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Abstract

Participants to an online study in Luxembourg are presented with fictitious real-estate advertisements and tasked to make an offer for each of them. A random subset is also shown sellers' names that are strongly framed to signal their origins. Our randomised procedure allows us to conclude that, keeping everything else constant, a seller with a sub-Saharan African surname is systematically offered lower prices. Our most conservative estimates suggest that the average racial appraisal penalty is equal to roughly EUR 20,000. This figure is highly heterogeneous and can amount up to around EUR 58,000. Last, we provide evidence suggesting that this appraisal bias may very well pass through onto the final sales price and that it may be due to statistical discrimination.

Keywords: Racial Discrimination, Housing, Randomised Online Experiment
JEL codes: J15, R21, R31

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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., the United States enacted the Civil Rights Act, of which Titles VIII and IX are commonly known as the *Fair Housing Right.* 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, differences in housing wealth between Blacks and Whites remain – 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 roughly a third of global private wealth being tied up to real estate (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 represent a non-random sample of the housing stock, i.e., those 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 information on ethnic or racial background alongside actual property prices. To overcome these issues, we drew from the literature on correspondence studies (e.g., Bertrand and Mullainathan, 2004) and conducted an online experiment

[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.

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 migrant group; and sub-Saharan African, one of 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, we estimate a penalty of 6.6% associated with the sub-Saharan African surname – a price reduction of about EUR 40,000. Using within-individual variation, we argue that around half of this penalty can be attributed to racial discrimination alone. 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. 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). We use insights from Luxembourgish statistics, laws and institutions to demonstrate that this penalty has 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. 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 four main contributions to the literature. First, there is no other study to our knowledge that explicitly documents discrimination stemming from the demand side of the housing market. By showing discrimination from the side of (experimental) buyers, our paper thus complements a literature dominated by analyses illustrating supply-side discrimination via landlords

(Christensen et al., 2022; Edelman et al., 2017; Ewens et al., 2014; Hanson and Hawley, 2011), owners (Agarwal et al., 2019a,b) and real-estate agents (Hanson and Hawley, 2023; Christensen and Timmins, 2022; Yinger, 1986). 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) or daily-life transactions, such as Ebay auctions (Ayres et al., 2015; Doleac and Stein, 2013) and tipping behaviour (Brewster and Lynn, 2014), rather than on the housing market. Third, our paper adds a new piece of evidence to the small literature documenting racial discrimination in the housing market in Europe (Auspurg et al., 2017; Carlsson and Eriksson, 2014; Bosch et al., 2010; Ahmed and Hammarstedt, 2008). 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 2,500 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 (UN) and the

European Union (EU). 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,[2] Luxembourg is one of the wealthiest countries in the world, 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%.[3] Figure 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 Figure 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 Figure 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%).

When did Luxembourg become such a diverse country? Periods of large-scale economic immigration, fostered by the country's economic success, have led to a highly diverse society that is reflected in the high degree of variation in the residents' nationalities, migration background and languages used. Historical population series from the country's National Statistical Office (STATEC) document that the foreign share of Luxembourgish residents has been steadily increasing since the 1960s, with some minor slow-downs during the Great Recession and the Covid-19 health crisis (Figure A1). Since industrialisation in the late 1800s, the Luxembourgish economy has relied on low-skilled workforce from Germany and, later on, Italy (Cordeiro, 1976). While German labour force inflows declined prior to the Second World War, Italian immigration increased in the

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

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

wake of the war, as the country was struggling to recover (7% of Luxembourgish residents were Italian in 1970, constituting over one-third of the total foreign population). Migration inflows from Italy slowed down after the 1970s, in the aftermath of Italy's economic boom. Around the same time, many Portuguese fleeing Salazar's authoritarian regime were welcomed to Luxembourg. The Portuguese diaspora to Luxembourg was further enhanced with the adoption of agreements regularising the working position of illegal migrants from outside the European Economic Community and allowing for family reunification.[4] The Portuguese in Luxembourg still constitute today the largest foreign group from one single country, accounting for 14.5% of the total Luxembourgish population in 2022.

While there is a large Portuguese presence in Luxembourg, the relative growth of first-generation migrants from Portugal has slowed down in recent years. Figure A2 documents recent population growth trends for selected groups by country or region of origin. Similar to Portuguese migrants, Luxembourgish natives and German migrants have also remained quite stable over the past decade.[5] On the contrary, there seems to have been a surge in migration from France, with the number of French nationals in Luxembourg increasing by almost 60% since 2011. Similarly, migrants from sub-Saharan African countries constitute another group whose presence in Luxembourg has become increasingly larger, almost doubling in the past decade. While relatively new, migration flows from sub-Saharan Africa are among the fastest-growing population inflows that Luxembourg is experiencing as of today.

Figure A3 disentangles the population growth trend for sub-Saharan Africa provided in Figure A2 into stocks of migrants from each country in the region, in order to better assess their relative contribution. As Cape Verde was part of Portugal until 1974, migration inflows from this country followed a similar pattern as the Portuguese inflows. As such, Cape Verde is still the largest single contributor to sub-Saharan African migration to Luxembourg, although its relative importance has been decreasing over time. Figure A3, which excludes Cape Verde because of scale reasons, reveals that Cameroon is the second most-represented sub-Saharan African country in

[4]Law of March 28th 1972 on 1. the entry and stay of foreigners; 2. the medical control of foreigners; 3. the employment of foreign labour force (Mémorial, Partie A, 1972-04-13, n. 24, pp. 818-823). As Portugal joined the EEC only in 1986, the Portuguese in Luxembourg were the group most extensively affected by the law upon its approval.

[5]Belgium, one of the three countries bordering countries, has also followed a similar path (not shown in the graph).

Luxembourg, followed by Niger and the Democratic Republic of Congo.

2.2 Diversity and Attitudes Towards Foreigners in Luxembourg

Luxembourg's high degree of diversity is also reflected in its three official languages: on top of the national language, Luxembourgish, French and German are additional administrative languages of the country. The 1984 law on the language regime established multilingualism in the public sphere, with the possibility of using either one of the three official languages in administrative and judicial matters. As a consequence, many aspects of life in Luxembourg are multilingual by design.[6]

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 less than 9% of the votes in the parliamentary election in 2018 and is at 7.5% of the voting intentions in April 2023 for the next election.

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).[7]

[6] For instance, the general language of instruction in schools changes from Luxembourgish in (compulsory) pre-school, to German in primary and lower-secondary education, to French for upper-secondary education. Additionally, English is taught to all students in secondary school and pupils can further add Latin or another modern language like Italian, Spanish or Portuguese to their curriculum.

[7] 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

While Luxembourgers display friendlier attitudes towards immigrants as compared to their neighbours, these might not be the same regardless 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. On top of that, 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.[8] 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 home-ownership, 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 vari-

stranger in my own country".

[8]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.

ables. 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. 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 Table 1. 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 Figure 2, each featuring their approximate location on Google Maps via a circle overlapping the exact address. The selected locations are shown in Figure 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.[9] Table 2 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 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.[10]

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

[9]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.

[10]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.

to *C1* are exclusively shown apartment features, leaving out any information on the seller (see panel (d) of Figure 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 Figure 2). We use two agencies with similar names and branding: LUXhouse and realLux (see the logos in Figure 3). [11] 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 Figure 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 A1, 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.

[11] 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 1,221 registered real-estate agencies in Luxembourg.

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 A6 for a sample screenshot), targeting Luxembourgish residents.[12] Additionally, a Luxembourgish online news outlet wrote about this campaign, further popularising participation. As revealed by Appendix Table A2, 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 A7). French and German translations of the key questions and adverts are reported in Appendix B.[13] The study was online between 9 February 2022 and 22 May 2022. Respondents

[12]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 A3 features conservative robustness tests restricting the estimation sample to those who logged in with an unmasked Luxembourg IP address.

[13]We have followed a forward-backward method to produce semantic equivalent versions of our

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 a 80% chance of not receiving nothing. The two options are worth the same in expectation. Wins were payed out in the form of vouchers usable on Letzshop, a popular e-commerce platform in Luxembourg offering products and services by local vendors.

