binary variable indicating whether refugees were accommodated for at least one month in neighborhood *n* during year *t*.²⁵ X_{int} is a vector of control variables that includes time-varying individual and neighborhood characteristics. The individual-level covariates include age, age squared, the number of children in the household, and categorical variables referring to highest level of education attained and marital status. The neighborhood covariates include the number of registered inhabitants in the neighborhood and in the municipality. In all regressions we use heteroskedasticity-robust standard errors that account for clustering at the neighborhood level.

We also estimate a specification that allows detecting an effect of exposure to refugees who

²⁴Individual-level fixed effects control for time-invariant differences between neighborhoods because individuals are nested in neighborhoods, making neighborhood-level fixed effects redundant.

²⁵This variable coincides with the time-invariant *Treatment* group indicator multiplied by a binary variable equal to 1 in the exposure years and thereafter. The *Treatment* variable is, however, collinear with the individual fixed effects and the post-exposure variable is collinear with the year fixed effects.

reside farther away in the municipality:

$$Y_{int} = \alpha_i + \tau_t + \beta_1 Exposed\ close_{nt} + \beta_2 Exposed\ far_{nt} + \gamma X_{int} + e_{int}, \quad (2)$$

where $Exposed\ far_{nt}$ is a binary variable indicating whether refugees were hosted in the same municipality as individual i but not in the same neighborhood (so other than n) for at least one month during year t .

4.2 Identifying assumption

To allow for a causal interpretation of the effect of exposure, we rely on the parallel-trend assumption; we assume that preferences of individuals in the *Treatment* group (who live in neighborhoods that would later host refugees) and the *Control* group (who live in neighborhoods without refugees) would have followed a similar pattern if the refugee crisis had not taken place conditional on the individual and time fixed effects and the controls.

The main threat to identification is that there are unobserved differences between individuals in *Treatment* and *Control*, unrelated to exposure to refugees, that could cause attitudes and preferences to evolve differently. For example, individuals in *Treatment* may have developed or started to develop a less hostile attitude towards ethnic minorities as compared to individuals in *Control* already before the refugees arrived. There are a number of reasons why this is unlikely to be the case. First, the balancing tests reported in Table 5 do not reveal worrisome imbalances between both groups of individuals. Second, in the case of a violation of the parallel-trend assumption, we should observe a diverting pattern in the dependent variables between *Treatment* and *Control* in the years prior to 2014. As shown in Section 4.3, we find no indication for such a pattern.

More in general, the European refugee crisis created a humanitarian emergency in many countries, including the Netherlands, and exerted an unprecedented pressure on reception facilities. Consequently, quick decisions had to be made and refugees were mostly placed in existing empty buildings, which were converted for that purpose.²⁶ Although the total share of refugees in the population increased substantially in this period of time, the new facilities typically had a smaller capacity and hosted fewer refugees than longer-term facilities planned well ahead.²⁷ Moreover, conversations with COA employees revealed that none of the new refugee facilities that closed down before the end of 2016 did so because of resistance in the neighborhood. Overall, these elements give us confidence that the presence of refugees focused on in this paper did not give rise to strong anticipatory reactions, either in favor or against, by the local population.

²⁶Only twelve percent of the refugee facilities that opened up between 2014 and 2016 in the Netherlands consisted of newly built units or pavilions, typically of a temporary nature.

²⁷To illustrate, on average 278 refugees were hosted in neighborhoods in the period 2014-2016 with a newly opened facility in that period as compared to 479 refugees in neighborhoods with a facility that opened before 2014 and still in place in 2016.

4.3 Pre-trend tests

We examine the plausibility of the parallel-trend assumption by testing whether during the period before the refugee crisis preferences of individuals in *Treatment* are on a different trend than those of individuals in *Control*. To do so, we run regressions based on pre-exposure data of individuals in *Treatment* and all data of individuals in *Control*.

We first regress the preferences variables on year fixed effects and year fixed effects interacted with a *Treatment* indicator, while controlling for individual fixed effects and covariates used in the main analysis. This allows us to test whether pre-exposure preferences differed between *Treatment* and *Control* during the period before individuals in *Treatment* were exposed. In a second set of regressions we test for pre-exposure differences between *Treatment* and *Control* by regressing the preferences variables on a linear time trend and a trend interacted with the *Treatment* indicator. We run both sets of regressions for 2011-2016, which is the sample period of our main analysis, and for 2008-2016, which includes several years prior to the sample period.²⁸ The results are presented in Table 6. Overall, pre-exposure patterns do not differ significantly between *Treatment* and *Control*; none of the interaction terms is statistically significant.

The same conclusion holds if we use parallel-trend tests based on the estimators developed by Borusyak, Jaravel, and Spiess (2021), which constitute a recent advance in difference-in-difference estimation for dynamic treatment onset. Indeed, as shown in Table A.15 in the appendix, pre-exposure patterns are statistically not significant.

Albeit statistically insignificant, for the attitude index the estimated interaction between *Treatment* and 2015 is somewhat larger than other interactions (panel A or C of Table 6). This may indicate that attitudes among individuals in *Treatment* who were exposed to refugees for the first time in 2016 have started to change in 2015, i.e. the year before they were exposed. To ascertain that our main result on the attitude index (reported in Table 5.1) holds if we focus on the effect of being exposed for the first time in 2015, we run our main regressions excluding individuals for the first time exposed to refugees in 2016. Reassuringly, we find that the results are indeed robust to excluding this group of individuals (see the last paragraph of Section 5.2 for more details).

Finally, to give an impression of the rejection rate in parallel-trend tests in a counterfactual analysis, we perform the tests, both for the main sample period and the extended period, after randomly reallocating refugee facilities one thousand times in the Netherlands. We then calculate how often we would reject (at the 5% level) at least one *Treatment* \times year coefficient. This provides an estimate of the rejection rate under the null. We would reject at least one coefficient in 61.2% of the cases in the case of the sample period and 78.1% of the cases in the case of the extended period. This shows that the zero rejection rate in our parallel-trend tests is well below

²⁸A small number of individuals in the sample may have lived in neighborhoods that hosted refugees between 2008 and 2010 (recall that the information provided by the COA covered the period 2011-2016). This is not a concern, however, because they are small in number relative to the total number of individuals in *Control*. Moreover, none of them lived in a neighborhood that hosted refugees between 2014 and 2016.

Table 6: Parallel-trend tests

<i>Dependent variable:</i>	(1) Attitude index	(2) Political index	(3) Far Right
A. Year fixed effects (main sample period)			
2012	0.019 (0.014)	-0.038 (0.073)	0.026 (0.026)
2013	0.028 (0.024)	-0.184 (0.142)	0.075 (0.051)
2015	-0.013 (0.045)	-0.468 (0.280)*	0.173 (0.100)*
2016	-0.020 (0.055)	-0.599 (0.351)*	0.216 (0.125)*
<i>Treatment</i> × 2012	-0.080 (0.057)	-0.048 (0.051)	0.010 (0.021)
<i>Treatment</i> × 2013	-0.005 (0.076)	-0.006 (0.064)	0.002 (0.026)
<i>Treatment</i> × 2015	0.108 (0.084)	-0.046 (0.103)	0.011 (0.042)
B. Time trend (main sample period)			
<i>Treatment</i> × Trend	0.025 (0.021)	-0.007 (0.022)	0.002 (0.009)
<i>N</i>	20,353	12,786	12,786
<i>Individuals</i>	5,409	4,484	4,484
<i>Clusters</i>	1,744	1,634	1,634
C. Year fixed effects (extended sample period)			
2009	-0.066 (0.014)***	-0.208 (0.053)***	0.075 (0.019)***
2010	-0.045 (0.018)**	-0.259 (0.103)**	0.081 (0.037)**
2011	0.010 (0.023)	-0.332 (0.148)**	0.107 (0.053)**
2012	0.033 (0.028)	-0.399 (0.198)**	0.144 (0.072)**
2013	0.042 (0.034)	-0.569 (0.246)**	0.202 (0.089)**
2015	-0.001 (0.045)	-0.903 (0.344)***	0.321 (0.124)**
2016	-0.010 (0.051)	-1.072 (0.394)***	0.378 (0.143)***
<i>Treatment</i> × 2009	-0.077 (0.058)	-0.135 (0.109)	0.047 (0.039)
<i>Treatment</i> × 2010	-0.072 (0.080)	-0.124 (0.152)	0.052 (0.054)
<i>Treatment</i> × 2011	-0.005 (0.064)	-0.069 (0.126)	0.029 (0.045)
<i>Treatment</i> × 2012	-0.082 (0.056)	-0.105 (0.111)	0.032 (0.040)
<i>Treatment</i> × 2013	-0.012 (0.061)	-0.071 (0.124)	0.030 (0.045)
<i>Treatment</i> × 2015	0.096 (0.073)	-0.073 (0.134)	0.029 (0.048)
D. Time trend (extended sample period)			
<i>Treatment</i> × Trend	0.010 (0.009)	-0.002 (0.018)	0.001 (0.007)
<i>Observations</i>	27,245	17,913	17,913
<i>Individuals</i>	4,975	4,315	4,315
<i>Clusters</i>	1,583	1,519	1,519

Notes: The baseline year is 2011 in panel A and 2008 in panel C. All regressions control for individual fixed effects and individual and neighborhood time-varying variables specified in Section 4.1. Regressions in panels B and D include year fixed effects. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

that obtained in a random data set in which the parallel-trend assumption holds by nature.

