The Price Penalty for Migrant Entrepreneurs: Evidence from a Field Experiment in South Africa

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Abstract

Migrants own and manage a substantial share of the world's retail firms, yet their "outsider" status may generate demand-side frictions that constrain growth. Despite the prevalence of migrant entrepreneurship, the market impact of consumer bias against migrant shops remains largely undocumented. This research addresses that gap by providing the first causal estimates of a consumer-side penalty against migrant-owned stores using a two-sided empirical strategy in Johannesburg, South Africa. On the demand side, we implement a large-scale incentive-compatible choice-based conjoint experiment (N = 1,974) in which shopkeeper nationality is randomized alongside price, brand, and store attributes. Within this, we embed an information experiment to distinguish taste-based from statistical discrimination. We complement these demand-side data with a supply-side census of retail firms in the same area (N = 432).

We estimate a large and statistically significant taste-based demand penalty for migrant-owned stores. Consumers are willing to pay R 1.72 more to avoid shopping at a migrant-owned store, roughly 9 percent of market price. Mechanism tests indicate that the penalty is not significantly driven by inferences about store quality: it is comparable for non-perishables and only partially attenuated when perceived store safety and service quality are varied. On the supply side, migrant-owned stores charge 0.41 standard deviations lower prices than comparable local competitors, even after controlling for unit costs, competition, product condition, customer composition, and granular location fixed effects. Together, the findings establish that seller identity-based consumer biases constitute a demand-side friction that distorts equilibrium prices and compresses migrant profit margins by about 22.7%.

Keywords: Social biases, Immigrants, Small Firm Growth, Entrepreneurship, Price Penalty, Conjoint Survey Experiment

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1 Introduction

Understanding the economic implications of migration is of first-order importance in an era where 281 million people live outside their country of birth (International Organization for Migration, 2024) and where substantial shares of citizens across countries view migrants as a burden, favoring anti-immigrant policies (Pew Research Center, 2018). While a rich literature examines how migrant workers impact formal labor markets, far less attention has been paid to migrants as entrepreneurs (Azoulay et al., 2022). This omission is consequential as migrant entrepreneurs are disproportionately represented in key sectors like retail: recent migrants own roughly one quarter of retail firms in the United States (U.S. Census Bureau, 2022) and account for half of the retail industry in our empirical setting of South Africa (Liedeman et al., 2013).

In this paper, we examine for the first time how societal attitudes toward migration manifest in product market interactions between migrant entrepreneurs and local consumers. First, we investigate whether local consumers' attitudes toward migration shape demand in grocery retail by estimating their willingness to pay (WTP) to avoid migrant-owned stores. Second, we examine whether migrant retailers act strategically by anticipating this demand penalty and compensating consumers with lower prices than local counterparts.

We study these questions in the context of small-scale retail firms in a large municipality of Johannesburg, South Africa. Small-scale (neighborhood) retail involves repeated physical interactions between store owners and consumers, making entrepreneur identity salient. Moreover, this retail format is not peripheral but a central channel for the distribution of essential goods: nearly 50 million small-scale stores serve nearly 4 billion consumers worldwide (Fransoo, Escamilla, and Ge, 2024). South Africa ranks among the top ten destination countries by migrant stock (Pew Research Center, 2018), presenting a setting that combines high migrant participation in retail with recurrent economic stress and episodes of xenophobic mobilization—this represents broader global patterns (Scheve and Slaughter, 2001)[1]. Thus, examining how migrant identity shapes demand and pricing here carries broad policy relevance.

Estimating consumer demand for migrant-owned stores is challenging because observational retail data rarely cover small retail stores, record owner nationality, or offer exogenous variation

[1] South Africa is the largest migrant-hosting country in Africa (United Nations, 2020), with migrants concentrated in Johannesburg.

in owner identity. Owner nationality, location, prices, assortment, and store quality are jointly determined, making it difficult to isolate the causal effect of national identity on demand from observational data. A choice-based conjoint experiment is therefore, arguably, the only feasible approach that allows for a clean identification of this causal effect. Choice-based conjoint methods are used in quantitative marketing, economics, and political science when key variables are difficult to randomize or the research is politically sensitive (Fong, Guo, and Rao, 2024; Moshary, Shapiro, and Drango, 2025; Hainmueller, Hopkins, and Yamamoto, 2014).

We implement an incentive-compatible, choice-based conjoint in the field with N=1,974 local consumers. Enumerators approached households door-to-door and endowed each respondent with a real shopping budget of R20 ($1.20). Respondents chose among experimentally generated store profiles that randomized price, brand, and store attributes, including the focal attribute of shopkeeper identity: local South African versus foreign national[2]. To ensure incentive compatibility, respondents were informed that one of their selected choices (out of the multiple choice tasks they completed) would be randomly drawn and the purchase would be executed: the chosen product would be procured from a store matching the chosen profile and sold to them at the chosen price out of the provided budget. Our demand estimation mobilized a field research team to procure and deliver nearly 2,000 products, worth approximately R40,000, over a two-month study period. This design delivers causal identification of demand parameters and rich individual-level choice data that we link to follow-up survey measures of employment status, political and social attitudes, beliefs about migrant-owned stores, and prior exposure to migrants.

Further, we embed an information experiment within the conjoint study to distinguish between taste-based and statistical discrimination in the sense of Becker (1957). Taste-based aversion refers to a utility cost incurred from interacting with members of a specific group. Statistical discrimination arises when consumers use observable group characteristics, such as migrant status, as a proxy for unobserved attributes, such as store quality. To separate these mechanisms, respondents are individually randomized such that 49% of the sample (N=966, *Control* group) complete the conjoint task without store-quality attributes. For this group, any WTP penalty for migrant-owned stores reflects a combination of taste-based aversion and statistical discrimination.

[2]Half of the respondents shop for bread, a perishable good, and half for potato chips, a non-perishable good. The randomization of product type enables us to show that penalties for migrant-owned stores persist across products.

The remaining 51% of respondents (N = 1,007, *Information Treatment* group) are randomized into completing a version of the conjoint that additionally discloses store-quality attributes identified through fieldwork as important to consumers and commonly associated with beliefs about migrant-owned stores. These include: (i) store safety, which captures concerns about personal risk when shopping and reflects documented links between out-group status and threat perceptions (Stephan and Stephan, 2013), and (ii) store services, which capture dimensions of retail quality such as long opening hours and the presence of a WhatsApp channel for customer communication. Qualitative fieldwork indicate that consumers often infer lower safety or lower service quality from migrant ownership. Conditional on this disclosure of quality attributes, any residual WTP penalty for migrant-owned stores more cleanly reflects taste-based aversion.

We find that the overall WTP penalty for migrant-owned stores, estimated in the control group, is R 1.72, or 9.0% of the average product price. Providing information about store quality attenuates the WTP penalty to R1.46 in the treatment group. This residual penalty, which we attribute to taste-based aversion, remains statistically significant at the 1% level and economically meaningful at 7.7% of price. Notably, these effects arise in a low-income sample[3] with real monetary stakes, highlighting that consumers will take personally costly actions to express aversion towards migrants.

To validate our interpretation that the residual penalty reflects taste-based aversion, we provide a placebo check showing it does not vary with stated consumer beliefs about the quality of migrant-owned stores but does vary with their stated anti-immigrant political attitudes. Heterogeneity analysis further shows that the penalty does not attenuate among consumers with stronger economic prospects, as employed and unemployed respondents exhibit similar effects. In contrast, the penalty is smaller among consumers with prior cooperative exposure to a migrant store owner, consistent with the contact hypothesis (Allport, 1954).

The demand-side results imply that migrant-owned stores face a structural disadvantage rooted in an immutable identity that reduces their ability to compete with locally-owned stores at prevailing prices. If these migrant entrepreneurs are rational economic actors, they would adopt compensatory pricing strategies to remain viable in equilibrium.

We test this implication using a census of retail stores in the municipality, covering $N = 432$

[3]The Alexandra municipality in Johannesburg has been described as a "squalid and neglected" area despite it's proximity to high-income municipalities (Reuters, 2019).

establishments. Migrant-owned stores charge substantially lower prices across 3 standard SKUs audited than locally-owned competitors. In the most saturated specification, which includes controls for unit costs, competition, product condition and service quality, customer composition, and granular location fixed effects, migrant stores price 0.412 standard deviations lower than comparable local stores ($p < 0.01$). Parallel specifications using standardized mark-ups yield similar results, indicating compressed margins rather than cost advantages. This translates into profit implications: across audited products, daily profits are 22.7 percent lower for migrant-owned stores.

These findings underscore that some retail markets are shaped by national identity. Reducing anti-migrant biases would therefore alter not only purchasing decisions but also the price structure observed in local retail markets. The rest of the paper is organized as follows: Section 2 develops the conceptual framework, followed by Section 3 describes the empirical setting and research design. Section 4 presents the demand-side evidence from the conjoint experiment. Section 5 examines supply-side responses using the retail census and Section 6 concludes.

