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The 2015 refugee inflow and concerns over immigration

Katia Gallegos Torres[1]

ZEW–Leibniz Centre for European Economic Research, L7 1, 68161 Mannheim, Germany

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ABSTRACT

How did the large asylum-seeker inflow to Germany in 2015 affect concerns about immigration? Using individual-level panel data for the years 2012–2018 and a policy that allocates asylum-seekers to districts, I identify the effect of exposure to asylum-seekers. In line with the contact hypothesis, living in a high refugee migration district reduced concerns about immigration by 3 pp. Alternatively, a 1 pp. increase in the share of asylum-seekers in the population reduced these concerns by 3.4 pp. The effect appears larger for right-leaning respondents and is driven by districts that do not host a large reception centre. However, the overall trend indicates that after 2015 concerns about immigration increased by about 21 pp. and support for extreme right-wing parties by about 1.7 pp. These trends show considerable heterogeneity for different demographic groups.

1. Introduction

In the second half of 2015, around 868,000 refugees arrived – either by sea or land – to Europe. This number was almost six times larger than the arrivals during the first half of 2015 (approx. 147,000 refugees) (UNHCR, 2018). This large increase in only a few months depicts the rapid escalation of forced migration due to civil conflicts. Germany alone received about half a million first-time asylum applications in 2015, representing 35.2% of all applications in the European Union (Eurostat, 2016). Refugee migration was not only a topic highly present in the media but also in the political arena. During this time, support for the Alternative for Germany (AfD) – the largest right-wing populist (RWP) party in Germany – increased rapidly in the polls and in regional elections (see Fig. 1). This leads to the question of what role the arrivals of asylum-seekers has played in shaping political preferences and concerns over immigration from the German population.

Few papers have studied how individual attitudes towards immigration changed after the refugee arrivals of 2015 (Altındağ and Kaushal, 2021; Hangartner et al., 2019; Schaub et al., 2020). Typically, these use cross-sectional surveys with either a difference-in-differences design, instrumental variables or an experimental component. Most of the recent literature studying the refugee arrivals of 2015 has focused on electoral outcomes using aggregate data at the district or municipality level (Steinmayr, 2021; Bredtmann,

E-mail address: katia.gallegostorres@zew.de.

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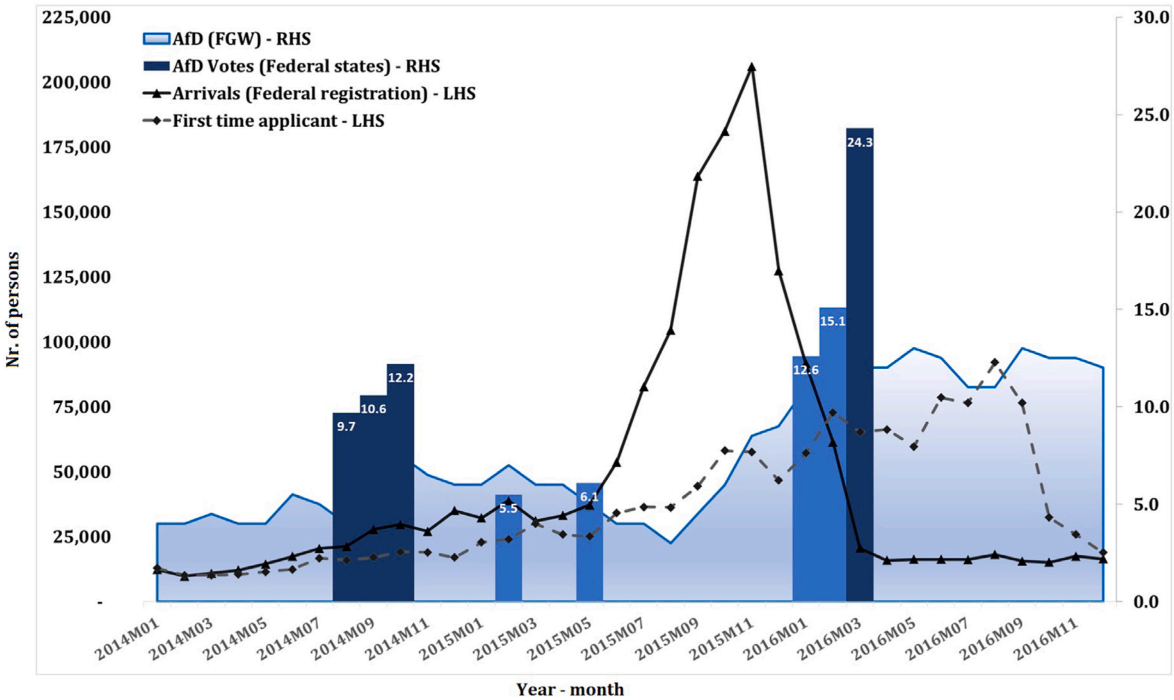


Fig. 1. Number of asylum-seekers' arrivals and first-time applications in Germany (2014–2016), AfD vote shares in state parliament elections and voting intention. *Notes:* In 2015 1,091,984 asylum-seekers were registered in the EASY-system and 441,800 first-time applicants submitted an asylum request. However, since no personal data was registered in the EASY system, it was not possible to exclude false and double entries. Furthermore, many asylum-seekers continued their journey (to other EU Member States). "Only with completion of the re-registrations until September 2016, it became clear that the number of entries in 2015 had actually been around 890,000 people" (Federal Office for Migration and Refugees, 2016). The solid line shows the arrivals to Germany and the dashed line the number of first-time applications. Both are depicted on the left-hand axis. The bars show election results for the AfD in regional parliament elections (*Landtagswahlen*) that took place during the plotted period. Dark-blue bars show East-German states, and light-blue ones West-German states. The shaded area shows the evolution of voting intention for the AfD (polling results from the *Forschungsgruppe Wahlen e.V. (FGW)*). The bars and shaded area are depicted on the right-hand axis. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
Source: Der Bundeswahlleiter (2017), Federal Ministry of the Interior, Building and Community (2017), Zicht and Cantow (2015), Eurostat (2018).

2022; Gehrsitz and Ungerer, 2022; Tomberg et al., 2021; Dinas et al., 2019; Gamalerio et al., 2021; Vertier et al., 2020)[2] which does not allow for the identification of changing individual preferences and how this change is shaped by individual characteristics. These studies describe two types of effects. First, at the macro-level, all citizens are exposed to the information that refugees are coming (via the media, political rhetoric, etc.). Second, at the micro-level, citizens are exposed to asylum-seekers[3] and might have direct contact with them (e.g. in their neighbourhood) which could change individual attitudes—as described by the contact hypothesis (Allport, 1954).

I separate both effects using individual-level panel data for the years 2012–2018 from the German Socio-Economic Panel (Goebel et al., 2019) which I merge with the inflow of asylum-seekers to Germany in 2015. This large and unexpected inflow together with the rules governing the allocation of asylum-seekers, which entail national and state quotas, provides a one-time shock with geographical variation. Since the share of allocated asylum-seekers varies across districts, this allows me to compare how their presence affects individual concerns over immigration and political preferences. For identification, I use the dispersal policy that assigned asylum-seekers to districts[4] together with the panel structure of my data. As stressed by Gehrsitz and Ungerer (2022), since the state quotas depend on tax revenues and population, the allocation of refugees is not completely random but does not allow for self-selection. However, given that shortage of accommodation was a major issue in 2015, one concern is selection on the receiving district's side: available accommodation might not be equally distributed across districts. Likewise, more welcoming districts could have created additional space for receiving asylum-seekers while the opposite might be true for districts in which residents were more reluctant to host asylum-seekers. To control for these unobservable characteristics, I estimate a dynamic difference-in-differences model with individual fixed effects. I assume that conditional on the individual fixed-effects the allocation of asylum-seekers was as good as random.

[2] See Cools et al. (2021) for a systematic review.
[3] For the purpose of this paper, I use the terms *refugees* and *asylum-seekers* interchangeably.
[4] There are 401 districts (*Kreise*) in Germany. Equivalent to the NUTS-3 level of the European classification. On average, districts have 200,000 inhabitants.

The general time trend suggests that after the large inflow of asylum-seekers to Germany (2016–2018) there was an increase in concerns over immigration by 21 percentage points (pp.) at the national level. Yet, concerns over immigration were almost 3 pp. lower among respondents living in high refugee migration districts relative to those living in low refugee migration districts.[5] This exposure effect is in line with the contact hypothesis (Allport, 1954). Similarly, Cools et al. (2021) hypothesise that immigration at the national level might be more important than local exposure to immigrants, which is in line with my findings. Political preferences for extreme right-wing parties increased by 1.7 pp. after the arrival of asylum-seekers at the national level (time trend). Yet, individual exposure – to the arrival of asylum-seekers in one's districts of residence – did not contribute to this increase. Furthermore, heterogeneity analysis shows that the reductions in concerns over immigration were larger among right-leaning individuals – comparable to the findings from Steinmayr (2021) and Bansak et al. (2016)[6] – and among those living in districts without a large reception centre, akin to Bredtmann (2022), Gamalerio et al. (2021), Vertier et al. (2020). Additionally, I find that the reductions in concerns over immigration due to exposure to asylum-seekers in the district of residence were overall similar across subgroups (sex, age, education, urban/rural, East/West Germany), similar to Bansak et al. (2016). Nonetheless, the point estimates suggest that the dynamics of the response might differ across subgroups, although these differences are not statistically different from each other. For example, exposure to asylum-seekers saw larger reductions in concern for female respondents, older individuals (over age 45), those with tertiary education, and those living in a high-income household. In addition, respondents living in West German and urban districts had larger and more persistent reductions in concerns until two years after the inflow. By the third year, the trend seems to reverse for respondents living in rural districts. Moreover, among respondents living in a low unemployment district the reduction in concerns over immigration is larger, showing that socio-economic context variables matter (Arzheimer, 2009; Tomberg et al., 2021). Notwithstanding, I do not find a significant effect of exposure to asylum-seekers on individual economic concerns such as the own economic situation and job security. I find a significant reduction in concerns over crime. The relation between concerns over immigration and crime has been well-described in the literature (Dehos, 2021). It is reassuring that my results find that the exposure effects for both concerns are similar in terms of direction and size.

My paper contributes to the literature in several ways. First, to the best of my knowledge, it is the first study that uses individual-level panel data for analysing exposure to the asylum-seekers inflow of 2015 in the district of residence.[7] Until now, papers studying this inflow for Germany have only used aggregate data to analyse how voting outcomes react to the share of asylum-seekers in a municipality/district (Gehrsitz and Ungerer, 2022; Bredtmann, 2022). Hence, they focus only on the micro-level effect. An advantage of my data is that I can also approximate the macro-level effect with year dummies that capture trends in concerns. I find that the macro-level effect (time trend) is larger than the micro-level exposure effect. The former result is close to the one found in Sola (2018) who shows that respondents from the 2015 wave of the German Socio-Economic Panel (SOEP), who were interviewed after June 2015, experienced an increase in concerns over immigration of 6 pp.[8] Although at the micro-level exposure to a higher share of refugees favours the contact hypothesis, by reducing concerns over immigration, I do not find a statistically significant effect of the share of allocated refugees on preferences for RWP parties. This is in line with Gehrsitz and Ungerer (2022) who do not find an effect of either refugee inflows or reception centre capacities on support for the AfD in the 2017 elections using district-level data. Instead, when using municipality-level data for the state of North Rhine-Westphalia, they find a small negative effect of the number of asylum-seekers on AfD's vote share. Similarly, Bredtmann (2022) find that in municipalities with zero inflows of refugees, the vote-share for RWP parties increased more than in municipalities with small but positive inflows of refugees. My findings are also in line with results for Austria (Steinmayr, 2021), France (Vertier et al., 2020), Italy (Gamalerio et al., 2021) for the 2013–2015 refugee arrivals, and (Dustmann et al., 2019) who study refugee migration to Denmark in the 1990s. All of the aforementioned studies find evidence supporting the contact hypothesis, although only in urban municipalities in the case of Dustmann et al. (2019).

Second, I describe effect heterogeneities based on individual and district characteristics. Until now, only two studies for Germany have performed a similar analysis (Schaub et al., 2020; Tomberg et al., 2021). Schaub et al. (2020) conduct an own survey in East German municipalities in 2018, where some municipalities received almost no refugees. They report null effects of exposure on right-wing support and find that younger respondents react more negatively to exposure. Finally, Tomberg et al. (2021) find that the inflow of asylum-seekers during 1998–2017 together with economic downturns (e.g. districts with high unemployment) increases voting for RWP. This paper complements both approaches.

Third, I test whether the inflow of asylum-seekers also affected other economic and social concerns. Many studies have investigated the determinants of attitudes towards immigration. I identify two large groups: studies that describe political-economy models favouring motives of economic self-interest (Mayda and Rodrik, 2005; Mayda, 2006; O'Rourke and Sinnott, 2006; Hanson et al., 2007; Facchini and Mayda, 2008) and another branch that argues individuals are not predominantly motivated by self-interest, but by other cultural and social beliefs (Hainmueller and Hiscox, 2010; Card et al., 2005; Hainmueller and Hopkins, 2014; Hopkins,

5 In my main analysis, I define low and high refugee migration districts based on their 2015 refugee allocation and their 2013 population. Districts above the median of the weighted distribution of refugee inflow (875 allocated refugees per 100,000 inhabitants) are labelled "high refugee migration districts", while districts below the median are labelled "low refugee migration districts".

6 Khalil and Naumann (2021) also find larger effects of contact for right-leaning individuals. They use the same outcome variable as this paper, but their main variable of interest is "declared personal visits to and from foreigners".

7 Matakos et al. (2021) and Lahdelma (2021) use Finnish panel data on politicians' policy preferences (pre-election pledges) to study how candidates' policy positions about redistribution and immigration change upon the arrival of asylum-seekers.

8 Using the SOEP for 2001–2015, Czymara and Dochow (2018) find that discussions in the German media around immigration topics, increase concerns about immigration by 13 pp.

2010; Card et al., 2012).[9] I do not find evidence in favour of the former: economic concerns are not affected by the inflow of asylum-seekers in the respondent's district of residence. I interpret the reductions in concerns over immigration and crime due to a higher exposure of asylum-seekers as support for the contact hypothesis, which falls into the latter branch of the aforementioned literature.

Fourth, I study the effects of asylum-seekers rather than economic migrants, where the latter has been the focus of much of the previous literature. Refugee migrants differ from economic migrants not only in the motives for migration but in how they economically and socially integrate into the host country (Brell et al., 2020). The impact of (economic) immigration (immigrants as a share of the population) has been broadly studied in the literature, mostly focusing on labour market outcomes, crime rates, and most recently on voting outcomes of RWP parties. In general, immigration poses an empirical challenge due to its endogeneity: migrants self-select in terms of destination. The political economy literature thus far has made use of quasi-random variation (Dustmann et al., 2019; Dinas et al., 2019; Hangartner et al., 2019) and shift-share IV approaches (Otto and Steinhardt, 2014; Mayda et al., 2022; Barone et al., 2016; Halla et al., 2017) to estimate the true effect of immigration on electoral outcomes. Edo et al. (2019) state that papers studying the impact of immigration on voting behaviour mostly find a positive effect on support for RWP/anti-immigration parties (effect sizes ranging between 0–3 pp.). My findings, on the other hand, support the contact hypothesis as in Dustmann et al. (2019), Steinmayr (2021), Vertier et al. (2020), Gamalerio et al. (2021). Steinmayr (2021) states that it is important to note these findings do not necessarily contradict those from the migration literature: likely the specific context under consideration (refugee migration) drives the difference in findings since more humanitarian concerns are taken into account, as suggested by Bansak et al. (2016) and Czymara and Schmidt-Catran (2017).

The following eight sections are organised as follows. Section 2 provides some theoretical considerations on attitudes and preference formation. Section 3 briefly describes the refugee crisis of 2015 and the German asylum system. Section 4 describes the data sources and presents descriptive statistics. Section 5 describes the empirical strategy, identification and treatment. Section 6 presents the main results. Section 7 describes the heterogeneity analysis by subgroups. Section 8 presents sensitivity checks and Section 9 concludes.

2. Theoretical considerations

The sociological, social psychology, and political science literature have all broadly investigated how exposure (with or without contact) to immigrants shapes the attitudes and political preferences of the receiving population. Here I will briefly describe the theories relevant for my analysis. Comprehensive reviews can be found in Hainmueller and Hopkins (2014) and Ceobanu and Escandell (2010).

2.1. The contact hypothesis

Currently known as the intergroup contact theory (Dovidio et al., 2005), this is based on the idea that contact allows knowledge of the out-group in turn reducing prejudice (Pettigrew et al., 2011). Allport (1954) mentioned that under certain conditions (equal status of the groups, common goals, intergroup cooperation, and authority support) interactions between opposing groups could reduce prejudice. This theory has found wide support among recent papers studying attitudes towards immigration and voting behaviour (Dustmann et al., 2019; Steinmayr, 2021; Hornuf et al., 2017; Bredtmann, 2022; Khalil and Naumann, 2021).[10] Although the optimal contact conditions facilitate the decrease of prejudice, they are not essential (Pettigrew et al., 2011). Moreover, Pettigrew and Tropp (2008) describe the three most-studied mediators of contact: increased knowledge, anxiety reduction and enhanced empathy. They mention that positive contact enhances empathy and allows the consideration of the out-group's perspective (how they feel and view the world) (Todd and Galinsky, 2014). Besides increasing empathy, intergroup contact can also enhance positive intergroup emotions that lead to a reduction in prejudice (Pettigrew et al., 2011). Christ et al. (2014) find that positive contact reduces prejudice at the group level not only via direct contact but because you are influenced by the behaviour of other in-group members. Furthermore, intergroup friendship plays a major role. However, negative contact can occur, boosted by involuntary contact and the feeling of being threatened (Dinas et al., 2019; Steinmayr, 2021; Hangartner et al., 2019; Enos, 2014).

2.2. The realistic group conflict hypothesis

As mentioned by Hangartner et al. (2019) proximity does not always lead to positive contact, and leads to an increased perception of threat. Intergroup interactions are seen as a competition leading to a zero-sum game (Blumer, 1958; Forbes, 1997; Quillian, 1995). Immigrants are perceived as an ethnic, cultural, and economic threat to the in-group. Therefore, the larger the out-group, the larger the perceived threat. In recent decades, social scientists have mainly differentiated two types of explanations for anti-immigrant attitudes: economic concerns and cultural/identity concerns. Economic concerns are evidenced either via fiscal and welfare (contributions) concerns (Hanson et al., 2007; Facchini and Mayda, 2008) or labour market competition (Mayda and Rodrik, 2005; Geishecker and Siedler, 2012; Lancee and Pardos-Prado, 2013). Cultural concerns arise due to religious, ethnic, or linguistic differences (cultural distance) (Hangartner et al., 2019).[11] Often, the literature has found little evidence supporting

[9] Other studies focus on the relationship between misperceptions about immigrants and support for redistribution (Alesina et al., 2022) and how exposure to violence increases the perceptions of risk associated with hosting refugees (Braithwaite et al., 2019).

