Slavs Only: Open Xenophobia and Racial Disparities in Rental Housing

Vladimir Avetian[*] Viktor Veterinarov[†]

March 26, 2025

Abstract

We collect and examine a unique six-month dataset of daily listings from Moscow's rental market, in which we find that 20% explicitly state racial preferences. Using a novel design that compares apartments within the same building, we establish a causal link between discrimination and prices, showing that discriminatory apartments are 4% cheaper. A subsequent correspondence experiment reveals that overt and subtle discrimination coexist and act as complements. The relative response rates match those observed in the most discriminatory U.S. cities. Our findings suggest that prejudiced landlords are willing to pay for their bias, thereby driving racial rent differentials.

JEL classification: J15, O18

[*]University Paris-Dauphine - PSL, *Chair of Governance and Regulation*: `vladimir.avetian@dauphine.psl.eu`

[†]Sciences Po, Economics Department: `viktor.veterinarov@sciencespo.fr`

We would like to thank Dmitry Arkhangelsky, Manuel Arellano, Roland Bénabou, Sergey Belev, Eric Brousseau, Clément de Chaisemartin, Pierre-Philippe Combes, Tom Cui, Ellora Derenoncourt, Ruben Durante, Ilya Eryzhenskiy, Roberto Galbiati, Sergei Guriev, Clément Imbert, Vladimir Ivanov, Andrei Kalk, Pamina Koenig, Benjamin Marx, Sultan Mehmood, John Nye, Florian Oswald, Stefan Pauly, Nina Roussille, Olga Suchkova, Koen Schoors, Ekaterina Zhuravskaya for their helpful comments and feedback. This paper combines two earlier manuscripts that were independently developed by two research teams: "Consider the Slavs: Overt Discrimination and Racial Disparities in Rental Housing" and "Slavs Only: Ethnic Discrimination and Rental Prices." We are grateful to Vladimir Ivanov, who mentored and contributed to the development of the second manuscript, and to Sergei Guriev, who closely supervised the first. We registered the correspondence experiment; registration ID: AEARCTR-0014583.

1. INTRODUCTION

Societies marked by racial prejudice pay a significant price. Not only do they miss out on the opportunities that come with the inclusion of diverse residents (Alesina & La Ferrara, 2005; Rohner & Zhuravskaya, 2023), but they also suffer from self-perpetuating inequality (Derenoncourt, Kim, Kuhn, & Schularick, 2023). This issue is particularly notable in global cities, which frequently serve as gateways to opportunity (Chetty, Hendren, Kline, & Saez, 2014). More specifically, a key aspect of city life that can obstruct this pathway is housing, which is often affected by considerable discriminatory constraints (Christensen, Sarmiento-Barbieri, & Timmins, 2021; Flage, 2018).

Although prejudice may be omnipresent in many societies, it is often invisible. To uncover discrimination, researchers have developed a comprehensive set of tools, such as correspondence and audit studies (Bertrand & Duflo, 2023; Bertrand & Mullainathan, 2004). However, these methods, while insightful, often rely on indirect and costly interventions. At the same time, in various countries, the rise of online platforms has made more blatant forms of discrimination observable, resembling those seen in the United States before the Civil Rights Act of 1964.[1] This trend becomes especially noticeable when content moderation is absent. Except for studies on gender discrimination in labor (Kuhn & Shen, 2013, 2023; Kuhn, Shen, & Zhang, 2020), such overt discrimination has received little attention in the economic literature, despite its presence in other areas, including housing. While overlooked in the literature, studying directly observable prejudice is particularly relevant for understanding the impact of racial prejudice on housing markets.

In this paper we draw on the unique dataset of Moscow's rental housing market, where landlords discriminate explicitly. On the most popular Russian online listing site, CIAN, at the time of this study, more than 20% of listings included racial requirements for tenants. Discriminatory language in listings is typically represented by phrases such as "considering Slavs[2] only", "apartment for Russian tenants only", "no Asians", where in this

[1]For example, in the US, where overt discrimination has been illegal since 1964, Craigslist's Chicago rental platform in 2006 hosted listings with overt discrimination, with some explicitly stating "NO MINORITIES", leading to a lawsuit by the Chicago Lawyers' Committee for Civil Rights. Although a court ruling absolved Craigslist of liability for user-submitted content, the platform began moderating and removing overtly discriminatory listings.

[2]The term "Slavs" in this context is loosely defined and often used to refer to "Whiteness" or a European appearance. Observations show that landlords often exclude people with darker skin, Asian or Caucasian features, ethnic names, or noticeable accents (Avrutin, 2022).

context, “Slavic” implies a European appearance. The overt discrimination in Moscow's housing market is measurable, consistent, represents a significant portion of the market, and, therefore, may serve as an ideal laboratory for studying discriminatory constraints and their consequences.

We collect daily snapshots of all available listings during a six-month observational period and compile an “atlas of prejudice” for Moscow, shedding light on a question previously difficult for empirical research to access: Does prejudice in the market distort prices and generate racial differentials in rents? The link between discrimination and racial disparities has been contested in economics since Becker (Becker, 2010; Heckman, 1998; Neal & Johnson, 1996). Notably, Heckman argued that observed disparities are likely not driven by discrimination but by differences in group characteristics, such as human capital, social capital, or selective participation in markets. Today, both discrimination – such as differences in response rates between minority and majority applicants – and rent differentials, in which minorities often pay higher prices for the same units, are well-documented in numerous studies (Christensen et al., 2021; Early, Carrillo, & Olsen, 2019). Yet, the link between these two phenomena remains unclear due to the elusive nature of discrimination. This is the gap we aim to address in the present study.

However, even when discrimination is directly observable, estimating its causal effect on prices remains challenging. For instance, in Moscow, more upscale areas tend to show lower levels of discrimination. Additionally, different segments of the market may attract varying demand compositions, including by race, which is also likely correlated with the level of discrimination. To overcome these challenges and to test whether or not overt discrimination leads to racial rent differential, we employ a unique design that leverages within-building variation in discrimination. In our baseline specification, we estimate a hedonic regression with building-level fixed effects and a set of apartment-level controls, including the number of days an apartment was present on the market before being rented and other proxies for apartment quality. We find that discrimination leads to a notable racial rent differential, with apartments labelled as “only for Russians” being 4% less expensive than comparable apartments in the same building that do not practice such discrimination. This finding is robust, demonstrating consistency across various specifications.

Even though our baseline specification, which includes building fixed effects, is highly stringent, it is still possible that discriminating and non-discriminating apartments differ in other aspects of their textual descriptions. For example, phrases indicating overt discrimination might be associated with poor-quality apartments, meaning our estimates could reflect apartment quality rather than the true effect of discrimination. To address this, we apply a rigorous approach leveraging advanced text-as-data techniques. Using an OpenAI-provided embedding model[3], we extract several semantic indicators from apartment descriptions that are positively correlated with rental prices. We then show that our main discrimination effect is not mitigated by these variables. Additional placebo tests in the Appendix further support our findings. Finally, while we work with advertised rents, we focus on the last listed price before the apartment is removed from the market, which is likely as close as possible to the transaction price, given that rents tend to decrease slightly over time[4]. The regression results for the first-day price yield coefficients are identical to the last-day price: rent and overt discrimination are observed in market equilibrium.

This result carries important implications. First, it indicates the racial price premium: minorities, on average, face higher rent than the majority for identical apartments in the same building as a consequence of discrimination. This aligns with existing theories on the impact of discrimination in search markets (Black, 1995; Lang & Lehmann, 2012). In this case, the intuition is that the presence of discriminators in the market increases search costs for minority tenants, which generates the racial rent differential.

Second, it reveals that landlords who discriminate are, in fact, willing to accept a lower rent as the price for their biases. The observation that discriminators are willing to pay for their prejudice coincides with the intuition of Becker's animus-based model. It also mirrors findings from previous empirical research that were obtained in more staged, experimental settings. In the labor market, Hedegaard and Tyran (2018) shows that coworkers are willing to forgo profit to avoid working with colleagues from particular ethnic groups.

In the second part of this paper, we conduct a standard correspondence experiment – an approach that traditionally has been utilized to uncover subtle discrimination (Bertrand & Mullainathan, 2004). We send two types of messages to a sample of listings the day after

[3]Approaches based on embedding have found wide applications in recent economic literature (Ash & Hansen, 2023; Gennaro & Ash, 2022).

[4]On average, the price of apartments decreases only by 1.8% from the first to the last day of the listing. 72.5% of apartments in our sample maintain the same price throughout their listing period.

their publication, manipulating the names of the sender (potential tenant) to sound either ethnically Russian or non-Russian.

