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Equal price for equal place? Demand-driven racial discrimination in the housing market☆

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ABSTRACT

We presented participants to an online study in Luxembourg with fictitious real-estate advertisements, tasking them to appraise the described properties. A random subset was also shown sellers' surnames, strongly framed to signal their origins. All else equal, sellers with sub-Saharan African surnames were systematically offered lower prices — amounting to an appraisal penalty of EUR 20,000. This figure is highly heterogeneous and can amount up to around EUR 58,000 for older and low-educated participants. We provide evidence that the appraisal bias likely passes through onto final sales prices and that it may be largely due to statistical rather than taste-based discrimination.

1. Introduction

Many governments have armed themselves with legal tools to protect minorities from discrimination in the housing market. One week after the death of Dr. Martin Luther King Jr. in 1968, the United States enacted the Civil Rights Act, of which Titles VIII and IX are commonly known as the *Fair Housing Act*. More recently, at the turn of the millennium, the European Union too has put in place a *Racial Equality Directive* to prohibit all forms of discrimination. Despite the existence of these legislative apparatuses, there is evidence of discrimination from institutional forces (Howell and Korver-Glenn, 2018; Kopkin, 2018; Bhutta and Hizmo, 2021; Park, 2021; Bartlett et al., 2022) partly contributing to the existing racial wealth gap both at the extensive margin, as reflected by the racial gaps in ownership rates (Goodman and Mayer, 2018), and at the intensive margin (Kuhn et al., 2020).

The objective of this paper is to investigate whether discrimination from the demand side of the housing market contributes to these racial gaps. Specifically, we examine whether a potential buyer's initial property appraisal is affected by the seller's race. This is important as a buyer's initial appraisal is indicative of their willingness-to-pay, which in turn reverberates on transaction prices (Howell and Korver-Glenn, 2018).[1] With real estate comprising one-third of global private wealth (Syz, 2008), investigating the prevalence of discriminatory appraisals by buyers in the housing market is hence crucial.

Standard housing transaction data provide limited assistance in addressing this research question for several reasons. First, they only

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[1] This echoes a recent lawsuit in California, where a Black couple sued a property appraiser for undervaluing their home. See https://www.theguardian.com/us-news/2023/mar/08/black-couple-house-value-discrimination-lawsuit, last accessed: 18 August 2023.

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represent a non-random sample of the housing stock, i.e., those properties that are the subject of a transaction. More importantly, the relationship between the seller's race and the price of a property may potentially confound the impact of factors that are not visible to the researcher. For example, patterns of geographical segregation of certain racial or ethnic groups makes it hard to cleanly disentangle the effect of sellers' (or buyers') race from overall neighbourhood characteristics on home prices, culprit the lack of an appropriate counterfactual. Last, in practice, it is uncommon to have access to demographic information in general, and on ethnic or racial background in particular, alongside actual property prices. To overcome these issues, we drew from the literature on correspondence studies (e.g., Bertrand and Mullainathan, 2004; Giulietti et al., 2019) and conducted an online experiment on discrimination and house price appraisals in Luxembourg.

Participants were asked to act as potential buyers and assign a value to four properties based on fictitious real-estate advertisements (adverts, from here onward). For half of our sample, the adverts were also accompanied by the contact details of a fictitious current owner, whose surname was primed to reflect a specific region or country of origin. As the sample we solicited was made up of Luxembourg residents, we used four surnames that are strongly associated with specific population groups present in Luxembourg: French and German/Luxembourgish, representing the majority of natives and cross-border migrants; Portuguese, the largest and long-established migrant group; and sub-Saharan African, one rather new and among the fastest growing migrant groups in Luxembourg. The main features of the experiment (i.e., the treatment assignment, the matching of adverts to the owners' names and their order of appearance) were all randomised to allow us identifying whether, all else equal, a name suggesting a foreign origin is sufficient to induce a reduction in the price appraisal of a dwelling.

While no appraisal differences are found for the French, German and Portuguese surnames, our most conservative figures suggest a penalty of 3.1% associated with the sub-Saharan African surname — a price reduction of about EUR 20,000. Nevertheless, the appraisal bias is far from uniform among respondents: for some it is null, while for others it amounts to roughly EUR 58,000. This heterogeneity can be linked to socio-economic characteristics of the bidder: on average, older and low-educated participants are associated with higher penalties. Contrary to what Agarwal et al. (2019a) found in Singapore and Deng et al. (2021) for Sydney, we do not observe an in-group premium (or out-of-group penalty).

While we are aware of the inherent limitations of online survey experiments, we use insights from Luxembourgish statistics, laws and institutions to argue that this appraisal penalty is likely to have important economic implications for both the buyer and the seller. Importantly, we argue that there is little reason to believe that the racial penalty in appraisals would not pass through to the final sales price: among other things, we show that the appraisal penalty is also found among the highest bidders — those who would be most likely to seal the deal in actual transactions. We last discuss our results in light of economic theories of animus-based and statistical discrimination. While we cannot formally rule in nor out the former, we provide evidence in favour of statistical discrimination.

Our paper makes five main contributions to the literature. First, there is limited evidence documenting discrimination stemming from the demand side of the housing market.[2] By showing discrimination from the side of (experimental) buyers, our paper thus complements a literature dominated by analyses illustrating supply-side discrimination via landlords (Hanson and Hawley, 2011; Ewens et al., 2014; Edelman et al., 2017; Christensen et al., 2022; Bao, 2024), owners (Agarwal et al., 2019a,b) and real-estate agents (Yinger, 1986; Christensen and Timmins, 2022; Hanson and Hawley, 2023). Second, we augment the existing body of work on demand-driven discrimination that has focused so far on the labour market (Bertrand and Mullainathan, 2004; Weber and Zulehner, 2014; Lang and Spitzer, 2020; Mudiriza and Edwards, 2021) or daily-life transactions, such as Ebay auctions (Doleac and Stein, 2013; Ayres et al., 2015), cars (Zussman, 2013) and tipping behaviour (Brewster and Lynn, 2014), rather than on the housing market. Third, while there is an existing body of literature documenting racial discrimination in the housing market across Europe (Ahmed and Hammarstedt, 2008; Bosch et al., 2010; Baldini and Federici, 2011; Drydakis, 2011; Carlsson and Eriksson, 2014; Acolin et al., 2016; Heylen and Van den Broeck, 2016; Auspurg et al., 2017; Le Gallo et al., 2019; Chareyron et al., 2023), no research has yet addressed this issue in Luxembourg to our knowledge. Our paper fills this gap by providing new evidence from this country, which has unique immigration dynamics compared to other European nations. Luxembourg stands out as one of the most multicultural and diverse developed economies, with nearly half of its population being foreign-born. Its reputation as a tolerant and inclusive society suggests that if racial discrimination exists in the Luxembourg housing market, such bias might be even more pronounced in countries with less diverse populations or a history of inter-group antagonism. This context highlights the broader relevance of our analysis, which could represent a lower bound for the magnitude of racial penalties in housing markets elsewhere. Fourth, we take a different approach from most experimental studies on discrimination in the housing market. Instead of measuring call-back rates to online advertisements, we conduct a survey experiment. This approach allows us to collect detailed socio-economic and demographic information from respondents, enabling us to systematically study the characteristics associated with discriminatory behaviour. This is particularly important for developing targeted policy recommendations. Last, our study complements the literature on the explanatory factors of racial inequalities in the housing market. Faber and Ellen (2016) conclude that these inequalities can be partly explained by differences in income, education and types of property, while others focused on the role of institutional actors, such as banks (Kopkin, 2018; Bhutta and Hizmo, 2021; Park, 2021; Bartlett et al., 2022) or private appraisal companies (Howell and Korver-Glenn, 2018). Our paper suggests that another explanation can be found in differences in buyers' appraisals based on the seller's origin.

The remainder of the paper is organised as follows. Section 2 describes the Luxembourgish context, first by summarising its history and migration background and then presenting some descriptive facts on racial discrimination in the country. The experimental set-up is laid out in Section 3. Section 4 features a presentation of our results and a discussion of their economic consequences and their underlying mechanisms. Finally, Section 5 concludes. A comprehensive appendix provides supplemental details.

2. Luxembourg: History and migration background

2.1. General facts and population trends

The Grand Duchy of Luxembourg is a small country of around 2500 km^2 located in the heart of Europe bordering France, Belgium and Germany. Independent from its neighbouring kingdoms since the 19th century, Luxembourg has historically played a central role in the European geo-political landscape and is one of the founding members of the United Nations and the European Union. Luxembourg is a representative democracy and the only remaining sovereign Grand Duchy in the world. Its capital, Luxembourg City, is one of the four institutional seats of the EU. With a GDP per capita of over 135,000 current USD in 2021,[3] Luxembourg is one of the wealthiest countries in the world,

[2] The study by Laouénan and Rathelot (2022) is the most closely related to our work, as it documents demand-side discrimination against ethnic minorities. However, it focuses on hosts on Airbnb.

