



# Discriminatory Residential Preferences in Germany—A Vignette Study

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**Abstract** The article focusses on the generating mechanisms of residential segregation for the demand side of housing markets, i.e., discriminatory residential preferences of inhabitants regarding the composition of their neighborhood. The data stem from an online survey among a random sample of the population of a mid-sized German city. In a vignette experiment, respondents were asked to rate example residential settings with respect to their attractiveness. The settings varied regarding the ethnic and religious composition of the neighborhood and other neighborhood characteristics that are positively or negatively related to residential attractiveness.

We find that respondents have discriminatory residential preferences toward migrants and the presence of a Muslim community in the neighborhood. One-half of the migrant effect is mitigated if other positively connoted residential characteristics exist. We take this as an indication for statistical discrimination. This does not hold for the “Muslim community” effect. Discrimination gets stronger with higher levels of perceived economic group-threat from migrants. We further find evidence for a cultural group-threat and for the contact hypothesis: religious people are more discriminatory than nonreligious people, and real-life contact with migrants entails less discrimination.

Online Appendix: <https://kzfss.uni-koeln.de/sites/kzfss/pdf/Wolter-et-al.pdf>

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**Keywords** Residential segregation · Taste-based discrimination · Statistical discrimination · Group-threat · Contact hypothesis · Vignette experiment

## Diskriminierende Wohnpräferenzen in Deutschland – Eine Vignettenstudie

**Zusammenfassung** Der Beitrag untersucht Entstehungsmechanismen residentieller Segregation für die Nachfrageseite von Wohnungsmärkten, d.h. diskriminierende Wohnpräferenzen von Einwohnern bezüglich der sozialen Zusammensetzung von Wohnvierteln. Analysiert werden Daten eines Online-Surveys unter einer Zufallsstichprobe von Bewohnern einer mittelgroßen deutschen Stadt. In einem Vignettenexperiment bewerteten die Befragten beispielhafte Wohnumgebungen hinsichtlich ihrer allgemeinen Attraktivität. Die Wohnumgebungen variierten nach ethnischen und religiösen Merkmalen der fiktiven Bewohner sowie nach weiteren Merkmalen, welche die Attraktivität der Wohnsituationen positiv oder negativ beeinflussen.

Im Ergebnis zeigen sich diskriminierende Wohnpräferenzen gegenüber Migranten und der Präsenz einer muslimischen Gemeinde in der Nachbarschaft. Sofern weitere positiv konnotierte Wohnmerkmale vorliegen, reduziert sich der negative Migranteneffekt um etwa die Hälfte. Dies spricht für statistische Diskriminierung als Wirkmechanismus. Für den Effekt einer muslimischen Gemeinde gilt dies indes nicht. Weiterhin zeigen wir, dass Diskriminierung mit zunehmender wahrgenommener ökonomischer Gruppenbedrohung zunimmt und sich Hinweise für die kulturelle Gruppenbedrohungsthese und die Kontakthypothese finden: Religiöse Menschen diskriminieren mehr als nichtreligiöse und tatsächlicher alltäglicher Kontakt zu Migranten verringert diskriminierende Wohnpräferenzen.

**Schlüsselwörter** Residentielle Segregation · Taste-based (präferenzorientierte) Diskriminierung · Statistische Diskriminierung · Gruppenbedrohung · Kontakthypothese · Vignettenexperiment

### 1 Introduction

Ethnic segregation in housing markets or in geographically delimited residential areas is ubiquitous. Many studies for different countries have shown that inhabitants of cities or geographic areas tend to group together along the lines of ethnic or religious belonging with respect to their residence (e.g., Charles 2003; Cutler et al. 1999; Johnston et al. 2004; Sager 2012). In the USA, the black–white dissimilarity index reached 55% in 2020, meaning that close to 6 out of 10 Black residents would have to move to achieve an even distribution (Logan and Stults 2021). In Germany, however, residential segregation is less racial–ethnic. Instead, it is based on specific migrant groups such as Italians or Turks, and based on religion as well (e.g., Muslims vs. Christians). The largest foreign-born groups in Germany are Turks and people from the former Soviet republics, with Turks being the most affected by structural segregation (Helbig and Jähnen 2018). For example, about 31% of Turkish residents

would have to move (in 2014) in West German cities to achieve equal distribution. Several other studies have confirmed the presence of ethnic residential segregation for Germany (Glitz 2014; Hinz and Auspurg 2017; Will 2003), and although it is less pronounced than in the USA and other countries, it still represents a meaningful lack of integration in the housing market.

Residential segregation is caused both on the supply side and on the demand side of housing markets. Apart from causing factors not directly related to the actors in housing markets, such as housing or social mixing policies, discriminatory preferences and the behavior of the actors on both sides of the market is one driver of segregation. On the supply side, landlords and other gate-keepers (e.g., real-estate agents, sellers) take into account race or ethnic origin when making housing offers and contracts. The underlying causal mechanisms have been well identified by a large body of (experimental) studies (for recent studies see Gusciute et al. 2022; Horr et al. 2018; Sawert 2020; for reviews, see Auspurg et al. 2019; Rich 2014). This literature shows that both taste-based and statistical discrimination (Arrow 1973; Becker 1971 [1957]; Phelps 1972) are at play. Taste-based discrimination means that actors have ethnic-related preferences; migrants or minorities are discriminated against *because* they belong to a certain social group and *because* this elicits direct costs or benefits for the discriminating person. Statistical discrimination occurs if, owing to a lack of information (e.g., on the productivity of employees in the labor market; on the living quality of a residence in housing markets), people refer to proxy variables such as belonging to a certain ethnic group that are supposed to yield the missing information (whether or not it is empirically true).

On the demand side of housing markets, segregation is produced by actual and future residents themselves by means of their residential preferences and choices. The famous Schelling (1978) model of segregation shows that only minor in-group preferences of actors are sufficient to generate strongly segregated structures or aggregates. With respect to the demand side, the literature is more scarce and the evidence regarding the degree to which taste-based discrimination or statistical discrimination are the causing mechanisms for segregation is mixed (Harris 2001; Havekes et al. 2013; Zangger 2021). Further, although some studies have been conducted in the USA and in other countries, there is almost no research regarding Germany. This is astonishing, because Germany is Europe's largest immigrant host country: it is home to more than 20% of all the foreign-born people living in the European Union (OECD/EU 2018) and has faced numerous immigration waves throughout post-war history. Investigating discriminatory residential preferences of Germans provides an insight into a different context in which immigration policy and political-economic structure differs from the US context. Germany has an open-door policy toward asylum seekers (the right to asylum and freedom of movement) and advanced immigration and migration of highly skilled people, specifically as its economy suffers from a labor force shortage, an aging population, and a decrease in rates of childbirth (Apap 2002). The welfare state is more on the generous side and income inequality relatively low. The USA, in turn, is characterized by a stricter immigration policy (especially following 11 September 2001, and the Trump administration), a neoliberal economy, and as having one of the highest rates of income inequality in the OECD (OECD 2022). With regard to religiosity, 6.7% of the German population are

Muslims with a migration background from a Muslim country of origin, compared with 1.1% in the USA (Pew Research Center 2021). This and the fact that Germany is experiencing an increase in Muslim population growth are motivation for looking at the religious dimension when it comes to discriminatory residential preferences too.

The present article is aimed at adding empirical evidence to this gap by investigating the underlying mechanisms of segregational residential preferences on the demand side of housing markets with respect to immigrants and the religious minority of Muslims. Using factorial survey (FS) experiments (Auspurg and Hinz 2015) and original data from a population survey ( $N=1159$ ) in the mid-sized German City of Konstanz, we will investigate (1) whether, and to what extent, ethnic and religious minorities are discriminated against with respect to residential preferences; (2) whether this can be attributed to taste-based or statistical discrimination; and (3) if other established theories of anti-immigrant attitudes and xenophobia, namely the group-threat (Quillian 1995) and contact hypotheses (Allport 1954), can shed additional light on the generating mechanisms of discriminatory and/or segregational residential preferences. With respect to the City of Konstanz studied here, Helbig and Jähnen (2018) found that the municipality is among the cities with the lowest ethnic segregation in Germany. Moreover, the decline in ethnic segregation from 2010 to 2014 was among the strongest here. Hence, our study is situated in a context where actual ethnic residential segregation is low; one could expect that the discriminatory mechanisms we find get stronger in areas with higher levels of segregation (Zangger 2021).