This exercise aims to achieve several goals. First, we validate our measure of overt discrimination by demonstrating that it is associated with a significantly lower likelihood of receiving a response from landlords for a minority-sounding tenant. Second, we reveal that both overt and subtle forms of discrimination coexist in the market. This is evident from the lower relative likelihood of responses to "non-Russian" tenants, even among listings that do not exhibit overt discrimination. Third, we show that overt and subtle discrimination act as complements rather than substitutes: the negative effect of a non-Russian-sounding name on the relative response rate is more than two times larger when the listing text includes a discriminatory disclaimer. This finding allows us to rule out an alternative explanation for the main result that the observed differential might not be driven by discrimination itself but rather by switching between overt and subtle forms of discrimination, possibly due to the social stigma associated with overt discrimination. Thus, this indicates that overt discrimination is indeed a good proxy for prejudiced preferences, and it is likely that racial differentials in rents are caused by these preferences.

This study contributes to several strands of literature. First, we introduce an insight into the literature on discriminatory constraints by establishing a direct link between discriminatory preferences of landlords and racial differentials in rent. Regarding the existence of racial differentials in both rent and sales, the bulk of the evidence comes from the U.S. (Bayer, Casey, Ferreira, & McMillan, 2017; Box-Couillard & Christensen, 2023; Early et al., 2019; Yinger, 1997). A recent study by Early et al. (2019) shows that Blacks pay 0.6 - 2.4% higher rent than Whites for identical housing in identical neighborhoods. Numerous papers document discrimination in the housing market through correspondence and audit experiments (Ahmed & Hammarstedt, 2008; Carpusor & Loges, 2006; Christensen et al., 2021; Christensen & Timmins, 2022, 2023; Hanson & Hawley, 2011). The most recent U.S. study Christensen et al. (2021) reports relative response rate differentials of -9.3% for African American renters. We also complement this literature by providing evidence from a region outside the U.S. and Western Europe. This evidence suggests that discriminatory constraints in other societies may be higher and that some lessons drawn from the U.S. context do not universally apply. For example, Christensen and Timmins (2023) found stronger discriminatory constraints in high-amenity and high-price neighborhoods, whereas this study reveals an opposite association. When it comes to the Russian context, Bessudnov and Shcherbak (2020) identifies differential treatment in the Russian labor market using a correspondence study. Finally, more specifically, we contribute to the literature on housing discrimination on online platforms (Edelman, Luca, & Svirsky, 2017; Laouénan & Rathelot, 2022).

Another significant contribution is to the literature on the economic and social impacts of racial prejudice. We enhance this field by presenting direct evidence of prejudice, based on the "revealed preferences" of landlords, and by demonstrating its connection to prices. Previously, researchers have measured prejudice using surveys (Bursztyn, Chaney, Hassan, & Rao, 2024; Charles & Guryan, 2008), newspaper analyses (Ferrara & Fishback, 2022), Google search data (Stephens-Davidowitz, 2014), or implicit bias tests (Bertrand, Chugh, & Mullainathan, 2005; Glover, Pallais, & Pariente, 2017). While we are the first to use data on overt racial discrimination in the housing market, overt discrimination is far from being a marginal phenomenon when it comes to online platforms. Moreover, in the neighboring field of gender discrimination in labor, several published works use overt discrimination data from China (Kuhn & Shen, 2013, 2023; Kuhn et al., 2020). Although we also use overt discrimination data, there are key differences: our focus is on housing and race, and we are the first to link overt discrimination to prices.

Finally, this paper sheds light on a subject that attracted little attention in economics before: the relationship between overt and subtle forms of discrimination. At the same time, discussion of the forms of discrimination has been extensively studied in other social sciences (Jones, Peddie, Gilrane, King, & Gray, 2016; Pager, 2007; Small & Pager, 2020). As an example, in the context of organizations, overt and subtle forms are known to be correlated (Jones et al., 2016). Our findings complement this evidence by suggesting that the two forms of discrimination in the housing market may behave as complements.

The remainder of the paper is organized as follows. Section 2 provides background information, and Section 3 covers data and descriptive statistics. Section 4 explains the empirical strategy, and Section 5 presents the main results. Section 6 is dedicated to the correspondence study, and Section 7 concludes the paper.

2. Background

Ethnicity and Discrimination. Russia is a multinational state, with 19% of its population identifying as non-ethnic Russians (Census, 2010). In addition, the country has a large immigrant population. According to UN data, approximately 11 million immigrants lived in Russia in 2019, making up 8% of the total population. This places Russia second only to the U.S. in terms of immigrant numbers. Notably, the vast majority of immigrants in Russia are citizens of former Soviet Union states or their descendants. Among the largest "non-Slavic" ethnic groups in Moscow are Tatars, Bashkirs, Chuvash, Chechens, Armenians, Avars, Mordvins, Kazakhs, Azerbaijanis, Uzbeks, Kyrgyz, and Tajiks.

Discriminatory attitudes are relatively common in Russia. According to a poll by the independent Levada Center, 63% of Moscow respondents are permissive about discriminatory rental listings, and every second respondent supports the slogan "Rossiya dlya Russkikh," meaning "Russia should be for ethnic Russians." On paper, the Russian Constitution guarantees equal rights and freedoms for all, regardless of gender, race, nationality, property status, official position, or place of residence. Article 136 of the Criminal Code prescribes fines for violations, and the Labor Code prohibits workplace discrimination based on age, gender, ethnicity, religion, or beliefs (Avrutin, 2022). However, while discrimination is prohibited by law, enforcement in the labor and housing markets is weak, and penalties are almost never imposed. Neither landlords nor platforms that allow racial preferences in rental listings typically face any consequences. In practice, discrimination and racial hierarchy operate through informal, unofficial practices rather than formal legal institutions, as seen in some other countries and historical periods (Small & Pager, 2020).

Moscow Housing. This study is largely possible due to the unique structure of Moscow's housing stock, which is shaped by Soviet-era policies. During the Soviet period, most housing was state-owned, and apartments were allocated by the government. Since the 1970s, urban development has focused on constructing large, high-rise buildings, mostly ranging from 9 to 16 floors. Today, private development continues in this same pattern. The typical building in Moscow has about 200 apartments, and units within the same building are usually similar in quality. By 2016, approximately 90% of apartments in Moscow were privatized due to the mass privatization program implemented after the collapse of the Soviet Union: state-owned apartments were transferred to their occupants, often for a nominal fee (Gunko, Bogacheva, Medvedev, & Kashnitsky, 2018). According to the official statistic, most landlords have only one flat to rent[5].

When it comes to residential patterns, despite Moscow's ethnic diversity, there is little evidence of racial segregation similar to what is seen in American or European cities (Vendina, 2002; Vendina, Panin, & Tikunov, 2019). In fact, there is no specific area in Moscow where minorities are concentrated. Instead, they can be found throughout the city with relatively equal frequency. This lack of segregation is likely a legacy of the strict housing policies enforced during Soviet times. This unique feature highlights that patterns observed in the U.S. and Western Europe may not always apply to other societies with different historical and cultural backgrounds.

Online Listings and Discrimination. In this paper, we analyze data from CIAN, Russia's largest platform for long-term rental. Over the past decade, the real estate market has primarily shifted online, with CIAN offering the most reliable representation of rental availability in the city. While other platforms exist in Russia, their listings largely overlap with those on CIAN. During the observation period, overt discrimination in rental listings was widespread across all platforms. To further support our findings, we replicate part of our analysis using data from another platform[6]. Landlords often disagreed on who qualified as a "Slav" or even as a citizen of the Russian Federation. However, according to research, they typically excluded people with darker skin, Caucasian or Asian features, or those with ethnic names and accents (Avrutin, 2022).

3. Data and Descriptive Statistics

This paper is based on extensive data collection, primarily from the major Russian real estate platform, CIAN. The data was collected daily over a period of nearly six months – 168 days – from May 27, 2018, to November 11, 2018, covering the entire Moscow area. The scraping process was initiated each night and reviewed the following day[7]. As a result, our dataset has two unique features worth noting. First, unlike many platform-

[5]According to Rosstat 2018, 36.1% of Moscow households owned additional residential properties besides their primary residence, and among them, 86.2% owned only one extra property.

[6]Available upon request.

[7]Data snapshots are missing for a few rare days due to temporary inaccessibility of the platform.

based observational studies of the housing market, we have access to the exact building addresses for the vast majority of apartments.[8] Second, our method of high-frequency data collection over an extended period allows us to obtain a large, representative sample of the market with sufficient variation to support our identification strategy, which focuses on within-building comparisons. Moreover, we can track each apartment's time on the market, which we use as a control variable. We also focus on the final listed price before an apartment leaves the market – our best approximation of the transaction price.[9] Finally, although our baseline strategy relies on cross-sectional offer data, we also possess a panel of offers, thanks to the daily snapshots, allowing us to observe individual offer trajectories over time.