4.4 Selection effects

As a final part of our empirical strategy, we show evidence that it is unlikely that our analysis suffers from selection bias. We focus on three sources of selection effects: attrition, selective voter turnout and selective migration (such as in the case of white flight).

Table 7: Attrition analysis

	(1) LISS	(2) Attitudes	(3) Political	(4) Turnout
Exposed close	0.006 (0.033)	-0.011 (0.010)	-0.046 (0.041)	-0.048 (0.034)
Observations	22,837	20,775	18,716	15,028
Individuals	5,430	5,432	5,432	5,008
Clusters	1,767	1,747	1,747	1,707
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	Yes	Yes

Notes: Estimates are from linear probability models. The dependent variable in column 1 is a binary variable taking a value of one if a respondent answered at least one of the questions in the LISS Core Survey on Politics and Values Survey. In this specification, individual-level or neighborhood-level controls cannot be included as we have no such information (for the particular year) for individuals who did not answer the LISS background questionnaire. The dependent variables in columns 2 and 3 are binary variables taking a value of one if a respondent answered the questions on attitudes toward ethnic diversity and the questions on political preferences, respectively. The dependent variable in column 4 is a binary variable taking a value of one if the respondent affirmed his/her intention to vote.

4.4.1 Attrition

Attrition occurs if LISS respondents drop out of the panel during the sample period or if they avoid answering questions about attitudes toward ethnic diversity or political preferences. We test whether these forms of attrition differ according to whether individuals were exposed to refugees. We create three binary variables: the first takes a value of one for individual i living in neighborhood n in year t if i answered at least one of the questions in the LISS Core Survey on Politics and Values Survey in t ; the second takes a value of one if i answered the questions on attitudes toward ethnic diversity in year t ; and the third takes a value of one if i answered the questions on political preferences in year t . We then estimate equation 1 using the three as dependent variables. The first three columns of Table 7 show that exposure to refugees in the neighborhood does not generate selective attrition.²⁹

4.4.2 Intended turnout at elections

Another possible selection mechanism relates to the intended turnout at elections, whereby exposure to refugees may affect the willingness to vote. This might occur if exposure leads to stronger opinions about how political parties should deal with immigration issues, thus translating into a higher willingness to vote. Since political preferences are only measured for individuals who reported that they would vote if an election were called, it is important to establish whether exposure to refugees affects the willingness to vote. If that is indeed the case, then the estimated effect of exposure on political preferences may be biased.

We regress the willingness to vote on our exposure variables using equation 1. The depen-

²⁹Using instead equation 2 leads to the same conclusion.

dent variable is a binary variable equal to one if individual i reports that (s)he would vote if there were an election in year t , and zero otherwise. The results are shown in column 4 of Table 7. They show that exposure to refugees in the neighborhood does not significantly affect an individual's willingness to vote, which suggests that selection issues in this context do not bias our results on political preferences.

4.4.3 Selective migration

A third possible source of selection relates to individuals selectively moving into or out of neighborhoods that host refugees. Given that our sample only includes individuals who did not move to another neighborhood between 2014 and 2016, it might be a concern if these individuals reacted differently to the presence of refugees than individuals who moved out during that period. In general, however, very few people in the Netherlands move to another neighborhood and this is also the case for our sample.

During the period 2014-2016, there were 548 moves by 484 individuals in the sample, implying that the 5,430 individuals who did not move represent the vast majority of the sample (92%). Nevertheless, if the arrival of refugees triggered the decision to move, then our results may suffer from selection bias by excluding these individuals. To shed light on this issue, we investigate the number of moves into and out of neighborhoods that either did not host refugees or hosted them at some point between 2014 and 2016. Of the 548 moves between 2014 and 2016, 505 were into unexposed neighborhoods and only 43 into exposed neighborhoods. Similarly, of the 548 moves, 523 were out of a neighborhood that did not host refugees, and only 25 were out of a neighborhood that hosted refugees.

If we narrow the time window in order to get a sense of the effect closer to the actual arrival of the refugees, a similar picture emerges: mobility toward neighborhoods that hosted refugees was higher than mobility away from neighborhoods that did not. Up to twelve (six) months before the opening of a refugee facility, there were only 9 (6) moves into and 8 (4) moves out of neighborhoods that hosted refugees. Following the opening of a refugee facility in a neighborhood, there were 15 moves into and 6 moves out of these neighborhoods within a period of six months. The relation between these two numbers does not change much if the time window is widened to up to twelve months after opening. An overview of the number of moves to neighborhoods with and without refugees is provided in Table A.2 in the appendix.

Overall, the number of moves within a window of twelve months before or after the arrival of refugees in a neighborhood is thus extremely low, making a formal statistical analysis redundant. Given that the number of moves to exposed neighborhoods is consistently larger than the number of moves away from them, we can conclude that selective out-migration (i.e. white flight) is highly unlikely to have occurred.³⁰ Moreover, an intention-to-treat analysis

³⁰The same cannot be said of selective in-migration. Although the numbers are too low to draw meaningful statistical conclusions, let us suppose that selective in-migration did indeed take place. Under the plausible assumption that individuals who move into neighborhoods with refugees have a more positive attitude toward ethnic minori-

Table 8: Main effects

	(1)	(2)	(3)	(4)
<i>A. Attitudes toward ethnic diversity</i>				
<i>Attitude index</i>				
Exposed close	0.117 (0.051)**	0.114 (0.054)**	0.116 (0.051)**	0.113 (0.054)**
Exposed far			-0.007 (0.023)	-0.006 (0.023)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
<i>B. Political preferences</i>				
<i>Political index</i>				
Exposed close	0.135 (0.046)***	0.135 (0.048)***	0.139 (0.047)***	0.139 (0.048)***
Exposed far			0.023 (0.034)	0.022 (0.034)
<i>Far Right</i>				
Exposed close	-0.046 (0.016)***	-0.046 (0.016)***	-0.048 (0.016)***	-0.048 (0.017)***
Exposed far			-0.012 (0.013)	-0.012 (0.013)
Observations	12,893	12,917	12,893	12,917
Individuals	4,504	4,513	4,504	4,513
Clusters	1,636	1,636	1,636	1,636
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The dependent variables are indicated in italics. Estimation results for equations 1 and 2 are reported in columns 1 and 3, respectively. Columns 2 and 4 report results when dropping the time-varying individual and neighborhood controls. Exposed close (Exposed far) is an indicator variable that takes a value of 1 if a refugee facility opened in the neighborhood of residence of the individual (in the municipality of residence but in a different neighborhood). Estimates with dependent variables Far Right are from linear probability models. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

shows similar results when individuals who moved between 2014 and 2016 are included (see Section 5.2).

5 Effects on attitudes and political preferences

5.1 Main results

Table 8 shows the results for the effects of exposure to refugees in one's neighborhood on the attitude index (panel A) and on political preferences (panel B). We report the results for specifications with and without time-varying individual and neighborhood characteristics.³¹ The effect are substantial and statistically significant: exposure increases the attitude index by about

ties than other individuals, the most likely consequence of excluding movers from our sample would be that our estimated exposure effects would be biased downwards. See Batut and Schneider-Strawczynski (2021) for a paper that studies the effect of refugee centers on residential choices.

³¹See Table A.3 in the appendix for estimation results on all covariates.

11.6 percent of a standard deviation and increases the political index by about 13.9 percent of a standard deviation relative to unexposed individuals. It thus leads individuals to shift their political preferences away from parties with an anti-multicultural or anti-immigration stance as compared to unexposed individuals, or at least makes them less sympathetic to those parties. More precisely, exposure to refugees decreases the likelihood of voting for the Far Right by about 4.8 percentage points relative to unexposed individuals.

Another important result is that there is no substantial effect of exposure to refugees who are located further away: preferences of individuals who were exposed to refugees in their municipality but not in their neighborhood are not affected by the exposure (columns 3 and 4 in Table 8). The fact that positive exposure effects are only present in the case that individuals live relatively close to refugees suggests that personal encounters with ethnic minorities may be important in shaping preferences. We elaborate on this in Section 6.

If we take into account that attitudes toward ethnic diversity generally became more negative between 2011 and 2016 in the Netherlands, and political preferences shifted toward parties with a tough stance on immigration (see Figure 3), then our results imply that exposure to ethnic minorities has *dampened* this dynamic. Thus, in neighborhoods that hosted refugees the trend in attitudes toward ethnic diversity and political preferences was less negative.

5.2 Robustness checks

To test whether the results are robust, we performed a series of robustness checks, reported in Tables A.4 to A.17 in the appendix. First off, the results are robust to changes in the sample. The results are qualitatively similar if we use a balanced panel (Table A.4) or if we focus exclusively on the sample of municipalities that hosted refugees at some point in time during the sample period (Table A.5). This latter restriction reduces the size of the *Control* group (and its size relative to the *Treatment* group) and narrows the comparison. Results are also robust to including the years 2008-2010 within the sample (Table A.6), and the results for political preferences are robust to extending the sample so as to include individuals who were randomly allocated to a different format of the political preferences questions in the LISS Core Survey on Politics and Values in 2016 (Table A.7).

