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Price and prejudice: Housing rents reveal racial animus[☆]

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ABSTRACT

We study the evolution of housing rents in the proximity of Swiss asylum seeker hosting centers whose populations differ by ethnic composition. Rental prices within 0.7km of an active center are found on average to be 3.8% lower than in the control group. The price response is significantly more pronounced when centers host a higher share of asylum seekers from sub-Saharan Africa. In contrast, neither religious affiliation nor inferred crime propensity of the center populations are found to affect local rental prices significantly. Our findings are consistent with phenotype-based racial animus as a significant driver of observed market outcomes.

1. Introduction

Xenophobia and discrimination are global social challenges. While voluntary international migration is efficient economically, it may give rise to political backlash, especially among host societies. For example in the United Kingdom, survey respondents systematically named immigration as the country's most important issue in the run-up to the Brexit vote of 2016 (Blinder and Richards, 2016). In the United States, immigration and race relations regularly feature very prominently among problems mentioned in nationwide polls, taking top spot during periods of heightened immigration pressure in 2014, 2018 and 2024 (Jones, 2024).

Reasons for anti-migrant sentiment can be manifold. One factor may be the perception that migrants are competing against natives in labor and housing markets. The resulting distributional conflict can be exacerbated by non-economic animus against people of different nationality or ethnicity. Such prejudice – the object of our paper – has been amply documented through laboratory experiments, field experiments and observational studies.[2]

Yet, not everybody is prejudiced. In a population of heterogeneous types, it is uncertain *a priori* whether and how much prejudice will matter at the aggregate level. Models of labor market discrimination, for example, show that non-prejudiced employers will arbitrage away the biases of prejudiced employers, so that prejudice may not affect the aggregate market outcome (Becker, 1957; Heckman, 1998). In the presence of market frictions, however, arbitrage will be incomplete, and discriminatory preferences will to some extent be reflected in market prices (Black, 1995). Moreover, prejudice has been shown to be a rather weakly held preference. People who express prejudiced opinions in unincentivized surveys or in choices among otherwise equivalent

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[1] This article does not necessarily reflect the view of the Federal Statistical Office.
[2] See Neumark (2018) and Lang and Kahn-Lang Spitzer (2020) for surveys.

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alternatives may not act on their prejudice when discrimination incurs a cost. In an incentivized field experiment, Hedegaard and Tyran (2018) found that the probability of ethnically discriminating falls by 9 percent for every 10 percent rise in the price of doing so. For both those reasons – coexistence of unequally prejudiced agents, and cost sensitivity of discriminatory behavior – real-world market outcomes could conceivably reveal no discrimination even if a nonzero share of market participants hold preferences that are consistent with prejudice in weakly incentivized settings.

Our aim is to measure such an aggregate-level equilibrium outcome in a market with the likely presence of prejudiced agents. For this, we need to be able to observe actual market prices in a setting that features measurable and plausibly exogenous changes in the scope for ethnic discrimination. Our approach is to track the evolution of housing prices in the neighborhood of state-run asylum seeker hosting centers (henceforth "asylum centers") with different occupant populations. The opening or closure of a center typically represents a salient and quantitatively relevant change in the local population of various foreign origins. Most asylum seekers, however, do not have access to local labor and housing markets (see, e.g., the discussion in Couttenier et al., 2019), which means that they do not compete directly with residents. Hence, any resulting changes in local housing prices plausibly reflect the market equilibrium outcome determined by prejudiced and non-prejudiced natives. Moreover, center openings and closures are driven by determinants outside the affected local community and are thus largely exogenous with respect to local conditions. Housing price movements in the vicinity of asylum centers therefore offer us a measure of the equilibrium market response to immigration. A unique feature of our research design is that data on compositional differences in the populations of asylum centers also allow us to explore market reactions to different immigrant types.

Specifically, our analysis draws on geo-coded data for (a) the universe of public residential rental postings in Switzerland and (b) the opening, closing and populations of asylum centers over the 2004–2014 period. We estimate the effect of non-vacant centers on local rental prices applying a comprehensive set of fixed effects to filter out time-invariant confounders. The identifying variation we consider stems from changes around the opening and closing of asylum centers, comparing housing units within the same neighborhood but located at different distances from the center, and by comparing centers with time-varying occupant populations.

We find that the opening of a center is on average associated with a drop of 3.8% in rental prices within 0.7 km of the center. This effect emerges immediately after the opening of a center and persists for at least two years after the opening. To investigate the underlying mechanisms, we exploit information on differences in the ethnic composition of different centers. We find that the drop in local rental prices in the vicinity of centers populated by above-median shares of asylum seekers from sub-Saharan Africa equals 4.2%, whereas around centers with below-median shares of sub-Saharan Africans it equals 3.0%. Sub-Saharan African origin is the only asylum-seeker characteristic we find to have a statistically significant impact on housing-price responses. This result persists when controlling for crime-related variables, suggesting that the drops in rental prices may at least in part be due to animus other than "statistical" discrimination based on crime propensities.

This paper is related to several strands of research. First, we contribute to an empirical literature on discrimination against ethnic minorities in various contexts, such as the labor market (Lang and Lehmann, 2012; Agan and Starr, 2018; Neumark, 2018; Hangartner et al., 2021), the housing market (Yinger, 1986; Ewens et al., 2014; Laouénan and Rathelot, 2022), citizenship applications (Hainmueller and Hangartner, 2013), or criminal justice (Lang and Kahn-Lang Spitzer, 2020). With regard to asylum seekers, there is evidence from a large non-incentivized survey that asylum seekers with greater employability, more consistent asylum testimonies, severe vulnerabilities and of Christian (rather than Muslim) faith are met with greater public acceptance in European host countries (Bansak et al., 2016).

Methodologically, we follow the hedonic pricing approach, using granular housing-market data to infer willingness to pay. This method has in the past been used to gauge the impact of a range of factors.