Every day, the platform has around 23,000 listings, with approximately 2,000 new rental listings being added and old ones removed as apartments are rented out. Our data contains variables on rental price, location, number of rooms, surface area, layout, floor number of the apartment, the total number of floors in the building, as well as the number of attached pictures and accompanying text. We further process this data by performing (1) geocoding of addresses and (2) text analysis of listings to detect overt discrimination and to construct text-based measures of apartment quality. We use the Yandex Maps API for geocoding and manually validate the results. For the main analysis, we limit our sample to observations with known exact building locations.

Figure I provides a quick look at typical cases of overt discrimination on the platform. While there is some variation, the racial disclaimers are largely uniform in style and phrasing, with derivatives of the word "Slavs" being the most common term. The reference to the "Slavic" group is commonly understood as a euphemism for Russians of European appearance. Other ethnic groups and races are also occasionally mentioned in rental listings. Due to the repetitive nature of these racial disclaimers, in our baseline measure of overt discrimination, we used a simple dictionary-based approach, which we validated using a more advanced method leveraging embedding models provided by OpenAI. In our dictionary approach, substrings such as *"slavyan"* (Slavic), *"kavkaz"* (a person from the Caucasus region), *"aziya"* (Asian), and *"vsem krome"* (everyone except) are used as rules for identifying patterns of discriminatory language.

[8] Buildings with validated exact locations cover 87.4% of all listings.

[9] Transaction data for Moscow is not publicly available.

We focus on a sample of apartments with known exact addresses and exclude buildings in the top 1 percentile for the number of apartments available during our observation period. We also exclude extremely cheap or expensive, as well as extremely small or large apartments, to remove potential errors and irregularities that are inevitably present on online platforms. In Panel A of Table A1, we report descriptive statistics at the apartment level. We calculate the rental prices in dollars at the time of data collection. The mean rental price[10] is \$893, while the median is \$597, reflecting the long tail typical of such markets. Overt discrimination is observed in around 22% of apartments. The mean apartment size is 57 square meters. The turnover in Moscow's rental market is rather quick, with a median time on the market of 9 days before an apartment is rented. Finally, the average apartment in the sample includes around 13 attached photos and a text description of approximately 764 characters in length.

It is important to characterize the features of the buildings, as our identification strategy relies on building groupings. Panel B of Table A1 presents building-level descriptive statistics. The average building in Moscow, and in our sample, is tall, with a mean of 11 floors and a median of 9 floors. During our observation period, a significant number of apartments were available per building, with a mean of 5 and a median of 4, ranging between 1 and 33. Figure II demonstrates a map of the buildings in our dataset. Each point on the map represents an individual building, with the color corresponding to the share of discriminatory apartments in the building – ranging from dark blue for the most discriminatory to light pink for the least. Our sample clearly provides uniform coverage across Moscow, with the exception of non-residential areas like parks and industrial zones. In fact, 25,556 buildings in our main sample serve as a good representation of Moscow's housing, covering around 70% of the total residential buildings in the city. Figure II also shows that discriminatory preferences are unevenly distributed across the city, with the central district being the least discriminatory and the periphery the most. Given that the central area is wealthier, more educated, and has better amenities, this suggests that a straightforward correlation between overt discrimination and price would not yield causal estimates, thus motivating our study design based on within-building variation. Finally, it is worth noting that discriminatory preferences remain remarkably stable over time, as

[10]Converted from Russian rubles to U.S. dollars using the exchange rate 1 U.S. dollar = 67 Russian rubles as of August 15, 2018

shown in Appendix Figure A1.

To further test our identification strategy, we use text-as-data methods, which have recently proven useful in economics (Ash & Hansen, 2023), to extract semantic variables from the listings' texts. These characteristics are then used as extra control variables. We focus on five key semantic dimensions: (i) bright apartments with good views, (ii) quietness and good isolation, (iii) the presence of a balcony or terrace, (iv) high-quality equipment and appliances, (v) design and style. First, we use the *text-embedding-3-small* model, provided by OpenAI, to vectorize the listing's text, representing it in semantic space. Second, we construct concise prompts that correspond to the dimensions outlined above. For instance, for high-quality equipment, we use the sentence: "The apartment is equipped with modern amenities, including air conditioning, a dishwasher, a washing machine, and other high-quality appliances." Finally, we calculate cosine distances between these prompts and the listings' texts. More details on the prompts can be found in Appendix Table A6.

4. Empirical Methodology

To trace the impact of overt discrimination on price, we estimate the equation of the following form:

$$\log(\text{Price}_i) = \beta_D \text{Overt Discrimination}_i + \gamma X_i' + \sigma_{b(i)} + \phi_{\tau(i)} + \epsilon_i \tag{1}$$

Each observation corresponds to a listing posted during the observation period. The subscript i represents an individual offer, $b(i)$ indexes the building, and $\tau(i)$ denotes the posting day. The outcome variable *log(Price)* is the logarithm of the rent on the last date before an apartment is rented. The variable *Overt Discrimination* is a dummy that captures the presence of discriminatory preferences in the listing's text. σ_b and ϕ_τ represent fixed effects for the building and the posting day, respectively.

The building fixed effects account for spatial and building-specific variations. The coefficient β_D is the main parameter of interest, estimating the cost of discrimination by reflecting differences in rent prices between listings with and without discriminatory content. The model also includes a set of control variables X_i, which represent apartment characteristics. These characteristics include directly observable features, such as the surface area and layout of the apartment, as well as features reflecting the apartment's quality, such as the length of the listing text and the number of attached photos. Less restrictive specifications are also tested: a model with subdistrict-level (*rayon*) fixed effects and a model with district-level (*okrug*) fixed effects. In both cases, we also introduce controls for the distance to the city center.

Our preferred specification includes building-level fixed effects, allowing us to compare apartments with and without overt discrimination within the same building. This approach, combined with various individual apartment-level controls used in our main specification and robustness checks, brings us as close as possible to a causal estimate of the effect of discrimination on price. Focusing on within-building variation helps eliminate confounding factors related to geographic aspects, such as the distribution of housing, jobs, amenities, and attitudes, differences in demand composition, as well as building-specific characteristics. Moreover, apartment class within a building tends to be homogeneous – a common feature of the Moscow housing stock. Our baseline specification focuses on the price just before the apartment is rented and leaves the market, offering the closest approximation to the transaction price.

As an additional measure of the cost of discrimination, we use the same specification with a different outcome variable, *log(Nb. Days Until Rental)*, which represents the logarithm of the number of days between the posting date and the date the apartment was rented. In this context, β_D captures the difference in tenant search duration between listings with discriminatory content and those without.

5. Main results

Racial Rent Differential. We begin by presenting our main results with the OLS estimates of Equation (1). The building-level fixed effects model is represented by the first coefficient at the top of Figure III. It indicates that overt discrimination significantly affects rental prices, leading to a 4.0% decrease for apartments with discrimination compared to those without in the same building. In other specifications, we progressively relax the model specifications by shifting spatial fixed effects from the building to the subdistrict level, then to the district level, followed by removing spatial fixed effects.

Building fixed effects model is our preferred one, which is likely to provide the causal estimate. Other models in Figure III demonstrate that a simple OLS regression of rental price on overt discrimination leads to biased estimates, inflating the absolute value. The bias progressively decreases with more rigorous specifications. This is an expected finding – Figure II shows that the central area of the city, which is more upscale, rich in amenities, and home to residents with higher levels of education, experiences less discrimination. As a result, the coefficients in regressions without geographic controls are naturally inflated.

Racial Search Duration Differential. Our findings on racial rent differentials are further reinforced when we examine the impact of overt discrimination on tenant search duration. The results in Figure IV show that discrimination extends the time required to find a tenant. Specifically, our preferred specification, which includes building-level fixed effects, estimates that overt discrimination leads to an 8.6% increase in the number of days until an apartment is rented. Given the average search duration of 20 days in the control group, this effect translates to approximately 2 additional days of search time. This delay imposes tangible financial consequences. For landlords, the immediate losses from this downtime can be quantified: assuming an average rent of 893 dollars, the 2-day delay results in an estimated loss of about 60 dollars [(2/30) * 893 = 60]. This provides compelling evidence that discrimination not only reduces rent but also inflates search times, exacerbating the overall losses. There is no statistically significant difference in coefficients between different FEs specifications because tenant search duration has less geographical variance. According to the basic rental market matching models, a tenant search is a stochastic process. The number of days until rental is less predictable by apartment characteristics compared to the rental prices: R^2=0.34 for the number of days vs R^2=0.93 for the rental prices, therefore, the estimations for the number of days are noisier.

Racial Rent Differential in Equilibrium. While we work with advertised rents, we focus on the last listed price before the apartment is removed from the market, which is likely as close as possible to the transaction price, given that rents tend to be stable over time. Regression results confirm that coefficients for the first-day price are nearly identical to those for the last-day price, indicating that overt discrimination in rents persists throughout the listing period and is observed in market equilibrium. The overt discrimination effect on prices is stable in all observation periods, whether focusing on the first, early, mid, late, or last listing price (Figure A2). This robustness underscores that our findings are not sensitive to the timing of price observations, and it is unlikely that it is different for the transaction price.

