Discrimination, Preferences, and Residential Sorting: Evidence from Delhi's Rental Housing Market

Hamza M. Syed

November 5, 2026

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Abstract

Where people live shapes their opportunities. Residential segregation restricts where people can live, and thus which opportunities they can access. It is an equilibrium outcome of what households want and what the market will let them have, but the two halves are usually measured on different people. This paper measures rental preferences and discriminatory barriers on the same set of people. To do this, it combines surveys on both sides of the market: 3,290 households - 1,015 tenants, 263 landlords, and 2,012 owner-occupiers, and 619 property brokers across Delhi, through three survey experiments that identify discrimination, preferences, and gatekeeper screening. I find that, *first*, changing an otherwise-identical tenant's name from Hindu (majority) to Muslim (minority) in the vignette experiment lowers acceptance by about 22 percentage points, and 97–98 per cent of that shift is outright refusal rather than a higher rent. *Second*, brokers openly acknowledge the Muslims' constrained access to rental markets across the city: a Muslim family would be accepted by landlords and resident associations in 24 of 100 local properties, against 86 for an upper-caste Hindu family. *Third*, when constraints are removed in the neighbourhood choice experiment, 51% of Muslim respondents choose a mixed or majority-Hindu neighbourhood, whereas only 23% upper-caste Hindu respondents opt to live outside their own group's areas. Combining the measured preferences and barriers in a sorting model, I find that brokers' own screening accounts for 38% of the access gap; holding brokers to the landlords' standard would widen the stock open to Muslims by 25%. However, segregation survives even on equalising access because of the majority's strong avoidance of Muslim areas rather than Muslims' preference for homophily. Reading observed sorting as revealed preference would miss the exclusion and overstate the role of minority preference.

1 Introduction

Neighbourhood environment shapes educational attainments, earnings, and inter-generational mobility (Chetty et al., 2016; Cutler and Glaeser, 1997), which makes the housing market one

[1]I thank the IGC and STICERD for funding the full and pilot surveys, respectively. I am grateful to my supervisors Tim Besley, Maitreesh Ghatak and Michael Callen for their feedback and guidance throughout the projects ups and downs, and to Sam Asher, Robin Burgess, Gharad Bryan, Clare Balboni, Pavithra Suryanarayan, Catherine Szermann, Gilat Levy, Michael Greenstone, and John van Reenen for comments. Sanjana Nahar, Ayush Dwivedi and Zoheen Shehzad provided excellent research assistance. The study received ethics clearance from the LSE Research Ethics Committee (Ref: 193409, 24 April 2023, amendment approved 24 May 2025).

of the principal mechanisms through which economic opportunity is allocated. Left only to economic factors, household will sort into areas they prefer and can afford, but discrimination and preferences play a major role in allocating housing.

The literature on housing choices follows two broad approaches for measuring the preferences and barriers that drive residential sorting and segregation. First, audit and correspondence studies establish that minority applicants are refused more often (Bertrand and Mullainathan, 2004; Turner et al., 2002; Ahmed and Hammarstedt, 2008; Flage, 2018). But this literature takes the preferences of the house-seekers as given. Second, residential sorting models recover what households want by inverting what they choose (Schelling, 1971; Bayer et al., 2007; Card et al., 2008). This approach assumes that agents can freely sort across space. However, residential sorting is an equilibrium outcome reflecting both preferences and frictions. To distinguish between the two, one must measure both demand and supply in a given market, and infer frictions from the mismatch between these forces.

In this paper, I document the drivers of housing segregation by measuring the preferences of both demand and supply sides, and the brokers that mediate between the two, through three survey experiments in 180 neighbourhoods across Delhi, India. I bring these data together in a spatial sorting model to decompose the relative contribution of preferences and frictions to housing segregation, and the welfare cost to minorities. The survey covers 3,290 households and 619 rental brokers, stratified by religious composition and geocoded, so that what a broker says about a neighbourhood can be set beside what the households living there say about themselves.

To measure discrimination, I use a vignette experiment, in which every respondent evaluated hypothetical rental applicants whose names were randomly Hindu or Muslim, with other details held fixed. Because the response options distinguish accepting at the advertised rent from accepting only at a higher one, it also measures which margin that discrimination operates on. For preferences, I use a neighbourhood choice experiment, in which households chose among hypothetical neighbourhoods in which every option was stipulated to be available and affordable. A broker, in this setting, holds the listings for a compact patch of the city, and they can be asked how much of the local stock each kind of applicant can reach to measure overall access. By surveying tenants, landlords, owner-occupiers, and brokers, this paper covers almost all the parties involved from both demand and supply sides in this market.

Delhi is an ideal setting for studying this. It is a segregated city. Across the 180 neighbourhoods surveyed for this paper, the dissimilarity index [1] between Muslim and non-Muslim households is 0.70, against 0.23 under a random allocation of the same households to the same

[1]The index ranges from 0 to 1 and is the share of one group that would have to move for the two groups to be identically distributed.

neighbourhoods [2]. The setting makes the identity unusually clean. Hindus and Muslims in Delhi are co-national, co-ethnic and co-linguistic, and have lived in the same city for generations; a name signals religion and little else.

The paper finds three main results. *First*, in the vignette experiment, changing the applicant's name from Hindu to Muslim reduces the probability of being accepted at the stated rent by around 22 percentage points, roughly a quarter of baseline acceptance. Almost all of it operates through refusal: the share of the reduction going to refusal rather than price is 0.980 and 0.973. A price penalty and a refusal are different economic objects, and the difference is non-trivial. If exclusion operated on price, a transfer would undo it: subsidise the household and it can buy its way in. A refusal cannot be undone that way at any plausible transfer. The margin also bears on whether competition erodes discrimination. Under Becker (1957), a landlord who indulges a taste for discrimination forgoes rent and should be undercut; where rents are posted, capped between contracts, and there is a queue of applicants, he forgoes nothing observable, and the rationing survives. That is the mechanism of Akerlof and Yellen (1990) and the wage-rigidity literature applied to a housing market.

Second, brokers report that 23.8 of 100 properties in their service area would accept a Muslim family, against 85.8 for an upper-caste Hindu family and 82.1 for a lower-caste Hindu family, and that resident welfare associations (RWAs) bar Muslim tenants in 26.9 per cent of areas against under 1 per cent for either Hindu group. Looking at it spatially: there is no neighbourhood in the sample where fewer than half the properties would accept an upper-caste or a lower-caste Hindu family, and there are only 20 where half would accept a Muslim family. The exclusion is clustered rather than scattered.

Third, 51.4 per cent of Muslim respondents choose a mixed or majority-Hindu neighbourhood when told every option is available in the choice experiment. Compared with the majority: 23.5 per cent of upper-caste Hindu respondents, who are the dominant group, would live outside their own group's areas. Muslim households are more than twice as willing to live in mixed neighbourhoods as the majority is, and they can reach about a quarter of the market.

I combine these measures in a residential sorting model to ask how much of the city's segregation discrimination explains, what would change without it, and what it costs Muslim households.

Each renting household chooses one neighbourhood. A neighbourhood's value to a household combines three things: its composition, valued as measured in the choice experiment; its rent;

[2]Documenting Muslim and Scheduled Caste segregation across 1.5 million Indian neighbourhoods, Asher et al. (2026) state that they cannot separate discrimination from preference, and name that separation as the obvious next step.

and everything else, such as location and amenities. Because households also differ in ways the researcher cannot see, each neighbourhood is chosen with a probability that rises with its value rather than with certainty. Discrimination enters as access: a neighbourhood where a Muslim family can rent a quarter of the stock carries a quarter of the weight in that family's choice. Summing these probabilities over households gives each neighbourhood's composition. Since households care about composition, the model is solved as an equilibrium in which choices and composition are consistent and, in the preferred version, rents adjust to fill a fixed housing stock. A counterfactual changes one input, such as giving Muslim households upper-caste Hindu access, and solves again. Welfare is the expected value of the best available option, which falls as options are removed.

The model follows Bayer et al. (2007) with one difference. Rather than inverting observed residence, which assumes everyone could have lived anywhere, it takes preferences and access as separately measured objects (Manski, 1977), and calibrates each neighbourhood's residual attractiveness to its total number of renters only (Berry, 1994), so who rents where is an out-of-sample prediction.

Because effective access is the product of what landlords and associations accept and what brokers pass through, the model can also attribute the gap: 62 per cent to landlords and associations and 38 per cent to brokers, of which 13 points is screening beyond the landlords' own standard. That decomposition supports a policy exercise the reduced form cannot. Rental brokers in India are unregulated while sales brokers are not, so extending registration with a non-discrimination condition is a feasible proposal. The model prices it: full compliance doubles the number of neighbourhoods in which half the local stock is open to a Muslim family, from 5 to 10. It also says where to enforce. Because the gain in a neighbourhood depends on how harshly its brokers screen relative to what its landlords would accept, and not on the size of the gap there, enforcement directed at the ten neighbourhoods with the most to gain captures about half the benefit of enforcing everywhere.

Exclusion imposes a large welfare cost on Muslim households, about 1.1 log points of expected utility (roughly ₹9,400 a month), almost all from a smaller choice set. Equalising access in every neighbourhood, however, reduces modelled dissimilarity by at most about a tenth, and the bootstrap interval includes zero. The force separating the groups is majority avoidance of Muslim areas rather than Muslim attachment to them, and equalising access leaves it untouched. Severe exclusion and modest segregation consequences are consistent. Reading the observed sorting as revealed preference would miss the first and overstate the role of minority preference in the second.

The paper contributes to five related literatures.

Discrimination in housing. Audit and correspondence studies establish substantial discrimination in housing markets, mainly in the US and Europe: minority applicants receive fewer responses and fewer viewings (Yinger, 1998; Turner et al., 2002; Ahmed and Hammarstedt, 2008; Flage, 2018; Datta and Pathania, 2016), agents withhold units or steer households toward particular neighbourhoods (Ondrich et al., 2003; Christensen and Timmins, 2022), and information about applicant quality attenuates the penalty in some settings, consistent with a role for statistical discrimination (Ewens et al., 2014; Auspurg et al., 2017). Those designs observe a real decision but only its first step, and only for properties the researcher can already see. Pairing a vignette with intermediary elicitation trades the realism of a live application for the margin of discrimination, a measure usable where credit files and tenancy records barely exist, and the properties never disclosed to an applicant.

Residential sorting. A large literature models residential location as an equilibrium outcome and recovers preferences from observed choices (Epple and Sieg, 1999; Bayer et al., 2007). Recent work shows that search frictions, information and discrimination shape where households end up: reducing search barriers changes neighbourhood choice (Bergman et al., 2020, 2024), and discriminatory constraints distort both sorting and the preferences inferred from it (Christensen and Timmins, 2023). This paper observes preferences and feasible choice sets separately in the same market. Both enter a sorting model that treats availability as a measured object rather than an assumption, in the manner of Manski (1977), and calibrates each neighbourhood's residual attractiveness to its *total* number of renters following Berry (1994), not to its composition. Because availability is measured rather than assumed, the model can be tested out of sample (correlation 0.79) and can attribute the gap to a particular gatekeeper.

Spatially embedded institutions of exclusion. Discriminatory institutions can attach to places and persist. Redlining restricted credit across neighbourhoods with lasting effects on racial composition and economic outcomes (Aaronson et al., 2021; Faber, 2020); zoning altered the composition of urban neighbourhoods (Shertzer et al., 2016); and restrictive covenants limited which households could buy or rent particular properties (Sood and Ehrman-Solberg, 2026). This paper documents a contemporary counterpart operating through resident associations and rental intermediaries.

How discrimination survives competition. In Becker's (1957) benchmark, competition erodes taste-based discrimination because discriminators forgo profitable transactions. Search models show the force weakens in frictional markets (Black, 1995; Lang and Lehmann, 2012), and Lang et al. (2005) show that where wages are posted before applicants arrive and employers select

among several, discriminatory outcomes persist because firms allocate jobs rather than adjust wages applicant by applicant. This paper supplies a housing-market analogue and measures the margin directly.

Religion as an axis of exclusion. Experiments document substantial discrimination against Muslims in labour and housing markets, but in European settings religious identity is correlated with national origin, migration history, ethnicity and language (Adida et al., 2010; Pierné, 2013; Valfort, 2020; Di Stasio et al., 2021). This paper studies a setting where Hindu and Muslim applicants are co-national, co-linguistic and long-settled in the same city. The Scheduled Caste comparison adds a benchmark sharing minority and socioeconomic disadvantage without differing in religion, which distinguishes religion-specific exclusion from discrimination against disadvantaged minorities generally. On Indian evidence, the paper sits alongside work documenting segregation (Susewind, 2017; Bharathi et al., 2021; Asher et al., 2026) and discrimination in housing and labour (Datta and Pathania, 2016; Thorat and Attewell, 2007), and adds the two halves measured together.

The rest of the paper is structured as follows: Section 2 describes the setting. Section 3 sets out the surveys and the three experiments. Section 4 presents the reduced-form results, Section 5 asks what kind of discrimination they describe, and Section 6 summarises the robustness analysis. Section 7 builds the sorting model and runs the counterfactuals, Section 8 draws out the policy implications, and Section 9 concludes.

2 The setting

Delhi's rental market is organised around brokers. A household looking for a flat approaches one, is screened, and is shown a subset of what the broker holds. Brokers serve a compact area, typically one and a half to two kilometres around their shop, and manage transactions for landlords with whom they deal repeatedly; most listings on housing platforms are uploaded by brokers rather than owners. A broker is paid half a month's rent by each side on a completed tenancy, so he earns nothing unless a match happens and bears none of the cost if a tenant later defaults.

Three features of the market bear on what follows.

Screening happens before search. The broker's pre-screen is the first gate and operates on income, family structure, dietary practice, marital status and identity. A property a broker does not mention is not refused; it is never seen. An audit or correspondence study, which works from listings the researcher can observe, cannot reach that margin by construction. This is the

main reason the paper surveys brokers rather than only testing landlords.

Prices do not clear the market. Rents are posted rather than negotiated and the Delhi Rent Control Act caps increases between contracts. Brokers report a median of 20 potential tenants and 5 completed matches in an average month, a median of about four applicants per let, so a landlord chooses among applicants rather than screening one and turning an applicant away does not leave the flat empty. Rent is also a property-level price: quoting two applicants different rents for the same flat, face to face, in a market of repeated local dealings, is awkward in a way refusing is not. Rejected applicants are not pushed out of the market, because informal neighbourhoods supply cheaper and worse accommodation as a fallback. The conditions under which a discriminating owner bears a cost, and is therefore undercut, are weak.

An additional gatekeeper. Resident Welfare Associations (RWAs) are elected bodies that govern a neighbourhood's common affairs, and in many areas they also regulate who may rent or buy. Their rules are rarely written down, but brokers and residents know them. There is no housing-market analogue of the non-discrimination provisions that apply to employment, property law gives owners wide latitude, and the Supreme Court has upheld a cooperative society's right to restrict membership by community (zor, 2005). Appendix A gives a fuller account of the setting and the housing market.

3 Data and measurement

3.1 Who the respondents are

Two surveys were fielded in person between mid-February to early-April 2026. The resident survey covers 3,290 households and the broker survey 619 rental brokers, of whom 535 handle rental transactions and were therefore asked the access module. That 535 is the denominator for every access figure in this paper.

All analysis of the vignette experiment pools residents and brokers, every respondent, resident and broker alike, evaluated the applicants as a hypothetical landlord. They were told to decide as though the property were theirs and to set aside what neighbours or the association might think. The pooled estimate is therefore not a measure of landlord behaviour alone; it is a measure of what people in this market say they would do in the owner's position. Section 4.3 reports the estimate separately by respondent type.

Table 1: Respondents and modules

	Respondents	Vignette experiment		Choice	Forced
		observations	respondents	tasks	choices
Tenant only	1,015	8,120	1,015	4,039	5,055
Landlord only	256	2,048	256	1,014	1,279
Tenant and landlord	7	56	7	28	35
Broker	619	4,920	615	2,416	3,056
Resident, neither module	2,012	15,800	1,975	7,771	9,815
Total	3,909	30,944	3,868	15,268	19,240

Notes. "Tenant" and "landlord" denote respondents who completed the corresponding module; the housing-category question allows both, and seven respondents did. Forty-one respondents have no applicant-experiment data. Of the 619 brokers, 535 handle rental transactions and were asked the access module. Resident respondents are 81.4 per cent Hindu and 15.1 per cent Muslim, close to the city's composition.

3.2 Sampling and geography

neighbourhoods were stratified by religious composition, constructed from counts of temples and mosques scraped for every area of the city, into four strata: mosques only, mixed, temples only, and neither. Within sampled neighbourhoods a random starting point was chosen and every fifth dwelling approached in a fixed direction. Brokers were sampled from scraped commercial listings within the same strata. Both surveys record coordinates. Of 418 areas with any respondent, 46 contain both a resident and a broker; 98.1 per cent of surveyed tenants have a surveyed broker within two kilometres, at a median distance of 0.42 km, which is the linkage the spatial analysis uses. Appendix B gives the sampling frame, response rates and variable construction.

3.3 Three experiments

The vignette experiment. Each respondent saw eight applicant profiles, each a photograph with a name, occupation, marital status and a quoted rent. They were asked to evaluate the profiles as if they were landlords. The quoted rent on the application was described as the rent that they had posted as landlords. Respondents were randomly assigned to one of three arms by a uniform draw made on the device: arm A at or below 0.33 (1,255 respondents), arm B between 0.33 and 0.66 (1,218), arm C above 0.66 (1,395). In arm A the applicant carries the baseline name; in arm B the same profile carries a different name; in arm C the different-named profile carries one additional line of information. The response options are to accept at the quoted rent, to accept only at a higher rent, in which case the respondent states it, or to refuse. Two of the eight profiles change identity from Hindu to Muslim, two changes from Muslim to Hindu, one changes from Hindu Upper Caste to Hindu Scheduled Caste; the remaining three are identical

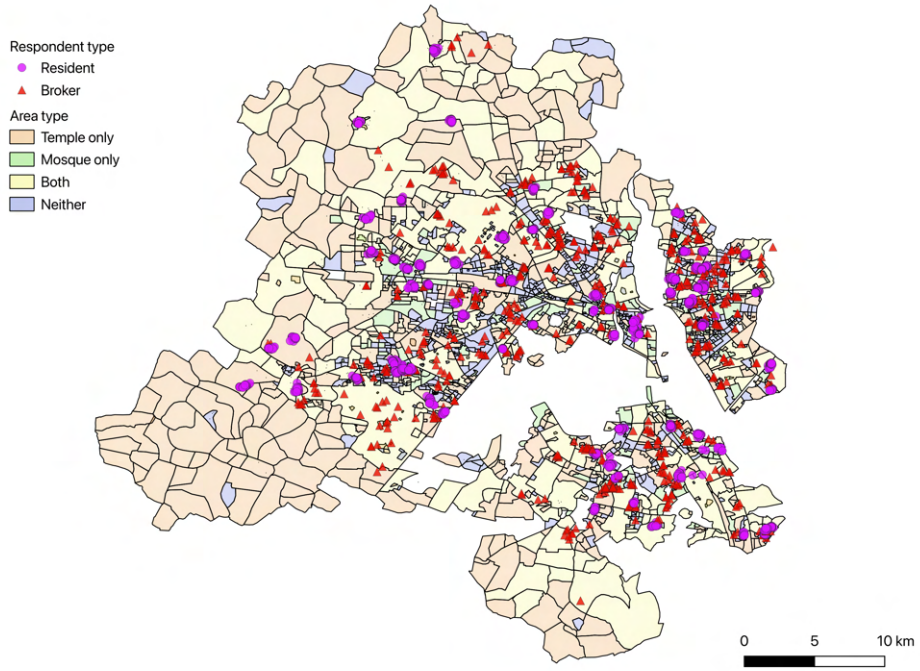


Figure 1: Survey areas. Points show surveyed resident households and rental brokers. Colour gradients show the area type between Temple only, Mosque only, Neither, and Both. Of the 180 neighbourhoods containing a surveyed resident, 46 also contain a surveyed broker; 39.4 percent of resident respondents live in a neighbourhood with broker coverage.

across arms and serve as placebos. Appendix D reproduces all of them.

Broker elicitation of access. Brokers were asked, as experts on their own area, how many of 100 properties there would accept a tenant of each of a list of types, and separately whether the local association has rules about who may rent, and for whom. They were also asked about their expert opinion over what factors matter to the landlords in their area when deciding which tenant to accept.

The neighbourhood choice experiment. Respondents chose among hypothetical neighbourhoods described by rent, commuting time and composition, and were told all options were available and affordable. Two tasks vary composition directly; two describe bundles of local facilities, including religious institutions, with a different stated household income in each task.

A hypothetical instrument has a known cost, and Section 6 tries to address the demand-effect concern. However, the vignette design is better for four things over an audit. It records the full response ladder, so the margin of discrimination is observed rather than inferred from callbacks. It holds the applicant's photograph, occupation, income and family composition exactly fixed while varying the name. It reaches the pre-listing margin by asking the intermediary about

properties an applicant would never learn exist. And it measures preferences and access on the same people, which is the main point of the exercise.

4 Results

4.1 Identity lowers the chance of acceptance

The obvious thing to measure is whether the applicant was accepted. But the four responses carry more information than that: they tell us *how* a respondent turns someone down. Collapsing "wait" into "refuse" as only 1.3 percent of responses are "wait", leaves three outcomes in a natural order of reluctance:

$$y_i \;=\; \begin{cases} 1 & \text{accept at the advertised rent,} \\ 2 & \text{accept only at a higher rent} \quad (\textit{the price margin}), \\ 3 & \text{refuse} \quad (\textit{the quantity margin}). \end{cases} \tag{1}$$

I model these with an ordered probit. The respondent has an unobserved degree of reluctance,

$$y_i^* \;=\; \beta_1 \underbrace{\mathbf{1}\{\text{Muslim name}\}}_{\text{arm B or C}} + \beta_2 \underbrace{\mathbf{1}\{\text{extra information}\}}_{\text{arm C}} + \mathbf{W}_i'\boldsymbol{\gamma} \;+\; \epsilon_i, \qquad \epsilon_i \sim N(0,1), \tag{2}$$

and chooses outcome 1, 2 or 3 according to whether y_i^* falls below the first cut point, between the two, or above the second. The specification includes no covariates. Arm assignment is random, so the comparison across arms is unbiased without adjustment, and Table B.4 reports balance on respondent characteristics. Section 6 reports an adjusted specification; it moves nothing.

$\beta_1 > 0$ means the Muslim name moves respondents toward refusal. β_1 is the identity effect, measured in units of the latent index, and β_2 is the further shift produced by the information in arm C. The model is estimated separately for each profile, one observation per respondent, so everything about the profile itself is absorbed by the cutpoints. No covariates enter, because the arm is randomly assigned.

β_1 tells us the direction and significance of the effect but not what it does to behaviour. For that I compute, for each of the three outcomes, how much the probability of that outcome changes when the name changes:

$$\Delta_k \;=\; \frac{\partial \Pr(y_i = k)}{\partial\, \mathbf{1}\{\text{Muslim name}\}}, \qquad k = 1, 2, 3. \tag{3}$$

These are the familiar marginal effects implied by β_1 together with the two cut points, evaluated

at the sample means of the controls. They must sum to zero: whatever probability decreases from "accept at the advertised rent" ($\Delta_1 < 0$) has to arrive either at the price margin (Δ_2) or at refusal (Δ_3). So β_1 answers "is there an effect, and how big in index units," and the Δ_k answer "where does the effect go."

The second question is about what form the discrimination takes, so I summarise the split in a single number, the share of the displaced probability that ends in refusal rather than a higher rent:

$$\Lambda \;=\; \frac{\Delta_3}{\Delta_2 + \Delta_3} \;=\; \frac{\Delta_3}{-\Delta_1} \;\in\; [0, 1]. \tag{4}$$

Λ near zero would describe a market that charges disfavoured applicants more but houses them. Λ near one describes a market that turns them away. It is unit-free, which is the point: a refusal measured in percentage points and a markup measured in rupees cannot otherwise be compared. Standard errors for Λ come from a bootstrap clustered on the respondent, 1,000 replications.

Table 2 estimates an ordered probit on the three-step response ladder (equation (2)), where higher values denote greater reluctance, for the two profiles whose identity changes from Hindu to Muslim across arms. The identity change raises reluctance by 0.708 and 0.654 index units. Panel B converts this to probabilities: it reduces the probability of being accepted at the quoted rent by 21.1 and 23.6 percentage points, against baseline acceptance of 88.8 and 78.7 per cent, which is roughly a quarter of it.

4.2 It operates on refusal, not on price

Panel B also decomposes where the reduction in acceptance probability goes. Of the movement out of acceptance at the quoted rent, 0.980 and 0.973 lands on refusal and the remainder on accepting at a higher rent; the quantity on the price margin is 0.4 and 0.6 percentage points.

Implication 1. *If a landlord chooses among several applicants rather than screening one, and refusing costs him little, then identity will be handled by refusing rather than by pricing, and information that raises an applicant's standing without changing his rank will not help him.*

Appendix E sets out the queuing model behind Implication 1. Two features of the data are consistent with it. The rationing share is stable across respondent types whose penalties differ by a factor of four (Table 4), which is what a mechanism operating on the same margin for everyone would look like. And the reliability cue in arm C does not move the ladder for the profile where it is tested.