This study contributes to the literature by adding empirical evidence for Germany showing that discriminatory/segregational residential preferences on the demand side of housing markets, i.e., by the individual residents themselves, exist; that it can be seen as the result of a mixture of taste-based and statistical discrimination; and that other established concepts and theories of xenophobia yield further insights into the generating mechanisms of these preferences. Further, we advance the existing literature methodologically by using the strategy of modeling interaction effects of vignette dimensions stemming from the FS experiment. This has only been carried out by very few existing studies in the field of research on discriminatory residential preferences. One further insight of our study is that, even if segregation caused by the supply side of housing markets were to be eliminated (by anti-discrimination laws or by housing affordability programs for minority groups, for instance), segregation caused by the demand side would continue to exist, which is in line with previous studies (e.g., Aldén et al. 2015).

The remainder of this article proceeds as follows. In Sect. 2, we roll out the theoretical framework and corresponding empirical literature on (segregational) residential preferences, arguments from classic theories of discrimination, and the group-threat and contact hypotheses. This section also presents our hypotheses and analytical strategy. Section 3 describes the study design, data, and methods. Section 4 presents the results; we report our findings in three analytical steps using multilevel regression models applied to the FS experiment data. The final section, Sect. 5, sums up the results, discusses their implications, and points to the limitations of this study and desiderata for future research.

## 2 Residential Segregation: Theory and Empirical Literature

### 2.1 General Framework and Discrimination Theory

Housing markets are divided into the demand side (renters, buyers, homeowners, people seeking new dwellings, etc.) and the supply side (landlords, sellers, constructors, real-estate agents, etc.) (Koopman 2011). This article focuses on the demand side. It investigates generating mechanisms of residential segregation caused by the residential preferences and search behavior of (future) residents themselves. Housing markets, compared with other markets such as (elementary) labor markets or simple consumer markets, or compared with what standard neoclassical economic theory presupposes, exhibit some peculiarities and market imperfections (Marsh and Gibb 2011): decisions about a future dwelling or the decision whether or not to move are high-cost decisions that exert important and long-term effects on an individual's personal life conditions and wellbeing. Further, residential decisions come with lock-in effects: once made, a residential choice cannot be withdrawn or modified without high costs, especially in periods of falling house prices and rising interest rates (Chan 2001; Ferreira et al. 2010). This is connected with the argument that there are allocation and matching problems between the demand side and the supply side in housing markets, meaning that transaction costs are high. Next, the search for a dwelling is characterized by a high amount of uncertainty and lack of information. For instance, subjects do not know about the arrival of possible future offers and their price and quality (Mulder 1996). Further, and unlike in (standard) labor market economics where the (distribution of) productivity of applicants or future employees is the only missing information regarding labor demand, dwelling seekers have many more characteristics to consider, because living quality and residential satisfaction depend on several aspects that cannot be assessed in advance: “[E]valuating a dwelling as a potential home involves constructing a scenario regarding what life—in all its diverse aspects—will be like in a particular location. [...] A consumer cannot fully appreciate their purchase until after it has been made and the good is being consumed” (Marsh and Gibb 2011, pp. 224–225).<sup>1</sup>

What follows from this? As has been argued in the literature (Clark 1993; Mulder 1996), the problem of searching for and accepting a residence is comparable with job searching in labor markets, for which reason basic labor market theories such as job search theory (Stigler 1962) can be applied—but with the need to account for the abovementioned peculiarities of housing markets. Theories of discrimination (Arrow 1973; Becker 1971 [1957]; Phelps 1972) represent a core approach for explaining preference-induced residential segregation. Also, because uncertainty and missing information in housing markets are always present, strategies for dealing with this lack of information are important and have to be taken into account (Marsh and Gibb 2011). This especially concerns the theory of statistical discrimination.

Discrimination can be defined as the unequal treatment of people because they belong to a certain social group, for example, an ethnic or religious minority (Hinz

<sup>1</sup> There are other market imperfections that we do not discuss here, for example, government regulations or corruption.

and Auspurg 2017). It can be based on taste or statistical. Taste-based discrimination (Becker 1971 [1957]) occurs if agents gain direct benefits from their discrimination. Regarding housing markets, for example, members of a certain ethnic group may simply not want to live with members from other ethnic origins. This corresponds to the classic homophily or in-group/out-group hypothesis (see Krysan et al. 2009; McPherson et al. 2001). Expressed more precisely, this kind of discrimination can occur because people are ethnocentric and/or have racist preferences. Statistical discrimination, on the other hand, is caused by the abovementioned lack of information when actors face decisions under uncertainty. When confronted with decisions about a (future) residence or neighborhood, people lack information about the future residential satisfaction and residential quality that their decision will bring about. In such situations, actors draw on external characteristics (e.g., race, religious belonging, or the social origin of other residents living in a neighborhood or borough) that serve as proxies for the missing information (Arrow 1973; Phelps 1972). Hence, when searching for an apartment in housing markets, external features such as the ethnic or social composition of a neighborhood are used as proxy variables for the quality of a residence or for the general living quality a residence entails. These external characteristics can be based on prejudice or on group-specific averages that are empirically applicable. One implication of this statistical discrimination mechanism is that the initial amount of discrimination will diminish if the amount of missing information on the target variable (the general living quality in the case of residential search and choice) is reduced, and/or if other information or proxy variables (signals) are available that are (positively) related to the target variable.

The issue of discriminatory residential preferences has been addressed by a number of studies, mainly from the USA. One fundamental insight of this literature in the first place is that focusing on *actual* housing, moving, or neighborhood choices of residents and on the actual characteristics of residential areas as dependent and independent variables (e.g., Aldén et al. 2015; Bader and Krysan 2015; Bobo and Zubrinsky 1996; Harris 1999, 2001; Krysan and Bader 2007; Myers 2004) is misleading for establishing causal effects. This is because independent variables (e.g., the racial composition, the average social status, or the crime rate of a neighborhood or borough) correlate greatly, together with a lack of variance, which does not allow the partial effects of taste-based and statistical discrimination to be separated. In fact, in the USA, for instance, there are simply no 100% Black neighborhoods with exclusively high-status inhabitants. This “fundamental research design problem” (St. John and Bates 1990, p. 49) has led to the consensus that standard methods building on data about actual residential characteristics are not suited to studying segregation-related (determinants of) residential preferences (see also Emerson et al. 2001). A more appropriate approach are FS experiments (or vignette designs, Auspurg and Hinz 2015) that allow the causal effects of the variables under interest to be extracted by modeling the full and uncorrelated combinations of residential and neighborhood characteristics. FS experiments unify the advantages of survey studies (large “representative” samples offering a high external validity in the sense of generalizability) with the benefits from experimental designs that allow the identification of causal effects and high internal validity (Auspurg and Hinz 2015).

Several studies have investigated segregational preferences using this approach. The US-American literature more or less exclusively focuses on racial segregation; here, taste-based vs. statistical discrimination is often studied under the terms “pure race hypothesis” and “racial proxy hypothesis.” In these studies, vignettes containing variables on the racial composition of neighborhoods and additional positively connoted characteristics (such as a high average social status, a low crime rate, or rising housing prices) are presented to respondents. The findings (Emerson et al. 2001; Krysan et al. 2009; Lewis et al. 2011; St. John and Bates 1990) more or less unequivocally show that, after controlling for these (selected) positively connoted characteristics, a “pure race” or taste-based component remains (usually more pronounced for white respondents than for Blacks, Latinos, and Asians). This is interpreted as being in favor of the “pure race hypothesis.” What this literature does not address, however, is an account of how much of the total race effect can be attributed to taste-based (pure race effect) or to statistical discrimination (racial proxy effect), because step-wise model-building is not carried out and interaction effects between vignette dimensions are not estimated.