[10] On the other hand, Bohrer et al. (2019) and Schneider et al. (2020) do not find support for the contact hypothesis.

[11] Using district-level data, Riaz et al. (2021) show, in the context of refugee migration to Germany, that local crimes attributed to immigrants increase the probability of hate crimes (which last for about four days after the event). This effect is likely driven by the mobilisation of already radicalised individuals, i.e. in areas with higher support for far-right parties.

economic concerns (Hainmueller and Hopkins, 2014). Furthermore, Lancee and Pardos-Prado (2013) indicate that most of the literature assumes ethnic threat happens at the group level, not at the individual one. But threat can also happen at the individual level, for example, an individual's fear of becoming unemployed might also change their perception of threat.[12] The perception of threat can therefore be collective and individual (Lancee and Pardos-Prado, 2013).

By definition, both theories – contact or threat – are not mutually exclusive but the question is which of the two dominates in a particular setting. More migrants at the group/regional level might induce concerns, but also make contact more likely. From the mixed evidence in the literature, we know the presence of refugees/migrants in a geographical area can induce both feelings of threat and positive feelings simultaneously. In absolute terms, the level (and types) of concern may increase in a given geographical region, but increased contact may also lessen these concerns. Given the particular setting of refugee migration in 2015 that I describe in the next section, I hypothesise that the contact hypothesis may play a larger role.

3. Background

3.1. The German asylum system until early 2016

According to the Federal Office for Migration and Refugees (Seedorf, 2014), as soon as all asylum-seekers reached the German border, they had to report their willingness to seek asylum to the border authority. They could also do so thereafter – within the country – by reporting to a security authority. The respective authority would then send the asylum-seekers to the closest *Erstaufnahmeeinrichtung* (EAEs, by its initials in German, or initial reception centres) where basic personal information would be entered into the EASY-system[13] for the posterior re-assignment of the asylum-seekers via a quota system: asylum-seekers are distributed according to the "Königsteiner Schlüssel" across federal states.[14] Hence, asylum-seekers might be assigned to an EAE in a different state to the one in which they arrived. At the assigned initial reception centre, asylum-seekers received a document of proof of arrival (*BüMA* until March 2016, or *Ankunftsnachweis* afterwards), were given accommodation and food and were told to register at the local Immigration Authority. They needed to wait for an appointment from the Federal Office for Migration and Refugees (BAMF, by its initials in German) in order to submit their asylum application.[15] After submitting their application, asylum-seekers were given a temporary residence permit and their status changed from asylum-seekers to asylum applicants.

Until 2014, 36 out of 401 districts in Germany hosted an initial reception centre. This number increased to 148 in late 2015. Due to the massive arrivals of asylum-seekers, many "emergency shelters" (*Notunterkünfte)* were opened during late 2015 and early 2016. The initial reception centres were vast compounds with an average size of 720 beds in 2014 and 1,090 in 2015 (data from Gehrsitz and Ungerer (2022)). The initial reception centre to which asylum-seekers are assigned may be responsible for both temporary and longer-term accommodation. The allocation to a specific reception facility depends on current capacities as well as the BAMF branch offices' competence by country of origin. Not all BAMF branch offices are responsible for processing asylum applications from all nationalities. Asylum-seekers may stay at the reception facilities for up to six months or until a decision is made on their application; during this period they are not allowed to work.[16] Only after this period are they assigned to follow-up accommodations, typically run by the districts in the federal state. Yet, back in 2015, the authorities were trying to quickly send the asylum-seekers to the subsequent accommodation to free space for the new arrivals in the initial reception centres. So, many asylum-seekers waited for the final decision on their application in the follow-up accommodation (Geis and Orth, 2016). Most of the German states used a two-level distribution system. In the first level, the states are responsible for providing accommodation in the initial reception centres. In the second level, asylum-seekers are transferred to group accommodations or apartments under the responsibility of the districts (*Kreise*) and municipalities (*Gemeinde*) (Geis and Orth, 2016; BBSR, 2017).[17] These distribution systems are regulated under the laws of each federal state, so in practice, one can consider having sixteen different distribution and accommodation systems (Beinhorn et al., 2019).[18] Allocation to a particular district depends on the federal state's own distribution rules, which assign quotas while taking

[12] This relates to the operationalisations of threat either via contextual (macro) variables such as immigration figures, unemployment and growth rates, charismatic leaders, media coverage (Arzheimer, 2009, 2017), or, with individual social background characteristics. Generally, less educated people, manual workers, unemployed persons, non-religious people, youths and men tend to vote for RWP/anti-immigration parties (Lubbers, 2001; Arzheimer, 2009, 2017; Goodwin and Heath, 2016; Alabrese et al., 2019; Becker et al., 2017).

[13] An IT-system for the allocation of asylum-seekers to the federal states. EASY stands for *Erstverteilung der Asylbegehrenden* (Initial Distribution of Asylum-Seekers).

[14] Defined on the previous year, according to the state's tax revenues (two thirds) and population (one third). This quota is assigned to each state annually to ensure an even spread of social burdens, in this case of asylum-seekers in particular. See Table A.1 for the 2015 state quotas.

[15] During 2016 processes were initiated to integrate the asylum-seeking procedures such that all responsible agencies are under one roof and avoiding long wait periods (Federal Office for Migration and Refugees, 2016).

[16] Until October 2015, the maximum number of months an asylum-seeker could stay in an initial reception centre was three months. This was increased for up to 6 months for families with children, and 18 months for asylum-seekers without children (§47 *Asylgesetz, AsylG*).

[17] The city-states Hamburg and Berlin only had a one-level system, given that they are also directly responsible for running the follow-up accommodation. The states of Baden-Württemberg, Bavaria, and Schleswig-Holstein had a three-level system, where on the second level the responsibility of accommodation lies in the administrative districts (*Regierungsbezirke*), and only on the third level, the asylum-seekers are sent to the districts and municipalities.

[18] Most federal states make suggestions about the minimum standard of group accommodations in their laws. For example, in the case of Baden-Württemberg, it is suggested that the location (of a group accommodation) should enable participation in social life, should have at least one common room and one room for children, and there should be an outdoor area for the leisure activities. Furthermore, the living and sleeping area should be at least 7 square metres per resident (Wendel, 2014).

into account the population size of a district, among other factors.[19] However, during the peak of the refugee crisis the share of refugees allocated to districts differed slightly from the state assigned quotas due to capacity constraints from the facilities (Gehrsitz and Ungerer, 2022; Bredtmann, 2022), which created additional variation in the allocation across districts (see Fig. A.2).

Furthermore, asylum-seekers are entitled to asylum benefits as soon as they state their desire to apply for asylum and have received a document of proof of arrival *(BüMA* or *Ankunftsnachweis)*.[20] Hence, data on recipients of asylum benefits might be more reliable than records from the Central Register of Foreign Nationals (*Ausländerzentralregister*, AZR) – at least for 2015 – given that there was a lag in registration of asylum applications in that year (see Fig. 1).

3.2. The unfolding of the refugee crisis in Germany

In early September 2015, Hungary was unable to register any more asylum-seekers, and the decision was made to send them to the Austrian Border. Germany and Austria agreed on opening their borders to allow asylum-seekers entry without being subject to border controls. Several days prior, on August 31, Chancellor Angela Merkel stated at the Federal Press Conference *"Wir schaffen das!"* (We can do it!), since Germany had temporarily abandoned the Dublin Regulation, i.e. allowing asylum-seekers to fill in applications in Germany even though it was not their EU country of entry.[21] From September 5 onwards, refugees arrived faster than expected and by September 13 all border controls with Austria were temporarily re-established. But it was only until early 2016 that this high influx of refugees ceased. Austria and the Balkan countries started to close their borders, and in March 2016 the European Union and Turkey came to an agreement that Turkey would retain the refugees and prevent onward migration to EU countries in exchange for 6 billion Euro in humanitarian aid for the refugees.[22]

The solid line in Fig. 1 displays the registration of asylum-seekers (EASY-system) and the dashed-line shows the number of first-time applicants. The bump in applications has a lag with respect to the arrivals given the waiting time for receiving an appointment to register an application. Shortly before the large influx of asylum-seekers, some states had regional parliament elections while some occurred shortly after. The bars show the results for the AfD (RWP party) in these elections. In order of appearance in Fig. 1 these states are: Saxony, Thuringia, Brandenburg, Hamburg, Bremen, Rhineland-Palatine, Baden Württemberg and Saxony-Anhalt. For comparison, East German states are depicted in dark blue while West German states are depicted in light-blue. Although the AfD vote shares were already higher in East German states prior to these arrivals when compared to West Germany, their vote shares sharply increased in both East and West German state elections.

3.3. Refugees welcome and contact with Germans

During their first three months in Germany, asylum-seekers are obliged to remain within the limits of the neighbourhood (*Bezirk*) of the Immigration Authority where the reception centre is located (*Residenzpflicht*) and need additional permission if they want to move beyond these administrative boundaries. However, they are free to leave and enter the assigned reception centre upon presentation of their photo ID-cards. Hence, contact with the local population is possible outside the reception centres but limited to a particular neighbourhood. When in the follow-up accommodation (such as collective accommodation or apartments) in the districts, they might be subject to a less stringent residential constraint: some states only allow free mobility within the boundaries of a district, or the state as a whole (*Wohnsitzauflage*).[23] These restrictions last for up to 3 years or until the asylum-seeker has found a "sufficient" job. Hence, due to fewer mobility restrictions and a potential longer stay, it seems more likely that asylum-seekers establish more meaningful contact when in the follow-up accommodations.[24]

The IAB-BAMF-SOEP Survey of refugees (Brücker et al., 2016) shows that in 2016 refugees have built up an average of three new contacts with Germans. The large influx of refugees in 2015 awoke a willingness to help in the German society. Karakayali and Kleist (2016) describe this as the *"Sommer des Willkommens"* (Summer of welcome): 88% of refugees that arrived in 2015/2016 declared they felt (very) welcome upon arrival (Brücker et al., 2016) and around 6% of the SOEP respondents in 2016 declared they had worked with refugees directly (e.g., accompanying them to government agencies, providing support in language learning) since 2015. If we scale this to the German population, this indicates around 4.8 million individuals volunteered during 2015. Additionally, about 27% declared they donated money or goods to help refugees. Furthermore, a representative survey of the organised civil society in Germany describes that around 15% of all organisations (about 90,000) were engaged in supporting refugees (Priemer and Schmidt,

[19] Some states, such as North-Rhine-Westphalia and Brandenburg, also take into account the area of a district or the number of employees subject to social security contributions, but these criteria are only given a weight of 10%. The overall quotas to the districts very much resemble a pure allocation by population.

[20] See Appendix B for further description.

[21] The Dublin Regulation stipulates which country is responsible for the asylum procedure in the European Union (EU). It is often the first EU country in which the asylum-seeker arrived, which back in 2015 posed a big burden for border states such as Greece, Italy and Hungary. The most common route was to enter via the Balkan countries through Turkey or the Mediterranean Sea, but many immigrants try to reach richer countries than their port of entry. In this sense, in 2015 Germany was the country receiving the highest amount of asylum applications (476,508) followed by Hungary (177,134), Sweden (162,451), Austria (88,159), Italy (83,540) and France (76,163) (UNHCR, 2018). Azarnert (2018) describes how refugee resettlement could be a potential solution.

[22] For studies engaging with the motives of forced migration, please refer to Micevska (2021), and Hernandez and Rudolph (2015).

[23] The states that do not pose any mobility restrictions after the first three months are Brandenburg, Bremen, Mecklenburg-Vorpommern and Thuringia.

[24] Bilateral communications I had with authorities from two federal states, would confirm this. Here is a quote from one of the interviews: "In principle, the residents of the initial reception centres can have contact with locals from the first day. Within their residence obligation, they can move freely (within a certain location) and also receive visitors. How realistic that is, I leave it open. Contact is more likely to come from outside (churches, welfare organisations offering advice, sports clubs). With the move to communal accommodation, contact is likely to intensify."

2017). Karakayali and Kleist (2016) conducted an online survey with over 2000 volunteers in Germany and report that most of the support in 2015 (27%) came from self-organised groups and local initiatives/projects (18%). Only around 13% of volunteers belonged to a registered association. Most of the help provided were language courses, supporting other volunteers, and dealing with authorities. State agencies also recognise that the challenging situation of late 2015 could hardly have been addressed without the support of volunteers (Institut für Demoskopie Allensbach, 2016). Regarding motives for volunteering, 92% of respondents mentioned "sense of community with other volunteers" as an important factor, 67.4% stated their "interest in people from other cultures", and 94.3% mentioned "learning new things about the world and other cultures" as a reason for their commitment.[25] As stated by Karakayali and Kleist (2016, p. 32): "it is by no means about voluntary work in general, but specifically about cooperation with migrants, even if this was not necessarily the original reason for the commitment."[26] Another survey of volunteers shows that 81% of respondents declared that the "arrival of refugees in their own place of residence or in their own region" played a major role in their decision to engage in volunteer work (TNS Infratest Politikforschung, 2016). This would go in line with the emergence of a great identification in the neighbourhoods regarding "our refugees" (BBSR, 2017).

From these descriptions, it seems very likely that individuals had contact with refugees at their place of residence. Moreover, most of the conditions described by Allport (1954) were met after the 2015 arrival of refugees. The common goal was to integrate and help refugees in the asylum process, and there was vast intergroup cooperation, and authority support (from both local authorities and volunteers). Furthermore, for those individuals with direct contact the mediators of intergroup contact towards positive effects were present: increased knowledge of the out-group, enhanced empathy, and the adoption of the out-group's perspective (Pettigrew et al., 2011). However, as mentioned by Christ et al. (2014) direct contact is not necessary to effect a reduction in prejudice. Given that when living in a reception centre, contact with the hosting population is more difficult, I would expect larger effects in districts that do not host refugees in a reception centre.

4. Data

4.1. German Socio-Economic Panel (SOEP)

The SOEP is a longitudinal survey of private households in Germany and has been carried out since 1984 by the German Institute for Economic Research (DIW, Berlin). I use data until 2018 (wave N^o 35), containing information for around 22,000 households and 35,000 adults. This dataset contains information on household composition and characteristics across all 16 German federal states, as well as information on all household members (employment, education, perceptions, etc.). The standard dataset identifies only the federal states, although access to further spatial disaggregation (e.g. at district level) is possible through the SOEPremote (on-line access) (Goebel et al., 2019; Liebig et al., 2019). This paper uses the SOEP data to evaluate changes in concerns about immigration and the support to political parties across time (during 2012–2018), exploiting the panel structure of the dataset. The following questions are relevant for constructing the variables of interest:

1. Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party? (Yes/No)
2. (If Yes) Which party do you lean toward?
3. (Since 1999) How concerned are you about the following issues? (The economy in general, your own economic situation, your own retirement pension, your health, environmental protection, the impacts of climate change, maintaining peace, crime in Germany, social cohesion in society, immigration to Germany, hostility towards foreigners or minorities in Germany, (if employed) your job security)

Table A.2 in the Appendix shows descriptive statistics from the Federal Statistical Office (Destatis), from the SOEP for the year 2016 (weighted), and for my main sample (weighted and unweighted). Even after my sample restrictions there are no major differences between my main sample of analysis and the official statistics. Column (1) shows official statistics from Destatis for the year 2016. Column (2) shows the descriptive statistics from the SOEP using cross-sectional weights, which reflect the official numbers quite well. Columns (3) and (4) show the descriptive statistics from my main sample of analysis (weighted and unweighted), which are also very much in line with the official statistics, validating the representativeness of the survey. Fig. A.1 in Appendix A displays the relationship between German federal election outcomes (y-axis) and the declared party affinity for the respective parties from the SOEP (x-axis). Their correlations are positive and significant at the 1% level, validating the use of the SOEP dataset for studying political preferences.[27] I group the political parties as follows: right-wing parties (NPD, REP, DVU, and AfD since 2013), centre-right parties (CDU/CSU, FDP), centre-left parties (SPD, Gruene/Buendnis90), and left-wing parties (Die Linke). I create dummies for these four groups of parties that equal one if the respondents lean towards any of the aforementioned parties and zero otherwise. If the respondent does not have a party preference, I code it as missing.

[25] 43.6% said that another reason for their commitment was the fact that support for refugees was important in their social environment, and 55% of those who started volunteer work in 2015 declared media reporting on the difficulties of the refugees as an additional motive for offering their support.

[26] Own translation.

[27] Among the studies that have used the SOEP to study changes in attitudes towards immigration/political preferences are: Avdeenko and Siedler (2017), Geishecker and Siedler (2012), Poutvaara and Steinhardt (2018), Khalil and Naumann (2021), Sola (2018), Goebel et al. (2015), Lancee and Pardos-Prado (2013), Czymara and Dochow (2018).

The answer options for the third question (about concerns) are: *very concerned, somewhat concerned, and not concerned at all*. I recode these answers to a dummy that equals one if the respondent is "very concerned" about an issue. Furthermore, the SOEP question is about "immigration to Germany" not specifically "refugee migration". Hence, for the years prior to the refugee crisis respondents might have had in mind their "standard" concerns on immigration (as described in the literature, i.e. economic self-interest, fiscal concerns, etc.), but during the refugee crisis they might have had different considerations, e.g. humanitarian concerns, deservingness, etc. Bansak et al. (2016). Therefore, I restrict the sample to years after 2011 when refugee migration started to receive more attention in terms of public debate.