[3] World Bank national accounts data: https://data.worldbank.org/indicator/NY.GDP.PCAP.CD?locations=LU, last accessed: 7 November 2022.

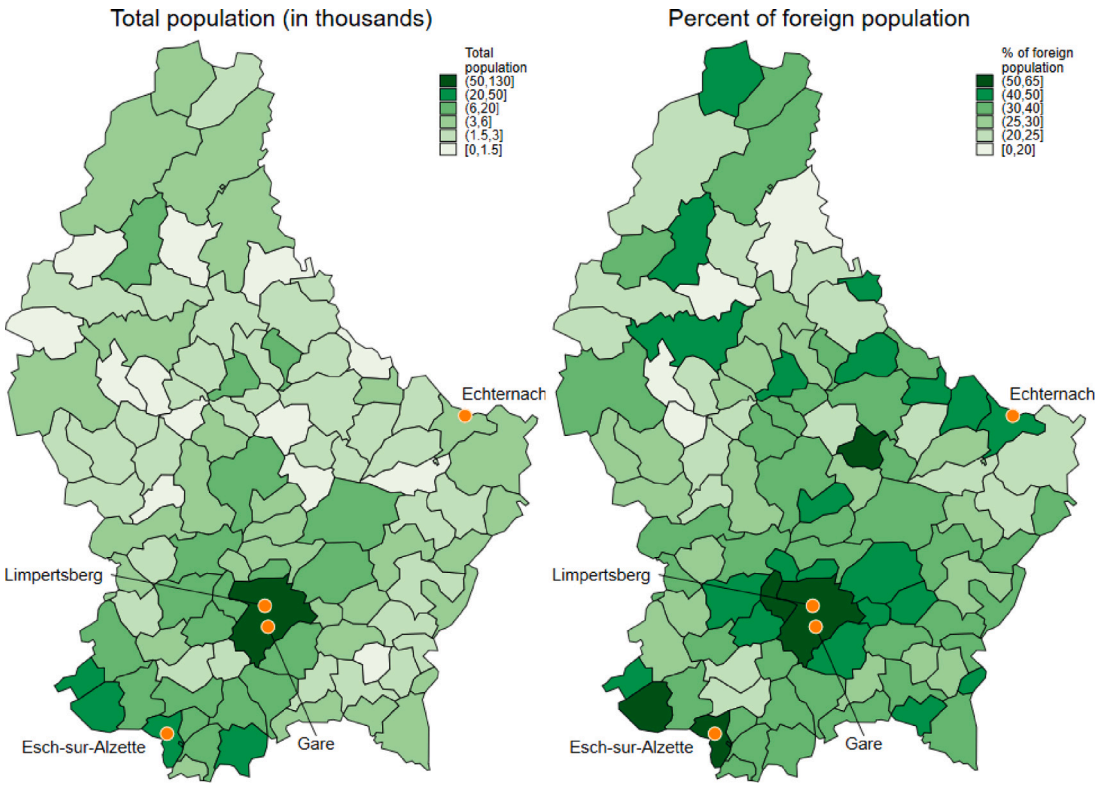


Fig. 1. Local and foreign population in Luxembourg by municipality.
Notes: The green shaded areas represent municipalities in Luxembourg (Local Administrative Units in the European Statistical System). The four orange dots indicate the locations used in the experiment, namely city centres of the municipalities of Echternach and Esch-sur-Alzette and Luxembourg City neighbourhoods of Gare and Limpertsberg (exact locations correspond to centroids of the map circles displayed in Fig. 2). Total population figures are expressed in thousands and refer to 2022 figures. The percent of foreign population is based on population statistics from the 2011 Census. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: STATEC and CTIE.

owing its fortune to the development of a prosperous steel industry in the early 20th century and, in more recent times, to the banking sector (Allegrezza, 2016).

Luxembourg's population, counting over 643,000 residents according to the 2021 Census, is highly diverse and currently the most 'international' among the OECD countries: the share of foreign-born residents is just shy of 50% — substantially above the OECD average of 14.5%.[4] Fig. 1 shows the geographical distribution of the total and foreign population in Luxembourg. The orange dots in the map indicate the locations that will be the object of our housing experiment: the neighbourhoods Gare and Limpertsberg in Luxembourg City; Esch-sur-Alzette, a city bordering France; and Echternach, a small town bordering Germany. As shown in the left panel of Fig. 1, the population of Luxembourg is geographically concentrated around the capital (Luxembourg City) and the southern municipalities (such as Esch-sur-Alzette, the second largest city). While the right panel of Fig. 1 shows that the share of foreign population is quite high almost everywhere in the country, the most populated areas tend to also display the largest share of foreigners (above 50%).

Luxembourg's diversity is rooted in large-scale economic immigration driven by the country's economic success. Since the 1960s, the foreign share of residents has steadily increased, with some slowdowns during the Great Recession and the Covid-19 crisis (Figure A1). Historically, Luxembourg relied on low-skilled labour from Germany, and later Italy (Cordeiro, 1976). Italian immigration surged after World War II, making Italians 7% of the population by 1970. Shortly after, due to Salazar's authoritarian regime, Portuguese migration took over, enhanced by agreements regularising migrants' status and promoting family reunification.[5] Today, Portuguese nationals represent 14.5% of the total population.

In recent years, the growth of first-generation Portuguese migrants has slowed (Figure A2). While the Luxembourgish, Portuguese, and German populations have remained stable, migration from France has surged by 60% since 2011. Additionally, sub-Saharan African migration, though relatively new, has almost doubled in the last decade, making it one of Luxembourg's fastest-growing inflows. Figure A3 breaks down the sub-Saharan African population growth shown in Figure A2. Cape Verde, formerly part of Portugal, follows the same migration trend as the Portuguese, though its significance is declining. Cameroon is now the second-largest sub-Saharan group, followed by Niger and the Democratic Republic of Congo.

2.2. Diversity and attitudes towards foreigners in Luxembourg

Despite (or, perhaps, thanks to) its multiculturalism, Luxembourg is one of the few countries in Western Europe where the support for populist and far-right parties, which usually channel anti-immigration sentiments, remains limited. For example, *Alternativ Demokratesch Reformpartei*, the most conservative of Luxembourg's parties, received 9.3% of the votes in the 2023 general election.

[4] OECD data on the foreign-born population in 2019: https://data.oecd.org/migration/foreign-born-population.htm, last accessed: 5 October 2022.

[5] Law of March 28th 1972 on the entry and stay of foreigners, medical control, and employment of foreign labor (Mémorial, Partie A, 1972-04-13, n. 24, pp. 818–823). The law significantly impacted the Portuguese before Portugal joined the EEC in 1986.

Given its political, socio-economic and cultural context, Luxembourg is a particularly interesting country to study racial discrimination, which one might imagine to be low or even non-existent. Luxembourg has been described as an 'immigration success story' (Fetzer, 2011). Luxembourgers also tend to display more open-minded attitudes towards immigrants as compared to other European nationals. In Appendix A, we summarise evidence from the European Values Study (EVS, 2022), a repeated cross-sectional survey across Europe covering individuals' beliefs, values and opinions on a variety of subjects. Figures A4 and A5 display average opinions and feelings about immigrants in Luxembourg and its neighbouring countries, surveyed in the 2008–2009 EVS wave. Overall respondents in Luxembourg are more open towards immigrants than their neighbours according to almost all of the dimensions documented in the Figures, especially when it comes to social integration aspects that have proved successful in Luxembourg (e.g., strain to the welfare system, societal order, job competition).[6]

While Luxembourgers display friendlier attitudes towards immigrants as compared to their neighbours, these might not be the same irrespective of the provenience of immigrants. For example, similar to other countries in Central Europe, feelings towards non-European immigrants in Luxembourg are more often negative than feelings towards European immigrants, as discussed by De Jonge (2021) using data from the 2018 Eurobarometer survey. A report from the European Union Agency for Fundamental Rights (FRA, 2018) documents that people of African descent in Luxembourg are severely discriminated against, regardless of their migration status.[7] 50% of Black people in Luxembourg felt discriminated against in the previous year because of their ethnic or immigrant background — the highest number in the EU. More than one third of Luxembourgish residents of African descent felt discriminated against in access to housing in the five years prior to the survey (the second highest number in the EU), with 28% stating that they were prevented from renting an accommodation from a private landlord because of their ethnic origin. When it comes to homeownership, 20% of Black people in Luxembourg are owner-occupiers, a figure that is 5 percentage points higher than the EU average. No information is available, to the best of our knowledge, on racial discrimination from the demand side of the housing market in Luxembourg (that is, whether perspective renters or buyers discriminate on the basis of the owner's race or ethnic origin).