We are aware of only four studies looking at interaction terms within FS designs. Brüggemann (2020, unpublished), as in our study, uses a population survey in the City of Konstanz, Germany (carried out in 2012). Similar to the abovementioned studies, he finds that after controlling for other characteristics of neighborhoods that are positively and negatively related to residential quality, a partial negative “foreigners” effect remains. The study further finds evidence for statistical discrimination by modeling an interaction of the “foreigners” variable with a lack of information regarding vandalism. The “foreigners” effect is stronger if no information about vandalism in the fictitious neighborhood is available. Diehl et al. (2013) analyze the residential preferences of German university students and find some evidence for taste-based discrimination against Turkish students (the effect being substantially low, however). No evidence in favor of statistical discrimination is found, but here the authors suspect that this could be due to flaws in the operationalization. Havekes et al. (2013) do not focus on residences or neighborhoods, but on the evaluation of fictitious inhabitants of neighborhoods. This study (carried out in the Netherlands) does not find main effects of ethnicity, nor is there evidence for statistical discrimination; instead, there is “a lack of consistent significant findings” (Havekes et al. 2013, p. 1088). Finally, in a study carried out in Switzerland, Zangger (2021) finds a negative main effect of “Turkish and Tamil shops” in his vignette design, which is not alleviated if it interacts with other factors relating to residential quality. Hence, there is no evidence in favor of statistical discrimination in this study.

The overall picture presented by the existing literature seems to suggest that taste-based discrimination with respect to segregational residential preferences is present. Nearly all the studies find evidence in this regard, although there is a distinct amount of variation regarding the substantial significance of the effects. Concerning statistical discrimination or the racial proxy hypothesis, the evidence is less clear and mixed. This is also due to different operationalizations and analytical strategies employed in the studies.

## 2.2 The Group-Threat and Contact Hypotheses

When studying residential segregation and discriminatory residential preferences, another strand of theories is relevant. The group-threat and contact hypotheses are common explanatory approaches regarding the general causes of xenophobia or anti-immigrant attitudes and can straightforwardly be applied for shedding further light on the generating mechanisms of discriminatory residential preferences.

The group-threat theory assumes a competition between ethnic groups (Blalock 1967; Quillian 1995); ethnic majorities feel collectively threatened by ethnic minorities (Weins 2011). Hence, xenophobia and anti-immigrant attitudes arise because people feel individually or sociotropically (i.e., referring to society as a whole) threatened by immigration. The literature differentiates between economic threat—employment, wealth, social security, etc.—and cultural threat, which refers to identity-related and cultural aspects (Diehl et al. 2018; Hainmueller and Hopkins 2014). Although the findings from empirical studies on the group-threat hypothesis are mixed owing to measurement issues or the need to distinguish different effects for different subgroups, different sub-dimensions of economic or cultural threats, and self-interest vs. sociotropic interest, the overall picture seems to suggest that the core argument is well established in research on xenophobia. The main hypothesis is that people who feel economically or culturally threatened by migrants in general are more hostile/discriminatory against them with respect to their residential preferences. Hence, this mechanism might be one driver of taste-based discrimination.

The contact hypothesis (Allport 1954) states that more frequent and more intense contacts between ethnic groups reduce prejudice and anti-immigrant attitudes. Numerous studies and a meta-analysis by Pettigrew and Tropp (2006) have clearly empirically confirmed it. A more recent examination by Paluck et al. (2019) confirms the assumption that increased contact generally leads to reduced prejudice, but also sees gaps in Pettigrew and Tropp's meta-analysis: the magnitude of the positive effect of contact differs, Paluck et al. found, according to the group affected by the prejudice. For example, they found that the effect was particularly strong for people with cognitive disabilities and less strong for ethnic or racial prejudice. However, a recent (quasi-experimental) study by Wolter et al. (2020) has shown that the contact hypothesis also works “geographically”; hostile attitudes against refugees and the geographic proximity of a respondent's dwelling to refugee asylums are negatively related, i.e., the nearer the dwelling is to an asylum, the *less* hostile the attitudes. The extension to ethnic residential segregation is straightforward; we can assume that discriminatory residential preferences get lower with increasing contact with immigrants. Regarding the theory of discrimination, this conjecture fits in two ways. First, contact with minorities can be interpreted as a reducer of taste-based discrimination; racism diminishes if one gets to know people from other ethnic groups. Second, and as the theory states, contact reduces prejudice, and this corresponds to the statistical discrimination thesis, according to which an information gain reduces discrimination.



### 3 Research Strategy and Hypotheses

The first part of the empirical analyses of our study will add empirical evidence to the discrimination literature summarized in Sect. 2.1. We are going to use an FS experiment in which respondents are asked to rate example residential settings with respect to their general attractiveness. The main focus is on varying attractiveness ratings depending on whether a large proportion of immigrants live in the neighborhood, and whether a Muslim community is active in the neighborhood. The FS experiment further contains other characteristics supposed to be positively or negatively related to residential attractiveness: the average social status of the inhabitants and whether the streetscape is run-down or well-maintained. The first step of the analysis investigates the degree to which immigrants and Muslims are discriminated against with regard to residential preferences. The second step then attempts to elicit whether the initial discrimination can be attributed to taste-based or to statistical discrimination. This is done by analyzing interaction effects among the vignette dimensions. If taste-based discrimination is the main driver of discriminatory residential preferences, we should observe that discriminatory preferences against immigrants and Muslims do *not* disappear if other positively connoted residential characteristics are present. If, on the contrary, statistical discrimination plays a role, we should find interaction effects of the form that (potentially) discriminatory effects against minorities do—at least partly—disappear if other positively connoted attributes exist. These hypotheses are summed up in the first part of Table 1.

The second part of the empirical analyses tests the conjectures derived from the group-threat and contact hypotheses (see Sect. 2.2). This is done by including respondent-level variables of the vignette dataset in the analysis and by investigating

**Table 1** Hypothesized Effects on Attractiveness Ratings of Vignette Examples

Independent Term	Hypothesized Effect
<b>Step 1: Main effects of vignette variables: is there discrimination?</b>	
Many foreigners	–
Muslim community	–
<b>Step 2: Vignette-variable interactions: taste-based or statistical discrimination (assuming negative effects in Step 1)?</b>	
Many foreigners × Other positive characteristics	0 if taste-based discrimination + if statistical discrimination
Muslim community × Other positive characteristics	0 if taste-based discrimination + if statistical discrimination
<b>Step 3: Cross-level interactions: group-threat and contact hypotheses?</b>	
Many foreigners × <i>Economic group-threat</i>	–
Many foreigners × <i>Cultural group-threat (religiosity)</i>	–
Many foreigners × <i>Contact with migrants</i>	+
Muslim community × <i>Economic group-threat</i>	–
Muslim community × <i>Cultural group-threat (religiosity)</i>	–
Muslim community × <i>Contact with migrants</i>	+

*Italic* variables are respondent-level characteristics; the others are vignette-level effects. “–” denotes a negative effect, “+” a positive effect, and “0” no effect

their interaction effects with the vignette dimensions (residential characteristics). We will use an established index of group-threat-related anti-immigrant attitudes as an indicator for an economic group-threat. A respondent's religiosity serves as a proxy indicator for cultural group-threat following the argument that religion can be a driver in feelings of cultural threat (Bloom et al. 2015; Sökefeld 2004; Xia 2022). Allport (1966, p. 447) stated in this respect that "it is a well-established fact in social science that, on the average, churchgoers in our country harbor more racial, ethnic, and religious prejudice than do non-churchgoers." Although Allport is referring to the US context with this statement, more recent studies in the European context found mixed results. Doebler (2014) describes a negative influence of religiosity on the acceptance of Muslim fellow citizens, whereas Ribberink et al. (2017) see the strongest anti-Muslim attitudes in European countries that are the most advanced in terms of secularization. Bloom et al. (2015) find positive and negative effects of religiosity on anti-immigrant attitudes depending on whether religious identity or religious belief are used as measures. This mixed evidence makes it all the more worthwhile investigating the religiosity effect on discriminatory preferences.<sup>2</sup> The core hypothesis, then, is that taste-based residential discrimination (more concretely, the presumed negative effect of many migrants and a Muslim community in the neighborhood) varies by the level of perceived group-threat and religiosity. To test the contact hypothesis, we estimate the interaction between a respondent's contact with migrants in real life and the vignette variables: more contact should reduce discriminatory effects against migrants and Muslims. "Step 3" in Table 1 sums up the hypotheses of this part of the empirical analyses.