3.4 Econometric Strategy

To detect discriminatory real estate appraisals, we estimate the following regression model:

$$\log(P_{i,n,h}) = \alpha + \beta_n \mathbb{1}(N = n) + \gamma_h \mathbb{1}(H = h) + \delta X_i + \lambda_s + \varepsilon_{i,n,h}, \tag{1}$$

where $P_{i,n,h}$ is individual i's appraisal (expressed in EUR) of property h offered by owner n. N and H are discrete random variables respectively modelling owner names and properties. Their realisations are correspondingly indicated with n and h. 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 session fixed-effects (corresponding to time by recruitment-medium fixed-effects). Last, $\varepsilon_{i,n,h}$ is an error term modelled here (in accordance with our data and common practice in the hedonic housing literature) as Gaussian. As mentioned earlier, we pool both control groups (*C1* and *C2*) in equation (1). Standard errors are clustered at the individual level.

Because of the randomisation procedure, one can conclude that β_n measures a discriminatory appraisal bias if, *ceteris paribus*, surname n in the treatment group attracts a statistically lower price evaluation as compared to appraisals from the control groups. However, the sheer presence of

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.

a surname appearing on an advert may not only signal membership to an ethnic or racial group: it may also be perceived as a signal in itself. If respondents believed that properties that are directly sold by the owner – without, for example, the brokerage of a real-estate agent – are on average of lower quality, then β_n might attract a negative coefficient regardless of discrimination (although any significant differences across β_n could still be interpreted as driven by discrimination).

We address this problem by augmenting equation (1) with individual fixed-effects μ_i in our second specification:

$$\log(P_{i,n,h}) = \alpha + \beta_n \mathbb{1}(N = n) + \gamma_h \mathbb{1}(H = h) + \lambda_s + \mu_i + \varepsilon_{i,n,h}. \tag{2}$$

The introduction of individual fixed-effects has several implications. First, it keeps constant all individual characteristics at the time of the interview that might affect pricing behaviour. This is why the vector X_i can no longer be included. Second, only treated individuals will contribute to the estimation of the β_n parameters. As such, the reference category will no longer be individuals in the control groups, but each treated respondent's own appraisal in correspondence to one of the four surnames she is presented with, i.e., one surname becomes the reference category. Relying on only within-respondents variation rules out any bias coming from between-respondents comparisons and allows us to focus on discriminatory appraisals across surnames. Note that 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 99^{th} 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. This selection produces a balanced panel of 2,756 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. Figure 4 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 meter 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 Figure 4. 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 meter) by location from the Observatoire de l'habitat of the Luxembourg Ministry of Housing. These are shown as orange diamonds in Figure 4 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 A8 and Table A4 respectively.

Next, we assess the quality of responses by looking at the time participants spent appraising each property. In Appendix Figure A9, 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 A10 shows that respondents spend more time on the adverts when they are assigned to group T as compared to the passive control group *C2*, 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 *C1* 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 A5, we show that our main results are unaffected when controlling for the time spent on each advert.

Last, we check whether respondents are representative of the adult Luxembourgish society. The demographic features of our estimation sample displayed in column (1) in Table 3 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 (3) of Table 3, 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.

4 Results

4.1 Main Results of the Experiment

Table 4 summarises our main results. We first check whether the sole fact of seeing a name attached to a real-estate advert induces individuals to evaluate house prices in a different way as compared to having no contact information or having the impersonal contact of a (fictitious)

[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

real-estate agency. As hinted by the difference in means at the top line of Table 3, the first column of Table 4 shows that treated individuals give on average lower appraisals as compared to those in the control group. This result could be explained by two concurrent yet non-exclusive arguments: on the one hand, the presence of a private seller could arguably be perceived as an indication for poorer apartment and/or seller quality, as transpiring by the implicit owner's decision of not listing their property with a professional agency. If this was the only reason driving the negative coefficient of the treatment dummy, we should not expect any differences among the coefficients associated with the names appearing on the listings once disaggregating the treatment into the four name dummies. On the other hand, the negative coefficient of the treatment dummy might hide a negative appraisal bias towards one or more owner names. If, for example, one specific surname attracted substantially lower price estimates compared to the other surnames, this could already be enough to detect an overall negative "surname effect."

When splitting the treatment dummy into the four surname dummies in column (2) of Table 4, results show that the negative overall effect is mainly driven by one single surname: 'Mutombo'. Treated survey respondents systematically give lower appraisals to adverts in which a person named 'Mutombo' acts as seller, as compared to evaluations from respondents in the control group. 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 (3) to (5) of Table 4. In the full model specification reported in column (5), we derive the cleanest measure of the size of this penalty which corresponds to 6.6% of the average property value – transformed into currency units, this means an average price reduction of roughly EUR 40,000.

On top of the evidence of price discrimination against owners with a sub-Saharan African surname, we cannot rule out that the quality-reducing signal that might come from seeing the owner's name also plays some minor role, as suggested by the negative point estimates attracted by the remaining three names. We can, however, keep these concerns constant by including individual fixed effects into the model equation (1). This intuition results into model (2), where only treated individuals contribute to the estimation of the coefficients associated with surnames. As the control group can no longer be the reference category, we set one of the surnames in the treated group to serve this purpose. We choose the French-sounding surname 'Clement' as the reference category because it consistently attracts estimates that are the closest to zero. Column (6) of Table 4 displays the estimates of model (2). Even in this setting, 'Mutombo' attracts a negative and significant penalty. Motivated by the fact that neither 'Schmitt' nor 'daSilva' attract a significant penalty, we estimate the average penalty associated with 'Mutombo' as compared to any other name in column (7) which is, unsurprisingly, still negative and statistically different from zero at the 10% level.

4.2 Economic Significance of the Penalty: Winners and Losers

Our most conservative estimates from columns (6) and (7) in Table 4 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'.[15]

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 A6 and document an average within-individual appraisal penalty for 'Mutombo' of 5.8% (an expected price reduction of around EUR 40,000).[16] While these numbers may already

[15]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 A3 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.

[16]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

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., 2022, for details on the respecive 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 4), a discriminatory 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 discriminatory 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 discriminatory buyer could save ranges between 95% and 145% of the one-time tax credit known as *Bëllegen Akt* that is granted in Luxembourg for purchasing a home, which amounted up to EUR 20,000 per person in 2022.[17]

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

'Mutombo' to be worse than they would be with other owners.

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

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 4 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' 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 Jr, 1978; Machado and Silva, 2019), as detailed in Appendix Table A7. 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 A11, 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 4) on sample splits based on the following individual characteristics: gender, age, education, partnership status and parental country of origin.[18] Results are reported in Table 5, 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 5).[19] Second, our estimates indicate that the average racial penalty exhibits significant heterogeneity, with values ranging from EUR 0 to EUR 57,940.[20]

Older individuals, who in Table 5 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, thus contributing to the penalisation of African-ancestry sellers. Conversely, younger and more educated individuals, who on average do not penalise Mutombo in our experiment, are more likely to be perspective buyers.[21] While this could imply that the price penalty for Mutombo will converge to zero in future housing transactions, our analysis

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

[19]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 A2), we expect to see a higher penalty for 'Mutombo' in this sample. This prediction is confirmed by Appendix Table A8, 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 A2), we observe a qualitatively similar penalty for 'Mutombo'.

[20]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 5, 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.

[21]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/

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 A9. 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 A9 remain similar to those in Table 4, 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.[22]

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. However, it is important to bear in mind that identifying

[22]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 A10, 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 last 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.

which type of discrimination is at play is not an easy task (Guryan and Charles, 2013). The modern literature in Psychology even suggests that statistical discrimination 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 5 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 5, 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 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 what we find in Table 6: even if the point estimates are not statistically different from zero (likely reflecting the lack of statistical power coming 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.

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. 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 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.