Table 2: The reluctance ladder

	H to M woman (doctor, ₹22,000)	H to M man (researcher, ₹10,000)
Panel A. Ordered probit on the three-step ladder		
Identity effect β_1	0.708***	0.654***
	(0.060)	(0.053)
Information effect β_2	−0.005	0.063
	(0.051)	(0.049)
Panel B. Where the displaced probability goes		
Δ_1: out of acceptance at the quoted rent	−0.211***	−0.236***
	(0.017)	(0.018)
Δ_2: onto the price margin	0.004***	0.006***
	(0.001)	(0.001)
Δ_3: onto refusal	0.207***	0.230***
	(0.017)	(0.018)
$\Lambda = \Delta_3/(-\Delta_1)$	0.980	0.973
	(0.003)	(0.003)
Baseline acceptance, arm A	0.888	0.787
Observations	3,868	3,868

Notes. Ordered probit on three outcomes - accept at the quoted rent, accept only at a higher rent, refuse, where refuse pools the reject and wait responses. Higher values denote greater reluctance, so a positive coefficient is a penalty. Panel B gives marginal effects of the identity change on each response probability, evaluated at sample means; the three sum to zero by construction. Λ is the share of the shift out of acceptance that lands on refusal rather than on a markup. Panel A standard errors clustered on the respondent; Panel B bootstrap standard errors, 1,000 replications clustered on the respondent. The specification includes no covariates: arm is randomly assigned, and Appendix B.8 shows the estimate is unchanged by adjustment.

What the price margin looks like. Table 3 reports the two-part model (a probit for any markup and a Tobit for its size), conditional on the respondent not refusing. For the first profile the identity change raises the probability of quoting a markup by 1.2 percentage points against a base rate of 0.5 per cent, and the Tobit coefficient is ₹8,081. Thirty-five respondents quote a markup on that profile and 115 on the second. For the second profile the identity change does not move price, but the arm-C income signal does: it raises the quoted markup by ₹4,395 (s.e. 1,796) and the markup as a share of rent by 44 per cent.

Table 3: The price margin, conditional on not refusing

	H to M woman			H to M man		
	P(markup)	₹	% rent	P(markup)	₹	% rent
Identity penalty β_1	0.386**	8081**	36.73**	0.063	457	4.57
	(0.179)	(3997)	(18.17)	(0.114)	(1809)	(18.09)
Information effect β_2	0.027	1061	4.82	0.250**	4395**	43.95**
	(0.148)	(3318)	(15.08)	(0.110)	(1796)	(17.96)
Marginal effect on P(markup)	0.012**			0.006		
	(0.006)			(0.011)		
Base rate in arm A	0.005			0.034		
Respondents quoting a markup	35	35	35	115	115	115
Observations	2,967	2,967	2,967	2,528	2,528	2,528

Notes. Conditions on the respondent not refusing. Columns 1 and 4 are probit coefficients for whether any markup is quoted; the remaining columns are Tobit coefficients, in rupees and as a percentage of the quoted rent. The arm-C cue differs between the two profiles: a previous landlord's reference on the first, a signal that the applicant's spouse is a senior corporate employee on the second. Because respondents with the largest identity penalty refuse rather than quote a price, selection into this sample falls by 9.5 points for tenants, 27.0 for landlords and 46.8 for brokers, the sample is selected toward the less discriminating part of the distribution and these are lower bounds. Appendix F reports Lee bounds. Standard errors clustered on the respondent.

The contrast is suggestive rather than established. The arm-C cue differs across profiles - a previous landlord's reference on the first, a signal that the applicant's spouse is a senior corporate employee on the second, and the interaction between the cue type and arm C is not distinguishable from zero on the ladder ($p = 0.34$), on the probability of a markup ($p = 0.63$), or on the size of the markup ($p = 0.68$). Appendix F reports the arm-C effect for all eight profiles; it is positive and significant on three of them and negative on one. For the profiles with Muslim applicant plus information, there seems to be no effect on being accepted, but for other applicants, depending on whether the information is positive or negative, acceptance changes accordingly. The market seems to value information, but not when the applicant is a Muslim.

Why the margin matters. A price penalty and a refusal are different economic objects. A transfer offsets a price penalty; no plausible transfer offsets a refusal. The margin also bears

on whether competition erodes discrimination. Under Becker (1957) a landlord who indulges a taste for discrimination forgoes rent and should be undercut; where rents are posted, capped between contracts and a queue of applicants is waiting, he forgoes little that he observes, and the rationing persists. That is the distinction drawn for labour markets by Akerlof (1982) and Akerlof and Yellen (1990), and the contribution here is to measure it rather than assume it.

4.3 Who does the excluding

Table 4 estimates the same specification by respondent type. The penalty rises along the chain of gatekeepers: 0.302 index units for respondents who live in rented accommodation, 0.887 for those who are landlords, and 1.298 for brokers. On binary acceptance, using only the accept and reject outcomes, the broker–landlord difference is 19.7 percentage points (s.e. 5.1, $\chi^2 = 6.70$). Brokers bear none of the cost of a bad tenant and earn nothing unless a tenancy completes, and their baseline acceptance of the benchmark applicant is no lower than landlords' (0.868 against 0.855). They are harsher only toward Muslim applicants.

Table 4: The identity effect rises along the chain of gatekeepers

	Pooled	Tenant	Landlord	Broker
β_1: identity effect	0.665***	0.302***	0.887***	1.298***
	(0.046)	(0.088)	(0.172)	(0.119)
Δ_2: onto the price margin	0.006***	0.004***	0.006**	0.004**
	(0.001)	(0.001)	(0.002)	(0.002)
Δ_3: onto refusal	0.217***	0.095***	0.293***	0.424***
	(0.014)	(0.027)	(0.052)	(0.033)
Λ: rationing share	0.971	0.962	0.980	0.990
	(0.003)	(0.006)	(0.008)	(0.005)
Observations	7,736	2,044	526	1,230
Respondents	3,868	1,022	263	615

Notes. All respondents answer the applicant experiment as hypothetical landlords, so the contrast across columns is in who the respondent is, not in what they were asked. Tenant and landlord are not mutually exclusive: the housing-category question allows a respondent to report both, and seven do; they appear in both columns but once in the pooled column. Bootstrap standard errors, 1,000 replications clustered on the respondent.

The rationing share is also marginally higher for brokers than for tenants (0.990 against 0.962, difference 0.028, $p < 0.001$), so brokers exclude more and do so slightly more completely on the quantity margin.

The estimate for respondents who live in rented accommodation is not a measure of tenant discrimination; it is what tenants say they would do as owners, informative about the norm rather than about market behaviour. The landlord column has the cleanest interpretation and

the fewest observations (263 landlord respondents), although statistically significant.

4.4 How much of the market each group can reach

Table 5 reports the broker elicitation. A Muslim family is said to be able to rent 23.8 of 100 properties in the broker's service area, against 85.8 for an upper-caste Hindu family and 82.1 for a lower-caste Hindu family. The gap between the two Hindu categories is 3.7 points; between upper-caste Hindu and Muslim families it is 62.0 percentage points. Associations are reported to bar Muslim tenants in 26.9 per cent of areas against 0.6 and 0.8 per cent for the Hindu categories. Four brokers answer "don't know" on the Muslim rule and are excluded rather than coded as no rule.

Figure 2 shows this access probability. The level for Muslims lies below every other category that was asked in the survey.

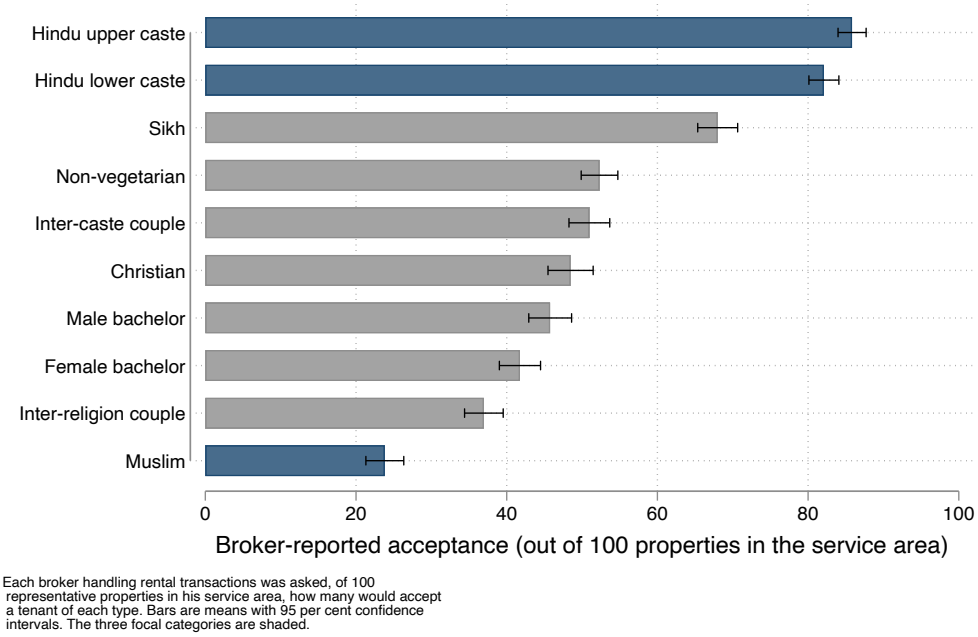


Figure 2: Broker-reported access by applicant type. Each of the 535 surveyed brokers who handle rental transactions was asked, of 100 representative properties in his service area, how many would accept a tenant of each type; bars are means across brokers. The Muslim–upper-caste-Hindu gap is 62.0 points; the lower-caste-Hindu gap is 3.7 points.

An independent count of all 1,960 registered housing societies in Delhi finds 68 per cent with no Muslim member (TwoCircles.net and Newslaundry, 2024), of the same order as the association figure reported here.

Table 5: How much of the local stock each group can rent

	Hindu, upper caste	Hindu, lower caste	Muslim
Properties out of 100 that would accept	85.8	82.1	23.8
	(21.9)	(23.3)	(29.5)
Gap relative to upper-caste Hindu	—	3.7	62.0
		(13.2)	(36.2)
Association bars this group (%)	0.6	0.8	26.9
Brokers	529	529	529

Notes. One observation per broker handling rental transactions. Of the 619 surveyed brokers, 535 were asked this module and 529 answered it. The association question offers allowed, not allowed, no rule, and don't know: 232 brokers report Muslim tenants allowed, 141 not allowed, 152 no rule and 4 don't know. The four don't-know responses are excluded rather than coded as no rule, which is why the denominator for the association row is 525.

Counting places rather than averaging probabilities. Table 6 asks, for each group, in how many neighbourhoods at least a given share of the local stock would accept them. There is no neighbourhood in the sample where fewer than half the properties would accept an upper-caste or lower-caste Hindu family. There are 20 where half would accept a Muslim family, holding 15.3 per cent of the surveyed rental stock. The caste comparison separates only at the strictest thresholds, while the Muslim penalty is visible at every one.

Table 6: Colonies reachable, by group and threshold

	Colonies			Share of rental stock (%)		
At least this share of local stock	Hindu UC	Muslim	SC	Hindu UC	Muslim	SC
25 per cent	139	72	139	100.0	53.7	100.0
40 per cent	139	34	139	100.0	24.7	100.0
50 per cent	139	20	139	100.0	15.3	100.0
60 per cent	135	15	135	95.1	9.6	94.0
75 per cent	129	11	112	89.1	8.1	71.9
90 per cent	57	8	37	47.0	6.2	30.5

Notes. Of the 139 colonies with both a broker-reported access figure and at least one surveyed renter. The stock columns are shares of the 1,018 surveyed rental units in those colonies, not of the city's stock.

The exclusion is clustered. Table 7 reports Moran's I [3] on a six-nearest-neighbour weight matrix with inference by permutation. Reported Muslim access, the access gap and association bans are all spatially concentrated: neighbourhoods resemble their neighbours.

[3]Moran's I measures global spatial autocorrelation by comparing the covariance of an outcome across spatially connected locations with its overall variance, using a pre-specified spatial-weight matrix. Positive values indicate that similar values cluster geographically, while negative values indicate that neighbouring locations tend to have dissimilar values. Under spatial randomness, its expected value is $-1/(N-1)$, which approaches zero as the number of spatial units increases.

Table 7: Spatial clustering of exclusion

	Moran's I	Permutation p	Colonies
Muslim access	0.409	0.001	139
Access gap, upper-caste Hindu minus Muslim	0.354	0.001	139
Association bars Muslim tenants	0.374	0.001	133

Notes. Six nearest neighbours, row-standardised weights; 999 permutations. A positive I means colonies resemble their neighbours.

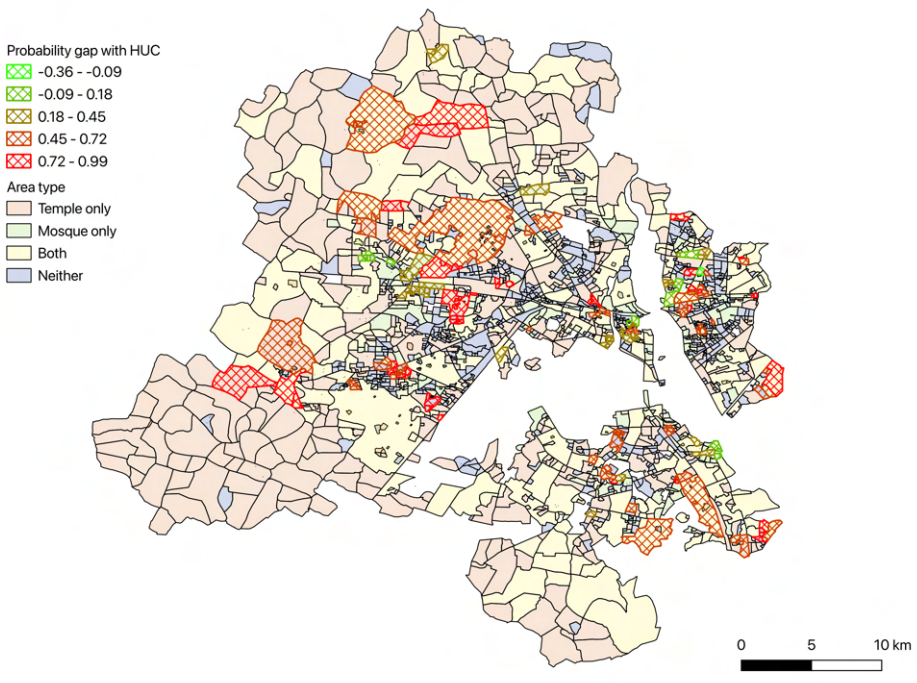


Figure 3: The Muslim access gap relative to Hindu Upper Caste, by neighbourhood

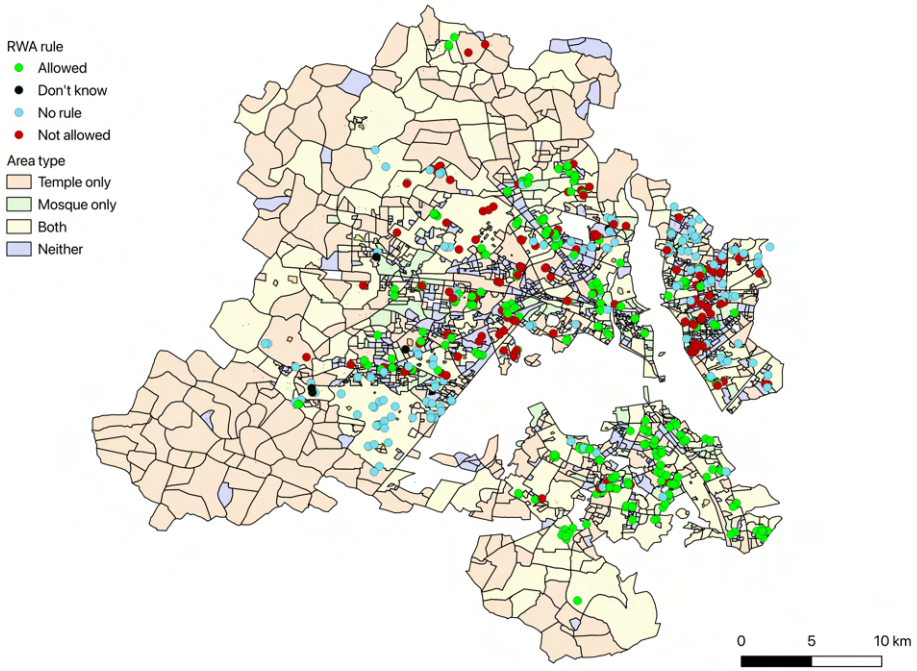


Figure 4: neighbourhoods where the association is reported to bar Muslim tenants

Reach from where a household already is. A citywide average understates the constraint if the accessible neighbourhoods are far from where a household needs to be. Table 8 reports the share of the rental stock within ten kilometres of each neighbourhood that each group could rent, and the distance to the nearest neighbourhood where at least half the stock would accept them. Within ten kilometres of anywhere in the city a Muslim household can reach about 31 per cent of the stock, with a tenth percentile of 23 and a ninetieth of 41. There is no part of the city in which the constraint lifts.

Table 8: Reach from each colony

	Hindu UC	Muslim	SC
Share of the rental stock within 10 km that would accept			
mean	0.862	0.312	0.821
10th percentile	0.796	0.234	0.726
90th percentile	0.907	0.407	0.895
Kilometres to the nearest colony where at least half the stock would accept			
median	0.14	3.37	0.14
90th percentile	1.21	9.19	1.21
maximum	3.72	16.87	3.72

Notes. Reported access never falls below 0.5 for either Hindu category in any surveyed colony, so for those groups the lower panel is the distance to the nearest colony of any kind.

Access and local composition. Reported Muslim access correlates 0.29 with the Muslim share of surveyed households in the neighbourhood. The relationship is descriptive and runs both ways: brokers may report higher access where more Muslim households already live because they live there, or they may live there because access is higher.

4.5 Which gatekeeper

The access question asks how many local properties *would accept* a tenant of a given type, which is a statement about landlords and associations. A broker's own screening i.e. which properties he mentions at all, sits on top of it, and the vignette experiment measures that second layer for the same brokers. I define a pass-through rate as the share of respondents of a given kind who accept the Muslim-named applicant divided by the share who accept the benchmark applicant. For brokers it is $0.428/0.868 = 0.49$; for landlords $0.559/0.855 = 0.65$. Base acceptance is nearly identical, so brokers are not harsher in general.

Table 9: Which gatekeeper does the excluding?

	Properties out of 100	Gap, log points (share)	
Upper-caste Hindu household	86.9	—	
Muslim, as brokers report local landlords	30.5	1.05	(62%)
Muslim, after broker screening	15.9	0.65	(38%)
Total gap		*1.70*	*(100%)*
Within the broker's share			
matching what landlords themselves do		0.42	(25%)
excess over the landlord standard		0.23	(13%)
Muslim access if brokers matched landlords	20.0		

Notes. Reported access is the mean over the 141 colonies in the sorting sample. The pass-through rate is the share of respondents of a given kind who accept the Muslim-named applicant divided by the share who accept the benchmark applicant: $0.428/0.868 = 0.49$ for brokers (415 and 200 respondents) and $0.559/0.855 = 0.65$ for landlords (170 and 93). Effective Muslim access is reported access times the broker pass-through rate, computed colony by colony from brokers within two kilometres where at least three of each arm are available (87 colonies) and from the area-type mean otherwise (54). Shares are of the total gap in logs, which splits additively because the two constraints multiply. The split assumes reported access captures the landlord and association layer with broker screening on top; if brokers already net their own screening out of the figure they report, the broker share is overstated. Brokers answered the applicant experiment as hypothetical landlords, so their pass-through rate proxies for showing behaviour rather than measuring it.

If a broker's screening multiplies the constraint his landlords already impose, the gap splits additively in logs. Of 1.70 log points between what an upper-caste Hindu household and a Muslim household can reach, 1.05, 62 per cent, is attributable to landlords and associations and 0.65, or 38 per cent, to brokers' own screening. The part exceeding the landlord standard is 0.23 points, 13 per cent of the total. A Muslim family could reach 20 of 100 local properties rather

than 16 if brokers screened no more harshly than the landlords whose properties they let.

Whose figure is the access number? Muslim and non-Muslim brokers do not report the same thing. Pooling, Muslim brokers report Muslim access of 0.71 and non-Muslim brokers 0.20, and since non-Muslim brokers are 92 per cent of the sample the headline figure is effectively theirs. Comparing brokers *within the same neighbourhood*, Muslim brokers report Muslim access 21.7 points higher (s.e. 8.5), while reporting upper-caste and lower-caste Hindu access slightly lower and not distinguishably so (-0.102 and -0.118). The disagreement is specific to Muslim access, which is not what sorting into different areas would produce. Reweighting the sample to a 13 per cent Muslim-broker share moves reported Muslim access from 0.238 to 0.266. The within-neighbourhood estimate is identified off the 14 neighbourhoods containing brokers of both religions, and the data cannot say which group is right: non-Muslim brokers may overstate exclusion, Muslim brokers may know a different segment of the stock, or access may genuinely depend on whose office an applicant walks into. The third possibility is consistent with the paper's argument.

4.6 What households want, and the gap

Figure 5 sets the two measurements side by side. Among respondents facing a task that offers their own group's area, 51.4 per cent of Muslim respondents choose a mixed or majority-Hindu neighbourhood. The comparison that makes this informative is not with 100 per cent but with the majority: 23.5 per cent of upper-caste Hindu respondents would live outside their own group's areas. Muslim households are more than twice as willing to live in mixed neighbourhoods as the majority is, and can reach about a quarter of the market. For both Hindu groups what they can reach exceeds what they say they would take; for Muslim households it falls well short. I call that difference the *preference–access gap*.

To put structure on this, let respondent i choose among neighbourhoods n in task t with deterministic utility

$$V_{int} = \beta_R \,\mathrm{Rent}_{nt} + \beta_C \,\mathrm{Commute}_{nt} + \theta_{MUS} \,\mathrm{MuslimArea}_{nt} + \theta_{SC} \,\mathrm{SCArea}_{nt} + \underbrace{\eta_{g(i)}}_{\substack{\text{own-group}\\ \text{preference}}} \mathrm{SameGroup}_{in}, \tag{5}$$

estimated by conditional logit with task fixed effects and standard errors clustered on the respondent, and reported as odds ratios in Panel B of Table 10.

Table 10: Neighbourhood choice, composition tasks

	Hindu UC area	Muslim area	SC area
Panel A. Share choosing the option, model free (%)			
own-group respondents	76.5	48.6	40.7
other respondents	43.1	4.1	16.5
difference (pp)	33.5***	44.5***	24.2***
Panel B. Conditional logit, own-group premium			
	Hindu UC	Muslim	SC
own-group × group	1.115***	2.914***	1.079***
	(0.081)	(0.108)	(0.095)
Area main effects, relative to an upper-caste Hindu area			
mixed		−0.148	
Muslim majority		−2.622	
Scheduled Caste		−1.524	
Choice observations		17,724	
Respondents		2,966	

Notes. The two composition tasks, in which options differ by rent, commuting time and the stated composition of the neighbours. Panel A compares the share of respondents choosing each option between those whose own group it is and all others. Panel B is a conditional logit with task fixed effects; the own-group terms and the area main effects come from the same specification. Rent and commuting time are omitted. Coefficients are log odds ratios from a conditional logit with area-type indicators. In each composition task the first-listed option (an upper-caste Hindu area in one task, a Muslim area in the other) is both the most expensive and the closest, and the mixed option has the longest commute, so rent and commute are correlated with composition by construction and their coefficients are wrong-signed. and the price coefficient used elsewhere comes from the amenity tasks (Table 14). Standard errors clustered on the respondent.

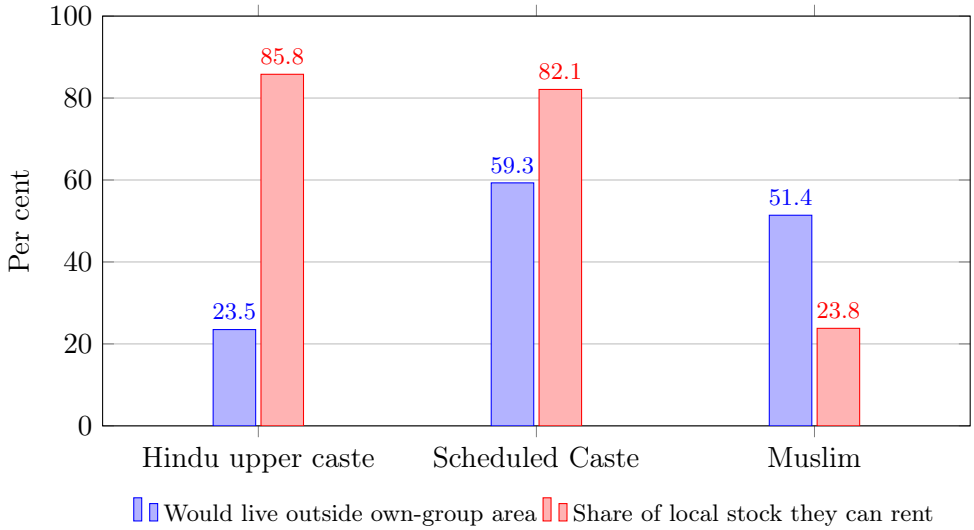


Figure 5: What each group would accept, and what each group can reach. The first bar is the share of respondents of that group choosing a neighbourhood that is not their own group's, among respondents facing a task that offers their own group's area; the second is the share of local rental properties brokers say would accept a family of that group. Taking the denominator over all composition tasks instead gives 61.8, 79.7 and 51.4, because the first two groups face tasks in which no own-group option appears. Scheduled Caste respondents are matched to the broker category "Hindu family, lower caste", which is related but not identical.