Controlling for Other Text-Based Semantic Variables. Although our preferred specification accounts for most potential confounders, it remains possible that discriminatory and non-discriminatory apartments within the same building differ in their textual descriptions, which could reflect variations in landlords' communication skills. These descriptions often convey information about features such as equipment, design, and view, as well as other apartment-level characteristics, which may be correlated with discriminatory language in the text.

We demonstrate that the 4% negative effect of overt discrimination on rental prices is a prime effect which is not mitigated by any other semantic features of descriptions. Our analysis shows that price differentials are not driven by confounding semantic elements. Using text analysis of apartment listings and utilizing a state-of-the-art embedding model, we confirm that overt discrimination has an independent substantial effect on rental prices which is more than two times bigger than the effect of any other text-based variables (Table A2). One by one, we use additional controls derived from text analysis of listings without any sizeable changes in our main estimation.

Extra Robustness Checks and Placebo. Our main results remain robust under various modifications. Table A4 explores different sample compositions based on building characteristics, confirming that the 4% negative effect of overt discrimination on rental prices is not sensitive to the number of observations in a building or the building's size. In Column 1, we include the top 1 percentile of buildings by the number of available apartments during the observation period (more than 33 observations per building), which were excluded in the baseline analysis. In Columns 2-3, we apply stricter limits on the number of available apartments per building. In Columns 4-5, we take the opposite approach and remove buildings with a small number of observations. Finally, in Columns 6-8, we repeat this process, excluding buildings based on their size, and sequentially removing both the tallest and shortest buildings in terms of floors. The 4% negative effect of overt discrimination on rental prices remains consistent over time. Table A5 demonstrates that, for any given full month during the observational study, the effect fluctuates only slightly. Finally, we perform an additional placebo test to show that, controlling for other factors, discrimination does not predict the floor number. Table A3 shows no significant effect of overt discrimination on an apartment's location within the building, using the same specifications as in our main results in Figure III. This indicates that overt discrimination is evenly distributed across floors.

6. Correspondence Experiment

Although we show that overt discrimination creates a rent price differential, there are still several unclear aspects inherent to our setting. Simply put, overt discrimination is uncommon today[11], which disconnects our findings from the more recent empirical scholarship on discrimination. To bridge this gap, we apply the well-established method of correspondence experiments, first proposed by Bertrand and Mullainathan (2004). This approach allows us to explore three key questions: (1) How does the level of discrimination in Moscow compare to findings from other countries? (2) Is our measure of overt discrimination reliable? (3) Do overt and subtle forms of discrimination act as complements or substitutes? This third question also helps rule out an alternative explanation for our main finding, which suggests that the rent differential may result from landlords switching between the two forms of discrimination.

In this experiment, we closely follow the Bertrand and Mullainathan (2004) protocol, adjusting for the specificities of the market and context. We conduct a paired-matching experiment, sending messages with both Russian-sounding and non-Russian-sounding names to the same listings, randomizing the order and content of the messages. More details on the experiment are provided in Appendix B. For the experiment results, we present the effects on relative response rates in line with Christensen et al. (2021). The experiment estimation equation is as follows:

$$\text{Response}_{ij} = \beta_{NR}\text{Non-Russian Name}_{ij} + \tau_i + \epsilon_i \tag{2}$$

[11] However, it was a predominant form of discrimination in the past. For an example from the pre-1964 Civil Rights Act era, see Pedriana and Abraham (2006)

The subscript i represents an individual landlord, j is a potential tenant type: Russian ($j = R$) or non-Russian-sounding name ($j = NR$). The outcome of this experiment is a binary: a landlord responded ($Response_{ij} = 1$) or a landlord did not respond ($Response_{ij} = 0$) to the message from a potential tenant j: Non-Russian Name$_{iNR} = 1$ and Non-Russian Name$_{iR} = 0$. We also control for individual landlord τ_i fixed effects to account for the time-invariant characteristics of a landlord. In our preferred approach, we classify messages as non-responses if they were read but not replied to.

$$RR_{NR} = \frac{P(\text{Response}|\text{Non-Russian Name} = 1)}{P(\text{Response}|\text{Non-Russian Name} = 0)} = \frac{\beta_{NR}}{\mu_R} \tag{3}$$

where β_{NR} is the effect of non-Russian-sounding names binary on response and μ_R is the average response for Russian-sounding names. Standard errors for relative response rates are obtained using the delta method.

Graph V presents the results of the experiment. The total experimental sample reveals the significant negative effect of non-Russian-sounding names on relative response rates. The estimated coefficient of -31.3% is comparable to the levels found in the most discriminatory areas of the United States, as reported in the recent large-scale experiment (Christensen et al., 2021). This is similar to the discrimination observed in Minneapolis and slightly lower than that in Chicago (-36%).

The same Figure V further breaks down the results by sub-samples – listings with overt discrimination and those without. For both sub-samples, the effects remain significant at the 1% level. However, it is evident that listings with overt discrimination show an effect that is two and a half times greater than those without. In a case of overt discrimination, the effect reaches -47.2%, which is 30% higher than in the most discriminatory regions of the U.S. (Christensen et al., 2021). These findings offer two key insights. Overt and subtle forms of discrimination appear to function as complements rather than substitutes, amplifying each other's impact on relative response rates.

Figure A3 further supports these findings by a positive correlation between the share of listings with overt discrimination in a neighborhood and subtle discrimination. This correlation is present across all listings (Panel A3a), as well as the subset without overt discrimination (Panel A3b), though for listings without overt discrimination responses to Russian-sounding names are not correlated with neighbors' overt discrimination. These results do not provide evidence for social stigma mechanism: areas where overt discrimination is less common do not have more subtle discrimination, which would be expected if landlords were switching from overt to subtle forms.

7. Conclusion

This paper examines the rental housing market in Moscow, where discrimination is evident, to directly link landlords' discriminatory preferences to rental prices. We demonstrate that landlords are willing to forgo around 4% of their monthly rent to maintain their discriminatory preferences, which in turn creates a racial rent differential. This differential, where minorities pay more for similar units compared to the majority, has been documented for decades, though its direct link to landlord prejudice remains debated in the literature.

Our study also brings attention to the interaction between overt and subtle discrimination – an issue that has largely been overlooked in the economic discourse. Previous research has been mostly focused on detecting the presence of discrimination, but this approach often cannot capture the more nuanced differences in discriminatory behavior. By focusing on explicit discriminatory acts, we shed light on how landlords' preferences and biases translate directly into economic outcomes.

We believe our study has important implications for the ongoing debate on racial prejudice and inequality. Russia is not the only society where diverse races and ethnicities coexist, where racial tensions emerge, or where inequality persists. Furthermore, such conflicts are increasingly central to political debates in many countries. More questions remain unanswered: If racial preferences shape markets and amplify inequality, what policies can balance racial biases in society? How do restrictions on overt discrimination influence inequality dynamics? What are the consequences when such restrictions are lifted – a trend that has recently gained momentum? What measures can strengthen mutual trust and social cohesion? To address these questions, researchers can leverage new data from the digital era, including social media platforms. Additionally, historical data from periods when overt discrimination was more common can provide valuable insights into the evolution of racial disparities.

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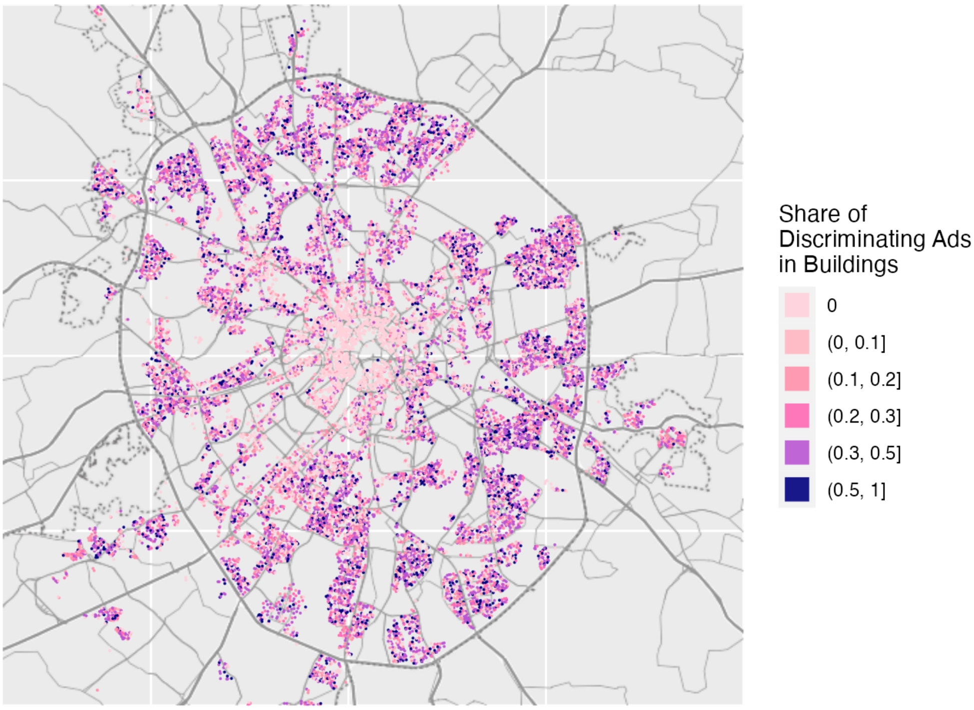
FIGURE I: Excerpts from Listings Showing Overt Discrimination