4.2. Administrative data at the district level

I merge socio-demographic characteristics and the number of asylum-seekers (*Schutzsuchende*) by district. District-level covariates (such as population by gender, youth quota, educational attainment, unemployment rates and per capita GDP) are extracted from INKAR (BBSR, 2018). Data on asylum-seekers by district were obtained from the Research Data Centre (RDC) from the Federal Statistical Office (RDC, 2015), who report statistics on recipients of asylum-seekers' benefits (*Statistik der Empfänger von Asylbewerberregelleistungen*). These data are reported for December 31 of each year at the district (*Kreise*) and municipality (*Gemeinde*) level. Hence, there is coverage for the period in which the large inflow of refugees occurred, in 2015. Furthermore, to better measure this inflow, I restrict the data to those asylum-seekers that started receiving benefits in 2015 (approximately 82% of the sample in 2015, or 803,225 asylum seekers). Since asylum-seekers must stay in their designated place of residence for at least 3 months, these data best reflect the inflow of refugees that arrived to Germany during the peak of the refugee crisis, from mid-2015 until the beginning of 2016, as shown in Fig. 1. Using the allocation of asylum-seekers per 100,000 inhabitants to each district in 2015, I define low and high refugee migration districts (based on their 2013 population). Those districts who are above the median of the weighted distribution of refugee inflow (which is 875 allocated refugees per 100,000 inhabitants) are labelled "high refugee migration districts", while districts below the median are labelled "low refugee migration districts".

4.3. Descriptive statistics

Table A.3 in Appendix A presents descriptive statistics by treatment status and their differences. The top panel shows the outcome variables that will be analysed in the next section while the remaining are covariates. In the pre-treatment period (average over the years 2012–2014) people living in low refugee migration districts had fewer concerns about immigration to Germany compared to those living in high refugee migration districts (3 pp. less). Post-treatment this difference has reduced to almost zero. Although an increase in concerns over immigration can be observed for both groups: pre-treatment only 22.1% of respondents were very concerned about immigration, while post-treatment this share increased to 39.7%. Around 48% of the interviewees did support a political party in the pre-treatment period, while in the post-period this percentage reduced to 46% in both groups. Pre-treatment support for a political party was 3.6 pp. lower in low refugee migration districts (46.4% vs. 50% in high refugee migration districts) but post-treatment this difference reduced to 1.8 pp. (45% vs. 46.8%).

Support for RWP parties increased in the post period for both groups, from 0.7% to 2.6%. This increase was 0.4 pp. higher in low migration districts, and statistically significant at the 1% level. Support for centre-right and centre-left parties decreased in the post-treatment period, while support for the left remained the same. Support for centre-left and left parties is significantly lower for individuals living in low refugee migration districts. Furthermore, the sample differs in age (slightly younger interviewees in low refugee migration districts), share of employed individuals (higher in low refugee migration districts), log household income (higher in low refugee migration districts), and the share of unemployed and retired people (both are lower in low refugee migration districts). In the pre-treatment period, respondents in low refugee migration districts were more satisfied with their lives (0.03 points more), while in the post period this difference doubled (0.06 points more). Overall, interviewees in low refugee migration districts had fewer concerns over immigration to Germany pre-treatment, reported less support for centre-left and left parties, are slightly younger, live in wealthier households, are more satisfied with their lives, and are less frequently unemployed. Looking at district-level characteristics (bottom panel), low refugee migration districts have a smaller population, are slightly younger on average, have a higher share of foreigners, a lower unemployment rate, and a slightly higher per capita GDP than high refugee migration districts. Moreover, low refugee migration districts are more rural and are less frequently located in East Germany.

I control for these individual and district characteristics in the estimations shown in Section 6.

5. Methodology

I estimate whether respondents who lived in high/low refugee migration districts in 2015 (as defined in Section 4.2) significantly change their preferences over immigration and party affinity due to higher exposure to refugees. I fix the treatment to the district where respondents resided in 2015 and follow their outcomes over time. For this, I use a difference-in-differences strategy following Eqs. (1) and (2) with individual fixed-effects to exploit the panel structure of the dataset. I start by estimating a static DiD equation, where $Post$ refers to the years 2016–2018:

$$y_{idt} = \alpha_i + \beta HighRef_{d2015} * Post_t + \tau Post_t + \gamma_1 X_{it} + \gamma_2 X_{dt} + \epsilon_{idt} \quad (1)$$

In the main specification, I estimate the following dynamic DiD equation, where 2014 is the reference year:

$$y_{idt} = \alpha_i + \phi_t + \sum_{r=-2}^{-1} \beta_r HighRef_{d2015,2014+r} + \sum_{r=0,r\neq 1}^{4} \beta_r HighRef_{d2015,2014+r} + \gamma_1 X_{it} + \gamma_2 X_{dt} + \epsilon_{idt} \tag{2}$$

where y_{idt} measures the outcome variables (concerns over immigration or preferences toward a particular group of parties) for person i in district d at period t (2012–2018). $HighRef_{d2015}$ is a dummy equal to one if the individual lives in a high refugee migration district (defined by their location in 2015). The β_r coefficients will measure the "exposure effect". ϕ_t are dummies for every year (2012–2018) but 2014 (reference year). X_{it} are individual characteristics and X_{dt} are district characteristics (measured at the end of the year prior to the interview).

Given that individuals might have unobserved time-invariant characteristics (e.g. intergenerationally transmitted preferences), the use of fixed-effects – represented by α_i in both equations – will help to control for any unobserved heterogeneity arising at the individual level.

Furthermore, a possible threat to identification is that deviations from the allocation quotas were not random, i.e. the fact that asylum-seekers were sent to places where housing was available could reflect poor economic conditions or positive attitudes toward migrants (e.g. room was offered in more friendly areas). In Fig. A.3 in Appendix A, I estimate the determinants of the 2015 allocation. The best predictor is having an initial reception centre in 2014 in the district. The economic and socio-demographic variables do not predict the allocation of asylum-seekers. Surprisingly, not even the established quotas do. Moreover, I assume that asylum-seekers are as good as randomly assigned conditional on individual fixed effects and time-varying covariates (Angrist and Pischke, 2008). Hence, even in the case that asylum-seekers were sent to "more friendly districts", this would be absorbed by the individual fixed-effects.

The specification of Eq. (2) also allows me to test whether pre-trends were parallel. Parallel pre-trends lend credibility to the parallel trends assumption, i.e. in absence of treatment individuals in high and low refugee migration districts would have followed the same trends in concerns. If the leads are not statistically different than zero, then there is evidence in favour of the parallel trends assumption.

The outcome variables are concerns about immigration (dummy equal one if an individual is very concerned about immigration to Germany, zero otherwise) and party preferences (dummies for the support towards the major political groups described in Section 4). I restrict the sample to those individuals aged 18 and above, who were interviewed in 2015 (for assigning treatment). Their first interview had to be before 2015 (for observing pre-treatment outcomes) and the last one after 2015. Even though I assign treatment based on the district of residence in 2015, I drop the observations from that year. As shown in Fig. 1, arrivals peaked in the second half of 2015 but most of the interviews were carried-out in the first half of the year. In 2015, 81.3% of the respondents were interviewed before July. Therefore, most of the 2015 respondents had not been fully treated by the time of the interview since the large inflow of late 2015 was not yet fully realised.[28] Given that the SOEP broadly asks about "concerns over immigration" and not "refugee migration", I only focus on the 2012–2018 period when the refugee topic was more salient in the media and the public debate. I normalise the year 2014 to zero (base year) and I do not consider the most recent migration samples (special top-up samples from the SOEP to oversample migrants carried out since 2013). This yields an unbalanced panel with 45,513 observations for the pre-treatment period and 51,820 for the post-treatment period.[29]

6. Results

Table 1 presents the results for all outcomes of interest (concerns about immigration to Germany and party support) with a standard DiD setting (Panel B). For comparison, the upper panel (Panel A) shows the results of a pooled OLS estimation (without individual FE). In all specifications, standard errors are clustered at district-level, based on district of residence in 2015.[30]

In column (1) the results for "concerns about immigration" show similar results in terms of magnitude and significance for both FE and pooled OLS estimations. On average, concerns over immigration increased by about 21–23 pp. in the post-treatment period (*Post* dummy). However, this increase was lower in high refugee migration districts by around 2–3 pp. These findings are in line with the contact hypothesis as in Steinmayr (2021), Dustmann et al. (2019), Vertier et al. (2020), Gamalerio et al. (2021), i.e. hosting refugees probably decreases prejudices towards them, hence leading to a reduction in concerns.

On the other hand, a high inflow of asylum-seekers to a district did affect party affiliation: there was a 1.2–1.5 pp. decrease in support for any political party in high refugee migration districts compared to low refugee migration districts (column (2)). Although this effect is only marginally significant, this implies a larger decrease in overall party support in high refugee migration districts, given the negative trend. However, high refugee migration does not seem to affect party preferences, as shown by the non-significance of the *High ref. dist.*Post* in columns (3)–(6) of both panels. When controlling for individual fixed-effects, the point

[28] Table A.4 in the Appendix A also shows the results when including 2015 in the post-period or as a baseline year.

[29] Overall, my panel is strongly-balanced: 93.37% of observations are from respondents that remained in the sample for 5 or more years between 2012–2018. See Table A.5 in Appendix A. Furthermore, during my period of analysis (2012–2018) respondents remained in the sample, on average for 5.9 years. The average attrition rate of households is around 15% (mainly due to refusals). Conditional on successful household re-contact, the attrition rate for individuals is 5% on average.

[30] Table A.6 in the Appendix A shows the full set of coefficients.

Table 1
Regression results: High vs. low refugee migration districts, concerns about immigration and party affinity.

	(1)	(2)	(3)	(4)	(5)	(6)
	Con. Immi.	Sup. pol. party	Centre-right	Centre-left	Left-wing	Right-wing
	Panel A: OLS					
High ref. dist*Post	−0.026**	−0.015*	0.002	0.001	0.006	−0.009
	(0.011)	(0.008)	(0.009)	(0.009)	(0.004)	(0.005)
Post dummy	0.213***	−0.031***	−0.046***	−0.001	0.003	0.046***
	(0.010)	(0.008)	(0.010)	(0.010)	(0.004)	(0.005)
R^2	0.094	0.106	0.065	0.056	0.106	0.064
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	97,333	96,983	45,638	45,638	45,638	45,638
N	19,686	19,681	13,215	13,215	13,215	13,215
	Panel B: FE					
High ref. dist.*Post	−0.020**	−0.012*	−0.010*	0.001	0.007*	−0.003
	(0.010)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
Post dummy	0.232***	−0.059***	−0.004	−0.006	−0.014***	0.017***
	(0.010)	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)
R^2 -within	0.094	0.106	0.065	0.106	0.064	0.056
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	97,333	96,983	45,638	45,638	45,638	45,638
N	19,686	19,681	13,215	13,215	13,215	13,215

Notes: This table shows the results for a standard DiD equation using $HighRef_{d2015} * Post_t$ instead of yearly interactions. Panel A uses an OLS specification while Panel B includes individual FE. The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(5) show the different outcomes: concerns about immigration, support for any political parties, or support for a particular group of parties. These variables are coded as dummies. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3. Controls at the individual level: age and age^2 in the OLS specification, age groups in the FE specification (ref. cat: $age <= 24$), sex (omitted in FE), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), disability status, migration background (omitted in FE), log net household income, number of children in the household. Controls at the district level: urban, average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All specifications include month of interview and state fixed effects. Statistically significant at $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

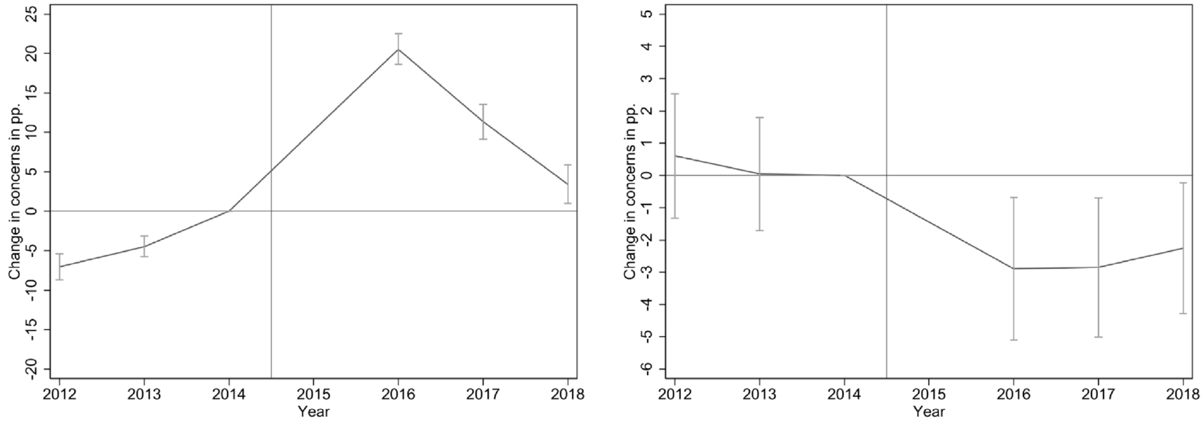
estimates for centre-right and left-wing support are significant at the 10% level: support for the centre-right decreased by 1 pp. in high refugee migration districts, while it increased by 0.7 pp. for the left-wing. The *Post* dummy has a negative coefficient for centre-right preferences (−4.7 pp.) and a positive one for right-wing parties (4.6 pp.) in the OLS specifications (both significant at the 1% level). When using individual fixed-effects, only the positive coefficient for right-wing preferences remains: a 1.7 pp. increase in the post inflow period for both high and low refugee migration districts. However, there is now a negative and significant point estimate for left-wing preferences (−1.4 pp.). This would suggest that there was a shift in preferences towards right-wing parties in the post treatment period in both treatment and control groups. Yet, there is no statistically significant difference between respondents in high and low refugee migration districts, i.e. the exposure effect is close to zero and not significant.

To disentangle the dynamics of the exposure effect (*High ref. dist.*Post* coefficient) for concerns about immigration, I interact the *High ref. dist.* dummy with year dummies as in Eq. (2). Fig. 2(a) shows the trend in concerns among all respondents in both groups (high and low refugee migration districts). A peak is evident in 2016, but the trend returns to pre-inflow values thereafter. Fig. 2(b) shows the treatment effects. Only the coefficients for the interactions post-2015 are significant at the 5% level (decrease in concerns by about 3 pp.). It is reassuring for validity of the parallel trends assumption to see a flat pre-trend before 2015: all pre-treatment leads are not statistically different from zero. Fig. 2(b) further shows a persistent effect in the medium-run with a slight upward trend in the last year, suggesting that the effect could decrease in the following years. Together, both panels of Fig. 2 show that there was an overall increase in concerns over immigration following the large and unexpected inflow of 2015 (Fig. 2(a)), but that this increase was smaller for individuals living in districts that hosted more asylum-seekers (Fig. 2(b)). I interpret the year dummies as reflecting the macro-level effect and the interaction terms as the micro-level exposure effect. Hence, if anything, respondents living in districts that hosted more asylum-seekers had a reduction in concerns over immigration.[31]

7. Effect heterogeneity

To investigate whether my results are driven by individual and/or district characteristics, I perform the analysis on different subgroups. Overall, I find similar reactions across subgroups since the confidence intervals mostly overlap. However, some patterns can be described.

[31] In order to allow comparison with other sociological and political science papers that have studied how concerns over immigration or political preferences are influenced by individual and district-level characteristics, I run a multilevel model with time as level 1, individuals as level 2, and districts as level 3. The results and interpretation of this model are shown in Table C.1 in Appendix C. The results are very much in line with those of my main specification.



(a) Time trend (b) Exposure effect

Fig. 2. Concerns over immigration over time and exposure effect.
Notes: Figure (a) shows the ϕ_t coefficients in Eq. (2). Figure (b) shows the β_r coefficients from the interaction of $HighRef_{d2015}$ with year dummies. These coefficients are multiplied by 100 to be directly interpretable as percentage points. The estimations are performed on the 2012–2018 unbalanced sample, where the observations for 2015 have been dropped and 2014 is set as the base year. All regressions include the covariates described in Table A.3, month of interview and state fixed effects. The continuous line joins the point estimates. The bars display the 95% confidence intervals.
Source: SOEP v35. Own graph.

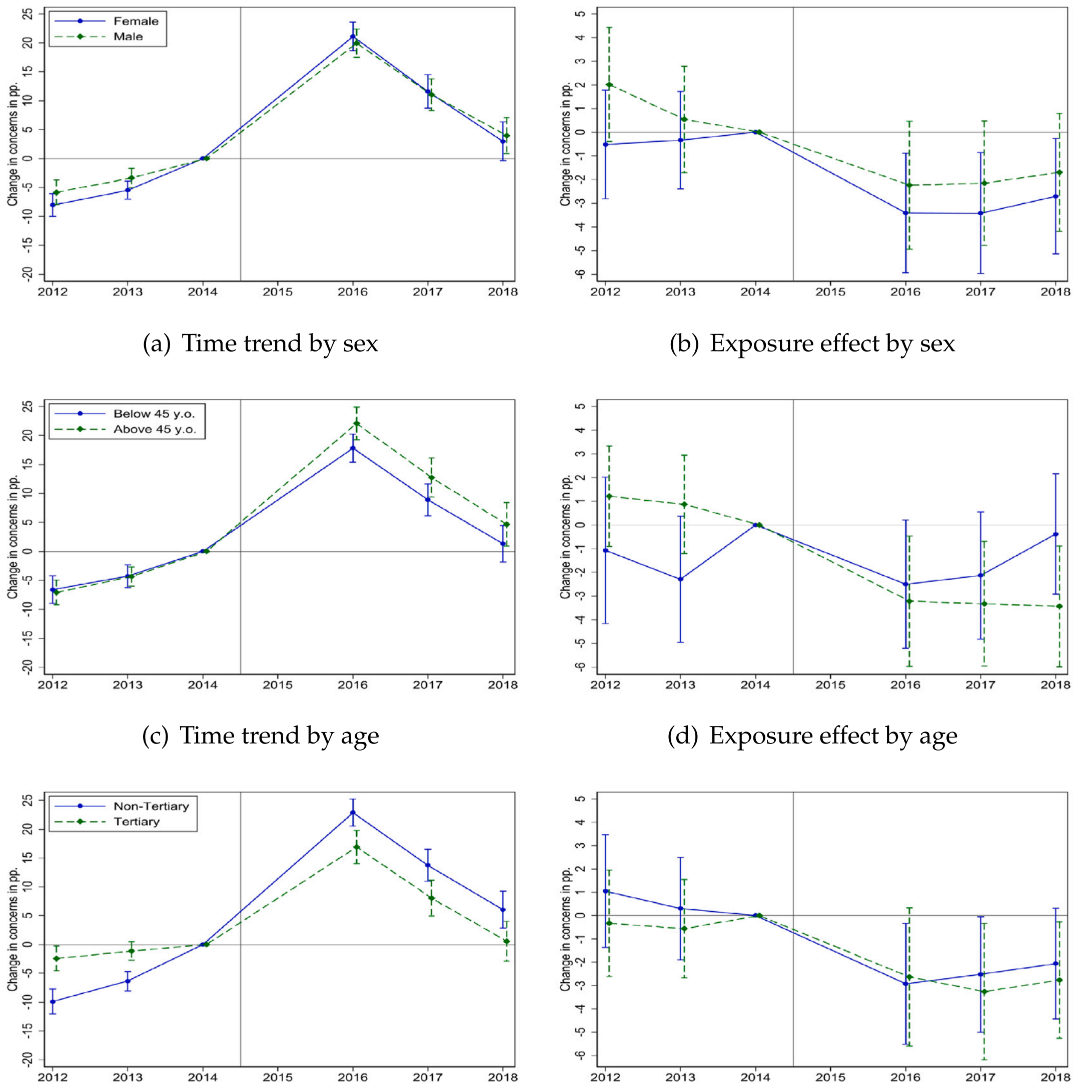
7.1. By individual characteristics

Fig. 3 shows on the left-hand side the overall evolution of concerns (time trend) and on the right-hand side the exposure effect (treatment effects). The first row depicts results by sex: the trend in concerns is almost identical for both male and female respondents. However, significance at the 1% level of the exposure effect remains only for female respondents (see Fig. 3(b)). For male respondents, the coefficients remain negative but are not statistically significant at conventional levels. When looking at individuals older and younger than 45 years of age, the former have an overall larger increase in concerns. Furthermore, the treatment effects remain significant and stable for older individuals aged 45 and above (see Fig. 3(d)). The treatment effects for younger respondents (below age 45) are negative and seem to return to pre-treatment levels three years after the inflow, but lack statistical significance. By education levels, the trend in concerns exhibits a larger increase for non-tertiary educated individuals than for those with tertiary education. Nonetheless, the exposure effect is only statistically significant at the 5% level for individuals with tertiary education.