Table 10 reports the underlying choice estimates. Own-group options are chosen more often by every group: the gap between own-group and other respondents is 33.5 percentage points for upper-caste Hindu areas, 44.5 for Muslim areas and 24.2 for Scheduled Caste areas. In the conditional logit, which reproduces these shares, the own-group premium is 1.115 for upper-caste Hindu respondents, 2.915 for Muslim respondents and 1.079 for Scheduled Caste respondents, against area main effects of -0.148 for a mixed area, -2.622 for a Muslim-majority area and -1.524 for a Scheduled Caste-majority area. The Muslim premium is the largest, but it is measured against the strong avoidance of Muslim areas by everyone else. Net of it, a Muslim household values a Muslim-majority area 0.44 log points above a mixed one, while an upper-caste Hindu household values its own area 3.74 log points above a Muslim-majority one.

The gap also varies across the city in a way consistent with Section 4.4: Muslim access runs from 37.2 of 100 in areas with mosques to 17.0 in areas with neither mosques nor temples, while the lower-caste gap is between 2.6 and 4.4 points in every stratum.

5 What kind of discrimination is this?

The experiments were designed to narrow the set of explanations rather than settle on one. Six patterns bear on it, and they do not all point the same way.

Table 11: Access and association rules by area type

	Mosques only	Mixed	Temples only	Neither	All
Properties out of 100 that would accept					
Hindu, upper caste	82.3	86.4	85.0	89.7	85.8
Hindu, lower caste	77.9	82.3	82.3	85.5	82.1
Muslim	37.2	25.1	18.0	17.0	23.8
Gaps relative to upper-caste Hindu					
lower-caste Hindu	4.4	4.1	2.6	4.1	3.7
Muslim	45.1	61.3	66.9	72.7	62.0
Association bars Muslim tenants (%)	24.1	21.9	27.3	35.7	26.9
Brokers	110	157	153	115	535

Notes. Strata are defined by scraped counts of temples and mosques in the area. The lower-caste gap is distinguishable from zero in every stratum ($p \leq 0.016$) and never exceeds 4.4 points; the Muslim gap ranges from 45 to 73.

The penalty is not shared by everyone. Muslim respondents, answering the same instrument as hypothetical landlords, apply no penalty to Muslim applicants; the estimate is +0.089 (s.e. 0.029) on binary acceptance, against −0.260 (s.e. 0.015) for non-Muslim respondents, a difference of 0.347 (s.e. 0.029). Taking the four identity groups separately, the penalty is −0.290 for upper-caste Hindu respondents, −0.213 for Scheduled Caste, −0.279 for OBC and +0.089 for Muslim. A norm that every participant in the market expects to face would not look like this.

Information about reliability does little if you're a Muslim. The arm-C cue is not constant across profiles. It includes information about recommendations from previous landlord, longer contract, police verification, rarely delayed payments in previous tenancy, recent move to the city, no delays in rent payment in previous tenancy, income signal. For the Muslim profiles, information on reliability and income doesn't change acceptance. For the non-Muslim profiles, longer contract, police verification improve acceptance by around 5 percentage points, and rarely delayed payments (which could be interpreted as some delays) decreases acceptance by 4.3 percentage points. The information on recent move doesn't affect the acceptance. The signal on no delays in rent payments in previous tenancy, when given with a Muslim profile, also doesn't change the acceptance probability. So, it seems like the market does use the information signal, but not when the applicant is a Muslim. This points to a strong taste-based discrimination against Muslims. Table Table F.1 summarises these results. Implication 1 supplies one reason a reference might not help: it raises an applicant's standing in absolute terms without changing his rank against the others in the queue.

Brokers anticipate. Brokers who report that local landlords weigh religion exclude 18.0 percentage points more (s.e. 5.7); brokers whose local association bars Muslim tenants exclude 24.1 points more (s.e. 6.4). More than half of brokers (55.3 %) report that landlords weigh religion, against 21.7 per cent for caste. The broker's own view is not separable from his forecast of his client's, and the paper does not claim to separate them.

Competition erodes it, somewhat. Using broker density computed from 1,723 scraped broker shops, the identity effect falls from -0.494 at the tenth percentile of density to -0.299 at the ninetieth (interaction 0.0057, s.e. 0.0022). This is the Becker margin and it appears where theory says it should. It is suggestive rather than causal: density is not randomly assigned, and brokers cluster where the market is thick, which correlates with rents and planning status. The minimum detectable effect at eighty per cent power is 0.0061, marginally above the estimate, so the evidence is consistent with the prediction rather than a sharp confirmation of it.

Experience does not correct it. The interaction of the identity penalty with years in the business is -0.0024 (s.e. 0.0028); across twenty years of experience the penalty moves by about 0.05 against a penalty of 0.36. One explanation is that a broker never observes the counterfactual: an applicant he does not forward generates no feedback. That is an explanation rather than a test of one.

Exclusion is concentrated in higher-rent areas. The reported access gap is larger where the broker describes the area as higher-income, with a monotone gradient across categories on 529 observations: relative to the top category, low-income areas show a gap 59.8 points smaller. The pattern is confirmed using mean neighbourhood rent in levels (0.077, s.e. 0.024) and in logs (0.452, s.e. 0.132) on the smaller matched sample of 75 brokers. A commission is half a month's rent, so a broker in a high-rent area has more at stake. The design cannot separate that from the simpler reading that richer, planned neighbourhoods, which are also the neighbourhoods with associations, are more exclusive.

The queue does not predict exclusion. Implication 1 rests on landlords choosing among applicants, and the comparative static that follows is that exclusion should rise with the length of the queue. It does not. Table 12 reports the broker-reported number of applicants per completed let against reported access, the access gap, the association rule and the vignette penalty. Every coefficient is indistinguishable from zero, and so is the lower-caste placebo. Two readings are available and the data do not separate them: the measure is self-reported, cross-sectional and

may capture scale-use heterogeneity rather than market tightness; or the queue is long enough everywhere in this market, the median broker reports four applicants per let, that the margin has no bite within the observed range. The level of queueing is consistent with the mechanism; its cross-sectional gradient does not support it.

Table 12: Exclusion does not rise with the length of the queue

	Muslim access	Muslim gap	Lower-caste gap (placebo)	Association bars Muslims	Vignette penalty
Tightness (standardised)	−0.695	−1.295	0.526	0.027	
	(1.356)	(1.825)	(0.696)	(0.020)	
Identity penalty × tightness					0.002
					(0.028)
Area type fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	529	529	529	525	1,070

Notes. Tightness is the broker's reported number of potential tenants in an average month divided by completed matches, winsorised at the first and ninety-ninth percentiles and standardised; the median is 4.2 applicants per let. Columns 1 to 4 are broker-level; column 5 is the applicant experiment on broker respondents, interacting the identity change with local tightness. Column 3 is the placebo: if tightness captured general screening intensity it should move the lower-caste gap too. Nine brokers reporting more matches than applicants or implausible volumes are excluded. Robust standard errors; column 5 clustered on the respondent.

On balance the evidence describes majority households acting on their own preferences about neighbours and tenants, with brokers anticipating those preferences and, on the margin Section 4.5 isolates, exceeding them. The discrimination does appear to be predominantly taste-based rather than statistical, at least based on the types of information provided in the experiment. It is possible that the statistical discrimination is based on some other aspect which the experiment doesn't test and is getting captured in the identity effect, but even that still leaves a significant taste part against Muslims. The fact that the same information seems to be working if the applicant is not Muslim, strengthens the argument for taste based discrimination.

6 Robustness

Appendix F reports the full analysis; this section states the concerns that bear most directly on the headline numbers.

Placebo profiles. Three of the eight applicant profiles are identical across arms. Estimating the arm effect on those profiles gives $F = 0.72$, $p = 0.54$, with the pooled effect bounded within $[-2.0, +3.4]$ percentage points against a measured 21 to 24 points on the profiles that do change. Arm assignment, order and fatigue cannot account for the result.

The effect reverses when the change reverses. One applicant runs the change the other way, replacing a Muslim name with a Hindu one. Acceptance rises by 10.6 points. The forward and reverse effects are not exactly equal in magnitude, which may be because the reverse applicant is single and the forward ones are married, so I do not claim symmetry, but the sign is right.

Randomisation balance. A multinomial logit of arm on respondent characteristics gives $\chi^2(14) = 17.98$, $p = 0.21$. Tested one at a time, eleven of twelve characteristics are balanced; the share of female respondents differs across arms ($F = 4.25$, $p = 0.014$). The estimates are unchanged by covariate adjustment: the marginal effect of the identity change on acceptance moves from -0.224 with no controls to -0.230 with applicant-profile, respondent and area-type controls, and two thirds of that movement comes from the 45 respondents dropped for missing covariates rather than from the controls.

Demand effects. The concern is that respondents report what they think is expected rather than what they would do. Social desirability in this setting runs toward understating exclusion, so the measured gap is more plausibly a lower bound than an upper one. The survey was introduced as a study of rental housing decisions, not of discrimination. And the association figure is corroborated by an independent count of registered societies (TwoCircles.net and Newslaundry, 2024). The experiment plainly measures stated behaviour; the claim is that its direction and relative magnitudes are informative, not that its level is a market-clearing rate of refusal. Datta and Pathania (2016) find a 13% lower callback rate for Muslims than Hindu Upper Caste applicants.

The cost of refusing. In the hypothetical, refusing is free; in the market it leaves a flat empty. If that inflates refusal, it inflates the *level* of refusal and not the *share* of the identity effect that lands on refusal rather than price, because both responses are equally free in the instrument. A second landlord module presents respondents with pairs of prospective tenants and asks them to choose between them. This is a forced-choice design and is in one respect cleaner than the accept/reject vignettes, since it removes scale-use heterogeneity and elicits a direct ranking. One pair is a near-exact identity test with a twist. Sameer Sharma and Akbar Khan are both bankers, both single, both offering ₹35,000 on a twelve-month lease. They differ in name, and in tenancy history, which runs in the *Muslim* applicant's favour: he carries a good reference from a previous landlord and the Hindu applicant has no previous record. Respondents choose the Muslim applicant 54.0 percent of the time ($n = 2{,}793$, $z = 4.2$ against a null of one half).

Additionally, across neighbourhoods, the identity effect from the applicant experiment correlates at 0.50 with the share choosing the Muslim applicant in a separate forced-choice module.

Selection in the price margin. The markup is observed only for respondents who do not refuse, and the identity change reduces selection into that sample by 18.7 and 23.8 percentage points on the two profiles. Lee bounds, which require only that the identity change weakly reduces the probability of quoting rather than refusing, put the effect on the markup within $[49.4, 115.9]$ rupees on the first profile and $[-34.2, 341.8]$ on the second. As a share of rent the bounds are $[0.22, 0.53]$ and $[-0.34, 3.42]$ per cent. Quoted markups cluster on round numbers - 18 of 35 markups on the first profile are exactly ₹3,000, so ties at the trimming quantile are a concern.

Attrition. Overall completion is 98.3 per cent, but mid-survey attrition differs across strata by 0.5 per cent in temple-only areas against 2.2 per cent in mixed areas ($\chi^2 = 11.56$, $p = 0.009$), so the applicant-experiment sample is mildly selected on area type.

7 A sorting model with a measured choice set

7.1 What the model is for

The measurements so far describe a constraint. They do not say what the city would look like without it, because a household displaced from one neighbourhood arrives in another and changes it for everyone there. A model is needed to trace that, and the model used here uses every parameter estimated from the experiments. Its job is to combine two separately measured objects into statements neither supports alone.

7.2 Structure

There are N neighbourhoods. Owner-occupiers stay put and count toward composition; renters choose. A group-g ($g \in \{HUC, MUS, HSC\}$) renter lives in neighbourhood n with probability

$$\sigma_{g,n} \;=\; \frac{e_{g,n}\exp(V_{g,n})}{\sum_m e_{g,m}\exp(V_{g,m})}, \qquad V_{g,n} \;=\; \lambda\, C_g(\mathbf{c}_n) \;+\; \beta_R r_n \;+\; \xi_n, \tag{6}$$

where $C_g(\cdot)$ values the neighbourhood's composition $\mathbf{c}_n$, r_n is rent, and ξ_n collects everything else about the neighbourhood. The composition parameters come from Table 10 and the price coefficient from the amenity tasks. The availability weight $e_{g,n}$ enters multiplicatively, in the manner

of Manski (1977): a neighbourhood enters the choice set with that probability, independently of the taste shock.

The substantive departure is that $e_{g,n}$ is itself a product of two measured layers,

$$e_{g,n} = \underbrace{p_{g,n}}_{\text{landlords and associations}} \times \underbrace{s_{g,n}}_{\text{broker screening}}, \tag{7}$$

with $p_{g,n}$ the broker-reported share of local properties that would accept a group-g family and $s_{g,n}$ the local pass-through rate from Section 4.5. Writing the constraint this way is what allows the counterfactuals in Section 8 to concern a particular gatekeeper rather than the constraint as a whole.

The neighbourhood term ξ_n is calibrated so the model reproduces each neighbourhood's *total* number of renters, following Berry (1994). It is never fitted to composition, so the model's prediction of *who* rents where is out of sample.

7.3 Assumptions

1. **Availability multiplies.** A neighbourhood enters the choice set with probability $e_{g,n}$, independent of the taste shock.

2. **Closed city.** Every renter lives in one of the surveyed neighbourhoods; there is no outside option.

3. **Owners are fixed.** They count toward composition but do not move.

4. **One scale.** Stated-preference coefficients may be attenuated relative to real choices; λ rescales them, and results are reported at $\lambda = 1$ and at a calibrated $\lambda = 1.5$.

5. **Composition is categorical in the instrument.** A smooth step maps the experiment's categories to continuous shares.

Assumption 1 has a consequence that recurs below: because availability enters multiplicatively and the denominator sums over neighbourhoods, a change in access *uniform across neighbourhoods* cancels and moves no one. Only the spatial pattern of a constraint affects sorting.

7.4 Parameters and fit

The model is fitted to neighbourhood renter totals alone, so its prediction of each neighbourhood's Muslim renter share can be tested. Predicting renters from owner-occupiers' composition only, so the households being predicted play no part in the prediction, gives a renter-weighted

correlation of 0.79 with the observed share. Simulated dissimilarity is 0.63 $[0.60, 0.65]$ against an observed 0.70, and simulated Muslim isolation 0.54 $[0.52, 0.56]$ against 0.57. The model gets the ranking of neighbourhoods right and under-predicts the level of segregation at every scale tried up to $\lambda = 4$.

Table 13: Model parameters, all estimated from the experiments

	Source	Estimate
Composition preferences		
η_{HUC} own-group premium, upper-caste Hindu	composition tasks	1.115
η_{MUS} own-group premium, Muslim	composition tasks	2.914
η_{HSC} own-group premium, Scheduled Caste	composition tasks	1.079
θ_{mix} mixed area	composition tasks	−0.148
θ_{mus} Muslim-majority area	composition tasks	−2.622
θ_{sc} SC-majority area	composition tasks	−1.524
Price		
β_R rent, thousands of rupees	amenity tasks	−0.118
β_C commuting time, minutes	amenity tasks	−0.045
Access		
$p_{HUC,n}, p_{MUS,n}, p_{HSC,n}$ reported access	broker elicitation	by colony
$s_{MUS,n}$ broker pass-through, Muslim	applicant experiment	by colony
Calibrated		
λ scale	matched to isolation	1.5
ξ_n colony term	matched to renter totals	by colony

Notes. The composition parameters are the conditional logit estimates in Table 10 expressed in the model's notation, with area fixed effects added. β_R and β_C come from the amenity tasks, where rent varies within task independently of composition; In each composition task the first-listed option (an upper-caste Hindu area in one task, a Muslim area in the other) is both the most expensive and the closest, and the mixed option has the longest commute, so rent and commute are correlated with composition by construction and their coefficients are wrong-signed. Calibrating λ against observed isolation spends that moment, so results are reported at $\lambda = 1$ as well.

Levels of dissimilarity in a sample of this size need a benchmark: under random allocation of the same households to the same neighbourhoods, dissimilarity is 0.23 rather than zero (Carrington and Troske, 1997), implying a systematic dissimilarity of 0.61. Counterfactual *comparisons* are unaffected, since both arms are model predictions.

7.5 What the constraint does to the choice set

Table 15 reports four access scenarios. Enforcing association rules raises mean effective access slightly. Bringing broker screening to the landlord standard raises it from 0.159 to 0.199 and doubles the number of neighbourhoods in which half the local stock is open, from 5 to 10. Doing both gives 0.211. Equalising the reported layer entirely - not a policy, but the bound on what

Table 14: Amenity tasks: the identified price coefficient

	Rent in rupees	Rent as a share of income
Rent (thousands)	−0.118***	
	(0.009)	
Rent as a share of stated income		−9.240***
		(0.773)
Commute time (minutes)	−0.045***	−0.041***
	(0.004)	(0.004)
Own-group × Hindu upper caste	0.460***	0.639***
	(0.055)	(0.058)
Own-group × Muslim	0.645***	0.584***
	(0.100)	(0.099)
Own-group × Scheduled Caste	0.223***	0.403***
	(0.078)	(0.080)
Choice observations	17,730	17,730

Notes. The two amenity tasks, in which options are described by bundles of local facilities and rent varies within task independently of composition. Own-group is signalled by the religious institutions in the bundle. The two tasks state household incomes of ₹90,000 and ₹45,000, so a rupee of rent is not the same object across them; column 2 uses rent as a share of the income stated in the task. At the column 1 estimates the implied willingness to pay to avoid thirty minutes of commuting is ₹11,414 (s.e. 1,025) a month, and the own-group premia are worth ₹3,897, ₹5,469 and ₹1,892. Standard errors clustered on the respondent.

the implementable policies reach, gives 0.427 and 33 neighbourhoods.

Table 15: Access under four scenarios

	Mean effective Muslim access	Colonies with at least half the stock open	Share of stock (%)
Observed	0.159	5	3.6
Association rules enforced	0.167	5	3.6
Brokers at the landlord standard	0.199	10	7.5
Both	0.211	10	7.5
Reported access equalised (benchmark)	*0.427*	*33*	*25.5*

Notes. Effective access is reported access times the local broker pass-through rate, as in (7), over the 139 colonies with both an access figure and a surveyed renter. The last row removes the entire landlord-and-association gap and is reported to show what share of the constraint the first three reach. The combined row assumes the two layers are independent; if a broker's screening already reflects the local association rule, it overstates.

7.6 Segregation

Equalising Muslim access in every neighbourhood reduces modelled dissimilarity by 10.8 per cent with composition and rents held fixed, 12.1 per cent when composition responds, and 4.4 per cent when rents also clear a fixed stock. At the calibrated scale the figures are 9.3, 10.3 and

1.5 per cent. Bootstrap intervals on the last include zero at both scales, so the result is best stated as a bound: the spatial pattern of the access gap accounts for at most about a tenth of Hindu–Muslim dissimilarity in this model, and possibly none of it.

Table 16: Share of dissimilarity attributable to the access gap

	$\lambda = 1$		$\lambda = 1.5$	
	Share	95% interval	Share	95% interval
Composition and rents fixed	0.108	[−0.098, 0.137]	0.093	[−0.164, 0.115]
Composition responds	0.121	[−0.027, 0.183]	0.103	[−0.094, 0.138]
Rents also clear	0.044	[−0.045, 0.104]	0.015	[−0.076, 0.062]
One-sided p against zero		0.165		0.350
Baseline dissimilarity	0.655	[0.596, 0.698]	0.689	[0.631, 0.747]

Notes. The counterfactual sets Muslim access equal to upper-caste Hindu access in every colony. Intervals are percentile intervals from 200 bootstrap replications that resample residents and brokers within survey type and stratum, rebuild the colony file including the access measures, re-estimate both conditional logits and re-solve the model. They do not propagate uncertainty in λ, which is held at its full-sample value. No replication failed. The p-value is the share of replications in which the attributable share at the third stage is at or below zero.

The reason is visible in the preference estimates rather than in the solver. For an upper-caste Hindu household, the gap between its own area (1.115) and a Muslim-majority area (−2.622) is 3.74 log points. For a Muslim household, a Muslim-majority area is worth $-2.622 + 2.915 = 0.29$ relative to an upper-caste Hindu area and 0.44 more than a mixed one: a modest premium. Muslim households are close to indifferent across compositions; the majority is not. Equalising access does not touch the majority's avoidance, and it is the majority's avoidance that separates the groups. Severe exclusion and modest segregation consequences are not in tension; they are what this configuration of preferences implies.

Because composition enters utility, the fixed point need not be unique. Starting the solver from an integrated allocation and from a segregated one gives stage-3 dissimilarity of 0.614 and 0.686 against 0.655 from the observed allocation. The spread across starting points exceeds the counterfactual effect, so no single level should be quoted; the *effect* is stable in sign and size from every start.

7.7 What the constraint costs a household

The access constraint can be expressed as a loss in expected utility. Table 17 reports it. A Muslim household loses 1.24 log points of expected utility against 0.13 for an upper-caste Hindu household, a differential of 1.11. The decomposition separates what any restriction of the same size would cost i.e. the loss of variety, from what is attributable to *which* neighbourhoods

are foreclosed. The targeted component is small and negative for Muslim households (−0.11), meaning the foreclosed neighbourhoods are if anything slightly less attractive to them than a random selection of the same number. The cost is lost options rather than relegation to worse places.

Table 17: What the access constraint costs a household

	Hindu upper caste	Muslim	Scheduled Caste
Share of areas reachable	0.876	0.257	0.838
Total loss, log points	0.131	1.242	0.177
of which variety	0.134	1.352	0.171
of which targeted	−0.003	−0.110	0.006
Differential against upper-caste Hindu	—	1.111	0.046
in rupees per month, at $\beta_R = -0.118$	—	9,415	387

Notes. Computed at the area level over the 263 areas carrying both an access figure and a composition classification; the area-level mean access of 0.876 and 0.257 differs from the broker-level means of 0.858 and 0.238 in Table 5 because brokers are averaged within area first. The total loss is an inclusive-value difference and equals minus the log of choice-weighted access identically, so the informative quantity is the decomposition: the variety term is what removing the same expected number of areas at random would cost, and the targeted term is the remainder. A negative targeted term means the foreclosed areas are slightly less attractive to the group than a random selection of the same number. The rupee conversion scales inversely with β_R; at the alternative estimate of −0.020 it would be about five times larger, which is why the log points are the primary figure.

A money figure, with a range. Converting the loss requires the marginal utility of income, and the price coefficient depends on which composition control enters the amenity tasks. With own-group indicators it is −0.118 (s.e. 0.009); with separate mosque and temple terms it is −0.020. The first implies a willingness to pay of ₹11,414 a month to avoid thirty minutes of commuting, which at a median imputed income of ₹30,000 which is about 40 per cent of median monthly income and values travel time at roughly three times the hourly wage. That is high against conventional estimates of a third to two-thirds of the wage, but far closer than the alternative, which implies eleven times. On that ground the paper uses −0.118 and reports a differential cost of about ₹9,400 a month for Muslim households, while noting that the figure scales inversely with a coefficient the design does not pin down tightly.

An external check is available in principle, the money value of commuting time can be read off observed rents, and it fails: the hedonic gradient of log rent on commuting time is indistinguishable from zero in every specification, including with neighbourhood fixed effects (Appendix H). Rents in this sample do not capitalise location value, which is itself consistent with Section 4.2, and which means the money figure rests on the experiment alone.

8 Policy

Rental brokers in India are unregulated. The Real Estate (Regulation and Development) Act covers sales intermediaries and leaves letting outside its scope, so extending registration to rental brokers, with a non-discrimination condition attached, is a change of coverage rather than of principle.

Section 4.5 gives the magnitude at stake: brokers account for 38 per cent of the access gap, and the part exceeding the standard set by the landlords they serve is 13 per cent of the total. Closing it would widen the stock a Muslim family can reach from 16 to 20 properties in 100, and double the number of neighbourhoods in which half the local stock is open to them.

Three features of the exercise bear on design.

Where to enforce matters more than how much. The gain from enforcing in a neighbourhood depends on how harshly local brokers screen relative to what local landlords would accept, not on the size of the local gap. Ranking neighbourhoods by the size of the gap which is the natural-looking rule, performs little better than random. Table 18 reports stock-weighted mean Muslim access under each rule. Enforcing in the best ten neighbourhoods of 139 captures about half the benefit of enforcing everywhere; ten chosen at random capture about a fourteenth of it.

Table 18: Targeted versus uniform enforcement

Colonies enforced	By gain × stock	By gain	By size of gap	Random
10	0.182	0.173	0.175	0.163
20	0.192	0.181	0.179	0.166
30	0.198	0.186	0.185	0.169
40	0.201	0.192	0.188	0.172
50	0.204	0.198	0.190	0.175

Notes. Stock-weighted mean Muslim effective access. Doing nothing gives 0.160 and enforcing in all 139 colonies gives 0.200 on the same measure, so enforcing in the best ten captures 54 per cent of the available benefit and ten chosen at random 7.5 per cent. The random column averages 100 draws. Nineteen colonies already have brokers screening at or above the landlord standard, so enforcement there changes nothing, which is part of why targeting pays.