no **asia**
strictly **Slavs**
offer for the **Slavs**
we take the **Slavs**
consider **Slavs only**
strictly decent **Slavs**
Russian non-smokers
married couple of **Slavs**
ideal for a **Slavic couple**
urgently consider the **Slavs**
any composition of the **Slavs**
consider all, **preferably Slavs**
consider **Slavs Jews Kazakhs**
consider any decent besides **Asia**
consider the clean and responsible **Slavs**
rent an apartment for a long time to the **Slavs**
a one-room apartment for rent for a **Slavic family**
decent and responsible **citizens of Slavic appearance**
will consider **Slavs and foreigners except the Caucasus and Asia**
rent an apartment to a young family or not violent students and **only Russians**

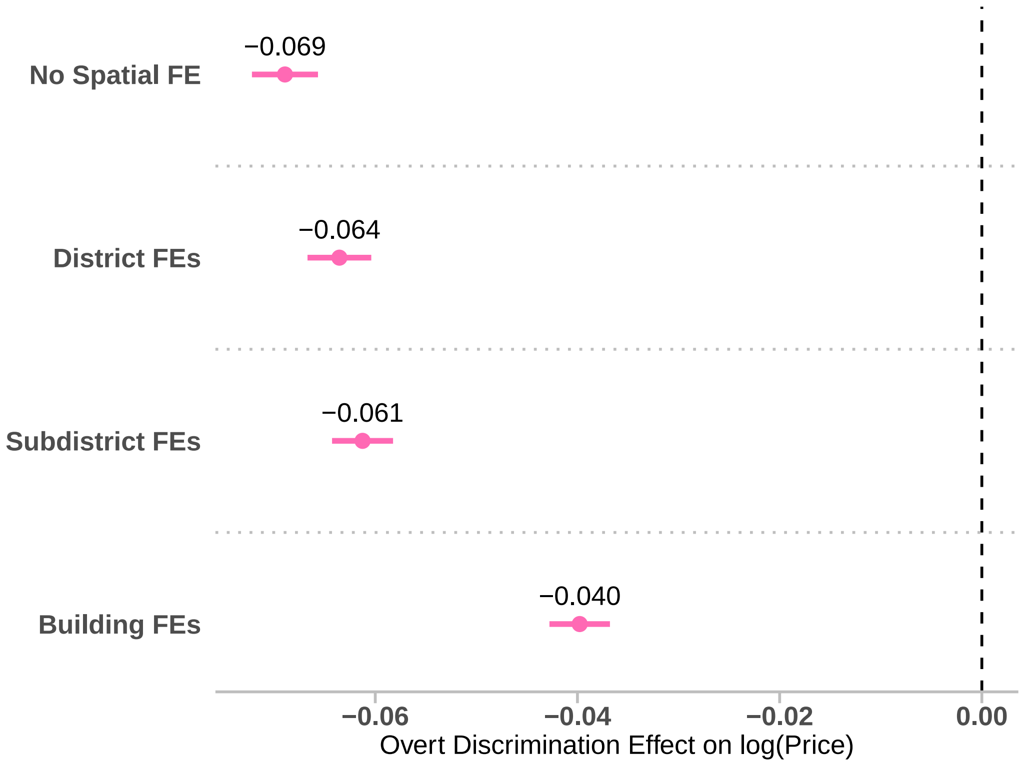
Notes: The figure presents an excerpt from typical discriminatory advertisements on CIAN. Racial preferences are commonly expressed with terms such as "Slavic," "Russian," or "Asian." More details about the construction of variables based on listing texts can be found in Section 3.

FIGURE II: The Map of Overt Discrimination



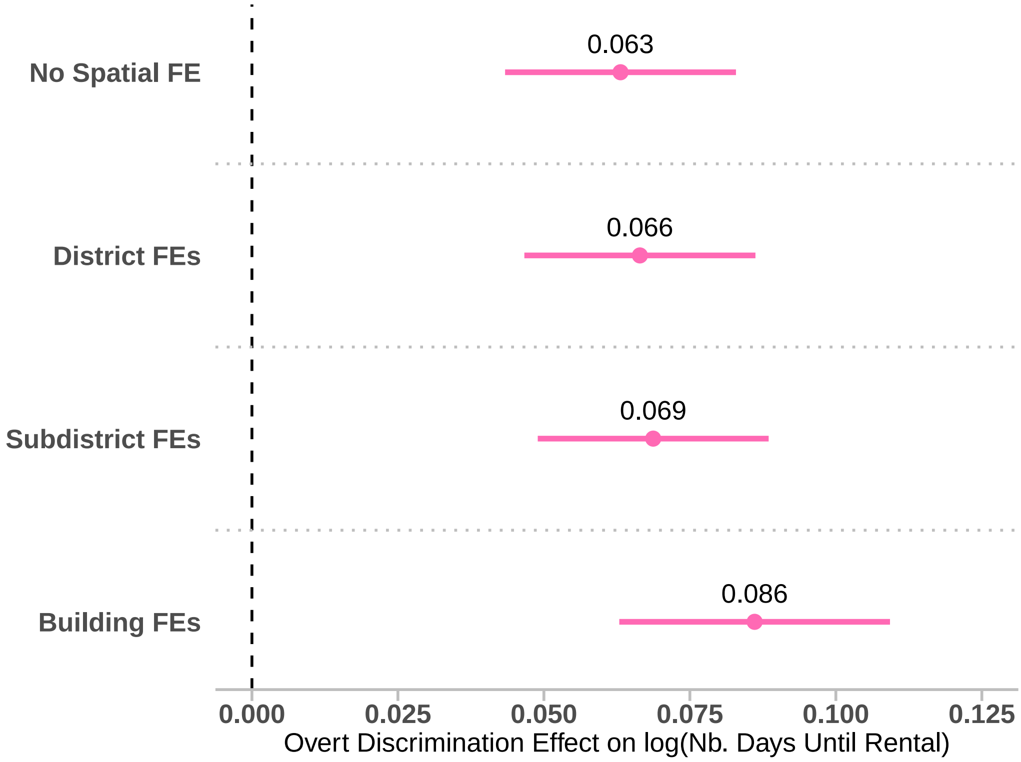
Notes: This figure presents a map of overt discrimination in Moscow. Each point on the map represents a building, with the color indicating the share of discriminatory apartments among the total number of apartments available during the six months of our observation period.