Fig. 4 shows further heterogeneities by household income and political preferences. Overall concerns increased slightly more for respondents from low-income households. A drop in concerns in 2016 is immediately evident for both below and above median household income respondents. However, there is a striking divide after 2016: for individuals with high household income, the reduction in concerns remains, for those with low household income this seems to vanish from 2017 onward. Finally, I split respondents into three groups based on their stated political preferences measured on an 11-point scale: left (<5), centre (=5) and right (>5).[32] The overall increase in concerns over immigration was very similar for right and centre-leaning individuals and somewhat lower for left-leaning respondents. On the other hand, the reduction in concerns for centre and left-leaning individuals appears to follow the same pattern and is similar in terms of magnitude (approx. 2 pp.). However, the largest reduction occurs for right-leaning individuals, where the reduction in concerns for 2016–2017 is about 6 pp. on average and is the only reduction that is significant at the 5% level. In 2018, although the point estimates are still negative, they are similar in magnitude for all individuals regardless of their political preferences. This is in line with the findings of Bansak et al. (2016) who show that although left-leaning respondents show stronger humanitarian concerns and weaker anti-Muslim bias, they also exhibit a greater penalty for asylum-seekers that migrate seeking economic opportunities than individuals on the right side of the scale do. As shown in Schaub et al. (2020) the presence of refugees appears to make right-leaning individuals less negative in their attitudes and behaviour toward foreigners. They interpret their results by a way of a "reality check" (p. 24): "while the actual presence of refugees might contrast the heated rhetoric that surrounds their arrival, eventually replacing the alarmism of the right as well as the sanguine views of the left with a more realistic and middle of the road experience".

One additional hypothesis is that respondents who have foreign friends might be more familiar with foreigners overall and thus have more positive attitudes. Columns (7) and (8) of Table A.7 show that the effects are smaller in magnitude and significance for

[32] In 2014 an 11-point left–right scale was asked in the SOEP: "In politics people often talk about "left" and "right" when it comes to characterise different political attitudes. If you think about your own political views: Where would you place yours?". In the 11-point scale, zero equals "completely left" and 10 equals "completely right".



(a) Time trend by sex

(b) Exposure effect by sex

(c) Time trend by age

(d) Exposure effect by age

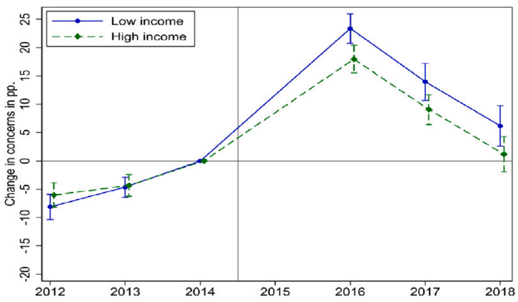
(e) Time trend by education

(f) Exposure effect by education

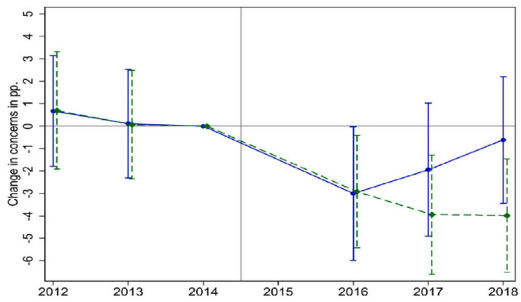
Fig. 3. Heterogeneities by individual characteristics.
Notes: The left-hand graphs show the ϕ_t coefficients in Eq. (2) (time trend). The right-hand graphs show the β_r coefficients from the interaction $HighRef_{d2015}$ with year dummies. The estimations are performed on the 2012–2018 unbalanced sample, where the observations for 2015 have been dropped and 2014 is set as the base year. All regressions include the covariates described in Table A.3, month of interview and state fixed effects, with the exception of the covariate for which the sample split is performed. I fix the covariates to their 2015 value for performing the heterogeneity analysis. Tertiary equals 1 if the respondent had completed tertiary education in 2015. The graphs show the coefficients multiplied by 100 so that they can directly be interpreted as percentage points. The continuous line joins the point estimates. The vertical bars display the 95% confidence intervals.
Source: SOEP v35. Own graph.

those respondents without friends with a migration background.[33] The point estimates for respondents with some friends with a migration background are larger in magnitude and significance throughout 2016–2018. These results would support the familiarity hypothesis. Unfortunately, this question was only asked in 2018. Hence, it could also be an outcome of my treatment and should be interpreted with caution. The full set of results and further heterogeneities by individual characteristics are shown in Tables A.7 and A.8 in Appendix A.

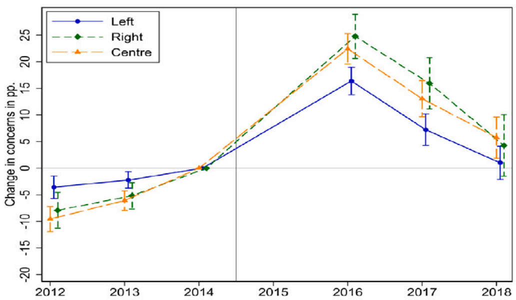
[33] In 2018 the SOEP asked “What is your circle of friends like: How many of your friends are not from Germany or have parents who are not from Germany, in other words, have a migration background?". The response options were: All of them have a migration background, Most, About half, About a quarter, Less than a quarter, None of them have a migration background.



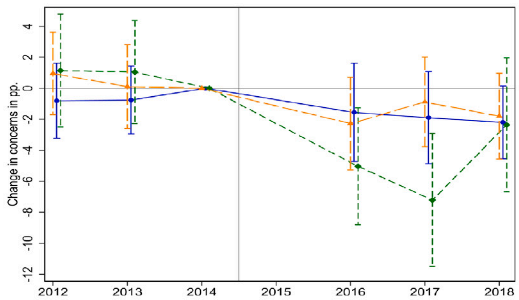
(a) Time trend by HH income



(b) Exposure effect by HH income



(c) Time trend by political scale



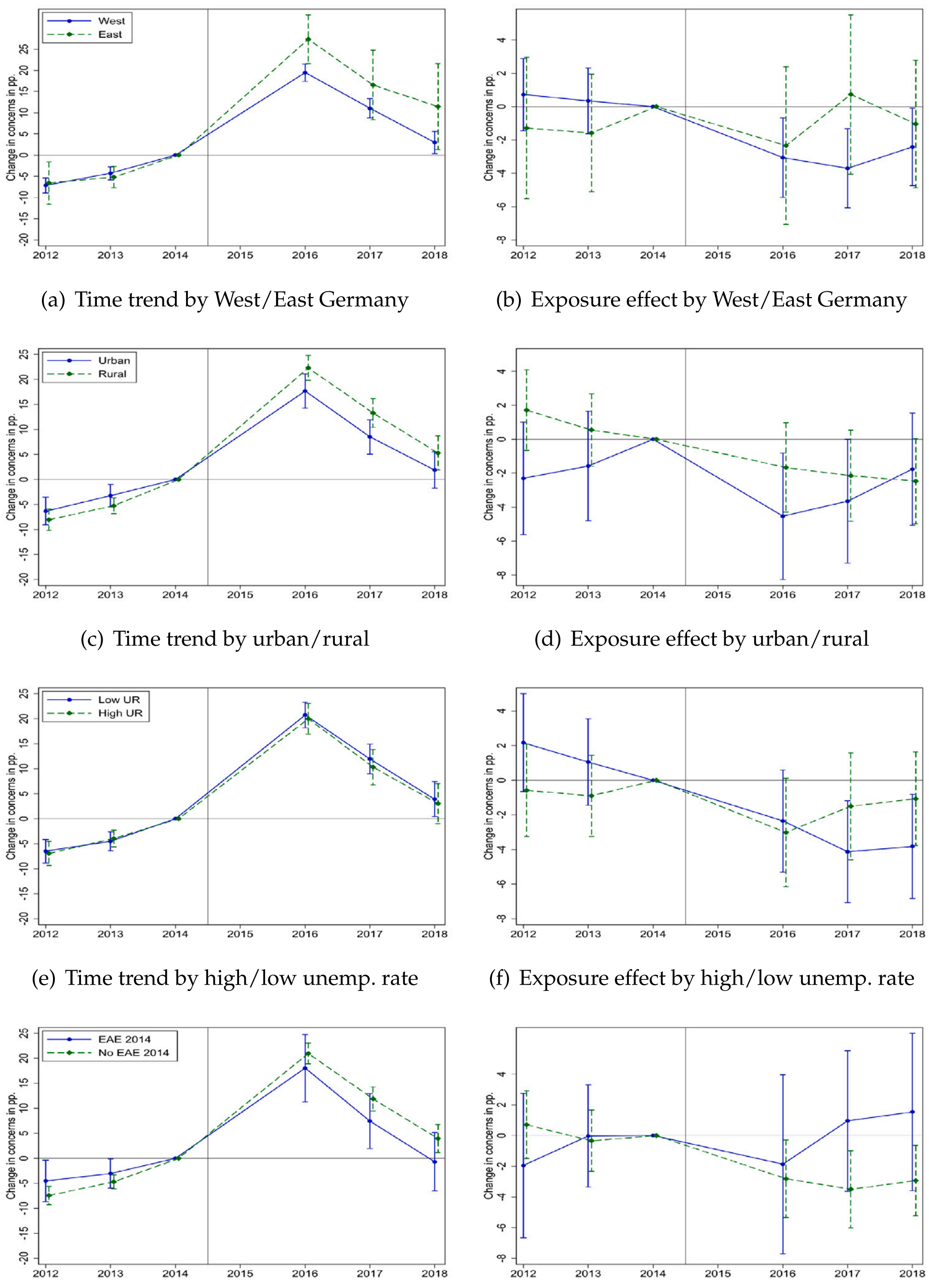
(d) Exposure effect by political scale

Fig. 4. Heterogeneities by individual characteristics (cont.).
Notes: The left-hand graphs show the ϕ_t coefficients in Eq. (2) (time trend). The right-hand graphs show the β_r coefficients from the interaction $HighRef_{d2015}$ with year dummies. The estimations are performed on the 2012–2018 unbalanced sample, where the observations for 2015 have been dropped and 2014 is set as the base year. All regressions include the covariates described in Table A.3, month of interview and state fixed effects, with the exception of the covariate for which the sample split is performed. I fix the covariates to their 2015 value for performing the heterogeneity analysis. High income equals 1 if in 2015 the respondent lived in a household with a log household income above the median (=7.94). Since the political scale was only asked in 2014, I fix its value to this year. Left equals 1 if the response in the scale was below 5, centre equals 1 if the response was equal to 5 and right equals 1 if the response was above 5. The graphs show the coefficients multiplied by 100 so that they can directly be interpreted as percentage points. The continuous line joins the point estimates. The bars display the 95% confidence intervals.
Source: SOEP v35. Own graph.

7.2. By district characteristics

Fig. 5 shows that overall concerns over immigration increased more in the East and in rural districts (panels (a)–(d)). These concerns were higher by almost 8 pp. in 2016 for East relative to West Germans, showing the classical East-West German divide. Similarly, the reduction in concerns is similar and statistically significant at the 5% level for respondents living in West Germany and urban districts. One interesting pattern is that concerns over immigration decrease more over time in rural areas. This would reflect that perhaps sceptical at first, respondents in rural areas might have closer interactions with refugees in the medium run. Schmidt et al. (2020) mention that in 2018 almost 60% of refugees in rural areas had frequent contact with Germans in their circle of friends. In urban areas, this was only 40%. Moreover, the exposure effect is larger in magnitude and significance (for the years 2017 and 2018) for people living in low unemployment districts compared to those living in high unemployment districts (Fig. 5(e)–(f)). The time trend, however, is similar for respondents in both high and low unemployment districts. This is in line with the findings by Dustmann et al. (2019), who show that in urban municipalities the unemployment rate exacerbated the increase in support for anti-immigration parties. Additionally, I compare individuals that lived in a district which hosted an EAE (initial reception centre) in 2014 – pre-treatment – with those who did not (Fig. 5(g)–(h)).[34] Although the standard errors are quite large due to the smaller sample of individuals living in a district with an EAE, the trend in concerns over immigration evolved similarly for respondents in both types of district. But there was only a significant reduction in concerns for those who lived in districts without an EAE. This is in line with the findings by Bredtmann (2022) who shows that the increase in right-wing support for the state of Rhineland-Palatine in 2016 was only driven by municipalities that hosted a reception centre. They are also in line with (Vertier et al., 2020) who find that above a certain threshold (large reception centres) there is an increase in vote shares for the far-right. Their results imply that

[34] In 2014 only 36 out of 401 districts in Germany hosted an EAE. In 2015, 148 districts hosted either an initial reception centre or some kind of collective group accommodation.



(a) Time trend by West/East Germany (b) Exposure effect by West/East Germany

(c) Time trend by urban/rural (d) Exposure effect by urban/rural

(e) Time trend by high/low unemp. rate (f) Exposure effect by high/low unemp. rate

(g) Time trend by EAE in district in 2014 (h) Exposure effect by EAE in district in 2014

Fig. 5. Heterogeneities by district characteristics.
Notes: The left-hand graphs show the ϕ_t coefficients in Eq. (2) (time trend). The right-hand graphs show the β_r coefficients from the interaction $HighRef_{d2015}$ with year dummies. The estimations are performed on the 2012–2018 unbalanced sample, where the observations for 2015 have been dropped and 2014 is set as the base year. All regressions include the covariates described in Table A.3, month of interview and state fixed effects, with the exception of the covariate for which the sample split is performed. I fix the covariates to their 2015 value for performing the heterogeneity analysis. West equals 1 if the district of residence in 2015 was in West Germany. Urban equals 1 if the district of residence in 2015 was classified as "Urban" *(Kreisfreie Stadt)* by the Federal Office for Building and Regional Planning (BBR). The graphs show the coefficients multiplied by 100 so that they can directly be interpreted as percentage points. The bars display the 95% confidence intervals.
Source: SOEP v35. Own graph.

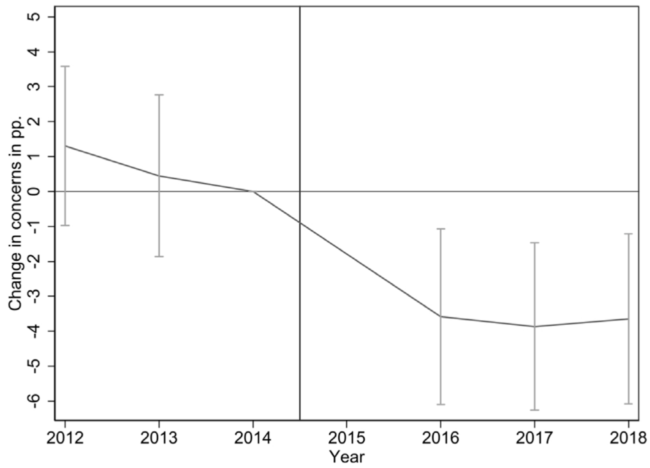


Fig. 6. Exposure effect for concerns over immigration — continuous treatment.
Notes: The figure shows the β_r coefficients from the interaction of $HighRef_{d2015}$ with year dummies in Eq. (2), where $HighRef_{d2015}$ is replaced by the actual share of allocated refugees (continuous treatment) instead of the treatment dummy. All regressions include the covariates described in Table A.3, month of interview and state fixed effects. The continuous line joins the point estimates. The bars display the 95% confidence intervals.
Source: SOEP v35. Own graph.

voters were against the presence of large receptions centres for refugees. A similar line of argument may apply for concerns about immigration. Then, when sent to the subsequent accommodation, asylum-seekers are more spread-out within the districts which facilitates contact with the local population.

As in the previous subsection, I also test the familiarity hypothesis at the district level. In column (1) of Table A.10, I show the results for respondents living in districts with a share of foreign residents above the median while column (2) shows the results for districts with below-median shares. The overall increase in concerns was larger for respondents living in districts with a lower share of foreigners. Although the exposure effect is similar in sign, the coefficients are larger in magnitude and are only significant at the 5% level for respondents living in a district with "high foreigner share" for the years 2016–2017.[35] Finally, comparing districts with high and low GDP per capita, the results are very similar and are statistically significant at the 10% level for the exposure effect in 2016. In 2017, the point estimate is larger and is significant at the 5% level for high GDP per capita districts. The full set of results are shown in Table A.9 in Appendix A.

Although there is some indication that different subgroups might have reacted differently in the short-run, and may also have different time dynamics, none of the coefficients are clearly different from the other coefficients (the confidence intervals mostly overlap). Therefore, I conclude that the reduction in concerns was quite similar across subgroups. This is in line with the findings by Bansak et al. (2016) who show that preferences toward refugees are homogeneous across respondents from different subgroups.