1.1 Related Literature

We build and contribute to five streams of literature in economics and marketing. A large literature in economics documents discrimination in labor markets (e.g., Bertrand and Mullainathan, 2004), However, less attention has been paid to discrimination against sellers. The closest antecedents to our paper come from the sharing economy. Laouénan and Rathelot (2022) document that ethnic minority hosts on Airbnb price their listings 3.2 percent below comparable majority hosts within the same neighborhood. Luca, Pronkina, and Rossi (2026) show that anti-Asian bias on Airbnb intensified sharply during the COVID-19 pandemic. We differ from this literature in two ways that are not merely incremental. First, our setting is an offline, high-frequency, essential-goods market and not a peer-to-peer platform for differentiated experience goods. Second, we hold the product constant. In the Airbnb context, by contrast, the listing and the host are inseparable; consumers cannot evaluate a property independently of its owner. Thus, our design allows a clean separation between the dis-utility of the seller's identity and any inference about product quality.

A growing literature emphasizes that observed discrimination may not be driven by animosity but can arise from biased beliefs, statistical discrimination. This literature shows that separating

taste-based from statistical discrimination is an empirically demanding task that generally requires experimental variation in the information available to decision-makers (Bohren, Haggag, and Pope, 2020; Hagmann, Sajons, and Tinsley, 2025). We contribute to this literature by embedding an information treatment into our conjoint design that directly reveals store quality to consumers, this allows for the decomposition the observed demand penalty into a statistical discrimination component — what consumers infer from migrant shops in the absence of information — and a residual taste-based component that persists even when store quality is fully known.

We also relate to work in marketing documenting that consumers use the national origin of a product as a cue for quality, safety, and prestige, the country-of-origin effect (Verlegh and Steenkamp, 1999)[4]. In this literature, origin is an attribute of the product itself, and consumer responses operate through quality inference rather than explicit aversion to a group. Our setting differs in two important respects. First, the products in our choice experiment are (multi-)nationally manufactured branded goods that are stocked near-universally in this market. Importantly, brand is randomized across all choice profiles, so any potential country-of-origin effect of product brands is not confounded with effects of store owner nationality. Second, the penalty we identify reflects explicit aversion to the person behind the counter rather than inference about product quality. This distinction carries direct policy implications. While country-of-origin preferences are difficult to regulate, consumer-biases against sellers is directly relevant for governments seeking to improve the economic conditions of migrants.

A large literature on firm performance in low-income settings has predominantly focused on supply-side barriers to firm growth, such as access to credit, managerial skills, and infrastructure (McKenzie and Woodruff, 2014; Anderson, Chandy, and Zia, 2018). A recent strand has shifted attention to some demand-side constraints, showing that limited consumer access and information frictions suppress the growth of high-quality firms even when supply-side conditions improve (Jensen and Miller, 2018). In contrast, we focus on a new demand-side friction: seller identity. Unlike the barriers studied in the existing literature, consumer bias against a seller's national origin cannot be resolved through access to capital, better information, or managerial training—it operates through identity, and we show it has direct consequences for how migrant-owned firms price and

[4]A related literature in finance documents that investors systematically overweight domestic stocks relative to the predictions of standard portfolio theory — the so-called home bias French and Poterba, 1991; Lewis, 1999)

compete. More broadly, we contribute to a literature on pricing in developing economies, where the determinants of price-setting by small firms remain poorly understood (Norton, 2026).

Finally, we relate to a large literature on intergroup contact theory. Building on Allport, 1954, a large body of work establishes that direct contact between members of different groups reduces prejudice and mistrust. Meta-analytic evidence across 515 studies confirms this pattern across a wide range of contexts, from residential integration to educational settings (Pettigrew and Tropp, 2006), to post-conflict environments (Mousa, 2020). We contribute by extending this literature to a commercial setting. Our exploratory heterogeneity results show that consumers with a prior relationship with a migrant shopkeeper levy a significantly smaller WTP penalty, providing the first evidence that repeated commercial contact may attenuate in-group bias against out-group entrepreneurs.

2 Conceptual Framework

2.1 Demand-Side Model

We develop a simple model of consumer choice to discipline our empirical strategy and clarify the conditions under which observed willingness-to-pay differentials can be attributed to taste-based discrimination (cost incurred by interacting with a specific group such as migrants) versus statistical discrimination (observable group characteristics, such as migrant status, used as a proxy for unobservable characteristics, such as quality). The framework draws on the canonical models of Becker (1957) and Arrow (1973), adapted to a retail market setting in which consumers choose among competing firms with their shopkeepers differing in national origin.

Consumer i's indirect utility from purchasing at store j is:

$$U_{ij} = \beta_0 - \beta_1 P_j + \beta_2 q_j - \beta_3 M_j + \varepsilon_{ij} \tag{1}$$

where $M_j \in \{0,1\}$ indicates migrant ownership, q_j is the unobserved store quality, and ε_{ij} is i.i.d. Type I extreme value. The parameter β_3 captures the utility cost of transacting with a migrant shopkeeper, with $\beta_3 > 0$ indicating taste-based aversion.

2.2 Identification

The key empirical challenge is that q_j is unobserved and may be correlated with M_j. Consumers may hold prior beliefs about store quality that differ systematically by shopkeeper nationality. We denote consumers' subjective expectation of quality conditional on information set $\mathscr{I}$ and migrant status as $\mathbb{E}[q_j \mid \mathscr{I}, M_j]$. In the absence of quality information ($\mathscr{I} = \varnothing$), consumers form priors $\mathbb{E}[q_j \mid \mathscr{I} = \varnothing, M_j = 1] = q^M$ and $\mathbb{E}[q_j \mid \mathscr{I} = \varnothing, M_j = 0] = q^L$, where in general $q^M \neq q^L$. Under statistical discrimination (Arrow, 1973; Phelps, 1972), consumers use shopkeeper nationality as a signal for unobservable quality attributes such as product handling, store conditions, or susceptibility to crime. Any observed penalty in willingness-to-pay would then confound animosity with rational belief updating, rendering naive estimates of β_3 upward biased.

Differencing the expected utility differential across information regimes recovers each component separately. When store quality is revealed, the consumer's quality beliefs equalize across ownership types and the utility differential collapses to $-\beta_3$, i.e., pure taste-based aversion. When quality information is withheld, the differential equals $\beta_2(q^M - q^L) - \beta_3$, conflating taste and statistical discrimination. Differencing across these two regimes recovers each component:

$$\Delta\mathbb{E}[U \mid \mathscr{I} = \varnothing] - \Delta\mathbb{E}[U \mid \mathscr{I} = I] \;=\; \beta_2(q^M - q^L) \tag{2}$$

The penalty that persists after quality revelation identifies β_3, and the attenuation identifies the statistical discrimination component $\beta_2(q^M - q^L)$.[5]

2.3 Implications for Demand-side Estimation: The Conjoint Design

This framework directly motivates our experimental design. By randomizing consumers into conditions with and without verifiable store quality information, we generate exogenous variation in $\mathscr{I}$. The no-information condition identifies the total penalty $-\beta_3 + \beta_2(q^M - q^L)$; the information condition isolates taste-based aversion $-\beta_3$. WTP is computed as β_3/β_1[6].

Note that two considerations discipline our choice of revealed store quality attributes. First,

[5] This decomposition does not require consumer beliefs to be Bayesian or prior quality gaps to be correctly signed. It only requires that revealing store quality information moves consumer beliefs in the direction of the true value.

[6] We estimate standard errors using the delta method.

the attributes must span the empirically relevant support of $\beta_2(q^M - q^L)$: they must correspond to dimensions along which consumers hold systematically different priors about migrant versus local stores. Revealing the exhaustive set of such dimensions is neither feasible nor cognitively tractable in a conjoint design. We therefore select ex-ante store safety and service provision as the quality attributes, which our pre-experiment census and qualitative fieldwork identify as the empirically dominant drivers of asymmetric quality beliefs in this market. Specifically, crime exposure and the modal service amenities of extended opening hours and WhatsApp communication (see Section 3.1).

Second, restricting the number of revealed attributes preserves respondent attention (Rao et al., 2014), avoiding satisficing (Boatwright and Monroe, 2012) and attribute non-attendance (Yegoryan, Guhl, and Klapper, 2020), both of which attenuate estimated preference parameters toward zero and reduce the precision of the decomposition. Restricting the information treatment to these two high-salience dimensions maximizes the power of the statistical-versus-taste decomposition while minimizing cognitive load on participants.

2.4 Supply-Side Model

The consumer preference model of Section 2.1 has direct implications for equilibrium pricing. Suppose that a local retail market contains $N_M \geq 1$ migrant-owned and $N_L \geq 1$ locally-owned stores, all selling the same product at common marginal cost c[7].

Consumer utility follows equation (1). Each store j sets price to maximize profit $\pi_j = (P_j - c)\, s_j$, where s_j is its logit market share, taking rival prices as given. In a symmetric Nash equilibrium, the first-order condition yields the standard logit markup (Anderson, De Palma, and Thisse, 1992):

$$P_j^* \;=\; c \;+\; \frac{1}{\beta_1 \,(1 - s_j^*)}. \tag{3}$$

This setup yields the following propositions.

Proposition 1 (Price penalty) *If $\beta_3 > 0$, then in any interior equilibrium $P_M^* < P_L^*$. If $\beta_3 = 0$, then $P_M^* = P_L^*$.*

[7]Note, we do not maintain this assumption in our empirical analysis of the supply-side. Instead, we measure marginal costs directly and control for them in our specifications.

