Price and Prejudice:
Housing Rents Reveal Racial Animus*

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Abstract

We study market rents in the neighborhood of asylum seeker hosting centers. Our empirical setting exploits the opening and closure of centers and the quasi-random spatial allocation of asylum seekers in Switzerland. Rents within 0.7km of an active center are found on average to be 3.8% lower than rents in the control group. The price drop is more pronounced when centers host a higher share of asylum seekers from Sub-Saharan countries. In contrast, neither the religious affiliation of asylum seekers nor their inferred crime propensity affect prices significantly. Our findings are consistent with racial animus as the dominant driver of observed market outcomes.

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Introduction

Xenophobia and discrimination are global social challenges. While voluntary international migration is economically efficient, 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 in 2016 (Blinder and Richards, 2016). Similar patterns can be observed across the continent: in a recent cross-country poll, 34 percent of Europeans stated immigration as the most important issue facing the EU, considerably ahead of the second-ranked item, climate change, stated by 24 percent of respondents (Eurobarometer, 2019). In the United States, immigration and/or race relations have featured among the top four most important problems according to nationwide polls every year between 2015 and 2022 (Brenan, 2022).[1]

The 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. That such prejudice exists 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 alternatives may not act on their prejudice when discrimination incurs a cost. In an incentivized field experiment, Hedegaard and Tyran (2018) have 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

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[1] Victims of discrimination and prejudice are diverse, but one group that has often been a target – in their origin as well as in their host country – are asylum seekers. Unsurprisingly, this population group has been found to suffer disproportionately from trauma and anxiety. As documented in Abbott (2016), for example, refugee migrants in Sweden have a three to four times greater hazard of suffering from psychoses than the Swedish born population. While psychological suffering may to an extent stem from war exposure and political persecution, also the asylum process and treatment in the host location plays a significant role (see Laban, Gernaat, Komproe, Schreuders and De Jong, 2004). One adverse effect of a lengthy asylum process is to harm employment prospects of refugees (Hainmueller, Hangartner and Lawrence, 2016).

[2] See Neumark (2018) and Lang and Kahn-Lang Spitzer (2020) for recent surveys.

neighborhood of state-run asylum seeker hosting centers (henceforth "asylum centers"). The opening of such a center typically represents a salient and quantiatively relevant increase in the local-level population of foreign origin. Asylum seekers, however, do not have access to local labor and housing markets, which means that they do not compete directly with residents. Hence, any resulting changes in local property prices plausibly reflect the market equilibrium outcome determined by prejudiced and non-prejudiced natives. Moreover, center openings 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.

We find that the opening of a center is on average associated with a drop of 3.8% in rental prices in close proximity to 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 the quasi-random allocation of asylum seekers across centers, implying exogenous differences in the nationality composition of different centers (Couttenier, Petrencu, Rohner and Thoenig, 2019). We find that the drop in local rental prices is significantly larger in the vicinity of centers populated by above-median shares of asylum seekers from Sub-Saharan Africa. 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.

These findings are quantitatively sizeable (as discussed below) and policy relevant. If housing price drops had merely reflected statistical discrimination, by pricing in higher local crime rates, then a natural policy conclusion would have been to step up local policing. However, given that the price drops do not appear to be driven by greater criminality, it turns out that policies to combat discrimination are rather located in the realm of championing public education. In the same vein, we observe somewhat lower rental price effects in subsamples of municipalities with higher education levels. This leaves us with a hopeful message: it may take time and education to tackle xenophobia and prejudice, but education looks like a feasible path towards greater tolerance.

This paper is related to several strands of existing research. First of all, we contribute to an empirical literature studying discrimination of ethnic minorities in various contexts, such as the labor market (Lang and Lehmann, 2012; Agan and Starr, 2018; Neumark, 2018; Hangartner, Kopp and Siegenthaler, 2021), the housing market (Yinger, 1986; Ewens, Tomlin and Wang, 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, Hainmueller and Hangartner, 2016).

Methodologically, we follow the hedonic pricing approach, using housing rents to infer willingness to pay. This method has in the past been used to gauge the impact of environmental factors (Chay and Greenstone, 2005; Currie, Davis, Greenstone and Walker, 2015), riots (Collins and Margo, 2007), armed conflict (Besley and Mueller, 2012), store locations (Pope and Pope, 2015), and crime (Linden and Rockoff, 2008; Pope, 2008*b*; Congdon-Hohman, 2013). In contrast to our approach, most prior studies use prices (rather than rents) with the idea of quantifiying the capitalized effect of *permanent* disamenities (e.g. road infrastructure, airport, etc.). In our context, using rents is more apropriate 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.[3]

Three papers are closely related to ours. Van Vuuren, Kjellander and Nilsson (2019) study how proximity to temporary housing for refugees affects rents in Gothenburg (Sweden), and Daams, Proietti and Veneri (2019) investigate how the opening of asylum seeker reception centers impacts housing prices in the Netherlands. Both studies find the opening of asylum centers to be associated with a significant drop in neighboring housing prices. Myohl and Stadelmann (2020) study native residents' decision to resettle after asylum center openings in Switzerland.

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 investigate the channels of transmissions of any price effects of center presence. In contrast, the aforementioned existing studies do not have information on the ethnic composition of center populations. Another asset of our study is the fact that our data spans various regions of a whole country (Switzerland), rather than just one urban area (Gothenburg in the case of Van Vuuren et al., 2019), and that we can draw on rental prices rather than sales prices.

The rest of the paper is organized as follows: Section 1 describes the method and data, Section 2 displays the main results, and Section 3 concludes. The Appendix contains further context and data description and additional robustness checks.