FIGURE III: Overt Discrimination Generates Racial Rent Differential



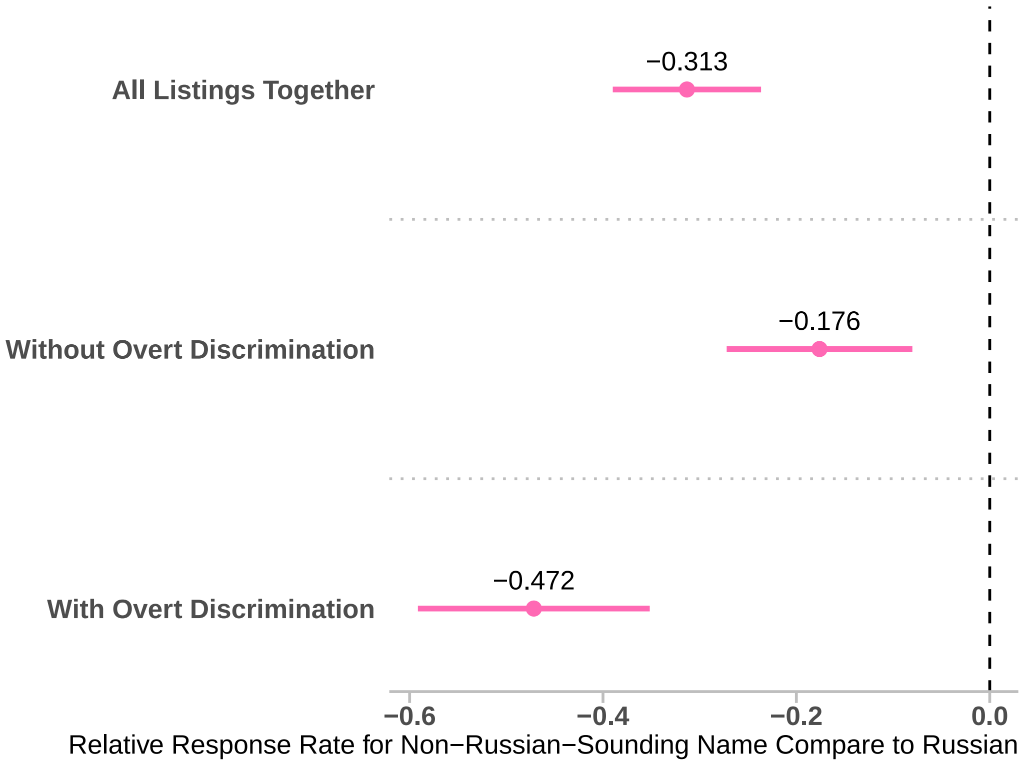
Notes: The figure shows the OLS estimation of the effect of overt discrimination on the logarithm of the rental price on the last date before an apartment is rented. The average rental price is 893 dollars. Each line from bottom to top presents progressively less stringent specifications. The bottom one with a -0.040 coefficient reports our preferred model, which includes building-level fixed effects. Each model includes apartment-level controls such as the area in square meters, layout measured as the ratio of living to total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. The average number of observations per building is 5. The average Subdistrict size is 19.4 square kilometres with an average of 971 observations per Subdistrict. The average District size is 213.5 square kilometres with an average of 11,244 observations per District. Confidence intervals are based on heteroskedasticity-robust standard errors at 1% significance level. 134,928 observations.

FIGURE IV: Overt Discrimination Generates Search Duration Differential



Notes: The figure shows the OLS estimation of the effect of overt discrimination on the logarithm of the number of days between the posting date and the date the apartment was rented. The average number of days until rental is 20. Each line from bottom to top presents progressively less stringent specifications. The bottom one with a 0.086 coefficient reports our preferred model, which includes building-level fixed effects. Each model includes apartment-level controls such as the area in square meters, layout measured as the ratio of living to total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. The average number of observations per building is 5. The average Subdistrict size is 19.4 square kilometres with an average of 971 observations per Subdistrict. The average District size is 213.5 square kilometres with an average of 11,244 observations per District. Confidence intervals are based on heteroskedasticity-robust standard errors at 1% significance level. 134,928 observations.

FIGURE V: Correspondence Experiment: Lower Response Rates to Non-Russian Names



Notes: This graph presents the results of a correspondence experiment. We employ a pair-matched design, sending two messages – one with a Russian-sounding name and one with a non-Russian-sounding name – to each listing. The graph shows the relative response rate of a non-Russian-sounding name compared to a Russian-sounding name. The effect intensifies for listings with overt discrimination. Each model includes control for individual landlord τ_i fixed effects to account for the time-invariant characteristics of a landlord. Confidence intervals are based on delta method standard errors at 5% significance level. 874 observations (437 treatment and 437 control) for all listings together. 444 observations (222 treatment and 222 control) without overt discrimination. 430 observations (215 treatment and 215 control) with overt discrimination.

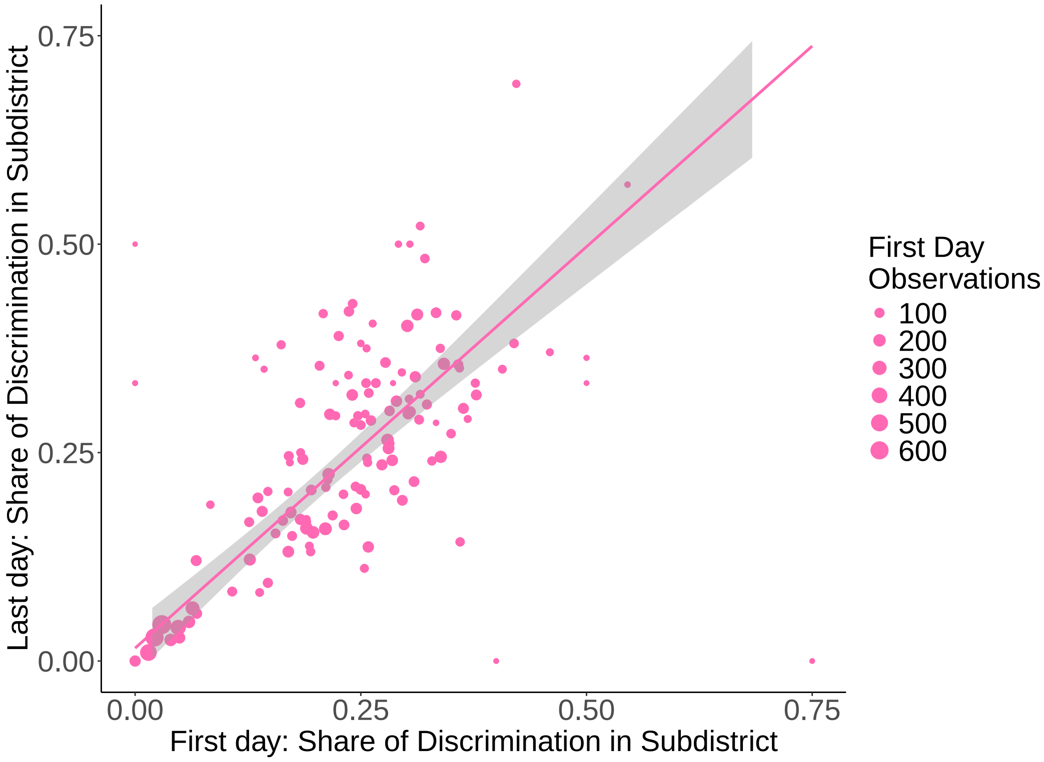
Appendix A: Additional Figures and Tables

TABLE A1
DESCRIPTIVE STATISTICS

	Mean	Median	Min	Q1	Q3	Max
Panel A: Apartment Level						
Rent (dollars)	893	597	216	478	821	15285
Nb. Days Until Rental	20	9	1	3	24	168
Overt discrimination	0.22	-	0	-	-	1
Area (sq.m)	57	46	10	39	61	500
Living/Total Area (perc.)	60	60	1	53	66	100
Number of Photos	13	12	1	8	17	51
Comment Length (symb.)	764	657	52	436	1003	3743
Ground floor	0.07	-	0	-	-	1
Last floor	0.09	-	0	-	-	1
						134,928 observations
Panel B: Building Level						
Apartment per Building	5	4	1	2	7	33
Number Floors	11	9	1	5	14	74
Distance to Center	12	12	0	8	15	60
						25,556 observations

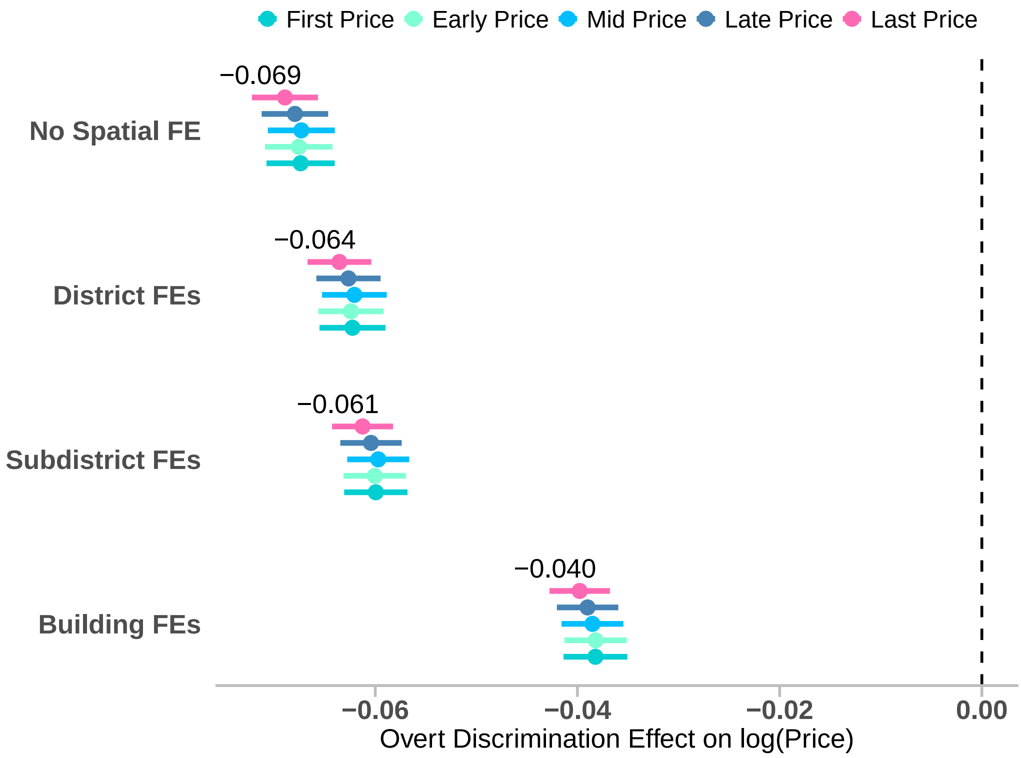
Notes: This table presents descriptive statistics of variables used in the main analysis, especially Figures III-IV. Panel A shows characteristics at the apartment level. Panel B at the building level. Detailed data descriptions are presented in Section 3.