Proposition 2 (Monotonicity in aversion) *The price gap is strictly increasing in taste-based aversion:*

$$\frac{\partial (P_L^* - P_M^*)}{\partial \beta_3} = \frac{s_M^*}{\beta_1} > 0.$$

Proofs are in Appendix A.

The taste-based penalty acts as a demand tax levied on migrant entrepreneurs by their own customers. Consumer aversion ($\beta_3 > 0$) shifts the migrant store's demand curve inward at any given price, shrinking its equilibrium share. Because the optimal markup in (3) is increasing in share, a smaller share compresses the migrant markup; demand displaced to local stores expands theirs. Migrant owners rationally price below prevailing market prices to recover volume, and the deeper the consumer bias, the deeper the required discount. This distortion is an equilibrium consequence of identity-based consumer preferences that persists as long as those preferences do.

2.5 Implications for Supply-Side Estimation

The propositions deliver a testable predictions.

Prediction 1 (Proposition 1). Within the same market, and conditional on costs and product quality, migrant-owned stores should charge lower prices than comparable local stores. This motivates the price regression below, in which the coefficient on *Migrant* identifies the ownership-identity component of the price gap after controlling for observable cost and quality shifters.

$$P_j = \alpha + \gamma \text{Migrant}_j + \mathbf{x}_j' \phi + \varepsilon_j. \tag{4}$$

3 Background and Research Design

3.1 Empirical Background

South Africa provides a compelling and relevant setting in which to study consumer biases against migrant entrepreneurs. First, small-scale informal grocery stores (locally referred to as spaza shops), serve residential neighborhoods and constitute the backbone of municipality retail, supplying everyday household goods to low-income communities. Approximately half of all spaza shops are

immigrant-owned, with Ethiopian and Somali entrepreneurs representing the largest share of foreign proprietors (Current Affairs, South Africa, 2025). This high density of migrant ownership means that consumers in Alexandra interact with foreign-owned retailers regularly, making our study ecologically valid: choices between local and migrant shopkeepers reflect decisions consumers genuinely face in their day-to-day lives.

With an official unemployment rate exceeding 30%, among the highest in the world and disproportionately concentrated among youth, South Africa's labor market leaves a large share of the population economically marginalized (World Population Review 2025). This lack of resources and an unstable economic environment has contributed to rising tensions between South African and foreign nationals, as immigrant communities are perceived as competing for economic opportunities (Nyam, Kunguma, and Selelo, 2025). The link between economic distress and out-group hostility is well-documented including in western countries as the USA and UK (Alesina and Tabellini, 2024a). South Africa is exemplifies this global and common pattern.

As a result of these economic frictions, anti-immigrant sentiment has periodically erupted into organized violence. Major waves of xenophobic attacks in 2008 and 2015 caused widespread vandalism of migrant-owned businesses, and in 2022 a far-right movement (Operation Dudula) explicitly targeted foreign nationals and their businesses, marking a further escalation of hostility (BBC News, 2015; BBC News, 2015; Reuters, 2022). These episodes reflect a broader global pattern documented across countries in which economic precarity and labor market stress amplify out-group bias, including against immigrant-owned businesses (Scheve and Slaughter, 2001). The recurrence of this hostility suggests that xenophobic attitudes are not latent but actively expressed.

South Africa thus offers a suitable setting in which the structural conditions for consumer discrimination against migrant shopkeepers are present in force, making it possible to quantify the extent of consumer biases and their implications for migrant firm performance. We focus on Alexandra, a local municipality in northeastern Johannesburg. Alexandra is home to over 500,000, predominantly black populations inhabitants who live in an area of 7.6 km^2.

3.2 Demand-Side Migrant Penalties: Conjoint Experiment

In the conjoint experiment, we measure consumer biases against migrants versus local store owners. We conducted the conjoint experiment with 1,974 consumers. We outline the details on product choice, conjoint design, information treatment design, and incentive compatibility subsequently.

3.2.1 Choice of Product

We identified two common product categories - white bread and potato chips - as suitable products for the conjoint experiment. Conceptually, the product categories reflect perishable (bread) and non-perishable products (chips).

For each product, we identified three locally-sold brands. The selection of product type and brand rests on three considerations: (a) both categories are frequently purchased by local consumers, as documented in South African consumer reports; (b) both are commonly stocked at local small retailers; and (c) their prevalence at local SMEs was confirmed through three rounds of qualitative fieldwork.

3.2.2 Conjoint & Treatment Design

We deployed a forced-choice scenario in which consumers selected their most preferred option from four alternatives. This design requires respondents to consider relative trade-offs among options rather than making absolute judgments, a key advantage for recovering willingness-to-pay estimates. As summarized in Table 1, each choice set always included the same three attributes: shop owner nationality — the focal attribute, product price and product brand.[8]

- *Focal Attribute: Shopkeeper Nationality* consisted of four attribute levels: South African(local), Ethiopian (migrant), Somalian (migrant) and Malawian (migrant). These nationalities were selected based on extensive qualitative field work to reflect which nationalities most commonly manage local retail stores. To reduce the salience of nationality, we also included a common surname for each nationality.

[8]We keep product quality constant across profiles (i.e., identical brands/products), since manipulating quality in an incentive-compatible design would require distributing inferior or potentially unsafe items.

- *Attribute 2: Price* consisted of five price levels ranging from R18 to R20, with a 0.5 increments.[9] These prices were benchmarked against market prices for the selected brands.

- *Attribute 3: Brand* consisted of three levels. For bread, we included Sasko, Albany and BR. For potato chips, we included Simba, Lays, and Taits. The prevalence of these brands at local small retailers was verified through fieldwork.

Table 1: Overview of Attribute Levels in the Control Condition

	White Bread	**Potato Chips**
Nationality	Ethiopian / Somalian / Malawian / South African	Ethiopian / Somalian / Malawian / South African
Price	18 to 20 (Δ=0.5)	18 to 20 (Δ=0.5)
Brand	Albany / Sasko / BR	Tait's / Lays / Simba

To disentangle statistical discrimination from taste-based animosity, we implemented a *store quality information* treatment condition. Participants randomized into the treatment group received information about the store quality - specifically about store services and store safety -, whereas those randomized into control group received no such information. Treatment randomization was cross-randomized within product type. Figure 2 provides an overview of the attributes included in the treatment condition.

- *Attribute 4 (treatment only): Store Services* consists of four levels: no store services; a customer WhatsApp channel; long opening hours; or both long opening hours or both a WhatsApp channel and long opening hours. We clarified to consumers that the WhatsApp channel is a channel through which the local retailer could provide information about product stocking and promotions and that long opening hours refers to hours of 5am until 10:30pm.

- *Attribute 5 (treatment only): Store Safety* consists of three levels: no safety incidents in last month, targeted by crime in the last month, and involved in crime in the last month. These latter levels distinguish whether the shopkeeper was a victim or a perpetrator of crime. That said, from the consumers' perspective, both may signal heightened personal risk.

[9] 1 RAND equals 0.062 USD at the time of the study

Table 2: Overview of Attribute Levels in the Treatment Condition

	White Bread	**Potato Chips**
Nationality	Ethiopian / Somalian / Malawian / South African	Ethiopian / Somalian / Malawian / South African
Price	18 to 20 (Δ=0.5)	18 to 20 (Δ=0.5)
Brand	Albany / Sasko / BR	Tait's / Lays / Simba
Store Services	None / WhatsApp channel / Long hours (5:00–22:30) / Both	None / WhatsApp channel / Long hours (5:00–22:30) / Both
Store Safety	None vs. Targeted by crime last month vs. Involved in crime last month	None vs. Targeted by crime last month vs. Involved in crime last month

Conditional on experimental arm, each respondent was provided with 8 or 12 choice tasks. Within each task, shopkeeper nationality and non-focal attributes were randomized. This randomization facilitates identification of attribute trade-offs and yields exogenous variation in price and shopkeeper nationality for estimating attribute-level utilities.

Respondents were explicitly informed that distance is held constant across all profiles. To rule out the possibility that the migrant-ownership cue proxies for neighborhood-level characteristics correlated with store location, we verify that migrant-owned retailers are not spatially concentrated using geo-referenced data from the retailer census (see Section 4.2 for the robustness checks).

Because the included product categories are common household staples and are routinely purchased in these local small-retail stores we study, our objective is not to estimate category incidence or the overall propensity to purchase. Instead, we focus on relative trade-offs among product attributes conditional on purchase. We therefore use a forced-choice conjoint without a 'no-choice' option. Including an outside option in this setting would primarily capture timing or situational factors (e.g., 'not today') rather than preferences over attributes, and would yield low-information responses that do not identify trade-offs across profiles, reducing statistical efficiency.

3.2.3 Incentive-Compatible Design

To ensure that the conjoint experiment elicits participants' true preferences, the conjoint experiment is designed to be incentive-compatible (Green and Srinivasan, 1978; Green and Srinivasan, 1990; Ding, Grewal, and Liechty, 2005). Following best practices in marketing research, when giving

consumer a shopping budget, we drew one of their choices at random as their actual purchase (Ding, Grewal, and Liechty, 2005; Fong, Guo, and Rao, 2024).