What broker regulation does not reach. Sixty-two per cent of the access gap sits with landlords and associations. No regulation of intermediaries touches it, and Table 15 shows the implementable policies moving effective access from 0.159 to about 0.21 against 0.43 if the landlord-and-association layer were removed entirely. Association rules are the harder target:

they are unwritten, they have legal cover (zor, 2005), and the external check available here suggests they are widespread (TwoCircles.net and Newslaundry, 2024).

One further implication follows from Section 4.2. Because exclusion operates on acceptance rather than price, instruments that work through price i.e. housing allowances, rent subsidies, vouchers, will not reach it. That is a conclusion about this market and this margin, and the contribution is that the margin was measured rather than assumed.

9 Conclusion

Residential segregation is usually read as a record of what households chose. In the housing market, that reading needs a premise to hold: that the households living apart could have lived together. This paper tests the premise in Delhi, where the index of dissimilarity between Muslim and non-Muslim households across the surveyed neighbourhoods is 0.70, against 0.23 under a random allocation of the same households. It does so by measuring, on the same households and the same neighbourhoods, both what households want and what the market will let them have.

The paper's most portable contribution is in how the two halves are measured and combined. The standard sorting approach recovers preferences by inverting observed residence, which requires that every household could have lived anywhere; audit and correspondence studies measure refusal but say nothing about where the refused applicants would have chosen to live. Here neither object is inferred from the other. Preferences come from choice tasks in which every option is stipulated to be available and affordable. Availability comes from the brokers who hold the listings for each part of the city. And the margin on which discrimination operates comes from a response ladder that separates refusal from acceptance at a higher rent, so that the distinction between rationing and pricing is measured rather than assumed.

Three features make this more than a pairing of instruments. First, eliciting access from the intermediary reaches the pre-listing margin: properties an applicant is never shown, which an audit cannot observe by construction. Second, because the choice set is measured rather than assumed, the sorting model can be fitted to neighbourhood renter totals alone, leaving its prediction of *who* rents where as an out-of-sample test. It ranks neighbourhoods by Muslim renter share at a correlation of 0.79, a test the inversion approach cannot offer, since it fits composition by construction. Third, writing effective access as the product of separately measured layers turns a question the literature has not been able to pose, which gatekeeper in a chain of them produces the constraint, into an accounting exercise with direct policy content. None of this is specific to Delhi. Wherever intermediaries screen before search begins, as in much of the rental

housing of South Asia, Africa and Latin America, or in labour markets run through recruiters, the same combination of a choice experiment, an applicant experiment and an elicitation from the intermediary can be fielded, and at the cost of a survey rather than an administrative record that rarely exists in such markets.

Three numbers summarise them. A Muslim name lowers the chance of acceptance by about 22 percentage points, and 97 to 98 per cent of that is refusal rather than a higher rent. A Muslim family can rent about 24 of 100 local properties against 86 for an upper-caste Hindu family, and in only 20 of 139 neighbourhoods would half the local stock accept them. Half of Muslim respondents would live in a mixed or majority-Hindu neighbourhood if they could, twice the share of upper-caste Hindu respondents willing to live outside their own group's areas.

Around those numbers the evidence has a consistent shape. The exclusion is specific to religion rather than to minority status: the access gap facing a lower-caste Hindu family is under four points in every type of area, against 45 to 73 points for a Muslim family. It is spatially clustered rather than scattered, so that within ten kilometres of anywhere in the city a Muslim household can reach about 31 per cent of the rental stock, with no part of the city in which the constraint lifts. It rises along the chain of gatekeepers, from tenants answering as hypothetical owners, to landlords, to brokers, who are no harsher than landlords toward the benchmark applicant and harsher only toward the Muslim one. And it is institutional as well as individual: brokers report that associations bar Muslim tenants in about a quarter of areas, against under one per cent for either Hindu group.

Taking those measurements into a sorting model does three things. It splits the constraint between the gatekeepers who produce it, and finds that brokers account for 38 per cent of the access gap and that 13 points of the total is excess over the standard set by the landlords they serve. It prices a regulatory change already under discussion: bringing brokers to the landlord standard would widen the stock a Muslim family can reach from 16 to 20 properties in 100 and double the number of neighbourhoods in which half the local stock is open to them, and enforcement directed at the ten neighbourhoods with the most to gain would deliver about half the benefit of enforcing everywhere. And it disciplines the inference that would otherwise be natural. Equalising access in every neighbourhood reduces modelled dissimilarity by at most about a tenth, with a bootstrap interval that includes zero, because the force separating the groups is majority avoidance of Muslim areas rather than Muslim attachment to them. The constraint is nonetheless costly to the households who face it: a Muslim household loses about 1.1 log points of expected utility more than an upper-caste Hindu household, roughly ₹9,400 a month at the preferred price coefficient, and the loss comes from the narrowing of options rather

than relegation to worse places.

Two claims should be kept apart. That Muslim households in Delhi face a severe constraint on where they may live is measured directly and does not depend on the model. That the constraint is what makes the city segregated is not supported by it. Severe exclusion and modest segregation consequences are not in tension; they are what this configuration of preferences implies, and a reading of the city's map as a record of choice would have missed the first while overstating the role of minority preference in the second.

Because exclusion here operates on acceptance rather than price, instruments that work through price, such as housing allowances, rent subsidies or vouchers, will not reach it. Because the cost of refusing is small in a market with posted, capped rents and a queue of applicants, competition should not be expected to erode it quickly, although the penalty does fall by about two-fifths across the observed range of broker density. Regulating rental brokers, who are at present outside the scope of the law that covers sales intermediaries, reaches the part of the gap brokers add. It leaves untouched the larger part that sits with landlords and associations, whose rules are unwritten, widespread, and have so far had legal cover.

A follow-up wanting a money metric would need a choice task in which rent varies independently of the rest of the bundle. Similarly, to separate a broker's own preferences from his forecast of his client's would need to ask him not what he would accept, but what he would show. And to know whether the constraint is hardening or easing would need the same measurements repeated over time, which the instruments here, being cheap to field and anchored to named places, make possible.

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A Background and context

A.1 Segregation, sorting, and opportunity in urban India

Frictions in housing markets impose welfare and efficiency costs beyond their distributional ones, by limiting households' ability to reach productive locations (Moretti, 2011; Hsieh and Moretti, 2019; Glaeser and Luttmer, 2003). Location also governs access to local public goods, and households sort across neighbourhoods, partly in response to differences in amenities and service provision (Tiebout, 1956; Bayer et al., 2007), so barriers to residential mobility may reinforce existing disparities rather than merely reflect them.

Residential segregation along religious and caste lines is a prominent feature of urban India, documented at national scale by Asher et al. (2026) and across metropolitan areas by Bharathi et al. (2021) and Singh (2019), who finds caste segregation persisting or increasing in a majority of Indian cities between 2001 and 2011. Work specifically on Muslim segregation shows it to be widespread but highly variable in extent and form across and within cities (Susewind, 2017; Shah, 2024). Galonnier (2015) argues that Muslim residential clustering can simultaneously reflect involuntary segregation from discrimination and insecurity, voluntary clustering on cultural grounds, and exclusionary practices by dominant groups, which is precisely the identification problem this paper addresses; Gayer and Jaffrelot (2012) document the historical trajectory. Reporting suggests movement of Muslim households into Muslim-majority neighbourhoods in Delhi has increased following episodes of communal violence (Reuters, 2024).

Concern about anti-Muslim hostility in India has grown over the past decade (Reuters, 2025; Human Rights Watch, 2024; U.S. Department of State, 2024; Pew Research Center, 2026). I record this because it is the context in which the data were collected, but the paper's contribution does not rest on it. The mechanisms it identifies i.e., selection from a pool of applicants, ex-ante screening under weak contract enforcement, and gatekeeping by intermediaries, are structural features of the market rather than functions of the political moment.

A.2 Religion as an axis of economic exclusion

The economics of discrimination has developed largely around race in the United States and ethnicity in Europe (Becker, 1957; Arrow, 1973; Phelps, 1972; Coate and Loury, 1993). Religion has been studied less, and in Western settings cannot easily be studied cleanly, because religious minority status is bundled with immigration, national origin and race. Adida et al. (2010) hold Senegalese origin fixed and vary only religious signalling in French applications, finding the Muslim applicant roughly 2.5 times less likely to be called for interview. Di Stasio et al.

(2021) generalise the design across five European labour markets and separate an anti-Muslim penalty from a "Muslim by default" origin stigma. In housing, Ahmed and Hammarstedt (2008) document substantially fewer callbacks to Arab/Muslim-named applicants in Sweden, and Flage (2018), pooling 25 OECD correspondence studies from 2006 to 2017 comprising more than 300 estimates and over 110,000 inquiries, finds majority applicants almost twice as likely to be selected as minority applicants generally and more than twice as likely relative to Arab or Muslim applicants specifically.

Evidence from India is thinner but pointed. Banerjee et al. (2009) find caste and religion affect callbacks in Indian white-collar labour markets; Thorat and Attewell (2007) and Siddique (2011) report analogous results. In housing, Datta and Pathania (2016) run a correspondence study in Delhi and find landlords markedly less responsive to Muslim than to comparable upper-caste Hindu applicants; Thorat et al. (2015) document caste and religion barriers in urban rental access. Gbadegesin and Ojo (2013) report ethnic bias in tenant selection in Ibadan. Reviewing the literature more broadly, Ifeanacho et al. (2023) find it concentrated almost entirely in the United States, Germany and Canada.

Two findings from this literature bear directly on the design here, and I return to both. First, the *information* margin: Ahmed et al. (2010) and Auspurg et al. (2017) find that supplying landlords with additional applicant information, or signalling high occupational status, reduces discrimination in Swedish and German rental markets, consistent with a statistical mechanism operating there. My vignette design includes a directly comparable reliability-signal arm, and it is informative that the signal moves acceptance in Germany but not in Delhi. Second, the *tightness* margin: Christensen and Timmins (2023) find discriminatory constraints substantially stronger for listings on the market fewer than three days than for those listed more than a week, and stronger still once a property manager has already received inquiries from other applicants.

A.3 Delhi's rental market: prices, contracts, and enforcement

Delhi is the national capital and draws people from across the country. Rental housing accounts for roughly 28 percent of the occupied urban residential stock in India (Gandhi et al., 2021), and Delhi accounts for a substantial share of it. Three features of this market shape what follows.

Landlords select from a pool. The brokers in this survey report a median of 4 potential tenants for every completed let in an average month, with a mean of 6.0 and an interquartile range of 2 to 7.5; the implied match rate is 0.31 (Table F.6, Panel A). That is the central institutional fact for what follows. A landlord facing an unwanted applicant does not have to

choose between letting to that applicant and holding the property empty. He has alternatives, and he does not have to lower the rent to reach them.

Rents do not do the allocating. Rental housing fell from 53 percent of the occupied urban residential stock in 1971 to 28 percent in 2011 (Gandhi et al., 2021), while the 2011 Census records 11.1 million vacant urban units, 12.4 percent of the stock, against 1.8 million in 1971. Gandhi et al. (2021) attribute the coexistence of shortage and vacancy to the interaction of pro-tenant rent control with weak contract enforcement, and estimate that permitting free rent revision reduces district vacancy by 2.8 to 3.1 percentage points. Gross rental yields run at 2 to 4 percent, below the risk-free rate. The Delhi government's Economic Survey for 2020–21 records 6.75 million people in low-income settlements, 695 slum settlements, 1,797 unauthorised neighbourhoods and 362 urban villages, and an additional requirement of 2.4 million housing units.

I flag what this evidence does and does not establish. It establishes that posted rents in this market are not a free instrument: they are constrained by statute, adjusted infrequently, and set property-by-property rather than applicant-by-applicant. It does *not* establish that the market is in aggregate excess demand, and a large vacant stock is on its face evidence in the other direction. The paper's empirical claim is about the *margin on which identity operates*, which I measure directly in Section 4.1. Excess demand is one micro-foundation for what I find, and Appendix E works it through, but the results do not depend on it.

Contracts are thin and enforcement is slow. Rent and tenancy are state subjects; Delhi is governed by the Delhi Rent Control Act 1958, which fixes standard rents, restricts revision, and constrains the grounds for recovering possession. Landlords have adapted rather than complied. The dominant instrument is the eleven-month leave-and-licence agreement, executed under the Indian Easements Act 1882, which grants permission to occupy without transferring an interest and, being under twelve months and unregistered, falls outside rent control. Roughly 19 percent of households in formal urban rental housing hold a written contract, and roughly 14 percent in slums (Gandhi et al., 2021). Contract enforcement in India took an average of 1,445 days as of 2019 (World Bank, 2020). The Model Tenancy Act 2021 would require written agreements, cap deposits and impose a sixty-day deadline on a dedicated dispute-resolution tier; adoption has been uneven and Delhi has not adopted it.

Screening therefore happens at the door. A landlord who cannot rely on ex post remedies must resolve his exposure ex ante. But the information available at the door is thin: incomes are

largely informal, and there are no credit scores, tenant databases or rental histories. Screening falls back on whatever is cheap and observable. This generates a testable prediction, because identity is being used where verification is absent, supplying verification directly should reduce the identity effect, and Section 4.1 shows that it does not.

A.4 The gatekeepers and the legal status of exclusion

How a rental transaction works. Brokers, community networks and online portals are the main search avenues in Delhi, with over 90 percent of tenants in the survey reporting having used one or more.[4] Brokers pre-screen applicants before taking a shortlist to the landlord; identity, occupation, family composition and dietary preferences are established pre-viewing, in the first meeting with the broker. The broker then arranges the viewing, conducts the first round of rent negotiation, and drafts the agreement. He typically charges half a month's rent from each side on completion and services an area of roughly 1.5 to 2 kilometres around his shop. A fuller narrative is in Appendix C.

The feature that matters analytically is that *pre-screening precedes the viewing*. Exclusion at the pre-screen is invisible to the applicant, who never learns which properties existed. A correspondence study observes screening outcomes for properties the applicant already knows about; it cannot observe the inventory withheld. Gatekeeper elicitation can.

Who exercises discretion. *Landlords* hold the residual right to select and bear the consequences of selection. *Brokers* hold and generate the listing inventory, pre-screen, vouch for the tenant, and draft the agreement, but do not own the property, set the rent, sign the contract, or bear the tenant's default risk. That last point carries weight: the broker's income is a commission on a completed match and his continuing business depends on his standing with local landlords, so his screening question is not "will this tenant perform?" but "will this tenant be accepted?". Brokers are also asked directly who normally makes the final tenant decision in their area, and 98 percent name the landlord (Section 5). *Resident welfare associations* administer eligibility norms that are often unwritten but enforced in practice, and that operate on the landlord rather than the tenant. Reporting based on the membership records of all 1,960 registered housing societies in Delhi found 68 percent with no Muslim member, with Muslim membership across societies barely exceeding 3 percent (TwoCircles.net and Newslaundry, 2024); I use this as an external benchmark for the elicited RWA measure in Section 4.4.

[4] 99acres captures roughly 30 percent of online property-search traffic in Delhi, MagicBricks roughly 25 percent and Housing.com roughly 20 percent, with the remainder fragmented across direct-from-owner sites, classifieds and social-media groups, many of the listings on which are posted by brokers. Source: Semrush Traffic Analytics, accessed 15 May 2026.

Both brokers and RWAs are *formal institutions*. Brokers operate from fixed premises and serve compact areas, so the population of relevant gatekeepers in any neighbourhood is small and observable. This is what makes the discrimination measured here a tractable target for policy rather than an immutable feature of preferences, and it parallels the argument of Kline et al. (2022) that discrimination concentrated in a small number of identifiable organisations is far more amenable to enforcement than discrimination diffused across many.

The legal position. India has no general statutory prohibition on discrimination in private housing. Article 15(2) of the Constitution reaches shops, public restaurants, hotels and places of public entertainment; it does not extend to private lettings. In *Zoroastrian Cooperative Housing Society Ltd. v. District Registrar, Cooperative Societies (Urban)*, (2005) 5 SCC 632, the Supreme Court *upheld* a housing society's bye-law restricting membership to a single religious community, holding that the governing Cooperative Societies Act did not prohibit such a restriction, that it was not contrary to public policy, and that the associational autonomy protected by Article 19(1)(c) was engaged. Identity-based eligibility rules in housing societies are therefore not, in general, unlawful. This reframes rather than weakens the policy contribution: the object is not to enforce an existing right but to identify where a regulatory hook can be created at low institutional cost. Section 9 argues that the broker, not the society, is that point.

B Data, sampling and construction

The analysis combines a survey of resident households, a survey of rental brokers, neighbourhood-level spatial data, and randomized experiments embedded in both surveys. The design observes, for the same neighbourhoods, both the preferences households reveal over locations and the access constraints they face.

neighbourhoods were stratified into four types by the religious institutions scraped for each area: mosques only, mixed, temples only, and neither. Table B.1 reports the resulting frame. Within each sampled neighbourhood a random starting point was chosen and every fifth dwelling approached in a fixed direction. Brokers were drawn from a separate scrape of commercial listings within the same strata.

Table B.1: Sampling frame by stratum

	Residents	Brokers	neighbourhoods	Temples (mean)	Mosques (mean)
Mosques only	797	131	89	0.00	2.43
Mixed	893	192	119	5.80	4.09
Temples only	815	167	126	2.36	0.00
Neither	785	129	116	0.00	0.00
Total	3,290	619	450	2.31	1.66

Notes. Counts of temples and mosques are from the scrape used to construct the strata, averaged over neighbourhoods within each stratum. The neighbourhood count is of neighbourhood-by-stratum cells containing at least one respondent. Produced by `03_descriptives.do`.

B.1 Sampling

Residents. Respondents were selected through a multi-stage design stratified by local religious composition. In the first stage I mapped neighbourhood amenities and religious institutions across the city and used them to classify neighbourhoods into strata, in particular areas with mosques versus those with temples, alongside mixed areas. In the second stage, neighbourhoods were drawn at random within strata. Within each selected neighbourhood, enumerators began from a random starting point and surveyed every fifth household along a fixed route, which limits enumerator discretion in selecting respondents. All resident and broker locations were geocoded.

Brokers. The broker sampling frame is a list of rental brokers scraped from Google Maps across the city. Brokers were drawn at random from this frame, controlling for area type as defined by

the same temple- and mosque-concentration classification used for the resident strata.

Overlap between the two surveys. The two surveys are linked through the geocoded neighbourhood rather than through a common sampling identifier, since brokers were drawn from a separate frame. Of the 180 neighbourhoods containing a surveyed resident household, 46 also contain a surveyed broker, and 39.4 percent of resident respondents live in a neighbourhood with broker coverage (Figure B.1). Where the analysis requires a broker measure for a resident's location I also report a distance-based linkage, matching each resident to the surveyed brokers within two kilometres, which is closer to the service radius brokers describe.

Conflict oversampling. Within each stratum the design oversamples neighbourhoods with a documented history of communal conflict: two conflict-exposed neighbourhoods are drawn per stratum. In the realised sample 24 of 417 neighbourhoods are conflict-exposed, containing 473 of 3,290 resident respondents but only 14 of 619 brokers. The resident-side comparison is therefore feasible and the broker-side comparison is not (Appendix F.6).

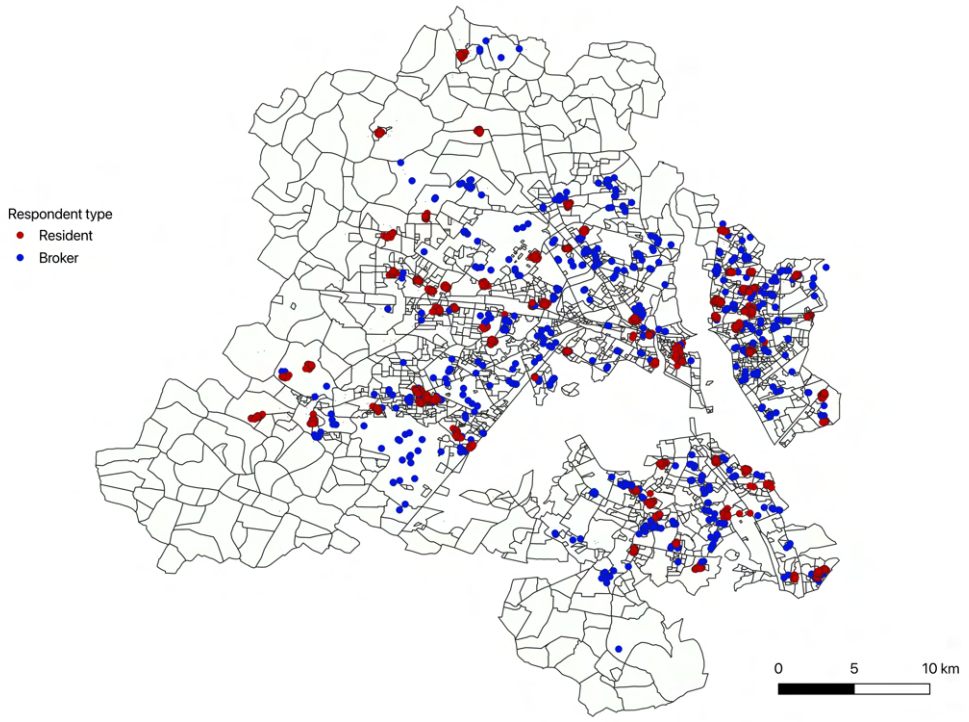


Figure B.1: Survey areas. Points show surveyed resident households and rental brokers. Of the 180 neighbourhoods containing a surveyed resident, 46 also contain a surveyed broker; 39.4 percent of resident respondents live in a neighbourhood with broker coverage.

B.2 Fieldwork, incentives, framing, and ethics

The study received ethics clearance from the LSE Research Ethics Committee (Ref: 193409, 24 April 2023, amendment approved 24 May 2025). Participation was voluntary and informed consent was obtained before each interview. Respondents received a household gift set worth approximately ₹200–250 for residents and ₹500 for brokers, and the response rate was approximately 98 percent

The surveys were introduced as a study of rental housing decisions in Delhi, not as a study of discrimination, and enumerators were trained to maintain that framing throughout. The sensitive modules are embedded among modules on search, satisfaction, amenities and tenancy experience. This reduces, though it cannot eliminate, the risk that respondents infer the object of measurement and adjust their answers, and it is one reason to treat the direct measures as informative rather than as pure expressions of what respondents think the researcher wants to hear.

B.3 Resident survey

The resident survey covers 3,290 households. For each respondent it records demographic characteristics, religion and caste, the religious composition of the current neighbourhood, housing and neighbourhood satisfaction, and recent housing-search experiences, together with the experimental modules below. Religion and caste are collected directly and used to construct the three identity groups that recur throughout: Hindu upper caste, Muslim, and Scheduled Caste. Table B.2 compares the sample composition with the 2011 Census.

B.4 Broker survey

The broker survey covers 619 rental brokers and is the paper's distinctive instrument.

What brokers are asked. For each broker the survey records reported landlord preferences in his area; his own expectations about tenant acceptability; the rules imposed by local resident welfare associations; identity-specific match probabilities; transaction volumes; who makes the final tenant decision; his experience with past tenancies; and his responses to the same vignette module that residents receive. Three of these do distinct work and should not be conflated. The *identity-specific match probabilities* - out of 100 representative properties in your service area, how many would accept a tenant of each type, give the access measure $p_{g,n}$. The *RWA rules* give an institution-level measure of rule-based exclusion. The *vignette responses* give a

Table B.2: Religious composition of survey respondents

Religion	Frequency	Per cent	Delhi (2011 Census)
Hindu	2,677	81.37	80.21
Muslim	495	15.05	12.78
Sikh	26	0.79	4.43
Jain	9	0.27	1.39
Christian	9	0.24	0.96
Buddhist	4	0.12	0.12
Other / not stated	71	2.16	0.10
Total	3,290	100.00	100.00

Notes. One observation per surveyed resident household. Census shares are the religious distribution of Delhi in the 2011 Census (Office of the Registrar General and Census Commissioner, India, 2011). The sample over-represents Muslim households relative to the city by construction: the sampling design stratifies on local religious composition in order to obtain adequate coverage of minority-concentrated neighbourhoods (Appendix B.1).

broker-specific identity effect directly comparable to the landlord's and the tenant's.

An important denominator. The access and RWA modules are served only to brokers who handle rental transactions; brokers who deal exclusively in sales are asked the corresponding questions about purchase rather than tenancy. The access measures therefore rest on **535 brokers**, not 619, and every figure derived from them carries that denominator.

Why elicitation rather than audit. First, coverage. A correspondence design must observe a listing before it can send an inquiry, so it cannot see properties withheld at the pre-screen. The broker report is a share of the local stock and therefore includes them. Second, spatial resolution. A broker serves a compact area, so his report is a neighbourhood-level measure, which is what the sorting analysis in Section 7 requires and what an applicant-side callback rate cannot supply. Third, incidence. Applicant-side measures capture the composition of refusal without locating it; asking the intermediary locates it in the intermediary.

Why brokers answer. The obvious objection is that a broker has every reason to understate his own exclusionary conduct. Three things cut against it. The survey is framed as a study of rental housing decisions (Appendix B.2), and brokers describe tenant screening as a service they provide to landlords rather than as discrimination they practise. The reported figures are, if anything, implausibly candid: brokers report Muslim access at under a third of that for upper-caste Hindus. And the elicited RWA measure can be benchmarked externally against the housing-society membership records of TwoCircles.net and Newslaundry (2024), which is done

in Section 4.4. Any residual bias from social desirability would attenuate the measured gaps, so the estimates are conservative in the direction that matters.