3. Experimental set-up

3.1. The need for an experimental setup

There are two challenges in measuring discrimination in the European housing market. First, official statistics in Luxembourg – just as in many other European countries – do not collect information on race. Second, sellers' demographic characteristics are not generally captured by transaction records. Notary deeds, as well as real estate advertisements (the primary sources for real estate data), provide little to no demographic information on the seller or buyer. Furthermore, estimating discriminatory behaviours parametrically through regression models (via the inclusion of a race indicator whenever available) could lead to biased interpretations due to omitted variables. Real-estate characteristics that are observed by the buyer but not by the researcher may be correlated with race: one example is geographic segregation, whereby individuals of a given race are more likely to be over- or under-represented in certain locations. Previous research has shown that the racial composition of an area significantly affects property appraisals (Howell and Korver-Glenn, 2018). Thus, even if the researcher attempts to include variables capturing the objective quality of the property, it would be impossible to determine whether a buyer penalised a seller solely on the basis of race.

The correspondence method was developed to overcome similar limitations and popularised in Economics by Bertrand and Mullainathan (2004). This method creates the conditions for an exchange between a real individual and a fictitious person whose traits can be modified in a controlled and fully observable way. Discrimination is detected if, all other things being equal, a trait identifying a minority is systematically associated with a different outcome.

Our aim is to find out whether the appraisals of real-estate properties depend on the implicitly assumed race of the seller. We therefore take inspiration from the correspondence literature and randomly pair fictitious property adverts with different owner profiles, each featuring a surname that is framed to evoke a country of origin or a specific racial background (Niggli, 2023; Ehrl and Monasterio, 2024). The details of our experimental setup are laid out below.

3.2. Core of the experiment

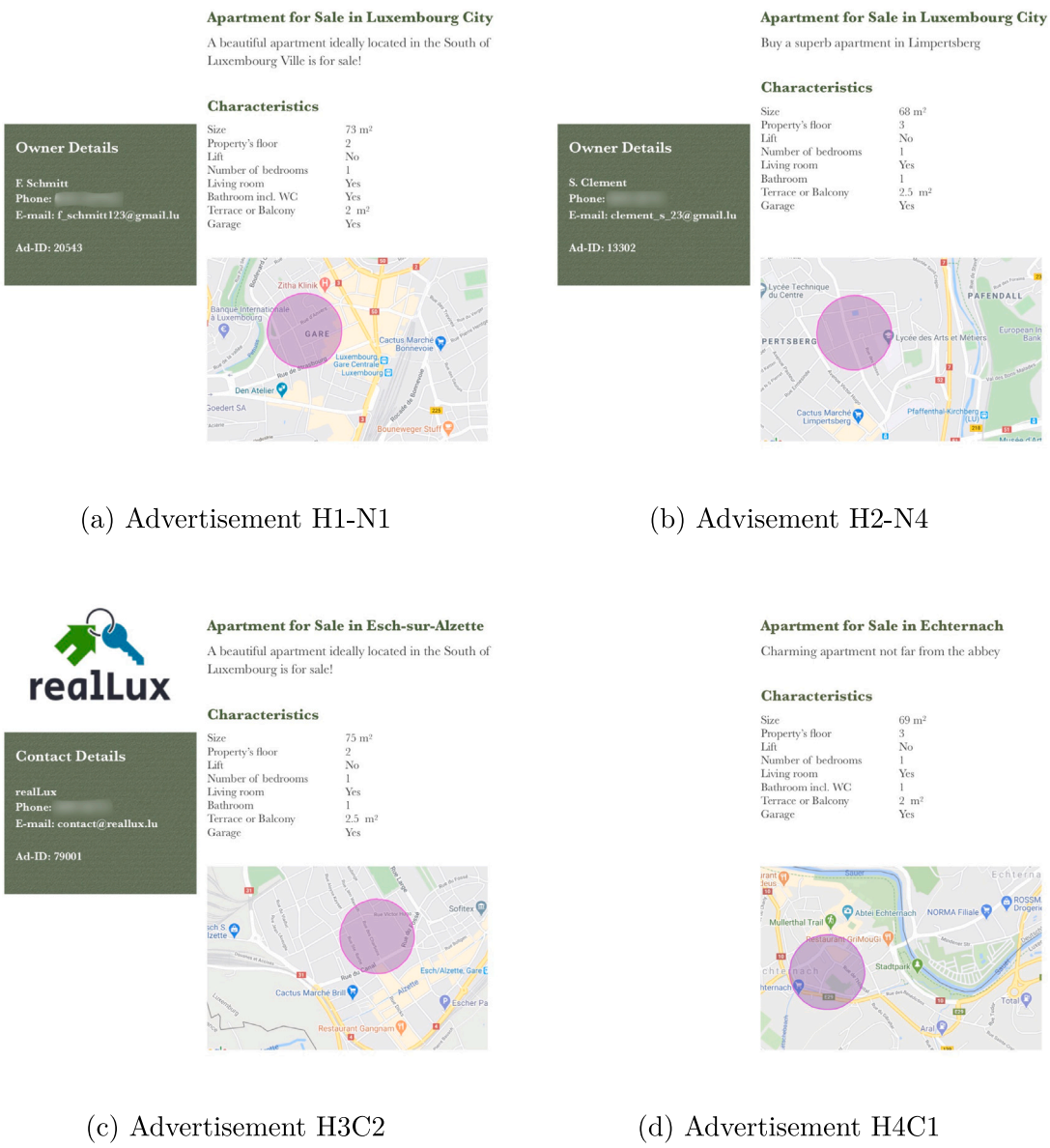
Our experimental module was administered as part of an online survey on housing market conditions and related perceptions of Luxembourgish residents (Waltl, 2021). At the beginning of the survey, every participant is sequentially shown the same set of fictitious real-estate adverts. Each advert refers to one of four different dwellings (*H1*–*H4*), each with a specific location and set of physical characteristics. All dwellings share some common features: they are all apartments in multi-storey buildings featuring one bedroom, one bathroom, a living room, a garage and a terrace/balcony; no lift is present in the building. Other characteristics (namely, the location, the floor, the living area, and the size of the terrace/balcony) differ across *H1* to *H4* and are summarised in Fig. 2 and Table A1. Except for the location, all other differences between apartment listings are rather minor and are set to not trigger relevant pricing differences. Examples of the adverts are shown in Fig. 2, each featuring their approximate location on Google Maps via a circle overlapping the exact address. The selected locations are shown in Fig. 1 and correspond to two neighbourhoods of Luxembourg City (Gare, around the central train-station, is a rapidly growing and diverse neighbourhood; Limpertsberg, in the north-west of the city, is a residential neighbourhood), the city centre of Esch-sur-Alzette (the second largest city of Luxembourg, with a strong industrial heritage and diverse population, bordering with France), and the city centre of Echternach (a smaller city in the north-east of the country, bordering with Germany).

As argued in Section 3.1, we are primarily interested in uncovering discrimination in housing appraisals. We do so by randomly assigning participants either into a treatment group *T* or one of two control groups (*C1* and *C2*). Half of our sample is assigned to group *T*, in which respondents are not only shown the real-estate adverts but also the name and contact details of a current (fake) owner who is willing to sell. Participants are shown four owner profiles (*N1* to *N4*), strongly framed to reflect different origins. To ensure that framing effects do not confound other sources of bias (e.g., gender or age) we leave out the owner's first name and only display a neutral initial. The initial and surnames are additionally mirrored by private, fictitious, email addresses.[8] Table 1 summarises the owner profiles, *N1* to *N4*. Owners *N1* and *N4*, respectively, have Luxembourgish/German- and French-sounding surnames, with their implied country of origin being either

[6] As compared to Luxembourgish EVS respondents, the French report slightly lower support for the following statements: "Immigrants increase crime problems" and "Dislike immigrants/foreign workers as neighbours"; and German respondents relate less often to the statement "Feeling like a stranger in my own country".

[7] Although the report calls for caution in the interpretation of results for Luxembourg: due to the lack of access to the sampling register in this country, the FRA applied quota sampling.

[8] The email address, as well as the provided phone number (blurred here), were non-existing when we set up the experiment. The contact box also contains a random, meaningless "Advert-ID" number.



(a) Advertisement H1-N1

(b) Advisement H2-N4

(c) Advertisement H3C2

(d) Advertisement H4C1

Fig. 2. Selected Adverts.
Notes: The French and German versions of the adverts are documented in Appendix B. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Luxembourg or one of its neighbouring countries (Belgium, France, and Germany). Due to the large presence of first- and second-generation immigrants from Portugal in Luxembourg (see Section 2) owner *N2* is attributed a Portuguese-sounding surname. Mirroring more recent migration flows, owner *N3* has a surname that implies provenience from a sub-Saharan African country.[9]

Different from the treatment group *T*, individuals in the control groups are shown real-estate adverts which do not feature the owner's contact details. Untreated individuals (50% of the sample) are randomly assigned with equal probabilities to either one of two control groups: a 'passive' control group, *C1*, and an 'active' control group, *C2* (Stantcheva, 2023). Respondents assigned to *C1* are exclusively shown apartment features, leaving out any information on the seller (see panel (d) of Fig. 2). Respondents assigned to control group *C2* are shown adverts that contain both house characteristics *and* the contact details of a fictitious real-estate agency in Luxembourg (see panel (c) of Fig. 2). We use two agencies with similar names and

Table 1
Owner profiles in the treatment group.