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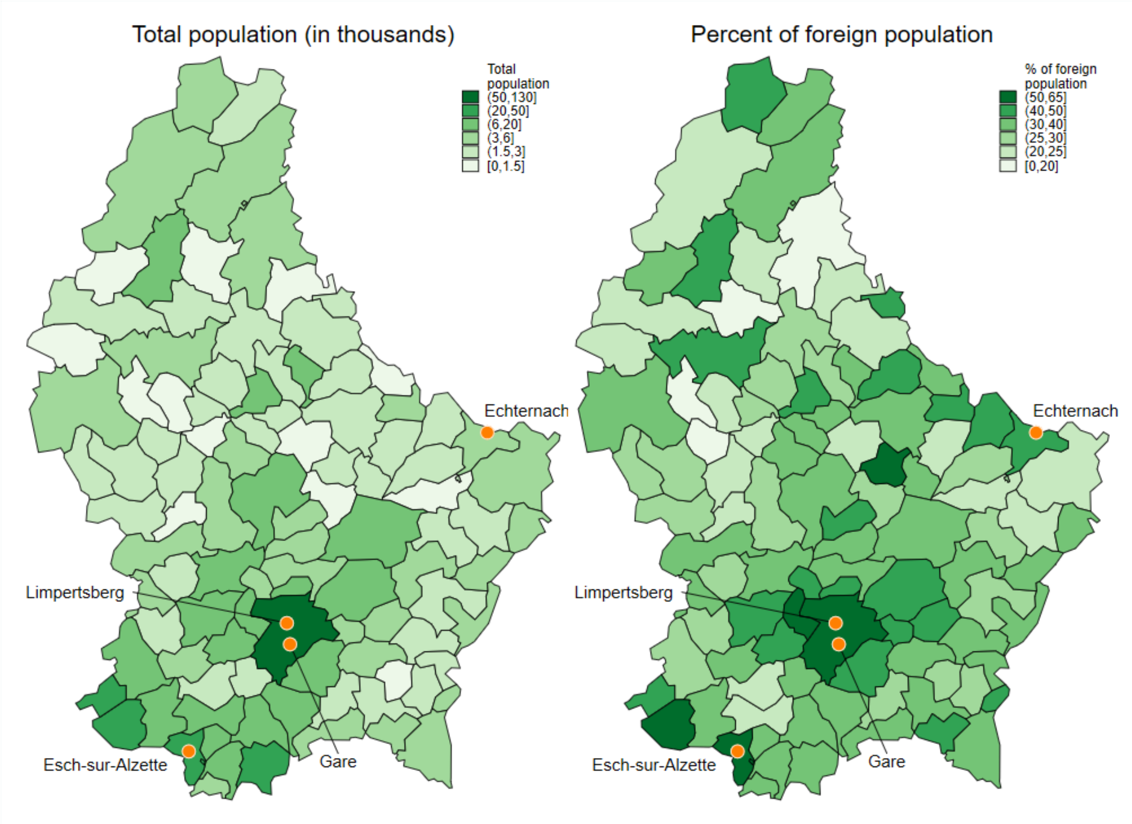
Figures and Tables

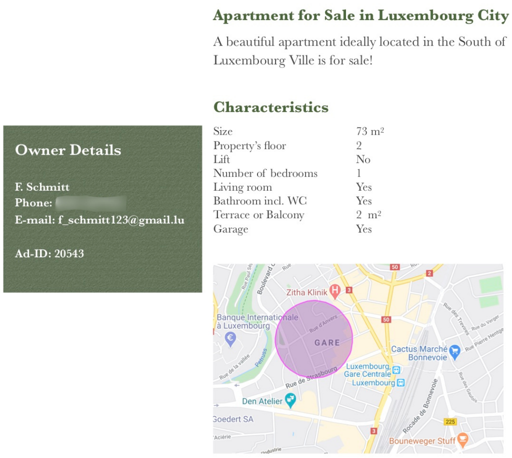
Figure 1: Local and foreign population in Luxembourg by municipality



Source: STATEC and CTIE.

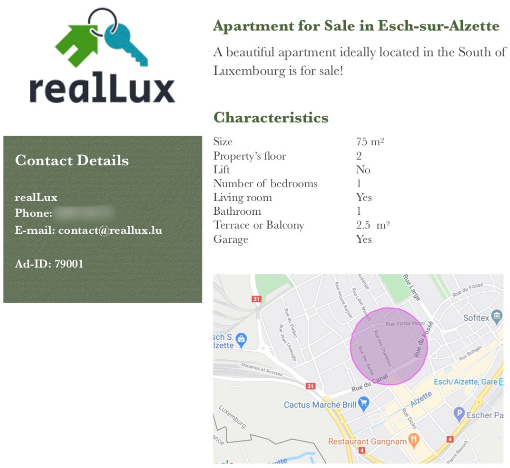
Notes: The green shaded areas represent municipalities in Luxembourg (Local Administrative Units in the European Statistical System). The four orange dots indicate the locations used in the experiment, namely city centres of the municipalities of Echternach and Esch-sur-Alzette and Luxembourg City neighbourhoods of Gare and Limpertsberg (exact locations correspond to centroids of the map circles displayed in Figure 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.

Figure 2: Selected Adverts

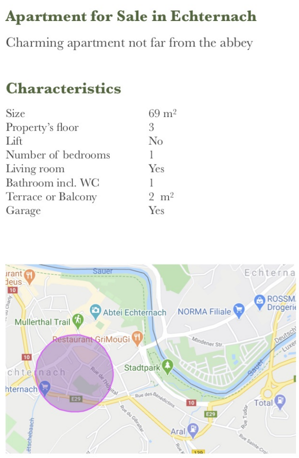


(a) Advertisement H1-N1

(b) Advisement H2-N4



(c) Advertisement H3C2



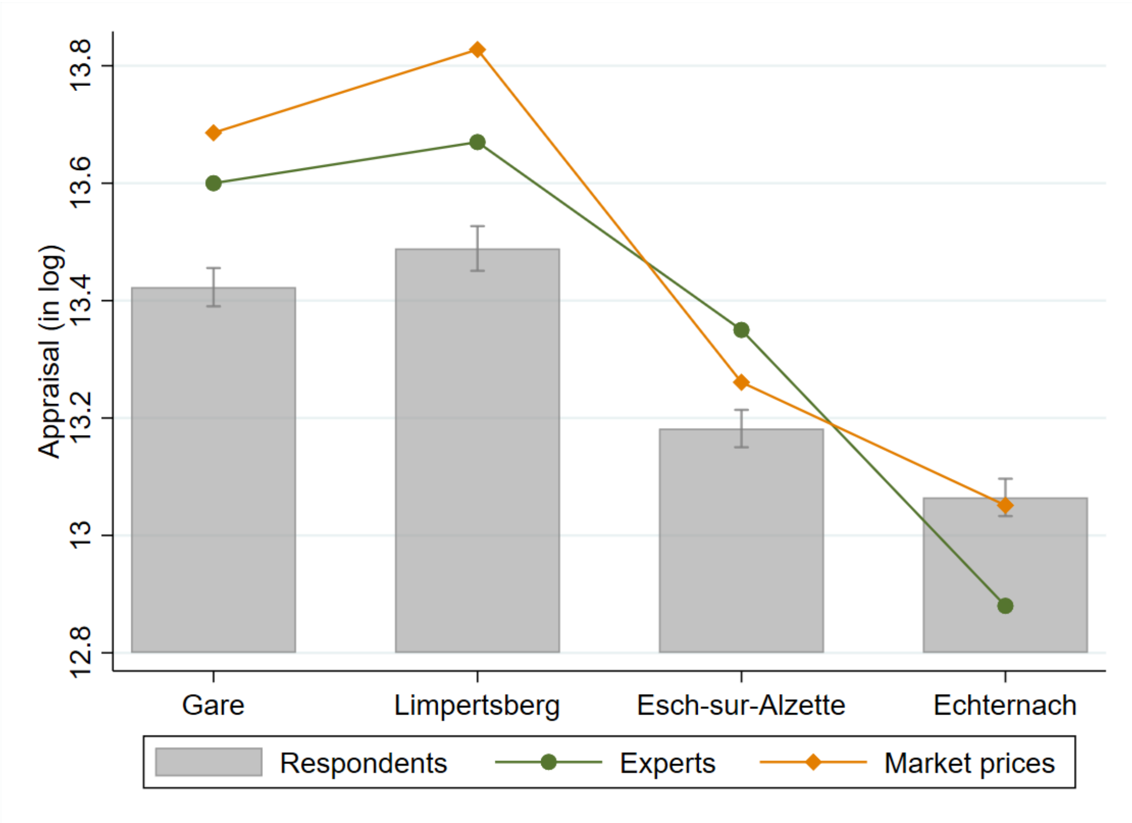
(d) Advertisement H4C1

Notes: The French and German versions of the adverts are documented in Appendix B.