[3]Further related literatures study the impact of asylum seeker arrival on crime (Bianchi, Buonanno and Pinotti, 2012; Bell, Fasani and Machin, 2013; Couttenier et al., 2019), on right-wing voting (Otto and Steinhardt, 2014; Barone, D'Ignazio, De Blasio and Naticchioni, 2016; Steinmayr, 2016; Dustmann, Vasiljeva and Piil Damm, 2019) and on policy preferences (Zimmermann and Stutzer, 2022). For another literature linking the population composition to urban outcomes, see Eberle, Henderson, Rohner and Schmidheiny (2020).

1 Method and Data

1.1 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, less frequently, 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 properties within a two-kilometer radius of asylum centers, and we include fixed effects for the closest asylum center, c. Moreover, we control for a vector of housing characteristics, $\mathbf{H}_h$, a vector of center fixed effects, λ_c, a vector of municipality fixed effects, γ_m, and a vector of year fixed effects, $\tau_{y[t]}$.

Formally we estimate the following equation:

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

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 distance from its nearest center c. Distance thresholds are algorithm-driven, as detailed below.

Notice that because of the fixed-effects structure of equation (1), we exploit only within-center variation to estimate β, our parameter of interest.[4] 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, Jaravel and Spiess, 2021). To further ease this concern we present a replication of our main estimates using an alternative stacked regression specification technique in Appendix A.3.

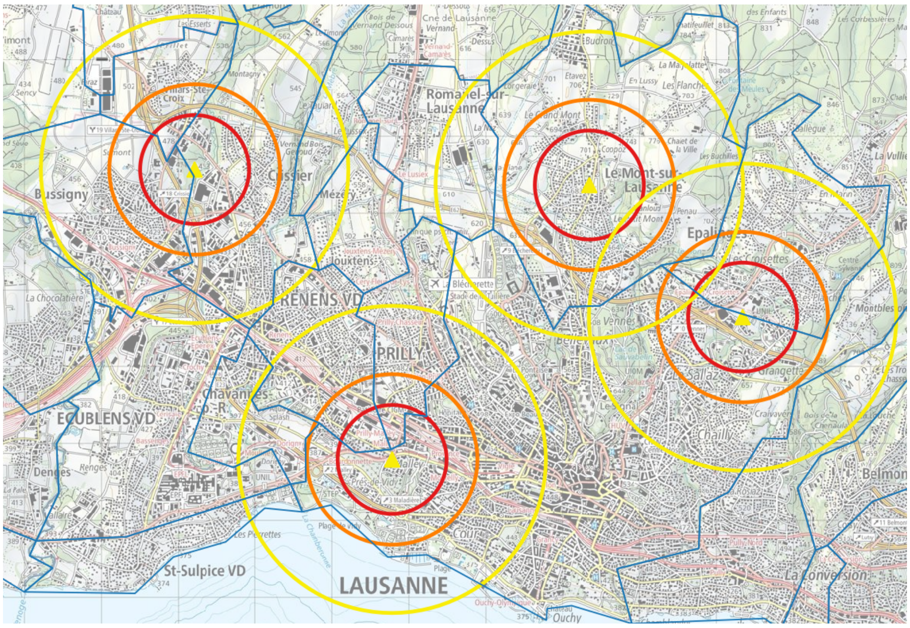
Our treatment and control groups are illustrated in Figure 1. Yellow rings show the 2-kilometer circles around asylum centers which delineate our estimation sample. Treated housing units are those located within the red rings. Properties located between the red and the orange circles are excluded from the sample in order to avoid capturing spillover effects.[5] 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

[4]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, $\delta_c Prox_{hc}$.

[5]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 Figure 2a 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 the rental prices there.

at different points in time.

Figure 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. The underlying map is obtained from `https://www.swisstopo.admin.ch/it/geodata/maps/smr/smr50.html`.

Imagine a house 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.

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, Farrell and Feng (2020). In Panel 2a of Figure 2, we show how the strongest effect is present until 709 meters from the centers. Then it attenuates from 710 to 1,110 meters, to become even smaller between 1,110 and 1,425 meters. We choose as the baseline spillover ring the 710–1,110 meter band, in order to avoid losing too many observations, but as can be noticed in column (2) of Panel 2b, results are robust if we drop also the 1,110–1,425 meter band.

1.2 Data

1.2.1 Asylum Centers and Surrounding Housing

Upon their 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 twenty-six cantons according to an exogenous allocation scheme based on population size (see discussion in Couttenier et al., 2019). 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.

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 only centers with a capacity of at least 30 beds and that stay open for at least four years within our sample period.

We draw on a database of internet advertisements for rental housing units in Switzerland over the period 2004-2014. The dataset originates from on-line publications on 26 national and regional web platforms and has been provided by meta-sys.ch, a consulting firm. Our sample consists of a repeated cross section of 157,708 housing units with an average of about 13 transactions per center neighborhood and month over our sample period. 82% of our units of observation correspond to single floor apartments, and the average annualized rent per square meter was 280 Swiss francs (CHF, with 1 CHF $\approx$ 1 USD). Further summary statistics about housing units in the sample can be found in Table A1. 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.

1.2.2 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 shares of different types of asylum center populations, based on 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 Swiss population and compute the time-varying weighted average of genetic distance for every asylum center ($Gendist$). Furthermore, we compute a time-invariant measure of *bilateral* genetic distance, taking into account the (time averaged) nationalities of host municipality and asylum center populations.[6]

[6]More precisely, we construct the weighted sum of the genetic distances among all nationalities present in the

1.2.3 Crime Data

We also 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). The data include precise information about 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 A3, which confirms that it is effectively correlated with the municipality-month-level number of crimes committed by asylum seekers.

2 Results

Figure 2 presents our baseline estimates of equation (1). Panel 2a illustrates non-parametric estimates of treatment effects in five bins with an equal number of observations.[7] It is apparent that the effect of an open asylum center on housing rents is negative in the immediate vicinity and dissipates with distance. We take this evidence to guide our baseline choice of radii to define our treatment and control groups, defining as the treatment group all housing units within a 709 meter radius of a given center and as the control group all housing units with the corresponding 1,110–2,000 meter distance band.