B.5 Spatial data

I assemble neighbourhood-level data on local amenities, including schools, hospitals, parks, religious institutions and prevailing rents.[5] These serve two purposes: they construct the amenity measures used in Section 7, and the location of religious institutions is used to classify neighbourhoods into area types, which underpins both the sampling strata and the area-type definitions used throughout.

B.6 Experimental modules

Both surveys share an instrument organised into three blocks. A *background* block records demographics, neighbourhood composition, housing satisfaction and exposure to neighbourhood disamenities. A *market-beliefs* block records landlord preferences, broker expectations, perceived matching probabilities by identity, and tenancy experiences. An *experiments* block contains the randomized modules: an applicant-evaluation vignette experiment (Section 4.1), a neighbourhood-choice experiment (Section 4.6), a pairwise tenant-choice module (Appendix F.9), and list experiments (Section 5). Full materials are in Appendix D.

Vignette experiment. Respondents are asked to imagine they are the landlord of a house they have advertised, and evaluate eight hypothetical applicant profiles. Profiles are randomized across three arms: a benchmark identity (A), the same profile with identity changed (B), and the identity change plus a tenant-reliability signal (C). The design is a factorial survey experiment in the sense of Auspurg and Hinz (2015). For each profile the respondent may (1) accept at the offered rent, (2) accept only at a higher rent, specifying the difference, (3) reject, or (4) wait x months and accept if no other application arrives. Comprehension-check accuracy exceeded 95 percent.

Three features of the fielded instrument matter. *The photograph is held constant across arms within a profile.* Photographs were AI-generated with prompt text instructing that the depicted person carry no religious markers, and were field-tested with survey staff to confirm that they did not read as identifiably Hindu or Muslim. Identity therefore varies through the name alone. *Arm C is not a constant signal*: it ranges from a landlord reference to police verification to a lease-length commitment to spousal employment, so I report its effect profile by profile and

[5] Amenity data were scraped from Google Maps by keyword; prevailing rents from MagicBricks.com.

distinguish cues that are pure reliability signals from those that also reveal ability to pay. *Three profiles carry identical names in arms A and B* and are therefore placebo transitions, reported in Appendix F.1.

Neighbourhood-choice experiment. Respondents choose among three hypothetical neighbourhoods varying in rent, commuting time, and either group composition or amenity bundle, in the spirit of conjoint analysis (Hainmueller et al., 2014). They are told the household income, that they can afford the rent in each option, and that they should choose the one that suits them best, so the exercise fixes availability by instruction and isolates preference from the access constraints that bind in the field. The game was repeated four times; comprehension accuracy exceeded 98 percent.

List experiments. To measure sensitive items free of social-desirability pressure, both surveys include list (item-count) experiments (Imai, 2011; Blair and Imai, 2012). Respondents report only the *number* of statements on a list that apply to them; a randomly assigned treatment group sees one additional sensitive item. Five lists were fielded: two behavioural (whether a tenant has faced identity-based discrimination; whether a landlord has declined a tenant on caste or religious grounds) and three attitudinal. The three attitudinal lists share a single control arm.

B.7 Sample construction and balance

The raw file contains 3,909 respondents: 3,290 residents and 619 brokers. Forty-one have no usable vignette data, so the experimental analysis rests on 3,868 respondents, which is the figure that recurs throughout. Table B.3 traces every reported N to this starting point.

Table B.3: Survey observations by respondent type

Respondent type	Respondents	Vignette obs.	Vignette resp.	Choice obs.	Pair obs.
Tenant only	1,015	8,120	1,015	4,039	5,055
Landlord only	256	2,048	256	1,014	1,279
Tenant and landlord	7	56	7	28	35
Broker	619	4,920	615	2,416	3,056
Resident, neither module	2,012	15,800	1,975	7,771	9,815
Total	**3,909**	**30,944**	**3,868**	**15,268**	**19,240**

Notes: The table reports the number of respondents and experimental observations by respondent type. "Vignette obs." denotes the total number of vignette-level observations, while "Vignette resp." denotes the number of respondents contributing vignette observations. "Choice obs." and "Pair obs." report the corresponding numbers of choice-level and pair-level observations.

One feature of the instrument explains why the respondent-type columns in the results tables do not sum to the pooled column. The housing-category question allows multiple selections, and

the tenant module is served to respondents who report living in rented accommodation while the landlord module is served to those who report letting a residential property. A respondent may report both, and a respondent who reports only owner-occupation receives neither. Tenant and landlord are therefore overlapping categories rather than a partition, and Table B.3 reports the overlap explicitly.

Table B.4: Randomisation balance across vignette arms

	Arm 1		Arm 2		Arm 3		Total	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Panel A: Respondent characteristics								
Female respondent	0.496	0.500	0.486	0.500	0.443	0.497	0.474	0.499
Hindu upper caste	0.562	0.496	0.566	0.496	0.570	0.495	0.566	0.496
Muslim	0.182	0.386	0.195	0.396	0.172	0.378	0.182	0.386
Scheduled Caste	0.256	0.437	0.240	0.427	0.258	0.437	0.251	0.434
Conflict-exposed neighbourhood	0.128	0.335	0.118	0.323	0.118	0.323	0.122	0.327
Tenant	0.255	0.436	0.278	0.448	0.261	0.439	0.264	0.441
Landlord	0.074	0.262	0.068	0.252	0.062	0.242	0.068	0.252
Panel B: Tenancy characteristics								
Sample: respondents who completed the tenant module								
Monthly rent (INR)	6038.59	3687.06	5924.56	3627.08	6308.52	4239.19	6097.02	3873.73
Proportion of income on rent (cetegory)	2.56	1.51	2.66	1.57	2.76	1.63	2.66	1.57
Months in current accommodation	80.14	99.85	81.76	106.38	86.30	93.58	82.82	99.88
Days searching for current house	45.17	166.17	43.01	75.57	51.41	163.44	46.79	143.19
Houses viewed before current one	2.98	3.28	2.92	4.17	2.51	2.83	2.79	3.47

Notes: The table reports means and standard deviations by assigned vignette arm. Panel A uses one observation per respondent. Panel B is restricted to respondents who completed the tenant module. Arm assignment is generated by a uniform random draw on the survey device: respondents are assigned to Arm 1 if the draw is at or below 0.33, Arm 2 if the draw is above 0.33 and at or below 0.66, and Arm 3 if the draw is above 0.66. "Conflict-exposed neighbourhood" indicates whether the respondent's neighbourhood is classified as conflict exposed. Monthly rent is measured in Indian rupees and excludes other housing-related charges.

B.8 Balance and arm assignment

Arm is assigned by a uniform draw made on the device, with cutoffs at 0.33 and 0.66 rather than thirds, so the realised split is 32.5, 31.5 and 36.1 per cent. The draw has mean 0.508 across 3,868 respondents.

Table B.5: Summary statistics for survey respondents

Panel A: Resident characteristics by identity group

	Hindu upper caste		Muslim		Scheduled Caste		Total	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Female respondent	0.57	0.50	0.47	0.50	0.63	0.48	0.57	0.50
Monthly rent (INR)	6386.49	3907.90	6747.64	4867.74	5222.98	3050.33	6244.57	4114.39
Share of income spent on rent	2.84	1.63	2.62	1.59	2.61	1.56	2.72	1.60
Imputed monthly income (band midpoint)	45237.35	49389.59	56796.92	72719.48	42602.45	48639.50	48369.52	58010.30
Months in current rented accommodation	86.16	109.92	70.70	91.09	78.36	91.88	78.90	99.50
Days spent searching for current house	54.46	185.33	60.29	196.67	30.09	77.17	49.99	168.07
Houses viewed before selecting current one	3.12	4.13	2.74	2.96	2.93	3.05	2.96	3.57
Round-trip commute (minutes per day)	66.08	191.34	69.00	372.41	68.10	253.47	67.34	263.14
Conflict-exposed neighbourhood	0.15	0.36	0.22	0.42	0.07	0.26	0.14	0.35

Panel B: Characteristics by respondent type

	Tenant only		Landlord only		Tenant and landlord		Broker		Resident, neither module		Total	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age group	2.56	1.23	3.15	1.61	2.57	0.98	3.83	1.19	3.14	1.49	3.10	1.45
Muslim	0.23	0.42	0.15	0.36	0.14	0.38	0.08	0.27	0.11	0.31	0.14	0.35
Conflict-exposed neighbourhood	0.17	0.38	0.22	0.42	0.57	0.53	0.02	0.15	0.12	0.32	0.12	0.33

Panel C: Broker characteristics

	Mean	SD	Median	N
Years working in the property business	14.77	10.33	14.00	613
Handles rental transactions	0.86	0.34	1.00	619
Handles property sales	0.90	0.29	1.00	619
Successful matches in an average month	7.28	8.13	5.00	529

Panel D: Neighbourhood characteristics by sampling stratum

	Only Mosque		Mixed		Only Temples		Neither		Total	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Number of temples	0.00	0.00	5.80	8.49	2.36	1.43	0.00	0.00	2.31	5.15
Number of mosques	2.43	1.95	4.09	6.99	0.00	0.00	0.00	0.00	1.66	4.20
Temple share	0.00	0.00	0.62	0.18	1.00	0.00	0.00	0.00	0.91	0.69
Respondents per sampled neighbourhood	10.43	15.84	9.12	13.83	7.79	11.66	7.88	12.00	8.69	13.24

Notes: Panel A reports summary statistics for respondents in the resident survey, separately by identity group. Rent, housing-search, and tenancy variables are reported for respondents for whom the relevant variables are observed. Panel B reports respondent characteristics by survey module. A respondent may be classified as tenant only, landlord only, both tenant and landlord, broker, or a resident who completed neither the tenant nor landlord module. Panel C reports characteristics of surveyed brokers. The access and RWA-rule modules were administered only to brokers handling rental transactions, so the number of observations for variables from these modules is smaller than the full broker sample. Panel D reports neighbourhood-level characteristics by the four sampling strata used in the resident survey, defined by the presence of temples and mosques. Temple and mosque counts are averaged across sampled neighbourhoods within each stratum, and respondents per sampled neighbourhood reports the mean number of surveyed respondents per neighbourhood. "Conflict-exposed neighbourhood" indicates whether the respondent's neighbourhood is classified as conflict exposed. Monthly rent and imputed monthly income are measured in Indian rupees.

Table B.6: Summary statistics by identity group

	Hindu upper caste	Muslim	Scheduled Caste
Age band (1–8)	3.13	2.66	2.87
Female	0.57	0.47	0.63
Monthly rent (₹)	6,386	6,748	5,223
Rent-to-income band (1–8)	2.84	2.62	2.61
Imputed monthly income (₹)	45,237	56,797	42,602
Months in current accommodation	86.2	70.7	78.4
Days spent searching	54.5	60.3	30.1
Houses viewed before choosing	3.12	2.74	2.93
Round-trip commute (minutes)	66.1	69.0	68.1
In a conflict-exposed area	0.15	0.22	0.07
Respondents	1,276	495	708

Notes. Resident respondents. Rent, the rent-to-income band, tenure, search and viewing variables are served only inside the tenant module, so their denominators are smaller than the respondent count. Imputed income is rent divided by the midpoint of the reported rent-to-income band and is undefined for the 31 respondents who report that someone else pays. Education is omitted: it is missing for about 94 per cent of respondents. Produced by `03_descriptives.do`.

Table B.7: Balance across arms

	Arm A	Arm B	Arm C	p
Age band (1–8)	3.17	3.04	3.08	0.064
Female	0.50	0.49	0.44	0.014
Hindu upper caste	0.56	0.57	0.57	0.926
Muslim	0.18	0.19	0.17	0.425
Scheduled Caste	0.26	0.24	0.26	0.603
Conflict-exposed area	0.13	0.12	0.12	0.671
Completed the tenant module	0.25	0.28	0.26	0.420
Completed the landlord module	0.07	0.07	0.06	0.488
Employment status		—		0.795
Education level		—		0.520
Monthly rent		—		0.401
Rent-to-income band		—		0.277
Respondents	1,255	1,218	1,395	

Notes. Each p-value is from a one-way analysis of variance on that variable's own sample, so a variable with heavy item non-response does not reduce the sample for the others. Means are suppressed for the four variables whose coding is categorical or whose denominator is the tenant module only. Education is missing for about 94 per cent of respondents and rent for 74 per cent. A joint multinomial logit of arm on age, sex, identity group, conflict exposure and module completion gives $\chi^2(14) = 17.98$, $p = 0.208$, on the 2,971 respondents with all five observed.

Table B.8: The estimate is unchanged by covariate adjustment

	No controls	Profile	Profile, same sample	Plus respondent characteristics	Plus area type
Index coefficient β_1	0.665	0.679	0.694	0.707	0.709
	(0.045)	(0.046)	(0.047)	(0.047)	(0.047)
Marginal effect on acceptance	−0.224	−0.224	−0.228	−0.230	−0.230
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Observations	7,736	7,736	7,646	7,646	7,646

Notes. Ordered probit, standard errors clustered on the respondent. In an ordered probit the coefficients are identified only up to the scale of the error, so adding covariates that predict the outcome inflates them mechanically whether or not assignment is balanced: the applicant-profile indicator alone carries a coefficient near 0.39. The marginal effect does not rescale and is the right comparison. It moves by 0.006 across the five columns, about four tenths of a standard error, and two thirds of that movement comes from the 45 respondents dropped for missing covariates rather than from the covariates: column 3 repeats column 2 on column 4's sample. Education is excluded throughout.

C Anatomy of a rental transaction

Brokers, community networks and online portals are the main avenues for rental housing search in Delhi and in most major Indian cities, with over 90 per cent of the tenants in the survey reporting having used one or more of these. The remainder rely on door-to-door searches for 'To-let' signs. Brokers often post and manage advertisements on housing portals at the behest of landlords; some portals market themselves specifically on putting tenants in direct contact with owners and saving both sides the brokerage.

Brokers handle the administrative documents and pre-screen potential tenants before taking the shortlisted applicants to the landlords. This was confirmed in focus group discussions and informal conversations with brokers in Delhi. Landlords specify the type of tenant they want and brokers screen accordingly. Identity, occupation, family composition, dietary preferences and similar attributes are established pre-viewing, in the first meeting with the broker.

After the pre-screening the broker arranges the viewing, usually taking the prospective tenant to the property himself. He typically charges half a month's rent from the tenant and the same from the landlord once the deal is formalised, though this is negotiable; there is normally no fee for showing a property, and brokers service an area of roughly 1.5 to 2 kilometres around their shop.

Post-viewing there may be negotiation over rent and deposit, in which the broker generally conducts the first round. Landlords specify an ideal rent and the amount they would be willing to concede, and applications may be rejected by the broker if they fall below the landlord's expectations. In some cases the landlord joins the negotiation at the broker's request.

Once rent and deposit are settled the applicant pays a token amount to hold the property pending checks such as police verification. The parties then sign an eleven-month leave-and-licence agreement, renewable before expiry and generally with some increase in rent, although a majority of agreements end up informal.

The feature that matters for measurement is that pre-screening precedes the viewing. Exclusion at the pre-screen is invisible to the applicant, who never learns which properties existed.

Table C.1: The broker's side of the market

	Mean	25th	Median	75th
Potential tenants per month	33.0	12	20	40
Completed matches per month	7.3	2	5	10
Applicants per completed match	6.1	2.0	4.2	7.5
Share of applicants matched	0.31	0.13	0.22	0.43
Years in the property business	14.8	5	14	20
Brokers handling rentals			535	

Notes. The applicants-per-match ratio is winsorised at the first and ninety-ninth percentiles; nine brokers reporting more matches than applicants, or volumes above 500 applicants or 200 matches a month, are excluded. The raw applicant count has a maximum of 300,000 and a median of 20, so the mean is not informative and the median is used in the text. A broker is paid half a month's rent by each side on a completed tenancy. Of brokers handling rentals, 98.3 per cent name the landlord as the party who decides.

D Survey materials

This appendix presents the experimental materials used in the survey: the landlord applicant-evaluation vignettes, the landlord pairwise tenant-choice module, and the tenant neighbourhood-choice tasks.

D.1 Landlord Game 1: Applicant Profiles

Respondents were shown eight applicant profiles and asked to evaluate each as if they were the landlord of an advertised property. Arms A and B differ only in the applicant's name; arm C adds a reliability or verification cue to arm B. The photograph is identical across the three arms within each profile. Photographs were AI-generated with prompt text specifying that the depicted person should carry no religious markers, and were field-tested with survey staff to confirm that they did not read as identifiably Hindu or Muslim.

Profiles 2, 5 and 7 (Sanjay Kumar, Aakriti Patel and Akbar Khan) carry *identical* names in arms A and B and are therefore placebo transitions; they are analysed in Appendix F.1. Profile 6 (Gulnaz Bano → Pooja Bhatia) runs the identity change in reverse (Appendix F.2). Profile 3 (Shakeel Ahmed → Soham Manjhi) is the Muslim-to-Scheduled-Caste contrast (Appendix F.8), and profile 4 (Arun Kumar → Atul Pandey) is a within-Hindu caste-marked surname contrast.

Table D.1: Landlord Game 1: applicant profiles 1–4

Photo	Arm A	Arm B	Arm C
	**Name:** Aarti Bhasin **Occupation:** Doctor **Rent:** ₹22,000 **Family:** Married, two children **Languages:** Hindi and English	**Name:** Atiya Rahman **Occupation:** Doctor **Rent:** ₹22,000 **Family:** Married, two children **Languages:** Hindi and English	**Name:** Atiya Rahman **Occupation:** Doctor **Rent:** ₹22,000 **Reference:** "Ideal tenant" according to previous landlord **Family:** Married, two children **Languages:** Hindi and English
	**Name:** Sanjay Kumar **Occupation:** Store Manager **Monthly Salary:** ₹45,000 **Rent:** ₹13,000 **Family:** Single **Language:** Hindi	**Name:** Sanjay Kumar **Occupation:** Store Manager **Monthly Salary:** ₹45,000 **Rent:** ₹13,000 **Family:** Single **Language:** Hindi	**Name:** Sanjay Kumar **Occupation:** Store Manager **Monthly Salary:** ₹45,000 **Rent:** ₹13,000 **Contract Duration:** Willing to sign a two-year contract **Family:** Single **Language:** Hindi
	**Name:** Shakeel Ahmed **Occupation:** Software Engineer **Monthly Salary:** ₹1,10,000 **Rent:** ₹24,000 **Family:** Single **Languages:** Hindi and English	**Name:** Soham Manjhi **Occupation:** Software Engineer **Monthly Salary:** ₹1,10,000 **Rent:** ₹24,000 **Family:** Single **Languages:** Hindi and English	**Name:** Soham Manjhi **Occupation:** Software Engineer **Monthly Salary:** ₹1,10,000 **Rent:** ₹24,000 **Reference:** Police verification successful **Family:** Single **Languages:** Hindi and English
	**Name:** Arun Kumar **Occupation:** School Teacher **Rent:** ₹18,000 **Family:** Married, two children **Languages:** Proficient in Hindi and English	**Name:** Atul Pandey **Occupation:** School Teacher **Rent:** ₹18,000 **Family:** Married, two children **Languages:** Proficient in Hindi and English	**Name:** Atul Pandey **Occupation:** School Teacher **Rent:** ₹18,000 **Reference:** According to previous landlord, rent payments were very rarely delayed **Family:** Married, two children **Languages:** Proficient in Hindi and English

Table D.2: Landlord Game 1: applicant profiles 5–8

Photo	Arm A	Arm B	Arm C
	**Name:** Aakriti Patel **Occupation:** Marketing Executive **Rent:** ₹16,000 **Family:** Divorced, one child, one pet **Languages:** Hindi, Gujarati and English	**Name:** Aakriti Patel **Occupation:** Marketing Executive **Rent:** ₹16,000 **Family:** Divorced, one child, one pet **Languages:** Hindi, Gujarati and English	**Name:** Aakriti Patel **Occupation:** Marketing Executive **Rent:** ₹16,000 **Reference:** Recently moved to the city **Family:** Divorced, one child, one pet **Languages:** Hindi, Gujarati and English
	**Name:** Gulnaz Bano **Occupation:** Police Constable **Monthly Salary:** ₹35,000 **Rent:** ₹11,000 **Family:** Single **Languages:** Hindi and English	**Name:** Pooja Bhatia **Occupation:** Police Constable **Monthly Salary:** ₹35,000 **Rent:** ₹11,000 **Family:** Single **Languages:** Hindi and English	**Name:** Pooja Bhatia **Occupation:** Police Constable **Monthly Salary:** ₹35,000 **Rent:** ₹11,000 **Reference:** Police verification completed **Family:** Single **Languages:** Hindi and English
	**Name:** Akbar Khan **Occupation:** Banker **Monthly Salary:** ₹1,45,000 **Rent:** ₹40,000 **Family:** Married, two children **Languages:** Hindi, Urdu and English	**Name:** Akbar Khan **Occupation:** Banker **Monthly Salary:** ₹1,45,000 **Rent:** ₹40,000 **Family:** Married, two children **Languages:** Hindi, Urdu and English	**Name:** Akbar Khan **Occupation:** Banker **Monthly Salary:** ₹1,45,000 **Rent:** ₹40,000 **Reference:** According to previous landlord, never delayed rent payments **Family:** Married, two children **Languages:** Hindi, Urdu and English
	**Name:** Rakesh Pandey **Occupation:** Researcher **Rent:** ₹10,000 **Family:** Married, one child **Language:** Hindi	**Name:** Arshad Alam **Occupation:** Researcher **Rent:** ₹10,000 **Family:** Married, one child **Language:** Hindi	**Name:** Arshad Alam **Occupation:** Researcher **Rent:** ₹10,000 **Reference:** Wife works as Head of Human Resources at a multinational company **Family:** Married, one child **Language:** Hindi

D.2 Landlord Game 2: Pairwise Tenant Choice

Respondents were presented with pairs of prospective tenants and asked to choose between Tenant A and Tenant B. Results are reported in Appendix F.9.

Table D.3: Landlord Game 2: tenant pair 1

Tenant A	**Tenant B**
	
Name: Jayant Maheshwari **Occupation:** Student **Monthly Rent:** ₹12,000 **Family:** Unmarried **Contract Duration:** 12 months	**Name:** Aftab Hussain **Occupation:** Student **Monthly Rent:** ₹14,000 **Family:** Unmarried **Contract Duration:** 12 months

Table D.4: Landlord Game 2: tenant pair 2

Tenant A	**Tenant B**
	
Name: Sanjay Kumar **Occupation:** Store Manager **Monthly Rent:** ₹25,000 **Family:** Unmarried **Contract Duration:** 18 months	**Name:** Jayant Pandey **Occupation:** Businessman **Monthly Rent:** ₹25,000 **Family:** Unmarried **Contract Duration:** 18 months

Table D.5: Landlord Game 2: tenant pair 3

Tenant A	Tenant B
	
Name: Imran Khan **Occupation:** Doctor **Monthly Rent:** ₹22,000 **Family:** Married, one child **Contract Duration:** 24 months	**Name:** Akash Adhikari **Occupation:** Software Engineer **Monthly Rent:** ₹20,000 **Family:** Married, two children **Contract Duration:** 24 months

Table D.6: Landlord Game 2: tenant pair 4

Tenant A	Tenant B
	
Name: Aarti Bhasin **Occupation:** Doctor **Monthly Rent:** ₹19,000 **Family:** Unmarried **Contract Duration:** 12 months	**Name:** Putul Gupta **Occupation:** Doctor **Monthly Rent:** ₹19,000 **Family:** Married, one child **Contract Duration:** 12 months

Table D.7: Landlord Game 2: tenant pair 5

Tenant A	Tenant B
	
Name: Sameer Sharma **Occupation:** Banker **Monthly Rent:** ₹35,000 **Family:** Unmarried **Tenancy History:** No previous record **Contract Duration:** 12 months	**Name:** Akbar Khan **Occupation:** Banker **Monthly Rent:** ₹35,000 **Family:** Unmarried **Tenancy History:** Good reference from previous landlord **Contract Duration:** 12 months

D.3 Tenant Neighbourhood-Choice Games

Respondents were presented with hypothetical residential areas varying in rent, commute time, and either neighbourhood demographics or available amenities, and were told they could afford the rent in every option.

Table D.8: Tenant game: neighbourhood demographics I

Area	Rent (₹)	Commute (min)	Neighbour demographics
A	15,000	20	Mostly Upper-Caste Hindu
B	14,000	35	Mixed – Hindu, Muslim, Christian
C	13,000	25	Mostly Muslim

Table D.9: Tenant game: neighbourhood demographics II

Area	Rent (₹)	Commute (min)	Neighbour demographics
A	16,000	15	Mostly Muslim
B	15,000	30	Mixed – Hindu, Muslim, Christian
C	14,500	20	Mostly SC

Table D.10: Tenant game: neighbourhood amenities I

Area	**Rent (₹)**	**Commute (min)**	**Amenities available in the area**
A	25,000	40	Schools, parks, mosques, metro connectivity
B	28,000	30	Schools, metro connectivity, mosques, temples
C	22,500	35	Metro connectivity, large hospital, parks

Table D.11: Tenant game: neighbourhood amenities II

Area	**Rent (₹)**	**Commute (min)**	**Amenities available in the area**
A	15,000	20	Metro connectivity, parks, good schools, temples
B	14,000	35	Markets, lake, religious institutions, stadium
C	13,000	25	Affordable food options, historical monuments

E A queuing model of the refusal margin

This appendix sets out the model behind Implication 1 and states what the null in Table 12 does and does not rule out.