#### 4 Study Design, Data, and Methods

The data stem from the autumn 2020 wave of a yearly local panel survey among the adult population of the City of Konstanz, Germany. The sample is an offline recruited stratified random sample of the Konstanz municipal population register. All analyses use post-stratification weights adjusting the analysis sample to the distribution in the population with respect to gender, age, city borough, and nationality. The survey was carried out online; the response rate amounts to 37% (for more details regarding the survey, see Spanner et al. 2021).

The questionnaire (besides questions regarding local concerns, the impact of the COVID-19 pandemic, and standard demographics) contained a module devoted to questions about residential and housing issues. The core of this module consists of an FS (vignette) experiment in order to measure residential preferences, or—more concretely—to estimate the effects of different residential characteristics on the overall attractiveness rating of example residential settings. Because these characteristics (the independent vignette variables) are uncorrelated by design in FS experiments,

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<sup>2</sup> Although the religious affiliation of the respondents would also be of importance in this analysis, our data unfortunately do not provide a sufficient number of respondents that are non-Christian to use this variable. Future studies, however, should investigate this further, i.e., clarify whether different indicators of religiosity (e.g., affiliation and religious service attendance) have different effects.

Residence example 123									
This residential location would <i>not change your current financial housing costs</i> (rent, utilities, interest, loan, maintenance costs). There are <i>many foreigners</i> living in the residential area. There is also <i>an active Muslim community</i> there. Furthermore, it is known that <i>many poor people</i> live there. Looking around the neighborhood, you can see that the streetscape looks <i>rather run-down and untidy</i> . An environmental tax for <i>global climate protection projects</i> is <i>financed differently for this residential area and is not payable</i> .									
In general terms, how attractive do you personally find this residence overall?									
Very unattractive									Very attractive
1	2	3	4	5	6	7	8	9	10

**Fig. 1** Example Vignette. (Vignette dimensions that were varied are depicted in italics)

their effects can be estimated independently of each other and assessed with regard to their relative impact. The vignette method has the further advantage that respondents have to make a trade-off between several characteristics, which avoids social desirability bias and ex-post rationalizations (Auspurg and Hinz 2015). Figure 1 shows an example vignette.

All vignette dimensions and levels are depicted in Table 2. The vignette universe adds up to 2592 different vignettes, from which a D-efficient ( $D=96.4$ ) vignette

**Table 2** Design of the Vignette Experiment

#	Dimension	Levels
1	Monthly housing costs	No change Minus 10% Minus 20% Minus 30%
2	Neighborhood composition	Almost only Germans Many foreigners Many elderly people Many students
3	Religious community in the neighborhood	No religious community present Active Christian community Active Muslim community
4	Average social status in the neighborhood	Many rich and wealthy people Mainly average earners Many poor people
5	Streetscape in the neighborhood	Rather run-down and untidy Nothing remarkable Above-average, clean and well maintained
6	Target of environmental tax	Local green space Global climate protection projects
7	Monthly costs of environmental tax	Zero (otherwise funded) 1 € per m <sup>2</sup> habitable surface per year 2 € per m <sup>2</sup> habitable surface per year

sample of 252 vignettes, blocked into 36 decks, was drawn using conventional methods as proposed by Kuhfeld (2010) and assuring that all vignette dimensions and also all second-order interactions are uncorrelated. The latter aspect is crucial for studying interactions between vignette dimensions and has been neglected in many studies (Auspurg 2018). No (potentially) implausible vignette combinations were excluded. Each respondent answered seven vignettes. The order of the vignettes within decks was randomized. The analysis in this article focuses on ethnic and religious neighborhood composition (“many foreigners” and an “active Muslim community”), the average social status in the fictitious neighborhood, and streetscape.<sup>3</sup>

Apart from the vignette variables, further respondent-level items will enter the analysis; they are reported in Table 3.<sup>4</sup> Besides standard demographics and controls, the core variables according to our hypotheses are an index variable of perceived economic group-threat toward migrants in general, religiosity, and (real-life) contact with migrants in the actual neighborhood in which respondents live.

The resulting dataset has a multilevel structure, with vignette variables at the lower level and respondent characteristics at the higher level. The number of cases is  $N = 1159$  respondents and  $N = 8113$  vignette cases; owing to item nonresponse, however, some cases drop out of the analysis. The data are analyzed using conven-

**Table 3** Description of Variables

Variable	Remarks/coding
<i>Dependent vignette variable:</i>	
Attractiveness of vignette residence	Ten-point scale from 1 = very unattractive to 10 = very attractive
<i>Independent respondent-level variables</i>	
Perceived economic group-threat	Mean index coded from four items, with 0 = low to 6 = high level of group-threat
Religiosity	Seven-point scale from 0 = not religious at all to 6 = very religious
Contact with migrants in the neighborhood	Respondent estimate of the portion of foreigners living in the neighborhood, four-point scale from 0 = very low to 3 = very high
Homeownership	1 = yes, 0 = no
Migration background	Not born in Germany or at least one parent not born in Germany, 1 = yes, 0 = no
Gender female	1 = yes, 0 = no
Age	In decades [1.7 ... 9.0]
Education	In years

For the (independent) vignette variables, see Table 1. Question wording is documented in the Online Appendix

<sup>3</sup> The remaining vignette dimensions (monthly housing costs, an imaginary environmental tax and its monthly amount) were included in the design for a separate research question on willingness to pay for public environmental goods; this has nothing to do with the present paper. Also note that a ten-point answer scale without a middle category was used (there were no special reasons for this). This might cause respondents' answers to be slightly biased to the left if they erroneously interpret “5” as the midpoint of the scale.

<sup>4</sup> A descriptive analysis of the variables is documented in Fig. A1 and Table A4 in the Online Appendix, as well as results from exploratory and confirmatory factor analysis (Table A5a–A5c) of the economic group-threat index (showing clear unidimensionality of the items).