Before completing the conjoint choice tasks, participants were informed that they would get a shopping budget of R20 that one randomly selected choice would be fulfilled at the chosen price from a comparable store, with the remaining change provided to them. For example, if a participant had selected Lays potato chips at R18, the respondent received the Lays chips and R2. All participants also received a R20 participation payment, equivalent to approximately one hour of minimum wage work in South Africa. Figure 1 shows the text explaining the incentive compatibility to the participant. Following data collection, we conducted verification checks with a randomly selected 20% of participants to confirm receipt of the promised product and change. and the change.

The design involves a necessary and limited element of deception. Although one randomly chosen option was fulfilled as an actual purchase, the product was not sourced from that specific shopkeeper — we could not, for instance, simulate store safety conditions in the actual fulfillment. Participants were explicitly informed in the briefing that the product would be purchased from a store that matches the profile listed in the option. This disclosure is standard in incentive-compatible conjoint design and is ethically required.[10] Crucially, it preserves the design's key property: participants had skin in the game when making their choices and therefore had a financial incentive to reveal true preferences. Importantly, the comparable store used to fulfill the chosen option reflects the typical retail environment participants shop in every day—a store similar to theirs, stocking the same branded products at similar prices. The deception is therefore narrow: it concerns which specific shopkeeper fulfills the order, not the nature of the retail environment itself.

One concern that might arise is that participants never have to interact in person with a migrant shopkeeper—they view a profile on a screen and make a choice. The physical co-presence and direct social exchange that characterize in-person retail transactions are entirely absent. These are precisely the conditions under which taste-based aversion become most salient and affectively charged. If anything, our design attenuates the very mechanisms that drive consumer aversion in real markets. Yet even under these attenuated conditions, we find a sizeable and statistically significant willingness-to-pay penalty. This makes our estimates a lower bound. In actual retail encounters, where the shopkeeper's identity is physically present, the penalty consumers levy on

[10]IRB was secured from Cornell University, IRB0148974.

migrant shopkeepers is plausibly larger than what we document here.

3.2.4 Implementation of Conjoint Survey Experiment

The conjoint experiment interface was built on Sawtooth. Figure 2 presents an example of the interface shown to consumers in the treatment condition. To prevent duplicate participation, we conducted weekly verification checks using phone-number and address matching.

To ensure geographic overlap with supply-side data, we restricted the sample to Alexandra municipality in Johannesburg, South Africa. The conjoint survey experiments were conducted by a team of trained local enumerators who systematically covered Alexandra municipality over the period: May–June 2025 for the pilot experiment and August–October 2025 for the full-scale experiment. To ensure full geographic coverage while avoiding duplicate participation, the municipality was mapped and divided into enumeration zones, with each enumerator assigned an exclusive street or block. Within their assigned area, enumerators approached residents directly—both by knocking on household doors and by intercepting individuals on the street—and screened them for eligibility. This approach ensured full coverage of the entire municipality.

The participant eligibility for each study was restricted by the following sampling requirements: a) above 18 years old, b) living in the local municipality, Alexandra, c) have been to a small-scale retail store to purchase a product in the past, and d) South-African citizen.

The restriction to South African citizens is deliberate. Our framework identifies a demand-side penalty that host-country consumers levy on migrant sellers; including migrant consumers would conflate this with potential co-ethnic preferences, a conceptually distinct mechanism. Empirically, our supply-side census confirms that South African nationals constitute the dominant customer base: both locally-owned and migrant-owned stores report that approximately 75–76 percent of their customers are South African, with no significant difference across ownership types (see Section 3.3). South African consumers thus represent the primary source of retail demand for migrant entrepreneurs in this market. Characterizing the preferences of this group is therefore of first-order importance for understanding the demand environment migrant-owned firms face.

Table 1 reports summary statistics. The average participant is 36.5 years old (median age is 25), 43.1% is male, and 82.1% has completed secondary school as their highest level of education. Furthermore, Table 1 and 2 also report covariate balance across conditions; we find no evidence of

differential assignment on observables.

Refusal rates were not systematically recorded. To ensure the achieved sample is broadly representative of the local population, we balanced the sample on three key characteristics: gender, age, and employment status. The latter warrants particular attention in this context: a substantial share of Alexandra residents are job-seeking (43.74%), informally employed (12.27%) or self-employed (10.92%), and their working hours do not conform to standard schedules.

3.2.5 Empirical Specification

We estimate a conditional logit model in which consumer i's utility from alternative j in choice task t is a linear function of the experimentally randomized attributes: price, a migrant-ownership indicator, brand dummies, and—in the information condition—store service and store safety attributes. The migrant indicator is interacted with the information treatment assignment to test whether the penalty attenuates when store quality is revealed. Under the conceptual framework of Section 2, the migrant coefficient in the no-information condition captures the total penalty (taste-based plus statistical discrimination), while the coefficient in the information condition isolates the taste-based component. We convert utility penalties into willingness-to-pay (WTP) by dividing the migrant coefficient by the price coefficient, with delta-method standard errors. Appendix D provides the full specification and derivations.

3.3 Supply-Side Response: Census of Retail Firms

In parallel, we seek to understand how demand-side bias against migrant shopkeepers transmits to pricing on the supply side. We conducted a census of 432 small-scale retailers operating in the same location as the conjoint experiment: Alexandra, Johannesburg (South Africa).

3.3.1 Census Design: Price Check

To capture differences in pricing behavior by migrant status of shopkeepers, we conducted a secret shopper exercise in which enumerators requested the shelf price of three products common to this retail context: a one-liter Coca-Cola, a standard loaf of Albany white bread, and a single pack of Topper biscuits. These branded products are widely stocked and comparable across retailers;

85.65% of surveyed retailers stocked all three. By eliciting actual transaction prices rather than self-reported figures, this approach ensures that our data reflect real market prices. We interpret resulting price differences as reflecting pricing on a benchmark basket of staples.

3.3.2 Census Design: Business Audit

If the respondent subsequently consented to a business audit (94.35% consented), we then collected a comprehensive set of store-level characteristics to adjudicate among alternative explanations for any observed price differentials between migrant- and non-migrant–owned stores. If migrant shopkeepers face a downward-shifted demand curve due to identity-based consumer bias, strategic price-setting implies lower market prices. However, observed lower prices could also reflect cost advantages, differences in location-based competition, quality differences (even as SKU is held constant, storage conditions may differ), or demand composition at the micro-market level.

First, to assess cost differences, we measured unit costs to retailers for the three products, as well as access to wholesalers, delivery arrangements, and purchase quantities.

Second, to capture the competitive landscape each store faces, we combined self-reported and geo-coded measures. Owners reported the number of nearby competitors. We also recorded GPS coordinates for each store and computed spatial measures of local competitive density, including distance to the nearest mall which contained supermarkets and the number of retailers within predefined radii.

Third, to rule out quality-based explanations, we directly assessed observable product and store quality. Enumerators rated product storage conditions (Likert 1-5). For store quality, we constructed a count measure of services provided from a comprehensive checklist of services in this market: has long opening hours, WhatsApp communication channels, sells on credit, holds keys, charges phones, offers digital payments, and provides free samples.

Fourth, to account for heterogeneous demand elasticities, we elicited the composition of each store's customer base: the share of local versus migrant customers. If migrant-owned stores disproportionately serve migrant customers with different price sensitivity, this channel could generate equilibrium price differences absent bias.

Fifth, we measured additional micro-market characteristics that may shape foot traffic and demand, including detailed street position (e.g., intersection type, square, informal road), proximity

to landmarks such as bars, migrant hostels, clinics, churches, and police offices. These data allow us to control flexibly for spatial demand conditions.

3.3.3 Implementation of Census

A team of locally recruited and trained enumerators administered the audits between May–June 2025, covering the full geographic extent of Alexandra municipality. Enumerators approached all identifiable small-scale retailers in their assigned zones and invited them to participate. See Figure 3 for a map of the retailers included in the census in the assigned area. Participating retailers received R50 as compensation for their time; the audit required approximately 40 minutes to complete.

We restricted the sample to small grocery retailers, selling common food and beverage products. We did so to ensure comparability in product assortment across retailers. Eligible respondents were either the shop owner or the primary decision-maker responsible for pricing and procurement.[11] No restrictions were imposed on gender, age, or education. In total, 94.35% of small-scale retailers in Alexandra participated in the audit. For retailers who declined to participate, enumerators nonetheless conducted the secret shopper price check and recorded observable store characteristics, such as product quality, ensuring that non-participation does not result in complete data loss for these observations.

Among audited retail firms, 48.8% of owners originate from Ethiopia, 32.6% from South Africa, and 11.1% from Somalia (see Table 5). Table 6 reports summary statistics for the retailer sample. The average store in our sample has been in operation for an average of 11 years (median: 8 years), indicating an established rather than nascent retail sector. The sample is predominantly male, only 14.4% of shops are run by women. The vast majority of shops (88.9%) employ no more than two additional workers, confirming the micro-enterprise character of the sector.

[11] In our setting, ownership and day-to-day customer interaction largely coincide: in 93.66% of shops, the shop owner and the shopkeeper share the same nationality.

4 Results

4.1 Demand-side Penalties for Migrant-Owned Stores

We present estimates of consumers' willingness to pay to avoid migrant-owned stores in Table 3. Column (1) reports results for the control group (N = 966), where consumers do not observe store quality information. This specification captures the overall penalty, which combines statistical discrimination based on inferred quality with taste-based aversion. Column (2) reports results for the information treatment group (N = 1,007), where store services and safety are disclosed. This specification isolates the residual, taste-based penalty. Column (3) pools both samples (N = 1,973) and interacts the migrant indicator (as well as price) with the treatment assignment to formally test whether the penalty attenuates when quality information is revealed.