In Appendix Table A1, we list the most closely related studies we are aware of, focusing on the price effects of factors that one may expect to be considered as disamenities.[3] The upper part of the table summarizes papers that have explored the effects of asylum centers. Our average estimated effect is close to the midpoint of those found for other countries. Switzerland therefore does not appear to exhibit particularly pronounced housing market responses to asylum centers. What sets our paper apart is that we draw on newly assembled, very fine-grained data on the asylum center nationality composition and on local and group-specific crime rates, which enables us to relate price effects to center population characteristics. In the lower part of the table, we list papers that use closely comparable estimation methods but consider other types of local disamenity. Again, our central elasticity estimate is within the range of those found elsewhere for disamenities including noise, toxic sites, and the presence of sex, gun or drugs offenders.

Appendix Table A1 also shows that, in contrast to our approach, most prior studies consider housing prices, rather than rents, with the idea of quantifying the capitalized effect of *permanent* disamenities. Instead, following Saiz (2003, 2007), we use housing rents. In our context, using rents is more appropriate given the transitory nature of the amenity shocks we are interested in and the fact that rents respond to market conditions more quickly than housing prices (Rosen and Smith, 1983). Indeed, we look not only at short-lived openings of asylum centers but also at their ethnic composition, which varies over time – often from one month to another.[4]

The remainder of the paper is organized as follows: Section 2 describes the method and data, Section 3 presents the main results, Section 4 shows robustness analyses, and Section 5 concludes.

2. Method and data

2.1. Estimation

We estimate the effect of asylum centers on local rental prices using a difference-in-differences strategy, where housing units located near an asylum center (treatment group) are compared to units located further away (control group), before and after the opening or closing of the center. Our basic observational unit, h, is a rental posting. In order to contrast housing units that are comparable in terms of local economic and topographic characteristics, we only consider dwellings within a two-kilometer radius of asylum centers. We assign each unit h to the closest asylum center, c, that is open at some point during the time range covered by our sample. Moreover, we control for a vector of housing characteristics, $\mathbf{H}_h$, center fixed effects, λ_c, municipality fixed effects, γ_m, and year fixed effects, $\tau_{y[t]}$.

Formally we estimate the following equation:

$$\ln(Rent)_{hcmt} = \alpha + \beta(Active_{hct} \times Prox_{hc}) + \theta_c Active_{hct} + \delta_c Prox_{hc} + \mathbf{H}'_h\boldsymbol{\Gamma} + \lambda_c + \gamma_m + \tau_{y[t]} + \varepsilon_{hcmt}, \tag{1}$$

[3] A key distinction to keep in mind is that while asylum centers may yield merely subjective disamenities for certain (prejudiced) individuals, several other forms of exposure listed in Appendix Table A1 lead to objective negative externalities affecting the entire neighboring population.

[4] Further related literatures study the impact of asylum seeker arrival on local population size (Batut and Schneider-Strawczynski, 2022), on crime (Bianchi et al., 2012; Bell et al., 2013; Couttenier et al., 2019), on right-wing voting (Otto and Steinhardt, 2014; Barone et al., 2016; Steinmayr, 2021; Dustmann et al., 2019; Vertier et al., 2022; Gamalerio et al., 2023) and on policy preferences (Zimmermann and Stutzer, 2022). For another literature linking the population composition to urban outcomes, see Eberle et al. (2020).

where the dependent variable is the natural logarithm of the rental price per square meter in rental posting h, assigned to its nearest asylum center c, located in municipality m, and published on day t. The binary variable $Active_{hct}$ is set to one if the center c assigned to rental posting h is open at time t when the property advert is recorded. The binary variable $Prox_{hc}$ is set to one if the property referred to by rental posting h is located within a certain threshold distance from its nearest center c. Distance thresholds are algorithm-driven, as detailed below.

Notice that because of the fixed-effect structure in Eq. (1) and the inclusion of the center-specific coefficients θ_c and δ_c, we exploit only within-center variation to estimate β, our parameter of interest.[5] This approach is important for two reasons. First, it allows us to contrast housing units located in comparable neighborhoods. Second, it reduces the concern that the "forbidden-comparisons" typical of staggered settings between already treated and newly treated units could bias our coefficients (e.g. Borusyak et al., 2024). To further ease this concern, we present a replication of our main estimates using an alternative stacked regression specification technique in Appendix A.3.

Our treatment and control groups are illustrated in Fig. 1. Yellow rings show the 2-kilometer circles around asylum centers which delineate our estimation sample. Treated housing units are those located within the red rings (i.e. below the retained threshold distance). Properties located between the red and the orange circles are excluded from the sample in order to avoid capturing spillover effects.[6] Moreover, we drop from the sample housing units located at the same time within the treated (red) ring of a center and the treatment (red) or spillover (orange) ring of their second closest center, in order to avoid contamination of the estimated effects by centers' opening and closing at different points in time.[7] We determine the length of the treatment (red) and spillover (orange) radii using a method proposed by Butts (2023) to estimate treatment effect decay as a function of distance non-parametrically, exploiting the partitioning-based least squares approach developed by Cattaneo et al. (2020). In Fig. 2, we show that the strongest effect is present until 709 m from the centers. Then it attenuates from 710 to 1110 m, to become even smaller between 1110 and 1425 m. We choose as the baseline spillover ring the 710–1110 m band, in order to avoid losing too many observations, but as can be noticed in column (2) of Table 1, results are robust if we also drop the 1110–1425 m band.