Moreover, the results are largely robust to changes in the definition of certain variables. Results for the attitude index are robust to re-defining the attitude index using standard principal components or polychoric correlation matrices to calculate factors (see Table A.8 and Table A.9, respectively). Results are also (at least qualitatively) robust to replacing the binary variable that measures exposure to refugees in the close neighborhood by a variable measuring the number of refugees (Table A.10) or the share of refugees in the local population (Table A.11). In addition, the result that exposure to refugees has an effect only if refugees are hosted in the immediate neighborhood and not if they live farther away continues to hold when we define farther-away neighborhoods as those adjacent to the neighborhood in which refugees are being hosted rather than any other neighborhood in the same municipality (Table A.12).

Furthermore, Table A.13 and Table A.14 show that results are robust to controlling for municipality-specific time trends and to multiple hypothesis testing. The latter table reports

p -values from the Romano-Wolf inference correction to account for the fact that we are running multiple-hypotheses regressions (Romano and Wolf, 2005, 2016). The results also continue to hold if we apply the estimators developed by Borusyak, Jaravel, and Spiess (2021) instead of the conventional two-way fixed-effects estimator (Table A.15).

We also carry out an intention-to-treat analysis that includes individuals who moved to another neighborhood between 2014 and 2016 and assign them to their last observed postal code before moving. Although this procedure provides lower bounds on the effects of exposure to ethnic minorities, the estimated effects have the same signs and are statistically significant (see Table A.16). As expected, the effect sizes are smaller than in the main analysis but still meaningful.

Finally, to test whether our main results are sensitive to excluding individuals who are treated for the first time in 2016, that is, in the midst of the refugee crisis, we ran the regressions removing these individuals. Any remaining effect of exposure to refugees would come from individuals in *Treatment* who were exposed for the first time in 2014 or 2015. For these individuals one may argue that the inflow of refugees was more likely to be unforeseen. We find that for our three dependent variables the estimated effect sizes are of the same order of magnitude if individuals treated for the first time in 2016 are excluded from the analysis (Table A.17).

5.3 Placebo regressions

We also used placebo regressions to further support the plausibility of our estimates. The first series of tests consists of randomization exercises in which we randomly reassigned the values of the exposure variable to individuals in the sample.³² In this way, we are essentially pretending that the COA facilities are randomly located in the neighborhoods included in the main sample. We repeat the process one thousand times, each time estimating the effect of this placebo treatment in regressions of equation 1. This provides a distribution of simulated counterfactual effects of exposure to refugees in the neighborhood (see Figure B.1 in the appendix). For all dependent variables, the mean of the counterfactual estimates is centered around zero whereas the true estimates are located in the tails of the distributions.

In a second series of placebo regressions we estimated equations 1 and 2 with as dependent variables two indices for which we expected no effect of exposure. The two indices capture attitudes toward marriage and parental care, respectively, and are based on five-point-scale statements surveyed in the LISS Survey on Politics and Values. The regression results indicate indeed that neither are affected by exposure to refugees (see Table A.19 in the appendix).

³²We randomized the exposure values within each year in the sample period in order to avoid the potential confounding effects of time trends.

5.4 Heterogeneous effects

We study whether effects of exposure to refugees in the neighborhood on our six dependent variables are heterogeneous along the following dimensions: gender (female/male), age (below 50/above 50), education (low/high), the presence of children in the household (yes/no), the degree of urbanization (urban/rural), the pre-exposure attitude index (above median/below median, referred to as positive and negative, respectively), and the pre-exposure political index (above median/below median, referred to as left-leaning and right-leaning, respectively).³³ For each pair of subgroups within these dimensions, we jointly estimate the effects of *Exposed close* on a dependent variable and test whether the effects are significantly different between the subgroups using a Wald test. If so, then this implies there is a heterogeneous effect for the associated dimensions and dependent variable.³⁴ The results are summarized in Table 9, in which each column corresponds to one of the six dependent variables.

We find that the positive effect of exposure on the attitude index is strongest for individuals below median age and individuals with children living at home, although in the latter case the difference is not statistically significant. The effect is also somewhat larger for women than for men, although the difference is not significant either. When it comes to political preferences, the strongest effects tend to be concentrated among men, individuals above the median age, individuals without children at home, and lowly educated, but in none of these cases are the differences statistically significant.

Furthermore, we find that the exposure effects do not differ significantly between urban and rural areas. Qualitatively, the largest positive change (in attitudes) is observed in rural areas. These results appear at odds with Dustmann et al. (2019), who find instead that the presence of refugees in rural municipalities increases support for anti-immigration parties. An important difference between the two studies is that our data are aggregated at a relatively low level (neighborhood), which makes it more likely that positive interethnic contact have a bearing on the results (see Section 7 for a discussion).

Finally, we find strong heterogeneous effects depending on pre-exposure political preferences. Specifically, exposure to refugees has significantly stronger effects on attitudes and political preferences of individuals who were relatively right-leaning before the refugees arrived in their neighborhood than on attitudes and preferences of left-leaning individuals. A qualitatively similar difference is observed between individuals with a below-median and an above-median attitude index, albeit statistically not as strong. Given the pervasiveness of ethnic prejudice (Quillian et al., 2017), the finding is not trivial and suggests that exposure to refugees in the neighborhood has reduced polarization.

³³Low education includes primary school, junior high school, high school and junior college, and high education includes college and university. Urban (rural) areas have a population density higher (lower) than 500 inhabitants per km².

³⁴Notice that we find no substantive heterogeneous effects of exposure to refugees located farther away.

Table 9: Heterogeneous effects

Dependent variable:	(1) Attitude index	(2) Political index	(3) Far Right
Female	0.144 (0.072)**	0.074 (0.087)	-0.026 (0.031)
Male	0.091 (0.068)	0.185 (0.043)***	-0.062 (0.015)***
Wald test <i>p</i> -value	0.573	0.238	0.285
Young (< 50)	0.283 (0.089)***	0.015 (0.097)	-0.001 (0.032)
Old (> 50)	0.008 (0.055)	0.172 (0.048)***	-0.059 (0.017)***
Wald test <i>p</i> -value	0.008	0.124	0.096
Low education	0.109 (0.073)	0.146 (0.060)**	-0.050 (0.021)**
High education	0.137 (0.107)	0.097 (0.023)***	-0.032 (0.007)***
Wald test <i>p</i> -value	0.849	0.424	0.392
Without children	0.066 (0.048)	0.158 (0.047)***	-0.059 (0.017)***
With children	0.243 (0.129)*	0.063 (0.111)	-0.002 (0.028)
Wald test <i>p</i> -value	0.208	0.427	0.080
Urban	0.081 (0.071)	0.155 (0.057)***	-0.050 (0.017)***
Rural	0.159 (0.075)**	0.119 (0.067)*	-0.042 (0.024)*
Wald test <i>p</i> -value	0.447	0.684	0.770
Negative attitude	0.134 (0.072)*	0.189 (0.062)***	-0.065 (0.021)***
Positive attitude	0.005 (0.110)	0.024 (0.033)	-0.003 (0.008)
Wald test <i>p</i> -value	0.404	0.019	0.007
Observations	19,109	12,158	12,158
Individuals	4,677	3,979	3,979
Clusters	1,618	1,526	1,526
Right-leaning	0.215 (0.078)***	0.212 (0.078)***	-0.067 (0.027)**
Left-leaning	-0.003 (0.114)	0.042 (0.025)*	-0.013 (0.006)**
Wald test <i>p</i> -value	0.077	0.046	0.056
Observations	15,644	11,863	11,863
Individuals	3,725	3,724	3,724
Clusters	1,495	1,494	1,494

Notes: Effects are reported of *Exposed close* for pairs of subgroups within a range of heterogeneity dimensions. The regressions include individual fixed effects, year fixed effects, and time-varying individual and neighborhood controls, as well as interaction terms between year fixed effects and the relevant heterogeneity dimensions. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6 Mechanisms

We consider three possible mechanisms to explain the results: contact with refugees, employment opportunities in the neighborhood of refugee facilities and confidence in the government.

6.1 Contact

The first mechanism involves contact between local residents and refugees in the neighborhood. This ranges from casual encounters (on the street, on the bus, in shops, etc.) to more personal contact such as at school. Such experiences may lead to a common ground and thus reduce negative stereotypes and prejudice (Allport, 1954). Support for the idea that contact is possibly an important mechanism affecting the residents' preferences comes from the absence

Table 10: Effects depending on duration of exposure and number of refugees

	Duration of exposure		Number of refugees	
	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Below-median exposure	0.061 (0.073)	0.054 (0.073)	0.145 (0.074)*	0.147 (0.082)*
Above-median exposure	0.175 (0.054)***	0.176 (0.059)***	0.094 (0.068)	0.085 (0.069)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
B. Political preferences				
<i>Political index</i>				
Below-median exposure	0.092 (0.042)**	0.098 (0.042)**	0.252 (0.081)***	0.250 (0.081)***
Above-median exposure	0.174 (0.083)**	0.168 (0.086)*	0.067 (0.044)	0.069 (0.047)
<i>Far Right</i>				
Below-median exposure	-0.035 (0.013)***	-0.039 (0.014)***	-0.079 (0.030)***	-0.078 (0.030)***
Above-median exposure	-0.055 (0.029)*	-0.053 (0.030)*	-0.027 (0.014)*	-0.028 (0.016)*
Observations	12,893	12,917	12,893	12,917
Individuals	4,504	4,513	4,504	4,513
Clusters	1,636	1,636	1,636	1,636
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The dependent variables are indicated in italics. The exposure variables refer to exposure in the neighborhood. The median duration of exposure is 6 months and the median number of refugees is 200. Estimation results including all covariates discussed in Section 4.1 are in columns 1 and 3, and columns 2 and 4 report results when dropping the time-varying individual and neighborhood controls. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

of a positive effect of exposure to refugees who live in neighborhoods farther away.