Figure 3: Real-estate Agencies: LUXhouse and realLux.

Notes: We created logos for the fake real-estate agencies.

Figure 4: Average Appraisal per Advertised Dwelling



Notes: The gray 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.

Table 1: Dwellings Characteristics

	Location	Floor	Living area	Balcony / Terrace Area
H1	Luxembourg City / Gare area	2	73 m^2	2 m^2
H2	Luxembourg City / Limpersberg	3	68 m^2	2.5 m^2
H3	Esch-sur-Alzette	2	75 m^2	2.5 m^2
H4	Echternach	3	69 m^2	2 m^2

Notes: Each dwelling is associated with a unique (but meaningless) 5-digits "Ad-ID". We select locations (imprecisely identifiable via a circle on a map, see Figure 2) referring to commonly known areas in Luxembourg: two in Luxembourg City and two in smaller towns (Echternach and Esch-sur-Alzette). Other dwelling characteristics are the same in all adverts: one bathroom, one bedroom, separate living room, no lift, included parking lot in a garage.

Table 2: 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.

Table 3: 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	680112	702094	656059	-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.

Table 4: Appraisal and Information Treatment: Main Results

	Appraisal (in log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.048*						
	(0.028)						
Clement		-0.030	-0.034	-0.029	-0.030		
		(0.034)	(0.032)	(0.032)	(0.032)		
Schmitt		-0.052	-0.041	-0.036	-0.037	-0.007	
		(0.033)	(0.031)	(0.032)	(0.031)	(0.024)	
daSilva		-0.033	-0.040	-0.034	-0.035	-0.005	
		(0.031)	(0.031)	(0.031)	(0.031)	(0.021)	
Mutombo		-0.075**	-0.070**	-0.065**	-0.066**	-0.036*	-0.031*
		(0.034)	(0.032)	(0.032)	(0.032)	(0.021)	(0.019)
Observations	2756	2756	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.

Table 5: 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, individual and recruitment method 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.

Table 6: Appraisal and Information Treatment: By Level of Animus

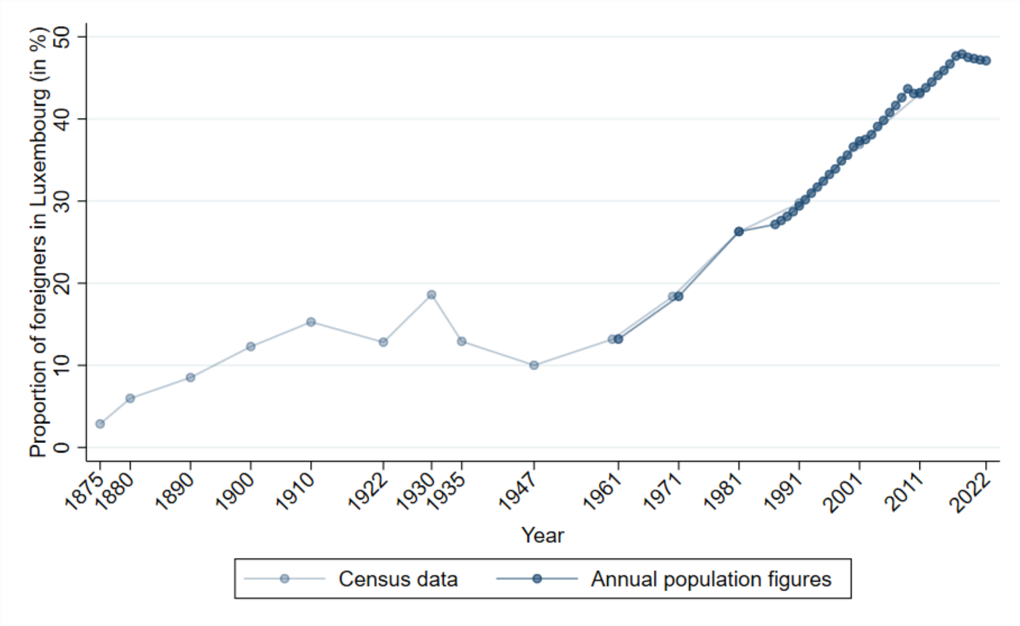
	Appraisal (in log)			
	Low animus		High animus	
	(1)	(2)	(3)	(4)
Schmitt	-0.013		0.000	
	(0.023)		(0.051)	
daSilva	-0.015		0.010	
	(0.025)		(0.039)	
Mutombo	-0.041	-0.031	-0.025	-0.029
	(0.029)	(0.023)	(0.032)	(0.034)
Observations	1756	1756	1000	1000

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Advert, individual and recruitment method 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.

Appendix A

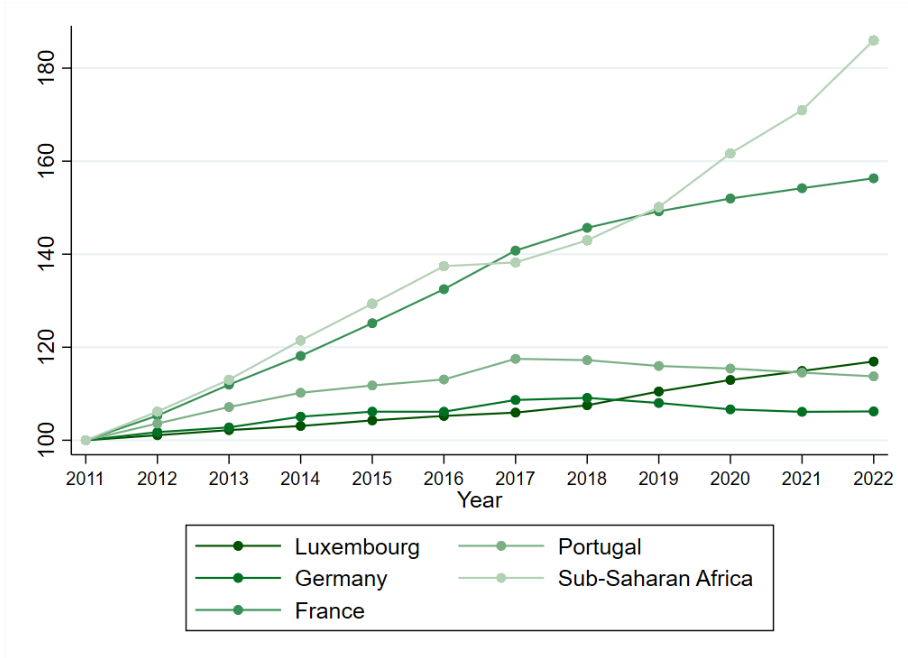
Additional Figures and Tables

Figure A1: Share of foreigners in Luxembourg over time (1875 to 2022)



Notes: The historical series dating farther back in time (lighter colour) comes from Census data collected by STATEC. The second series (darker colour) is given by annual population data on 1^{st} January collected by STATEC and the Centre des technologies de l'information de l'État (CTIE).