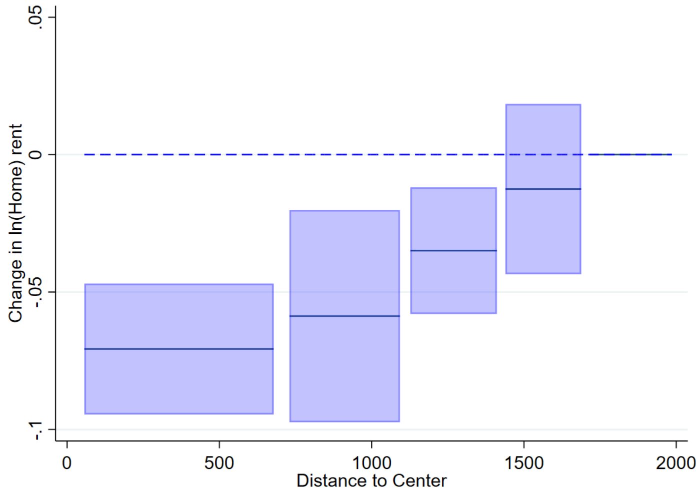
In Panel 2b we show difference-in-differences estimates of the price effects for varying radii. Column (1) applies our baseline definition of treated, spillover and control groups. According to our baseline estimate, the opening of an asylum center reduces average rental prices in the vicinity by 3.8 percent. 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 1,425 meters. Doing so we exclude from the sample also all those observations which are in the third bin (from left to right) in Panel 2a. Finally, in columns (3)-(4) we show that the result does not significantly change when we apply round distance cutoffs at 500, 1,000 and 1,500 meters. Decreasing the treatment radius from 709 to 500 meters results in a somewhat larger coefficient of -5.1 percent (column 3), which is however not statistically significantly different from our baseline estimate.

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.

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

Figure 2: Average Effect on Rental Prices of Asylum Centers

(a) Distance Gradient



(b) Baseline Estimates

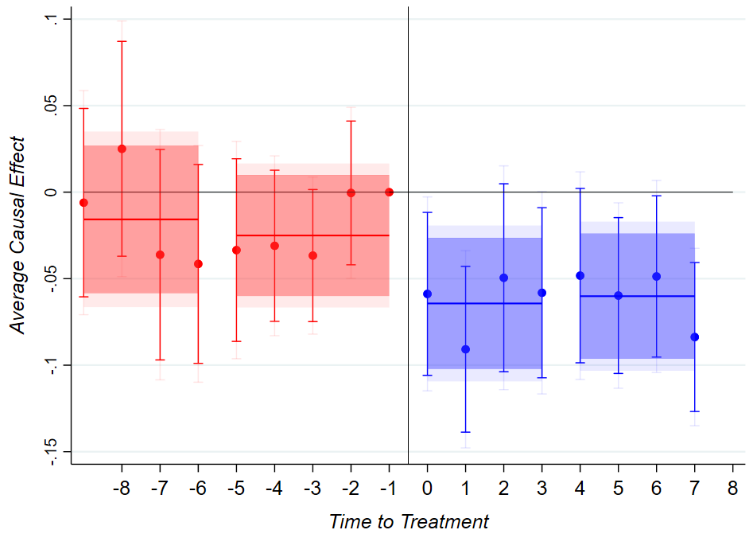
Dep. variable	(1) ln(Rent)	(2) ln(Rent)	(3) ln(Rent)	(4) ln(Rent)
Radii in meters:	*baseline*			
treatment; spillover	709; 1,110	709; 1,425	500; 1,000	500; 1,500
$Active \times Prox$	-0.0383***	-0.0299**	-0.0508***	-0.0428**
	(0.0124)	(0.0115)	(0.0174)	(0.0183)
Observations	154,708	106,833	152,730	78,277
R^2	0.453	0.446	0.456	0.449

Notes: In Panel (a), we plot the non-parametric estimates of the treatment effect as a function of distance, following Butts (2023). In Panel (b), we report the estimated coefficient β from equation (1). The unit of observation is a housing unit advert h published on day t. The sample covers observations within 2 kilometers 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 1.2.1), 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 equation (1). 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The difference-in-differences estimates presented in Figure 2 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 (2022).[8] We consider quarterly time intervals two years prior to and two years after the opening of asylum centers, focusing on the baseline treatment and control definitions. Those estimates are shown in Figure 3.

[8] As far as we are aware, among the estimation methods recently developed (see Borusyak et al., 2021, Callaway and Sant'Anna, 2021, and Sun and Abraham, 2021) to estimate leads and lags of treatment effects, 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.

Figure 3: Time Pattern of Rental Prices Around the Opening of Asylum Centers



Notes: The figure displays the event-study estimates of the main effect $Active \times Prox$, 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 (2022).

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 subsequent to 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 consider two types of heterogeneity. One approach is to consider the "crime propensity" of asylum seekers, as described in Section 1.2.3. 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 1, with complementary estimates shown in the Appendix (Tables A6, A7; Figure A1). 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 find only a borderline significant negative effect. The total effect obtained by summing the double and triple interactions of $Crime$ is not statistically different from zero. 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 African nationality. According to the estimates of column (3) in Table 1, the opening of a "low-African" center reduces local rental prices by 2.7 percent, but that of a "high-African" center reduces them by 4.8 percent. When we consider both interactions jointly (column 4 of Table 1), the interaction with $Crime$ is insignificant while that with $African$ remains statistically significant and quantitatively large.