Across specifications, the coefficient on *Migrant* is negative and statistically significant. In the pooled model, the implied overall willingness-to-pay penalty in the absence of information equals R 1.71. Given market price ranges, this corresponds to roughly 9 percent of the product price (R 19). When store quality is revealed, the penalty attenuates in magnitude. The residual taste-based penalty equals R 1.46, or roughly 8 percent of price. However, the pooled model fails to reject equality of the two WTP estimates. This suggests that statistical discrimination plays a negligible role in explaining preferences against migrants — taste-based aversion seems to drive the bulk of this effect.

Placebo Mechanism Tests. Table 4 probes whether the residual penalty in the information condition truly reflects taste-based aversion. We test this with two placebo checks. All specifications restrict the sample to the treatment group ($N = 1{,}007$), where store services and safety are disclosed.

Column (1) interacts the Migrant attribute with an indicator for respondents who endorse anti-migrant statements. Among respondents who do not express anti-migrant attitudes ($X_{HTE} = 0$), the willingness-to-pay penalty equals R 0.894. Among respondents who do express such attitudes ($X_{HTE} = 1$), the penalty increases to R 1.720. The interaction term is negative and statistically significant, and we reject equality of the two WTP estimates. The taste-based penalty is therefore substantially larger among respondents who explicitly hold anti-migrant views. This pattern is consistent with a taste-based account in which animus toward migrants directly lowers utility from transacting with them.

Table 3: Conditional Logit Estimates and Willingness-to-Pay for Migrant-Owned Stores

	(1) Control Group Only	(2) Treatment Group Only	(3) Pooled Sample
Panel A: Logit Coefficients			
Focal Attributes			
Migrant	-1.185***	-0.463***	-1.177***
	(0.044)	(0.030)	(0.043)
Migrant × InfoTreat			0.712***
			(0.053)
Price	-0.693***	-0.317***	-0.689***
	(0.030)	(0.019)	(0.030)
Price × InfoTreat			0.371***
			(0.036)
Store Service (Base: No Services)			
Long Hours		0.532***	0.534***
		(0.034)	(0.034)
Whatsapp Channel		0.169***	0.170***
		(0.028)	(0.028)
Whatsapp Channel and Long Hours		0.632***	0.634***
		(0.038)	(0.038)
Store Safety (Base: No Crime)			
Involved in Crime		-1.807***	-1.813***
		(0.056)	(0.056)
Targeted by Crime		-1.129***	-1.133***
		(0.038)	(0.038)
Panel B: Willingness-to-Pay			
Migrant WTP (InfoTreat= 0)	-1.709***	—	-1.708***
	(0.100)		(0.100)
Migrant WTP (InfoTreat= 1)	—	-1.458***	-1.461***
		(0.135)	(0.135)
p-value: WTP(0) = WTP(1)	—	—	0.142
Brand Dummies	Y	Y	Y
$N_{consumers}$	966	1007	1973
N_{tasks}	8694	14098	22792

Notes: This table presents the conditional logit model estimates of Equation . Robust standard errors clustered at the consumer-level are in parentheses. The pooled specification interacts the information treatment with price, migrant status, store service, and store safety attributes, allowing all preference parameters to differ across arms. WTP in each arm is computed as $-\hat{\beta}^{(t)}_{Migrant}/\hat{\beta}^{(t)}_{Price}$, where superscript $t \in \{0,1\}$ indexes the arm-specific coefficients recovered from the pooled model; standard errors use the delta method. The p-value for WTP(0) = WTP(1) is from a Wald test on the pooled estimates. Stars denote $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Column (2) instead interacts the Migrant attribute with unfavorable beliefs about migrant store quality. If the information treatment successfully isolates pure taste-based aversion, the residual penalty should not vary with these beliefs. Consistent with this logic, the interaction term is statistically insignificant, and we fail to reject equality of WTP across belief groups ($p = 0.232$). The implied penalties are similar in magnitude: R 1.360 for respondents without unfavorable quality beliefs and R 1.599 for those with such beliefs. Together, these tests reinforce that the residual penalty captures direct disutility from migrant ownership rather than lingering differences in perceived store quality.

Table 4: Placebo Check: Taste-Based Migrant Penalty by Anti-Migrant Attitudes and Quality Beliefs

	(1) X_{HTE}: Anti-Migrant Attitudes	(2) X_{HTE}: Unfavorable Quality Beliefs
Panel A: Logit Coefficients		
Focal Attributes		
Migrant	-0.284***	-0.432***
	(0.042)	(0.039)
Migrant $\times$ X_{HTE}	-0.262***	-0.076
	(0.058)	(0.063)
Price	-0.318***	-0.317***
	(0.019)	(0.019)
Panel B: Willingness-to-Pay		
Migrant WTP ($X_{HTE} = 0$)	-0.894***	-1.360***
	(0.142)	(0.152)
Migrant WTP ($X_{HTE} = 1$)	-1.720***	-1.599***
	(0.171)	(0.188)
p-value: WTP(0) = WTP(1)	<1e-4	0.232
Brand Dummies	Y	Y
Store Service Levels	Y	Y
Store Safety Levels	Y	Y
$N_{consumers}$	1007	1007
N_{tasks}	14098	14098

Notes: This table presents conditional logit estimates in the information condition (InfoTreat=1). Robust standard errors clustered at the consumer-level are in parentheses. WTP is computed as $-\beta_{Migrant}/\beta_{Price}$ for $X_{HTE} = 0$ and as $-(\beta_{Migrant} + \beta_{Migrant \times X_{HTE}})/\beta_{Price}$ for $X_{HTE} = 1$, with delta-method standard errors. Stars denote $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Robustness. In Appendix B, Table 3 we replicate our primary pooled specification in Column

(3) separately for the perishable (bread) and non-perishable product (chips). The primary purpose of this is to explore whether the taste-based penalty still persists for the non-perishable product, where we expect product storage beliefs to matter less in consumer choice. We find very similar taste-based penalties across both samples, suggesting that this aversion generalizes across products.

In Appendix C, Table 4 we ask whether our demand-side estimates are consistent with the prices actually observed in the supply-side census. This analysis proceeds as follows: in real markets consumers observe migrant ownership without the verifiable quality disclosure provided in our information treatment, so the no-information WTP—which conflates taste-based with statistical discrimination—is the margin that should govern equilibrium pricing. We feed the control-arm estimates for the perishable (bread) product ($\hat{\beta}_p = 0.655$, $\hat{\beta}_3 = 1.134$, WTP(0) = R1.73) into the logit Bertrand equilibrium of Section 2.4, using observed costs and self-reported competitor counts to construct market structure. Equalizing costs per what we find in Table 6, the model predicts a price gap of R0.30, compared to an observed gap of R0.22. We view this as reassuring evidence that the demand-side preference estimates are broadly consistent with observed supplier behavior.

Heterogeneity in Migrant Penalty. Table 5 examines whether the residual taste-based penalty in the information condition varies with economic vulnerability and prior contact, two prominent explanations in the political economy of anti-immigrant attitudes. All specifications continue to restrict the sample to the treatment group ($N = 1{,}007$).

Column (1) interacts the Migrant attribute with an indicator for being unemployed. If economic threat drives anti-immigrant preferences, the taste-based penalty should be larger among economically vulnerable respondents. We do not find evidence consistent with this prediction. The interaction term is small and statistically insignificant, and we fail to reject equality of WTP across groups ($p = 0.784$).

Column (2) instead interacts the Migrant attribute with an indicator for having no prior cooperative contact with migrants. Here, we find strong heterogeneity. Among respondents with prior cooperative contact ($X_{HTE} = 0$), the taste-based penalty equals R 1.153. Among those without such contact ($X_{HTE} = 1$), the penalty rises to R 1.992. The interaction term is negative and statistically significant, and we reject equality of the two WTP estimates. This pattern is consistent with the contact hypothesis: prior cooperative interaction attenuates the disutility associated with migrant ownership, while the absence of contact substantially magnifies it.

Table 5: HTE: Taste-Based Migrant Penalty by Economic Status Threats and Cooperative Prior Contact

	(1) X_{HTE}: Is Unemployed	(2) X_{HTE}: No Cooperative Contact w. Migrant
Panel A: Logit Coefficients		
Focal Attributes		
Migrant	-0.473***	-0.366***
	(0.048)	(0.037)
Migrant $\times$ X_{HTE}	0.017	-0.267***
	(0.063)	(0.065)
Price	-0.317***	-0.318***
	(0.019)	(0.019)
Panel B: Willingness-to-Pay		
Migrant WTP ($X_{HTE} = 0$)	-1.490***	-1.153***
	(0.185)	(0.139)
Migrant WTP ($X_{HTE} = 1$)	-1.436***	-1.992***
	(0.154)	(0.212)
p-value: WTP(0) = WTP(1)	0.784	<1e-4
Brand Dummies	Y	Y
Store Service Levels	Y	Y
Store Safety Levels	Y	Y
$N_{consumers}$	1007	1007
N_{tasks}	14098	14098

Notes: This table presents conditional logit estimates in the information condition (InfoTreat=1). Robust standard errors clustered at the consumer-level are in parentheses. WTP is computed as $-\beta_{Migrant}/\beta_{Price}$ for $X_{HTE} = 0$ and as $-(\beta_{Migrant} + \beta_{Migrant \times X_{HTE}})/\beta_{Price}$ for $X_{HTE} = 1$, with delta-method standard errors. Stars denote $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Summary. We estimate a large and statistically significant willingness-to-pay penalty to avoid migrant-owned stores. Economically, this implies that the demand curve facing migrant entrepreneurs is shifted downward relative to local competitors, all else held equal. A downward demand shift has direct implications for competition and pricing. We therefore turn to the supply side to test whether migrant-owned stores offset this demand disadvantage through systematic pricing differences.