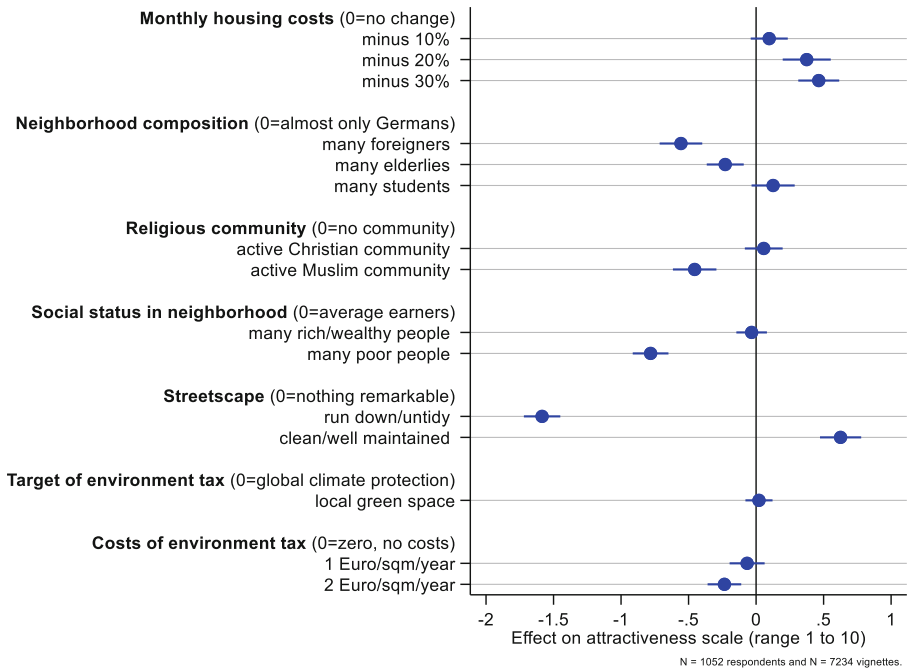
tional linear mixed models for multilevel data (Hox 2010). Our analysis proceeds in three steps.

1. First, a baseline model presents the main effects of the vignette variables. This permits investigating whether, and to what degree, discriminatory residential preferences with respect to migrants in general and Muslims in particular exist.
2. Second, we investigate two-way interaction effects between vignette dimensions. Here, we examine if effects of statistical discrimination occur, i.e., whether positively connoted characteristics of the residence mitigate the assumed negative effects regarding migrants and Muslims. Concretely, we model the interactions between the average social status in the neighborhood, streetscape, and the two migrant-related variables.
3. Third, models containing two-way cross-level interaction effects between vignette dimensions and respondent characteristics examine the degree to which discriminatory residential preferences are shaped by respondent variables, namely economic group-threat, religiosity, and contact with migrants in real life.

In building the models, we follow the advice by Hox (2010) and proceed cautiously and step by step, because multilevel models with many (cross-level) interaction effects tend to be rapidly over-fitted, resulting in estimation problems.

## 5 Results: Statistical Discrimination or Taste for Discrimination?

The first step in the analysis investigates the degree to which there is discrimination against migrants in general and Muslims in particular with respect to residential preferences. Figure 2 depicts the effects of the residential attributes figuring in the vignette experiment on the overall attractiveness rating. One can see that, if “many foreigners” live in the neighborhood, the attractiveness rating goes down by about 0.6 points compared with the reference category (“almost only Germans”). A similar effect can be observed for an “active Muslim community,” which has a negative effect of about  $-0.5$  points. A side note is that poor people are discriminated against as well, with an effect of  $-0.8$  compared with average earners. The largest effect relates to streetscape. Compared with the reference middle category, a “rather run-down and untidy” streetscape in the neighborhood causes ratings to go down by 1.6 points; a “clean and well-maintained” streetscape is associated with a 0.6-point higher attractiveness rating. The effects of the remaining vignette dimensions are not of primary interest here but, interestingly, the “monthly housing costs” variable has effects that are smaller than or within the same range as our discrimination effects. For instance, a 30% reduction of monthly costs has a slightly smaller impact on residential preference than living with many foreigners in the same neighborhood. If we recode the housing costs variable into a metric one and estimate the cross-elasticity (willingness to pay) for the migrant effect (not documented), we arrive at an estimate of  $-3\%$ , meaning that the monthly housing costs people are ready to



**Fig. 2** Attractiveness Rating (Residential Preference) in Dependence of Residential Attributes (Vignette Dimensions). (Linear multilevel regression, dependent variable: attractiveness rating of example [vignette] residence. Unstandardized regression coefficients and 95% CI [robust standard errors]. See Table A6 in the Online Appendix for an extended table with more information)

pay if many migrants live in their neighborhood have to be 33% lower in order to yield the same attractiveness rating.<sup>5</sup>

Summing up, our first result is that we do indeed observe discriminatory residential preferences against migrants and Muslims. The next analysis step is to examine whether this can be attributed to taste-based or to statistical discrimination. If the negative migrant and Muslim effects are taste-based discrimination, there should be no change in the effects if other positively connoted residential attributes exist. If there is statistical discrimination, the negative effects should mitigate. We first have a look at the “many foreigners effect”; Table 4 displays the main effects and two-way interactions between this covariate and the social status and streetscape variables respectively. For ease of interpretation, we estimate separate models, depicted as “Model 1” and “Model 2” and illustrate the main results graphically in Fig. 3a,

<sup>5</sup> If we do the same exercise for the “run down/untidy streetscape”, the estimated cross-elasticity is  $-94\%$ . Bearing in mind that the City of Konstanz is a very neat and picturesque city at the border of the Lake Constance, this means that, for the average Konstanz citizen, a residence in an untidy neighborhood has only half the value of a residence in an averagely maintained surrounding.

<sup>6</sup> Note that the usual approach to control for the respective other effect is not necessary here because all vignette variables and all two-way interactions are uncorrelated by design. Robustness checks have also

**Table 4** Two-Way Vignette Interactions with “Many Foreigners” Effect

	Model 1		Model 2	
	<i>b</i>	<i>p</i> value	<i>b</i>	<i>p</i> value
Neighborhood composition (0=almost only Germans)				
Many foreigners	-0.674	0.000	-0.685	0.000
Many elderly people	-0.232	0.001	-0.231	0.001
Many students	0.128	0.117	0.129	0.114
Religious community in the neighborhood (0= no religious community)				
Active Christian community	0.057	0.424	0.057	0.428
Active Muslim community	-0.457	0.000	-0.454	0.000
Average social status in the neighborhood (0= mainly average earners)				
Many rich and wealthy people	-0.118	0.093	-0.033	0.562
Many poor people	-0.788	0.000	-0.783	0.000
Streetscape in the neighborhood (0= nothing remarkable)				
Rather run-down and untidy	-1.583	0.000	-1.606	0.000
Above-average, clean and well-maintained	0.625	0.000	0.551	0.000
<i>Two-way vignette interactions</i>				
Many foreigners × Rich/wealthy	0.348	0.031	–	–
Many foreigners × Poor	0.015	0.918	–	–
Many foreigners × Run-down/untidy	–	–	0.088	0.571
Many foreigners × Clean/well-maintained	–	–	0.304	0.047
Constant	5.984	0.000	5.979	0.000

Linear multilevel regression, dependent variable: attractiveness rating of example (vignette) residence. Unstandardized regression coefficients. Not all effects included in the model are reported; see Tables A7 and A8 in the Online Appendix for the full regression tables.  $N(\text{Respondents})=1052$ ;  $N(\text{Vignettes})=7234$

which shows the marginal effects of the “many foreigners” variable conditional on social status and streetscape.<sup>6</sup>