8. Sensitivity analysis

8.1. Alternative treatment specification: Using the actual share

As a first robustness check, I define as an alternative treatment the refugee allocation in 2015 as a share of the total district population.

Fig. 6 presents the results on concerns about immigration to Germany. The figure follows the same pattern as the one displayed in Fig. 2. These results are for the restricted sample, without outliers in asylum-seeker reporting (i.e. I take out those districts that are below or equal the first and above or equal the ninety-ninth percentile of the allocation distribution).[36] The interaction between the share of allocated refugees and the year dummies 2016–2018 are significant at the 5% level. In 2016, a 1 pp. increase in the share of allocated refugees led to a decrease in concerns about immigration by about 3.6 pp. in line with the estimates using the high/low refugee migration districts dummy. These results are not only reassuring but show that the intensive margin also matters, similar to Hangartner et al. (2019). However, since the results are driven by respondents living in districts without a large reception centre (as seen in the previous section), the effect might not be linear. Thus, the allocation of asylum-seekers to subsequent accommodation also matters.

[35] The sex of the immigrants does not seem to play a particular role, neither do the nationalities. Columns (3)–(6) of Table A.10 show the sample splits by sex of foreigners in a district. Table A.11 show further sample splits by different nationalities. Country groups based on the UNGeographicRegions.

[36] In the results using the full sample, only the interaction with the year 2016 is statistically significant at the 1% level.

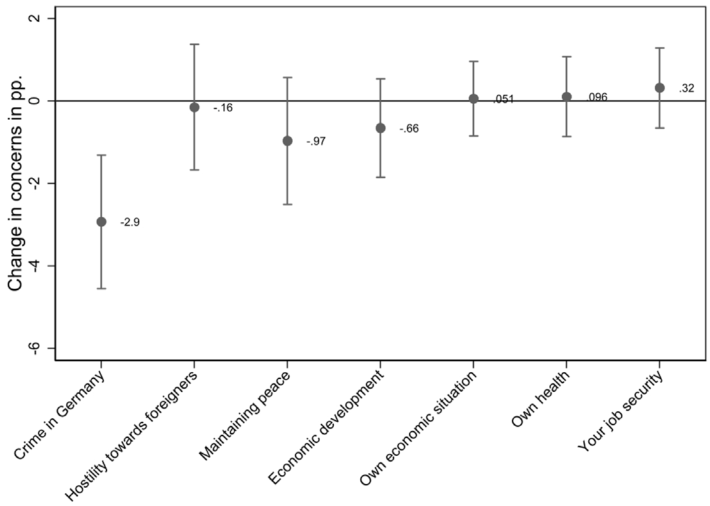


Fig. 7. Exposure effect on other outcomes.
Notes: Coefficient plots from standard DiD regressions using $HighRef_{d2015} * Post_t$ instead of yearly interactions. The graph shows the coefficients multiplied by 100 so that they can directly be interpreted as percentage points. The outcomes measure concerns about different topics: crime to Germany, hostility towards foreigners or minorities in Germany, maintaining peace, the economy in general, own economic situation, own health, own job security. They are all coded as dummies equal to 1 if the respondent is "very concerned".
Source: SOEP v35. Own graph.

8.2. Other sample restrictions

In column (1) of Table A.12, I report the full set of results depicted in Fig. 2 for comparison. Column (2) shows the same results without covariates, which do not drastically vary from my main specification. In column (3) I drop outliers in the inflow of asylum-seekers, i.e. districts which in 2015 received below or equal to the first percentile (0.25%) and above or equal to the ninety-ninth percentile (4.3%) of the distribution of asylum-seekers.[37] Column (4) only considers individuals of working age, who were at least 18 and at most 65 in 2015, i.e. those who may feel threatened by migrants entering the labour market. Column (5) shows results excluding the five largest districts in Germany (Berlin, Hamburg, Hanover, Cologne and Munich, all of which have a population greater than 1 million) to address concerns that these large districts might be driving the results. Column (6) only considers a balanced panel, i.e. individuals who were interviewed throughout 2012–2018, although this drastically reduces the sample size by almost half. And column (7) shows results excluding movers, i.e. respondents that changed their district of residence after 2015. Across all these sample restrictions, the results remain virtually unchanged.

Finally, Table A.13 shows the results for German citizens and those individuals without a migration background.[38] Column (1) shows the main results for comparison. Column (2) only considers respondents without German nationality, while column (3) shows the results for German citizens only. Given that 95.74% of my sample are German nationals, the results from column (3) very much resemble those from column (1). The results for non-German citizens are slightly smaller in magnitude and are always not statistically significant. Column (4) shows the results for respondents without a migration background (i.e. "natives"), and column (5) shows only the results for those with a migration background. Since 87.3% of my sample do not have a migration background, the results from columns (4) and (1) are almost identical. Nonetheless, column (5) – although not statistically significant – shows that the point estimates for respondents without a migration background are slightly larger for the first two years after the shock (2016–2017). Overall, the results for non-natives do not significantly differ from those of natives.

In all specifications presented in Table A.12 and Table A.13, the main results remain similar in magnitude and significance, i.e. there was an overall increase in concerns in 2016 (by about 21 pp.) which was lower in districts that received a larger inflow of asylum-seekers in 2015 (by about 2–3 pp.). My results are in line with others in the literature, i.e. Steinmayr (2021) finds that the presence of asylum-seekers in a municipality reduces the vote share for RWP parties by about 3.86 pp. in Austria, while Dustmann et al. (2019) finds a reduction of about 3.8 pp. for the 5% largest municipalities in Denmark.

[37] The district with the highest reported inflow of asylum-seekers in 2015 received 10,672 asylum-seekers per 100,000 inhabitants, while the one with the lowest inflow only 3. These extreme values might reflect some reporting errors and introduce large variations.

[38] Having migration background means the respondent was born abroad or has one parent that was born abroad.

8.3. Other concerns

As in Poutvaara and Steinhardt (2018) and Sola (2018), I also test the treatment on other concerns as shown in Fig. 7 (for the complete results see Table A.14). The *Post* dummy is positive and significant for many of the other concerns (crime in Germany, hostility towards foreigners, maintaining peace and economic development).

The interaction *High ref.district*Post* is significant and negative at the 1% level only for those individuals who are very concerned about crime in Germany. Hence, even though concern increased in the post treatment period, having a higher allocation of refugees in a district reduced concerns about crime in Germany by about 2.9 pp. One of the largest events related to refugees and crime was New Year's Eve 2015/16. Sexual assaults and thefts were reported in Cologne and some other major cities in Germany that were attributed to foreign men, almost half of them asylum-seekers. This event could have influenced the response toward some of these additional outcomes, i.e. concerns about crime. Since these debates happened at the national level, they might be captured in the *Post* dummy. In any case, the reduction in concerns about crime would also support the contact hypothesis if this is related to the reduction in concerns about immigration. Furthermore, it is important to notice that other concerns such as "own economic situation" and "job security" all have point estimates close to zero and are not statistically significant. Hence, this indicates that the "economic concerns" might not be the mechanism through which immigration shapes attitudes.[39]

Given that concerns over immigration and crime are both affected by the inflow of asylum-seekers to a district, I test whether they are also related to political preferences. As seen in Table 1 the exposure effect was not significant for any of the party preferences, only the *Post* dummy was significant for leaning towards right-wing parties. In Table A.15 in Appendix A, I test whether being "very concerned about immigration to Germany" and "very concerned about crime to Germany" could be possible mediators of the treatment. These results have no causal interpretation. Being very concerned about immigration is negatively correlated with preferences toward centre-left and left-wing parties (decrease by about 0.6 pp.), while being positively correlated with leaning toward right-wing parties (by about 1.6 pp., significant at the 1% level). Furthermore, being very concerned about crime only increases the probability of leaning toward right-wing parties by about 0.7 pp. (half the magnitude of "concerns about immigration"). These results suggest that the treatment (living in a high refugee migration district) could have an indirect impact on political preferences via concerns about immigration and crime, where the former effect is stronger than the latter.

9. Conclusion

By exploiting regional distribution and timing of arrival of asylum-seekers during the largest migration wave to Germany since World War II, I investigate how concerns about immigration and party preferences at the individual level reacted to this inflow. I show that respondents living in districts that experienced a large inflow of asylum-seekers had a reduction in concerns by about 2–3pp.

My results are in line with the literature studying refugee migration (Dustmann et al., 2019; Steinmayr, 2021; Gehrsitz and Ungerer, 2022; Sola, 2018; Bredtmann, 2022; Schaub et al., 2020; Gamalerio et al., 2021; Vertier et al., 2020). At the macro-level, there was an increase in concern about immigration by about 21 pp. and a smaller increase in support for RWP parties (1.7 pp.). However, this was not driven by exposure to asylum-seekers in the district of residence.

On the contrary, at the micro-level, the allocated share of asylum-seekers reduced concerns about immigration by about 3 pp. in 2016, or alternatively a 1 pp. increase in the share of allocated refugees reduced concerns by about 3.6 pp. These results are in line with the contact hypothesis and remain negative and significant even three years after the 2015 inflow of asylum-seekers. Finally, my analysis shows different dynamics for the treatment effects by individual and district-level characteristics. The effect is larger for right-leaning individuals and in districts without a reception centre. However, most of the heterogeneities are not statistically different from each other for the different subgroups, thus I conclude the reduction in concerns was quite homogeneous across subgroups.

Putting all of these pieces together, it seems that the macro-level effect plays a major role when shaping attitudes towards immigration and support for RWP parties compared to micro-level exposure. This result is in line with the results of Steinmayr (2021) and the review by Cools et al. (2021). What is particularly interesting, is that the effects I find at the micro-level (reduction in the concerns over immigration) persist over time whereas the macro-level effect reverts to the pre-treatment mean by the third year after the asylum-seekers' arrival.

Yet, the focus of my paper is to clearly identify micro-level effects while controlling for general trends (the macro-level effect). Given the dominance of the macro-level effect in the short run, future research should aim at improving our understanding of the macro effect, e.g. through which channels it operates (traditional media and social media, political campaigns, and (perceived) overall immigration at the national level) and what conditions tend to amplify the effect. Moreover, further research is needed to investigate the conditions which make this reduction in concerns (micro-level effect) sustainable, in order to derive potential policy implications.

[39] In additional heterogeneity analysis (available upon request), I checked whether this holds for individuals who were unemployed in 2015, to avoid compositional effects. There are no statistically significant effects either for "own economic situation" or for "job security". The latter is only asked for employed individuals, meaning that the results are evaluated for respondents that were unemployed in 2015 but employed in another year during 2012–2018. Although the treatment effect is statistically not significant, it is interesting to see that there is some indication of an increase in concerns about your own job security for individuals who were unemployed in 2015 but employed in some other year during the period of analysis. This would be in line with threat theory at the individual level as pointed out by Lancee and Pardos-Prado (2013).

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

Appendix A. Additional figures and tables

See Tables A.1–A.15 and Figs. A.1–A.3

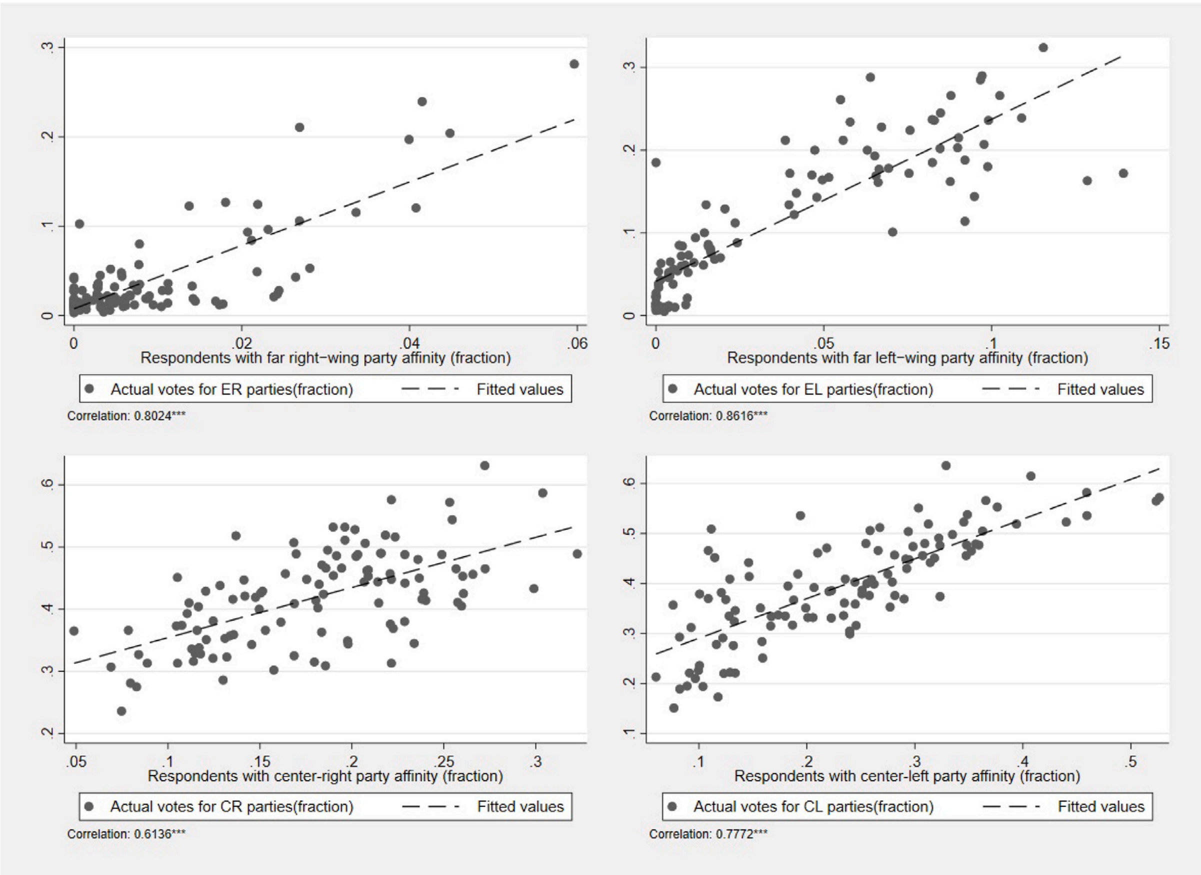
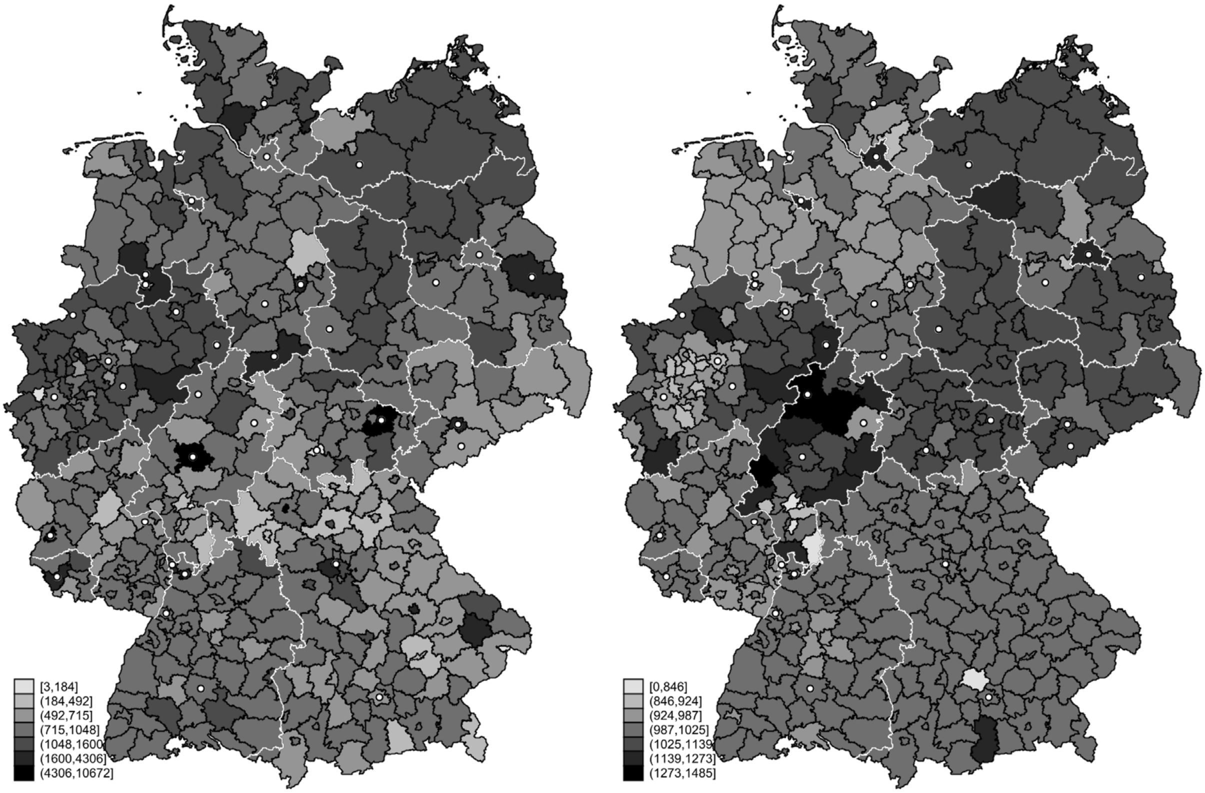


Fig. A.1. Actual allocation of asylum-seekers.
Notes: The figure displays the proportion of votes for right-wing parties (NPD, REP, DVU, and AfD since 2013), centre-right parties (CDU/CSU, FDP), centre-left parties (SPD, Gruene/Buendnis90), and left-wing parties (Die Linke) at seven federal elections (1994, 1998, 2002, 2005, 2009, 2013 and 2017) at the state level; with the proportion of party preferences from SOEP respondents for the respective years.
Source: Der Bundeswahlleiter (2017), Liebig et al. (2019) (SOEP v35).



(a) Actual allocation of asylum-seekers (b) Theoretical allocation

Fig. A.2. Actual vs. theoretical allocation of asylum seekers per 100,000 inhabitants.
Notes: The maps show the actual vs. the theoretical allocation of asylum-seekers in 2015. The theoretical allocation is based on the established quotas by the states to the districts. The white lines represent state borders. The red crosses mark the presence of an initial reception centre in the district in 2014.
Source: RDC (2015) and Gehrsitz and Ungerer (2022).

Table A.1
Koenigsteiner Key vs. actual allocation shares.