E.1 Setup

A landlord has one unit to let at a posted rent r. In the letting period he receives N applicants, and applicant i delivers him a flow payoff

$$u_i \;=\; q_i \;-\; d\,\mathbf{1}[g_i = M], \tag{8}$$

where q_i is the applicant's quality as the landlord assesses it, drawn independently from a continuous distribution F with support on $[\underline{q}, \bar{q}]$, g_i is the applicant's identity, and $d \geq 0$ is the landlord's distaste for a Muslim tenant. Nothing below requires d to be a taste: it may equally be a belief about an unobserved correlate of identity, or an anticipation of what neighbours will accept. The model is about the margin on which d is expressed, not about where d comes from.

The landlord has three responses to each applicant. He may accept at r; he may demand a markup m, which the applicant accepts with probability $\pi(m)$, decreasing, and which costs the landlord $c > 0$ to negotiate; or he may refuse and continue with the remaining applicants. Rent increases between contracts are capped, so $m \leq \bar{m}$.

E.2 The option value of refusing

Let V_k denote the landlord's value with k applicants left to consider. Refusing applicant i yields V_{k-1}, so the landlord accepts i at the posted rent when $u_i \geq V_{k-1}$ and otherwise refuses or prices. Because V_{k-1} is the expected maximum of $k-1$ draws, it is increasing in k and approaches $\bar{q}$ as k grows:

$$V_{k-1} \;=\; \mathbb{E}\Big[\max_{j \leq k-1} u_j\Big] \;\longrightarrow\; \bar{q} \quad \text{as } k \to \infty. \tag{9}$$

The landlord prices rather than refuses only if doing so is worth more than continuing, that is only if

$$\pi(m)\,\big(u_i + m\big) + \big(1 - \pi(m)\big)V_{k-1} - c \;>\; V_{k-1}, \tag{10}$$

which rearranges to $\pi(m)\,(u_i + m - V_{k-1}) > c$. Two things follow.

Implication 2. *The price margin is used only when the queue is short. As N rises, V_{k-1} rises by (9), the left-hand side of (10) falls, and beyond a threshold N^* no markup within the cap $\bar{m}$ satisfies it. For $N > N^*$ the landlord refuses rather than prices, so the rationing share $\Lambda \to 1$.*

Implication 3. *Information that raises an applicant's standing without changing his rank does not help him. A reference raises q_i by δ. It changes the outcome only if it lifts u_i above V_{k-1}, which requires $V_{k-1} - \delta < u_i < V_{k-1}$. The probability of landing in that window falls as N rises and the distribution of $\max_j u_j$ concentrates near $\bar{q}$. A signal about ability to pay is different: it enters through $\pi(m)$ rather than through q_i, raising the applicant's willingness to meet a markup and so relaxing* (10) *directly.*

Implication 3 is the distinction the arm-C design tests, and it predicts exactly the asymmetry in Table 3: a reliability cue should do nothing while an income cue should move price. Section 5 reports that the data are consistent with this within profiles but that the difference between the two cue types is not itself significant.

E.3 What the tightness null does and does not rule out

Implication 2 is a comparative static in N, and Table 12 finds no cross-sectional gradient of exclusion in the broker-reported queue length. Three readings are consistent with the model.

First, and most simply, $N > N^*$ everywhere in this market. The median broker reports about four applicants per completed let, and if that is already past the threshold then $\Lambda \approx 1$ for every broker and there is no variation in the margin left to explain. The measured Λ of 0.97 to 0.99, and its stability across respondent types whose penalties differ fourfold, is what this reading predicts.

Second, the measure may be noisy in a way that attenuates the gradient. Queue length is self-reported at the broker rather than at the property, on a question whose raw answers range from 0 to 300,000 per month, and brokers may differ in what they count as an applicant.

Third, the model's N is the number of applicants for a given unit, while the data record the number reaching a broker across his book. The two coincide only if the broker's portfolio is homogeneous.

The level of queueing is therefore consistent with the mechanism and the cross-sectional gradient does not support it. A design that varied N within respondent - asking the same landlord to choose with two applicants and then with six, would separate the readings, and is the natural follow-up.

F Robustness in full

Table F.1: The arm-C effect, by profile

Profile Cue	H to M Reference	SC to SC Contract	M to SC Police	SC to UC Delayed pay	UC to UC Moved	M to H Police	M to M Payments on time	H to M Income
Identity β_1	−0.188***	0.005	0.140***	0.009	−0.001	0.106***	0.016	−0.229***
	(0.016)	(0.020)	(0.020)	(0.012)	(0.020)	(0.020)	(0.020)	(0.018)
Information β_2	0.002	0.051**	0.046**	−0.043***	0.016	0.052**	−0.020	−0.017
	(0.018)	(0.020)	(0.020)	(0.012)	(0.019)	(0.020)	(0.019)	(0.019)

Notes. Binary acceptance; 3,868 observations per column; standard errors clustered on the respondent. Only profiles 1 and 8 change identity across arms; profiles 3 and 4 change caste rather than religion. The arm-C cue differs: a previous landlord's reference (1, 4, 7), police verification (3, 6), a two-year contract commitment (2), a note that the applicant recently moved to the city (5), and a signal that the applicant's spouse is a senior corporate employee (8). Profile 4's cue is arguably negative rather than reassuring and should not be pooled with the reliability cues. Interacting the cue type with arm C gives $p = 0.34$ on the ladder, $p = 0.63$ on the probability of a markup and $p = 0.68$ on its size, so the contrast between reliability and income cues is not significant.

F.1 Placebo transitions

The vignette instrument contains three profiles in which arms A and B are identical in every respect, including the applicant's name and the offered rent: Sanjay Kumar, Akriti Patel and Akbar Khan. For these the A $\rightarrow$ B contrast changes nothing, so β_1 is zero by construction. It is. The pooled placebo effect is bounded between −2.0 and +3.4 percentage points with 95 percent confidence, and the joint test that the three placebo penalties are zero gives $F = 0.719$ ($p = 0.540$). The measured effect on the real transitions is 21 to 24 points, an order of magnitude outside that interval.

This is the strongest internal validation the design offers. It rules out the possibility that the measured penalty reflects an arm effect, a position effect, or any feature of the instrument other than the applicant's name.

F.2 The reverse transition

One profile runs the identity change in the opposite direction, replacing a Muslim-named applicant (Gulnaz Bano, a single police constable) with an otherwise identical Hindu-named one (Pooja Bhatia). Acceptance *rises* by 10.6 percentage points. The design therefore passes an internal-consistency check, and the estimate supports the monotonicity condition required by the Lee (2009) bounds.

The forward and reverse effects are not exactly symmetric: pooling the three profiles and summing gives +0.127 ($p < 0.01$), where symmetry would give zero. The reverse profile is a single woman and the two forward profiles are married people, so the asymmetry may reflect

Table F.2: Placebo tests for the identity effect

	Placebo transitions				Hindu → Muslim transitions	
	Sanjay Kumar	**Akriti Patel**	**Akbar Khan**	**Pooled**	**H to M woman**	**H to M man**
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ordered probit — reluctance ladder						
Identity effect	-0.014	0.002	-0.041	-0.017	0.708***	0.654***
	(0.050)	(0.050)	(0.051)	(0.034)	(0.060)	(0.053)
Information effect	-0.128***	-0.040	0.043	-0.042	-0.005	0.063
	(0.049)	(0.049)	(0.050)	(0.033)	(0.051)	(0.049)
Observations	3,868	3,868	3,868	11,604	3,868	3,868
Panel B: Binary acceptance						
Identity effect	0.0055	-0.0008	0.0160	0.0069	-0.1880***	-0.2290***
	(0.0200)	(0.0199)	(0.0196)	(0.0135)	(0.0157)	(0.0179)
Information effect	0.0510***	0.0160	-0.0204	0.0155	0.0023	-0.0169
	(0.0196)	(0.0195)	(0.0191)	(0.0133)	(0.0179)	(0.0194)
Observations	3,868	3,868	3,868	11,604	3,868	3,868
Joint test of placebo penalties	$F = 0.719,\ p = 0.540$					
Pooled placebo 95% CI				$[-0.0196,\ 0.0335]$		

Notes: Columns (1)–(3) report the three clean placebo transitions, for which the profile shown in the identity-treatment arm is identical to the profile shown in the control arm in all respects, including name and offered rent. For Sanjay Kumar, arm C information was that he has sometimes paid rent with a delay. Column (4) pools the three placebo transitions. Columns (5) and (6) report the two clean Hindu → Muslim transitions for comparison.

Panel A estimates an ordered probit using the three-category reluctance ladder. Higher values indicate greater reluctance, so a positive coefficient represents a penalty. Panel B estimates a linear probability model for binary acceptance, coded one for acceptance at the offered or a higher rent and zero for waiting or rejection; a negative coefficient therefore represents a penalty.

The identity effect should be zero in columns (1)–(4) by construction. The joint test in Panel B tests whether the identity penalties for the three individual placebo profiles are jointly equal to zero. The reported 95% confidence interval is for the pooled binary placebo estimate. Standard errors are clustered at the respondent level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

family status rather than the direction of the identity change. I note it rather than claiming symmetry.

F.3 Binary and alternative codings

The ladder of (2) is the preferred specification because it uses every response category. For comparability with the correspondence literature I also report the binary specification

$$\text{Accept}_i \;=\; \beta_0 \;+\; \beta_1\,\mathbf{1}\{B_i \text{ or } C_i\} \;+\; \beta_2\,\mathbf{1}\{C_i\} \;+\; \varepsilon_i, \tag{11}$$

where acceptance pools accept-at-offered-rent and accept-at-a-higher-rent against refusal. Pooling the two clean profiles and all respondents, the identity effect is -0.208 (s.e. 0.014) and the information effect -0.007 (s.e. 0.016). Restricting to outright accepts and outright rejects, dropping the two intermediate responses, gives -0.212 (s.e. 0.014). The result is insensitive to the coding.

Table F.3: Identity penalties by respondent type and applicant profile

	Pooled	**Tenant**	**Landlord**	**Broker**
Panel A: H to M woman				
Identity effect β_1	-0.188***	-0.082***	-0.242***	-0.406***
	(0.016)	(0.029)	(0.062)	(0.039)
Information effect β_2	0.002	0.010	-0.018	-0.048
	(0.018)	(0.031)	(0.074)	(0.049)
Observations	3,868	1,022	263	615
Panel B: H to M man				
Identity effect β_1	-0.229***	-0.085**	-0.299***	-0.483***
	(0.018)	(0.035)	(0.068)	(0.043)
Information effect β_2	-0.017	-0.054	0.022	-0.038
	(0.019)	(0.036)	(0.077)	(0.048)
Observations	3,868	1,022	263	615
Broker − landlord, identity effect		−0.197***	(0.051)	

Notes: The table reports linear probability models of binary acceptance for the two clean Hindu → Muslim profile transitions. Panel A corresponds to the H to M-woman profile and Panel B to the H to M-man profile. The pooled column uses all respondents; the remaining columns restrict the sample to tenants, landlords, and brokers, respectively. Tenant and landlord categories are not mutually exclusive. The identity effect, β_1, captures the change from the Hindu-named to the otherwise comparable Muslim-named applicant. The information effect, β_2, captures the additional arm-C signal. All respondents evaluate the same hypothetical applicants, so differences across columns reflect respondent type rather than differences in the vignette. The final row reports the broker–landlord difference in the identity effect. Standard errors are clustered at the respondent level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.4: The identity effect under alternative outcome codings

	Ladder (1)	**Binary** (2)	**Strict binary** (3)	**Binary, profile FE** (4)
Identity effect β_1	0.665***	-0.208***	-0.212***	-0.208***
	(0.045)	(0.014)	(0.014)	(0.014)
Information effect β_2	0.031	-0.007	-0.011	-0.007
	(0.043)	(0.016)	(0.017)	(0.016)
Profile fixed effects	No	No	No	Yes
Observations	7,736	7,736	7,484	7,736

Notes: The table tests the robustness of the estimated identity effect to alternative codings of the vignette outcome, pooling the two clean Hindu → Muslim profile transitions. Column (1) estimates an ordered probit using the three-category reluctance ladder; higher values denote greater reluctance, so a positive coefficient represents a penalty. Column (2) uses binary acceptance, coded one for acceptance at the offered or a higher rent and zero for waiting or rejection. Column (3) uses a stricter binary outcome, dropping the intermediate responses and comparing outright acceptance with outright rejection. Column (4) repeats the binary specification in column (2) while including profile fixed effects. The identity effect, β_1, captures the effect of changing the applicant from a Hindu to an otherwise comparable Muslim identity. The information effect, β_2, captures the additional signal introduced in arm C. Standard errors, clustered at the respondent level, are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F.4 Dropping the ordering assumption

The ladder imposes that acceptance at a higher rent lies between acceptance at the offered rent and refusal on a single latent scale. The interior cutpoints are close together, so the ordering of the two interior states is not strongly supported by the data, and a reader may reasonably doubt that quoting a markup is "less reluctant" than refusing. I therefore re-estimate the decomposition from a multinomial logit over the three unordered outcomes of (1), which imposes no ordering at all.

F.5 Market tightness

The broker survey measures whether exclusion rises with the number of competing applicants directly: a median of 4 potential tenants per completed let, a mean of 6.0, an interquartile range of 2 to 7.5, and an implied match rate of 0.31.

The gradient, however, is absent. Regressing broker-reported Muslim access on standardised local tightness gives −0.49 (s.e. 1.42); on the Muslim–upper-caste gap, −1.30 (s.e. 1.83); on RWA prohibition of Muslim tenants, +0.027 (s.e. 0.020). The lower-caste Hindu gap, which serves as a placebo, gives +0.53 (s.e. 0.70). Nothing moves in either direction. Nor does the vignette penalty vary with local tightness among broker respondents.

Three caveats bear on how much weight the null can carry. Tightness is self-reported at the level of the broker, not the property, so it is a proxy for pool size rather than pool size.

Table F.5: Multinomial logit: no ordering imposed

	H to M woman (1)	**H to M man** (2)
Out of acceptance at offered rent, Δ_1	-0.220***	-0.234***
	(0.019)	(0.019)
Onto the price margin, Δ_2	0.006	-0.008
	(0.008)	(0.008)
Onto the quantity margin, Δ_3	0.213***	0.242***
	(0.018)	(0.018)
Rationing share, Λ	0.971***	1.034***
	(0.032)	(0.034)
Observations	3,868	3,868
Bootstrap replications	1,000	1,000

Notes: The table reports marginal effects from multinomial logit models that impose no ordering on the three possible vignette responses. Column (1) reports results for the H to M-woman profile and column (2) for the H to M-man profile. Δ_1 is the change in the probability of accepting the applicant at the offered rent following the Hindu $\rightarrow$ Muslim identity change. Δ_2 is the change in the probability of moving onto the price margin, while Δ_3 is the change in the probability of moving onto the quantity margin, or refusal.
The three marginal effects sum to zero by construction. The rationing share, $\Lambda = \Delta_3/(-\Delta_1)$, measures the share of the decline in acceptance at the offered rent that appears as refusal rather than as a higher rent requirement. Values close to one therefore indicate that the identity effect operates primarily through quantity rationing rather than price adjustment. Standard errors are obtained from 1,000 bootstrap replications clustered at the respondent level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The comparison is cross-sectional, so brokers in tight areas may differ in other ways. And the demand-side counterpart, tenant search duration, is itself a null (Appendix F), so the two sides of the market agree in finding nothing rather than contradicting each other.

F.6 Conflict exposure

The sampling design oversamples conflict-exposed neighbourhoods, two per stratum, which supports a within-stratum comparison holding composition approximately fixed. The identity effect is no larger in conflict-exposed neighbourhoods: the interaction is −0.080 (s.e. 0.062), with a wild-cluster bootstrap p-value of 0.24 across 24 exposed neighbourhoods of 417.

The broker sample cannot be used for this comparison. Only 14 of 619 brokers are located in a conflict-exposed neighbourhood, against 473 of 3,290 residents, so the elicited access and RWA measures cannot be compared across exposure. I report the resident-side interaction only.

F.7 Two preference elicitations

The choice instrument contains four tasks: two describing neighbourhoods by religious composition and two by amenity bundle. They do not identify the same parameters, and the reason is a design limitation worth stating plainly rather than working around.

In the *composition* tasks the own-group option is also the most expensive and the least distant in every task in which it appears, so rent is positively correlated with own-group composition by construction and the estimated rent coefficient is wrong-signed. Those tasks identify own-group preference but cannot identify a price coefficient.

In the *amenity* tasks the listed bundles differ completely across the three options, so the amenity indicators are option indicators and their coefficients are not separately identified. Willingness to pay for a metro station, a school or a park therefore cannot be recovered from this design. What these tasks *can* identify is the price coefficient, because rent varies within task while the religious institutions in the bundles function as composition signals rather than as facilities. Table F.9 establishes that respondents read them that way. Every group responds to at least one religious institution, and the significant responses are overwhelmingly to institutions the respondent would not use. Upper-caste Hindu respondents avoid options whose bundle includes a mosque (−0.76, s.e. 0.16) and favour those that include a temple (0.88, s.e. 0.19). Scheduled Caste respondents avoid mosques (−0.64, s.e. 0.24) and are indifferent to temples. Muslim respondents avoid temples (−0.98, s.e. 0.32) and are indifferent to mosques. An amenity reading predicts neither avoidance of a facility the household would never use nor indifference

Table F.6: Market tightness and the identity gap

Panel A: Broker-reported market tightness

	Mean	**25th pct.**	**Median**	**75th pct.**	N
Potential tenants per month	33.01	12.00	20.00	40.00	526
Completed matches per month	7.27	2.00	5.00	10.00	526
Potential tenants per completed match	6.10	2.00	4.17	7.50	526
Match rate	0.306	0.125	0.222	0.429	525

Panel B: Market tightness and identity-specific access

	Muslim access		**Muslim gap**		**Lower-caste gap**		**RWA bars Muslims**	
	No FE	**Area FE**	**No FE**	**Area FE**	**No FE**	**Area FE**	**No FE**	**Area FE**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tightness (standardised)	-0.491	-0.695	-1.612	-1.295	0.509	0.526	0.027	0.027
	(1.419)	(1.356)	(1.882)	(1.825)	(0.695)	(0.696)	(0.020)	(0.020)
Brokers	529	529	529	529	529	529	525	525
R-squared	0.000	0.066	0.002	0.070	0.001	0.005	0.004	0.017

Panel C: Vignette identity penalty and market tightness

	Binary	**Binary**	**Ladder**
	(1)	(2)	(3)
Identity effect β_1	-0.425***	-0.424***	1.244***
	(0.038)	(0.038)	(0.122)
Tightness (standardised)	0.002	0.005	-0.033
	(0.016)	(0.016)	(0.087)
Identity effect × tightness	0.007	0.002	0.025
	(0.028)	(0.028)	(0.104)
Information effect β_2	-0.035	-0.031	0.073
	(0.046)	(0.046)	(0.117)
Area type fixed effects	No	Yes	Yes
Observations	1,070	1,070	1,070

Notes: Panel A reports descriptive statistics for broker-reported rental-market tightness. Potential tenants and completed matches are the numbers of prospective tenants and successful matches that brokers report receiving in an average month. Nine broker observations with implausible reported volumes are excluded. Tightness is the number of potential tenants per successful match and is winsorised at the 1st and 99th percentiles. The match rate is completed matches divided by potential tenants. A higher value of tightness therefore indicates that more potential tenants compete for each completed match.
Panel B regresses four measures of identity-specific access on standardised market tightness. For each outcome, the first specification excludes and the second includes area-type fixed effects. Muslim access is the broker-reported number of properties, out of 100, in which a Muslim family could obtain housing. The Muslim gap is the corresponding access gap between Muslim and upper-caste Hindu families. The lower-caste gap serves as a placebo for the interpretation that market tightness specifically amplifies religious exclusion rather than general screening intensity. The final two columns use an indicator for whether the local resident welfare association (RWA) prohibits Muslim tenants.
Panel C restricts the sample to broker respondents and interacts the experimental identity treatment with standardised market tightness. Columns (1) and (2) use binary acceptance, while column (3) uses the three-category reluctance ladder. In the binary specifications, a negative identity effect indicates lower acceptance of the Muslim applicant. In the ladder specification, higher values indicate greater reluctance, so a positive coefficient represents a penalty. Standard errors in Panel C are clustered at the respondent level. Standard errors are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.7: Gatekeeping in conflict-exposed neighbourhoods

	Resident-side vignette		**Audit**
	No stratum FE	**Stratum FE**	**Own record only**
	(1)	(2)	(3)
Identity effect β_1	-0.199***	-0.199***	-0.199***
	(0.020)	(0.020)	(0.020)
Conflict-exposed neighbourhood	-0.007	-0.012	-0.012
	(0.022)	(0.023)	(0.023)
Identity effect × conflict-exposed	-0.076	-0.080	-0.080
	(0.062)	(0.062)	(0.062)
Information effect β_2	-0.007	-0.008	-0.008
	(0.017)	(0.017)	(0.017)
Stratum fixed effects	No	Yes	Yes
Own-record conflict flag only	No	No	Yes
Observations	7,736	7,736	7,736
neighbourhoods	417	417	417

Notes: The table examines whether the experimental identity effect is larger in conflict-exposed neighbourhoods. The outcome is binary acceptance of the vignette applicant. Column (1) includes no stratum fixed effects. Column (2) adds stratum fixed effects, exploiting comparisons within the four neighbourhood-composition strata. Column (3) is an audit specification restricted to observations whose conflict-exposure flag comes from the respondent's own record rather than being inherited from another respondent in the same neighbourhood.

The identity effect, β_1, captures the change in acceptance when the applicant changes from a Hindu to an otherwise comparable Muslim identity. The coefficient on Identity effect × conflict-exposed therefore measures whether this penalty differs in conflict-exposed neighbourhoods. In the preferred specification in column (2), the interaction is -0.080 (s.e. 0.062). Standard errors are clustered at the neighbourhood level.

There are 417 neighbourhoods in the estimation sample, of which 24 are conflict-exposed. Because the number of exposed clusters is small, inference for the interaction is also assessed using a wild-cluster bootstrap with 9,999 replications; the bootstrap p-value is 0.244. Broker-side conflict regressions are omitted because conflict exposure is extremely sparse among brokers: only 14 of 619 brokers are located in conflict-exposed neighbourhoods, yielding very imprecise estimates.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to the one it would; a composition reading predicts both. The pattern is also asymmetric in a way that echoes Section 5: for both minority groups, the institution that moves choice is the marker of the group they would avoid, not the marker of their own.

The two elicitations give different magnitudes but the same ordering. On the amenity tasks the own-group coefficients are $\hat{\eta}_{MUS} = 0.645$, $\hat{\eta}_{HUC} = 0.460$ and $\hat{\eta}_{SC} = 0.223$; on the model-free choice shares of **??** the gaps are 44.5, 33.5 and 24.2 points in the same order. That agreement is reassuring, since the two elicitations use different tasks, different respondents within the same survey, and different identifying variation. The common finding, and the one the analysis relies on, is that own-group preference is substantial and statistically strong for every group. The instrument should nonetheless be corrected in a future round by varying one attribute at a time across options, so that amenity-specific willingness to pay can be recovered.

F.8 Is this generic minority disadvantage? The caste comparison

Scheduled Caste applicants are a stigmatised minority who are neither religiously distinct from the majority nor distinguishable by the markers that separate Muslims from Hindus, so the comparison separates religion-specific exclusion from generic minority disadvantage. Four pieces of evidence point the same way.

In the broker data, the Muslim access gap relative to upper-caste Hindus is 62.0 percentage points while the lower-caste Hindu gap is 3.7 (Figure 2). In the RWA data, Muslim tenants are reported barred in 26.9 percent of areas against 0.8 percent for lower-caste Hindus. In the vignette instrument, replacing a Muslim name with "Manjhi", a Scheduled-Caste-associated surname, *raises* acceptance by 14.0 points ($p < 0.01$). And a within-Hindu contrast that replaces "Kumar" with the caste-marked surname "Pandey" moves acceptance by 0.9 points, a precise null.

The two vignette contrasts are the weakest of the four, because the instrument identifies caste only through surnames and contains no clean upper-caste-versus-Scheduled-Caste transition. The credible caste evidence is the broker and RWA comparison. Whatever is operating in this market is religion-specific and is not a proxy for poverty, low status, or a general tendency of gatekeepers to prefer applicants like themselves. Full caste-specific analysis is in Appendix I.