	Name	e-mail address	Implied Origin
N1	F. Schmitt	f_schmitt123@gmail.lu	German/Luxembourgish
N2	G. daSilva	G_Da_Silva@gmail.lu	Portuguese
N3	A. Mutombo	a_789_mutombo@gmail.lu	Sub-Saharan African
N4	S. Clement	clement_s_23@gmail.lu	Belgian/French

Notes: The adverts with the owner's contact details (administered to the treatment group) also contain a Luxembourgish phone number. Typical native Luxembourgish names consist of a French-sounding first name and a German-sounding surname. In our setting, German and Luxembourgish identities are hence indistinguishable. 'Mutombo' is a common name in several sub-Saharan countries, such as the Democratic Republic of the Congo.

[9] According to surname data collected on the Forebears genealogy portal (https://forebears.io/surnames/), 'DaSilva' is the most frequent Portuguese surname in Luxembourg and 'Mutombo' is a common name in several sub-Saharan countries, such as the Democratic Republic of Congo and Mozambique.

branding: LUXhouse and realLux (see the logos in Figure A6).[10] The inclusion of a second control group *C2* finds its rationale in the fact that the mere presence of contact details could affect respondents' pricing behaviour, e.g., by lowering their attention due to the larger amount of information to process. Thus, any differences between individuals in *T* and *C1* could be simply be due to group *T* being exposed to a larger information set (house characteristics plus the green box with contact details, see Fig. 2). It could also be argued that the degree of trust in real-estate agencies may also systematically affect the behaviour of the respondents in *C2*. When comparing price estimates in *C1* and *C2*, we do not observe any statistically significant differences across the two groups (see Appendix Table A2, which reports the effect of being assigned to *C2* as opposed to *C1* on house prices). Accordingly, in the main analysis, we will pool observations from *C1* and *C2* into a single control group to maximise statistical power.

After showing each real-estate advert, we asked participants to put themselves into the shoes of perspective buyers by asking them the following question: *"In your opinion, how much is this home worth? In other words, how much would you pay for purchasing it today?"*. Each respondent was also tasked to forecast the price in five years. We use the latter appraisal to perform a persistence test in Section 4.3.

To sum up, we applied the following randomisation strategy to isolate the causal effect of potential discrimination related to a sub-Saharan African name. First, each individual is randomly assigned to either treatment group *T*, control group *C1* or control group *C2*. The assignment probabilities for each of these groups are 50%, 25% and 25% respectively. Second, the sequence of appearance of the adverts featuring *H1* to *H4* is randomised, as are the pairings of owners' names (for treatment group *T*) and agency names (for control group *C2*) with the adverts.

3.3. Experimental details and sampling procedure

The survey and experimental module were scripted through the platform `Lioness`, based on JavaScript (Giamattei et al., 2020). Participants received an invitation link and filled the questionnaire using their preferred device and web-browser. Access was locked after first clicking on the link.

Participants to the study can be grouped into two complementary recruitment pools: the EU-SILC sample and a social media sample. The first is based on participants to the Luxembourg section of the EU-SILC (EU Statistics on Income and Living Conditions) survey fielded in 2019. Each surveyed person had the opportunity to register for being contacted to participate in future experimental studies conducted by the Luxembourg Institute of Socio-Economic Research (LISER). The sample was drawn to be representative of the adult population registered in the national social security system and residing in Luxembourg. To top up the sample, we ran social media campaigns on Facebook, Instagram and Twitter via sponsored advertisements (see Figure A7 for a sample screenshot), targeting Luxembourgish residents.[11] Additionally, a Luxembourgish online news outlet wrote about this campaign, further popularising participation. As revealed by Appendix Table A3, participants in the two samples have different observable characteristics. For example, participants sampled via social-media campaign are on average 7.5 years younger and better educated. We will discuss the implications of such differences for our findings in more detail in Section 4.2.

To recruit participants, we took into account the multilingual reality of Luxembourg and have thus offered English, French and German versions of our questionnaire. Respondents could pick the language they felt most comfortable with upon starting the survey (see the welcome screen in Figure A8). French and German translations of the key questions and adverts are reported in Appendix B.[12] The study was online between 9 February 2022 and 22 May 2022. Respondents received a compensation for participating in the study, with the incentive being communicated upon recruitment and repeated upon starting the survey to avoid drop-outs. They could choose between a fixed payment of EUR 10 or participation in a lottery with a 20% chance of winning EUR 50 and an 80% chance of receiving nothing. The two options are worth the same in expectation. Wins were payed out in the form of vouchers useable on Letzshop, a popular e-commerce platform in Luxembourg offering products and services by local vendors.

3.4. Econometric strategy

Thanks to our experimental setting, we can estimate the following models to detect discriminatory real estate appraisals:

$$\log(P_{i,n,h}) = \alpha + \beta_n \mathbb{1}(N_i = n) + \theta C_i + \gamma_h \mathbb{1}(H_i = h) + \delta X_i + \lambda_s + \varepsilon_{i,n,h}, \quad (1)$$

and

$$\log(P_{i,n,h}) = \alpha + \beta_n \mathbb{1}(N_i = n) + \gamma_h \mathbb{1}(H_i = h) + \mu_i + \varepsilon_{i,n,h}. \quad (2)$$

In both Equations, $P_{i,n,h}$ is individual i's appraisal (expressed in EUR) of property h offered by owner n. N_i and H_i are discrete random variables respectively modelling owner names and properties. Their realisations are correspondingly indicated with n and h. The vector of coefficients β_n captures discriminatory appraisal bias by reflecting how different surnames may systematically influence assessed prices. Significant differences among the β_n coefficients would indicate the presence of discrimination, suggesting that certain surnames are disproportionately penalised in the appraisal process, holding all other factors constant.

In Eq. (1), we include the variable C_i, which is a dummy indicating whether respondents were exposed to adverts without private sellers' names (i.e., control groups *C1* and *C2*) or not, and by that captures any systematic difference in appraisals arising from the absence of seller-specific information in the adverts. For example, respondents may believe that properties that are directly sold by the current owner – without the brokerage of a real-estate agent – are on average of lower quality. In this case, all owner names should attract similar negative coefficients and θ should be positive. Further, X_i is a vector of standard controls, which in the full specification includes both a set of arguably exogenous controls (age and its square, gender, and a dummy for post-secondary education) and a set of controls that can be seen as endogenous, but might still be informative on the pricing strategies of individuals in our sample and are likely uncorrelated with the treatment. The latter are monthly net household income (in log EUR) and dummies for having a partner, having at least one child, being employed, and being a homeowner. Furthermore, λ_s are

[10] It is unlikely that participants would question whether the agencies are fictitious or not: the names are very similar (yet not identical) to existing agencies. On top of that, the market of real-estate agencies is very thick: according to the Luxembourgish Ministry of Economy, in 2019 there were 1221 registered real-estate agencies in Luxembourg.

[11] Despite the campaigns being launched in Luxembourg only and the survey referring to the Luxembourgish housing market, 18% of respondents in the social-media sample had an IP-address outside of Luxembourg. While this could be because of residents filling in the survey from abroad or using VPN-clients to mask their physical location, we cannot rule out the presence of non-residents among our respondents. To attenuate measurement-error concerns stemming from subjects with a more limited knowledge of the Luxembourgish housing market, Table A4 features conservative robustness tests restricting the estimation sample to those who logged in with an unmasked Luxembourg IP address.

[12] We have followed a forward–backward method to produce semantic equivalent versions of our questionnaire. For translations, we have relied on native speakers and translation machines. The translations also underwent a thorough final check by researchers familiar with the project and the Luxembourgish context, and fluent in the respective checked language.

session fixed-effects (corresponding to time by recruitment-medium fixed-effects).

Eq. (2), our preferred specification, takes advantage of the fact that each respondent appraises four different adverts. This allows us to introduce individual fixed effects μ_i, which has several implications. First, relying only on within-respondents variation rules out any bias coming from between-respondents comparisons. Second, it keeps constant all individual characteristics at the time of the interview that might affect pricing behaviour. This is why C_i, X_i and λ_s can no longer be included.

Note that $\epsilon_{i,n,h}$ is an error term modelled here (in accordance with our data and common practice in the hedonic housing literature) as Gaussian in both Equations. Standard errors are clustered at the individual level. Last, we rule out a competing hypothesis, that is *in-group favouritism*, and discuss the nature of discrimination in the interpretation of our results in Section 4.3.

3.5. Analysis sample and validity checks

Our analysis sample is composed of participants who completed the survey and performed the main experimental task, i.e., they reported an appraisal for each of the four adverts they were shown. To avoid results being driven by outliers (which may include erroneous numbers entered by accident), the sample was trimmed to keep only respondents who reported values between the first and the 99th percentile of the appraisals distribution. As an additional quality assurance measure, we exclude the data of three respondents who gave the same price evaluation to all four properties.[13] This selection produces a balanced panel of 2756 observations, corresponding to four observations by 689 individuals.