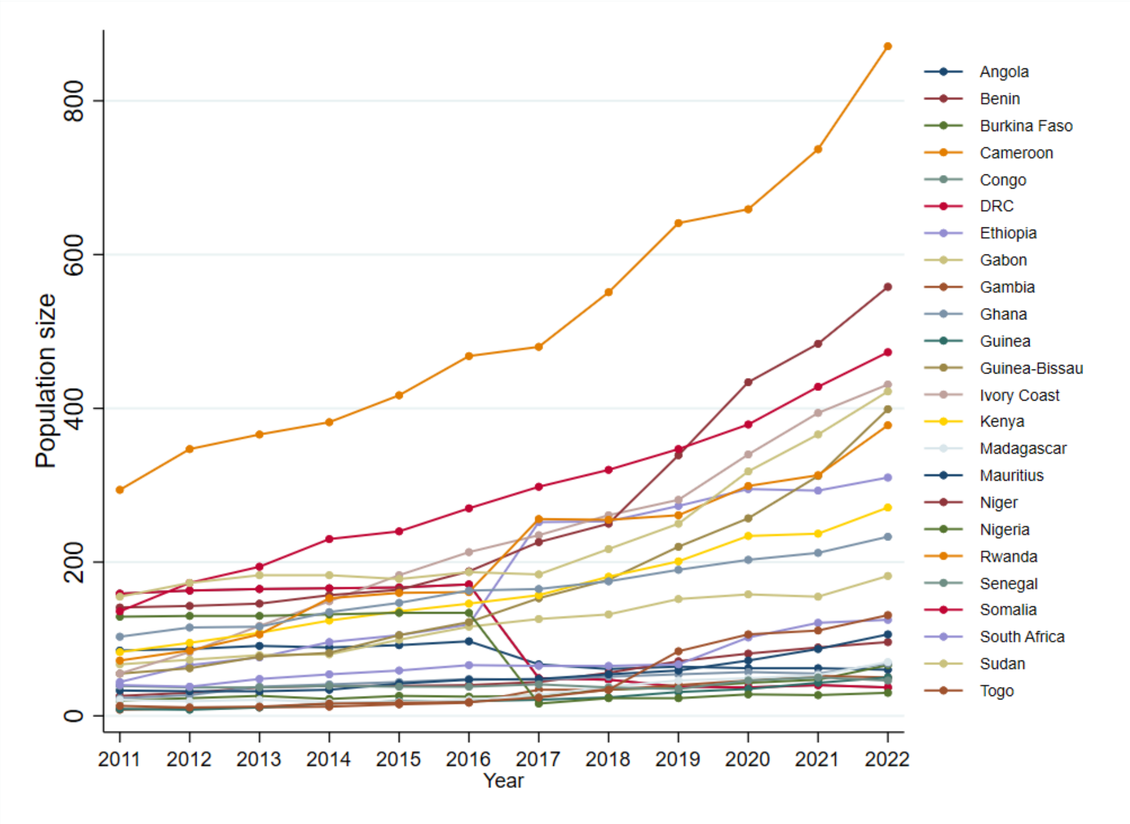
Figure A2: Luxembourg population over time by country/region of origin



Source: STATEC.

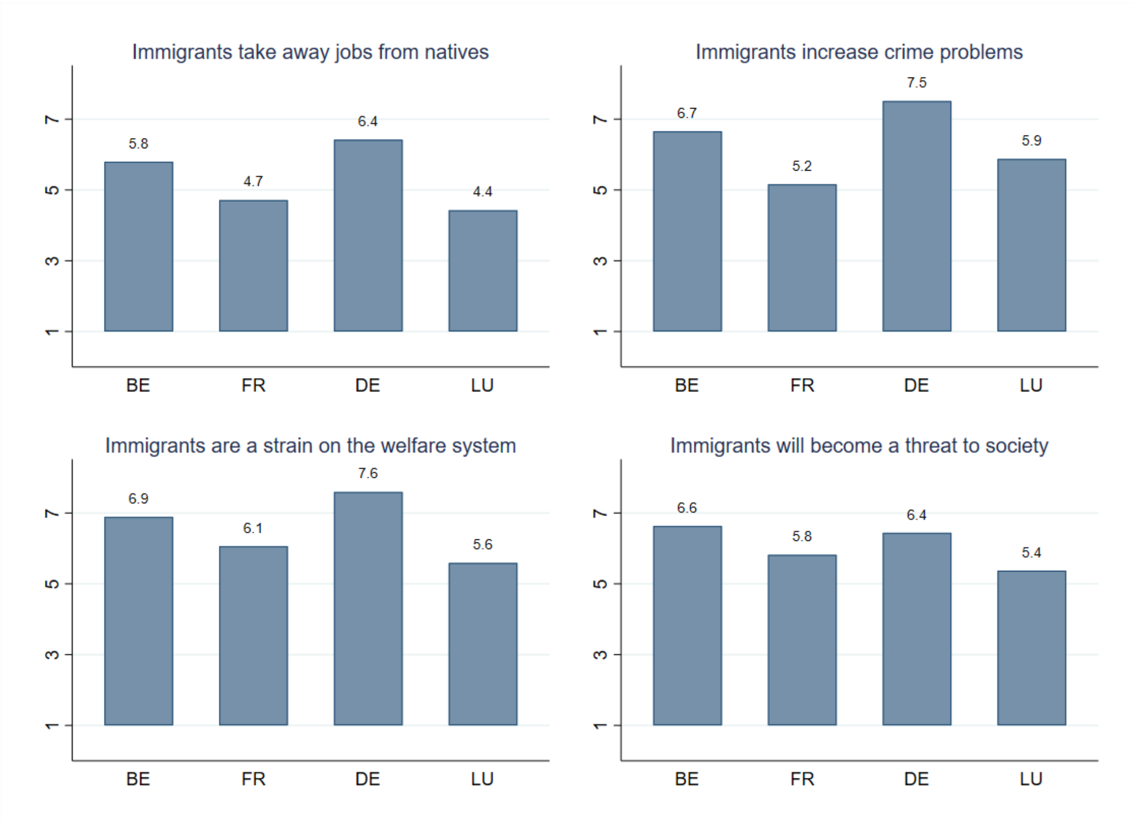
Notes: population figures are relative to base-year 2011. Absolute population figures by country of origin for 2011 are the following: 291,831 people from Luxembourg; 82,363 from Portugal; 31,456 from France; 12,049 from Germany; 4,425 from sub-Saharan African countries.

Figure A3: Luxembourg residents from sub-Saharan African countries over time



Source: STATEC.
Notes: For ease of representation, we here only consider sub-Saharan African countries with at least 50 nationals residing in Luxembourg at any given time-point between 2011 and 2022 (excluded countries with non-zero migration flows to Luxembourg are the following: Botswana, Burundi, Central African Republic, Chad, Comoros, Equatorial Guinea, Eritrea, Eswatini, Lesotho, Liberia, Malawi, Mali, Mauritania, Mozambique, São Tomé e Príncipe, Seychelles, Sierra Leone, South Sudan, Tanzania, Uganda, Zambia, Zimbabwe). Due to scale reasons, we also excluded Cape Verde, the country contributing the most to sub-Saharan African migration to Luxembourg (ranging from 50% of the total in 2011 to 30% in 2022).

Figure A4: Feelings about immigrants, by country (European Value Study)



Notes: Figures are based on the authors' own elaboration of 2008-2009 data from the European Values Study (EVS, 2022). Country abbreviations, indicated in the x-axis, are the following: "'BE" for Belgium, "FR" for France, "DE" for Germany, and "LU" for Luxembourg. Bars represent country means for each of the outcomes specified above the sub-graphs. All outcomes are expressed in a 1 (disagree) - 10 (agree) Likert scale.

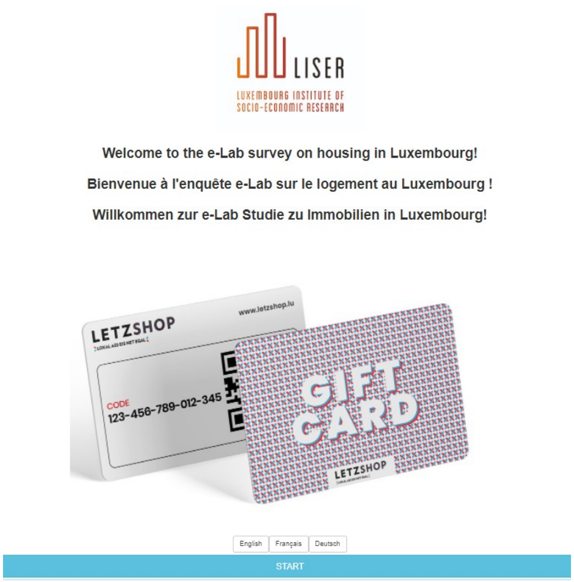
Figure A5: Average opinions about immigrants, by country (European Value Study)

Notes: Figures are based on the authors' own elaboration of 2008-2009 data from the European Values Study (EVS, 2022). Country abbreviations, indicated in the x-axis, are the following: "BE" for Belgium, "FR" for France, "DE" for Germany, and "LU" for Luxembourg. Bars represent country means for each of the outcomes specified above the sub-graphs. Outcome variables "Feel like a stranger in my own country" and "Too many immigrants in my country" are coded as binary indicators being equal to 1 if the respondent agrees or strongly agrees with the statement, and 0 otherwise. "Dislike immigrants/foreign workers as neighbours" is a dummy equal to 1 if the respondent mentioned immigrants/foreign workers as groups they would not like as neighbours, and zero otherwise.