2.2. Data

2.2.1. Asylum centers and surrounding housing

Upon arrival in Switzerland, asylum seekers are required to register at one of seven federal registration centers located at the main border crossings and airports, where identity checks and first interviews take place. There, asylum seekers are assigned to one of the 26 cantons according to an exogenous allocation scheme based on population size (see Couttenier et al., 2019, and Egger et al., 2022). The cantonal authorities are then responsible for the distribution of asylum seekers among municipalities, and they decide on the opening of new centers and where to locate them.[8]

We obtained non-publicly available data on asylum-seeker hosting centers from 13 cantonal authorities or, in some cases, from private bodies mandated by the cantons to manage the centers. The data include the date of opening and, where relevant, closing, the precise location, and the hosting capacity for all centers which were opened at some point in time over the 2004–2014 period. As we seek to retain those asylum centers that are plausibly salient to the local population, we consider the 91 centers featuring a capacity of at least 30 beds and that stayed open for at least four years within our sample period.[9]

We draw on a database of internet advertisements for rental housing units in Switzerland over the period 2004–2014. Unlike posted purchase prices, posted rental prices are rarely revised or renegotiated in Switzerland and therefore usually correspond to final contractual prices.[10] The dataset originates from online publications on 26 national and regional web platforms and has been provided by meta-sys.ch, a consulting firm.[11] Our sample consists of 154,573 rental postings, which implies an average of 39 postings per center neighborhood and quarter over our sample period. 82% of our observations relate to single floor apartments, and the average annualized rent per square meter was 280 Swiss francs (CHF, with 1 CHF ≈ 1 USD). Further summary statistics are provided in Appendix Table A2. Among the information available in the dataset, we rely on the publication date of the offer, the geo-coordinates of the housing unit, its rental price and its floor space. We also include dummy variables for the number of rooms and a set of 11 categories that define the type of housing unit. Throughout the analysis we take housing price to be the annualized rent in CHF per square meter. We match housing units to asylum centers on distance "as the crow flies" regardless of the activity status of the center.

2.2.2. Asylum seekers

We have access to non-publicly available individual-level administrative data for all asylum seekers arriving in Switzerland during our sample period (see Couttenier et al., 2019). We match this information to hosting centers, based on the address and time. In this way, we are able to retrieve the nationality of origin and the stated religion of individuals living in each hosting center at any given time. These data also allow us to construct our main heterogeneity measures: shares of different types of asylum center populations according to nationality and religion.

As an additional variable to characterize asylum center populations, we consider the average genetic distance between nationalities, as constructed by Spolaore and Wacziarg (2018). Given that phenotype and genotype are imperfectly correlated, this offers an alternative measure of "otherness". We retain the genetic distance of asylum seekers living in the centers with respect to the *overall* population of Swiss nationals and compute the time-varying weighted average of genetic distance for every asylum center ($Gendist$). Furthermore, we compute a time-invariant measure of *local* genetic distance between the (time averaged) nationalities of host municipality and asylum center populations ($LocalGendist$), for splitting the sample in that dimension.[12]

[5] This is due to the inclusion of three sets of fixed effects: center-specific fixed effects, λ_c, center-specific dummy variables for housing units observed during the open spell of a center, $\theta_c Active_{ct}$, and center-specific dummy variables for housing units within the treatment radius, $\delta_c Prox_{hc}$.

[6] We can think of two kinds of spillover effects. One is the negative effect on housing rents that might propagate further away than our treatment radius, as Fig. 2 suggests. The second type of spillover effect is individuals moving away from the treatment area after a center's opening, re-settling in the control group region and thus increasing rental prices there.

[7] Imagine a posting for a unit located in the intersection area of the red circle and the orange circle, respectively, of two different centers A and B. Center A is closer to the housing unit than center B (by definition). If we observe a rental posting for the unit when center A is still closed but center B is already open, then we would get a biased estimate considering the house as untreated while it might be affected by the fact that center B is already open.

[8] The replication package, which includes the code and data required to reproduce our results, is available at Brülhart et al., 2026.

[9] See also Appendix Table A3.

[10] Fleury (2018) reports that in 82% of cases, posted rental prices correspond to final prices.

[11] Examples of such postings can be found e.g. on the large real estate portals https://www.homegate.ch/en and https://www.immoscout24.ch/en. Representative screenshots are reproduced in Appendix Figure A1.

[12] We construct $LocalGendist$ as the weighted sum of the genetic distances among all nationalities present in the municipality and all nationalities present in the center. The weights are given by the product of time-averaged nationality shares in the local population and among center residents.

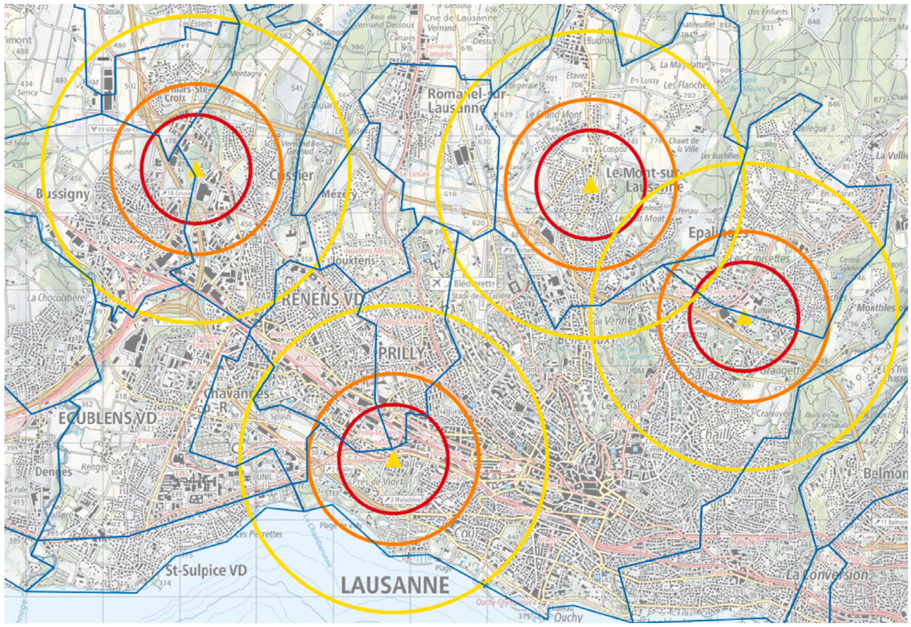

Fig. 1. Asylum centers and their neighborhoods.
Notes: The figure shows, by way of example, the four asylum centers in the urban area of Lausanne with the radii for the respective treatment and control groups. Blue lines are municipality borders. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: The underlying map is obtained from https://www.swisstopo.admin.ch/it/geodata/maps/smr/smr50.html.