We conduct a set of pre-analysis checks that test the plausibility, representativeness and balance of the data we collected. As a first plausibility check, we compute property-specific average appraisals. By design, the properties have similar physical characteristics yet the chosen locations induce large price-relevant variation. As a consequence of the randomisation procedures described in Section 3.2, differences in average prices across adverts thus mainly capture the marginal contribution of location to property appraisals, in a hedonic fashion. To test whether participants understand well the importance of location as price-determining factor, we can hence check whether the average reported appraisals per location reproduce the observable hierarchy of locations in the housing market. Fig. 3 plots average appraisals per location given by respondents in our estimation sample (as the natural logarithm of the stated amount in EUR). The properties in Luxembourg City attract the highest appraisals, while that in Echternach is associated with the lowest value. This is in line with average realised sales prices per square metre for existing apartments according to Luxembourg's official housing observatory (see Figure 1 in STATEC and Observatoire de l'habitat, 2020).

As a second plausibility check, we test for coherence between provided estimates in our experiment and external appraisals. We first benchmark responses in the sample with the professional evaluations of a panel of five experts, operating as real-estate agents in Luxembourg. We provided them with our fictitious listings (without seller surnames to rule out appraisals being affected by similar racial biases) and confronted them with the very same task as participants in the experiment. The average values attributed by these experts appear as green round markers in Fig. 3. Although there are some minor differences in levels, relative price differentials match between the experts and our participants: both identify highest prices in Limpertsberg followed by Gare (both in Luxembourg City), Esch-sur-Alzette and lastly Echternach. In addition, we retrieved the 2022 realised market prices (EUR per square metre) by location from the Observatoire de l'habitat of the Luxembourg Ministry of Housing. These are shown as orange diamonds in Fig. 3 and relative price differentials are again consistent with the evaluations by both experts and respondents. The distributions of appraisals per advert and additional descriptive statistics are displayed in Appendix Figure A9 and Table A5 respectively.

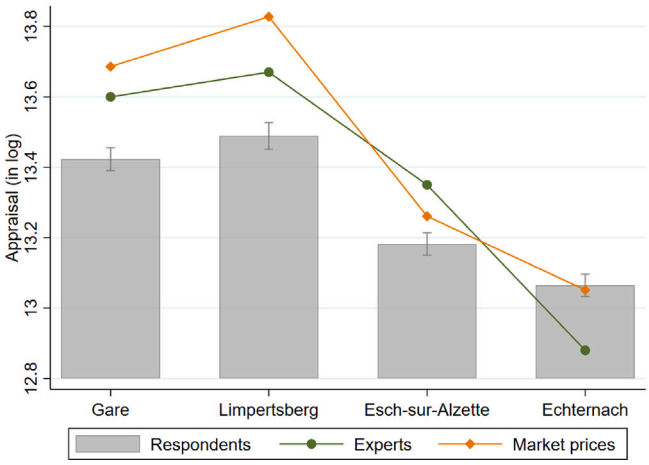


Fig. 3. Average appraisal per advertised dwelling.
Notes: The grey bars refer to our the average appraisal per advertisement in log EUR (with 95% confidence intervals). The green dots report the average valuation per advertisement coming from a pool of professional real-estate agents operating in Luxembourg. The orange diamonds are average market prices of our adverts, coming from the 2022 statistics published by the Habitat Observatory of the Luxembourg Ministry of Housing. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Next, we assess the quality of responses by looking at the time participants spent appraising each property. In Appendix Figure A10, we first show that, unsurprisingly, respondents spend significantly more time on the first advert they are presented with (about one additional minute). Furthermore, we check whether the treatment assignment affects the time spent on appraising properties. Appendix Figure A11 shows that respondents spend more time on the adverts when they are assigned to group *T* as compared to the passive control group *C1*, where no information about the seller is shown. This is consistent with the amount of information they are requested to process, suggesting that participants do pay attention to the box specifying owners' contact-details. Coherently, individuals assigned to the active control group *C2* seeing a real estate agency as contact, who are arguably exposed to the same amount of information as those in group *T*, spend on average the same amount of time on the adverts as the treated do. In Appendix Table A6, we show that our main results are unaffected when controlling for the time spent on each advert. In order to test for any contamination effects of the social-media campaign over time, we additionally look the trend of home appraisals for the social-media sample. Figure A12 shows that average appraisals follow a completely flat linear trend over the data collection period, indicating that those who were exposed longer to the social-media campaign did not answer differently from those exposed for a shorter period of time.

We then check whether respondents are representative of the adult Luxembourgish society. The demographic features of our estimation sample displayed in column (1) in Table 2 closely mirror those of the population living in Luxembourg in 2022.[14] Finally, we test for the success of our induced randomisation strategy. Descriptive statistics for the control and treatment groups are shown in columns (2) and

[13] Including these three respondents in the analysis sample does not affect our main results.

[14] National statistics for 2022 can be found on this official document produced by STATEC: https://statistiques.public.lu/dam-assets/catalogue-publications/en-chiffres/2022/demographie-en-chiffre-22.pdf.

Table 2
Descriptive statistics and balancing tests.

	Whole Sample (1)	Control (2)	Treated (3)	Difference (3)-(2)
Appraisal (in log)	13.290	13.312	13.265	−0.048*
	[0.374]	[0.378]	[0.370]	(0.029)
Appraisal	680 112	702 094	656 059	−46035
	[646113]	[427947]	[552877]	(42162)
Age	42.348	41.856	42.888	1.032
	[12.130]	[12.043]	[12.235]	(0.926)
Male	0.483	0.486	0.480	−0.006
	[0.500]	[0.501]	[0.500]	(0.038)
Female	0.509	0.506	0.514	0.008
	[0.500]	[0.501]	[0.501]	(0.038)
Non-binary	0.007	0.008	0.006	−0.002
	[0.085]	[0.091]	[0.078]	(0.006)
Post-secondary Education	0.538	0.542	0.535	−0.007
	[0.499]	[0.499]	[0.500]	(0.038)
Luxembourgish	0.517	0.533	0.499	−0.034
	[0.500]	[0.499]	[0.500]	(0.038)
Homeowner	0.698	0.692	0.705	0.014
	[0.459]	[0.462]	[0.457]	(0.035)
In a relationship	0.572	0.683	0.726	0.043
	[0.494]	[0.466]	[0.446]	(0.035)
At least a child	0.572	0.550	0.596	0.046
	[0.495]	[0.498]	[0.491]	(0.038)
Employed	0.826	0.836	0.814	−0.022
	[0.379]	[0.371]	[0.389]	(0.029)
Monthly HH income (in log)	7.937	7.901	7.976	0.075
	[1.866]	[2.016]	[1.691]	(0.142)
EU-SILC sample	0.367	0.394	0.337	0.057
	[0.482]	[0.489]	[0.473]	(0.037)
Observations	689	360	329	

Notes: Columns (2) and (3) report the average values of participants' characteristics in the control and treatment groups respectively. Standard deviations are in square brackets and standard errors are in parentheses. Statistical significance is coded following the standard notation: *** if the p-value is lower than 0.01, ** if the p-value is lower than 0.05, * if the p-value is lower than 0.1.

(3) of Table 2, respectively, with column (4) indicating whether any difference in means between the two groups is statistically significantly different from zero. As one would expect from the randomisation of the treatment assignment, we observe no significant differences in the control variables X_i across the two groups.

3.6. Limitations of using an online survey

While our experimental approach potentially offers valuable insights into housing market discrimination, it also comes with inherent limitations stemming from the nature of online survey experiments.

First and foremost, no actual property sale occurs in our experiment. Respondents know that they are participating in a survey rather than engaging in a real market transaction. As a result, their stated appraisals may not fully reflect the dynamics present in real-world home-buying scenarios. In the housing market, buyers with racial animus might avoid visiting certain properties altogether, whereas our survey compels them to provide an appraisal. Similarly, respondents discriminating against sellers of African origin in an online survey might exhibit exaggerated discriminatory behaviour due to reduced social desirability bias and the absence of real-world consequences. Our Section 4.3 provides a detailed discussion of how the findings from our online survey experiment may (or may not) translate into actual market transactions.

Second, the absence of personal financial stakes in the survey environment could influence response accuracy. Since participants do not face real monetary consequences from over- or under-bidding, some might provide less precise or less thoughtful appraisals than they would in a genuine purchase decision. Although we offered monetary incentives to encourage serious engagement, responses in a hypothetical setting may still differ from those in an actual high-stakes market. However, since treatment assignment in our experiment is random, we expect any bias resulting from reduced response accuracy to be equally distributed across the treatment and control groups, ensuring the internal validity of our findings.

Table 3
Appraisal and information treatment: Main results.