As we show in Appendix Table A6, religion, defined as the share of center residents who self-declare as Muslim, does not appear to drive differences in rental-price responses. Genetic distance relative to Swiss natives, however, has a similarly strong effect as that of the share of Sub-Saharan Africans – which is not surprising, given that the two variables have a correlation coefficient of 0.90. Overall, our estimates suggest that the skin color of center residents is the main source of heterogeneous rental price responses. This is consistent with racial prejudice rather than statistical discrimination.[9]

Table 1: Socio-Ethnic Differences in Center Populations

	(1)	(2)	(3)	(4)
Dep. Variable	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)
Effect	Base	Crime	African	African & Crime
$Active \times Prox$	-0.0383***	-0.0314**	-0.0266**	-0.0239**
	(0.0124)	(0.0129)	(0.0107)	(0.0108)
$Active \times Crime$		0.0067		0.0065
		(0.0045)		(0.0051)
$Active \times Prox \times Crime$		-0.0107*		-0.0058
		(0.0057)		(0.0059)
$Active \times African$			0.0008	-0.0006
			(0.0084)	(0.0083)
$Active \times Prox \times African$			-0.0221***	-0.0203***
			(0.0049)	(0.0059)
Observations	154,708	154,708	154,708	154,708
R^2	0.453	0.453	0.453	0.453

Notes: The table reports estimated coefficients β from equation (1) as well as interaction effects with socio-demographic variables representing the inferred crime propensity of a center's population ($Crime$) and the of 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 kilometers 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 1.2.1), 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 equation (1)). Clustered standard errors by municipality are reported in parentheses. The number of sample municipalities (clusters) is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[9]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 A7 shows that the effect of $African$ is robust to varying the cutoff values for generating the binary variable. Appendix Figure A1, based on a nonparametric estimation explained in Appendix A.4, suggests the effect of $African$ to be nonlinear, with notable discontinuity above the 60th percentile, which corresponds to an African share of 28%. 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.

Finally, we investigate whether rental market price effects differ across localities with different characteristics. We split the sample by the median across municipalities of one of two variables: the average education level of the local municipality's population, and bilateral genetic distance between local and center populations. As these characteristics are available only at the level of municipalities, we assign to the housing units within 2km 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 exposure to formal education (see, e.g., Dustmann and Preston, 2007; Hainmueller and Hiscox, 2007). Similarly, if rental prices respond more sensitively to the opening of an asylum center in municipalities where the local population is more dissimilar from asylum seekers hosted in the center, this could be (loosely) interpreted as consistent with the "contact hypothesis" (see, e.g., Allport, Clark and Pettigrew, 1954; Pettigrew and Tropp, 2006; Rohner, Thoenig and Zilibotti, 2013; Rohner and Zhuravskaya, 2023), in the sense that areas that are already very diverse before the opening of an asylum center, are better used to interacting with immigrants of diverse national origins.[10]

Results are shown in Table 2.

Table 2: Differences in the Composition of Local Resident Populations

	(1)	(2)	(3)	(4)
Dep. Variable	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)
	$Education$		$Gendist$ (bilateral)	
	High	Low	High	Low
$Active \times Prox$	-0.0300**	-0.0519***	-0.0442**	-0.0332
	(0.0128)	(0.0193)	(0.0179)	(0.0201)
Observations	79,581	74,866	79,765	73,813
Clusters	57	142	107	100
R^2	0.330	0.429	0.426	0.479
T-test (p)	.345	.345	0.709	0.709

Notes: The table reports estimated coefficients β_1 from equation (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 two municipal characteristics: $Education$, defined as the share of residents with either a university degree or a higher professional qualification ("école professionnelle supérieure"), and genetic distance ($Gendist$). 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 1.2.1), 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 equation (1). In the bottom row of the table, we report p-values of two sided t-tests for the equality of the double interaction $Active \times Prox$. Clustered standard errors by municipality are reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[10] 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 of generalized open-mindedness towards other groups.

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), although the difference is not statistically significant. When we split the sample according to bilateral genetic distance (columns 3 and 4), rental prices seem to respond slightly more sensitively to the opening of an asylum center in municipalities where the local population is more dissimilar from asylum seekers hosted in the center, in line with the (loosely defined) contact hypothesis. This difference, however, is not statistically significant either.

3 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 the fact that asylum seekers in Switzerland are allocated across centers quasi-randomly. Market rents of housing units within 0.7km 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 -2.7%, but for centers with above-median shares, that effect is -4.8%. 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 racial animus as the dominant driver of the observed market outcomes. We also find suggestive evidence consistent with the education and contact hypotheses: the estimated rental price effect of center opening is somewhat stronger in municipalities where local population is less educated and more diverse in terms of genetic distance from asylum seekers hosted in the center.

How large are these effects? Our estimate of the impact of an asylum center is quantitatively comparable to what is typically found in the hedonic pricing literature regarding the impact of perceived criminality at the local level (e.g. presence of sex offenders, narcotics labs).[11] When compared to housing-market reactions to other types of exposures, the opening of an asylum center has a larger impact than the one caused by airport noise but is only about a quarter as large as that due to shale gas extraction sites at a comparable distance, and one third as large as the effect of toxic plants.[12] The greater price impact of shale gas extraction

[11] Linden and Rockoff (2008) estimate a reduction in house prices of 4% within 170m from the known home of a sex offender, while Pope (2008*b*) finds an impact of 2.3%. Dealy, Horn and Berrens (2017) find a drop in house prices of 6.48% when a methamphetamine lab is discovered within 170m from a housing unit, while Liang and Alexeev (2022) find a reduction in house prices of 5% within 800m from a facility for the safe injection of illicit drugs. These are similar magnitudes to what we find. Note, however, that most of those studies consider more localized effects (treatment radius of about 170m), while the impact we find affects a broader area making the effect more relevant in aggregate terms.

[12] Pope (2008*a*) finds a drop in housing prices affected by airport noise of 2.9%. Muehlenbachs, Spiller and

and toxic plants is intuitive, as they represent objective harm to the exposed populations due to environmental degradation, which is not the case for asylum housing to the extent that the price effect is driven by racial animus.

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 A1). 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 (=$0.038 \times 280 \times 80$), or some 0.7% of average gross household income (SFSO, 2011).