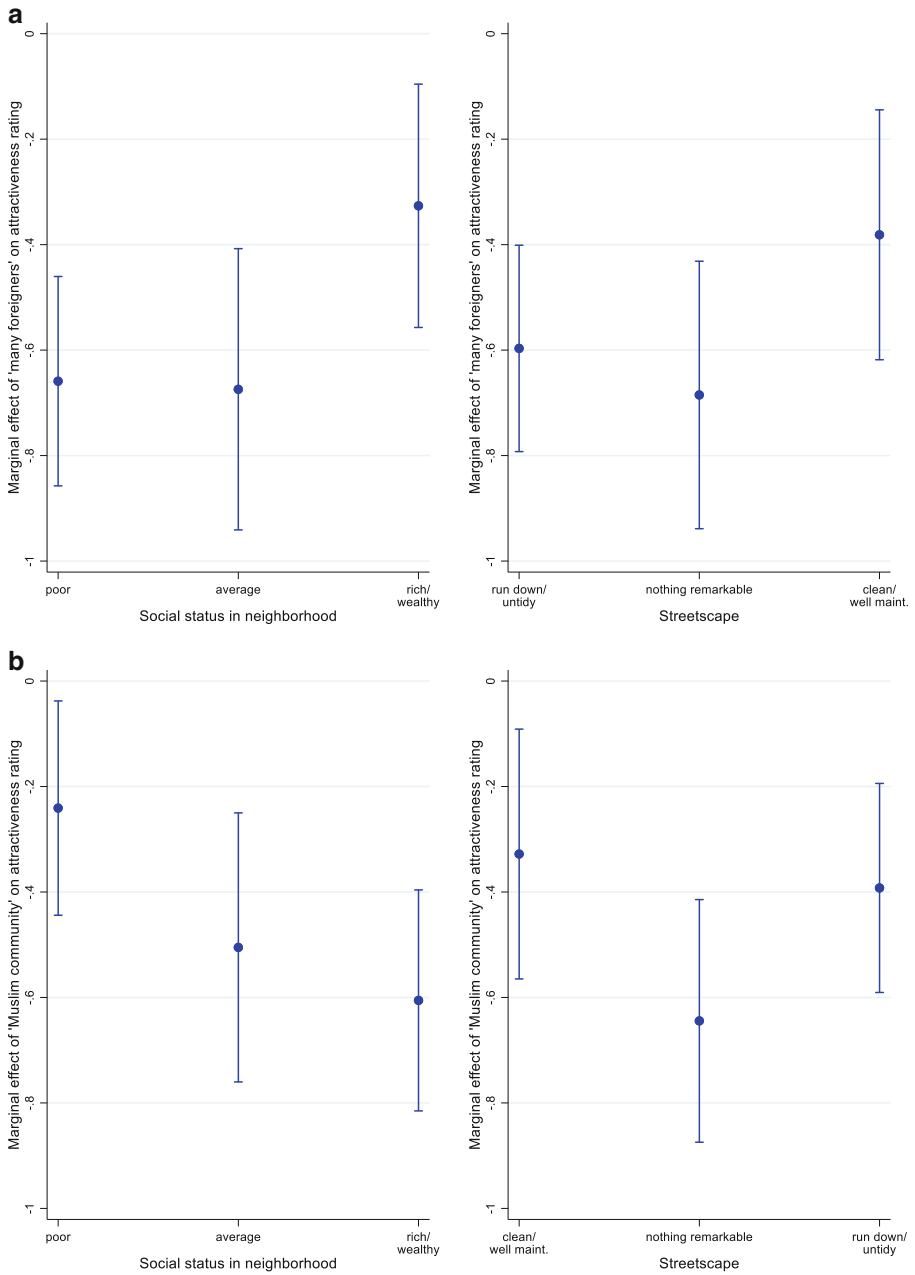
As the theory predicts, we see an effect of statistical discrimination. If “many foreigners” go along with a neighborhood that is “rich and wealthy,” about one-half of the negative main effect vanishes (as indicated by the significant positive interaction effect). Similarly, the interaction between a “clean and well-maintained neighborhood” and “many foreigners” in the neighborhood is also positively significant. Hence, the negative assessment of many migrants in the residential neighborhood by the respondents is mitigated if other positively connoted aspects of the neighborhood reduce lack of information regarding whether the residential constellation entails a good quality of living—we take this as an indication for statistical discrimination.<sup>7</sup>

Table 5 and Fig. 3b report the same interaction analysis for the “Muslim community” effect. Here, the results are not as clear-cut. First, there is a (marginally signif-

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shown that a full model containing all interaction effects at once (including those for “Muslim community”) yields the same results (not documented).

<sup>7</sup> One could object here that our design did not vary the amount of lack of information, for example, by omitting the variables on social status and streetscape and by assessing whether the migrant effect is stronger if no additional information is available. This would have been closer to the theoretical argument. In the discussion section, we elaborate on this in more detail.



**Fig. 3** **a** Marginal Effects of “Many Foreigners” Conditional on Social Status and Streetscape. (Predictions derived from regression models in Table 4. 95% CI). **b** Marginal Effects of “Muslim Community” Conditional on Social Status and Streetscape. (Predictions derived from regression models in Table 5. 95% CI)



**Table 5** Two-Way Vignette Interactions with the “Muslim Community” Effect

	Model 1		Model 2	
	<i>b</i>	<i>p</i> value	<i>b</i>	<i>p</i> value
Neighborhood composition (0=almost only Germans)				
Many foreigners	-0.561	0.000	-0.561	0.000
Many elderly people	-0.237	0.001	-0.231	0.001
Many students	0.121	0.136	0.120	0.139
Religious community in the neighborhood (0= no religious community)				
Active Christian community	0.061	0.390	0.058	0.416
Active Muslim community	-0.505	0.000	-0.644	0.000
Average social status in the neighborhood (0= mainly average earners)				
Many rich and wealthy people	-0.004	0.958	-0.037	0.525
Many poor people	-0.868	0.000	-0.790	0.000
Streetscape in the neighborhood (0= nothing remarkable)				
Rather run-down and untidy	-1.587	0.000	-1.666	0.000
Above-average clean and well-maintained	0.613	0.000	0.520	0.000
<i>Two-way vignette interactions</i>				
Muslim community × Rich/wealthy	-0.100	0.492	–	–
Muslim community × Poor	0.264	0.060	–	–
Muslim community × Run-down/untidy	–	–	0.252	0.040
Muslim community × Clean/well-maintained	–	–	0.316	0.033
Constant	5.981	0.000	6.021	0.000

Linear multilevel regression, dependent variable: attractiveness rating of example (vignette) residence. Unstandardized regression coefficients. Not all effects included in the model are reported; see Tables A9 and A10 in the Online Appendix for the full regression tables.  $N(\text{Respondents})=1052$ ;  $N(\text{Vignettes})=7234$

icant) positive interaction between “Muslim community” and a poor neighborhood. This means that a Muslim community is rated more favorably if the neighborhood is poor than if average earners live in it. Contrary to what we found above with respect to the “many foreigners” interaction, a wealthy neighborhood does not alleviate the negative main effect of the “Muslim community,” so here the anticipated effect of statistical discrimination is not present. The second interaction with streetscape does not show a clear picture either. Counterintuitively, we find a u-shaped relationship—the negative effect of a Muslim community is mitigated both by a run-down and untidy streetscape and by a clean and well-maintained streetscape.<sup>8</sup> Our interpretation regarding these findings is twofold. On the one hand, it seems that the underlying discriminatory mechanisms for foreigners (in general) and for the religious minority of Muslims are different. Hence, ethnic residential segregation is something different than discrimination vis-à-vis the religious minority of Muslims; but we concede that we do not have a theoretical explanation for this unexpected ef-

<sup>8</sup> We performed several robustness checks regarding these findings. Modeling all possible interactions, including the “Christian community” covariate (see Table 2), and accounting for possibly censored responses by estimating a tobit model (as proposed by Auspurg and Hinz 2015) did not yield different results.

fect. On the other hand, we cannot rule out that there might be methodological issues that bias the results. For instance, some combinations of vignette levels might have been too unrealistic, causing respondents to get confused. Also, although robustness checks using tobit regression did not yield different results, censored responses could be an issue here.

We now turn to the third step of the analysis and investigate effects of respondent-level variables. The intra-class correlation is 26%, which indicates that, overall, 26% of the variance of the dependent variable is between-respondent variance. A check for random slopes reveals that both vignette dimensions “many foreigners in the neighborhood” and “active Muslim community in the neighborhood” have varying effects across respondents (not documented). We then analyze cross-level interactions of these two vignette variables with perceived economic group-threat, respondents’ religiosity as an indicator for perceived cultural group-threat, and contact with migrants in the neighborhoods where the respondents actually live. We further add basic socio-demographic controls to the models. Because multilevel models with many cross-level interactions tend to become over-fitted and not estimable, we proceed step by step and only integrate one interaction at a time in the models.<sup>9</sup> Table 6 shows the results. For an easier interpretation, the predicted values of the six interaction effects are plotted in Fig. 4a–f.