2.2.3. Crime data

We have access to non-publicly available data on all crimes detected by the police in Switzerland between 2009 and 2014 (see Couttenier et al., 2019). We consider only violent and property crimes. The data report the perpetrators' nationality and residence status. We exploit this information to distinguish crimes committed by asylum seekers and build a measure of "crime propensity" by nationality. The measure is constructed by dividing the total number of crimes committed by asylum seekers of a given nationality over the total number of asylum seekers from that nationality living in Switzerland. We then build a center-level measure of crime propensity, by taking a weighted average of national crime propensities of individuals from different countries present in a center on a given day. We validate this measure in Appendix Table A4, which confirms that it is statistically significantly correlated with the municipality-month-level number of crimes committed by asylum seekers.

3. Results

Fig. 2 illustrates non-parametric estimates of treatment effects in five bins with an equal number of observations. Effects are estimated non-parametrically within each bin, comparing units pre and post treatment. The estimated effect from the most distant bin of observations is then subtracted from the others by way of normalization.[13] We adopt this approach in order to establish in a data-driven way the optimal radius for defining our treatment and control groups.

Open asylum centers are clearly associated with lower rental prices in their immediate vicinity, and the effect dissipates monotonically with distance. We take this evidence to guide our baseline choice of radii to define our treatment and control groups, considering as the treatment group all housing units within a 709 m radius of a given center and as the control group all housing units within the corresponding 1110–2000 m distance band.

In Table 1, we show difference-in-differences estimates of the housing price effects considering different radii (Panel a), and the possibility that openings and closures do not affect housing prices symmetrically (Panel b).

Column (1) of Panel (a) applies our preferred definition of treated, spillover and control groups. According to this baseline estimate, the opening of an asylum center reduces average rental prices in the vicinity by 3.8 percent.[14] Columns (2)–(4) show that this estimate is robust to changing the definition of the treatment and spillover groups. In column (2), we increase the spillover radius to 1425 m. Doing so, we also exclude observations located in the third distance bin (from left to right) of Fig. 2. Finally, in columns (3)–(4) we show that the result does not significantly change when we apply round distance cutoffs at 500, 1000 and 1500 m. Decreasing the treatment radius from 709 to 500 m results in a somewhat larger coefficient of −5.1 percent (column 3), which is however not statistically significantly different from our baseline estimate.

In Panel (b) of Table 1, we investigate whether opening and closures have a differential impact on housing prices. The results shown in column (1) are based on the sub-sample of centers that were already open in 2004, the start of our sample period, and closed at some point thereafter. We report the interaction effect *First-time inactive* $\times Prox$, which equals one for housing units near a center (within 709 m) after its closure. Conversely, in column (2) we restrict the sample to centers that opened during the sample period and remained open until 2014. There, we report the interaction effect *First-time active* $\times Prox$, which equals one for housing units near a center (within 709 m) after its opening.

The two estimates have opposite signs, as expected, and are of similar magnitude, despite the different samples. This is reassuring for several reasons. First, our baseline estimates do not seem to be driven specifically by either closures or openings. Second, these estimates suggest that the potential endogeneity of openings and closures is not a major concern. For example, if decision makers were to open centers

[13] Specifically, we follow the approach developed by Butts (2023), splitting the sample into distance quantiles following Cattaneo et al. (2020).

[14] We systematically report coefficient estimates $\hat{\beta}$ rather than $(e^{\hat{\beta}} - 1)$, as the difference is negligible within the value ranges that we obtain.

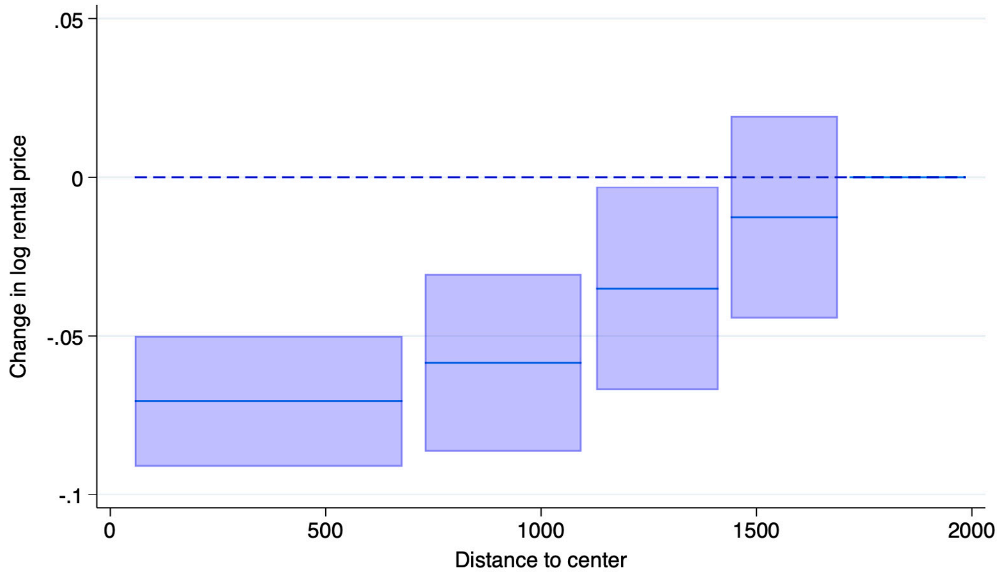

Fig. 2. Rental prices near asylum centers: Distance gradient.
Note: The figure plots non-parametric estimates of the treatment effect as a function of distance, following Butts (2023).

Table 1
Average effect on rental prices of asylum-center openings and closures.