	(1) Königsteiner Key	(2) Refugees' allocation	(3) Difference (1)–(2)
Baden-Wuerttemberg	12.86%	12.34%	0.52%
Bayern	15.52%	12.79%	2.73%
Berlin	5.05%	4.45%	0.60%
Brandenburg	3.06%	3.10%	−0.04%
Bremen	0.96%	1.18%	−0.22%
Hamburg	2.53%	2.11%	0.42%
Hesse	7.36%	6.55%	0.81%
Mecklenburg-Vorpommern	2.03%	2.09%	−0.06%
Niedersachsen	9.53%	10.47%	−0.94%
North Rhine-Westphalia	21.21%	23.92%	−2.71%
Rhineland-Palatinate	4.84%	5.08%	−0.24%
Saarland	1.22%	1.22%	0.00%
Saxony	5.08%	4.76%	0.32%
Saxony-Anhalt	2.83%	3.04%	−0.21%
Schleswig-Holstein	3.40%	3.78%	−0.38%
Thuringia	2.72%	3.11%	−0.39%

Notes: Königsteiner Key for 2015 was obtained from Bundesministerium der Justiz und für Verbraucherschutz (2014). Data on refugee allocation for 2015 is from the RDC (2015).

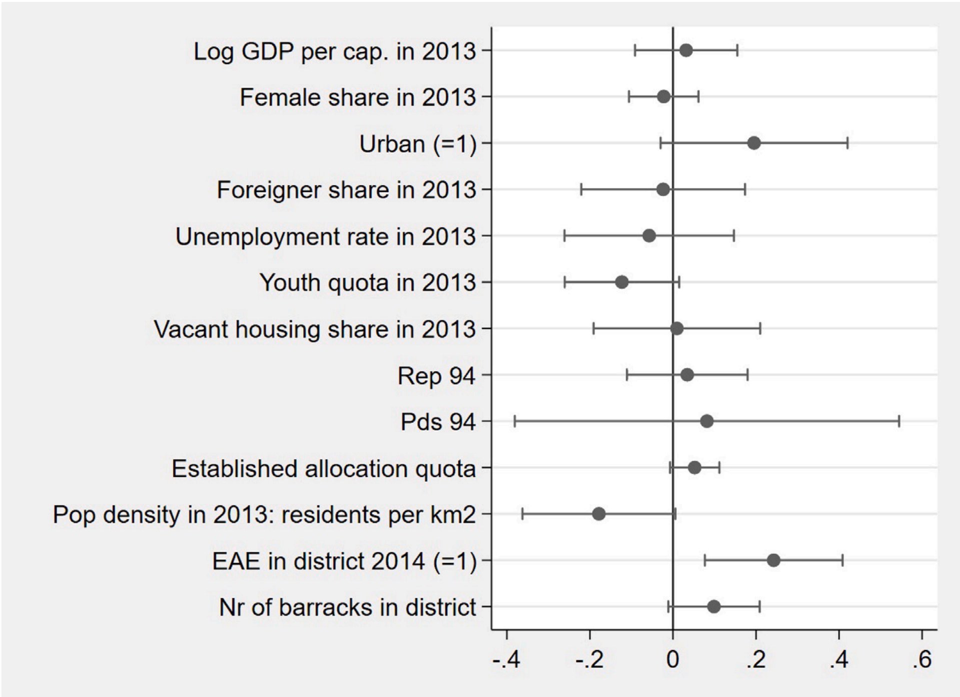


Fig. A.3. Determinants of asylum-seekers allocation in 2015 (standardise beta coefficients).
Notes: The dependent variable is the share of allocated refugees at the district-level in 2015. Most of the covariates refer to 2013 since for determining the 2015 Königsteiner Key the relevant variables are taken from 2013. These variables were obtained from Destatis. I thank Martin Ungerer for sharing his data on the initial reception centres in 2014 and Paul Berbée for sharing his data on military vacancies.

Table A.2
Comparison: official statistics and SOEP.

	(1) Destatis	(2) SOEP weighted	(3) My sample (w.)	(4) My sample (unw.)
Sex and employment				
Male	48.88	48.48	48.40	45.35
Female	51.12	51.52	51.60	54.75
Employed %	59.68	58.73	58.79	61.50
Schooling				
Secondary School Degree	31.59	30.24	31.28	27.79
Intermediate School Degree	29.61	29.19	29.92	31.60
Upper Secondary Degree	30.95	29.68	30.15	32.11
Other Degree	–	5.75	5.83	5.83
Dropout, No School Degree	3.99	1.21	1.30	1.25
No School Degree Yet	3.66	0.86	0.37	0.52
No response	0.21	3.05	1.16	0.90
State of residence				
Baden-Württemberg	13.27	12.73	12.73	11.43
Bayern	15.67	15.08	14.81	15.88
Berlin	4.33	4.05	3.85	3.75
Brandenburg	3.02	3.18	3.24	4.30
Bremen	0.82	0.89	0.86	0.62
Hamburg	2.19	2.29	2.32	1.65
Hessen	7.53	7.08	7.11	6.69
Mecklenburg-Vorpommern	1.95	2.01	2.03	2.52
Niedersachsen	9.63	9.93	10.05	9.75
Nordrhein-Westfalen	21.68	21.62	21.60	19.54
Rheinland-Pfalz	4.93	4.71	4.75	4.80
Saarland	1.21	1.13	1.15	0.87
Sachsen	4.95	6.13	6.31	6.90
Sachsen-Anhalt	2.71	2.90	2.89	3.75
Schleswig-Holstein	3.49	3.56	3.53	3.42
Thüringen	2.62	2.73	2.79	4.11

Notes: The table displays the descriptive statistics for individuals aged 18 and above for sex and employment. For schooling, the Federal Statistical Office (Destatis) reports these from age 15 onwards. Therefore column (1) and (2) for schooling are for individuals age 15+, for columns (3) and (4) this is only done for individuals 18+ (my main sample). For the states of residence, column (1) shows the % for individuals of all ages, while columns (2)–(4) only for individuals aged 18 and above due to data availability.

Table A.3
Descriptive statistics (SOEP).

	Pre-treatment				Post-treatment			
	All	Low refugee	High refugee	Diff	All	Low refugee	High refugee	Diff
Very concerned about immigration	0.221	0.206	0.237	−0.032***	0.397	0.395	0.399	−0.004
Political support (Yes)	0.481	0.464	0.500	−0.036***	0.459	0.45	0.468	−0.018***
Party afil: right	0.007	0.007	0.007	0.001	0.026	0.028	0.024	0.004***
Party afil: centre-right	0.205	0.205	0.206	−0.001	0.190	0.193	0.187	0.005
Party afil: centre-left	0.217	0.203	0.232	−0.029***	0.190	0.180	0.200	−0.020***
Party afil: left	0.031	0.026	0.035	−0.009***	0.032	0.027	0.037	−0.010***
Age	51.963	51.726	52.212	−0.486***	52.987	52.828	53.156	−0.328**
Age ≤24	0.061	0.062	0.060	0.002	0.050	0.052	0.047	0.004**
Age 25–34	0.111	0.109	0.113	−0.004	0.096	0.094	0.097	−0.003
Age 35–44	0.165	0.17	0.159	0.011***	0.176	0.176	0.177	−0.001
Age 45–54	0.218	0.222	0.215	0.006	0.231	0.237	0.226	0.011***
Age 55–64	0.188	0.186	0.189	−0.003	0.177	0.176	0.178	−0.002
Age 65+	0.257	0.251	0.264	−0.013***	0.270	0.265	0.275	−0.009**
Male	0.460	0.460	0.460	0.001	0.451	0.451	0.45	0.001
Primary	0.021	0.02	0.023	−0.003**	0.013	0.011	0.015	−0.004***
Secondary Education	0.667	0.671	0.661	0.010**	0.665	0.667	0.663	0.004
Tertiary Education	0.312	0.309	0.315	−0.006	0.322	0.322	0.322	−0.001
Married	0.637	0.636	0.638	−0.002	0.641	0.641	0.64	0.001
Divorced	0.095	0.096	0.093	0.003	0.102	0.103	0.102	0.000
Single	0.201	0.202	0.201	0.001	0.189	0.190	0.189	0.001
Widowed	0.067	0.066	0.068	−0.002	0.068	0.067	0.069	−0.002
Employed	0.594	0.606	0.582	0.025***	0.620	0.628	0.611	0.017***
Retired	0.217	0.212	0.223	−0.011***	0.230	0.226	0.235	−0.009**
Maternityleave	0.015	0.015	0.015	0.000	0.013	0.013	0.013	0.000
Unemployed	0.039	0.032	0.046	−0.014***	0.034	0.029	0.039	−0.010***
Non Working	0.082	0.081	0.082	−0.001	0.069	0.069	0.069	0.000
In Education	0.023	0.023	0.024	−0.002	0.015	0.015	0.016	−0.001
Other Non Working	0.029	0.03	0.028	0.003*	0.018	0.019	0.018	0.002
Disability status	0.128	0.122	0.136	−0.014***	0.128	0.123	0.133	−0.010***
Ln(Net HH Income)	7.866	7.894	7.836	0.058***	7.965	7.993	7.936	0.057***
Migration Background	0.121	0.123	0.118	0.006*	0.133	0.131	0.134	−0.003
Number of children (mean)	0.534	0.55	0.516	0.034***	0.662	0.662	0.661	0.002
Life satisfaction (mean)	7.221	7.234	7.207	0.027*	7.305	7.333	7.275	0.059***
Asylumseekers per 100k (2015)					998.102	711.1	1301.3	−590.200***
Total county population	437,582	315,796	565,936	−250,140***	446,171	324,150	575,077	−250,926***
Average age	43.848	43.781	43.918	−0.138***	44.174	44.091	44.262	−0.170***
Foreigner share (in %)	7.747	8.148	7.325	0.823***	10.35	10.734	9.945	0.789***
Youth quota (in %)	19.954	20.119	19.780	0.339***	20.393	20.504	20.275	0.229***
Female share (in %)	51.078	51.077	51.080	−0.003	50.682	50.665	50.701	−0.037***
Unemployment rate (in %)	7.208	6.415	8.044	−1.629***	6.243	5.554	6.971	−1.417***
Housing vacancies (in %)	4.989	5.093	4.881	0.212***	5.194	5.145	5.246	−0.101***
Ln(GDP per cap in 1,000€)	3.439	3.463	3.415	0.048***	3.546	3.574	3.515	0.059***
Urban	0.305	0.272	0.341	−0.069***	0.299	0.266	0.333	−0.066***
East	0.226	0.213	0.238	−0.025***	0.216	0.203	0.23	−0.027***
Obs.	45,513	23,354	22,159		51,820	26,621	25,199	

Notes: The table displays the sample means for outcomes and covariates for the pre and post-treatment periods, by low and high refugee migration districts. The upper panel presents the outcome variables, which are all dummy variables; the middle panel presents all the covariates; and the bottom panel presents district characteristics obtained from INKAR that were merged into the SOEP dataset. Age is measured in years, age brackets are dummies (for the FE estimations). Education variables are classified according to the ‘‘International Standard Classification of Education (ISCED)’’ of 2011 and are dummies. Marital status and employment variables are also dummies. Life satisfaction is measured on an 11 point scale (being 0 completely dissatisfied and 10 completely satisfied). GDP is measured in 1.000 euro per inhabitant. District characteristics are as of the end of the previous year (i.e. for 2016, the reported values for 2015 are considered). Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4
Main results including 2015 interviews.

	(1) Base 2015	(2) Base 2014 (w. 2015)	(3) Main
High ref. dist*Y2018	−0.013	−0.022**	−0.023**
	(0.009)	(0.010)	(0.010)
High ref. dist*Y2017	−0.019**	−0.029***	−0.029***
	(0.009)	(0.011)	(0.011)

(*continued on next page*)

Table A.4 (*continued*).

	(1) Base 2015	(2) Base 2014 (w. 2015)	(3) Main
High ref. dist*Y2016	−0.019**	−0.028**	−0.029**
	(0.010)	(0.011)	(0.011)
High ref. dist*Y2015	.	−0.009	.
	.	−0.008	.
High ref. dist*Y2014	0.009	.	.
	(0.008)	.	.
High ref. dist*Y2013	0.009	0.000	0.000
	(0.009)	(0.009)	(0.009)
High ref. dist*Y2012	0.014	0.004	0.006
	(0.009)	(0.010)	(0.010)
Y2018	−0.022**	0.036***	0.034***
	(0.010)	(0.012)	(0.013)
Y2017	0.058***	0.116***	0.113***
	(0.009)	(0.010)	(0.011)
Y2016	0.147***	0.205***	0.205***
	(0.008)	(0.009)	(0.010)
Y2015	.	0.058***	.
	.	(0.006)	.
Y2014	−0.058***	.	.
	(0.006)	.	.
Y2013	−0.098***	−0.040***	−0.045***
	(0.008)	(0.006)	(0.007)
Y2012	−0.124***	−0.066***	−0.070***
	(0.009)	(0.008)	(0.008)
Covariates	Yes	Yes	Yes
Obs.	116,639	116,639	97,333
N	19,720	19,720	19,686

Notes: Column (1) uses 2015 instead of 2014 as a baseline year and column (2) uses 2014 as a baseline year – as in my main specification – but considers the observations for 2015 as part of the analysis. Column (3) again presents the main results for comparison purposes. Column (1) shows that the treatment effects for 2016–2018 are almost 1 pp. smaller than in my main specification, and only the coefficients for 2016 and 2017 are statistically significant at the 5% level. This is explained in column (2) where the point estimate for the interaction effect is already negative, although not statistically significant for 2015. Hence, since some of the respondents in 2015 were already treated, taking this as the baseline year underestimates the actual treatment effect. Columns (2) and (3) where 2014 is set as the baseline year show very similar results. However, I prefer to drop the observations for 2015 to compare the responses before and after treatment occurred. I consider 2015 the year of treatment roll-out.

Table A.5
Main sample — number of observations by years in panel.

Panel years	Observations	%
1	71	0.07
2	386	0.40
3	1,575	1.62
4	4,421	4.54
5	18,573	19.08
6	13,783	14.16
7	58,524	60.13
Total	97,333	100.00

Notes: This table shows the number of years an individual stays in my sample and the number of observation (person-years). Overall, my unbalanced panel (preferred sample) is a quite strong balanced panel if we consider those individuals who stay 5 years or more in my sample (93.37% of the observations). Panel years refer to the number of years an individual appears in my sample between 2012–2018 (including 2015). The observations are the number of person-years in my estimation sample (excluding 2015).

Table A.6
Main results including covariates.

	(1) No cov.	(2) Soc. Dem	(3) State + month FE	(4) Life sat. FE	(5) Macro vars
Post	0.202***	0.209***	0.209***	0.209***	0.232***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.010)
High ref. dist*Post	−0.024**	−0.025***	−0.024***	−0.024***	−0.020**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)

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Table A.6 (*continued*).

	(1) No cov.	(2) Soc. Dem	(3) State + month FE	(4) Life sat. FE	(5) Macro vars
Ref. Age 18–24					
Age 25–34		−0.042***	−0.042***	−0.042***	−0.040***
		(0.014)	(0.014)	(0.014)	(0.015)
Age 35–44		−0.074***	−0.074***	−0.074***	−0.066***
		(0.019)	(0.019)	(0.019)	(0.019)
Age 45–54		−0.093***	−0.091***	−0.092***	−0.079***
		(0.022)	(0.022)	(0.022)	(0.022)
Age 55–64		−0.089***	−0.087***	−0.088***	−0.070***
		(0.024)	(0.024)	(0.024)	(0.024)
Age 65+		−0.089***	−0.089***	−0.089***	−0.067**
		(0.027)	(0.027)	(0.027)	(0.028)
Ref. Primary					
Secondary		−0.039*	−0.037*	−0.037*	−0.033
		(0.022)	(0.022)	(0.022)	(0.022)
Tertiary		−0.116***	−0.113***	−0.113***	−0.103***
		(0.026)	(0.026)	(0.026)	(0.026)
Ref. Married					
Single		0.032**	0.032**	0.032**	0.029*
		(0.016)	(0.016)	(0.016)	(0.016)
Divorced		−0.009	−0.01	−0.01	−0.01
		(0.015)	(0.015)	(0.015)	(0.015)
Widowed		−0.003	0.000	0.000	0.006
		(0.021)	(0.021)	(0.021)	(0.021)
Retired		0.011	0.011	0.011	0.014
		(0.011)	(0.011)	(0.011)	(0.011)
Mat. Leave		−0.010	−0.011	−0.010	−0.010
		(0.013)	(0.013)	(0.013)	(0.013)
Ref. Employed					
Unemployed		−0.016	−0.016*	−0.017*	−0.018*
		(0.010)	(0.010)	(0.010)	(0.010)
Nonworking		0.003	0.002	0.002	0.002
		(0.009)	(0.009)	(0.009)	(0.009)
In education		0.016	0.016	0.016	0.014
		(0.012)	(0.012)	(0.012)	(0.012)
Other non-working		0.017*	0.017*	0.017*	0.016*
		(0.010)	(0.010)	(0.010)	(0.010)
Disabled		0.014	0.014	0.014	0.015
		(0.010)	(0.010)	(0.010)	(0.010)
Ln. Net HH income		−0.002	−0.002	−0.002	0.001
		(0.006)	(0.006)	(0.006)	(0.006)
Num. Children		0.015***	0.014***	0.014***	0.011**
		(0.004)	(0.004)	(0.004)	(0.004)
Ref. Life sat = 0					
1				−0.024	−0.024
				(0.036)	−0.036
2				−0.022	−0.02
				(0.030)	−0.029
3				−0.012	−0.012
				(0.030)	−0.03
4				−0.028	−0.028
				(0.030)	−0.03
5				−0.022	−0.022
				(0.030)	−0.03
6				−0.02	−0.021
				(0.031)	−0.03
7				−0.026	−0.026
				(0.030)	−0.03
8				−0.022	−0.023
				(0.030)	−0.03
9				−0.028	−0.029
				(0.030)	−0.03
10				−0.029	−0.029
				(0.031)	−0.031

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Table A.6 (*continued*).