Table F.8: Stated-preference estimates

	Amenity tasks Rent level	**Amenity tasks Rent share**	**Composition tasks Rent level**
	(1)	(2)	(3)
Rent (thousands of rupees), β_R	-0.118***		0.483***
	(0.009)		(0.031)
Rent as a share of stated income, β_{share}		-9.240***	
		(0.773)	
Commute time (minutes), β_C	-0.045***	-0.041***	0.115***
	(0.004)	(0.004)	(0.003)
Own-group × Hindu upper caste, η_{HUC}	0.460***	0.639***	2.598***
	(0.055)	(0.058)	(0.071)
Own-group × Muslim, η_{MUS}	0.645***	0.584***	1.616***
	(0.100)	(0.099)	(0.076)
Own-group × Scheduled Caste, η_{SC}	0.223***	0.403***	1.265***
	(0.078)	(0.080)	(0.081)
Choice observations	17,730	17,730	17,724
Respondents	2,968	2,968	2,966

Notes: The table reports conditional-logit estimates from the stated-preference neighbourhood-choice tasks. Columns (1) and (2) use the two amenity tasks, in which rent and commute time vary within task and religious institutions provide signals about neighbourhood composition. Column (1) measures rent in thousands of rupees and is the specification used in the welfare exercise. Column (2) instead measures rent as a share of the household income stated in each task. The two amenity tasks state different household incomes, so the rent-level and rent-share specifications need not generate the same ordering of own-group coefficients. Unless otherwise stated, the text and welfare calculations refer to column (1). Column (3) uses the two composition tasks. In these tasks, neighbourhood composition is mechanically correlated with rent and commute time: In each composition task the first-listed option (an upper-caste Hindu area in one task, a Muslim area in the other) is both the most expensive and the closest, and the mixed option has the longest commute, so rent and commute are correlated with composition by construction and their coefficients are wrong-signed. The positive coefficients on rent and commute are therefore wrong-signed and show why these tasks are not used to recover the price and commuting coefficients for the welfare exercise. They are reported rather than suppressed to make this limitation explicit. The column (3) estimates are not used in the model.
The own-group coefficients interact the indicator for choosing an own-group-majority neighbourhood with the respondent's identity group. Standard errors, clustered at the respondent level, are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.9: Religious institutions are read as composition signals, not as amenities

	(1) Hindu upper caste	(2) Muslim	(3) Scheduled Caste
Mosque in the bundle	-0.758^{***}	0.119	-0.642^{***}
	(0.163)	(0.266)	(0.238)
Temple in the bundle	0.879^{***}	-0.976^{***}	0.014
	(0.190)	(0.320)	(0.271)
Choice observations	10,020	3,240	4,470
Respondents	1,670	540	745

Notes. Conditional logit on the two amenity tasks, in which options are described by a bundle of local facilities rather than by the composition of the neighbourhood, estimated separately by respondent group. The unit of observation is an alternative: each respondent faces three options in each of two tasks. Entries are coefficients on indicators for whether an option's bundle includes a mosque or a temple, with rent and commuting time included. Standard errors clustered on the respondent.

Under an amenity reading, a household should be indifferent to a religious institution it would not use and attracted to one it would. The table shows the reverse for both minority groups: Muslim respondents are indifferent to mosques and avoid temples, and Scheduled Caste respondents are indifferent to temples and avoid mosques. That is what a composition reading predicts.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

F.9 Forced pairwise choice

A second landlord module presents respondents with pairs of prospective tenants and asks them to choose between them. This is a forced-choice design and is in one respect cleaner than the accept/reject vignettes, since it removes scale-use heterogeneity and elicits a direct ranking.

One pair is a near-exact identity test with a twist. Sameer Sharma and Akbar Khan are both bankers, both single, both offering ₹35,000 on a twelve-month lease. They differ in name, and in tenancy history, which runs in the *Muslim* applicant's favour: he carries a good reference from a previous landlord and the Hindu applicant has no previous record. Respondents choose the Muslim applicant 54.0 percent of the time ($n = 2{,}793$, $z = 4.2$ against a null of one half). On the two pairs where identity is confounded with rent or occupation and no reference distinguishes the applicants, respondents choose the Muslim applicant 39.7 and 42.1 percent of the time.

Taken at face value this cuts against Implication 3: a reliability signal does move behaviour. I think the two findings are consistent and together sharper than either alone. Implication 3 says a signal does not overcome the identity effect when the respondent can simply decline; the forced-choice module says that when declining is unavailable and the comparison between two applicants is explicit, verification is worth something. What makes identity decisive in this market is therefore not that information is worthless but that refusal is available. That is the

Table F.10: Caste-marked transitions, by the respondent's own caste

	Ahmad → Manjhi		Kumar → Pandey	
	(1) SC vs UC	(2) Adding OBC	(3) SC vs UC	(4) Adding OBC
Effect of the name change	0.252***	0.248***	0.003	0.004
	(0.028)	(0.027)	(0.016)	(0.015)
× SC respondent	−0.145***	−0.145***	−0.034	−0.034
	(0.045)	(0.045)	(0.025)	(0.025)
× OBC respondent		−0.118**		0.032
		(0.049)		(0.033)
SC respondent	0.038	0.038	0.018	0.018
	(0.036)	(0.036)	(0.019)	(0.019)
OBC respondent		−0.032		−0.047*
		(0.040)		(0.028)
Information effect	0.035	0.043*	−0.032**	−0.034***
	(0.025)	(0.022)	(0.015)	(0.013)
Arm A acceptance, UC respondents	0.306	0.306	0.922	0.922
Observations	2,433	2,973	2,433	2,973

Notes. Binary acceptance, one observation per respondent for the indicated profile. In columns 1 and 2 arm A shows Shakeel Ahmad, a Muslim software engineer, and arms B and C show Soham Manjhi, a Scheduled-Caste-associated surname, with every other attribute and the photograph unchanged; the effect of the name change is therefore positive. In columns 3 and 4 the name changes from Arun Kumar to Atul Pandey. The omitted group is upper-caste Hindu respondents, so the first row is the effect among them and the interactions are the difference for Scheduled Caste and OBC respondents. The "SC respondent" and "OBC respondent" rows compare acceptance of the arm-A applicant across respondent groups. Muslim respondents and respondents of other religions are excluded. Standard errors clustered on the respondent.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

paper's thesis, stated more precisely.

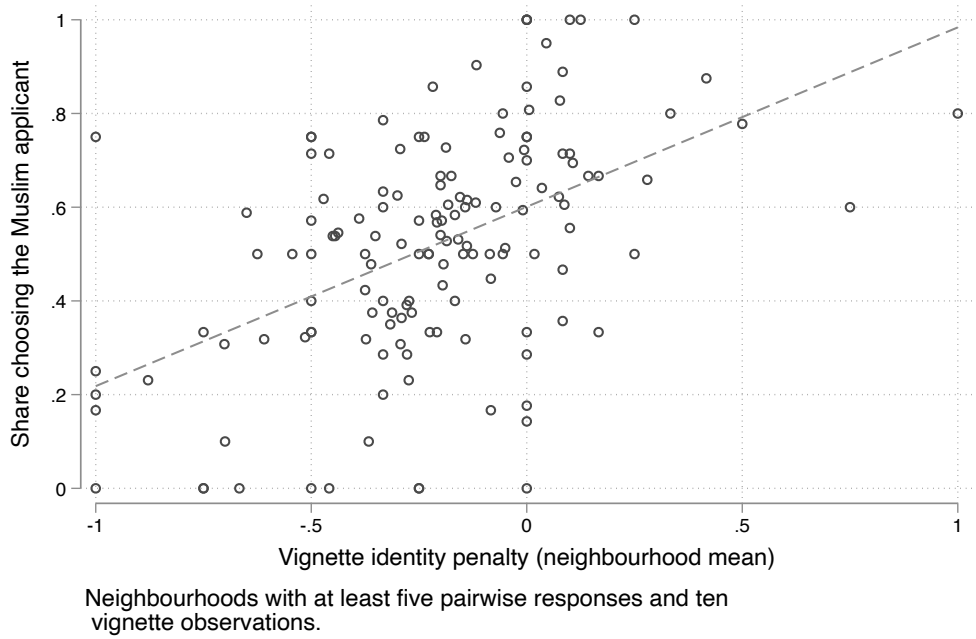


Figure F.1: Cross-instrument validation at the neighbourhood level. Scatter of the neighbourhood-mean vignette identity effect against the neighbourhood-mean share choosing the Muslim applicant in the clean pairwise contrast, with a fitted line, restricted to neighbourhoods with at least five pairwise responses and ten vignette observations. Correlation 0.495.

F.10 Do the stated measures track behaviour?

Every estimate in this paper is elicited rather than revealed: preferences from a hypothetical choice experiment, gatekeeping from hypothetical vignettes, access and RWA rules from broker reports. This is the paper's principal vulnerability and deserves a direct answer.

Five things support the measures. First, an *external behavioural benchmark* exists for this city: Datta and Pathania (2016) run a correspondence study in the Delhi rental market and find landlords markedly less responsive to Muslim than to comparable upper-caste Hindu applicants. Second, *cross-instrument agreement*: the vignette penalty, the broker access measure and the RWA-rule measure are three instruments with different response formats, different respondents and different failure modes, and they agree. Third, *cross-instrument agreement at the neighbourhood level*: the correlation between the neighbourhood-level vignette identity effect and the neighbourhood-level share choosing the Muslim applicant in the forced pairwise module is 0.495. Two instruments with entirely different response formats rank neighbourhoods the same way. Fourth, *administrative corroboration*: the elicited RWA prohibition rate is of the same order as the housing-society membership records of TwoCircles.net and Newslaundry (2024). Fifth, the

Table F.11: Forced pairwise choice: the clean identity pair

	Chooses Muslim applicant			Rejects both
	Respondent type	**Respondent religion**	**Conflict exposure**	**Respondent type**
	(1)	(2)	(3)	(4)
Respondent type				
Landlord only	0.000			0.059*
	(0.040)			(0.031)
Tenant and landlord	-0.186			0.072
	(0.220)			(0.171)
Broker	-0.238***			0.079***
	(0.029)			(0.023)
Resident, neither module	-0.018			0.079***
	(0.022)			(0.017)
Respondent religion				
Muslim respondent		0.330***		
		(0.021)		
Conflict exposure				
Conflict-exposed neighbourhood			0.019	
			(0.044)	
Baseline outcome	0.586***	0.489***	0.603***	0.214***
	(0.018)	(0.010)	(0.032)	(0.013)
Omitted group	Tenant only	Non-Muslim	Non-conflict	Tenant only
Area-type fixed effects	No	No	Yes	No
Observations	2,793	2,793	2,793	3,833

Notes: The table reports results from the clean forced-choice pair in the pairwise choice experiment. Sameer Sharma and Akbar Khan are both single bankers, offer the same monthly rent, and seek twelve-month leases. The two profiles differ in name and tenancy history. The information difference favours the Muslim applicant: Akbar Khan has a good recommendation from a previous landlord, while Sameer Sharma has no previous tenancy record. Thus, choosing Sameer Sharma amounts to preferring an unverified Hindu applicant to an otherwise comparable Muslim applicant with favourable prior-tenancy information.

The outcome in columns (1)–(3) equals one if the respondent chooses Akbar Khan, conditional on choosing one of the two applicants rather than rejecting both. Column (1) reports differences by respondent type, with tenant-only respondents as the omitted group. The tenant-only baseline probability of choosing the Muslim applicant is 0.586. Brokers are 23.8 percentage points less likely than tenant-only respondents to choose the Muslim applicant. Column (2) compares Muslim and non-Muslim respondents. Muslim respondents are 33.0 percentage points more likely to choose the Muslim applicant than non-Muslim respondents. Column (3) compares respondents in conflict-exposed and other neighbourhoods and includes area-type fixed effects; the estimated difference is small and statistically insignificant. Column (4) instead uses an indicator for rejecting both applicants as the outcome. The omitted category is again tenant-only respondents, whose baseline probability of rejecting both is 0.214. Landlord-only respondents, brokers, and residents who complete neither the tenant nor landlord module are more likely to reject both than tenant-only respondents.

Because columns (1)–(3) condition on not rejecting both, their sample contains 2,793 respondents, compared with 3,833 respondents in column (4). Standard errors in columns (1), (2), and (4) are heteroskedasticity-robust. Column (3) clusters standard errors at the neighbourhood level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

methodological literature finds conjoint and vignette designs to track real behaviour reasonably well (Hainmueller et al., 2015), and supplies bounds on experimenter demand (de Quidt et al., 2018) and design standards for survey experiments (Haaland et al., 2023).

Against this, the one revealed-behaviour measure available in the data, tenant search duration, does not show a gap (Appendix F). I do not treat that as decisive, for the reason given there: search duration measures the time to find a home within the accessible set, not the size of the accessible set. But it is the strongest piece of evidence against the elicited measures and it should be reported as such.

One further check combines the instruments rather than comparing them. The sorting model of **??** takes preferences from the choice experiment and access from the brokers, and predicts the Muslim share of each neighbourhood's renters from the composition of its owner-occupiers, without being fitted to it. The prediction correlates at 0.79 with the observed share. The model under-predicts the overall level of segregation, so the check supports the instruments' ability to rank neighbourhoods more than their ability to reproduce levels.

None of this makes elicited measures equivalent to revealed ones. It makes the specific claim, that Muslim applicants face large exclusion on the quantity margin in this market, robust to the failure of any one instrument.

F.11 Biases and bounds

The paper makes several bounding claims, in different directions and about different objects. Table F.12 sets them out together.

F.12 Selection into the price margin, and Lee bounds

The markup is observed only for respondents who do not refuse, and the identity change reduces selection into that sample sharply: by 9.5 percentage points among respondents who rent, 27.0 among those who let, and 46.8 among brokers. The types with the largest penalty show the largest drop, which is both the monotonicity condition the bounds require and the reason the estimates in Table 3 are lower bounds.

Table F.12: Biases and the direction of the resulting bound

Concern	Direction	Why	Evidence or check
Social desirability in the vignette	Lower bounds	Respondents may wish to appear secular and overstate acceptance, which would attenuate β_1; expressive responding could push the other way but unlikely (de Quidt et al., 2018; Haaland et al., 2023).	Cross-instrument agreement, including the neighbourhood-level correlation of 0.495 with the forced-choice module (Appendix F.10); survey framed as a study of housing decisions.
Selection into the markup sample	Lower bound on the price penalty	Only non-refusers can quote a price, and identity induces refusal most among those who object most.	Selection falls from 0.893 to 0.707; Lee (2009) bounds, Table F.13.
Broker self-report of access	Conservative: understates the gap	Any incentive to appear non-discriminatory pushes reported Muslim access up toward reported Hindu access.	External benchmark against housing-society membership records (TwoCircles.net and Newslaundry, 2024).
Design effects in the behavioural list experiments	Estimates not interpretable	Adding the sensitive item changed responses to the innocuous items, so the identifying assumption fails.	Blair and Imai (2012) test returns negative implied proportions. No inference drawn.
Amenity counts omitted from the welfare exercise	Lower bound on the cost of exclusion	Utility varies across areas only through composition, the one dimension on which exclusion does not disadvantage Muslim households (Table J.2).	Table J.2 shows a private-provision gap the exercise cannot price.
No outside option in the sorting model	Understates the segregation effect of exclusion's level	Every renter must live in a surveyed neighbourhood, so only the spatial pattern of access affects sorting, not its level.	The level is measured directly (Figure 5).
Unexplained dissimilarity in the sorting model	Direction cannot be signed	The model leaves 6 to 10 percent of observed dissimilarity unexplained; if that is unmeasured exclusion, exclusion's share is understated.	Table 16, Panel A.
Rents held fixed	Upper bound on the gain from equalising access	Equalising access would shift demand across areas and rents would adjust, partly offsetting the gain (Bayer et al., 2007).	Relaxed in Section 7, where rents clear a fixed stock.

Notes: The last two bounding assumptions concern different objects and therefore do not conflict. The omission of amenities bounds the estimated cost of the access constraint that exists, whereas the fixed-rent assumption bounds the estimated benefit of removing that constraint.

Table F.13: Lee bounds on the identity effect on the markup

	Married woman		Married man	
	Rupees	Per cent of rent	Rupees	Per cent of rent
Lower bound	49.4	0.224	−34.2	−0.342
	(45.4)	(0.205)	(99.6)	(0.993)
Upper bound	115.9***	0.527***	341.8***	3.418***
	(29.9)	(0.138)	(45.2)	(0.474)
Trimming proportion	0.209	0.209	0.292	0.292
Selected observations	2,967	2,967	2,528	2,528

Notes. Bounds on the effect among respondents who would not refuse under either identity. They require no exclusion restriction and no parametric selection model, only that the identity change weakly reduces the probability of quoting rather than refusing for every respondent. Selection falls from 0.893 to 0.707 on the first profile and from 0.814 to 0.576 on the second, so the control group is trimmed. The estimator assumes a continuous outcome and quoted markups cluster on round numbers - 18 of the 35 markups on the first profile are exactly ₹3,000, and 42 of 115 on the second are ₹5,000, so ties at the trimming quantile are a concern. Bootstrap standard errors, 1,000 replications.

G The sorting model in full

G.1 Calibrating the scale

Stated-preference coefficients may be attenuated relative to real choices, and λ rescales them. Table G.1 reports the fit across a grid, where the simulated moments come from predicting renters from owner-occupiers' composition alone.

Table G.1: Scale calibration

λ	Simulated D	Observed D	Simulated isolation	Observed isolation
0.50	0.582	0.697	0.474	0.568
0.75	0.607	0.697	0.510	0.568
1.00	0.628	0.697	0.538	0.568
1.25	0.643	0.697	0.557	0.568
1.50	0.652	0.697	0.568	0.568
2.00	0.662	0.697	0.579	0.568
3.00	0.673	0.697	0.586	0.568
4.00	0.678	0.697	0.588	0.568

Notes. $\lambda = 1.5$ minimises the gap on isolation, which is the calibration used in the second column of Table 16. Calibrating on isolation spends that moment: the fit on isolation at $\lambda = 1.5$ is good by construction, which is why the paper reports $\lambda = 1$ throughout as well. Dissimilarity is under-predicted at every scale tried, including $\lambda = 4$.

G.2 Out-of-sample validation

Fitting ξ_n to neighbourhood renter totals leaves composition free, so the model's prediction of each neighbourhood's Muslim renter share can be tested. Predicting renters from owner-occupiers' composition alone, so the households being predicted play no part, gives a renter-weighted correlation of 0.794 with the observed share across the 141 neighbourhoods with renters, of which 11 have no surveyed owner and fall back to the full composition. Simulated dissimilarity is 0.629 $[0.600, 0.654]$ against 0.697 observed, and simulated isolation 0.539 $[0.519, 0.562]$ against 0.568.

G.3 Multiple equilibria

G.4 The counterfactual on the amenity-task scale

The segregation counterfactual in Section 7.6 uses the composition-task parameters. Running it on the amenity-task parameters instead, which share one normalisation with the price coefficient, at the cost of measuring composition through religious institutions rather than directly, gives

Table G.2: Stage-3 dissimilarity from three starting allocations

	Observed start	Integrated start	Segregated start
Baseline	0.655	0.614	0.686
Equalised	0.626	0.585	0.638
Share attributable	0.044	0.048	0.070

Notes. $\lambda = 1$. Composition enters utility, so the fixed point need not be unique. The spread across starting points, 0.072, exceeds the counterfactual effect of 0.029, so no single level of dissimilarity should be quoted. The attributable share is stable in sign and size from every start.

a stage-3 baseline dissimilarity of 0.544, a counterfactual of 0.488 and an attributable share of 10.4 per cent. The two sets of parameters bracket the estimate rather than agreeing on it, which is a further reason to report the result as a bound.

G.5 Religious institutions as composition signals

The amenity tasks describe neighbourhoods by their facilities rather than by their residents, and the religious institutions in those bundles carry the composition information. Table G.3 shows respondents read them that way: a mosque is not a facility a Hindu household consumes, and the pattern is what a composition reading predicts rather than an amenity reading.

Table G.3: Religious institutions are read as composition signals

	Hindu upper caste	Muslim	Scheduled Caste
Mosque in the bundle	−0.758***	0.119	−0.642***
	(0.163)	(0.266)	(0.238)
Temple in the bundle	0.879***	−0.976***	0.014
	(0.190)	(0.320)	(0.271)
Choice observations	10,020	3,240	4,470

Notes. Conditional logit on the two amenity tasks, with rent and commuting time included. Standard errors clustered on the respondent. Two patterns are worth noting against the simple reading. Muslim respondents do not respond to mosques, and the coefficient that moves them is the temple term; Scheduled Caste respondents respond negatively to mosques and not at all to temples. Both minority groups therefore react to the other group's marker rather than their own, which is why the paper takes composition preferences from the composition tasks and uses these estimates only for the price coefficient.

H The hedonic exercise

Section 7 converts the access loss into money using the price coefficient from the amenity tasks. This appendix reports the external check on that coefficient, which fails, and explains what follows.

H.1 Units

The survey asks for the average duration of commute for the day, which is a round trip; the choice experiment's table is headed "Commute time (minutes)" with values of 20, 35 and 25, which respondents will have read as one way. The gradient below is therefore expressed in one-way minutes, which halves the survey variable. The reported round trip has a median of 30 minutes and a ninetieth percentile of 90, so the implied one-way median is 15 minutes against the experiment's 20 to 35 range.

H.2 The gradient

Table H.1: Rents do not capitalise commuting time

	Raw	Area type	Plus dwelling	neighbourhood fixed effects
Commute (one-way minutes)	0.0004	0.0002	0.0002	0.0009
	(0.0013)	(0.0012)	(0.0012)	(0.0014)
Implied rupees per minute	−2.58	−1.14	−1.02	−5.74
95 per cent interval	[−18.3, 13.1]	[−16.1, 13.8]	[−15.7, 13.6]	[−22.5, 11.0]
R^2	0.000	0.024	0.088	0.412
Observations	480	480	480	480

Notes. Dependent variable is log monthly rent; robust standard errors. Dwelling controls are the rental type and the security deposit; the instrument does not record bedrooms, rooms or floor area, so dwelling quality is only partly held fixed and the gradient would if anything be biased toward finding a relationship rather than away from one. The implied rupees per minute is the gradient times the mean rent of ₹6,295, so a positive gradient gives a negative money value. Commute is recorded for 480 of the 1,004 renters with a rent, and the one-way distribution has a tenth percentile of 2.5 minutes, which suggests many respondents work locally or at home and compresses the variation the gradient needs.

The gradient is indistinguishable from zero in every specification and positive in all four, so no money value of travel time can be read off observed rents. Three readings are available. Commute may be measured too coarsely, or on a selected subsample. Posted rents, the cap on increases between contracts and the thinness of formal contracting may genuinely decouple rents from location value. Or location value may be capitalised in dwelling quality rather than in rent, with the same money buying a worse flat closer in. The first is a limitation; the second is consistent with Section 4.2 and is the reading the paper favours; the third the data cannot

address without a measure of floor area.

H.3 Which price coefficient to use

The experiment yields two estimates depending on how composition enters the amenity tasks: −0.118 with own-group indicators and −0.020 with separate mosque and temple terms. Table H.2 applies the conventional check.

Table H.2: Implied value of travel time

	β_R	Cost of 30 one-way minutes	Implied value per hour
Own-group indicators	−0.118	₹11,414	₹519
Mosque and temple terms	−0.020	₹41,192	₹1,873

Notes. The monthly cost of thirty extra one-way minutes is $30\beta_C/\beta_R$ in thousands of rupees; the hourly value divides by the implied hours of extra travel per month, taking 22 commuting days. Against a median imputed monthly income of ₹30,000, an hourly wage of about ₹170 implies that the first estimate values travel time at roughly three times the wage and the second at eleven times. Conventional estimates place the commuting value of travel time between a third and two thirds of the wage, so both are high and the first is far closer. The paper uses −0.118 on that ground and reports that the money figures scale inversely with it.

I Caste-specific results

Table I.1: The identity penalty by respondent group, including OBC

	Hindu upper caste	Muslim	Scheduled Caste	Hindu OBC
Identity penalty	−0.290***	0.089***	−0.213***	−0.279***
	(0.022)	(0.029)	(0.030)	(0.040)
Information effect	−0.016	−0.005	0.008	0.053
	(0.026)	(0.023)	(0.036)	(0.043)
Observations	3,370	1,086	1,496	1,080
Respondents	1,685	543	748	540

Notes. Binary acceptance, pooling the two profiles that change from a Hindu to a Muslim name. Hindu OBC respondents are excluded from the three-group analysis in the body because the broker access measure offers only "Hindu family, lower caste", which matches neither OBC nor Scheduled Caste exactly. Their penalty is close to the upper-caste Hindu figure, so the exclusion does not drive the result. Standard errors clustered on the respondent.

Table I.2: Caste-marked transitions, by the respondent's own caste

	Muslim to SC surname		Kumar to Pandey	
	SC vs UC	Adding OBC	SC vs UC	Adding OBC
Effect of the name change	0.252***	0.248***	0.003	0.004
	(0.028)	(0.027)	(0.016)	(0.015)
× SC respondent	−0.145***	−0.145***	−0.034	−0.034
	(0.045)	(0.045)	(0.025)	(0.025)
× OBC respondent		−0.118**		0.032
		(0.049)		(0.033)
Observations	2,433	2,973	2,433	2,973

Notes. Binary acceptance. The first pair of columns moves a Muslim-named applicant to a Scheduled Caste surname, which raises acceptance by 25 points among upper-caste Hindu respondents and by 11 among Scheduled Caste respondents. The second pair moves between two Hindu surnames of different caste standing and does nothing for anyone, which is the cleanest evidence that the name manipulation works through religion rather than through caste marking in general. Standard errors clustered on the respondent.

The composition item asks about "people with the same caste/religion as you" and so bundles the two, which is why the paper does not use it to separate caste homophily from religious homophily. Table I.3 corrects two figures carried over from an earlier draft, which reported the own-group ranking as 73, 70 and 50 per cent for Muslim, Scheduled Caste and upper-caste Hindu respondents, and the religion-in-landlords'-top-three share as about 60 per cent. Those used the conditional denominator.