FIGURE A1: Overt Discrimination is Persistent in Time



Notes: The graph shows the weighted OLS of "Last day: Share of Discrimination" on "First day: Share of Discrimination" with weighting by number of observations in the First day. It gives almost a 1 to 1 connection (0.96) between shares of discrimination by Subdistrict at the beginning and at the end of our observational period. The confidence interval is based on standard errors at a 1% significance level.

FIGURE A2: Overt Discrimination Effect Is the Same for the First and Last Listing Prices



Notes: The figure shows that the observation day choice does not change results. The effect is the same for earlier listing days. We show results for the First Price (the first day when the apartment was posted), Early Price (first-fourth of the listing period), Mid Price (one-half of the listing period), Late Price (third-fourth of the listing period) and Last Price (the day when the apartment was rented – benchmark from our main results) as dependent variables for every specification from Figure III: specification including building-level fixed effects and apartment-level controls such as the area in square meters, layout measured as the ratio of living to the total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. Confidence intervals are based on heteroskedasticity-robust standard errors at 1% significance level. 134,928 observations.

TABLE A2
OVERT DISCRIMINATION EFFECT CONTROLLING FOR OTHER TEXT-BASED VARIABLES

	(1)	(2)	(3)	(4)	(5)	(6)
	Dep Var: log(Price)					
Overt Discrimination	-0.039***	-0.039***	-0.039***	-0.038***	-0.037***	-0.036***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
City View/Sunlight	0.006***					0.0002
	(0.001)					(0.002)
Quiet		0.007***				0.0004
		(0.001)				(0.001)
Balcony/Terrace			0.008***			0.002
			(0.001)			(0.002)
Equipped				0.009***		0.0002
				(0.001)		(0.001)
Modern Design					0.020***	0.019***
					(0.001)	(0.001)
Building FEs	✓	✓	✓	✓	✓	✓
Day of Publication FEs	✓	✓	✓	✓	✓	✓
Apartment Features	✓	✓	✓	✓	✓	✓
Observations	134,928	134,928	134,928	134,928	134,928	134,928

Notes: The table shows results of adding extra semantic control variables to assess that overt discrimination effect on a price is not driven by other text-based characteristics. Each specification is based on our preferred model with Building FEs in Figure III: specification including building-level fixed effects and apartment-level controls such as the area in square meters, layout measured as the ratio of living to the total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. The only difference is an extra semantic control variable: (1) bright apartments with good views, (2) quietness and good isolation, (3) the presence of a balcony or terrace, (4) high-quality equipment and appliances, (5) design and style, (6) all together. Confidence intervals are based on heteroskedasticity-robust standard errors at 1% significance level. 134,928 observations.

TABLE A3
PLACEBO: NO EFFECT OF OVERT DISCRIMINATION ON LOCATION INSIDE BUILDING

	(1)	(2)	(3)	(4)
		Dep Var: Floor		
Overt Discrimination	-0.015	0.023	0.021	0.021
	(0.026)	(0.022)	(0.021)	(0.021)
Building FEs	✓			
Subdistrict FEs		✓		
District FEs			✓	
Day of Publication FEs	✓	✓	✓	✓
Apartment Features	✓	✓	✓	✓
Observations	134,928	134,928	134,928	134,928

Notes: The table shows no effect of overt discrimination on the location inside the building. Specifications are the same as in Figure III. Each model includes apartment-level controls such as the area in square meters, layout measured as the ratio of living to total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. Heteroskedasticity-robust standard-errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

TABLE A4

OVERT DISCRIMINATION EFFECT IS ROBUST TO BUILDING SIZE COMPOSITION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dep Var: log(Price)								
Overt Discrimination	-0.040***	-0.040***	-0.037***	-0.040***	-0.040***	-0.040***	-0.041***	-0.040***	-0.040***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Sample	Full	<Q90th Obs.	<Q75th Obs.	>Q25th Obs.	>Q10th Obs.	<Q90th Floor	<Q75th Floor	>Q25th Floor	>Q10th Floor
Building FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Day of Publication FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓
Apartment Features	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	152,093	88,807	52,553	138,649	146,677	114,338	88,103	129,322	129,322

Notes: The OLS estimation assesses the effect of overt discrimination on the logarithm of the rental price on the last date before an apartment is rented. Our main specification in Figure III is with dropping observations over Q99th for a number of observations by building: 134,928 observations after excluding buildings with 34 or more observations. The effect is not sensitive to the sample selection by the building size. Using the full sample (without dropping observations over Q99th for a number of observations by building) or dropping observations over Q90th and Q75th or under Q25th and Q10th do not change results: we check both, quantiles of number of observations by building and quantiles of number of floors in the building. Q99th Obs. = 34, Q90th Obs. = 12, Q75th Obs. = 7, Q25th Obs. = 2, Q10th Obs. = 1, Q90th Floor = 17, Q75th Floor = 14, Q25th Floor = 5, Q10th Floor = 5. The specification is the same as in model with Building FEs in Figure III: it includes building-level fixed effects and apartment-level controls such as the area in square meters, layout measured as the ratio of living to the total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. Heteroskedasticity-robust standard-errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