To further explore the role of contact, we exploit variation in the duration of exposure, due to the refugees' arrival in the neighborhood in different months of the year and the different lengths of their stay, as well as variation in the number of refugees hosted in a neighborhood. It seems plausible that the likelihood of interethnic contact is higher, the longer the exposure and the larger the number of refugees residing in the neighborhood. To test this, we created two binary variables that correspond to below-median and above-median duration of exposure, respectively. The variables indicate whether at the time of measurement an individual was exposed to refugees in the neighborhood for less or more than the median duration of exposure (i.e. 6 months). In order to estimate the effect of number of refugees, we calculated an annual indicator from the monthly data by averaging the number of refugees hosted in neighborhood n across months in which there was at least one refugee present in n . The median of this variable is about 200. We constructed two binary variables according to whether the number of refugees is above or below 200. The effect of the duration of exposure and of the number of refugees on the attitude index, the political index and the Far Right is reported in panel A and B of Table 10.

The presence of refugees in the neighborhood for more than six months leads to a statistically significant increase in the attitude index and the political index of about 17.5 percentage points relative to unexposed individuals. If exposure lasts less than six months, then the effect is still positive but not statistically significant.³⁵ Exposure to less than 200 refugees leads to an increase of 14.5 and 25.2 percentage points in the attitude and political index respectively, relative to unexposed individuals. The effect when the number of refugees is greater than 200 is still positive but not statistically significant.³⁶ One possible explanation is that a countervailing effect kicks in if the number of refugees is “too large” because a larger number of refugees may be more likely to be perceived as a threat (see, for example, Quillian, 1995). It may also be that a larger number of refugees, if associated with more conflicts in and around the facilities, leads to negative contact. Whereas both longer exposure and a larger number of refugees arguably increase the salience of refugees and the chance of a random, brief encounter, it is conceivable that only longer exposure increases the likelihood of positive contact (e.g. more personal contact). The finding that the effect of long exposure is strong and positive but the effect of a high number of refugees is not suggests that positive contact or lack of negative contact are important for generating a positive exposure effect.

Further support that local positive experiences, or lack of negative experiences, are a channel behind the positive exposure effects comes from a policy paper that reports on three case studies of refugee facilities (i.e. Kuppens et al., 2019). The case studies are based on interviews performed with local managers and COA employees at the facilities and residents living in the neighborhood. Among the interviewees, a clear consensus existed that the contact between the refugee facilities and residents in the neighborhood was generally positive.³⁷ To obtain and maintain positive relations quite some effort was invested to involve and inform residents, for example, by means of organizing visits to the refugee facilities and regular sounding board meetings. One manager noted that the support of local residents grew as they got to know the refugees better due to an adjustment of the negative stereotypes they held initially.

6.2 Employment opportunities

Exposure to refugees may improve local residents’ attitudes and influence their intentions to vote by way of increased employment opportunities as a result of the opening of a refugee facility.³⁸ We therefore test for our sample whether the employment status of individuals living in close proximity to a refugee facility is more likely to improve relative to that of individu-

³⁵Small perturbations of the cutoffs do not change the results.

³⁶Positive exposure effects that diminish as the number of refugees increases can also be seen in quadratic regressions in which the binary exposure variables are replaced by (logs or shares of) numbers of refugees (see columns (3) and (4) in Table A.10 and Table A.11 in the appendix).

³⁷This is especially noteworthy given that only facilities that had faced some problems according to some (social) media outlets were selected for the case studies.

³⁸It has been shown that fears of increased labor market competition can negatively affect attitudes toward immigrants (Ortega and Polavieja, 2012). Since we find that attitudes toward immigrants improve in exposed neighborhoods, we are able to rule this channel out.

Table 11: Effects on employment and confidence in the government

	(1)	(2)	(3)	(4)
A. Employment				
Exposed close	-0.021 (0.023)	-0.021 (0.023)	-0.024 (0.023)	-0.024 (0.024)
Exposed far			-0.012 (0.015)	-0.015 (0.015)
Observations	20,775	20,831	20,775	20,831
Individuals	5,432	5,446	5,432	5,446
Clusters	1,747	1,748	1,747	1,748
B. Confidence in government				
Exposed close	-0.137 (0.064)**	-0.139 (0.062)**	-0.145 (0.065)**	-0.147 (0.063)**
Exposed far			-0.036 (0.030)	-0.037 (0.030)
Observations	19,752	19,798	19,752	19,798
Individuals	5,366	5,378	5,366	5,378
Clusters	1,740	1,741	1,740	1,741
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The dependent variables are a binary variable indicating employment (panel A) and an index representing confidence in government (panel B). Estimation results for equations 1 and 2 are in columns 1 and 3, respectively, and columns 2 and 4 report results when dropping the time-varying individual and neighborhood controls. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

als in the *Control* group. To do so, we regress a self-reported employment measure (salaried employment, family business, self-employed professional, voluntary work or unpaid work), as reported in the LISS Background Survey, on the exposure variables according to equations 1 and 2. Panel A of Table 11 shows that the presence of refugees in the neighborhood has basically no effect on local employment. We therefore conclude that increased employment opportunities are most likely not an important explanation of changes in attitudes toward ethnic diversity or political preferences.

6.3 Confidence in the government

A third possible mechanism involves the financial compensation of municipalities that agree to host refugees, which allows them to increase public spending in the relevant neighborhoods.³⁹ Such an increased spending may increase one's support for the government, which may in turn covary with one's attitude toward ethnic diversity. In the absence of access to information on local public spending, we used individual-level panel data from the LISS Core Survey on Politics and Values on satisfaction with the Dutch government. Specifically, we used five

³⁹There is evidence suggesting that public spending may decrease: Tabellini (2019) shows that between 1915 and 1930 an increased inflow of African-Americans into US cities had a negative effect on public spending. This occurred as a result of decreased property values which generated a decline in tax revenues.

statements from the survey to compose a confidence index, which then served as a dependent variable in equation 1 and 2.⁴⁰

As shown in panel B of Table 11, exposure to refugees had a negative and significant effect on the confidence index. In other words, it reduced confidence in the government. It is therefore highly unlikely that an improved attitude toward ethnic diversity and changes in political preferences can be explained by increased support for the government.

7 Effect on behavior in a lab-in-the-field experiment

We also examined revealed preferences with regard to ethnic minorities using a behavioral measure elicited in an incentivized lab-in-the-field experiment.

7.1 The data

The main subjects in the experiment are a sample of native Dutch citizens of the LISS panel. They were asked to make choices in the role of trustee in three binary trust games.⁴¹ About half of the trustees were matched with a trustor who also had a majority background (*Majority* condition) while the other half was matched to a trustor with a non-Western immigration background (*Minority* condition). The trustees were informed of the matched trustors' first name, as well as his/her gender and age group (i.e. between 16 and 89 years old). The name served as a signal of ethnic background. We interpret lower reciprocation rates in the minority condition relative to the majority condition as evidence of discrimination against minorities (see Cettolin and Suetens, 2018).

The experiment was conducted in four waves: December 2014, February 2015, December 2015 and December 2016. In each of them, a random sample of about 800 participants was selected. Furthermore, all the trustees in the December 2014 and December 2015 waves were asked to participate again in December 2016. Those that agreed remained in the same condition as previously. We focused on the behavior of trustees who took part in two waves, corresponding either to the December waves in 2014 and 2016 or the December waves in 2015 and 2016.⁴² Since the largest numbers of refugees arrived in 2015 and 2016, the experiment makes it possible to test whether exposure to refugees has an effect on reciprocation choices.

Merging the data from the experiment with the data on refugee facilities yields a sample of 931 trustees. Of these, 485 were in the *Majority* condition and 446 in the *Minority* condition.

⁴⁰Individuals were asked to indicate on a scale from 0 to 10 their level of confidence in and the extent to which they were satisfied with both the Dutch government and politicians.

⁴¹In this game a trustor decides whether to trust a matched trustee who in turn and if trusted by the trustee decides whether to reciprocate the trust. If the trustor does not trust the trustee, both parties earn 35 euros. If the trustor trusts the trustee but the trustee does not reciprocate, the trustor earns 20 euros and the trustee earns 85 euros. If the trustor trusts the trustee and the trustee reciprocates, then both earn 40, 60, or 80 euros. Further details on the experiment are provided in Section C of the appendix.

⁴²Participants of the February 2015 wave did not participate again in a later wave.

After excluding individuals who were exposed to refugees before 2014 or moved to a different neighborhood between 2014 and 2016, we were left with a sample of 761 individuals. Table C.1 in the appendix provides an overview of the reciprocation rates across the different waves. Overall, the reciprocation rate towards a *Majority* partner remains quite constant across the three years (58-60 percent), whereas the reciprocation rate towards a *Minority* partners increases over time (from 55 to 63 percent). Table C.1 also includes reciprocation rates of individuals in *Control* (who were not exposed to refugees) and of individuals in *Treatment* (who were exposed to refugees in 2015 or 2016) across years before and after they were exposed. In pre-exposure years, individuals in *Treatment* reciprocate trust of a *Minority* partner 22 percentage points less frequently than that of a *Majority* partner, whereas after exposure they instead reciprocate trust 9 percentage points more frequently in the former case than in the latter case. In *Control* the reciprocation rate is similar for both *Majority* and *Minority* partners.