Dep. Variable	(1) ln(Rent)	(2) ln(Rent)	(3) ln(Rent)	(4) ln(Rent)
Panel (a): ***Baseline estimates***				
Radii: treatment–spillover (meters)	709–1110	709–1425	500–1000	500–1500
Active × Prox	−0.0380***	−0.0295**	−0.0511***	−0.0431**
	(0.0124)	(0.0114)	(0.0173)	(0.0184)
Observations	154,573	106,793	152,603	78,142
R^2	0.453	0.446	0.456	0.449
Panel (b): ***Center closures and openings***	(1)		(2)	
	Only closures		Only openings	
First-time inactive × Prox	0.0464***			
	(0.0135)			
First-time active × Prox			−0.0646***	
			(0.0204)	
Observations	115,119		18,543	
R^2	0.424		0.531	

Notes: In Panel (a), we report the estimated coefficient β from Eq. (1). The unit of observation is a rental posting, $h = hcmt$. The sample covers observations within 2 km of an asylum center that was open for at least four years within the period 2004–2014 and had a hosting capacity of at least 30 beds. In each column we control for a set of housing characteristics (described in Section 2.2.1), year and municipality fixed effects, as well as the fixed effects to exploit within center variation $\theta_c Active_{hct}$, $\delta_c Prox_{hc}$ and λ_c in accordance with Eq. (1). In Panel (b) we consider closures and openings separately. In column (1) we estimate the impact of closures *First-time inactive* $\times Prox$ in the sample of housing units assigned to the centers already opened in 2004 and closing only once before 2014. In column (2) we estimate the impact of openings *First-time active* $\times Prox$ in the sample of housing units assigned to the centers opening for the first time later than 2004 and closing after 2014. Clustered standard errors by municipality are reported in parentheses. The total number of municipalities (clusters) present in the sample is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in areas already experiencing price declines, we would be unlikely to observe similar-sized recoveries in local rental prices after a center's closure.

The baseline difference-in-differences estimates presented in Table 1 examine rental prices over the pre- versus post-opening periods. Taking an event-study approach, we can study the time profile of price changes and test for pre-trends. In particular, given the structure of our dataset, with some centers closing and re-opening over time, we use the methodology developed in De Chaisemartin and d'Haultfoeuille (2024).[15] We consider quarterly time intervals two years prior and two years subsequent to the opening of asylum centers, focusing on the baseline treatment and control definitions. Those estimates are shown in Fig. 3.

While the price series are somewhat volatile, the negative effect of center openings on rental prices in the immediate neighborhood again emerges clearly. Interestingly, that effect appears already in the first quarter following the opening of a center. This suggests that the rental price effect of asylum centers does not build up gradually, as the practical impacts of a center's presence get noted in the neighborhood, but happens immediately upon the activation of a center. We do not find statistically significant evidence of any pre-trends in local rental prices, which supports the interpretation of our estimates as causal effects of asylum centers on rental prices.

Next, we add interaction effects to our difference-in-differences regressions in order to explore whether different asylum center populations generate different rental price effects. We focus on two types of heterogeneity. One approach is to consider the "crime propensity" of asylum seekers, as described in Section 2.2.3. We take this as a variable that could proxy for statistical discrimination, whereby rental price movements might reflect observed or latent changes in local crime risks due to the presence of asylum seekers. Our alternative approach is to consider simple socio-ethnic distinctions: religion, average genetic distance from Swiss natives, and skin color. We take such variables as potential proxies for prejudice (sometimes also referred to as "animus" or "taste-based discrimination").

We present the main results in Table 2, with complementary estimates shown in the Appendix (Tables A7, A9; Figure A2). When we interact the baseline effect with a dummy variable set to one for centers whose populations at a given time have above-median inferred crime propensity, we do not find any significant effect for the triple interaction. We detect a considerably larger and more precisely estimated difference when we split asylum centers into those with above-median and below-median shares of residents of sub-Saharan

[15] As far as we are aware, among the estimation methods recently developed to estimate leads and lags of treatment effects (see Borusyak et al., 2024, Callaway and Sant'Anna, 2021, and Sun and Abraham, 2021), correcting the potential bias due to negative weights, this is the only one allowing for a setting where the treatment can switch on and off over time.

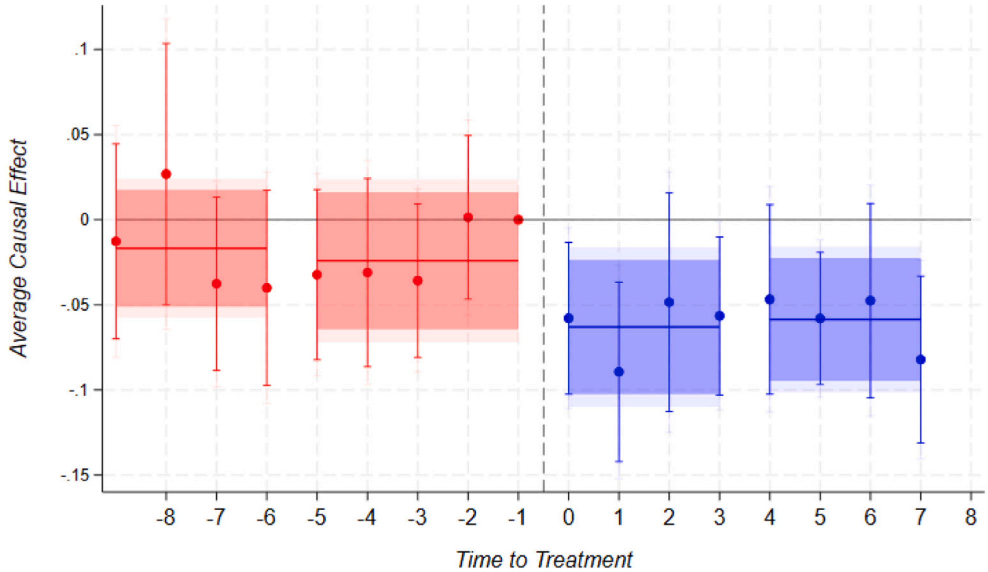

Fig. 3. Time pattern of rental prices around the opening of asylum centers.
Notes: The figure displays the event-study estimates of the main effect $Active_{hct} \times Prox_{hc}$, at quarterly frequency for two years prior and two years after the opening of an asylum center. The estimates are computed using the approach proposed by De Chaisemartin and d'Haultfoeuille (2024).