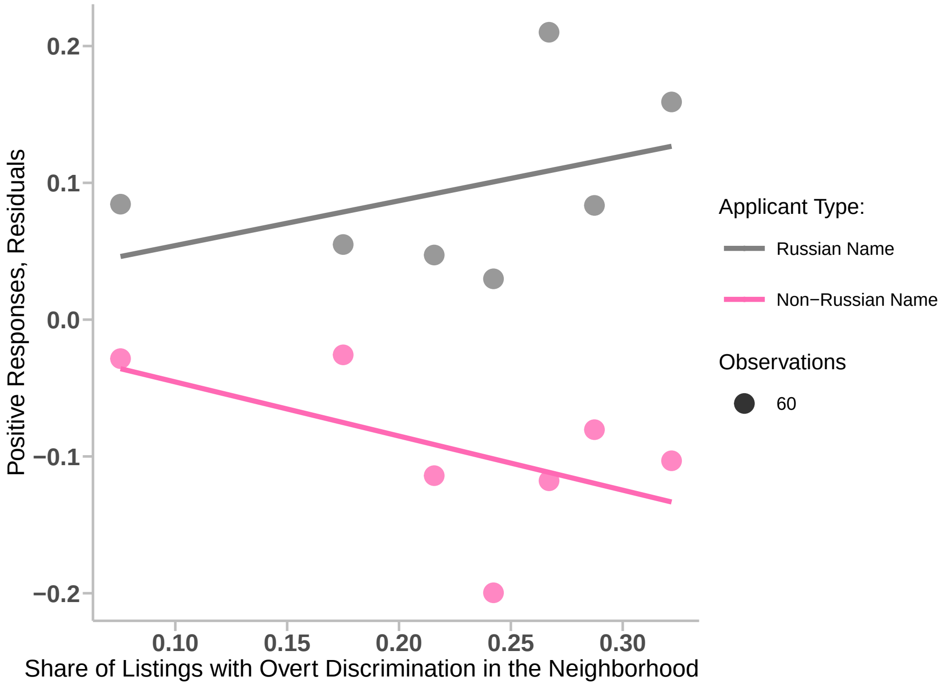
TABLE A5

OVERT DISCRIMINATION EFFECT IS CONSISTENT ACROSS MONTHS

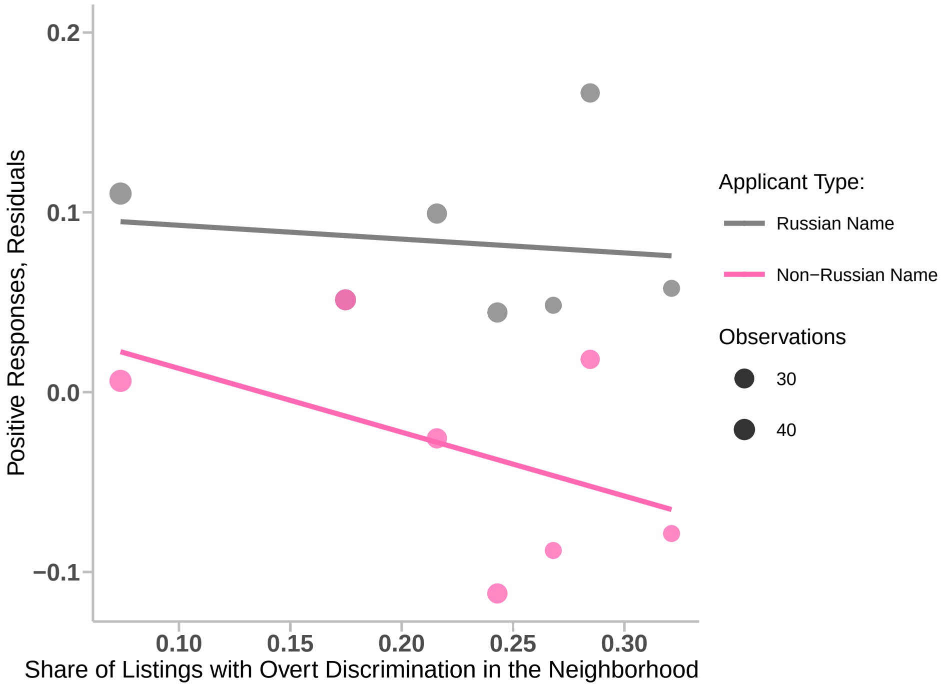
	(1)	(2)	(3)	(4)	(5)
	Dep Var: log(Price)				
Overt Discrimination	-0.042***	-0.040***	-0.041***	-0.035***	-0.037***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Month	June	July	August	September	October
Building FEs	✓	✓	✓	✓	✓
Day of Publication FEs	✓	✓	✓	✓	✓
Apartment Features	✓	✓	✓	✓	✓
Observations	40,999	45,100	40,144	35,948	29,996

Notes: The OLS estimation assesses the effect of overt discrimination on the logarithm of the rental price on the last date before an apartment is rented. The effect is not sensitive to the month-based sub-samples. The specification is the same as in model with Building FEs in Figure III: it includes building-level fixed effects and apartment-level controls such as the area in square meters, layout measured as the ratio of living to the total area, the logarithm of the number of photos, the logarithm of comment length, dummies for ground and top floors, as well as day-of-publication fixed effects. Heteroskedasticity-robust standard-errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

FIGURE A3: Correlation of Individual Subtle and Neighbors' Overt Discriminations



(A) All Listings



(B) Listing Without Overt Discrimination

Notes: This figure presents the results of a correspondence experiment. We employ a pair-matched design, sending two messages – one with a Russian-sounding name and one with a non-Russian-sounding name – to each listing. Positive responses are residualized from the size of the apartments. Panel A shows that for all listings (860 observations) the difference in positive callbacks to potential tenants with Russian-sounding names and non-Russian-sounding names increases in neighbors' overt discrimination. Panel B shows that for listings without overt discrimination (440 observations) the difference in positive callbacks to potential tenants with Russian-sounding names and non-Russian-sounding names still slightly increases in neighbors' overt discrimination, but for listings without overt discrimination the positive responses to Russian-sounding names are not correlated with neighbors' overt discrimination.

TABLE A6

PROMPTS FOR SEMANTIC POLES USED IN TEXT ANALYSIS

City View/Sunlight	The apartment features beautiful city views, bright and sunny with large windows.
Quiet	The apartment has enhanced sound insulation, ensuring quiet and peace even in the city center.
Balcony/Terrace	The apartment is equipped with a spacious balcony/terrace where you can enjoy fresh air and city views.
Equipped	The apartment is equipped with modern amenities, including an air conditioner, dishwasher, washing machine, and other high-quality appliances.
Modern Design	The apartment is designed in a modern style with quality finishing and furniture.

Appendix B: Design of Experiment

Since the seminal paper by Bertrand and Mullainathan (2004), economists have widely used correspondence experiments (Bertrand & Duflo, 2023) to uncover racial, ethnic, or gender discrimination in various markets. The main idea is to directly manipulate characteristics, such as names, within the field experiment setup. For instance, Bertrand and Mullainathan randomly assigned African-American-sounding or White-sounding names to job applicants' resumes, sent them to real employers in Boston and Chicago, and compared the response rate. The study found that applicants with African-American names have significantly lower relative response rates. This result has been replicated multiple times in various contexts, including the rental market (Christensen et al., 2021).

In our correspondence experiment, we utilized the contact form on an online rental platform, which enables users to message the individual who posted a listing. For each listing, we sent two separate messages from two different accounts: one using a Russian-sounding name and the other using a non-Russian-sounding name. The experiment was carried out in two rounds.

Messages

The platform provides users with two options for contacting landlords or agents: either by public mobile phone or through an online form. The online form is commonly used for submitting short, clarifying questions about a listing. To make sure that the only difference in applications is the name, the online form was chosen for this experiment.

We used two simple questions for the intervention. Translations of these questions are as follows:

> ***Q1.*** *Hello, I'm interested in your apartment. May I contact you later tonight? [First name]*
>
> ***Q2.*** *Good afternoon, I'm interested in your apartment. I would like to ask a clarifying question. When could one move into the apartment? [First name]*

The content of the questions is unrelated to ethnic discrimination. Their purpose is simply to elicit a response based on the name of the applicant. The message is a preliminary step before a phone conversation, which precedes a personal visit to the apartment. The online form is rarely used to finalize deals or negotiate terms. The experiment was designed to allow landlords to ignore the potential tenant, disrupting interaction at the very first stage.

Names and Identities

When applicants submit their messages through the form, landlords only see the content of the message. Nevertheless, separate accounts with realistic email addresses were created for each identity.

The variation in perceived ethnicity based on names is the key factor in the experiment. Two rounds of the experiment were conducted, with different approaches to name selection.

It is important to note that in Russia, there is no comprehensive dataset of birth names. For the first round, we selected two names: the Russian name Andrei and the Turkic name Arslan, both popular and recognizable in Russia.

In the second round, we adopted a more systematic approach to name selection. We compiled a dataset of popular names in Russia using statistics from the social network *vk.com*[12]. We constructed a ranking of name frequencies for each Russian city.

The most Russian-sounding names were selected using data from Moscow, while the most non-Russian-sounding names were chosen based on data from Makhachkala. According to the 2010 Census, Moscow's population is 90 per cent ethnically Russian, whereas in Makhachkala, only 6.3 per cent of the residents are Russian. The largest ethnic groups from Makhachkala (Avars, Kumyki, Dargins, Lezgins, Laks) face systematic discrimination in Moscow's housing and labor markets.[13]

The ten most popular names from Moscow were selected to represent Russian-sounding names, while the ten most common names from Makhachkala were chosen to represent non-Russian-sounding names. These selected names were used in the second round of the study. To avoid overlap, names that ranked highest at the national level, as well as those used in the first round of the experiment, were excluded.

[12] The most commonly used media in Russia comparable in popularity to Facebook in the U.S.

[13] Bessudnov and Shcherbak (2020) found that Chechen job seekers had one of the lowest response rates. Given the overlap between names of major ethnic groups in Dagestan and Chechen names, this result applies to many of the most popular names in Makhachkala.

Sending Messages

The experiment was conducted in two rounds: June 20-21, 2018, and December 13-14, 2019. The only difference between the rounds was the selection of Russian-sounding and non-Russian-sounding names. The experiment targeted fresh listings that were no older than one day. To prevent potential bias from contacting the same person through different listings, we excluded all listings with non-unique phone numbers.

We obtained 430 listings with overt discrimination and balanced them with 444 listings without overt discrimination for our heterogeneity analysis, resulting in a total sample of 874 listings in the experiment.

We manually sent the first message through the contact form to each listing, randomising Russian-sounding and non-Russian-sounding names. Automating the process was not feasible due to platform restrictions. The next day, we sent the second set of messages with alternative names (Russian-sounding name if there was a non-Russian-sounding name on the first day, and vice versa) from different accounts. A one-day gap was chosen to make control and treatment groups more comparable and minimize the chance that listings would become unavailable by the time of the second message.

Randomization of Russian-sounding and non-Russian-sounding name message order and text content ensured that timing would not bias results.

Classification of Responses

Landlords and agents could respond in various ways. The responses were classified into the following categories:

1. Answer the question or ask the applicant to call
2. Ask for extended identification or explicitly inquire about ethnicity
3. State that the apartment has already been rented
4. Do not read the message
5. Read the message but do not respond
6. Reject the applicant, citing ethnicity

7. Reject the applicant, other reasons

Since landlords or agents could not communicate with applicants in other ways, no additional response types were possible.

For the analysis of the experiment's results, this classification was simplified. Category 1 was considered "likely non-discriminating," while categories 2, 3, 5-7 were combined into a single "likely discriminating" group. Responses classified under Category 4 were excluded from the analysis.