4.2 Supply-Side Analysis

We first provide basic, descriptive evidence of price differentials based on migrant status. Figure 1 plots standardized unit prices by store ownership. Migrant-owned stores charge prices that are 0.49 standard deviations lower than local stores ($p < 0.001$). This raw price gap is economically large and consistent with compensatory pricing in response to a downward shift in the demand curve documented on the demand side.

Figure 1: Raw Price Differences at Migrant-Owned vs. Locally-Owned Retail Stores



We next assess whether this price differential reflects underlying cost, competition, or quality differences. We find no evidence of cost-based explanations. Unit costs are statistically indistinguishable across migrant and local stores ($p = 0.859$), and the number of nearby competitors is nearly identical ($p = 0.858$). Observable, objective store quality metrics also don't offer a credible

alternative explanation for this price differential. Migrant-owned stores exhibit *higher* product condition ratings and a greater number of verified amenities (both $p < 0.001$). The share of local customers does not differ significantly across ownership types ($p = 0.171$). Taken together, the evidence indicates that migrant entrepreneurs charge lower prices despite similar costs and competitive environments, and in the presence of equal or stronger observable quality provision.

Table 6: Price Differential for Migrant vs. Local Stores and Potential Drivers

	Local mean	Migrant mean	Diff	SE	p
Observed Price Differential					
Unit price (std.)	0.342	-0.148	0.490***	0.063	0.000
Potential (Alternative) Drivers					
Unit cost (std.)	0.008	-0.003	0.011	0.062	0.859
Number of competitors	4.695	4.598	0.097	0.544	0.858
Product condition rating (std.)	-0.364	0.157	-0.521***	0.063	0.000
Number of verified amenities	3.298	3.704	-0.407***	0.078	0.000
% of local customers	76.929	75.423	1.506	1.099	0.171

Conditional Price Regressions. Table 7 formally estimates whether migrant-owned stores charge lower prices conditional on observable characteristics. Column (1) replicates the raw gap: migrant stores charge prices that are 0.49 standard deviations lower than local stores.

Columns (2) through (6) progressively add controls for unit costs, competitive intensity, product condition and service quality, customer composition, and granular location fixed effects. The migrant price differential remains negative, large, and statistically significant in every specification. Even in the most saturated model with market fixed effects and detailed spatial controls, migrant-owned stores charge prices that are 0.41 standard deviations lower than comparable local stores.

Observable cost and market structure variables behave as expected. Higher unit costs are associated with higher prices, and more competitors are associated with slightly lower prices. The share of local customers loses significance once location controls are introduced.

Table 7: Do migrant-owned retail stores price lower, all else held equal?

	DV: Unit Price (Standardized)					
	(1) No controls	(2) + Unit Cost	(3) + Competition	(4) + Product & Service Qual.	(5) + Local share (%) of customers	(6) + Location Controls
Migrant Store	-0.490***	-0.487***	-0.488***	-0.473***	-0.460***	-0.412***
	(0.063)	(0.060)	(0.060)	(0.067)	(0.066)	(0.058)
Unit cost (std.)		0.240***	0.239***	0.242***	0.228***	0.136***
		(0.081)	(0.081)	(0.080)	(0.078)	(0.046)
Number of competitors			-0.003*	-0.003*	-0.003*	-0.005**
			(0.002)	(0.002)	(0.002)	(0.002)
Product condition rating (std.)				-0.015	-0.021	0.004
				(0.035)	(0.035)	(0.028)
Number of customer services				-0.016	-0.022	-0.049***
				(0.023)	(0.022)	(0.019)
% of local customers					0.004**	-0.001
					(0.002)	(0.002)
Observations	1218	1217	1217	1217	1217	1194
R^2	0.051	0.108	0.109	0.110	0.114	0.151
Adjusted R^2	0.050	0.107	0.107	0.106	0.110	0.127

Robust standard errors in parentheses. Model (6) includes granular location controls that are suppressed from display: 18 market FE, street-position dummies (on a street with no crossings; on a square; 4-way cross; 3-way cross; informal road between houses), landmark indicators (shebeen/bar, migrant hostel, clinic/hospital, dancing club, church, police office), and distance to the mall (km). * p¡0.10, ** p¡0.05, *** p¡0.01.

Taken together, the persistence of the migrant coefficient across specifications indicates that lower prices are not explained by unit costs, competition, observable quality, customer mix, or micro-market conditions. Instead, migrant-owned stores appear to price lower, all else held equal, consistent with compensatory pricing in response to lower demand. We replicate the analysis using standardized mark-ups in Table 8 as the dependent variable and obtain parallel results: migrant-owned stores exhibit significantly lower mark-ups across specifications. This carries profit implications. In addition to P and MC, we measured daily Q to compute profits. Across these three products audited, daily profits are 22.7% lower $(p < 0.05)$ for migrant-owned stores.

4.2.1 Robustness Check: Spatial Concentration

A threat to identification is that migrant-owned retailers could be spatially concentrated in particular neighborhoods, so that the estimated price differential reflects location-specific demand or cost conditions rather than compensatory pricing. We assess this concern using geo-referenced data from the retailer census ($N = 430$) and multiple definitions of locality (Appendix E). Nearest-neighbor distances are statistically indistinguishable across ownership types (55 vs. 52 meters, $p = 0.637$), indicating that foreign and local retailers operate in equally dense micro-environments. The Dissimilarity Index (Massey, 1991), the share of one group that would need to relocate across neighborhoods to equalize the two groups' spatial distributions, is 0.208, well below the conventional low-segregation threshold of 0.30. Four of six landmark types recorded during the census show no significant difference in proximity between foreign and local shops: shebeens/bars ($p = 0.748$), migrant hostels ($p = 0.650$), churches ($p = 1.000$), and police offices ($p = 0.985$); two show modest differences (clinics/hospitals, $p = 0.010$; dancing clubs, $p = 0.041$). Street position does not differ significantly between foreign and local retailers (χ^2 test, $p = 0.478$). Crucially, the foreign-price coefficient is stable across specifications with and without granular spatial controls (Table 7), ruling out spatial sorting as a driver of the estimated price differential.

5 CONCLUSION

Our paper provides the first causal estimates of consumer-side discrimination against migrant retail entrepreneurs, and its upstream consequences for market prices. In doing so, we disentangle whether

the nature of this discrimination is driven by underlying statistical and taste-based motives. Using two incentive-compatible conjoint experiments with nearly 2,000 consumers, we document that consumers display a 8-9% lower willingness-to-pay at a migrant shopkeeper as compared to an otherwise identical local. Providing information about store safety and services offered, slightly attenuates this penalty (though not significantly), and leaves a significant residual that is invariant to product perishability - consistent with pure taste-based bias rather than rational inference.

This demand-side penalty has observable supply-side consequences: our census of 432 retailers shows that migrant-run stores price roughly 0.41 standard deviations lower than stores run by locals, after controlling for input costs, competition, location, customer composition, product condition and service amenities, providing independent market-based validation.

Heterogeneity analysis further supports the taste-based account. Results show that the residual penalty rises with explicit anti-immigrant attitudes and is unrelated to unemployment status (weighing against a simple economic-threat mechanism). In line with contact theory (Allport, 1954), we find that the bias is significantly attenuated among consumers with prior cooperative contact with migrant shops. To the best of our knowledge, this is the first study to extend the contact hypothesis to a business context. Taken together, these findings suggest that the identity of the seller, not just the quality of the product, shapes equilibrium retail prices, and that consumer bias constitutes a meaningful and underappreciated constraint on migrant firm performance.

The implications of our estimates are substantial. A 8-9% willingness-to-pay penalty, sustained across the full range of everyday purchases that small-scale retail shops supply, represents a significant and ongoing tax on migrant entrepreneurs. Because this penalty is not offset by corresponding cost advantages - migrant and local retailers face similar input costs and competitive environments - it directly compresses migrant profit margins, potentially constraining the capital accumulation and business investment that are required for sustained growth of small firms. Note that the supply-side price discount documented in our paper reflects migrant shopkeepers' more aggressive price competition to offset consumer aversion, consistent with profit-maximizing behavior. Thus, policy frameworks that focus exclusively on supply-side barriers to migrant entrepreneurship, such as access to credit, regulatory compliance, or business training, will miss a critical demand-side friction that limits returns to these investments.