African nationality. According to the estimates of column (3) in Table 2, the opening of a "low-African" center reduces local rental prices by 3.0 percent, but that of a "high-African" center reduces them by 4.2 percent. When we consider both interactions jointly (column 4 of Table 2), the interaction with $Crime$ is insignificant while that with $African$ remains essentially unchanged.[16]

As we show in Appendix Table A7, religion, defined as the share of center residents who self-declare as Muslim, does not appear to drive differences in rental-price responses. The variable $Gendist$ that captures genetic distance relative to the overall population of Swiss nationals, does not turn out to be statistically significant either in any regression specification. This result suggests that discrimination targets mainly black asylum seekers and not necessarily those having more different traits as explained by genetic dissimilarity.[17]

Overall, we find the skin color of center residents to be the only statistically discernible source of heterogeneous rental price responses. This is consistent with racial prejudice rather than statistical discrimination.

Finally, we investigate whether rental price responses differ across localities with different characteristics. We split the sample by the median across municipalities of one of three variables: the average education level of the municipality's population, the local genetic distance between municipality and center populations ($LocalGendist$), and the share of foreign nationals who lived in the given municipality in 2003, one year prior to the start of the sample period. As these characteristics are available at the level of municipalities, we assign to housing units within 2 km from a center the characteristics pertaining to the municipality where the center itself is located.

The education variable can serve to proxy for the "education hypothesis", whereby ethnic and national prejudice diminishes with ex-

Table 2
Socio-Ethnic differences in center populations.

Dep. Variable	(1) ln(Rent)	(2) ln(Rent)	(3) ln(Rent)	(4) ln(Rent)
Effect	Base	Crime	African	African-Crime
Active × Prox	−0.0380*** (0.0124)	−0.0347** (0.0134)	−0.0303*** (0.0112)	−0.0296** (0.0115)
Active × Crime		0.0099* (0.0057)		0.0090 (0.0063)
Active × Prox × Crime		−0.0054 (0.0067)		−0.0008 (0.0066)
Active × African			0.0057 (0.0069)	0.0039 (0.0067)
Active × Prox × African			−0.0173*** (0.0047)	−0.0180*** (0.0064)
Observations	154,573	154,573	154,573	154,573
R^2	0.453	0.453	0.453	0.453

Notes: The table reports estimated coefficients β from Eq. (1) as well as interaction effects with socio-demographic variables representing the inferred crime propensity of a center's population ($Crime$) and the sub-Saharan African presence in a given center ($African$). $Crime$ and $African$ are binary variables set to one whenever the population of a given center has an above-median crime propensity (inferred from nationalities, see Appendix A.2) and an above-median share of sub-Saharan African people, respectively, with relevant medians calculated over the entire data sample. The unit of observation is a rental posting, $h = hcmt$. The sample covers observations within 2 km of an asylum center that was open for at least four years within the period 2004–2014 and had a hosting capacity of at least 30 beds. In each column we control for a set of housing characteristics (described in Section 2.2.1), year and municipality fixed effects, as well as the fixed effects to exploit within center variation $\theta_c Active_{hct}$, $\delta_c Prox_{hc}$ and λ_c in accordance with Eq. (1). Clustered standard errors by municipality are reported in parentheses. The number of sample municipalities (clusters) is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[16] Note that in our data sub-Saharan origin and crime propensity do not correlate. In fact, the crime propensity of asylum seekers from sub-Saharan Africa is one-third lower than the crime propensity of non-sub-Saharan asylum seekers. This difference, however, is not statistically significant (p-value = 0.34). Appendix Table A9 shows that the effect of $African$ is robust to varying the cutoff values for generating the binary variable. Appendix Figure A2, based on a nonparametric estimation explained in Appendix A.5, suggests the effect of $African$ to be nonlinear, with notable discontinuity above the median, which corresponds to an African share of 29%. This implies a threshold effect and likely explains why replacing $African$ by its continuous version does not yield statistically significant estimates on the triple interaction term. The estimated effects of $African$ turn out also to be robust to the simultaneous inclusion of the municipality-level variables $Education$ and $LocalGendist$ (see Appendix Section A.4 and Table A8).

[17] Although $Gendist$ is strongly correlated with the share of sub-Saharan Africans, it reflects genetic variation across populations more broadly. For instance, it also takes high values for populations from East Asia or native Latin America.

Table 3
Differences in the composition of local resident populations.

Dep. Variable	(1) ln(Rent)	(2) ln(Rent)	(3) ln(Rent)	(4) ln(Rent)	(5) ln(Rent)	(6) ln(Rent)
	Education		*Local Gendist*		*Foreign*	
	High	Low	High	Low	High	Low
Active × *Prox*	−0.0292**	−0.0521***	−0.0284	−0.0454***	−0.0467**	−0.0326*
	(0.0127)	(0.0193)	(0.0184)	(0.0169)	(0.0185)	(0.0182)
Observations	79,370	74,942	79,348	74,095	81,923	72,391
Clusters	57	142	99	104	57	143
R^2	0.330	0.429	0.431	0.469	0.362	0.490