	(1) No cov.	(2) Soc. Dem	(3) State + month FE	(4) Life sat. FE	(5) Macro vars
Urban					0.263***
					(0.041)
Foreigners %					−0.030***
					(0.004)
Youth %					−0.005
					(0.006)
Total pop.					0.000***
					0.000
Female %					−0.098***
					(0.016)
Unemp. Rate					0.003
					(0.004)
Empty housing %					−0.008**
					(0.003)
Rep94%					0.028
					(0.023)
PDS 94%					0.000
					(0.005)
Ln. GDP per cap.					0.018
					(0.046)
State FE	No	No	Yes	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Covars	No	Yes	Yes	Yes	Yes
R-sq	0.08	0.081	0.083	0.083	0.087
Obs.	97,333	97,333	97,333	97,333	97,333
N	19,686	19,686	19,686	19,686	19,686

Notes: This table shows the results for the static DiD equation. The results are estimated using the 2012 2018 unbalanced sample, where observations for 2015 are dropped. Column (1) shows the results without covariates, column (2) adds individual socio-demographic characteristics, column (3) adds state and month of interview fixed-effects, column (4) adds the life satisfaction scale, and column (5) adds district characteristics. The last column reflects my main specification. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.7
Effect heterogeneity — individual characteristics (i).

	(1) Male	(2) Female	(3) Below 45 yo.	(4) Above 45 yo.	(5) Married	(6) Not married	(7) No for. Friends	(8) W. for. Friends
High ref. dist*Y2018	−0.017	−0.027**	−0.004	−0.034***	−0.036***	0.000	−0.016	−0.030**
	(0.013)	(0.012)	(0.013)	(0.013)	(0.012)	(0.014)	(0.013)	(0.012)
High ref. dist*Y2017	−0.022	−0.034***	−0.021	−0.033**	−0.031**	−0.024	−0.021	−0.038***
	(0.013)	(0.013)	(0.014)	(0.013)	(0.012)	(0.015)	(0.014)	(0.013)
High ref. dist*Y2016	−0.022	−0.034***	−0.025*	−0.032**	−0.026**	−0.034**	−0.027*	−0.033**
	(0.014)	(0.013)	(0.014)	(0.014)	(0.013)	(0.015)	(0.014)	(0.014)
High ref. dist*Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
High ref. dist*Y2013	0.005	−0.003	−0.023*	0.009	−0.002	0.005	0.014	−0.021*
	(0.011)	(0.010)	(0.014)	(0.011)	(0.011)	(0.013)	(0.012)	(0.012)
High ref. dist*Y2012	0.02	−0.005	−0.011	0.012	0.013	−0.005	0.017	−0.012
	(0.012)	(0.012)	(0.016)	(0.011)	(0.011)	(0.015)	(0.012)	(0.012)
Y2018	0.039**	0.029*	0.013	0.046**	0.046**	0.01	0.082 ***	0.002
	(0.016)	(0.017)	(0.016)	(0.019)	(0.018)	(0.016)	(0.018)	(0.015)
Y2017	0.110***	0.116***	0.089***	0.127***	0.128***	0.085***	0.152***	0.081***
	(0.014)	(0.015)	(0.014)	(0.017)	(0.015)	(0.016)	(0.015)	(0.013)
Y2016	0.199***	0.211***	0.178***	0.221***	0.215***	0.188***	0.243***	0.165***
	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)	(0.013)	(0.013)	(0.012)
Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
Y2013	−0.033***	−0.055***	−0.043***	−0.043***	−0.042***	−0.052***	−0.061***	−0.021***
	(0.008)	(0.008)	(0.010)	(0.008)	(0.008)	(0.010)	(0.009)	(0.008)
Y2012	−0.058***	−0.080***	−0.066***	−0.071***	−0.080***	−0.056***	−0.094***	−0.041***
	(0.011)	(0.010)	(0.012)	(0.011)	(0.010)	(0.013)	(0.011)	(0.010)

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Table A.7 (*continued*).

	(1) Male	(2) Female	(3) Below 45 yo.	(4) Above 45 yo.	(5) Married	(6) Not married	(7) No for. Friends	(8) W. for. Friends
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	44,309	53,024	34,657	62,676	62,055	35,278	56,831	40,502
N	8,912	10,774	7,850	11,836	12,274	7,412	11,669	8,017

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(8) show the different subgroups for which the results are estimated. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3, with the exception of the covariate for which the sample split is performed. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.8
Effect heterogeneity — individual characteristics (ii).

	(1) Tertiary	(2) Non-Tertiary	(3) Employed	(4) Not-employed	(5) Low income	(6) High income	(7) Left (<5)	(8) Centre(=5)	(9) Right (>5)
High ref. dist*Y2018	−0.028**	−0.019	−0.026**	−0.019	−0.006	−0.040***	−0.022*	−0.018	−0.024
	(0.013)	(0.012)	(0.011)	(0.016)	(0.014)	(0.013)	(0.012)	(0.014)	(0.022)
High ref. dist*Y2017	−0.033**	−0.025**	−0.036***	−0.018	−0.019	−0.039***	−0.019	−0.009	−0.072***
	(0.015)	(0.013)	(0.011)	(0.017)	(0.015)	(0.013)	(0.015)	(0.015)	(0.022)
High ref. dist*Y2016	−0.026*	−0.029**	−0.031**	−0.027*	−0.030**	−0.029**	−0.016	−0.023	−0.050***
	(0.015)	(0.013)	(0.012)	(0.016)	(0.015)	(0.013)	(0.016)	(0.015)	(0.019)
High ref. dist*Y2014	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.
High ref. dist*Y2013	−0.006	0.003	−0.008	0.012	0.001	0.001	−0.008	0.001	0.010
	(0.011)	(0.011)	(0.011)	(0.013)	(0.012)	(0.012)	(0.011)	(0.014)	(0.017)
High ref. dist*Y2012	−0.003	0.010	−0.002	0.018	0.007	0.007	−0.008	0.009	0.011
	(0.012)	(0.012)	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)	(0.014)	(0.019)
Y2018	0.005	0.058***	0.009	0.074***	0.062***	0.011	0.01	0.057***	0.043
	(0.018)	(0.016)	(0.013)	(0.022)	(0.018)	(0.016)	(0.016)	(0.020)	(0.029)
Y2017	0.080***	0.137***	0.087***	0.155***	0.140***	0.090***	0.072***	0.130***	0.159***
	(0.016)	(0.014)	(0.011)	(0.020)	(0.017)	(0.013)	(0.015)	(0.017)	(0.025)
Y2016	0.169***	0.228***	0.184***	0.239***	0.233***	0.179***	0.164***	0.224***	0.248***
	(0.015)	(0.012)	(0.010)	(0.016)	(0.013)	(0.012)	(0.013)	(0.015)	(0.021)
Y2014	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.
Y2013	−0.011	−0.063***	−0.048***	−0.041***	−0.046***	−0.043***	−0.022***	−0.061***	−0.052***
	(0.008)	(0.009)	(0.008)	(0.010)	(0.009)	(0.010)	(0.008)	(0.009)	(0.013)
Y2012	−0.024**	−0.098***	−0.066***	−0.078***	−0.081***	−0.060***	−0.035***	−0.095***	−0.079***
	(0.011)	(0.011)	(0.010)	(0.013)	(0.011)	(0.011)	(0.011)	(0.012)	(0.017)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	30,813	66,520	58,527	38,806	49,246	48,087	32,019	39,657	23,298
N	5,997	13,689	12,040	7,646	9,892	9,794	6,299	7,866	4,645

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(9) show the different subgroups for which the results are estimated. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3, with the exception of the covariate for which the sample split is performed. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.9
Effect heterogeneity — district characteristics.

	(1) Low GDP pc	(2) High GDP pc	(3) Low unempl.	(4) High unempl.	(5) West	(6) East	(7) Rural	(8) Urban
High ref. dist*Y2018	−0.022	−0.024*	−0.038**	−0.011	−0.024**	−0.01	−0.025*	−0.017
	(0.015)	(0.013)	(0.015)	(0.014)	(0.012)	(0.019)	(0.013)	(0.017)
High ref. dist*Y2017	−0.025	−0.033**	−0.041***	−0.015	−0.037***	0.007	−0.022	−0.037**
	(0.016)	(0.014)	(0.015)	(0.016)	(0.012)	(0.024)	(0.014)	(0.018)
High ref. dist*Y2016	−0.027*	−0.030*	−0.024	−0.030*	−0.031**	−0.023	−0.016	−0.046**
	(0.015)	(0.016)	(0.015)	(0.016)	(0.012)	(0.024)	(0.013)	(0.019)

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Table A.9 (*continued*).

	(1) Low GDP pc	(2) High GDP pc	(3) Low unempl.	(4) High unempl.	(5) West	(6) East	(7) Rural	(8) Urban
High ref. dist*Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
High ref. dist*Y2013	−0.001	0.001	0.011	−0.009	0.003	−0.016	0.005	−0.016
	(0.013)	(0.012)	(0.013)	(0.012)	(0.010)	(0.018)	(0.011)	(0.016)
High ref. dist*Y2012	0.013	−0.004	0.022	−0.006	0.007	−0.013	0.017	−0.023
	(0.015)	(0.012)	(0.014)	(0.014)	(0.011)	(0.021)	(0.012)	(0.017)
Y2018	0.054**	0.029**	0.039**	0.03	0.030**	0.113**	0.053***	0.019
	(0.022)	(0.014)	(0.018)	(0.020)	(0.013)	(0.051)	(0.017)	(0.018)
Y2017	0.135***	0.102***	0.120***	0.103***	0.110***	0.165***	0.133***	0.085***
	(0.018)	(0.014)	(0.015)	(0.018)	(0.012)	(0.041)	(0.015)	(0.017)
Y2016	0.224***	0.192***	0.207***	0.199***	0.195***	0.273***	0.223***	0.176***
	(0.015)	(0.014)	(0.013)	(0.016)	(0.011)	(0.029)	(0.013)	(0.017)
Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
Y2013	−0.051***	−0.040***	−0.045***	−0.039***	−0.043***	−0.051***	−0.052***	−0.032***
	(0.010)	(0.009)	(0.010)	(0.009)	(0.008)	(0.013)	(0.008)	(0.011)
Y2012	−0.081***	−0.064***	−0.065***	−0.069***	−0.071***	−0.065**	−0.080***	−0.063***
	(0.013)	(0.010)	(0.012)	(0.012)	(0.009)	(0.025)	(0.011)	(0.014)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	52,770	44,563	47,090	50,243	75,923	21,410	67,970	29,363
N	10,633	9,053	9,671	10,015	5,492	4,194	13,815	5,871

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(8) show the different subgroups for which the results are estimated. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3, with the exception of the covariate for which the sample split is performed. Controls at the individual level: age groups (ref. cat: *age* <= 24), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.10
Effect heterogeneity — share of migrants in the district and EAE location.

	(1) Low foreig.	(2) High foreig.	(3) Low men for.	(4) High men for.	(5) Low fem. For	(6) High fem for.	(7) No EAE 14	(8) EAE 14
High ref. dist*Y2018	−0.028**	−0.012	−0.026*	−0.017	−0.025**	−0.017	−0.029**	0.016
	(0.013)	(0.017)	(0.013)	(0.016)	(0.013)	(0.017)	(0.012)	(0.025)
High ref. dist*Y2017	−0.02	−0.036**	−0.016	−0.043***	−0.016	−0.043**	−0.035***	0.009
	(0.015)	(0.016)	(0.015)	(0.016)	(0.014)	(0.017)	(0.013)	(0.023)
High ref. dist*Y2016	−0.016	−0.045***	−0.013	−0.051***	−0.013	−0.048***	−0.028**	−0.019
	(0.014)	(0.016)	(0.014)	(0.017)	(0.014)	(0.016)	(0.013)	(0.029)
High ref. dist*Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
High ref. dist*Y2013	0.002	−0.003	0.000	0.000	0.000	−0.001	−0.003	0.000
	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.010)	(0.016)
High ref. dist*Y2012	0.02	−0.019	0.017	−0.016	0.016	−0.013	0.007	−0.020
	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.014)	(0.011)	(0.023)
Y2018	0.074***	0.024*	0.076***	0.022	0.084***	0.016	0.039***	−0.007
	(0.021)	(0.014)	(0.021)	(0.015)	(0.019)	(0.015)	(0.014)	(0.029)
Y2017	0.142***	0.105***	0.142***	0.108***	0.150***	0.098***	0.118***	0.074***
	(0.018)	(0.013)	(0.017)	(0.014)	(0.016)	(0.015)	(0.012)	(0.027)
Y2016	0.234***	0.188***	0.232***	0.192***	0.236***	0.187***	0.209***	0.180***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.013)	(0.015)	(0.011)	(0.033)
Y2014	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
Y2013	−0.057***	−0.035***	−0.056***	−0.037***	−0.057***	−0.038***	−0.047***	−0.031**
	(0.010)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)	(0.014)
Y2012	−0.083***	−0.063***	−0.083***	−0.067***	−0.084***	−0.069***	−0.074***	−0.045**
	(0.013)	(0.010)	(0.013)	(0.011)	(0.012)	(0.011)	(0.009)	(0.020)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	54,462	42,871	55,091	42,242	54,001	43,332	80,186	17,147
N	11,015	8,671	11,132	8,554	10,948	8,738	16,267	3,419

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(8) show the different subgroups for which the results are estimated. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3, with the exception of the covariate for which the sample split is performed. Controls at the individual level: age groups (ref. cat: *age* <= 24), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.11
Effect heterogeneity — share of different country groups in the district.

	(1) Low W. Asia	(2) High W. Asia	(3) Low S. Asia	(4) High S. Asia	(5) Low Turkey%	(6) High Turkey%	(7) Low EU 15%	(8) High EU 15%	(9) Low EU 13%	(10) High EU 13%
High ref. dist*Y2018	−0.022	−0.022	−0.014	−0.029*	−0.027**	−0.013	−0.01	−0.036**	−0.025*	−0.032*
	(0.015)	(0.014)	(0.013)	(0.016)	(0.013)	(0.016)	(0.012)	(0.017)	(0.013)	(0.018)
High ref. dist*Y2017	−0.013	−0.040***	−0.02	−0.035**	−0.018	−0.037**	−0.005	−0.057***	−0.028*	−0.043***
	(0.017)	(0.015)	(0.014)	(0.016)	(0.014)	(0.017)	(0.014)	(0.017)	(0.016)	(0.016)
High ref. dist*Y2016	−0.021	−0.028*	−0.021	−0.038**	−0.015	−0.042**	−0.017	−0.042**	−0.027*	−0.043**
	(0.016)	(0.015)	(0.015)	(0.016)	(0.014)	(0.017)	(0.014)	(0.016)	(0.015)	(0.017)
High ref. dist*Y2014	.	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.	.
High ref. dist*Y2013	0.005	−0.009	−0.006	0.005	−0.01	0.009	0.007	−0.01	0.004	−0.009
	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)	(0.013)
High ref. dist*Y2012	0.021	−0.013	0.011	−0.004	0.017	−0.011	0.019	−0.014	0.006	−0.011
	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.013)	(0.015)	(0.014)
Y2018	0.046**	0.037**	0.075***	0.027*	0.078***	0.019	0.084***	0.016	0.076***	0.037***
	(0.021)	(0.017)	(0.021)	(0.015)	(0.019)	(0.015)	(0.019)	(0.016)	(0.024)	(0.013)
Y2017	0.125***	0.109***	0.151***	0.096***	0.144***	0.105***	0.148***	0.102***	0.150***	0.108***
	(0.018)	(0.015)	(0.017)	(0.014)	(0.016)	(0.015)	(0.015)	(0.015)	(0.020)	(0.012)
Y2016	0.226***	0.184***	0.237***	0.185***	0.233***	0.191***	0.240***	0.183***	0.236***	0.196***
	(0.015)	(0.013)	(0.014)	(0.014)	(0.012)	(0.015)	(0.012)	(0.015)	(0.016)	(0.012)
Y2014	.	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.	.
Y2013	−0.054***	−0.035***	−0.061***	−0.035***	−0.051***	−0.042***	−0.060***	−0.033***	−0.053***	−0.038***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)	(0.008)
Y2012	−0.081***	−0.063***	−0.092***	−0.065***	−0.082***	−0.067***	−0.088***	−0.062***	−0.075***	−0.068***
	(0.013)	(0.011)	(0.014)	(0.010)	(0.012)	(0.011)	(0.013)	(0.010)	(0.015)	(0.010)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	51,348	45,985	51,632	45,701	53,724	43,609	53,897	43,436	53,557	43,776
N	10,393	9,293	10,464	9,222	10,869	8,817	10,903	8,783	10,779	8,907

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(10) show the different subgroups for which the results are estimated. Country groups are defined as follows. Western Asia: Armenia, Azerbaijan, Bahrain, Georgia, Iraq, Israel, Yemen, Jordan, Qatar, Kuwait, Lebanon, Oman, Palestinian territories, Saudi Arabia, Syria, Turkey, United Arab Emirates. Southern Asia: Afghanistan, Bangladesh, Bhutan, India, Iran, Maldives, Nepal, Pakistan, Sri Lanka. EU-15: Belgium, France, Italy, Luxembourg, Netherlands, Denmark, Ireland, Greece, Portugal, Spain, Finland, Austria, Sweden, United Kingdom. EU-13: Estonia, Latvia, Lithuania, Malta, Poland, Slovakia, Slovenia, Czech Republic, Hungary, Cyprus, Bulgaria, Romania, Croatia. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3, with the exception of the covariate for which the sample split is performed. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.12
Sensitivity analysis: different sample restrictions.