The key advantage of the experiment is that there are material consequences for both the trustor and the trustee, thus providing information about revealed preferences. Disadvantages are that we observe behavior only twice for each individual and that the sample is relatively small (about 13 percent of the individuals included in the main sample). To illustrate, if the experimental sample is used to estimate the effect of exposure on attitudes toward ethnic diversity, we find that the attitude index becomes 31 percentage points of a standard deviation more positive among individuals who are exposed to refugees in their neighborhood as compared to only 11.6 percentage points in the case of the main sample (see Table C.2 in Section C of the appendix). The effect size based on the experimental sample is clearly much larger and not representative of the larger sample: therefore, the size of the estimated effects based on the data of the experiment should be interpreted with caution. In what follows, we describe our empirical strategy and the results.

7.2 Empirical strategy

In order to identify the effect of exposure to refugees on the trustees' choices, we employ a similar approach to that used in the main analysis. One difference is that in the experimental data trustees make choices in one of the two conditions (majority or minority) and that the key measure of interest is the difference in choices between the two conditions. Equation 3 represents the regression specification for individual i living in neighborhood n in year t :

$$Reci_{int} = \alpha_i + \tau_t + \beta_1 Minority_{it} + \beta_2 Exposed\ close_{nt} + \beta_3 Minority_{it} \cdot Exposed\ close_{nt} + \gamma X_{int} + e_{int}, \quad (3)$$

where $Reci_{int}$ refers to the reciprocation rate of trustee i in neighborhood n in year t across the three binary trust games, α_i refers to individual fixed effects and τ_t refers to year fixed effects. $Exposed\ close_{nt}$ is a binary variable indicating whether refugees are hosted for at least one month in neighborhood n in year t (as previously). X_{int} is a vector of control variables that includes time-varying individual, neighborhood and municipal characteristics. $Minority_{nt}$ is equal to 1 if the trustee is matched to a trustor with a minority background and to 0 if matched to a trustor with a majority background. To determine how exposure to ethnic minorities influ-

Table 12: Effects on reciprocation rate

	(1)	(2)	(3)	(4)
Exposed close	-0.127 (0.102)	-0.098 (0.103)	-0.206 (0.118)*	-0.168 (0.120)
Exposed close \times minority	0.269 (0.171)	0.252 (0.173)	0.330 (0.183)*	0.307 (0.185)*
Exposed far			-0.165 (0.086)*	-0.160 (0.089)*
Exposed far \times minority			0.124 (0.122)	0.117 (0.126)
Observations	1,522	1,522	1,522	1,522
Individuals	761	761	761	761
Clusters	578	578	578	578
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The dependent variable is the reciprocation rate of individuals in the role of trustee across three trust games. Standard errors are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ences discrimination in reciprocation, we focus on β_3 which measures the interaction between being matched to a trustor with a minority background in the trust game *and* being exposed to minorities in the neighborhood.

In analogy with our main empirical approach, we also run regressions that allow detecting whether there is an effect of exposure to refugees in the municipality who do not reside in the same neighborhood. This adds two independent variables to equation 3, namely *Exposed far* and the interaction term *Minority \cdot Exposed far*.

7.3 Results

Table 12 presents the regression results. We find a positive but statistically insignificant interaction between being exposed and being matched to a minority trustor in our main regressions (columns 1 and 2). The effect is marginally significant if controlling for exposure farther away. The interpretation is that individuals exposed to refugees in their neighborhood tend to be more inclined to reciprocate trust from trustors with a minority background as compared to trustors with a majority background, than trustees who have not been exposed. Tendencies in the behavioral data are thus largely consistent with those in the main analysis.

8 Conclusion

We exploit the sudden inflow of refugees in some neighborhoods in the Netherlands to study whether exposure to ethnic minorities influences ethnic preferences. We find that individuals in neighborhoods which experienced an influx developed a more positive attitude toward ethnic diversity after refugees arrived and became less inclined to vote for political parties with an anti-immigration agenda than individuals in other neighborhoods. Behavioral data from a lab-in-the-field experiment qualitatively support these results; local exposure to refugees increased cooperation with individuals with an ethnic minority background relative to individuals from the majority. Neighborhoods thus have an important role to play in shaping attitudes and

behavior toward ethnic minorities and political preferences, as they do in influencing mental health (Kling, Liebman and Katz, 2007; Ludwig et al., 2013), crime (Damm and Dustmann, 2014), and employment and education (Chetty et al., 2018).

While exposure to refugees influences the preferences of residents in the neighborhood, it essentially has no effect on people living farther away from the refugee facilities, suggesting that personal contact is likely an important channel for the estimated effects of exposure. This interpretation is further corroborated by the finding that effect sizes are largest when exposure lasts for a relatively long time. Individuals who live closer to a refugee facility or are exposed for a longer period are more likely to come into contact with refugees in their daily routine. Such contacts may increase the likelihood of discovering a common humanity and diminish in-group out-group thinking, in line with the intergroup contact hypothesis of Allport (1954) and the subsequent empirical literature. This complements the finding of Alesina, Miano, and Stantcheva (2018) that priming individuals with a positive story about an immigrant reduces stereotyping and improves attitudes toward immigrants. In line with Depetris-Chauvin, Durante, and Campante (2020), our results indicate that *experiencing* such a story has a similar effect.⁴³

Contact with refugees may also convey information to individuals from the majority, in the sense that it may alter preconceived notions about ethnic minorities. For example, individuals may come to realize the falsity of their beliefs that ethnic minorities cause nuisances, commit crimes or are unwilling to work.⁴⁴ Case studies and door-to-door interviews carried out in the vicinity of refugee facilities in the Netherlands suggest that a negative attitude toward minorities is not related to negative local experiences involving minorities, but is instead associated with negative media coverage and general social discontent (Kuppens et al., 2019). This all the more shows that in order to understand the effects of ethnic diversity on perceptions about individuals with a different ethnic background, and ultimately on political preferences and societal outcomes, it is important to distinguish between local and aggregate diversity (see Bazzi, Gaduh, Rothenberg, and Wong, 2019; Desmet, Gomes, and Ortuno-Ortin, 2020; Enos, 2017).

Our results with regard to political preferences and, in particular, the intention to vote for the Far Right are consistent with the findings of Steinmayr (2021) and Vertier, Viskanic, and Gamalerio (2020). These studies show for Austria and France, respectively, that Far Right parties have lost popularity in areas with sustained interaction between local residents and refugees during the European refugee crisis. Nonetheless, if we consider the many other studies, then the general conclusion on the effect of exposure to ethnic minorities on political preferences is mixed (as shown in the meta-study of Cools et al., 2021). How then can our results be squared with the literature? First off, local conditions vary substantially across countries. The COA, which manages the hosting of asylum seekers in the Netherlands, and the host mu-

⁴³Depetris-Chauvin, Durante, and Campante (2020) find that collectively experiencing a national football teams' victory increases interethnic trust and reduces interethnic conflict.

⁴⁴Exposure can induce majority members to take the perspective of minority members, which helps increasing the likelihood of interethnic ties and reducing ethnic segregation (Alan, Baysan, Gumren, and Kubilay, 2021).

nicipalities jointly invest in the integration of refugees within society, and maintain a dialogue with local residents by means of local liaisons. This may create circumstances that contribute to a positive influence on local residents' perceptions and attitudes. Furthermore, during the European refugee crisis the annual proportion of refugees in the population of a typical Dutch neighborhood was 4 percent and never exceeded 28 percent, while on Greek islands close to the Turkish coast that number was up to 500 percent in 2015. The exceptionally large number of refugees on these islands may have evoked strong negative reactions among local residents, leading to a surge in the popularity of the Far Right (Dinas et al., 2019; Hangartner et al., 2019; Vasilakis, 2018).

A second factor that may contribute to the mixed findings in the voting literature relates to the level of data aggregation. It is noteworthy that the literature on the effect of contact with minorities on attitudes toward them, yielding overwhelmingly positive effects, is largely based on individual-level data and thus allows identifying highly localized effects.⁴⁵ Instead, the literature on political preferences, which often reports negative effects, is largely based on relatively aggregated data (such as municipality-level data, as in Dahlberg et al., 2012; Dustmann et al., 2019). Also Bazzi, Gaduh, Rothenberg, and Wong (2019), who study the effect of ethnic diversity on nation building and integration, report stronger positive effects of diversity as the diversity measure becomes increasingly local. To illustrate the role of data aggregation, we re-estimate the effects of exposure to refugees on attitudes toward ethnic diversity and political preferences, but now with data aggregated at the municipality level rather than the neighborhood level. We find that the effects are fourfold smaller than the neighborhood effects and are not statistically significant.⁴⁶ We conjecture that in an aggregate analysis the net exposure effect is a mixture of the effect stemming from personal contact with ethnic minorities and the (probably less positive) effect of less personal forms of exposure, such as through the media and hearsay, which are likely to fuel fear and anti-immigrant sentiment.

Finally, our findings show that attitudes toward ethnic diversity are an important channel through which exposure to ethnic minorities influences political preferences. Since a political party's stance on immigration and multiculturalism is just one of the elements that influence voters, this is not trivial. The implication is that insights from the contact literature, which focuses on how attitudes toward the out-group are influenced by personal intergroup contact, are relevant in order to understand political preferences. This leads us to the policy implication of our findings. In general, any policy that facilitates contact between ethnic minorities and the majority can lead to a shift in political preferences. Applied to the case of refugees, our findings suggest that a policy of geographically spreading refugees combined with the creation

⁴⁵Yet, using micro data on social housing buildings in France, Algan, Hémet, and Laitin (2016) show that ethnic diversity reduces housing quality and leads to fewer social relationships with neighbors.