Notes: The table reports estimated coefficients β_1 from Eq. (1). The unit of observation is a rental posting, $h = hcmt$. The sample covers rental postings for housing units located within 2km of a hosting center, for the period 2004–2014. We split the total sample by three municipal characteristics: $Education$, defined as the share of residents with either a university degree or a higher professional qualification ("école professionnelle supérieure"), local genetic distance ($LocalGendist$), and foreign share of the total population living in the municipality in the year before the start of our sample period (2003). In even-numbered (odd-numbered) columns, the sample is composed of observations in municipalities with below-median (above-median) values of the given municipal characteristic. In each column we control for a set of housing characteristics (described in Section 2.2.1), year and municipality fixed effects, as well as the fixed effects to exploit within center variation $\theta_c Active_{hct}$, $\delta_c Prox_{hc}$ and λ_c in accordance with Eq. (1). Clustered standard errors by municipality are reported in parentheses. Statistical significance is represented by $^*p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

posure to formal education (see, e.g., Dustmann and Preston, 2007 and Hainmueller and Hiscox, 2007). In the same vein, if rental prices respond less sensitively to the opening of an asylum center in municipalities where the incumbent local population is less dissimilar from asylum seekers hosted in the center or contains a higher share of foreign nationals, this could be (loosely) interpreted as consistent with the "contact hypothesis" (see, e.g., Allport et al., 1954; Pettigrew and Tropp, 2006; Rohner et al., 2013 and Rohner and Zhuravskaya, 2023), in the sense that areas that are already ethnically diverse *before* the opening of an asylum center are better used to interacting with people of diverse national origins.[18] Our identification strategy here exploits cross-municipality variations in the local genetic distance and share-of-foreigners variables (see Appendix Table A2). This approach relies on the fact that Switzerland is a small country that hosts a diverse and large population of immigrants. Hence, Swiss municipalities differ considerably in terms of the nationalities of origin of their residents even absent any asylum centers.

Results are shown in Table 3. Consistent with the education hypothesis, we find that rental prices in municipalities with above-median educational attainment react somewhat less strongly to asylum centers than those in municipalities with below-median educational attainment (columns 1 and 2). When we divide the sample based on local genetic distance (columns 3 and 4) or on the incumbent share of foreign nationals (columns 5 and 6), rental prices appear to respond somewhat more strongly in municipalities whose populations are more "similar" to the asylum seekers hosted in the local center. This result runs contrary to the (loosely defined) contact hypothesis. The statistical power of these sample splits, however, is limited, and none of the observed differences is statistically significant. Responses of rental prices to asylum centers turn out to be uniformly negative across different types of municipalities.

[18] Note that this corresponds to a broad interpretation of the contact hypothesis, since in its narrow definition this hypothesis only applies to contact between specific groups. Put differently, in the standard formulation of the contact hypothesis, more contact between people from, say, Switzerland and Senegal would not affect Swiss attitudes towards people from a third country, say, Mali. Also, in the game-theoretic micro foundations of Rohner et al. (2013), the trust building effect of peaceful interaction is confined to matching between two specific groups, and does not give rise to generalized open-mindedness towards other groups.

4. Robustness

Center closures. Our main coefficient of interest is the interaction between center activity and proximity to centers. In our baseline estimations both openings and closures contribute to the identification of this coefficient, with housing units observed during the closed spell(s) of a center belonging to the control group. Moreover, we estimate triple interaction effects, e.g. with the presence of sub-Saharan African asylum seekers. This exploits changes in the composition of asylum seekers from different countries during the open spell(s) of a center. The event study shown in Fig. 3 is the natural setting to check for pre-trends and to track the evolution of center activity over time. In Table 1 we show quasi-symmetric effects when looking separately at the treatment of openings and closures. For completeness, Appendix Figure A3 presents event-study evidence based on closures. The qualitative effects are symmetric to the opening-based event study (Fig. 3), showing an increase in housing rents following a closure. Leading up to closures, however, we observe a certain negative pre-trend, which suggests that the timing of closures might be driven to some extent by negative localized externalities.

Balancing of housing characteristics. Appendix Tables A10 and A11 highlight the importance of comparing housing units within center neighborhoods. Appendix Table A10 reports balance statistics comparing housing characteristics between the treatment and control groups for the entire sample during the pre-treatment period. Here, we find statistically significant differences for most characteristics: neighborhoods are not alike. In contrast, Appendix Table A11 summarizes differences in housing characteristics when conditioning on center fixed effects. Most characteristics, and rental prices in particular, are no longer significantly different in the treated and control regions prior to center opening (column 2). In line with our main result, rental prices are found to be significantly lower for treatment-area housing units during open spells (column 3). Meanwhile, we find that most other observable housing characteristics remain statistically unchanged. There is some evidence, however, of higher-end rental objects such as duplex apartments and detached houses to be in shorter supply during opening spells. Given that we control for housing characteristics in our baseline estimates, however, our main findings should be unaffected.

Assignment and characteristics of asylum seekers. Given that our estimation strategy identifies the impact of sub-Saharan African presence within-center over time we do not require that assignment of asylum seekers to centers be fully random, but that assignment of individuals with different characteristics be exogenous with respect to highly localized differential housing market trends around hosting centers. In Appendix Table A12, we show that the share of children (aged below 12), the share of young males (aged 18–39, identified as the "salient" group in terms of crime propensity by Couttenier et al., 2019), and the average age of males within the 18–39 age range, do not differ significantly between centers with high and low shares of sub-Saharan African asylum seekers. In Appendix Table A13, we explore the impact of expanding the list of asylum-seeker characteristics considered as controls: Middle Eastern and North African (MENA) origin, log per-capita GDP in the country of origin, and the share of males aged 18–39. None of these variables turns out to affect housing rents statistically significantly, and the sub-Saharan African effect remains robust to their inclusion. Finally, in Appendix Table A14, we investigate whether the share of asylum seekers who originate from French-speaking countries has a different impact on rental prices in French-speaking parts of Switzerland. We find this interaction effect to be statistically insignificant.

As there might be some autocorrelation (positive or negative) in center assignments of asylum-seeker groups, we moreover consider lagged versions of our African share indicator. As shown in Appendix Table A15, lagged measures of African share remain statistically significant. When we consider lagged and contemporaneous effects jointly, the contemporaneous effect turns out to be stronger. The exact lag structure chosen for our estimations does not significantly affect our results.