We can cross-validate this estimate against an external data source by considering the case of the Swiss municipality of Oberwil-Lieli (population $\approx 2,300$), where in 2016 a majority of citizens voted in favour of collectively paying a fine of 110 CHF per day and person for not hosting asylum seekers assigend 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.[13]

An important 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 and open-mindedness are win-win and feature a double dividend – for both victims and discriminators. A growing literature studies how inter-group tolerance can be fostered, drawing on the theoretical premises of the contact hypothesis, according to which more frequent (fair and peaceful) interactions with people of different ethnic and national background reduce prejudice (see, e.g., Allport et al., 1954; Pettigrew and Tropp, 2006; Rohner et al., 2013; Rohner and Zhuravskaya, 2023). Recent empirical evidence from national (military) service (Samii, 2013; Okunogbe, 2018; Cáceres-Delpiano, De Moragas, Facchini and González, 2021), soccer (Mousa, 2020; Alrababa'h, Marble, Mousa and Siegel, 2021) or reconciliation ceremonies (Cilliers, Dube and Siddiqi, 2016) show that inter-group interaction can foster inter-group trust and reduce tensions. A promising avenue for future research would be to explore the effect of inter-group interaction on racial animus towards asylum seekers more closely.

Timmins (2015) estimate a negative effect on housing prices of 16% within 1km of a shale gas extraction site for units whose water gets polluted from the extraction process. Currie et al. (2015) estimate a decrease in housing prices of 11% for properties located within 800m of industrial plants that emit toxic pollutants.

[13] 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 ($= 110 \times 30 \times 365$). For a population of some 2,300, this translates into 524 CHF per citizen per year. The representative housing unit in Switzerland hosts 2.2 individuals (SFSO, 2023); hence we obtain a willingness to pay of 1,152 CHF per year and housing unit. If instead we take the the mean center capacity of 95 (see Table A.1 Panel A), this amount even increases to 3,648 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.

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A Appendix

A.1 Summary Statistics

In Table A1 we report summary statistics for our sample of rental postings and hosting centers. Panel A lists characteristics of housing units advertised in our sample rental postings, Panel B lists variables relating to the status of the nearest hosting center by rental posting, and in Panel C we summarize the data on capacity and open spells of our sample hosting centers.

A.2 Crime Propensity Measure: Validation

In this Appendix we validate the measure of crime propensity used as our proxy for statistical discrimination. One possible issue is that, differently from $African$, our taste-based discrimination measure that is clearly visible for the local population, residents nearby an asylum center may not really realize if asylum seekers living there are more or less crime-prone.

To verify that our crime propensity variable proxies for the number of crimes committed locally by asylum seekers, we estimate the following Poisson regression model:

$$\begin{aligned}\ln \mathbb{E}(\lambda_{ms}) = \beta_1 Active_{ms} + \beta_2 (Active_{ms} \times \ln Capacity_{ms}) + \beta_3 (Active_{ms} \times Crime_{ms}) + \\ \mathbf{X}'_{ms}\mathbf{\Gamma} + \tau_m + \gamma_{y[s]} + \delta_{k[s]}.\end{aligned} \tag{A.1}$$

We report the estimates of the model in Table A3. The units of observation are municipalities, m, in which at least one asylum center of our sample is present. The dependent variable, $\ln \lambda_{ms}$, is the log number of crimes committed by asylum seekers (odd columns) or by the total population (even columns) in each municipality and month (s). The time span is determined by the availability of crime data (2009-2014). The variable $Active_{ms}$ takes the value of 1 if at least one center is active in municipality m and month s. The variable $Capacity_{ms}$ corresponds to the total installed capacity in the centers opened in municipality m during month s. The variable $Crime_{ms}$ takes the value of 1 if the asylum seekers' crime propensity in the center in municipality m during month s is higher than the median value in the sample. In the vector $\mathbf{X}_{ms}$ we gather other controls such as the natural logarithm of population in municipality m at time s and the interaction between center activity and the high African presence dummy, $Active \times African$, which we control for in columns (5) to (8) of Table A3. τ_m is a municipality fixed effect, $\gamma_{y[s]}$ is a year fixed effect, and , $\delta_{k[s]}$ is a calendar-month fixed effect (to filter out potential seasonality in criminal activity).

Our main coefficients of interest is β_3, which captures the effect of having crime-prone asylum seekers hosted in the center(s) located in a given municipality. The coefficient is positive and statistically significant only when we have as dependent variable the number of crimes perpetrated by asylum seekers. This confirms that our measure of crime propensity is a good proxy for statistical discrimination. Yet, one can notice that high presence of Sub-Saharan African individuals does not have any statistically significant effect on overall local crime.

A.3 Stacked Regression

As an alternative to our main specification (equation 1), in order to ease potential concerns that negative weights may affect and bias our main results, we replicate Figure 2 (Panel B) and Table 1 estimates using a stacked difference-in-differences approach (see Cengiz, Dube, Lindner and Zipperer 2019, and Deshpande and Li 2019). Even if with the fixed effect structure in equation 1 we compare housing units within each center, for those centers that open and close multiple times in our sample period (9 out of 91) we have the treatment event of opening happening at different points in time. This might bias our estimates in the case treatment effects are heterogeneous over time.[14] More precisely, the model we use is:

$$\begin{aligned}\ln(Rent)_{hemt} = \tilde{\alpha} + \tilde{\beta}(Active_{het} \times Prox_{he}) + \mathbf{H}'_h\mathbf{\Gamma} + \tilde{\theta}_e Active_{het} \\ + \tilde{\delta}_e Prox_{he} + \tilde{\lambda}_e + \tilde{\gamma}_m + \tilde{\tau}_{y[t]} + \tilde{\varepsilon}_{hemt}.\end{aligned} \tag{A.2}$$

The difference between equations 1 and A.2 is that in the latter we define an event e as the opening of a center and we include an event-specific fixed effects structure. In this way, for those centers opening and closing over our sample period we are implicitly defining different treatment and control groups of rental postings, stacking opening events and avoiding "forbidden comparisons" between already treated and newly treated housing units. For those centers which open (close) one time during our sample period, the units of observations' assignment to the event (opening or closing) coincides with the one of our main specification equation (1). In contrast, for those centers opening and closing multiple times we firstly define each opening (closure) as an event and then assign housing units to each event according to a criterion based on time proximity.[15] As one can observe from Tables A4 and A5, our results are very similar following this alternative approach.