	Appraisal (in log)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Ref. = Clement					
Schmitt	−0.023	−0.007	−0.007	−0.007	−0.007
	(0.029)	(0.024)	(0.024)	(0.024)	(0.024)
daSilva	−0.004	−0.005	−0.005	−0.005	−0.005
	(0.025)	(0.022)	(0.022)	(0.022)	(0.021)
Mutombo	−0.045*	−0.036*	−0.036*	−0.036*	−0.036*
	(0.027)	(0.022)	(0.022)	(0.022)	(0.021)
Control group: No last name	0.030	0.034	0.029	0.030	
	(0.034)	(0.032)	(0.032)	(0.032)	
Observations	2756	2756	2756	2756	2756
Panel B: Ref. = All other names					
Mutombo	−0.036	−0.031*	−0.031*	−0.031*	−0.031*
	(0.023)	(0.019)	(0.019)	(0.019)	(0.019)
Control group: No last name	0.039	0.039	0.033	0.034	
	(0.029)	(0.029)	(0.029)	(0.029)	
Observations	2756	2756	2756	2756	2756
Experiment controls	.	✓	✓	✓	✓
Exogenous controls	.	.	✓	✓	.
Endogenous controls	.	.	.	✓	.
Individual FE	.	.	.	.	✓

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Experimental controls are advert and session fixed effects. Exogenous controls are age, age squared, gender and a dummy for post-secondary education. The endogenous controls are a dummy for having a partner, having a child, being employed, being a homeowner and the monthly net household income (in log). Statistical significance is coded following the standard notation: *** if the p-value is lower than 0.01, ** if the p-value is lower than 0.05, * if the p-value is lower than 0.1.

4. Results

4.1. Main results of the experiment

Table 3 summarises our main results and presents two versions of Eqs. (1) and (2). In Panel A, we set one of the surnames from the treated group as the reference category. We choose the French-sounding surname 'Clement' for this purpose because it consistently yields estimates closest to zero. Selecting a different surname would not alter our conclusions, as the model structure would remain isomorphic. In Panel B, we use all surnames except 'Mutombo' together as the reference category. Across the columns of Table 3, we begin with the simplest version of Eq. (1), which includes only dummies for the owner names and the indicator of not seeing the name of a private seller. We conclude with our preferred specification, Eq. (2), which incorporates individual fixed effects. This model serves as the basis for all subsequent analyses.

Looking at the surname dummies in the first column of Panel A in Table 3, results show that the only negative and significant coefficient is associated with 'Mutombo'. Treated survey respondents systematically provide lower appraisals to adverts in which a person named 'Mutombo' acts as seller, compared to when the seller's name is 'Clement'. No other surname attracts statistically significant price penalties. The appraisal penalty associated with 'Mutombo' is robust to the inclusion of experimental controls (namely, property and session fixed-effects), as well as individual controls, as shown in columns (2) to (4) of Table 3. Results are similar when including Schmitt and daSilva in the reference category in Panel B.

We now turn to the last column of Table 3, which displays the estimates of Eq. (2). 'Mutombo' still attracts a negative and significant

penalty in Panel A. In line with the fact that neither 'Schmitt' nor 'daSilva' attract a significant penalty, the average penalty associated with 'Mutombo' in Panel B in column (5) is, unsurprisingly, still negative and statistically significantly different from zero at the 10% level.[15]

In addition to uncovering evidence for price discrimination against owners with a sub-Saharan African surname, our analysis also examines whether the mere presence of a name attached to a real-estate advert influences how individuals evaluate house prices as compared to adverts without contact information or those listing a (fictitious) real-estate agency. The first four columns of Table 3 reveal that respondents who did not see a seller's name tend to provide slightly higher appraisals indicated by consistently positive estimates across specifications, yet these estimates are not statistically significantly different from zero according to conventional levels. This pattern aligns with the small yet significant difference in means reported in the top line of Table 2. The presence of a private seller's name could plausibly be interpreted as a signal of lower property or seller quality, possibly reflecting an implicit assumption that an owner's decision not to list the property with a professional agency could indicate reduced desirability.

4.2. Economic significance of the penalty: Winners and losers

Our estimates from the last column in Table 3 indicate that the average price penalty associated with the name 'Mutombo' ranges between 3.1% and 3.6%. Translating these figures into currency units, this implies an average price penalty of approximately EUR 18,300 to EUR 21,300 for an owner named 'Mutombo'.[16]

Additionally, we perform a test of persistence using the estimated *expected* market sales price in five years (instead of the estimated *current* price) as the dependent variable. The results appear in Appendix Table A8 and document an average within-individual appraisal penalty for 'Mutombo' of 5.8% (an expected price reduction of around EUR 40,000).[17] While these numbers may already appear substantial to the reader, we provide below insights from the Luxembourgish context to offer a more comprehensive assessment of the overall economic implications of this racial discrimination for both sellers and buyers.

Were the appraisal penalty directly applied to the final sales price, it would result into significant windfall gains for the potential buyer. Not only would the buyer benefit from a lower sales price, but they would also save on payable fees and taxes that are proportionate to the sales price. In Luxembourg, these transaction costs amount to 7%–10% of the final sales price. They include the registration tax (6%), the transcript tax (1%), and, for properties located within Luxembourg City, an additional surtax of 3% (see Naidin et al., 2025, for details on the respective taxes and fees in Luxembourg). Using the average appraisals of properties outside of Luxembourg City in the control group as a benchmark, a non-discriminating buyer would pay a final sales price of EUR 560,456, with the 7% transaction costs amounting to EUR 39,232. Applying the most conservative price penalty of 3.1% (see column (7) in Table 3), a discriminating buyer would pay EUR 543,082 for the same property when sold by Mutombo. With only EUR 38,016 in taxes, this buyer would save a grand total of EUR 18,950. In a similar fashion, a discriminating buyer purchasing Mutombo's property in Luxembourg City (average price in the control group: EUR 843,732) would end up saving EUR 28,771 in total (lower price and reduced taxes).

To put this into perspective, the amount a discriminating buyer could save ranges between 95% and 145% of the one-time tax credit known as *Bëllegen Akt* that is granted in Luxembourg to first-time home buyers, which amounted up to EUR 20,000 per person in 2022.[18]

It can be argued that certain mechanisms may prevent or limit the full pass-through of racial discrimination in appraisals to final sales prices. Below, we discuss what we believe to be the most salient mechanisms. First, if buyers' discriminatory behaviours are known to sellers, the latter can insure themselves against it by shielding their identity until the transaction price is agreed upon, e.g., by employing a professional real-estate agency to handle the transaction. However, as with any economic decision, a rational and informed seller would adopt this strategy only if its benefits outweigh the costs. There are two important factors to consider in this context: first, real estate commissions in Luxembourg are fixed at 3% of the sales price; second, there is a non-zero risk that the seller's identity may be revealed early on in the transaction process (for instance, during the first property visit), leading to a potential reduction in the initial price appraisal. While estimating the costs associated with the risk of the seller's identity reveal is challenging, it is worth highlighting that none of the within-individual penalties we have reported in Table 3 statistically exceed 3% anyway. Therefore, even if the seller's identity was perfectly shielded throughout the entire transaction process, the real-estate agent's commission alone would already equate to the penalty associated with 'Mutombo' — making the decision to hire a real-estate agent unlikely to be financially advantageous.

Second, we have thus far focused on average appraisals and by that overlooked the fact that final sales prices are determined by market forces, i.e., through negotiations between the seller and a pool of potential buyers. Therefore, if the highest bidder does not discriminate (and is unaware of the bids made by other potential buyers), the final transaction price should remain the same regardless of the seller's origins. Conversely, if all buyers perceive property offered by 'Mutombo'

[15] Although individuals in the control group do not contribute to the estimation of the coefficients associated with the surnames, they still contribute to the estimation of the other coefficients in the models. This is why we do not exclude them from the sample. Nevertheless, for the sake of transparency, we have replicated the results from Table 3 after excluding the control group from the estimation sample. The results, reported in Appendix Table A7, are almost identical to those presented in Table 3. This is unsurprising as the name 'Clement' always serves as a reference category both in Eqs. (1) and (2).

[16] It could be argued that the recruitment procedure we used for our sample attenuates the penalty associated with the name of 'Mutombo' due to measurement error. This is because, even if both the EU-SILC sample and the social-media recruitment campaign targeted Luxembourgish residents, it may very well be the case that people living outside of Luxembourg replied to the online survey. This may be especially true as the "Greater Region" (composed of Luxembourg and parts of Germany, France and Belgium) forms a well-integrated labour market with many people crossing the border every day. To avoid potential measurement error stemming from non-residents (e.g., cross-border workers), who might not have the same knowledge of the Luxembourgish housing market as compared to residents, column (2) of Table A4 restricts the estimation sample to respondents who either belong to the EU-SILC sample or who completed the survey from an IP address within Luxembourg (82% of the social-media sample). Column (1) reports the baseline estimates for comparison purposes. In addition, as respondents from the 2019 EU-SILC sample might have changed their residence since 2019, column (3) additionally excludes the 11% of EU-SILC respondents who completed the survey from an IP address outside of Luxembourg. Compared to the baseline estimates, the penalties associated to 'Mutombo' are slightly larger in columns (2) and (3), as one would expect when attenuating the bias coming from measurement error. However, we cannot conclude that these penalties are statistically larger than the penalty estimated in the whole sample.