⁴⁶See Table A.20 in the appendix. If data are aggregated at the municipality level, exposure only has an effect of 2.6 percentage points of a standard deviation in the case of the attitude index and 3.3 percentage points in the case of the political index, whereas the neighborhood effects correspond to 24 and 25 percentage points of a standard deviation, respectively.

of positive local conditions for integration has the potential to improve attitudes toward ethnic minorities, particularly among right-leaning individuals, and thus to prevent polarization and the rise of the Far Right.

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Appendix - For online publication only

A Supplementary tables

Table A.1: Summary statistics of all dependent variables

	Mean	S.D. (overall)	S.D. (within)	Min	Max	Nr. of individuals	Average nr. of years	<i>N</i>
Attitude index	-0.020	1.017	0.392	-2.869	3.158	5,444	3.792	20,645
Political index	0.027	0.966	0.396	-2.304	0.904	4,513	2.862	12,917
Far Right	0.142	0.349	0.147	0	1	4,513	2.862	12,917
Vote turnout	0.912	0.283	0.158	0	1	5,018	3.001	15,060
Employment	0.526	0.499	0.184	0	1	5,446	3.825	20,831
Confidence in government	-0.102	1.039	0.518	-2.676	2.680	5,378	3.681	19,798
Attitude toward marriage	-0.063	0.985	0.412	-2.345	4.347	5,444	3.790	20,635
Attitude toward parental care	-0.076	0.993	0.495	-2.529	3.140	5,444	3.790	20,635
Attitude index (principal components)	-0.017	1.016	0.386	-2.822	3.156	5,444	3.792	20,645
Attitude index (polychoric)	-0.007	1.013	0.380	-2.638	3.007	5,444	3.792	20,645
Reciprocation rate	0.599	0.341	0.225	0	1	761	2	1,522

Notes: Summary statistics based on the main sample of variables used as dependent variables in regressions. *N* denotes the number of observations.

Table A.2: Migration into and out of neighborhoods

	<i>Unexposed neighborhoods</i>		<i>Exposed neighborhoods</i>			
	(1)	(2)	(3)	(4)	(5)	(6)
	2014-2016	2014-2016	12 months before exp.	6 months before exp.	6 months after exp.	12 months after exp.
Nr. of moves into	505	43	9	6	15	15
Nr. of moves out of	523	25	8	4	6	6
Total nr. of moves	548					

Notes: Absolute frequencies of moves out of and into another neighborhood. One observation is one move by an individual in the LISS panel in a given month. Column 1 refers to unexposed neighborhoods, columns 2-6 refer to exposed neighborhoods. Columns 1 and 2 count the total number of moves throughout the months from January 2014 to December 2016 and across individuals. Columns 3 to 6 narrow down the months considered for exposed neighborhoods.

Table A.3: Main effects - detailed results

<i>Dependent variable:</i>	(1)	(2)	(3)
	Attitude index	Political index	Far Right
Exposed close	0.117 (0.051) **	0.135 (0.046) ***	-0.046 (0.016) ***
Year			
2012	0.015 (0.014)	-0.039 (0.072)	0.026 (0.026)
2013	0.025 (0.025)	-0.183 (0.141)	0.074 (0.050)
2015	-0.019 (0.046)	-0.466 (0.277) *	0.173 (0.099) *
2016	-0.025 (0.055)	-0.592 (0.348) *	0.214 (0.124) *
Inhabitants			
neighborhood (in 1,000s)	-0.007 (0.006)	0.016 (0.009) *	-0.006 (0.003) **
municipality (in 100,000s)	0.037 (0.016) **	-0.015 (0.014)	0.005 (0.005)
Education			
primary school	-0.061 (0.067)	0.102 (0.073)	-0.023 (0.025)
junior high school	0.105 (0.063) *	0.028 (0.071)	0.013 (0.023)
high school	-0.052 (0.071)	0.008 (0.109)	0.008 (0.040)
college	-0.014 (0.068)	0.008 (0.075)	0.015 (0.027)
university	0.139 (0.085)	0.093 (0.070)	-0.007 (0.024)
Age	0.032 (0.016) **	0.078 (0.070)	-0.026 (0.025)
Age ² (divided by 100)	-0.037 (0.008) ***	0.014 (0.013)	-0.007 (0.005)
Number of children	-0.013 (0.015)	-0.038 (0.022) *	0.013 (0.008)
Marital status			
separated	-0.116 (0.101)	0.013 (0.151)	-0.021 (0.063)
divorced	-0.097 (0.066)	0.052 (0.205)	-0.032 (0.080)
widow(er)	0.086 (0.072)	0.113 (0.074)	-0.039 (0.025)
never married	-0.039 (0.057)	-0.135 (0.105)	0.054 (0.040)
Observations	20,589	11,863	11,863
Individuals	5,430	4,504	4,504
Clusters	1,745	1,636	1,636

Notes: All regressions include individual fixed effects. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.4: Balanced panel

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Exposed close	0.129 (0.058)**	0.129 (0.061)**	0.131 (0.058)**	0.130 (0.062)**
Exposed far			0.011 (0.027)	0.011 (0.026)
Observations	12,943	12,966	12,943	12,966
Individuals	2,611	2,615	2,611	2,615
Clusters	1,216	1,216	1,216	1,216
B. Political preferences				
<i>Political index</i>				
Exposed close	0.152 (0.058)***	0.153 (0.060)**	0.154 (0.059)***	0.155 (0.061)**
Exposed far			0.012 (0.041)	0.009 (0.040)
<i>Far Right</i>				
Exposed close	-0.052 (0.021)**	-0.053 (0.022)**	-0.053 (0.021)**	-0.054 (0.022)**
Exposed far			-0.009 (0.015)	-0.008 (0.015)
Observations	8,366	8,379	8,366	8,379
Individuals	2,336	2,340	2,336	2,340
Clusters	1,164	1,164	1,164	1,164
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample only includes individuals who answered to all five waves of the LISS Core Survey on Politics and Values. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.5: Excluding municipalities without refugee facilities

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Exposed close	0.121 (0.053)**	0.112 (0.057)**	0.087 (0.069)	0.081 (0.073)
Exposed far			-0.040 (0.049)	-0.036 (0.049)
Observations	3,576	3,579	3,576	3,579
Individuals	965	966	965	966
Clusters	266	267	266	267
B. Political preferences				
<i>Political index</i>				
Exposed close	0.096 (0.054)*	0.094 (0.054)*	0.026 (0.065)	0.033 (0.068)
Exposed far			-0.089 (0.056)	-0.077 (0.058)
<i>Far Right</i>				
Exposed close	-0.029 (0.019)	-0.028 (0.019)	-0.003 (0.023)	-0.007 (0.024)
Exposed far			0.033 (0.020)*	0.026 (0.021)
Observations	2,273	2,274	2,273	2,274
Individuals	786	786	786	786
Clusters	252	252	252	252
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample excludes municipalities that do not accommodate refugees between 2011 and 2016. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.6: Extending sample with years 2008 to 2010

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Exposed close	0.126 (0.053)**	0.128 (0.054)**	0.121 (0.053)**	0.123 (0.055)**
Exposed far			-0.015 (0.018)	-0.014 (0.018)
Observations	28,083	28,100	28,083	28,100
Individuals	5,105	5,105	5,105	5,105
Clusters	1,671	1,671	1,671	1,671
B. Political preferences				
<i>Political index</i>				
Exposed close	0.165 (0.063)***	0.164 (0.065)**	0.160 (0.064)**	0.157 (0.065)**
Exposed far			-0.016 (0.027)	-0.022 (0.027)
<i>Far Right</i>				
Exposed close	-0.058 (0.022)***	-0.058 (0.023)**	-0.057 (0.022)***	-0.056 (0.023)**
Exposed far			0.003 (0.010)	0.006 (0.010)
Observations	18,399	18,411	18,399	18,411
Individuals	4,423	4,423	4,423	4,423
Clusters	1,585	1,585	1,585	1,585
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample includes data points for the years 2008 to 2010 of individuals in our main estimation sample. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.7: Extending sample with alternative political preferences questions in 2016

	(1)	(2)	(3)	(4)
<i>A. Attitudes toward ethnic diversity</i>				
<i>Attitude index</i>				
Exposed close	0.117 (0.051)**	0.114 (0.054)**	0.116 (0.051)**	0.113 (0.054)**
Exposed far			-0.007 (0.023)	-0.006 (0.023)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
<i>B. Political preferences</i>				
<i>Political index</i>				
Exposed close	0.116 (0.037)***	0.118 (0.038)***	0.122 (0.037)***	0.123 (0.038)***
Exposed far			0.028 (0.029)	0.029 (0.029)
<i>Far Right</i>				
Exposed close	-0.047 (0.013)***	-0.048 (0.013)***	-0.049 (0.013)***	-0.051 (0.014)***
Exposed far			-0.013 (0.011)	-0.013 (0.011)
Observations	1,4941	14,970	1,4941	14,970
Individuals	4,805	4,814	4,805	4,814
Clusters	1,679	1,679	1,679	1,679
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample includes individuals who were randomly allocated to another format of the political preferences questions in the LISS Core Survey on Politics and Values in 2016. These individuals were asked to state the percent chance they would vote for each single party if election would take place at that time. We coded these answers as follows: dummy variables for the same groupings of parties (Far Right) were generated and took the value one if a respondent reported more than 50 percent probability of voting for a party. Individuals who reported less than 50 percent probability of voting for any party were not included. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.8: Attitude index based on principal components analysis