A.4 Effects of African Share: Nonparametric Estimation

In order to explore the effect of the share of Sub-Saharan Africans on rental prices in greater detail, we conduct a nonparametric estimation by dividing the center-time-specific African-share measure into five segments. We estimate the following equation:

$$\begin{aligned}\ln(Rent)_h = \ln(Rent)_{hcmt} = \hat{\alpha} + \beta_0(Active_{hct} \times Prox_{hc}) + \mu_1(Active_{hct} \times Quintile1_{hct}) + \\ \beta_1(Active_{hct} \times Prox_{hc} \times Quintile1_{hct}) + \mu_2(Active_{hct} \times Quintile2_{hct}) + \\ \beta_2(Active_{hct} \times Prox_{hc} \times Quintile2_{hct}) + \mu_3(Active_{hct} \times Quintile3_{hct}) + \\ \beta_3(Active_{hct} \times Prox_{hc} \times Quintile3_{hct}) + \mu_4(Active_{hct} \times Quintile4_{hct}) + \\ \beta_4(Active_{hct} \times Prox_{hc} \times Quintile4_{hct}) + \hat{\theta}_c Active_{hct} + \hat{\delta}_c Prox_{hc} + \\ + \mathbf{H}'_h\mathbf{\Gamma} + \hat{\lambda}_c + \hat{\gamma}_m + \hat{\tau}_{y[t]} + \hat{\varepsilon}_{hcmt}\end{aligned} \tag{A.3}$$

[14] See e.g. Goodman-Bacon (2021) explaining the source of the bias due to the comparison of already treated with newly treated units in difference-in-differences regressions.

[15] More precisely, we compute the time between two consecutive events related to a given center, and we assign all housing units observed within the first half of this spell to the first event and all units observed in the second half of the spell to the second event.

This equation is an extended version of the regression we estimate in column 3 of Table 1. In Table 1 we include interactions with the binary variable $African$, which takes the value of one if the Sub-Saharan African population share in the nearest center at time t is above the median value in the sample (which equals 24%). Here, we instead include a set of dummy variables to explore possible nonlinearities. The variables $Quintile\#_{hct}$ are dummies set equal to one if the closest center to a given rental posting contains a share of Sub-Saharan African individuals above the #th quintile of African shares observed in the sample.[16] The sample African shares are 14% at the 1st quintile, 21% at the 2nd quintile, 28% at the 3rd quintile and 39% at the 4th quintile. Figure A1, where we report our estimated coefficients $\beta_1 - \beta_4$ of equation A.3, shows a statistically significant discontinuity around the 3rd quintile.

[16] Note that a given observation can correspond to several quintile dummies taking a value of one. If an observation is e.g. at the 45th percentile, it would result in the two dummy variables $Quintile1_{hct}$ and $Quintile2_{hct}$ both being set to one, while the $Quintile3_{hct}$ and $Quintile4_{hct}$ dummies would be set to zero.

A.5 Supplementary Tables

Table A1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: *Characteristics of rental postings*					
Yearly Rent per sqm (CHF)	279.7	94.0	15.7	2620	154,708
Surface (sqm)	80.2	37.0	11.0	1125	154,708
Standard (single-floor apartment)	0.820	0.384	0	1	154,708
Duplex	0.063	0.242	0	1	154,708
Attic	0.031	0.172	0	1	154,708
Studio	0.014	0.118	0	1	154,708
Furnished apartment	0.037	0.190	0	1	154,708
Terrace-apartment	0.002	0.046	0	1	154,708
Independent house (villa)	0.023	0.15	0	1	154,708
Row house	0.006	0.076	0	1	154,708
Semi-detached house	0.003	0.052	0	1	154,708
Farm	0.001	0.033	0	1	154,708
Other type of housing unit	0	0.022	0	1	154,708
Less than 2 rooms	0.131	0.338	0	1	154,708
2-2.5 rooms	0.211	0.408	0	1	154,708
3-3.5 rooms	0.323	0.468	0	1	154,708
4-4.5 rooms	0.245	0.430	0	1	154,708
5 rooms or more	0.090	0.286	0	1	154,708
Panel B: *Center-specific variables (by rental posting)*					
Dummy for location within 709 m of center ($Prox$)	0.246	0.431	0	1	154,708
Dummy for closest center being open ($Active$)	0.704	0.456	0	1	154,708
Share of sub-Saharan Africans in closest center ($African$)	0.277	0.168	0	1	154,708
Estimated crime propensity of population in closest center ($Crime$)	0.003	0.002	0	0.021	154,708
Average genetic distance of center population w.r.t. native Swiss population ($Gendist$)	0.019	0.007	0	0.051	154,708
Average bilateral genetic distance between local and center populations	0.013	0.004	0.003	0.027	153,578
Share of Muslim asylum seekers in closest center ($Muslim$)	0.557	0.156	0	1	154,682
Average education level of local population ($Education$)	0.163	0.047	0.071	0.289	154,708
Panel C: *Center characteristics*					
Hosting capacity	95	88	30	694	91
Duration of opening (years)	14	8	4	50	91

Notes: The table reports summary statistics for our sample of rental postings and centers. Characteristics of rental postings were obtained from the consulting firm meta-sys.ch. Information on asylum seekers ($African$, $Crime$, $Muslim$) was obtained from Couttenier et al. (2019), data on genetic distances ($Gendist$) are from Spolaore and Wacziarg (2018), and education are from the Federal Statistical Office. The descriptive statistics shown here are for the continuous version of those variables. In our estimations, we transform them into binary variables, using the median value as cutoff unless otherwise stated. The data on hosting center capacity and opening duration were obtained from 13 cantonal authorities or private bodies mandated by the cantons to manage the centers. For the list of included cantons, see Table A2.