Figure A6: Sample Social-Media Advert

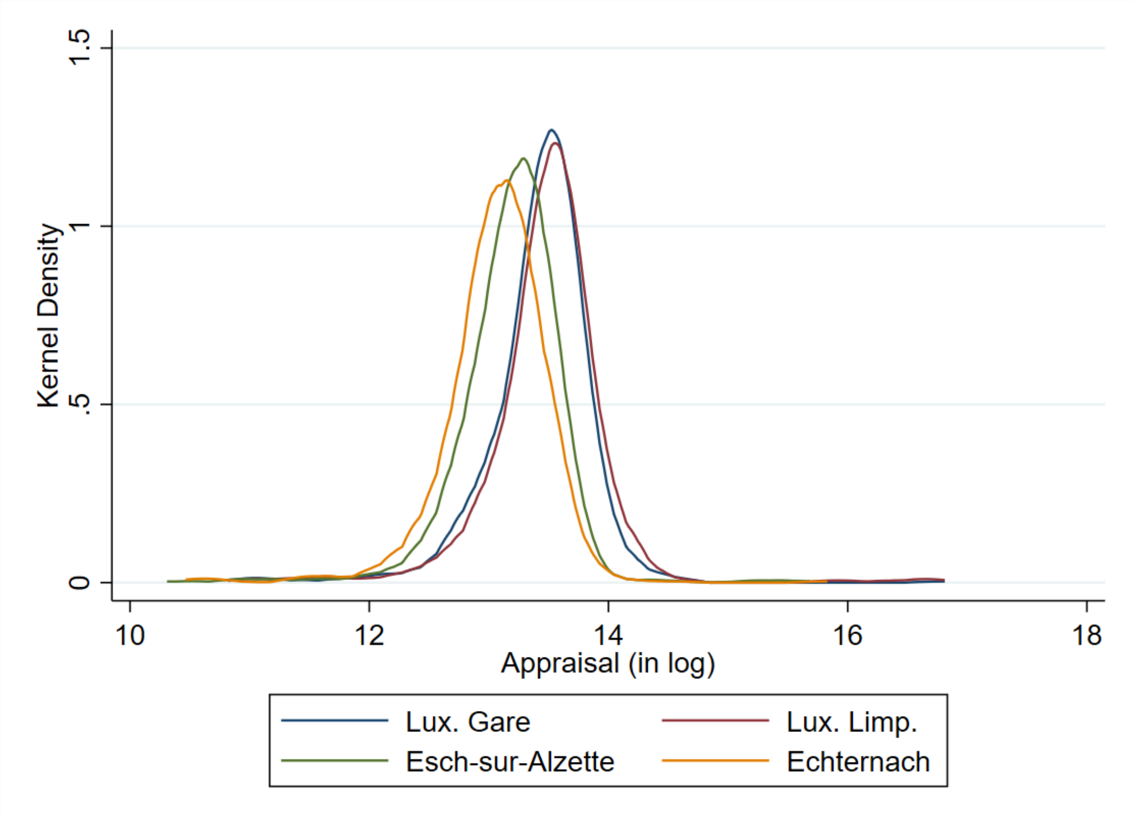
Notes: The figure shows a sponsored advert recruiting participants in our study.

Figure A7: Welcome Screen



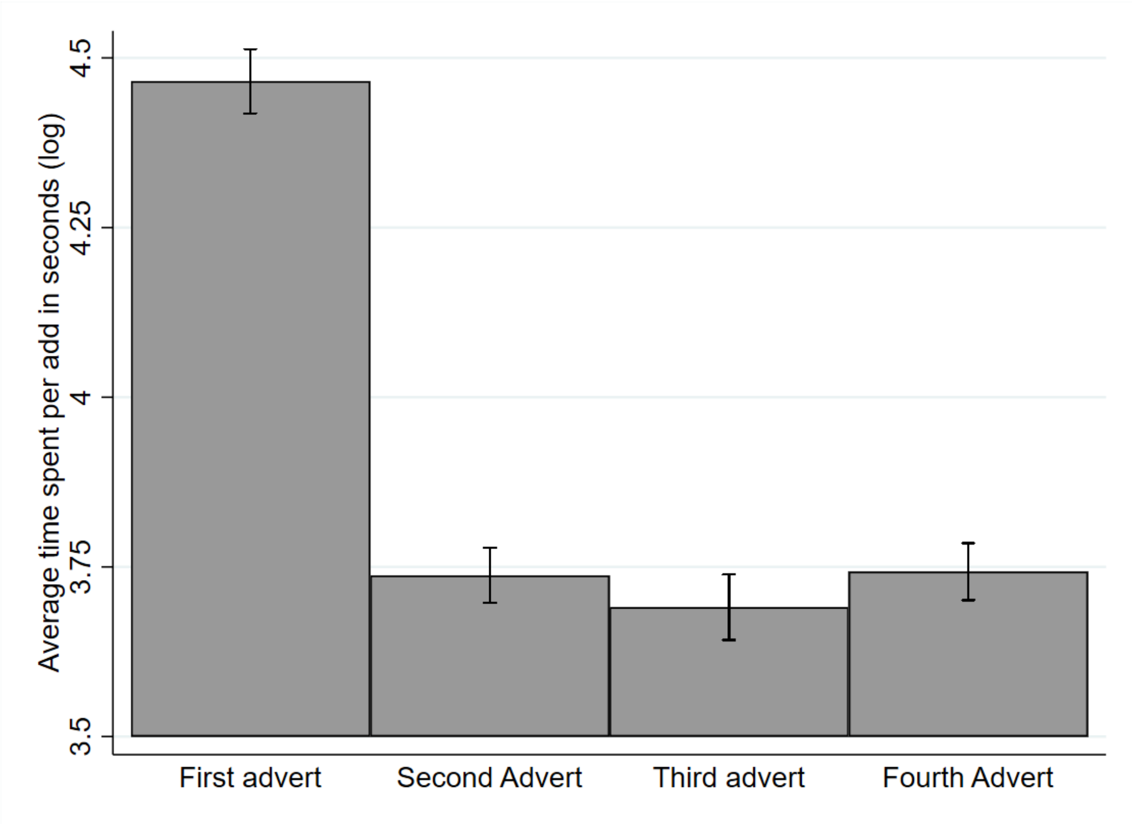
Notes: The figure shows how participants were able to select their preferred language version. Displaying their potential reward acted as an additional incentive to participate.

Figure A8: The Distribution of Appraisals by Location



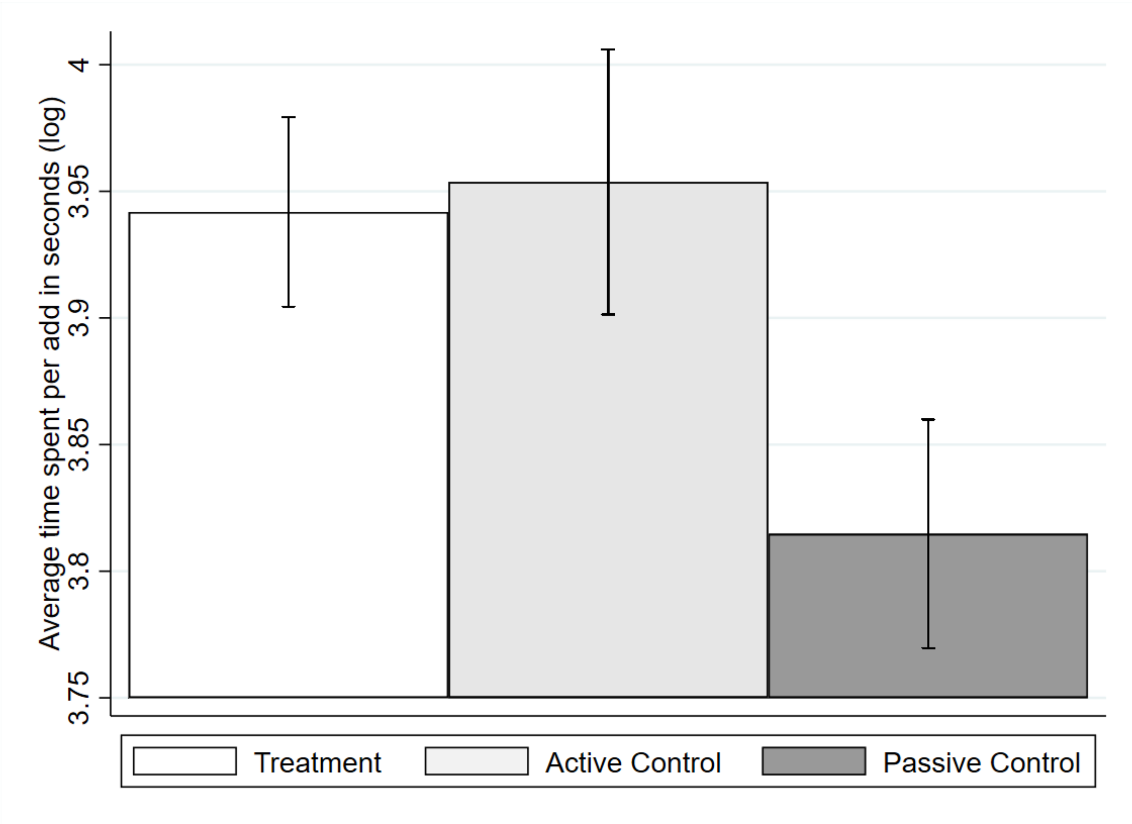
Notes: This figure refers to the treatment group. Appraisals are grouped by location. Epanechnikov kernel is used to estimate the density.