	(1)	(2)	(3)	(4)
Exposed close	0.109 (0.050)**	0.106 (0.053)**	0.107 (0.050)**	0.104 (0.053)*
Exposed far			-0.009 (0.022)	-0.008 (0.022)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The outcome variable is built from the original six questions which are averaged to construct the Attitude index. We run principal component analysis to make sure that answers to the six questions are driven by a main factor. We then create a variable using the factor loadings estimated by PCA. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.9: Attitude index based on factor analysis using polychoric correlation matrices

	(1)	(2)	(3)	(4)
Exposed close	0.101 (0.050)**	0.098 (0.054)*	0.099 (0.051)*	0.097 (0.054)*
Exposed far			-0.007 (0.022)	-0.007 (0.022)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The outcome variable is built from the original six questions which are averaged to construct the Attitude index. We run factor analysis using polychoric correlation matrices when calculating eigenvalues. This accounts for the fact that answers constitute an ordered factor variable. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.10: Number of refugees as dependent variable

	(1)	(2)	(3)	(4)
<i>A. Attitudes toward ethnic diversity</i>				
<i>Attitude index</i>				
LogNumber	0.019 (0.010)*	0.018 (0.010)*	0.092 (0.040)**	0.101 (0.043)**
LogNumber squared			-0.013 (0.006)**	-0.015 (0.007)**
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
<i>B. Political preferences</i>				
<i>Political index</i>				
LogNumber	0.019 (0.007)***	0.019 (0.007)***	0.112 (0.049)**	0.110 (0.049)**
LogNumber squared			-0.016 (0.008)*	-0.016 (0.008)*
<i>Far Right</i>				
LogNumber	-0.007 (0.002)***	-0.007 (0.002)***	-0.036 (0.017)**	-0.035 (0.018)**
LogNumber squared			0.005 (0.003)*	0.005 (0.003)*
Observations	12,893	12,917	12,893	12,917
Individuals	4,504	4,513	4,504	4,513
Clusters	1,636	1,636	1,636	1,636
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: LogNumber is the logarithm of the number of refugees, and the number of refugees refers to the average number of refugees accommodated in a neighborhood and year across months in which there is at least one refugee present in that neighborhood/year. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.11: Share of refugees as a dependent variable

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Share	0.015 (0.009)	0.014 (0.010)	0.021 (0.020)	0.019 (0.020)
Share squared			-0.000 (0.001)	-0.000 (0.001)
Observations	20,589	20,645	20,589	20,645
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
B. Political preferences				
<i>Political index</i>				
Exposed	0.008 (0.004)**	0.008 (0.003)**	0.027 (0.010)***	0.027 (0.010)***
Exposed Squared			-0.001 (0.001)**	-0.001 (0.001)**
<i>Far Right</i>				
Share	-0.003 (0.001)***	-0.003 (0.001)***	-0.008 (0.003)***	-0.008 (0.003)***
Share squared			0.000 (0.000)**	0.000 (0.000)**
Observations	12,893	12,917	12,893	12,917
Individuals	4,504	4,513	4,504	4,513
Clusters	1,636	1,636	1,636	1,636
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: Share is the ratio of the number of refugees to the total population in a given year and neighborhood. The number of refugees refers to the average number of refugees accommodated in the neighborhood/year across months in which there is at least one refugee present in that neighborhood/year. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.12: Effect of exposure to refugees in adjacent neighborhoods

	(1)	(2)	(3)	(4)
<i>A. Attitudes toward ethnic diversity</i>				
<i>Attitude index</i>				
Exposed close	0.118 (0.048)**	0.113 (0.050)**	0.119 (0.048)**	0.115 (0.050)**
Exposed adjacent neighborhood			-0.022 (0.019)	-0.024 (0.019)
Observations	21224	21282	21224	21282
Individuals	5583	5597	5583	5597
Clusters	1782	1783	1782	1783
<i>B. Political preferences</i>				
<i>Political index</i>				
Exposed close	0.094 (0.052)*	0.094 (0.052)*	0.093 (0.051)*	0.093 (0.052)*
Exposed adjacent neighborhood			0.047 (0.028)*	0.046 (0.028)*
<i>Far Right</i>				
Exposed close	-0.033 (0.018)*	-0.033 (0.018)*	-0.032 (0.017)*	-0.033 (0.018)*
Exposed adjacent neighborhood			-0.018 (0.011)*	-0.017 (0.011)*
Observations	13,336	13,362	13,336	13,362
Individuals	4,639	4,648	4,639	4,648
Clusters	1,675	1,675	1,675	1,675
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: Exposed adjacent neighborhood is an indicator variable that takes a value of 1 if a refugee facility opened in neighborhoods adjacent to the neighborhood of residence of the individual. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.13: Controlling for municipality time trends

	(1)	(2)
<i>A. Attitudes toward ethnic diversity</i>		
<i>Attitude index</i>		
Exposed close	0.155 (0.057)***	0.160 (0.057)***
Exposed far		0.013 (0.026)
Observations	20,589	20,589
Individuals	5,430	5,430
Clusters	1,745	1,745
<i>B. Political preferences</i>		
<i>Political index</i>		
Exposed close	0.112 (0.049)**	0.114 (0.051)**
Exposed far		0.004 (0.041)
<i>Far Right</i>		
Exposed close	-0.040 (0.017)**	-0.041 (0.018)**
Exposed far		-0.005 (0.015)
Observations	12,893	12,893
Individuals	4,504	4,504
Clusters	1,636	1,636
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Time-varying controls	Yes	Yes
Municipal time trends	Yes	Yes

Notes: Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.14: Robustness to multiple hypothesis testing (MHT)

	Standard p -value	MHT p -value
Attitude index	0.030	0.052
Political index	0.002	0.008
Far Right	0.003	0.008
Vote turnout	0.173	0.203
Employment	0.394	0.434
Confidence in government	0.022	0.040
Attitude toward marriage	0.596	0.438
Attitude toward parental care	0.100	0.147
Attitude index (PCA)	0.037	0.076
Attitude index (polychoric)	0.047	0.104
Reciprocity	0.110	0.163

Notes: MHT p -values refer to significance levels corrected for estimating the same specification (i.e., Equation 1) with multiple outcomes using the methodology of Romano and Wolf (2005, 2016).

Table A.15: Dynamic estimation (Borusyak et al., 2021)

<i>Dependent variable:</i>	(1) Attitude index	(2) Political index	(3) Far Right
A. Parallel-trend tests			
Exposed close $t - 3$	-0.017 (0.058)	-0.023 (0.086)	-0.001 (0.031)
Exposed close $t - 2$	0.061 (0.147)	0.010 (0.116)	-0.003 (0.045)
Exposed close $t - 1$	0.131 (0.083)	-0.024 (0.119)	0.007 (0.047)
B. Main effects			
Exposed close	0.115 (0.055)**	0.131 (0.054)**	-0.045 (0.018)**
Observations	20,603	12,890	12,890
Individuals	5,423	4,493	4,493
Clusters	1,745	1,634	1,634
Individual fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All coefficients are estimated using the Stata module DID.IMPUTATION provided by Borusyak (2021). Pre-exposure coefficients in panel A are estimated by a separate regression on non-treated observations only. Observation numbers refer to the main effects regressions. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.16: Intention-to-treat estimates

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Exposed close	0.098 (0.049)**	0.097 (0.052)*	0.097 (0.049)**	0.096 (0.052)*
Exposed far			-0.004 (0.022)	-0.004 (0.021)
Observations	21878	22023	21878	22023
Individuals	5749	5764	5749	5764
Clusters	1771	1772	1771	1772
B. Political preferences				
<i>Political index</i>				
Exposed close	0.111 (0.051)**	0.107 (0.052)**	0.117 (0.052)**	0.113 (0.053)**
Exposed far			0.038 (0.033)	0.036 (0.033)
<i>Far Right</i>				
Exposed close	-0.036 (0.018)**	-0.035 (0.018)*	-0.039 (0.018)**	-0.038 (0.019)**
Exposed far			-0.016 (0.012)	-0.016 (0.012)
Observations	13,639	13,699	13,639	13,699
Individuals	4,765	4,779	4,765	4,779
Clusters	1,669	1,669	1,669	1,669
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample includes individuals who move to another neighborhood between 2014 and 2016, and assigns them to their last observed postcode before moving. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.17: Excluding individuals treated for the first time in 2016

	(1)	(2)	(3)	(4)
A. Attitudes toward ethnic diversity				
<i>Attitude index</i>				
Exposed close	0.116 (0.067)*	0.118 (0.074)	0.114 (0.067)*	0.116 (0.074)
Exposed far			-0.020 (0.023)	-0.019 (0.023)
Observations	20,253	20,309	20,253	20,309
Individuals	5,339	5,353	5,339	5,353
Clusters	1,722	1,723	1,722	1,723
B. Political preferences				
<i>Political Index</i>				
Exposed close	0.122 (0.060)**	0.119 (0.062)*	0.125 (0.060)**	0.121 (0.062)**
Exposed far			0.018 (0.036)	0.017 (0.036)
<i>Far Right</i>				
Exposed close	-0.040 (0.019)**	-0.039 (0.020)*	-0.041 (0.019)**	-0.040 (0.021)*
Exposed far			-0.011 (0.013)	-0.010 (0.013)
Observations	12,684	12,708	12,684	12,708
Individuals	4,429	4,438	4,429	4,438
Clusters	1,614	1,614	1,614	1,614
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The sample excludes individuals who are treated for the first time in 2016. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

Table A.18: Unrelated dependent variables

Attitudes toward marriage	
1.	Married people are generally happier than unmarried people.
2.	People that want to have children should get married.
3.	A single parent can raise a child just as well as two parents together.
4.	It is perfectly fine for a couple to live together without marriage intentions.
5.	For a couple that wants to get married, it is good to first start living together.
6.	A divorce is generally the best solution if a married couple cannot solve their marital problems.
Attitudes toward parental care	
1.	Children ought to care for their sick parents.
2.	When parents reach old age, they should be able to live with their children.
3.	Children that live close by ought to visit their parents at least once a week.
4.	Children ought to take unpaid leave in order to care for their sick parents.