[17] The 'Mutombo' penalty on future property prices is twice as large as the penalty on current price appraisals. With the average 5-year price appreciation being around 30% in our sample, this suggests that respondents believe the properties owned by 'Mutombo' will appreciate at a lower rate compared to other properties. While we cannot identify the exact motives behind the respondents' expectations of a discriminatory price appreciation, results on future prices might be indicative of the fact that they perceive the current conditions and upkeep of real-estate owned by 'Mutombo' to be worse than they would be with other owners.

[18] As of 2023, the maximum amount has increased to EUR 30,000 per person (law of 16 May 2023).

as less valuable, no corrective mechanism through standard market forces occurs, resulting in a lower final sales price. To explore this possibility, we employ a conditional quantile regression with individual fixed-effects (Koenker and Bassett, 1978; Machado and Silva, 2019), as detailed in Appendix Table A9. Although we observe a consistent reduction in the 'Mutombo' penalty across the whole appraisals distribution, none of the estimates associated with 'Mutombo' are statistically different from one another. Additionally, a Kolmogorov–Smirnov test confirms that the entire cumulative distribution function (CDF) of appraisals for properties offered by 'Mutombo' is significantly shifted downward as compared to the CDF of appraisals for all other names (p-value: 0.033). These CDFs, depicted in Figure A13, additionally reveal that the CDF of appraisals received by 'Mutombo' has a shorter upper tail. In other words, the highest bids in our sample are not placed on properties offered by individuals named 'Mutombo'. Overall, these tests suggest that racial discrimination occurs evenly across the appraisals distribution — including at its top. As the highest bid usually seals the deal, the race penalty observed in appraisals will likely be reflected in final prices too.

Last, it is crucial to assess whether the racial penalty in appraisals exhibits any significant heterogeneity. In the simplest case, some respondents may strongly discriminate, while others may not discriminate at all. To investigate this aspect, we first replicate our main within-individual models (using first 'Clement' as a reference category, and then all names but 'Mutombo' — as we have done in the last two columns of Table 3) on sample splits based on the following individual characteristics: gender, age, education, partnership status and parental country of origin.[19] Results are reported in Table 4, which will be further discussed in Section 4.3. Several findings stand out: first, we show that the sub-Saharan name penalty is only found for individuals over 40 years of age, those with no tertiary education and those with arguably the weakest ties to sub-Saharan Africa (we define the latter as individuals with parents coming from countries where individuals identifying as Black do not exceed 5% of the total population according to the latest available Census data; see the footnotes of Table 4).[20] Second, our estimates indicate that the average racial penalty exhibits significant heterogeneity, with values ranging from EUR 0 to EUR 57,940.[21]

Older individuals, who in Table 4 are more likely to display discriminatory pricing behaviour against Mutombo, are also those that are more likely to be homeowners in our sample. All else equal, it is then likely that these individuals have already behaved in a discriminatory way in the housing transactions they participated in. Conversely, younger and more educated individuals, who on average do not penalise Mutombo in our experiment, are more likely to be perspective buyers.[22] While this could imply that the price penalty for Mutombo will converge to zero in future housing transactions, our analysis does not guarantee that sub-Saharan African sellers will not encounter any discrimination in the housing market. As long as even a fraction of their pool of potential buyers offers systematically lower prices, sub-Saharan African sellers will face larger search costs to obtain an offer that matches the market price of their home.

4.3. Mechanisms and interpretation

Although the introduction of individual fixed-effects rules out the issues emerging from inter-individual comparisons, it could still be argued that the appraisal penalty associated with a sub-Saharan African name does not reflect discrimination only. Provided that most of our sample is of Luxembourgish, French, German and Portuguese origins, it may very well be the case that this penalty is a statistical artefact driven by *in-group favouritism*. Agarwal et al. (2019a) show, for instance, that Chinese sellers earn 1.7% higher premia when selling homes to Chinese buyers in Singapore. Although discrimination and in-group favouritism arguably stem from the same origins, i.e., group membership, they are not equivalent in that one does not necessarily imply the other.

As we do not observe ethnicity or race in our survey, we cannot analyse in-group premia based on these dimensions. However, we do observe respondents' country of birth, as well as those of their parents. Based on this information, we build a binary variable that can be used to keep the influence of potential in-group premia based on country of origin (or national homophily) constant. This dummy is equal to one for the following cases: participants from France or Belgium (or with at least a parent from one of these countries) when they evaluate the property owned by 'Clement'; German participants (or with a German parent) when they evaluate the property owned by 'Schmitt'; Portuguese participants (or with a Portuguese parent) when evaluating the property of 'daSilva'; and sub-Saharan African participants (or with a parent from a sub-Saharan African country) when evaluating the property owned by 'Mutombo'. Based on the institutional context, we additionally assign a value of one to the dummy variable measuring national homophily to Luxembourgers with a Luxembourgish, French or Belgian parent when they evaluate the dwellings owned by 'Clement'; to the Luxembourgers with a Luxembourgish or German parent when they evaluate 'Schmitt'; and to the Luxembourgers with a Portuguese parent when they evaluate 'DaSilva'.

We re-estimate the baseline models after introducing our measure of in-group premium as a new control variable and report the results in Appendix Table A11. We never find a significant premium. The same holds when using alternative versions of our measure of in-group favouritism: first, only based on the respondent's own country of origin; then, only based on the country of birth of her parents; last, based on the main language spoken by the respondent. None of these alternative measures attracts an in-group premium that is different from zero at conventional significance thresholds. The direct consequence of the absence of a premium for national homophily is that the coefficients attracted by the different names in Appendix Table A11 remain similar to those in Table 3, thereby suggesting that the penalty associated with the sub-Saharan African name is unlikely to reflect in-group favouritism.

After ruling out the issues of inter-individual comparisons and the possibility for in-group favouritism to play a major role, we argue that the average appraisal penalty of roughly EUR 20,000 for the properties listed by someone with a sub-Saharan African name can be attributed to racial discrimination.[23]

[19] Note that, as we have no time dimension in our setup, we cannot disentangle the effect of age from that of cohort.

[20] As mentioned earlier, the EU-SILC and the social media campaign samples display some differences. Given that the EU-SILC participants are older and have lower rates a of post-secondary education (see Appendix Table A3), we expect to see a higher penalty for 'Mutombo' in this sample. This prediction is confirmed by Appendix Table A10, where we estimate all our models separately by recruitment strategy. From the Table, we conclude that racial discrimination is only observed in the EU-SILC sample. Consistently, when we re-weight the social media sample to match the socio-economic characteristics of the representative EU-SILC sample (documented in Appendix Table A3), we observe a qualitatively similar penalty for 'Mutombo'.

[21] It is worth addressing whether the penalty imposed on 'Mutombo' is mitigated by the proportion of foreigners (or individuals with sub-Saharan African origin) residing in the geographical area covered by our adverts. To explore this possibility, we conducted a separate estimation of our main model for adverts with the highest and lowest densities of foreigners (Luxembourg Gare and Esch-sur-Alzette versus Limpertsberg and Echternach). As displayed in columns (11) and (12) of Table 4, we found comparable penalties for individuals with sub-Saharan African names in both cases — although the penalty appears to be qualitatively larger for properties in low-diversity areas.

[22] According to the following report from the leading real-estate agency in Luxembourg, in 2019 two-thirds of individuals seeking a mortgage were below the age of 40: https://www.athome.lu/blog/acheter/pret-immobilier/quel-est-le-profil-des-demandeurs-demprunt-en-2019/

Table 4
Appraisal and information treatment: Heterogeneity analysis.

	Appraisal (log)											
	Gender		Below age 40		Education		In couple		Ties to Africa		Low diversity	
	Men	Women	Yes	No	Low	High	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Ref. = Clement												
Schmitt	−0.009	−0.001	0.032	−0.043	−0.036	0.021	0.029	−0.020	−0.008	−0.006	−0.003	−0.014
	(0.024)	(0.041)	(0.035)	(0.032)	(0.028)	(0.037)	(0.052)	(0.026)	(0.042)	(0.025)	(0.037)	(0.036)
daSilva	−0.023	0.013	0.026	−0.033	−0.006	0.000	0.030	−0.019	−0.027	0.014	−0.001	0.011
	(0.028)	(0.032)	(0.030)	(0.031)	(0.025)	(0.034)	(0.042)	(0.025)	(0.038)	(0.022)	(0.034)	(0.042)
Mutombo	−0.043	−0.026	0.032	−0.098***	−0.089**	0.013	−0.045	−0.032	−0.078**	0.002	−0.020	−0.064
	(0.032)	(0.029)	(0.023)	(0.037)	(0.036)	(0.025)	(0.035)	(0.026)	(0.038)	(0.022)	(0.036)	(0.044)
Observations	1332	1404	1412	1344	1272	1484	816	1940	1240	1516	1378	1378
Panel B: Ref. = All other names												
Mutombo	−0.033	−0.030	0.012	−0.072**	−0.074**	0.006	−0.064	−0.019	−0.066*	−0.001	−0.018	−0.062
	(0.028)	(0.026)	(0.020)	(0.032)	(0.032)	(0.021)	(0.040)	(0.021)	(0.035)	(0.017)	(0.027)	(0.041)
Observations	1332	1404	1412	1344	1272	1484	816	1940	1240	1516	1378	1378

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Advert and individual FE are included. "High" education stands for post-secondary education. Column (10) is based on respondents with at least one parent coming from an African country or a country where individuals identifying as black account for at least 5% of the total population. Countries in the sample that are included in the latter group are the following: Brazil, Colombia, the Dominican Republic, Ecuador, France, Jamaica, Luxembourg, Saudi Arabia, and the US. Statistical significance is coded following the standard notation: *** if the *p*-value is lower than 0.01, ** if the *p*-value is lower than 0.05, * if the *p*-value is lower than 0.1.