Table A2: Number of Centers and Rental Postings per Canton

Canton	Centers	Rental postings
Aargau	21	22,405
Geneva	17	8,778
Glarus	1	603
Graubünden	4	9,053
Jura	2	501
Neuchâtel	2	581
Schaffhausen	2	3,441
Solothurn	3	576
Thurgau	2	5,895
Ticino	1	1,609
Valais	7	1,978
Vaud	8	25,514
Zurich	21	73,774
Total	91	154,708

Notes: The table reports the total number of centers and rental postings observed in the baseline estimation sample for each canton.

Table A3: Validation of Crime Propensity Measure

Dep. Variable	(1) Crime AS	(2) Crime All	(3) Crime AS	(4) Crime All	(5) Crime AS	(6) Crime All	(7) Crime AS	(8) Crime All
$Active$	-1.956	-1.950	-2.467	-1.864	-1.718	-1.832	-2.231	-1.753
	(2.311)	(2.101)	(2.221)	(1.897)	(2.309)	(2.091)	(2.209)	(1.900)
$Active \times \ln(Capacity)$	0.208	0.406	0.234	0.402	0.226	0.406	0.252	0.403
	(0.502)	(0.498)	(0.505)	(0.485)	(0.500)	(0.499)	(0.503)	(0.485)
$Active \times Crime$			0.521^{**}	-0.095			0.520^{**}	-0.092
			(0.251)	(0.228)			(0.251)	(0.228)
$Active \times African$					-0.380	-0.180	-0.376	-0.173
					(0.431)	(0.256)	(0.405)	(0.259)
Observations	4,184	4,184	4,184	4,184	4,184	4,184	4,184	4,184

Notes: For the estimations reported in this table, the unit of observation is a municipality-month, ms. The sample consists of the municipalities for which we have information on at least one hosting center. The dependent variable is the number of violent or property crimes committed by asylum seekers (full population) in odd (even) columns. In each column we control for the natural logarithm of the population living in the municipality at yearly frequency as well as for municipality, year and calendar month fixed effects. Clustered standard errors by municipality are reported in parentheses. The total number of municipalities (clusters) present in the sample is 59. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Stacked DiD: Baseline Estimates

Dep. variable	(1) ln(Rent)	(2) ln(Rent)	(3) ln(Rent)	(4) ln(Rent)
Radii in meters:	*baseline*			
treatment; spillover	709; 1,110	709; 1,425	500; 1,000	500; 1,500
$Active \times Prox$	-0.041***	-0.032***	-0.057***	-0.048***
	(0.012)	(0.011)	(0.016)	(0.016)
Observations	154,707	106,833	137,010	78,277
R^2	0.453	0.446	0.456	0.450

Notes: For the estimations reported in this table, 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. In each column we control for a set of housing characteristics (described in Section 1.2.1), year and municipality fixed effects, as well as the fixed effects to exploit within event variation $\tilde{\theta}_e Active_{het}$, $\tilde{\delta}_e Prox_{he}$ and $\tilde{\lambda_e}$ in accordance with equation A.2. The fixed-effects structure we impose compares rental postings within each event of center opening (closing) (stacked DiD, see Appendix A.3 for further details). Clustered standard errors by municipality are reported in parentheses. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Stacked DiD: 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
$Active \times Prox$	-0.0409***	-0.0335***	-0.0282***	-0.0254**
	(0.0116)	(0.0121)	(0.0101)	(0.0102)
$Active \times Crime$		0.0060		0.0057
		(0.0046)		(0.0052)
$Active \times Prox \times Crime$		-0.0115**		-0.0061
		(0.0058)		(0.0059)
$Active \times African$			0.0006	-0.0006
			(0.0087)	(0.0087)
$Active \times Prox \times African$			-0.0247***	-0.0227***
			(0.0050)	(0.0059)
Observations	154,707	154,707	154,707	154,707
R^2	0.453	0.453	0.453	0.453

Notes: For the estimations reported in this table, 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. In each column we control for a set of housing characteristics (described in Section 1.2.1), year and municipality fixed effects, as well as the fixed effects to exploit within event variation $\tilde{\theta}_e Active_{het}$, $\tilde{\delta}_e Prox_{he}$ and $\tilde{\lambda_e}$ in accordance with equation A.2. The fixed-effects structure we impose compares rental postings within each event of center opening (closing) (stacked DiD, see Appendix A.3 for further details). Clustered standard errors by municipality are reported in parentheses. The sample number of municipalities (clusters) is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Alternative Dimensions of Heterogeneity: Muslim and Genetic Distance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Variable	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)	ln(Rent)
Effect	Crime	Muslim	Genetic	African	Genetic-Crime	Genetic-Muslim	African-Crime	African-Muslim	Genetic-Crime-Muslim	African-Crime-Muslim
$Active \times Prox$	-0.0314**	-0.0378***	-0.0279***	-0.0266**	-0.0254**	-0.0259**	-0.0239**	-0.0246**	-0.0233**	-0.0217**
	(0.0129)	(0.0118)	(0.0107)	(0.0107)	(0.0110)	(0.0108)	(0.0108)	(0.0106)	(0.0109)	(0.0106)
$Active \times Crime$	0.0067				0.0068		0.0065		0.0071	0.0067
	(0.0045)				(0.0049)		(0.0051)		(0.0049)	(0.0052)
$Active \times Prox \times Crime$	-0.0107*				-0.0053		-0.0058		-0.0056	-0.0061
	(0.0057)				(0.0059)		(0.0059)		(0.0057)	(0.0058)
$Active \times Muslim$		-0.0023				-0.0030		-0.0024	-0.0033	-0.0028
		(0.0055)				(0.0049)		(0.0049)	(0.0048)	(0.0048)
$Active \times Prox \times Muslim$		-0.0011				-0.0039		-0.0041	-0.0038	-0.0040
		(0.0087)				(0.0089)		(0.0084)	(0.0085)	(0.0081)
$Active \times Genetic$			-0.0024		-0.0038	-0.0029			-0.0044	
			(0.0081)		(0.0077)	(0.0078)			(0.0075)	
$Active \times Prox \times Genetic$			-0.0201***		-0.0184***	-0.0209***			-0.0190***	
			(0.0053)		(0.0064)	(0.0061)			(0.0072)	
$Active \times African$				0.0008			-0.0006	0.0003		-0.0012
				(0.0084)			(0.0083)	(0.0081)		(0.0080)
$Active \times Prox \times African$				-0.0221***			-0.0203***	-0.0229***		-0.0209***
				(0.0049)			(0.0059)	(0.0053)		(0.0063)
Observations	154,708	154,682	154,708	154,708	154,708	154,682	154,708	154,682	154,682	154,682
R^2	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453	0.453