Figure A9: The Time Spent on Adverts by Order of Appearance



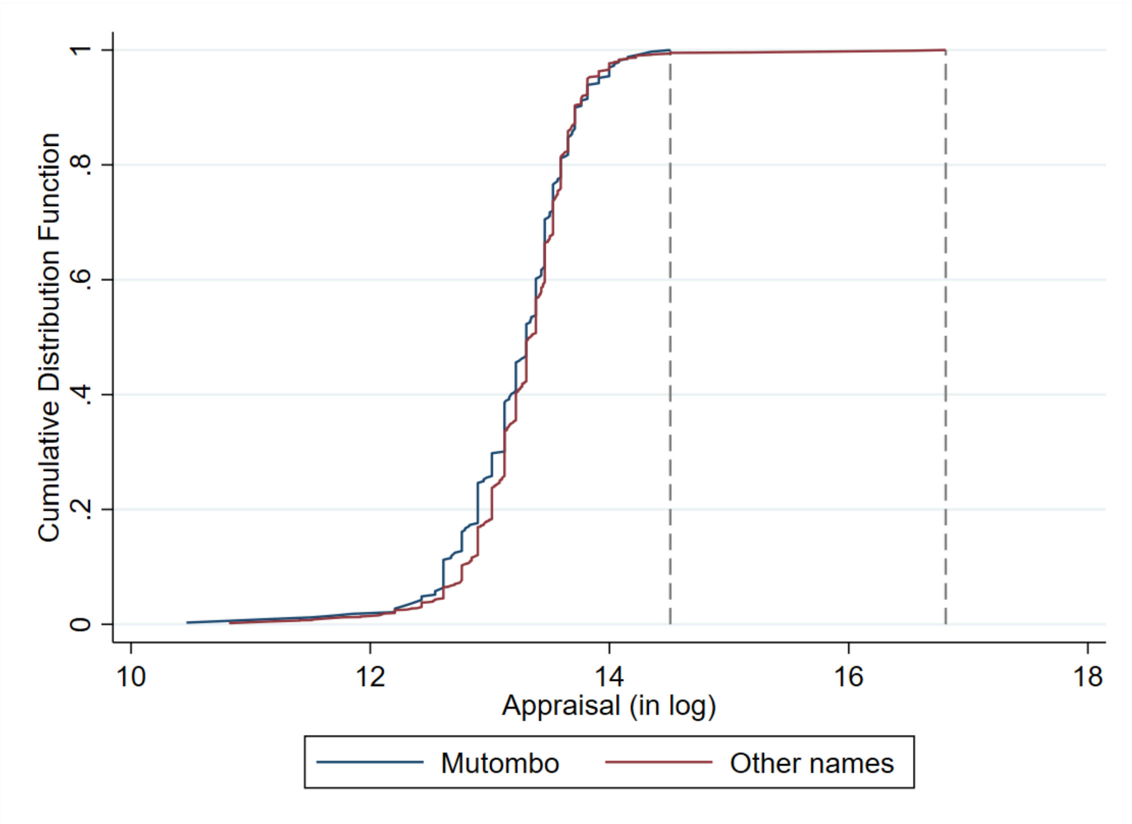
Notes: This figure refers to estimation sample. Adverts are grouped by order of appearance.

Figure A10: The Time Spent on Adverts by Treatment Status



Notes: This figure refers to estimation sample. Observations are grouped by treatment status.

Figure A11: Cumulative Distribution Functions of Appraisals: Mutombo vs. Other Sellers



Note: This figure refers to the treatment group. Appraisals for properties offered by Mutombo are contrasted against all other price sellers. The vertical dashed lines show the maximal appraisal received by Mutombo and the other sellers.

Table A1: Appraisal and Control Groups

	Appraisal (in log)			
	(1)	(2)	(3)	(4)
Fake Real-Estate Agency	-0.041	-0.038	-0.048	-0.049
	(0.040)	(0.039)	(0.040)	(0.039)
Observations	1440	1440	1440	1440
Experiment controls	.	✓	✓	✓
Exogenous controls	.	.	✓	✓
Endogenous controls	.	.	.	✓

Notes: These are linear regressions, based on the sample of individuals belonging to the two control groups. Standard errors in parentheses are clustered at the individual level. Experiment 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.

Table A2: Descriptive statistics by sample type

	Social Media (1)	EU-SILC (2)	Difference (2)-(1)
Age	39.578	47.123	7.545***
	[10.249]	[13.591]	(0.916)
Male	0.452	0.538	0.086**
	[0.498]	[0.500]	(0.039)
Female	0.539	0.458	-0.080**
	[0.499]	[0.499]	(0.039)
Non-binary	0.009	0.004	-0.005***
	[0.095]	[0.063]	(0.007)
Post-secondary education	0.624	0.391	-0.233***
	[0.485]	[0.489]	(0.038)
Luxembourg citizen	0.245	0.387	0.142***
	[0.431]	[0.488]	(0.036)
Homeowner	0.617	0.838	0.221***
	[0.487]	[0.369]	(0.035)
In couple	0.674	0.755	0.081**
	[0.469]	[0.431]	(0.036)
With a child	0.507	0.684	0.177***
	[0.501]	[0.466]	(0.039)
Employed	0.880	0.731	-0.149***
	[0.324]	[0.444]	(0.029)
Monthly HH income (in log)	7.864	8.063	0.199
	[2.044]	[1.511]	(0.147)
Observations	689	360	329

Notes: Columns (1) and (2) report the average values of participants' characteristics in the Social Media and EU-SILC sample 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.

Table A3: Appraisal and Information Treatment: Residents Only

	Appraisal (in log)		
	(1)	(2)	(3)
Panel A: Ref. = Clement			
Schmitt	-0.007	-0.010	-0.012
	(0.024)	(0.026)	(0.027)
daSilva	-0.005	-0.013	-0.013
	(0.021)	(0.023)	(0.024)
Mutombo	-0.036*	-0.045*	-0.045*
	(0.021)	(0.023)	(0.024)
Observations	2756	2492	2380
Panel B: Ref. = All other names			
Mutombo	-0.031*	-0.038*	-0.036*
	(0.019)	(0.021)	(0.021)
Observations	2756	2492	2380

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.

Table A4: Descriptive statistics by advert

	Q10	Q25	Q50	Q75	Q90	Mean
Appraisal (in log)						
Luxembourg Gare	12.90	13.30	13.47	13.65	13.82	13.42
Luxembourg Limpertsberg	13.02	13.30	13.53	13.71	13.91	13.49
Esch-sur-Alzette	12.77	13.02	13.22	13.42	13.59	13.18
Echternach	12.61	12.90	13.12	13.30	13.46	13.06
Appraisal (in thousand EUR)						
Luxembourg Gare	400	600	710	850	1,000	749.2
Luxembourg Limpertsberg	450	600	750	900	1,100	875.0
Esch-sur-Alzette	350	450	550	670	800	581.0
Echternach	300	400	500	600	700	515.3

Note: These figures refer to our estimation sample.