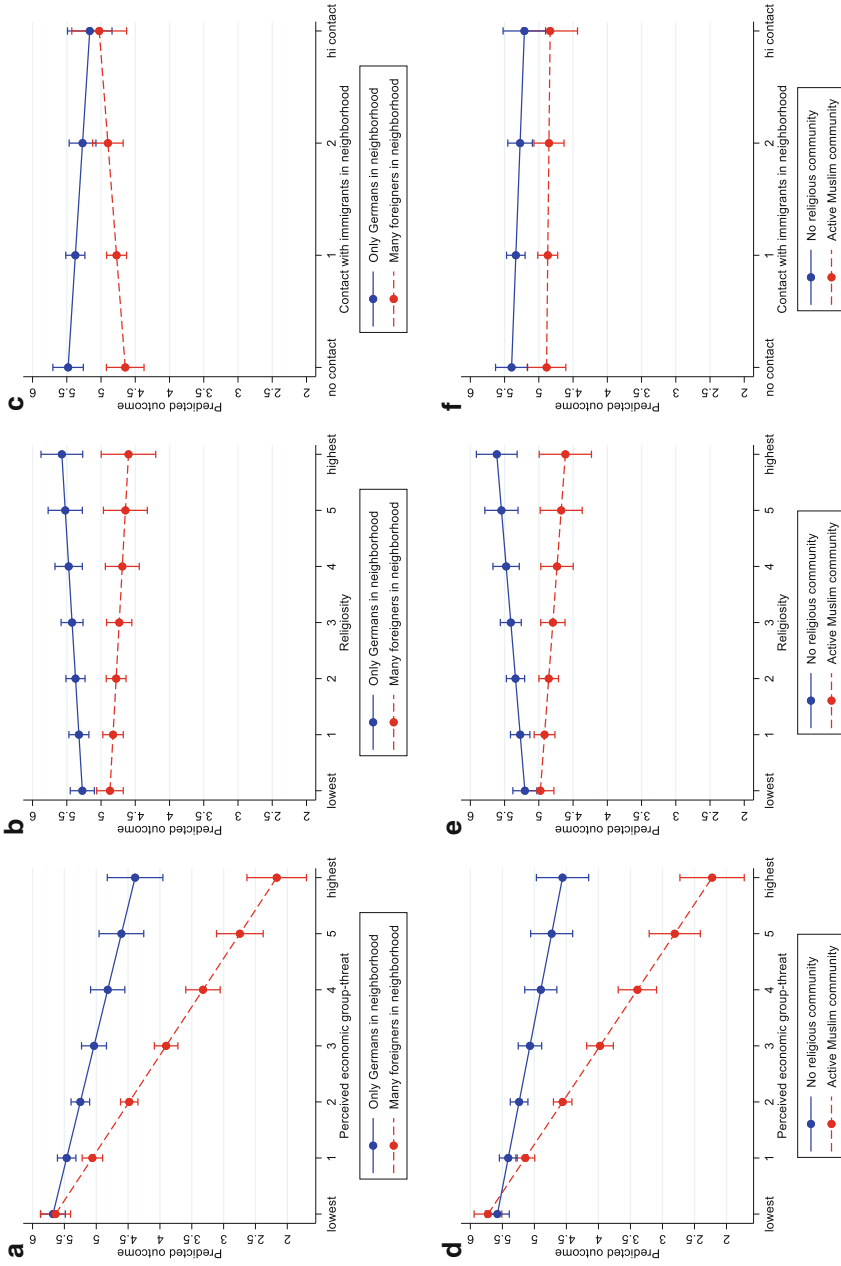
In Model a (Fig. 4a), we see that there is no “foreigners in the neighborhood” main effect if perceived economic group-threat is at the lowest level (0; i.e., the interaction term cancels out of the regression equation). However, for every point on the seven-point scale of group-threat, the foreigner effect gets larger by  $-0.36$  points. This is an indication of what might explain (part of) the discriminatory effect toward migrants found above in Fig. 2: a general perceived economic group-threat. A side note is that the main effect of this variable is also negative. This means that, regardless of all other variables in the model, respondents tend to rate the example vignettes as less attractive with growing group-threat. To clarify this effect is beyond the scope and hypotheses of this paper; one could conjecture (but this admittedly is speculation) that people scoring high on the group-threat index also have fears or reservations toward other issues or people, are more anti-social, and hence look more negatively at things in general. Model b (Fig. 4b) tells us that, regardless of the level of religiosity, foreigners are rated more negatively than if only Germans were living in the neighborhood. Interestingly, however, this gap widens with growing religiosity, so more religious people tend to be more discriminatory in this regard than less religious ones. We take this as evidence in favor of the cultural group-threat hypothesis (if one accepts that religiosity is a proxy variable for cultural group-threat). Model c (Fig. 4c) shows evidence in favor of the contact hypothesis (Allport 1954; Wolter et al. 2020). The main effect of  $-0.83$  for “foreigners in the neighborhood” holds for respondents actually having no or very few migrants among their neighbors. The cross-level interaction is statistically significant, meaning that the more people come into contact with migrants in their neighborhood, the less

<sup>9</sup> As a side note, we do not find significant interaction effects for homeownership (vs. renting) and a migration background (not documented). For robustness analysis, a full model containing all cross-level interactions at once can be found in the Online Appendix (Table A13).

**Table 6** Cross-Level Interactions: Attractiveness Rating (Residential Preference) in the Dependence of Perceived Group-Threat and Real-Life Contact with Migrants

	Model a		Model b		Model c		Model d		Model e		Model f	
	b	p value	b	p value	b	p value	b	p value	b	p value	b	p value
<i>Main effects: vignette variables</i>												
Many foreigners in the neighborhood	-0.040	(0.757)	-0.403	(0.000)	-0.833	(0.000)	-0.551	(0.000)	-0.557	(0.000)	-0.559	(0.000)
Active Muslim community	-0.458	(0.000)	-0.467	(0.000)	-0.464	(0.000)	0.148	(0.173)	-0.227	(0.047)	-0.511	(0.001)
Many rich/wealthy people	-0.035	(0.554)	-0.042	(0.476)	-0.048	(0.423)	-0.034	(0.560)	-0.028	(0.633)	-0.027	(0.646)
Many poor people	-0.784	(0.000)	-0.791	(0.000)	-0.794	(0.000)	-0.793	(0.000)	-0.791	(0.000)	-0.790	(0.000)
Run-down/untidy streetscape	-1.621	(0.000)	-1.623	(0.000)	-1.618	(0.000)	-1.611	(0.000)	-1.616	(0.000)	-1.618	(0.000)
Clean/well-maintained streetscape	0.637	(0.000)	0.636	(0.000)	0.636	(0.000)	0.643	(0.000)	0.632	(0.000)	0.630	(0.000)
<i>Main effects: respondent variables</i>												
Perceived economic group-threat	-0.215	(0.000)	-0.289	(0.000)	-0.289	(0.000)	-0.170	(0.000)	-0.268	(0.000)	-0.266	(0.000)
Religiosity	0.029	(0.345)	0.050	(0.123)	0.031	(0.318)	0.037	(0.225)	0.068	(0.035)	0.038	(0.212)
Contact with migrants in the neighborhood	-0.056	(0.449)	-0.059	(0.428)	-0.105	(0.187)	-0.053	(0.480)	-0.051	(0.496)	-0.062	(0.441)
<i>Cross-level interactions:</i>												
Many foreigners×Economic group-threat	-0.364	(0.000)	-	-	-	-	-	-	-	-	-	-
Many foreigners×Religiosity	-	-	-0.095	(0.021)	-	-	-	-	-	-	-	-
Many foreigners×Contact with migrants	-	-	-	-	0.232	(0.021)	-	-	-	-	-	-
Muslim community×Economic group-threat	-	-	-	-	-	-	-0.414	(0.000)	-	-	-	-
Muslim community×Religiosity	-	-	-	-	-	-	-	-	-0.129	(0.001)	-	-
Muslim community×Contact with migrants	-	-	-	-	-	-	-	-	-	-	0.045	(0.666)
N (vignette cases)	6794	-	6794	-	6794	-	6794	-	6794	-	6794	-
N (respondents)	982	-	982	-	982	-	982	-	982	-	982	-