Nevertheless, an important question remains unanswered: can the nature of this discrimination be determined? To address this question, we appeal to the literature that traditionally differentiates between *taste-based* and *statistical* discrimination (Guryan and Charles, 2013). The original model of taste-based discrimination was developed by Becker (1957) in the context of labour markets to explain why some employers may have a distaste for hiring minority workers. More generally, taste-based discrimination relates to animus, or hostility, towards a given group. When looking at statistical discrimination instead, differences in the treatment of minorities emerge as a by-product of the costs of signal-extraction in case of imperfect information. When individual information is not available, group information (that likely reflect stereotypes) can be used as a proxy and thus be adopted for rational decision making. For example, in the Luxembourg housing market, prospective buyers might tend to attribute varying levels of quality to homes or perceive their bargaining position differently when evaluating a property owned by someone from sub-Saharan Africa.

However, it is important to bear in mind that identifying which type of discrimination is at play is not an easy task (Guryan and Charles, 2013). A rare exception is the study by Arefeva et al. (2024) who develop a theoretical framework that allows them to test for the type of discrimination in rental markets. However, this framework is not applicable to the sales market. The modern literature in Psychology even suggests that statistical discrimination generally cannot be distinguished from taste-based discrimination (Bertrand and Duflo, 2017). The formation of prejudice and adoption of discriminatory behaviours are contingent to the formation of group membership, that is itself the basis of our social identity. The lack of information driving statistical discrimination becomes then endogenous to animus-based discrimination against out-group members.

The difficulty to disentangle statistical from taste-based discrimination is confirmed by the results from Table 4, which are consistent with both theories of discrimination. The lack of a statistical difference in the racial penalty across gender and partnership status is not surprising, as there is little reason to believe that either group is particularly more hostile towards or less knowledgeable about minorities. On the contrary, the stronger penalty found for those over 40 and those with no tertiary education may be explained the fact that they are plausibly the groups with stronger animus (Wilson, 1996; Wodtke, 2012) and more limited access to accurate information (Loos and Nijenhuis, 2020). In addition, this finding is in-line with cross-country evidence that in rich countries (high per capita GDP), individual skills and education positively correlate with pro-immigration preferences (Mayda, 2006) — implying that low-skilled natives display more scepticism towards immigrants.

In Table 4, we also find that individuals with stronger ties to sub-Saharan Africa do not discriminate. This may be interpreted as a falsification test: due to their stronger ties to Africa, we expect this group to display less animus and to have access to a more complete, less stereotypical information set. As such, they should have less or no motive to penalise 'Mutombo', according to the theoretical tenets of taste-based and statistical discrimination models.

Our experimental setting has not been designed to disentangle statistical from taste-based discrimination, so we cannot rule taste-based discrimination in or out of our setting. Yet, we believe that statistical discrimination (accurate or inaccurate, see Bohren et al., 2023, for a discussion) may be a more salient mechanism here for two reasons. First, it could be argued that discrimination motivated by animus should reduce the likelihood of a buyer to contact sellers from a minority to start with, in a real-life housing market transaction. Since our experiment skips this step and directly asks respondents to appraise the value of properties, statistical discrimination might mechanically be more salient. Second, survey participants were asked to respond to the following question: "*People from many different countries come to live in Luxembourg. Do you think this is rather advantageous or disadvantageous?*". Responses to this question are the closest proxy for animus at our disposal in the survey. Finding a racial penalty for the group without animus (i.e., those who think that foreigners are an advantage) would support the theory of statistical discrimination. This is indeed what we find in Table 5: even if the point estimates are not statistically different from zero (likely reflecting the lack of statistical power coming

[23] While the layers of randomisation in the experiment provide internal validity to our estimates of racial discrimination, we also produce some falsification tests to show that our experiment did not affect individuals' opinions about the housing market or other subjective evaluations. Our experiment was followed by a battery of questions eliciting the opinions of respondents on different topics. We take advantage of this to check whether being part of the treatment group affects the answers we collected. In the first four columns of Appendix Table A12, we estimate the effect of being exposed to the treatment on the probability of expecting increasing overall house prices and an increase in the value of their own real-estate (with and without control variables). In the next four columns, we estimate the same effect on life satisfaction and self-assessed health. None of the estimates is significantly different from zero. This confirms that our experiment did not have unexpected consequences on other subjective variables, both related and unrelated to the housing market.

Table 5
Appraisal and information treatment: By level of animus.

	Appraisal (in log)			
	Low animus		High animus	
	(1)	(2)	(3)	(4)
Schmitt	−0.013		0.000	
	(0.023)		(0.051)	
daSilva	−0.015		0.010	
	(0.025)		(0.039)	
Mutombo	−0.041	−0.031	−0.025	−0.029
	(0.029)	(0.023)	(0.032)	(0.034)
Observations	1756	1756	1000	1000

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Advert and individual FE are included. Statistical significance is coded following the standard notation: *** if the p-value is lower than 0.01, ** if the p-value is lower than 0.05, * if the p-value is lower than 0.1.

from the sample split), the appraisal penalty for 'Mutombo' does not vary with the level of animus and exists even for the group with the lowest self-reported animosity towards foreigners.[24]

5. Conclusion

In this paper, we present results from an online experiment in Luxembourg, in which we task respondents to put themselves into the shoes of perspective real-estate buyers and appraise the value of a sequence of real-estate adverts, each displaying the dwelling characteristics and a randomly varied seller profile. For respondents in our treatment group, these profiles are strongly framed to reveal the origin of private sellers. We detect a systematic appraisal penalty for sellers of sub-Saharan African origin: ceteris paribus, our most conservative estimates suggest that these sellers receive offers that are on average 3.1% to 3.6% lower than other sellers, i.e., an average penalty of EUR 18,300 to EUR 21,300. This penalty is substantial and likely to pass trough onto the final sales price. We also show that the racial bias in appraisals is not influenced by in-group favouritism, but rather differs in correspondence to other exogenous individual characteristics. The moderating effect of individual heterogeneity we find is consistent with traditional economic theories of taste-based and statistical discrimination: respondents that are older, less educated, or are less exposed to African-origin communities are the ones that penalise sellers with a sub-Saharan African sounding name the most (with average penalties among these groups amounting up to EUR 58,000). Although we cannot rule taste-based discrimination in or out, we present evidence suggesting that statistical discrimination is more salient in our context.

We believe our results to be of great importance. We are the first to show evidence of racial discrimination on the demand side of the housing market — an understudied aspect when it comes to discriminatory practices. Our analysis also highlights the existence of discriminatory practices in a country, Luxembourg, where almost half of the population is foreign-born and that is considered as one of the most multicultural and tolerant contexts among developed economies. We are therefore concerned that our findings may constitute a lower bound and that the racial penalty for home sellers would be even larger in countries with a less diverse population or with a history of inter-group antagonism.

Although we argue that a racial bias affecting appraisals sets the bar for later negotiations and likely translates into higher costs for the discriminated group, our online experiment does not involve actual home sales. Further research is needed to craft innovative solutions to empirically assess the extent to which the racial bias we uncovered transmits to final sales prices.

[24] It could be argued that social desirability bias influences responses to our measure of animus. Our reading of Table 5 would be invalid if this bias was different across the treated and control groups. This could be the case, since the measure of animus was collected after the experimental module in the survey. We rule out this possibility in the last two columns of Table A12, where we show that there are no systematic differences in the probability to belong to the high animus group by treatment status.

CRediT authorship contribution statement

Anthony Lepinteur: Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Formal analysis, Data curation, Conceptualization. **Giorgia Menta:** Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Formal analysis, Conceptualization. **Sofie R. Waltl:** Writing – review & editing, Writing – original draft, Resources, Project administration, Methodology, Funding acquisition, Data curation, Conceptualization.

Declaration of competing interest

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

Appendix A. Online appendix

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2025.104089.

Data availability

Data will be made available on request.

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