Table A.19: Regression results unrelated dependent variables

	(1)	(2)	(3)	(4)
<i>Attitude toward marriage</i>				
Exposed close	0.026 (0.038)	0.025 (0.039)	0.021 (0.038)	0.020 (0.039)
Exposed far			-0.023 (0.022)	-0.025 (0.023)
Observations	20,579	20,635	20,579	20,635
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
<i>Attitude toward parental care</i>				
Exposed close	-0.108 (0.068)	-0.110 (0.068)	-0.108 (0.068)	-0.110 (0.068)
Exposed far			0.000 (0.027)	0.001 (0.027)
Observations	20,579	20,635	20,579	20,635
Individuals	5,430	5,444	5,430	5,444
Clusters	1,745	1,746	1,745	1,746
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: The two dependent variables computed as averages of the items reported in Table A.18. Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.

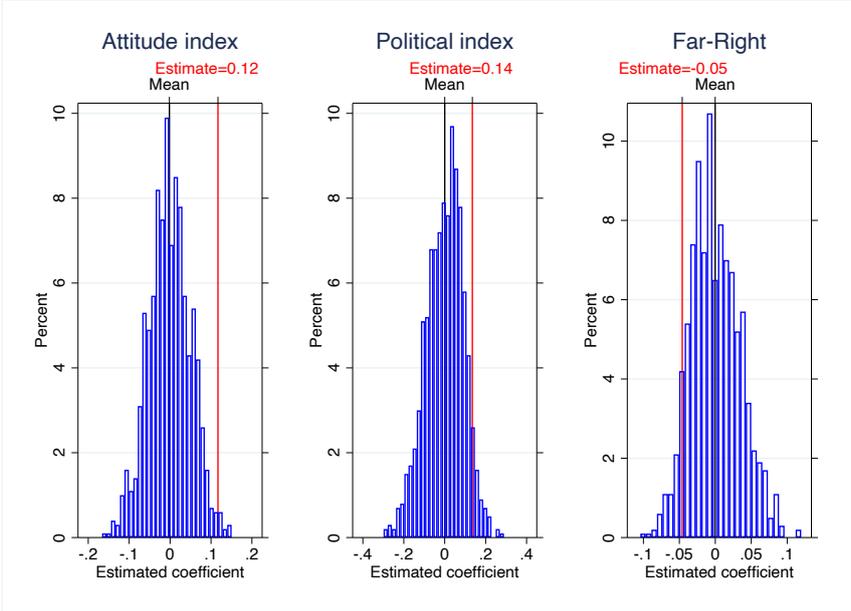
Table A.20: Aggregated municipality-level analysis

<i>Dependent variable:</i>	Attitude index	Political index	Far Right
Exposed municipality	0.026 (0.057)	-0.033 (0.107)	0.018 (0.040)
Observations	1,787	1,697	1,697
Nr. of municipalities	363	361	361
Municipal fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Municipal controls	Yes	Yes	Yes

Notes: Results of regressions based on data aggregated at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are adjusted for clustering at the municipality level.

B Supplementary figures

Figure B.1: Counterfactual treatment estimates



Notes: The figure shows the distributions of counterfactual estimations of the exposure effect. For these estimations we reallocate refugee facilities 1,000 times randomly across neighborhoods in the Netherlands and estimate the effect of exposure. The red lines indicate our estimates based on actual exposure (reported in Table 8).

C Details on the lab-in-the-field experiment

The experiment was set up with a focus on choices of participants in the role of trustee. The number of participants was equal to 619 in December 2014, 591 in December 2015, and 1560 in December 2016 out of respectively 839, 725, and 1967 individuals in the LISS sample.

C.1 Decision-making setting and incentives

In all waves, participants made choices in three binary trust games that varied in terms of gains to mutual cooperation. In each game a trustor decided to trust or not and the matched trustee decided to reciprocate conditional on being trusted. If the trustor decided not to trust, both trustor and matched trustee earned €35 in all three games. If the trustor decided to trust and the matched trustee did not reciprocate, the former earned €20 and the latter earned €85 in all three games. If the trustor decided to trust and the matched trustee reciprocated, then the payoffs were respectively €40, €60, and €80 in the three games.

The participants were informed that they were paid a fixed amount for participating and that, in addition, there was about a one-out-of-forty chance of being paid for their choices. It was explained to the participants that at the end of the research five trustor/trustee pairs would be randomly drawn for payment among the 200 pairs that were expected to participate. The fixed fee was €1.5 and was calibrated by CentERdata on the basis of an expected duration of 6 minutes. In terms of hourly earnings, trustees could expect to earn around €30 an hour.

C.2 Procedures

The procedures related to individuals who made choices in the December 2014 wave of the experiment are documented in the Online Appendix of Cettolin and Suetens (2018). Sections B and C provide a detailed account of the procedures and instructions related to individuals who made choices in the role of trustee, and section F provides a description of the procedures related to individuals in the role of trustor. In the December 2015 and 2016 waves, the instructions related to the description of the experiment, the choice setting and the pay-out rate provided to individuals in the role of trustee were the same as those in the 2014 wave. In the belief elicitation phase, which took place after choice elicitation, a small adjustment was made. Specifically, instead of eliciting all beliefs about the choice of the matched trustor on one computer screen, the beliefs were elicited for each of the three games separately (on three consecutive computer screens).

With respect to the trustors, whose behavior is not relevant for our research but whose choices are needed to match to choices of trustees, the following procedures were followed. In the December 2014 wave a sample was drawn of 382 individuals in the LISS sample with a non-Western immigrant background (what we refer to as *Minority* condition in the main text) and 379 individuals with a native Dutch background (the *Majority* condition). These individuals were each matched to a trustee, who was communicated the first name of the matched trustor. In the December 2015 wave, the same sample of trustors from the 2014 *Minority* condition

Table C.1: Reciprocation rates

	Overall by year			<i>Control</i>	<i>Treatment</i>	
	2014	2015	2016		Before	After
<i>Majority</i> condition	0.60	0.58	0.60	0.60	0.63	0.50
<i>Minority</i> condition	0.55	0.61	0.63	0.61	0.41	0.59
Total	0.57	0.59	0.61	0.60	0.51	0.55
N	350	411	761	1,454	23	45

Notes: Reciprocation rates are shown by year, and depending on individuals in *Control* (who have not been exposed to refugees), and individuals in *Treatment* (who have been exposed to refugees in 2015 or 2016) The reciprocation rate is calculated as the number of times an individual decided to reciprocate across the three trust games divided by three.

(minus those who had left the panel in the meantime) and a random sample of 390 native Dutch participants was used to match to the trustees, who were again communicated the first name of the matched trustor. In the December 2016 wave, we adopted a lower-cost strategy and used a one-to-many strategy in which the same trustor was matched to several trustees, with 28 trustors in the *Minority* condition and 30 trustors in the *Majority* condition. Participants in the role of trustor were recruited among personnel working at Tilburg University. Pairs were again randomly selected for payment using a 1/40 pay-out rate. Each time a trustor-trustee pair was randomly drawn with a trustor that had been randomly drawn already (as part of another pair), we drew a new pair. This way the same trustor could not be paid out more than once.

Table C.2: Main effects with experiment sample

	(1)	(2)	(3)	(4)
<i>A. Attitudes toward ethnic diversity</i>				
<i>Attitude index</i>				
Exposed close	0.309 (0.097)***	0.294 (0.095)***	0.298 (0.098)***	0.284 (0.096)***
Exposed far			-0.051 (0.048)	-0.052 (0.048)
Observations	3248	3248	3248	3248
Individuals	755	755	755	755
Clusters	584	584	584	584
<i>B. Political preferences</i>				
<i>Political index</i>				
Exposed close	0.091 (0.050)*	0.091 (0.051)*	0.115 (0.053)**	0.114 (0.053)**
Exposed far			0.143 (0.073)*	0.151 (0.072)**
<i>Far Right</i>				
Exposed close	-0.055 (0.020)***	-0.055 (0.020)***	-0.063 (0.021)***	-0.063 (0.020)***
Exposed far			-0.039 (0.031)	-0.041 (0.030)
Observations	2179	2179	2179	2179
Individuals	683	683	683	683
Clusters	537	537	537	537
Individual fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Time-varying controls	Yes	No	Yes	No

Notes: Standard errors (in parentheses) are adjusted for clustering at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See the note to Table 8 for additional details.