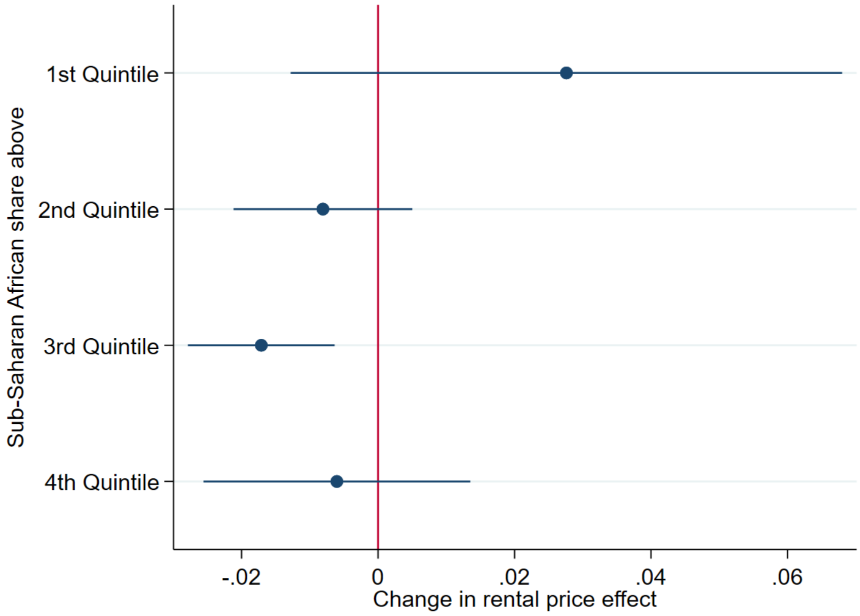
Notes: For the estimations reported in this table, the unit of observation is a rental posting, $h = hcmt$. The sample covers rental postings for housing units located within 2km of a hosting center, for the period 2004-2014. In each column we control for a set of housing characteristics (described in Section 1.2.1), 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 equation (1). Clustered standard errors by municipality are reported in parentheses. The sample number of municipalities (clusters) is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Alternative Cutoffs for Binary Variables

Dep. Variable	(1) Ln Rent	(2) Ln Rent	(3) Ln Rent	(4) Ln Rent	(5) Ln Rent	(6) Ln Rent
Effect	Perc 50	Perc 60	Perc 75	Perc 50	Perc 60	Perc 75
$Active \times Prox$	-0.0266**	-0.0302***	-0.0313***	-0.0239**	-0.0283***	-0.0309***
	(0.0107)	(0.0105)	(0.0119)	(0.0108)	(0.0107)	(0.0113)
$Active \times African50$	0.0008			-0.0006		
	(0.0084)			(0.0083)		
$Active \times Prox \times African50$	-0.0221***			-0.0203***		
	(0.0049)			(0.0059)		
$Active \times African60$		0.0079			0.0065	
		(0.0070)			(0.0070)	
$Active \times Prox \times African60$		-0.0188***			-0.0178**	
		(0.0054)			(0.0072)	
$Active \times African75$			0.0212			0.0231
			(0.0135)			(0.0146)
$Active \times Prox \times African75$			-0.0214**			-0.0204*
			(0.0103)			(0.0119)
$Active \times Crime50$				0.0065		
				(0.0051)		
$Active \times Prox \times Crime50$				-0.0058		
				(0.0059)		
$Active \times Crime60$					0.0073	
					(0.0063)	
$Active \times Prox \times Crime60$					-0.0039	
					(0.0067)	
$Active \times Crime75$						-0.0096
						(0.0063)
$Active \times Prox \times Crime75$						-0.0015
						(0.0075)
Observations	154,708	154,708	154,708	154,708	154,708	154,708
R^2	0.453	0.453	0.453	0.453	0.453	0.453

Notes: This table reports estimates for different variants of the binary variables $African$ and $Crime$, where the suffix '50' denotes the sample median as the cutoff value, '60' denotes the 60th percentile, etc. These estimations serve as robustness tests of the results shown in columns (3) and (4) of Table 1. For the estimations reported in this table, 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. In each column we control for a set of housing characteristics (described in Section 1.2.1), year and municipality fixed effects, as well as including fixed effects to exploit within-center variation $\theta_c Active_{hct}$, $\delta_c Prox_{hc}$ and λ_c in accordance with equation (1). Clustered standard errors by municipality are reported in parentheses. The sample number of municipalities (clusters) is 192. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Effects of African Share: Nonparametric Estimation



Notes: The figure shows triple interaction estimates from equation A.3. For example, the coefficient for ' 1st Quintile' the marginal change in rental price effect when the Sub-Saharan African share of the nearest center rises above the first quintile of Sub-Saharan African shares observed in the sample. The baseline (omitted) category contains rental postings with Sub-Saharan African shares of their nearest hosting centers below the 20th percentile.