Center capacity. In Appendix Table A16, we show estimates for different subsamples within varying capacity ranges. We find no more significant effects when considering only centers with less than 30 beds. This is unsurprising given the low visibility of such facilities–typically consisting of small clusters of unmarked apartments within multi-unit buildings. When we increase the minimum capacity cutoff for our sample of centers, we find remarkably stable effects across thresholds ranging from 10 to 100, despite the sample size shrinking by a factor of three when raising the cutoff within that range.

Market prices and quantities. Contractually agreed rental prices could potentially differ from those posted online. A practitioner study by Fleury (2018) reports that posted rental prices in Switzerland correspond to final prices in 82% of cases. Nonetheless, we re-compute our main estimates using only those housing units whose advertisement remained on the website for less than a month, as price revisions or renegotiations are particularly unlikely in such cases. Appendix Table A17 shows that our coefficient of central interest is essentially identical across postings with different spell length.

Our analysis focuses on housing rents, as we have a clear prior on the sign of the price response (conditional on there being one) but not on the quantity response. Nevertheless, one may ask whether a center's activity has an impact on the volume of rental transactions. Residential vacancies in the vicinity of active centers could increase, e.g. due to increased out-moves by incumbent residents. Or they could decrease, e.g. if owners were to 'hoard' vacant units, awaiting the closure of the center. In Appendix Table A18, we take the quantity of listings as our outcome variable, and we find no statistically significant impact of asylum center activity. When using a Poisson specification (Appendix Table A19), we observe a statistically significant quantity effect in the top price quintile. This is consistent with some hoarding behavior at the higher end of the market. As we control for housing unit characteristics, however, such behavior cannot explain the price responses of central interest in this paper.

5. Summary and discussion

Does individual-level ethnic prejudice affect aggregate market outcomes, or are such biases arbitraged away? To answer this question, we investigate equilibrium prices in a setting featuring the likely presence of prejudiced agents: real estate transactions in the neighborhood of asylum seeker hosting centers. We employ difference-in-differences estimation of rental housing prices, exploiting information on the changing ethnic composition of center populations. Market rents of housing units within 0.7 km of an active center are found on average to be 3.8% lower than market rents in the control group, for at least two years (the length of our observation window in the event-study analysis). Arbitrage by non-prejudiced agents is therefore partial at best.

The price drop varies markedly with the share of asylum seekers of sub-Saharan African origin: in the vicinity of centers with below-median sub-Saharan African shares, the price effect is −3.0%, but for centers with above-median shares, that effect is −4.2%. In contrast, we find no statistically significant effect heterogeneity with respect to inferred crime propensity or religious affiliation of asylum seekers. Those findings are consistent with phenotype-based racial animus as the dominant driver of the observed market outcomes. We find weak evidence consistent with the education hypothesis and no evidence supporting the contact hypothesis.

How large are our estimated effects? One approach is to compare our observed rental-price responses to those found in the hedonic pricing literature with respect to local disamenities such as the presence of sex offenders, narcotics labs or toxic sites. Appendix Table A1 shows those estimates to range between −1.4% and −17.6%. Taken at face value, this comparison suggests asylum centers to be among the less severe 'local disamenities'.

Another way of benchmarking our estimated effect is to calculate the implied equivalent (monetary) variation. In our sample, the average yearly rent per square meter is 280 CHF, and the average surface is equal to 80 sqm (see Appendix Table A2). Hence, the average drop of 3.8% in the local rental price implies that the yearly willingness to pay in order to avoid an asylum center opening nearby is estimated to be 851 CHF per year and housing unit ($= 0.038\times280\times80$), or some 0.7% of average gross household income (SFSO, 2011). The implied willingness to pay for an asylum center's sub-Saharan African population share to drop from above-median to below-median is 492 CHF or some 0.4% of income.

We can cross-validate our main estimate against an external data source by considering the case of the Swiss municipality of Oberwil-Lieli (population $\approx$2300), where in 2016 a 52% majority of citizens voted in favor of collectively paying a fine of 110 CHF per day and asylum seeker for *not* hosting asylum seekers assigned to their municipality by the allocation rule. A back-of-the envelope calculation shows that the willingness to pay as elicited by this vote is actually comparable to the one based on our empirical analysis of housing-market price responses.[19]

A general lesson from the current paper is that there is a "price of prejudice". Similar to the well-known result of Dal Bó and Dal Bó (2011) that criminal and appropriative activities make everybody worse off, including the criminal, racial prejudice imposes costs not only on the objects of prejudice but also on the subjects, as shown in our computations of willingness to pay. This means that policies promoting tolerance yield a double dividend – for both victims and discriminators.

CRediT authorship contribution statement

Marius Brülhart: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Gian-Paolo Klinke:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Andrea Marcucci:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Dominic Rohner:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Mathias Thoenig:** Writing – review & editing, Writing – original draft,

[19] The minimum hosting capacity of asylum centers considered in our study is 30 beds. Hence, for preventing the opening of such an asylum center, the citizens of Oberwil-Lieli would be ready to pay 1,204,500 CHF per year ($= 110\times30\times365$). For a population of some 2300, this translates into 524 CHF per citizen per year. Let us assume, very conservatively, that the 52% of residents who voted yes had a willingness to pay corresponding to that amount, whereas the remaining 48% had a willingness to pay of zero. This would imply an average willingness to pay of 272 CHF. The representative housing unit in Switzerland hosts 2.2 individuals (SFSO, 2023); hence we obtain a willingness to pay of 598 CHF per year and housing unit. If instead we take the mean center capacity of 95 (see Table A2 Panel C), this amount even increases to 1894 CHF per year and housing unit. Such an extrapolation is probably unrealistic, given that the actual number of asylum seekers assigned to that municipality was 10. Moreover, Oberwil-Lieli is a rural and politically conservative place. Nonetheless, the outcome of that uniquely informative local referendum suggests that our estimated willingness to pay of 851 CHF per year and housing unit is not implausibly high.

Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jue.2026.103866.

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