Table A5: Appraisal and Information Treatment: Main Results when keeping Response Time Constant

	Appraisal (in log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.045						
	(0.029)						
Clement		-0.025	-0.030	-0.025	-0.027		
		(0.034)	(0.032)	(0.032)	(0.032)		
Schmitt		-0.053	-0.043	-0.038	-0.039	-0.011	
		(0.033)	(0.031)	(0.032)	(0.031)	(0.024)	
daSilva		-0.029	-0.035	-0.031	-0.032	-0.006	
		(0.031)	(0.031)	(0.031)	(0.030)	(0.021)	
Mutombo		-0.071**	-0.066**	-0.062*	-0.063**	-0.036*	-0.031
		(0.034)	(0.033)	(0.032)	(0.032)	(0.021)	(0.019)
Time per advert	-0.047**	-0.048**	-0.054***	-0.051***	-0.049***	-0.032**	-0.032**
(in log sec)	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.015)	(0.015)
Observations	2756	2756	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.

Table A6: Future Appraisal and Information Treatment: Main Results

	Future Appraisal (in log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.046						
	(0.032)						
Clement		-0.016	-0.019	-0.011	-0.012		
		(0.040)	(0.039)	(0.039)	(0.039)		
Schmitt		-0.055	-0.042	-0.035	-0.036	-0.023	
		(0.038)	(0.037)	(0.036)	(0.036)	(0.030)	
daSilva		-0.027	-0.031	-0.024	-0.025	-0.013	
		(0.037)	(0.037)	(0.036)	(0.037)	(0.032)	
Mutombo		-0.084**	-0.077**	-0.069*	-0.070*	-0.058**	-0.046*
		(0.039)	(0.038)	(0.037)	(0.037)	(0.030)	(0.026)
Observations	2756	2756	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. Experiment 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.

Table A7: Appraisal and Information Treatment: Quantile Analysis

	Appraisal (in log)								
	Q10	Q20	Q30	Q40	Q50	Q60	Q70	Q80	Q90
Panel A: Ref. = Clement									
Schmitt	0.001	-0.001	-0.003	-0.005	-0.007	-0.009	-0.011	-0.013	-0.015
	(0.048)	(0.034)	(0.029)	(0.035)	(0.049)	(0.067)	(0.081)	(0.097)	(0.118)
daSilva	-0.000	-0.002	-0.003	-0.004	-0.005	-0.007	-0.008	-0.009	-0.010
	(0.048)	(0.034)	(0.030)	(0.035)	(0.050)	(0.068)	(0.082)	(0.097)	(0.119)
Mutombo	-0.040	-0.039	-0.038	-0.037	-0.035	-0.034	-0.033	-0.032	-0.031
	(0.049)	(0.034)	(0.030)	(0.035)	(0.050)	(0.068)	(0.083)	(0.098)	(0.120)
Observations	2756								
Panel B: Ref. = All other names									
Mutombo	-0.040	-0.038	-0.036	-0.034	-0.031	-0.029	-0.027	-0.025	-0.023
	(0.036)	(0.029)	(0.025)	(0.021)	(0.019)	(0.021)	(0.025)	(0.029)	(0.036)
Observations	2756								

Notes: Standard errors in parentheses are bootstrapped. 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.

Table A8: Appraisal and Information Treatment: Main Results per Sample

	Appraisal (in log)													
	Social Media Sample							EU-SILC Sample						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treatment	-0.036							-0.073						
	(0.036)							(0.047)						
Clement		-0.025	-0.043	-0.042	-0.051				-0.041	-0.019	-0.016	-0.007		
		(0.042)	(0.040)	(0.040)	(0.040)				(0.056)	(0.053)	(0.054)	(0.054)		
Schmitt		-0.033	-0.028	-0.027	-0.035	0.015			-0.094*	-0.064	-0.061	-0.052	-0.045	
		(0.041)	(0.039)	(0.039)	(0.039)	(0.031)			(0.054)	(0.054)	(0.055)	(0.054)	(0.035)	
daSilva		-0.017	-0.034	-0.034	-0.042	0.009			-0.068	-0.050	-0.048	-0.039	-0.032	
		(0.039)	(0.039)	(0.038)	(0.039)	(0.029)			(0.051)	(0.051)	(0.052)	(0.051)	(0.029)	
Mutombo		-0.069*	-0.050	-0.050	-0.058	-0.008	-0.016		-0.090	-0.106*	-0.104*	-0.095	-0.088**	-0.062
		(0.042)	(0.039)	(0.038)	(0.038)	(0.024)	(0.021)		(0.059)	(0.059)	(0.059)	(0.058)	(0.043)	(0.038)
Observations	1744	1744	1744	1744	1744	1744	1744	1012	1012	1012	1012	1012	1012	1012
Experiment controls	.	.	✓	✓	✓	✓	✓	.	.	✓	✓	✓	✓	✓
Exogenous controls	.	.	.	✓	✓	.	.	.	.	.	✓	✓	.	.
Endogenous controls	.	.	.	.	✓	.	.	.	.	.	.	✓	.	.
Individual FE	.	.	.	.	.	✓	✓	.	.	.	.	.	✓	✓

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Experiment 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.

Table A9: Appraisal and Information Treatment: Keeping the In-Group Premium Constant

	Appraisal (in log)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.054*						
	(0.029)						
Clement		-0.040	-0.044	-0.023	-0.025		
		(0.039)	(0.036)	(0.036)	(0.035)		
Schmitt		-0.058*	-0.047	-0.033	-0.034	-0.009	
		(0.034)	(0.032)	(0.031)	(0.031)	(0.025)	
daSilva		-0.035	-0.041	-0.033	-0.034	-0.008	
		(0.031)	(0.031)	(0.031)	(0.030)	(0.024)	
Mutombo		-0.075**	-0.071**	-0.064**	-0.065**	-0.039*	-0.032*
		(0.034)	(0.033)	(0.032)	(0.032)	(0.023)	(0.019)
Intra-Nationality Premium	0.026	0.019	0.017	-0.011	-0.010	-0.007	-0.004
	(0.029)	(0.029)	(0.035)	(0.030)	(0.030)	(0.020)	(0.018)
Observations	2756	2756	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. Experiment 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.

Table A10: Falsification Tests

	Expect Market Growth		Expect Rise in Own House Value		Life Satisfaction		Self-Assessed Health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.017	0.015	-0.005	-0.007	0.201	0.174	0.028	0.018
	(0.026)	(0.026)	(0.020)	(0.020)	(0.151)	(0.147)	(0.062)	(0.061)
Observations	689	689	689	689	689	689	689	689
Controls	.	✓	.	✓	.	✓	.	✓

Notes: These are linear regressions. Standard errors in parentheses are clustered at the individual level. Controls are advert and session fixed effects, age, age squared, gender, a dummy for post-secondary education, 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.

Appendix B

German and French Translations

Taking into account the multilingualism of the Luxembourg company, participants in the survey-experiment could choose between three language versions: English (EN), French (FR) and German (DE). The translations have been double-checked by native French- and German-speaking economists.

Figure B1 displays the French and German translations of the adverts in Figure 2. The core questions presented in section 3.2 are printed here in all three languages:

Question (i)

EN In your opinion, how much is this home worth? In other words, how much would you pay for purchasing it today?

FR À votre avis, combien vaut cette maison? En d'autres termes, combien paieriez-vous pour l'acheter aujourd'hui?

DE Wie viel ist dieses Zuhause Ihrer Meinung nach wert? Mit anderen Worten, wie viel würden Sie heute für den Kauf bezahlen?

Question (ii)

EN And in 5 years from today?

FR Et dans 5 ans à partir d'aujourd'hui?

DE Und in 5 Jahren ab heute?

Figure B1: Selected Adverts: French and German versions.

(a) Advert H1-N1: French

(b) Advert H2-N4: French

(c) Advert H3C2: French

(d) Advert H4C1: French

(e) Advert H1-N1: German

(f) Advert H2-N4: German

(g) Advert H3C2: German

(h) Advert H4C1: German

Notes: the English version of the advertisements above is presented in Figure 2.

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