Two policy channels emerge from our findings. First, our heterogeneity results suggest that

interventions that increase positive inter-group exposure - whether through community programs, shared commercial spaces, or information campaigns that highlight the contributions of migrant-owned businesses - may attenuate taste-based penalties over time. This is consistent with a broader literature showing that contact reduces prejudice in labor, housing, and political contexts (Alesina and Tabellini, 2024b). The retail setting is particularly promising because it is one in which contact is already regular and transactional – which prior theory suggests should be among the most effective conditions for prejudice reduction (Allport, 1954; Pettigrew and Tropp, 2006). The policy challenge is to structure these interactions so that they build familiarity rather than reinforce distance. Second, our decomposition of statistical from taste-based discrimination has direct implications for the design of information interventions. We show that transparency about store safety and service quality partially closes the penalty gap, suggesting that credible certification schemes or public safety record disclosure could reduce the statistical discrimination component for consumers who are not yet convinced that migrant-owned stores meet local quality standards.

Several caveats warrant mention. Our study is conducted in a single municipality in a context characterized by high and volatile anti-immigrant sentiment. Whether the magnitude of the penalty generalizes to other countries or less politically salient immigrant groups is an open empirical question.

Our demand-side estimates likely represent a lower bound on the true consumer penalty. In our conjoint experiment, participants made choices by viewing profiles on a screen and thus, they never had to physically interact with the shopkeepers or enter the actual stores. The physical co-presence, social exchange, and potential discomfort that characterize actual in-person retail transactions are entirely absent from our design. These face-to-face interactions are precisely the conditions under which taste-based aversion becomes most salient. The fact that we find a sizeable and statistically significant willingness-to-pay penalty even under these attenuated conditions suggests that in actual retail encounters, where the shopkeeper's identity is physically present and social interaction is required, the penalty consumers levy on migrant shopkeepers is plausibly larger than what we document here.

Additionally, our demand-side experiment samples only South African consumers. As documented in Section 3.3, this group constitutes approximately three-quarters of the customer base at both migrant-owned and locally-owned retailers and thus represents the primary demand-side force

shaping migrant firm performance. However, the remaining quarter of customers - migrants themselves - may exhibit systematically different preferences, including co-ethnic loyalty or favoritism towards migrant-owned stores. Whether such preferences partially offset the host-country consumer penalty we document, and how they interact with the competitive dynamics between migrant and local retailers, are important open questions for future research.

Although we verify that immigrant-owned and local retailers are not geographically concentrated, and show that the migrant penalty does not attenuate for non-perishable products (where quality and storage concerns should be less salient), a limitation is that we restrict the number of attributes to keep tasks tractable and preserve statistical power for our primary estimand—consumer bias against migrant-owned stores. As a result, we do not experimentally vary additional dimensions, such as product quality signals or local geographic embeddedness, that could further disentangle migrant status from other correlated in-/out-group perceptions.

Our supply-side estimates rely on cross-sectional price comparisons: while we control for a rich set of cost, competition, and quality covariates and find that the migrant price discount is stable across specifications, we cannot rule out residual selection on unobservables. In addition, our price audit covers only three widely stocked benchmark staples. We, therefore, interpret the estimated discount as applying to this standardized basket rather than to overall price levels across each shop's full product portfolio. Future work using longitudinal shop-level data, ideally around quasi-experimental shocks to consumer composition or to migrant shop entry, could provide tighter identification of the supply-side adjustment margin.

In closing, our results establish a new and underappreciated dimension of the economic costs of anti-immigrant sentiment: a consumer-side penalty that distorts retail prices, compresses migrant profits, and potentially limits the growth of a sector through which hundreds of millions of households in the developing world access essential goods. The Spaza shop is not an exotic economic institution, it is the modal form of retail enterprise globally. Understanding how identity shapes competition in this setting illuminates broader questions about market discrimination, migrant integration, and the micro-foundations of retail market structure in developing economies.

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A Proofs of Supply-Side Propositions

Setup. Recall the equilibrium markup condition from equation (3):

$$P_j^* \;=\; c \;+\; \frac{1}{\beta_1\,(1-s_j^*)},$$

where the logit share of store j is

$$s_j^* \;=\; \frac{\exp(\beta_0-\beta_1 P_j^*+\beta_2 q-\beta_3 M_j)}{\sum_k \exp(\beta_0-\beta_1 P_k^*+\beta_2 q-\beta_3 M_k)}.$$

[Proof of Proposition 1] Suppose for contradiction that $P_M^* \geq P_L^*$. The mean indirect utilities at the candidate equilibrium are $V_M = \beta_0-\beta_1 P_M^*+\beta_2 q-\beta_3$ and $V_L = \beta_0-\beta_1 P_L^*+\beta_2 q$. Under the supposition, $-\beta_1 P_M^* \leq -\beta_1 P_L^*$, so

$$V_M \;\leq\; V_L-\beta_3 \;<\; V_L \quad (\text{since } \beta_3>0).$$

Logit shares are strictly increasing in mean utility, so $s_M^* < s_L^*$, which gives $(1-s_M^*) > (1-s_L^*)$ and hence

$$P_M^*-c \;=\; \frac{1}{\beta_1(1-s_M^*)} \;<\; \frac{1}{\beta_1(1-s_L^*)} \;=\; P_L^*-c,$$

i.e., $P_M^* < P_L^*$—a contradiction. This holds for any N_M, N_L: the competitive composition affects equilibrium share levels but not the direction of the gap. When $\beta_3 = 0$, the migrant and local stores are symmetric and $P_M^* = P_L^*$ follows immediately from the uniqueness of the symmetric equilibrium.

[Proof of Proposition 2] Let $F(P_M,\beta_3) \equiv \beta_1(P_M-c)(1-s_M)-1=0$ denote the migrant store's first-order condition, where we treat the competitive environment (all rival prices and shares) as fixed. We apply the implicit function theorem.

Denominator.

$$\frac{\partial F}{\partial P_M} \;=\; \beta_1(1-s_M) \;+\; \beta_1(P_M-c)\,\beta_1 s_M(1-s_M) \;=\; \beta_1(1-s_M)\left[1+\frac{s_M}{1-s_M}\right] \;=\; \beta_1 \;>\; 0,$$

where the second equality uses $\beta_1(P_M-c) = 1/(1-s_M)$ from (3).

Numerator. Since $\partial s_M/\partial \beta_3 = -s_M(1-s_M)$ (logit derivative with respect to mean utility, holding rivals fixed),

$$\frac{\partial F}{\partial \beta_3} = \beta_1(P_M - c)\, s_M(1-s_M) = \frac{s_M(1-s_M)}{1-s_M} = s_M > 0.$$

Conclusion. By the implicit function theorem,

$$\frac{\partial P_M^*}{\partial \beta_3} = -\frac{\partial F/\partial \beta_3}{\partial F/\partial P_M} = -\frac{s_M}{\beta_1} < 0.$$

Because β_3 does not enter the local store's first-order condition directly, $\partial P_L^*/\partial \beta_3 = 0$ (holding rivals fixed). Therefore,

$$\frac{\partial (P_L^* - P_M^*)}{\partial \beta_3} = \frac{s_M^*}{\beta_1} > 0.$$

B Demand-Side: Conjoint Experiment

Figure 1: Incentive Compatibility Briefing to Consumers

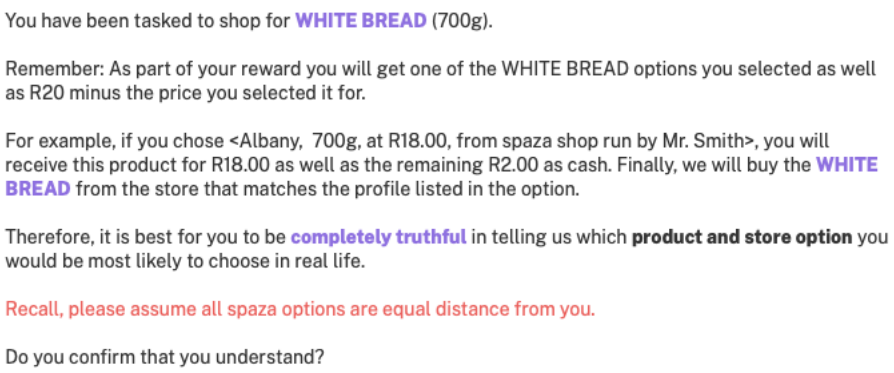


Figure 2: Example Conjoint Task in Treatment Condition

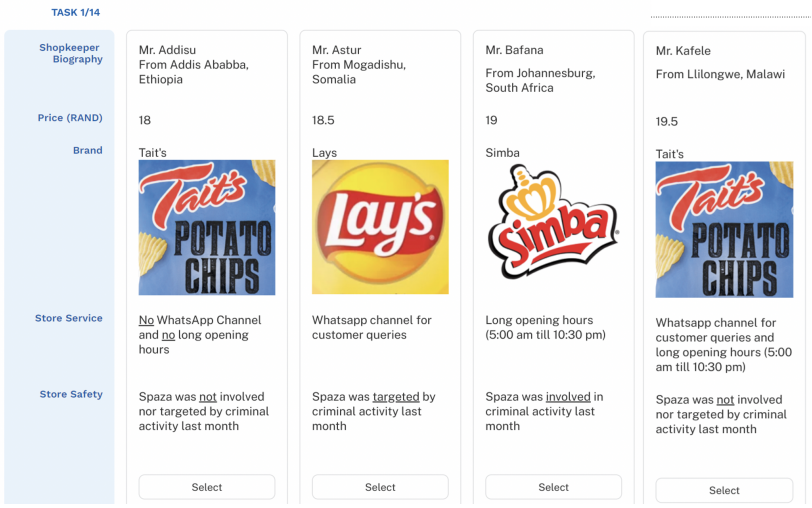


Table 1: Descriptives & Balance between Store Quality Information Treatment

	(1) Control		(2) Treatment		(1)-(2) Pairwise t-test	
Variable	N	Mean/(SE)	N	Mean/(SE)	N	Mean diff.
Female	967	0.58 (.016)	1007	0.57 (.0158)	1974	.012
Age	962	36.8 (.446)	1001	36.3 (.43)	1963	.48
Seeking Employment	967	.438 (.016)	1007	.438 (.0156)	1974	.001
Full-time Employed	967	.17 (.0121)	1007	.164 (.0117)	1974	.006
No Government Support	967	.308 (.0149)	1007	.319 (.0147)	1974	-.011
Votes ANC	967	.579 (.0159)	1007	.554 (.0157)	1974	.025
Buys at foreigner-run shop	967	1.17 (.0121)	1007	1.17 (.012)	1974	-.005
F-test of joint significance (P-value)					0.93	
F-test, number of observations						1963