	(1) Main	(2) w/o covars	(3) No extremes	(4) Age <= 65	(5) Pop <p99	(6) Balanced	(7) No movers
High ref. dist*Y2018	−0.023**	−0.019*	−0.024**	−0.019*	−0.021*	−0.024*	−0.020*
	−0.01	(0.010)	(0.010)	(0.010)	(0.011)	(0.013)	(0.011)
High ref. dist*Y2017	−0.029***	−0.026**	−0.030***	−0.029**	−0.025**	−0.026*	−0.026**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.014)	(0.011)
High ref. dist*Y2016	−0.029**	−0.027**	−0.029**	−0.025**	−0.022*	−0.034**	−0.029**
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.014)	(0.012)
High ref. dist*Y2014	.	.	.	.	.	.	.
	.	.	.	.	.	.	.
High ref. dist*Y2013	0.000	−0.001	0.000	−0.007	−0.002	−0.001	0.002
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)	(0.009)
High ref. dist*Y2012	0.006	0.004	0.005	0.003	0.008	0.001	0.007
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.010)
Y2018	0.034***	0.087***	0.032**	0.026**	0.043***	0.043***	0.032*
	(0.013)	(0.007)	(0.012)	(0.013)	(0.014)	(0.016)	(0.017)
Y2017	0.113***	0.153***	0.111***	0.101***	0.120***	0.129***	0.113***
	(0.011)	(0.007)	(0.011)	(0.011)	(0.012)	(0.014)	(0.015)
Y2016	0.205***	0.230***	0.204***	0.191***	0.211***	0.230***	0.207***
	(0.010)	(0.007)	(0.010)	(0.010)	(0.011)	(0.013)	(0.012)
Y2014	.	.	.	.	.	.	.
	.	.	.	.	.	.	.
Y2013	−0.045***	−0.054***	−0.045***	−0.050***	−0.046***	−0.032***	−0.046***
	(0.007)	(0.006)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)
Y2012	−0.070***	−0.089***	−0.070***	−0.073***	−0.076***	−0.058***	−0.070***
	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	97,333	97,333	95,099	71,572	88,337	58,524	91,832
N	19,686	19,686	19,239	15,018	17,896	9,754	18,359

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (1)–(7) show the different samples for the robustness checks. Column (1) shows the result of Eq. (2). Column (2) does the same but without including any individual or district covariates. Column (3) exclude districts were the reported inflow of asylum-seekers was too low (below the first percentile) or too high (above the ninety-ninth percentile). Column (4) only considers individuals aged 18-65 (in working age). Column (5) excludes respondents in the 5 largest cities (Berlin, Hamburg, Hanover, Cologne, Munich). Column (6) uses only a balanced panel. Column (7) considers only respondents who did not change their district of residence. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications – except column (2) – include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.13
Results by German citizenship and migration background.

	(1) Main	(2) Not Germans	(3) Germans	(4) No mig. Back	(5) With mig. Back
High ref. dist*Y2018	−0.023**	0.017	−0.024**	−0.027**	0.007
	(0.010)	(0.033)	(0.011)	(0.011)	(0.022)
High ref. dist*Y2017	−0.029***	−0.01	−0.029**	−0.028**	−0.034
	(0.011)	(0.038)	(0.011)	(0.012)	(0.023)
High ref. dist*Y2016	−0.029**	−0.027	−0.029**	−0.028**	−0.037
	(0.011)	(0.042)	(0.011)	(0.011)	(0.027)
High ref. dist*Y2014	.	.	.	.	.
	.	.	.	.	.
High ref. dist*Y2013	0.000	0.047	−0.002	0.001	−0.004
	(0.009)	(0.043)	(0.009)	(0.009)	(0.026)
High ref. dist*Y2012	0.006	0.014	0.005	0.006	−0.003
	(0.010)	(0.040)	(0.010)	(0.010)	(0.025)
Y2018	0.034***	−0.071	0.036***	0.038***	0.003
	(0.013)	(0.054)	(0.013)	(0.013)	(0.032)
Y2017	0.113***	0.078	0.113***	0.112***	0.117***
	(0.011)	(0.050)	(0.012)	(0.012)	(0.028)
Y2016	0.205***	0.176***	0.205***	0.207***	0.194***
	(0.010)	(0.047)	(0.010)	(0.010)	(0.028)

(*continued on next page*)

Table A.13 (*continued*).

	(1) Main	(2) Not Germans	(3) Germans	(4) No mig. Back	(5) With mig. Back
Y2014	.	.	.	.	.
	.	.	.	.	.
Y2013	−0.045***	−0.030	−0.045***	−0.044***	−0.046***
	(0.007)	(0.028)	(0.007)	(0.007)	(0.017)
Y2012	−0.070***	−0.035	−0.070***	−0.069***	−0.073***
	(0.008)	(0.030)	(0.009)	(0.009)	(0.019)
Covariates	Yes	Yes	Yes	Yes	Yes
Obs.	97,333	4,165	93,168	84,975	12,358
N	19,686	909	18,777	16,969	2,717

Notes: This table shows the results for Eq. (2) (exposure effects and time trends). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. The columns (2)–(5) show the different subgroups for which the results are estimated. Standard errors are clustered at the level of the district of residence in 2015. Column (1) shows the main results for comparison. All regressions include the covariates described in Table A.3. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.14
Robustness check: High-low refugee migration districts and other concerns.

	(1) Very concerned about	(2)	(3)	(4)	(5)	(6)	(7)
	Crime in Germany	Hostility towards foreigners or minorities in Germany	Maintaining peace	Economic development	Own economic situation	Own health	Your job security
High ref. dist*Post	−0.029***	−0.002	−0.010	−0.007	0.001	0.001	0.003
	(0.008)	(0.008)	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)
Post	0.196***	0.327***	0.238***	0.066***	0.011**	0.016***	−0.002
	(0.009)	(0.011)	(0.009)	(0.007)	(0.006)	(0.005)	(0.006)
R^2 -within	0.055	0.098	0.082	0.011	0.034	0.022	0.018
Obs.	97,255	97,102	97,228	97,167	97,216	97,257	58,833
N	19,686	19,685	19,686	19,683	19,684	19,686	14,570
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the results for a standard DiD equation using $HighRef_{d2015} * Post_t$ instead of yearly interactions, using as outcome the concerns mentioned in columns (1)–(7). The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.15
Concerns about immigration, crime and political preferences.

	(1)	(2)	(3)	(4)
	Lean towards			
	Centre-right	Centre-left	Left-wing	Right-wing
High ref. dist*Post	−0.010*	0.001	0.006*	−0.003
	(0.005)	(0.005)	(0.003)	(0.004)
Very concerned about immigration	−0.004	−0.008**	−0.006***	0.016***
	(0.004)	(0.004)	(0.002)	(0.002)
Very concerned about crime	−0.001	−0.002	−0.002	0.007***
	(0.004)	(0.004)	(0.002)	(0.002)
Post	−0.003	−0.004	−0.013***	0.013***
	(0.006)	(0.006)	(0.004)	(0.005)
R^2 -within	0.004	0.007	0.007	0.025
Obs.	45,475	45,475	45,475	45,475
N	13,205	13,205	13,205	13,205
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Month of int. FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

Notes: This table shows the results for a standard DiD equation using $HighRef_{d2015} * Post_t$ instead of yearly interactions, where the outcomes are preferences toward the group of parties mentioned in columns (1)–(4). The main regressors are $HighRef_{d2015} * Post_t$, a dummy that equals one if the respondent is very concerned about immigration, and a dummy that equals one if the respondent is very concerned about crime in Germany. The results are estimated using the 2012–2018 unbalanced sample, where observations for 2015 are dropped. Standard errors are clustered at the level of the district of residence in 2015. All regressions include the covariates described in Table A.3. Controls at the individual level: age groups (ref. cat: $age <= 24$), education level (ref. cat: primary), marital status (ref. cat: married), employment status (ref. cat: employed), log net household income, number of children in the household. Controls at the district level: average age, % of foreigners, dependency ratio, total population, % of female, unemployment rate, log GDP per capita, % of empty housing. All covariates are as of the year previous to the interview. All specifications include month of interview and state fixed effects. Statistically significant at $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Appendix B. Asylum-seekers' data

The data used in this paper for asylum-seekers comes from Research Data Centre from the German (RDC) (RDC, 2015) (*Asylstatistiken*). This dataset contains information on the recipients of asylum-seeker benefits under the Asylum Seekers Benefits Act (AsylbLG). These statistics are reported annually (as of December 31 of each year) and comprise comprehensive information on the asylum-seekers (date on which they started receiving benefits, country of origin, gender, employment status, type of benefit received, etc.). Data access has to be requested and paid for, access is only possible at the venues of the RDC.

Since I want to analyse the impact of the refugee inflow of 2015, and given the mobility restrictions during the first 6 months in Germany, I consider this dataset to provide a comprehensive and reliable measure of this inflow. Given that the major inflow occurred in the second half of 2015, the new asylum-seekers must be reported on the *Asylstatistiken* of 2015.

Appendix C. Multilevel model

Papers studying voting preferences have often relied on multilevel models (MLM) to differentiate individual and contextual effects. Below I provide an MLM of my main specification (with yearly interactions of the treatment). Table C.1 shows a pyramid of multilevel models where the dependent variable is a dummy for being very concerned about immigration. M0 is the empty model. It shows that most of my variation comes from within the individual over time (0.1308), which is the main source of variation I seek to exploit. Further variation comes from between respondents (0.0793) and, to a lesser extent, from between districts (0.0065).[40] Model M1 adds year dummies and their interactions with my treatment variable (high/low refugee migration district). With these additional variables the variance within individuals over time reduces to 0.1176, and the interactions for the years 2016–2018 are statistically significant at the 5% level as in my main DiD specification. Model M2 adds individual characteristics. This model shows that age and "male" are positively correlated with being very concerned about immigration, while having tertiary education reduces the probability of being very concerned. The variance components between districts and between respondents are reduced. Model M3 adds month of interview and state fixed effects, which further reduces the variance between districts from 0.0054 to 0.0037. Model M4 adds dummy variables for life satisfaction, which does not impact the variance components. Finally, model M5 adds district-level time-varying covariates. The variance components remain very similar to those of model M4. While models M3 and M4 showed concerns about immigration were on average higher in some federal states (primarily in East Germany), when adding district characteristics these state effects are no longer statistically significant. Model M5 shows identical treatment effects to those of my main DiD specification. This demonstrates my results are very robust to the inclusion of different covariates and when using an MLM.

[40] Since in my DiD specification I assign treatment at the district of residence in 2015, I also use the district of residence in 2015 for the random intercept in the MLM.

Table C.1
Full multilevel-models.

	(1) M0	(2) M1	(3) M2	(4) M3	(5) M4	(6) M5
High ref. dist* Y2018		−0.019 *	−0.019 *	−0.019 *	−0.020 *	−0.023 **
		(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
High ref. dist* Y2017		−0.025 **	−0.026 **	−0.026 **	−0.026 **	−0.029 ***
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
High ref. dist* Y2016		−0.027 **	−0.027 **	−0.026 **	−0.026 **	−0.028 **
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
High ref. dist* Y2014		.	.	.	.	.
		.	.	.	.	.
High ref. dist* Y2013		0.001	0.001	0.001	0.001	0.002
		(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
High ref. dist* Y2012		0.005	0.006	0.006	0.006	0.008
		(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
Y2018		0.084 ***	0.085 ***	0.087 ***	0.088 ***	0.059 ***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.010)
Y2017		0.151 ***	0.151 ***	0.151 ***	0.151 ***	0.131 ***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.009)
Y2016		0.228 ***	0.228 ***	0.229 ***	0.229 ***	0.219 ***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Y2014		.	.	.	.	.
		.	.	.	.	.
Y2013		−0.052 ***	−0.053 ***	−0.053 ***	−0.053 ***	−0.049 ***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Y2012		−0.086 ***	−0.088 ***	−0.087 ***	−0.087 ***	−0.079 ***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
High ref. District		0.031 ***	0.031 ***	0.017	0.017	0.018
		(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Ref. Age 18-24						
Age 25–34			0.049 ***	0.046 ***	0.045 ***	0.044 ***
			(0.010)	(0.010)	(0.010)	(0.010)
Age 35–44			0.064 ***	0.060 ***	0.057 ***	0.056 ***
			(0.011)	(0.011)	(0.010)	(0.010)
Age 45–54			0.068 ***	0.066 ***	0.061 ***	0.061 ***
			(0.010)	(0.010)	(0.010)	(0.010)
Age 55–64			0.075 ***	0.071 ***	0.066 ***	0.066 ***
			(0.011)	(0.011)	(0.011)	(0.011)
Age 65+			0.067 ***	0.061 ***	0.059 ***	0.058 ***
			(0.014)	(0.014)	(0.014)	(0.014)
Male			0.015 ***	0.014 ***	0.014 ***	0.014 ***
			(0.004)	(0.004)	(0.004)	(0.004)
Ref. Primary						
Secondary			−0.016	−0.014	(0.012)	−0.009
			(0.013)	(0.013)	−0.013	(0.013)
Tertiary			−0.170 ***	−0.169 ***	(0.166) ***	−0.161 ***
			(0.013)	(0.013)	−0.013	(0.013)
Ref. Married						
Single			−0.023 ***	−0.023 ***	−0.024 ***	−0.024 ***
			(0.008)	(0.008)	(0.008)	(0.008)
Divorced			0.004	0.005	0.003	0.003
			(0.008)	(0.008)	(0.008)	(0.008)
Widowed			−0.003	−0.003	−0.006	−0.007
			(0.009)	(0.009)	(0.009)	(0.009)
Retired			0.017 *	0.016	0.014	0.014
			(0.009)	(0.009)	(0.009)	(0.009)
Mat. Leave			0.006	0.004	0.006	0.006
			(0.012)	(0.012)	(0.012)	(0.012)
Ref. Employed						
Unemployed			0.007	0.006	−0.001	0.000
			(0.009)	(0.009)	(0.009)	(0.009)
Nonworking			0.006	0.005	0.003	0.003
			(0.007)	(0.007)	(0.007)	(0.007)
In education			−0.036 ***	−0.038 ***	−0.037 ***	−0.037 ***
			(0.010)	(0.010)	(0.010)	(0.010)
Other non-working			0.003	0.001	0.001	0.001
			(0.009)	(0.009)	(0.009)	(0.009)

(*continued on next page*)

Table C.1 (*continued*).

	(1) M0	(2) M1	(3) M2	(4) M3	(5) M4	(6) M5
Disabled			0.033 ***	0.033 ***	0.029 ***	0.028 ***
			(0.007)	(0.007)	(0.007)	(0.007)
Ln. Net HH income			−0.031 ***	−0.030 ***	−0.025 ***	−0.026 ***
			(0.005)	(0.005)	(0.005)	(0.005)
Migration background			−0.012	−0.009	−0.009	−0.009
			(0.008)	(0.008)	(0.007)	(0.008)
Num. Children			−0.008 ***	−0.006 **	−0.006 **	−0.006 **
			(0.003)	(0.003)	(0.003)	(0.003)
Ref. State = Schleswig-Holstein						
Hamburg				−0.036	−0.036	−0.029
				(0.030)	(0.029)	(0.042)
Lower Saxony				0.015	0.014	0.000
				(0.018)	(0.018)	(0.019)
Bremen				0.022	0.020	−0.002
				(0.026)	(0.025)	(0.027)
North Rhine-Westphalia				0.064 ***	0.062 ***	0.049 **
				(0.018)	(0.017)	(0.020)
Hesse				0.023	0.021	−0.009
				(0.018)	(0.018)	(0.026)
Rhineland-Palatinate				−0.009	−0.010	−0.037
				(0.022)	(0.022)	(0.027)
Baden-Württemberg				0.007	0.005	−0.042
				(0.019)	(0.019)	(0.029)
Bayern				0.043 **	0.041 **	0.000
				(0.017)	(0.017)	(0.027)
Saarland				0.057 **	0.056 **	0.051
				(0.028)	(0.028)	(0.033)
Berlin				0.051 **	0.047 **	0.046
				(0.023)	(0.022)	(0.079)
Brandenburg				0.108 ***	0.103 ***	0.057
				(0.025)	(0.025)	(0.050)
Mecklenburg-Western Pomerania				0.043 *	0.041 *	−0.007
				(0.023)	(0.023)	(0.061)
Saxony				0.082 ***	0.079 ***	−0.009
				(0.024)	(0.024)	(0.047)
Saxony-Anhalt				0.121 ***	0.117 ***	0.052
				(0.023)	(0.023)	(0.052)
Thuringia				0.098 ***	0.096 ***	0.034
				(0.031)	(0.031)	(0.048)
Ref. Month = January						
February				−0.004	−0.002	−0.003
				(0.024)	(0.024)	(0.024)
March				−0.014	−0.012	−0.013
				(0.024)	(0.024)	(0.024)
April				−0.024	−0.022	−0.023
				(0.024)	(0.024)	(0.024)
May				−0.033	−0.031	−0.032
				(0.024)	(0.024)	(0.024)
June				−0.016	−0.014	−0.015
				(0.025)	(0.025)	(0.025)
July				−0.021	−0.020	−0.021
				(0.025)	(0.025)	(0.025)
August				−0.037	−0.036	−0.037
				(0.025)	(0.025)	(0.025)
September				−0.030	−0.029	−0.030
				(0.027)	(0.027)	(0.027)
October				−0.031	−0.029	−0.030
				(0.027)	(0.027)	(0.027)
November				−0.106 ***	−0.102 ***	−0.102 ***
				(0.031)	(0.031)	(0.031)
December				−0.162 **	−0.159 **	−0.161 *
				(0.090)	(0.090)	(0.089)

(*continued on next page*)

Table C.1 (*continued*).

	(1) M0	(2) M1	(3) M2	(4) M3	(5) M4	(6) M5
Ref. Life sat = 0						
1					−0.069 **	−0.069 **
					(0.034)	(0.034)
2					−0.061 **	−0.063 **
					(0.028)	(0.028)
3					−0.063 **	−0.064 **
					(0.028)	(0.029)
4					−0.080 ***	−0.081 ***
					(0.029)	(0.029)
5					−0.077 ***	−0.078 ***
					(0.029)	(0.029)
6					−0.089 ***	−0.090 ***
					(0.029)	(0.029)
7					−0.105 ***	−0.106 ***
					(0.029)	(0.029)
8					−0.112 ***	−0.113 ***
					(0.029)	(0.029)
9					−0.131 ***	−0.132 ***
					(0.028)	(0.029)
10					−0.119 ***	−0.120 ***
					(0.029)	(0.029)
Urban						0.043 **
						(0.021)
Average age						0.012 ***
						(0.004)
Foreigners %						0.001
						(0.002)
Youth %						0.013 ***
						(0.003)
Total pop.						0.000
						(0.000)
Female %						−0.016 **
						(0.008)
Unemp. Rate						−0.003
						0.003
Empty housing %						0.007 ***
						(0.002)
Rep 94%						0.016 *
						(0.009)
PDS 94%						0.001
						(0.002)
Ln. GDP per cap.						0.008
						(0.020)
Constant	0.317 ***	0.239 ***	0.481 ***	0.452 ***	0.526 ***	0.495
	(0.005)	(0.007)	(0.044)	(0.049)	(0.053)	(0.379)
Variance components						
District 2015	0.0065 ***	0.0066 ***	0.0054 ***	0.0037 ***	0.0036 ***	0.0037 ***
Respondent	0.0793 ***	0.0827 ***	0.0750 ***	0.0749 ***	0.0740 ***	0.0739 ***
Within individual variation	0.1308 ***	0.1176 ***	0.1177 ***	0.1176 ***	0.1177 ***	0.1175 ***
Statistics						
Obs. (TxN)	97,333	97,333	97,333	97,333	97,333	97,333
N	19,686	19,686	19,686	19,686	19,686	19,686

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