Table I.3: Experience and perception, by group

	Hindu UC	Muslim	SC	Hindu OBC
Ever denied by a landlord or broker	0.171	0.172	0.248	0.093
Among those denied, believes the reason was religion	0.140	0.425	0.275	0.222
Among those denied, believes the reason was caste	0.158	0.200	0.200	0.278
Ranks religion among landlords' top three factors	0.143	0.196	0.156	0.131
Ranks own-group composition in their own top three	0.136	0.256	0.156	0.206
Reported difficulty gap, different against same religion	0.43	0.19	0.37	0.23

Notes. Resident respondents. The denial question is served inside the tenant module, so the first row is a share of tenants. The ranking items are taken over everyone who answered the ranking at all, since no item is required and an unranked item means not important; taking them over those who ranked the item inflates the shares by roughly a factor of two. The difficulty gap is the reported difficulty of finding a rental for a household of a different religion less that for one of the respondent's own, on a five-point scale, so a positive number means the respondent thinks it is harder for the other group. Muslim respondents report the smallest gap and are the most likely to attribute their own denial to religion. On the broker side, 55.3 per cent report that landlords weigh religion against 21.7 per cent for caste.

I.1 OBC respondents

Table I.4 reports the Hindu → Muslim identity effect separately for each respondent group, including the OBC respondents the main text sets aside. OBC respondents penalise the Muslim-named applicant by 27.9 points, indistinguishable from the 29.0 points among upper-caste Hindu respondents. Scheduled Caste respondents penalise by 21.3 points, somewhat less than upper-caste Hindu respondents (a difference of 7.7 points, $z \approx 2.1$ treating the two samples as independent). Muslim respondents, as in Table J.5, show no penalty. Setting OBC respondents aside therefore changes nothing; if anything, including them would slightly strengthen the non-Muslim penalty.

Table I.4: The Hindu → Muslim identity effect, by respondent group

	(1) UC Hindu	(2) Muslim	(3) Scheduled Caste	(4) Hindu OBC
Identity effect β_1	-0.290^{***}	0.089^{***}	-0.213^{***}	-0.279^{***}
	(0.022)	(0.029)	(0.030)	(0.040)
Information effect β_2	−0.016	−0.005	0.008	0.053
	(0.026)	(0.023)	(0.036)	(0.043)
Observations	3,370	1,086	1,496	1,080
Respondents	1,685	543	748	540

Notes. Binary acceptance, pooling the two clean Hindu → Muslim profiles; one observation per respondent × profile. A negative coefficient is a penalty. Respondent counts include brokers, whose caste is recorded in the same way. OBC respondents are set aside in the three-group analysis of the main text because the broker access measure has no matching category. Standard errors clustered on the respondent.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

J Additional results

J.1 Search, from the household side

The access measure is elicited from brokers. The tenant module supplies a recalled behavioural counterpart: how long the search took and how many properties were viewed. Table J.1 finds no difference by group.

Table J.1: Search duration and properties viewed

	Days searching		Properties viewed	
Muslim	5.12	7.11	−0.289	−0.167
	(5.32)	(5.28)	(0.218)	(0.236)
Scheduled Caste	−0.47	0.62	−0.065	−0.095
	(4.43)	(4.10)	(0.311)	(0.306)
Controls	No	Yes	No	Yes
Tenants	727	724	727	724

Notes. The omitted group is upper-caste Hindu. Both outcomes are winsorised at the first and ninety-ninth percentiles. Controls are area type, age band, sex and employment status; education is omitted for item non-response. Standard errors clustered on the neighbourhood. The null is not strong evidence against the access result: search duration is recalled, it is recorded for only 383 tenants before winsorising, and a household that knows where it will be accepted need not search longer to end up with a smaller choice set, which is precisely the mechanism the paper describes.

J.2 Do the broker and household measures agree?

Linking each tenant to the brokers within two kilometres, tenants in areas where brokers report more applicants per completed let also report viewing more properties at the neighbourhood level (0.064, s.e. 0.030 across the 38 neighbourhoods containing both a surveyed broker and a surveyed tenant), though the relationship does not survive at the individual level (0.012, s.e. 0.043 across 984 tenants). The two sides of the market agree weakly.

J.3 Amenity access: a null worth recording

An earlier version of this paper proposed to express the access gap in amenity terms, using counts of schools, hospitals, parks, transport and the rest scraped for each neighbourhood. The exercise does not add anything to the access figures and is reported here so that the attempt is on the record.

Two further nulls from the same exercise are worth recording. Amenity provision is not

Table J.2: Amenity reach adds nothing to the access figures

	neighbourhoods	Land	Public	Private	Ambiguous
Hindu upper caste	0.872	0.870	0.850	0.858	0.857
Muslim	0.314	0.225	0.220	0.227	0.236
Scheduled Caste	0.834	0.822	0.799	0.800	0.797
Provision per unit of land, category reach divided by land reach					
Hindu upper caste			0.976	0.985	0.985
Muslim			0.977	1.006	1.044
Scheduled Caste			0.973	0.973	0.970

Notes. Access-weighted reach across the 141 neighbourhoods in the sorting sample. Public amenities are parks, sports facilities, metro stations, police stations, courts, water tanks and monuments; private are clinics, ATMs and private educational establishments; ambiguous are schools and hospitals, which the scrape does not separate into public and private provision. Religious institutions are excluded from all three, because the sampling strata were built from temple and mosque counts and including them would make the statistic partly a measure of composition. The second panel shows the accessible neighbourhoods are provisioned about as well per square kilometre as the rest of the city for every group, so the amenity figures track the land figure, which in turn tracks the access figure. The land gap is itself driven by seven very large neighbourhoods holding 64 per cent of the surveyed land: excluding them, Muslim land reach rises from 0.225 to 0.306, against a neighbourhood-count figure of 0.314. The access figures in Section 4.4 therefore carry the point without the scrape.

measurably worse where Muslim households live: regressing the public amenity index on the neighbourhood's Muslim share with area-type controls gives −0.12 with a standard error of 0.29, and the private and ambiguous indices give −0.27 (0.30) and −0.25 (0.40). And amenity exposure does not vary with household income: the correlation is below 0.05 for every category on either income construction, so the natural framing of the access gap as equivalent to a position in the income distribution is not available in these data.

Table J.3 shows that the estimated identity effect is highly robust to covariate adjustment. The ordered-probit coefficient rises only modestly, from 0.665 in the unadjusted specification to 0.709 after adding applicant-profile characteristics, respondent demographics, and area-type controls. In contrast, the information effect remains close to zero and statistically insignificant throughout.

Because ordered-probit coefficients are scale-dependent, these coefficient movements should not be interpreted as substantive changes in the treatment effect. Adding covariates that explain the outcome can mechanically alter the estimated scale of the latent index even under random assignment. The corresponding marginal effects, discussed in the text, are therefore the cleaner comparison across specifications. Overall, the results indicate that the main identity effect is not driven by observable differences in respondent or area composition.

Table J.3: The identity effect is unchanged by covariate adjustment

	No controls	**Profile**	**Plus respondent**	**Plus area type**
	(1)	(2)	(3)	(4)
Identity effect	0.665***	0.679***	0.707***	0.709***
	(0.045)	(0.046)	(0.047)	(0.047)
Information effect	0.031	0.031	0.029	0.033
	(0.043)	(0.044)	(0.044)	(0.044)
Applicant profile	No	Yes	Yes	Yes
Respondent age, sex, employment	No	No	Yes	Yes
Area type	No	No	No	Yes
Observations	7,736	7,736	7,646	7,646

Notes: The table reports ordered probit estimates for the reluctance outcome. Column (1) reproduces the baseline specification reported in the main results. Column (2) adds applicant-profile controls, column (3) additionally controls for respondent age, sex, and employment status, and column (4) further adds area-type controls. Education is excluded because it is missing for approximately 94 per cent of respondents.
The identity coefficient is positive and highly statistically significant across all four specifications, while the information effect remains small and statistically insignificant. Because ordered probit coefficients are identified only up to the scale of the latent error term, adding covariates that predict the outcome can mechanically rescale the coefficients even under random assignment. The marginal effects reported in the text are therefore the more appropriate object for comparing magnitudes across specifications. Standard errors are clustered at the respondent level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

J.4 List Experiment Diagnostics

Table J.7 reports diagnostic checks for the five list experiments. The two behavioural lists perform noticeably worse than the three attitudinal lists. The tenant and landlord lists have relatively large minimum detectable prevalences, at 23.2 and 47.6 percentage points, respectively, and substantial floor effects: 36.2 per cent of treated tenants and 48.3 per cent of treated landlords report zero items. The Blair and Imai (2012) design-effect diagnostic also produces negative implied response-type proportions for both behavioural lists -two violations for the tenant list and one for the landlord list, which is inconsistent with the no-design-effect restriction underlying the standard difference-in-means interpretation.

The attitudinal lists are better behaved on these diagnostics. None generates a negative implied proportion in the Blair–Imai check, and their minimum detectable prevalences are smaller, at roughly 17–19 percentage points. Floor shares are also substantially lower, at around 13–14 per cent. The main exception is the Hindu-nation list, for which 10.6 per cent of treated respondents are at the ceiling and the treatment and control variances differ sharply. These diagnostics suggest treating the behavioural-list estimates cautiously, both because of limited power and because the response distributions show evidence against the simplest list-experiment assumptions, while the attitudinal lists provide cleaner evidence overall.

Table J.4: Perceived, experienced, and admitted discrimination

Panel A. Direct survey measures, by respondent religion		
	Non-Muslim	Muslim
Experienced a denial of housing	15.8%	17.2%
of which attributed to religion	20.8%	42.5%
Ranks religion in landlords' top-3 factors	59.8%	62.6%
Panel B. Attitudinal list experiments		
	All respondents	Non-Muslim
India should be a Hindu nation	0.501***	0.553***
	(0.068)	(0.074)
Supports caste/religion reservation	0.227***	0.254***
	(0.063)	(0.069)
Believes the Manusmriti caste hierarchy	0.154**	0.120
	(0.062)	(0.067)
Panel C. Behavioural list experiments — uninformative		
	Prevalence	Min. detectable
Tenant: faced identity discrimination	0.011 (0.083)	0.232
Landlord: declined a tenant on identity	0.103 (0.170)	0.476

Notes. Panel A: one observation per respondent. The first two rows are among tenant respondents, since the denial question is asked within the tenant module; the second conditions on having reported a denial (165 tenants).

Panels B and C: list (item-count) experiments (Imai, 2011; Blair and Imai, 2012). Respondents report only the *number* of items on a list that apply to them; a randomly assigned treatment group sees one additional sensitive item. Each entry is the treatment−control difference in mean item counts, which identifies the prevalence of the sensitive item under the assumption that adding the item does not change responses to the others. The three attitudinal lists share a single control arm of 854 respondents. Respondents answering "prefer not to say" are excluded.

Panel C reports the two behavioural lists for completeness only. Both fail the Blair and Imai (2012) design-effect test, returning negative implied proportions of control-item response types, so the identifying assumption does not hold. The final column reports the minimum prevalence detectable at 80 percent power and 5 percent size: treated cells are 486 and 114 respectively. No inference is drawn from these two rows. Full diagnostics are in Table J.7.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J.5: Does the identity effect depend on the respondent's own religion?

	Binary acceptance			Reluctance ladder		
	(1) Non-Muslim	(2) Muslim	(3) Pooled	(4) Non-Muslim	(5) Muslim	(6) Pooled
Identity effect β_1	-0.260^{***}	0.089^{***}	-0.260^{***}	0.805^{***}	-0.364^{***}	0.802^{***}
	(0.015)	(0.029)	(0.015)	(0.049)	(0.131)	(0.048)
Information effect β_2	-0.001	-0.005	-0.002	0.012	0.097	0.019
	(0.018)	(0.023)	(0.016)	(0.046)	(0.124)	(0.043)
Muslim respondent			-0.036			0.135
			(0.025)			(0.096)
$\beta_1\times$ Muslim respondent			0.347^{***}			-1.122^{***}
			(0.029)			(0.116)
Observations	6,650	1,086	7,736	6,650	1,086	7,736
Respondents	3,325	543	3,868	3,325	543	3,868

Notes: The table reports estimates of the identity effect in equations (11) and (2), estimated separately by the religion of the respondent and in the pooled sample with an interaction. The unit of observation is a respondent × profile, pooling the two clean Hindu → Muslim profiles. All respondents, Muslim and non-Muslim alike, answer as hypothetical landlords evaluating the same applicants, so the contrast is in who is doing the screening. The two outcomes are signed in opposite directions: for binary acceptance (columns 1–3), a negative coefficient represents a penalty, while for the reluctance ladder (columns 4–6), a positive coefficient represents a penalty. Both therefore indicate that non-Muslim respondents penalise the Muslim-named applicant, while Muslim respondents are slightly more likely to accept that applicant than the otherwise identical Hindu-named applicant. A penalty that vanishes among respondents of the applicant's own group is evidence against an account in which every gatekeeper enforces a norm that they expect to face regardless of their own identity. Standard errors are clustered at the respondent level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table J.6: The broker effect and the preferences he expects to face

	Elicited Muslim access gap		Own vignette acceptance	
	(1)	(2)	(3)	(4)
Landlords in area weigh religion	14.58***		0.029	
	(3.01)		(0.034)	
Local RWA bars Muslim tenants		20.09***		−0.025
		(2.90)		(0.045)
Identity effect β_1			−0.321***	−0.354***
			(0.050)	(0.042)
× landlords weigh religion			−0.180***	
			(0.057)	
× RWA bars Muslim tenants				−0.241***
				(0.064)
Information effect β_2			−0.036	−0.034
			(0.046)	(0.045)
Area-type fixed effects	Yes	Yes	Yes	Yes
Observations	529	525	1,070	1,050
Brokers	529	525	535	525

Notes. Brokers who handle rental transactions. Columns 1 and 2: one observation per broker; the outcome is his reported access for an upper-caste Hindu family less that for a Muslim family, in percentage points out of 100 local properties, regressed on two things he also reports, whether landlords in his area weigh an applicant's religion, and whether the local resident welfare association bars Muslim tenants. Robust standard errors. Columns 3 and 4: one observation per broker × profile, pooling the two clean Hindu → Muslim profiles; the outcome is his own binary acceptance in the vignette experiment, and the identity effect is interacted with the same two reports. The coefficient on β_1 is the effect among brokers who do not make the report; the interaction is the additional effect among those who do. Standard errors clustered on the broker. Brokers answering "don't know" about association rules are excluded, which accounts for the smaller sample in columns 2 and 4.

These are associations, not a causal decomposition of (**??**). Both moderators are reported by the broker himself and neither is randomised, so a broker with a strong personal aversion could both perceive local landlords as prejudiced and penalise more heavily in the vignette. The association-rule moderator is less exposed to this than the landlord-preference one, because a rule is a verifiable fact about the area rather than a perception, and the elicited rule rate is corroborated by administrative membership records. Columns 1 and 2 are further exposed to consistency bias, since the outcome and the moderator are both elicited from the same broker in the same interview.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table J.7: List experiment diagnostics

	Behavioural lists		**Attitudinal lists**		
	Tenant discrimination	**Landlord** discrimination	**Reservation** support	**Manusmriti** hierarchy	**Hindu** nation
	(1)	(2)	(3)	(4)	(5)
Sample and prevalence					
Control observations	453	144	854	854	854
Treated observations	486	114	896	906	871
Prevalence estimate	0.011	0.103	0.227	0.154	0.501
	(0.083)	(0.170)	(0.063)	(0.062)	(0.068)
MDE, 80% power	0.232	0.476	0.177	0.175	0.190
Floor and ceiling effects among treated respondents					
Ceiling share	2.1%	1.8%	2.2%	1.8%	10.6%
Floor share	36.2%	48.3%	13.8%	13.7%	13.1%
Blair–Imai design-effect diagnostic					
Implied proportions by control-item count, (π_k^F, π_k^C):					
$k = 0$	(0.015, 0.238)	(**-0.052**, 0.192)	(0.022, 0.206)	(0.024, 0.234)	(0.030, 0.146)
$k = 1$	(**-0.004**, 0.177)	(0.037, 0.033)	(0.068, 0.176)	(0.040, 0.199)	(0.128, 0.121)
$k = 2$	(**-0.033**, 0.202)	(0.078, 0.185)	(0.060, 0.138)	(0.037, 0.157)	(0.115, 0.070)
$k = 3$	(0.012, 0.010)	(0.023, 0.003)	(0.054, 0.115)	(0.035, 0.119)	(0.122, 0.031)
$k = 4$	(0.021, 0.000)	(0.018, 0.000)	(0.022, 0.000)	(0.018, 0.000)	(0.106, 0.000)
Negative implied proportions	**2**	**1**	0	0	0
Variance diagnostic					
Control variance	1.577	1.548	1.641	1.641	1.641
Treated variance	1.657	2.072	1.847	1.777	2.314
Variance-ratio test, p-value	0.592	0.099	0.081	0.238	< 0.001

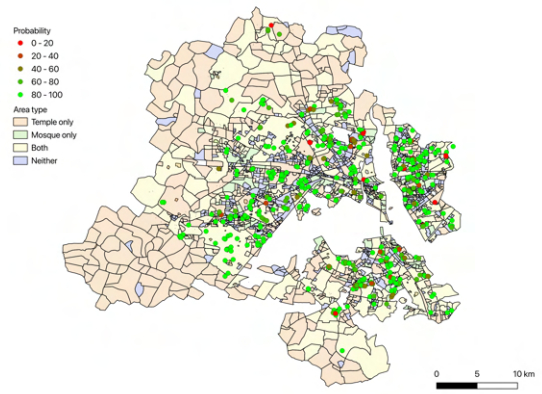
Notes: The table reports diagnostics for the five list experiments. Columns (1) and (2) are behavioural lists. The tenant list asks whether the respondent has experienced discrimination in the rental market on the basis of identity; the landlord list asks whether the respondent has declined a prospective tenant on the basis of caste or religion. Columns (3)–(5) are attitudinal lists whose sensitive items concern support for caste- or religion-based reservation, belief in the caste hierarchy described in the Manusmriti, and the view that India should be a Hindu nation, respectively.

The prevalence estimate is the difference in mean item counts between the treated and control lists. Robust standard errors are reported in parentheses. The minimum detectable prevalence is calculated for 80 per cent power as 2.8 times the standard error. The behavioural lists are relatively underpowered: the tenant and landlord lists have minimum detectable prevalences of 23.2 and 47.6 percentage points, respectively. Ceiling and floor shares are calculated among treated respondents. The two behavioural lists exhibit substantial floor effects: 36.2 per cent of treated tenants and 48.3 per cent of treated landlords report zero items. Ceiling shares are small for both behavioural lists. The Hindu-nation attitudinal list has a larger ceiling share of 10.6 per cent.

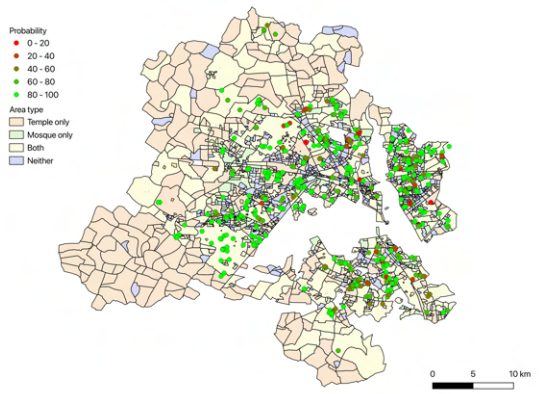
The Blair–Imai design-effect diagnostic reports the implied proportions of latent control-item response types at each possible count k. Each entry is (π_k^F, π_k^C), where the two quantities are the implied proportions obtained from the lower- and upper-tail restrictions used in the design-effect check. Under the no-design-effect assumption these implied proportions should be non-negative. Negative values are shown in bold. The tenant list produces two negative implied proportions, at $k = 1$ and $k = 2$, while the landlord list produces one, at $k = 0$. All three attitudinal lists satisfy the non-negativity check.

The final panel compares the variance of reported item counts across the control and treatment groups. The reported p-value is from a two-sided variance-ratio test. Taken together, the floor effects and Blair–Imai violations indicate that the behavioural-list estimates should be interpreted cautiously; the attitudinal lists perform better on the non-negativity diagnostic, although the Hindu-nation list displays a clear variance difference across arms. Respondents selecting "prefer not to say" are excluded.

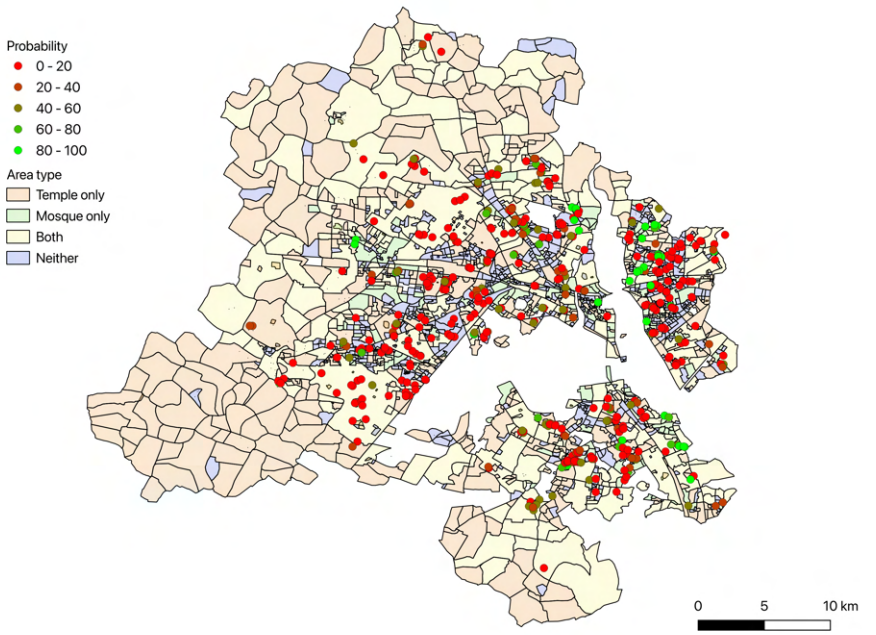
K Additional figures



(a) Hindu Upper Caste

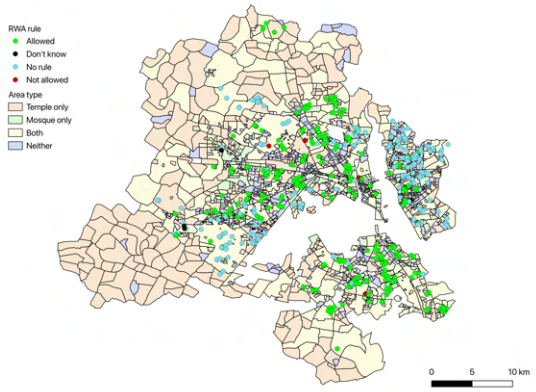


(b) Hindu Scheduled Caste

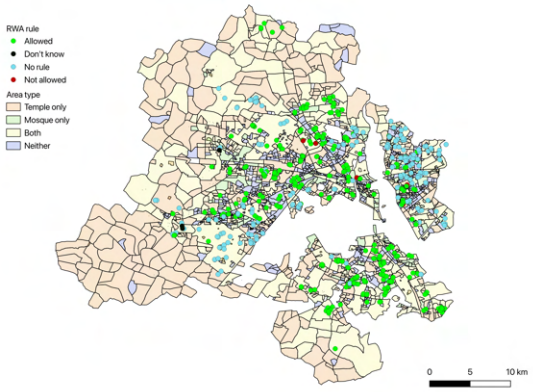


(c) Muslim

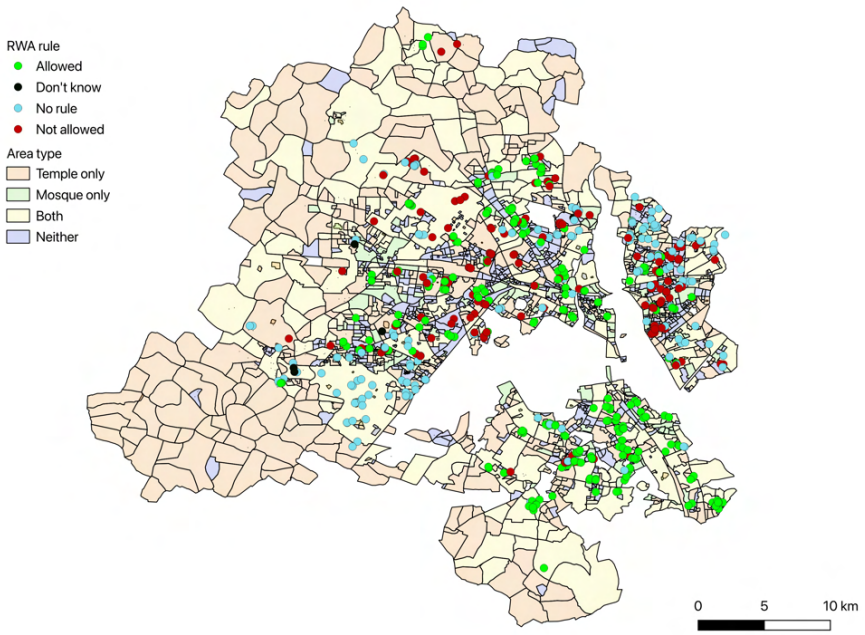
Figure K.1: Broker-reported probabilities of finding a house in their area.



(a) Hindu Upper Caste



(b) Hindu Scheduled Caste



(c) Muslim

Figure K.2: Broker-reported RWA rules on tenant acceptance, by identity.