Also included but not reported are the effects of the remaining vignette variables, homeownership, migration background, gender, age, education, and the regression constant. Linear multilevel regression, dependent variable: attractiveness rating of example (vignette) residence. Unstandardized regression coefficients. See Tables A11 and A12 in the Online Appendix for full regression tables



**Fig. 4** Conditional-Effects Plots of Cross-Level Interactions from Regression Models in Table 6: Predicted Attractiveness Ratings

negatively they assess the vignette variable on foreigners. One weakness that should be mentioned at this point is the ambiguity about the causal direction of the effect. Although we assume here that living in an area with many migrants leads to increased contact and thus reduces prejudice, it is equally conceivable that people with less prejudice are more likely to move to areas characterized by a high proportion of residents with a migration background. We are aware of this issue and see the need for further analyses on the causal direction of this effect.

Model d (Fig. 4d) is basically the same finding as Model a, but for an “active Muslim community” in the neighborhood. With a growing generally perceived economic group-threat from migrants (!), the religious minority of Muslims is more discriminated. Model e (Fig. 4e) shows the same result as Model c, but more distinctively: the more religious people are, the more negatively they assess the presence of a Muslim community in their neighborhood. Again, this finding points to a cultural group-threat mechanism. The remaining cross-level interaction (Model f, Fig. 4f) is not statistically different from zero. This means that, contrary to what we found with respect to migrants in general, real-life contacts to migrants do not alleviate the “Muslim community” effect. As we have already argued above, this in our view means that discrimination against migrants in general is something different than discrimination against the religious minority of Muslims.<sup>10</sup>

## 6 Discussion

The present article investigated the degree to which residential segregation is due to factors associated with the demand side of housing markets, i.e., segregational and/or discriminatory preferences and residential choices of the residents themselves. Against the backdrop of a lack of studies focusing on the demand side of housing markets, mixed evidence in the literature on the role of taste-based and statistical discrimination with respect to segregational/discriminatory residential preferences, and there being almost no empirical studies for Germany (using state-of-the-art methods), we presented findings from an FS experiment carried out in a population survey among citizens of the City of Konstanz, Germany. The results show, first, that migrants and a Muslim community in the neighborhood have negative effects on a respondent’s attractiveness rating of residence/neighborhood constellations. Hence, people do hold discriminatory residential preferences against minority groups. Second, roughly one-half of the negative migrant effect can be attributed to statistical discrimination, i.e., it is alleviated if other positively connoted residential characteristics exist (higher social status of the neighborhood and a clean and well-maintained streetscape). This does not hold for the “Muslim community” effect, for which the results are somewhat erratic. Third, we find evidence in favor of the group-threat and contact hypotheses. The discriminatory effects against migrants and

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<sup>10</sup> More detailed statistics on the explained variance of the multilevel models are reported in the Online Appendix (Tables A7 and A8). The respective findings show that the (significant) cross-level interactions can account for some parts of heterogeneity in the “many foreigners” and “Muslim community” effects (from 5 to 37%), yet, it is clear that there are other unobserved and unaccounted factors at play.

Muslims get stronger with higher levels of perceived economic group-threat from migrants, religious people are more discriminatory than nonreligious people, and people actually living in residential areas with higher fractions of migrants express less discriminatory preferences than those with no contact with migrants in real life.

Our findings fit with, and expand on, what has been found in the existing literature. They confirm earlier findings that, net of other residential characteristics that are negatively or positively related to residential quality, a taste-based component of discriminatory residential preferences remains. Put differently, there is taste-based discrimination *and* statistical discrimination. However, only very few studies have investigated statistical discrimination by modeling interaction effects (which yield mixed results). Our findings show that this approach yields useful insights and should be taken as a basis for conducting further studies. They can also be interpreted such that basically, the generating mechanisms of segregation found in studies on racial segregation in the USA also apply to residential discrimination against migrants and (less clearly) against Muslims in Germany. Finally, the study confirms that ethnic residential segregation can be ascribed to factors on the demand side of housing markets, aside from factors on the supply side. The conclusion, therefore, is that even if discriminatory mechanisms on the supply side were to be eliminated by anti-discrimination laws, for instance, the causes on the demand side would remain, which in turn means that, in our view, ethnic residential segregation will continue to exist.

Naturally, there are several limitations of our study and blind spots that should be addressed in further empirical work. A first objection concerns our setup for testing for statistical discrimination. Properly, the theory states that statistical discrimination only occurs if there is a lack of information. In labor market economics, the theory assumes that—taste-based discrimination aside—(only) if employers are unsure about the productivity of an applicant, they revert to proxy variables such as ethnic origin. This entails that testing for statistical discrimination requires modeling of the interaction between the amount of information on some target variable (productivity in labor economics, residential quality, and satisfaction in the case of residential choice) and proxy variables. This was not implemented in our design. However, we pointed out in the theory section that housing markets are different from simple neoclassical markets in that a decision for a house, residence setting, or neighborhood *always* entails a lack of information regarding future wellbeing. This view is generally confirmed by our results providing evidence in favor of statistical discrimination without modeling a varying lack of information. We thus suppose that adding a variable modeling the latter would perhaps yield a significant interaction “on top” of what we found in this study. The degree to which this test strategy reflects reality, however, is debatable, simply because no residential decisions can be made without a lack of information. Nevertheless, we propose to investigate this issue in further studies. Another limitation is that our vignette variable “many foreigners in the neighborhood” might be too vague and should be more precise, for example, by differentiating between ethnic groups or countries of origin: a Turkish neighborhood is probably perceived differently than a neighborhood with many Western European migrant workers from the EU or from Switzerland (Konstanz is located directly on the Swiss border). This extension could easily be carried out in future studies.

Future work, of course, should also be based on larger populations and should not be limited to one city. Moreover, although vignettes offer a valuable alternative to traditional surveys, nonetheless, the transferability of the statements in such experiments to real life and thus behavioral validity remains debatable (Barabas and Jerit 2010). What we have also not discussed is the distinction between in-group and out-group mechanisms, i.e., the question whether the desire to live among people like oneself or the desire to avoid out-groups is more important (Lewis et al. 2011). This, however, is probably a different story for countries like Germany compared with racial segregation in the USA, which has been the main focus of the existing literature. Another pathway for future research concerning group-threat is crime-related threat, which could be an additional explanatory factor for discriminatory or segregational residential preferences (e.g., Meyerhoffer 2015). In fact, one of the items that we used for our economic group-threat index alludes to crimes of immigrants (see Online Appendix), but we did not find any other effects for this single item compared with the other ones on genuine economic group-threat (not documented). Further, future work should discuss more extensively the relationship between (and the operationalization of) cultural group-threat, religiosity, and anti-immigrant discriminatory preferences. Two remaining limitations relate to the contact hypothesis. First, our measure of contact (proportion of foreigners in the neighborhood) could capture opportunities for contact rather than actual contact. Second, future work should concentrate on the causality direction of the contact hypothesis effect. We assumed that contact with migrants in the actual neighborhood yields fewer segregational preferences, yet the mechanism could also work the other way round. Zangger (2021) found at least weak evidence in this regard and points out that “residential preferences and segregation patterns mutually reinforce each other and should thus be studied together.” We concur. One should also bear in mind that the local context in which this study was conducted is one characterized by *low* levels of actual segregation (see above). If higher actual levels of segregation induce less inter-ethnic contact, which in turn fosters segregational preferences (as found in our study), one could expect a self-reinforcing system.

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