Note: This table reports summary statistics and balance tests for respondents assigned to the no information *control* condition (Column 1) and the store quality information *treatment* condition (Column 2). Each column shows the mean with the standard error in parentheses. Column (1)–(2) reports the difference in means (perishable minus non-perishable). The final rows report an F-test of joint significance of all covariates and the number of observations used in that test. "Votes ANC" indicates whether the respondent supports the African National Congress. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Descriptive Statistics & Balance between Perishable and Non-Perishable Condition

	(1) Perishable		(2) Non-perishable		(1)-(2) Pairwise t-test	
Variable	N	Mean/(SE)	N	Mean/(SE)	N	Mean diff.
Female	987	0.56	987	0.58	1974	-.017
		(.0159)		(.0158)		
Age	983	36.8	980	36.2	1963	.648
		(.448)		(.427)		
Seeking Employment	987	.443	987	.434	1974	.009
		(.0158)		(.0158)		
Full-time Employed	987	.156	987	.177	1974	-.021
		(.0116)		(.0122)		
No government support	987	.32	987	.307	1974	.013
		(.0149)		(.0147)		
Votes ANC	987	.567	987	.565	1974	.002
		(.0158)		(.0158)		
Buys at foreigner-run shop	987	1.18	987	1.17	1974	.006
		(.0121)		(.0119)		
F-test of joint significance (P-value)						0.79
F-test, number of observations						1963

Note: This table reports summary statistics and balance tests for respondents assigned to the *perishable* condition (Column 1) and the *non-perishable* condition (Column 2). Each column shows the mean with the standard error in parentheses. Column (1)–(2) reports the difference in means (perishable minus non-perishable). The final rows report an F-test of joint significance of all covariates and the number of observations used in that test. "Votes ANC" indicates whether the respondent supports the African National Congress. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Pooled Conditional Logit Estimates by Product Type

	(1) Perishable Sample Only	(2) Non-Perishable Sample Only
Panel A: Logit Coefficients		
Focal Attributes		
Migrant	-1.092***	-1.167***
	(0.059)	(0.059)
Migrant × InfoTreat	0.707***	0.604***
	(0.071)	(0.076)
Price	-0.465***	-0.458***
	(0.024)	(0.024)
Store Service (Base: No Services)		
Long Hours	0.554***	0.532***
	(0.046)	(0.051)
Whatsapp Channel	0.167***	0.181***
	(0.039)	(0.040)
Whatsapp Channel and Long Hours	0.618***	0.673***
	(0.052)	(0.056)
Store Safety (Base: No Crime)		
Involved in Crime	-1.824***	-1.861***
	(0.081)	(0.081)
Targeted by Crime	-1.160***	-1.148***
	(0.056)	(0.054)
Panel B: Willingness-to-Pay		
Migrant WTP (InfoTreat= 0)	-2.349***	-2.551***
	(0.184)	(0.199)
Migrant WTP (InfoTreat= 1)	-0.827***	-1.230***
	(0.096)	(0.123)
p-value: WTP(0) = WTP(1)	<1e-4	<1e-4
Brand Dummies	Y	Y
$N_{consumers}$	986	987
N_{tasks}	11389	11403

Notes: This table replicates the pooled specification (Model 3) separately by product type. Robust standard errors clustered at the consumer-level are in parentheses. The specification estimates Migrant × InfoTreat and includes interactions of InfoTreat with store service and store safety attributes. WTP is computed as $-\beta_{Migrant}/\beta_{Price}$ with delta-method standard errors. The WTP equality test is conducted within each product type. Stars denote $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 4: Logit Bertrand Calibration: Bread Price Gap

	Observed	Model-Predicted
Panel A: Equal costs — isolates demand-side penalty		
Observed price gap $P_L - P_M$ (ZAR)	0.22	—
Model-predicted price gap $P_L - P_M$ (ZAR)	—	0.30
Panel B: Demand parameters and model inputs		
Price sensitivity $\hat{\beta}_p$		0.655
Total demand penalty $\hat{\beta}_3$		1.134
WTP(0) $= \hat{\beta}_3/\hat{\beta}_p$		R1.73
Gap / WTP		17.2%
Mean N_M (self-reported)		4.28
Mean N_L (self-reported)		2.74
Cost gap $c_L - c_M$ (ZAR)		−0.29

Notes: The table reports a logit Bertrand–Nash equilibrium calibrated to the bread market, following the framework in Section 2.4. *Demand parameters.* Estimates are drawn from Table 3, Column (1) (perishable subsample, no-information arm): $\hat{\beta}_p = 0.655$, $\hat{\beta}_3 = 1.134$. The no-information arm is used because consumers in real markets observe migrant ownership without verifiable quality disclosure; WTP(0) therefore captures both taste-based and statistical discrimination, which is the combined margin that governs equilibrium pricing. *Market structure.* N_M and N_L are store-owner self-reports of the number of migrant-owned and locally-owned competitors, averaged to the polygon level and used to construct the denominator of each store's logit share. *Equal-cost assumption.* Costs are equalized ($c_M = c_L$) so that the predicted price gap is driven entirely by the demand-side penalty rather than cost pass-through. This assumption is empirically supported: Table **??** shows that migrant and locally-owned stores do not differ significantly on any surveyed cost margin—and the implied cost gap ($c_L - c_M = -$R0.29) is statistically indistinguishable from zero ($p = 0.48$). *Equilibrium solution.* Given $(\hat{\beta}_p, \hat{\beta}_3, N_M, N_L, c)$, equilibrium prices (P_M^*, P_L^*) are solved numerically from the symmetric logit Bertrand first-order conditions in equation (3) using Newton–Raphson iteration.

C Supply-Side: Retail Census

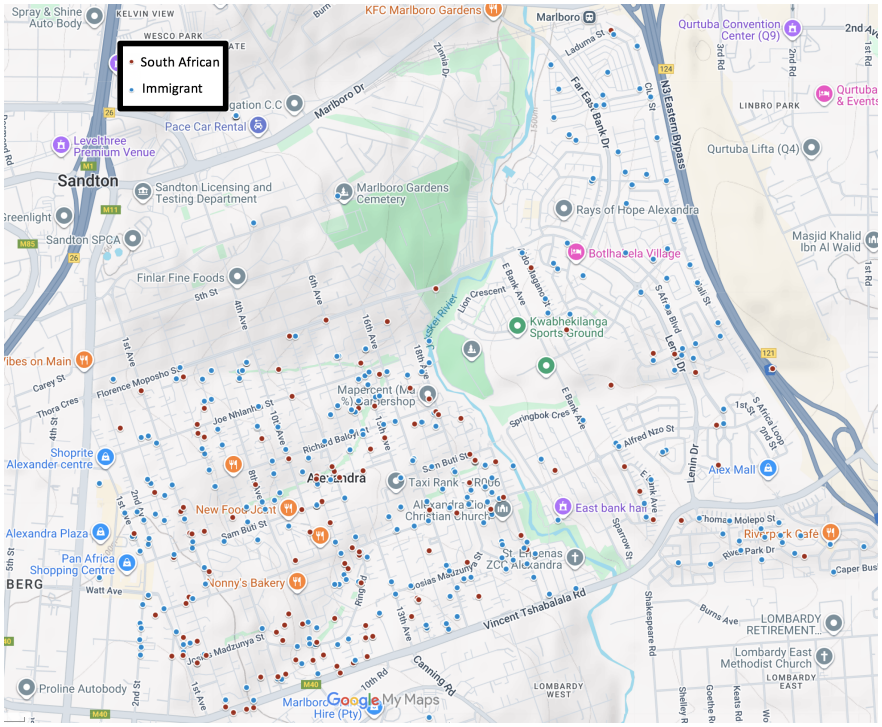


Figure 3: N = 432 Retail Firms in Alexandra by Migrant-Ownership Status

Table 5: Country of Origin of Shopkeepers in the Spaza Sample

Country of Origin	Share of Spaza Shops (%)
Ethiopia	48.84
South Africa	32.64
Somalia	11.11
Zimbabwe	3.47
Mozambique	1.62
Malawi	1.39
Other Immigrant	0.92

Table 6: Summary Statistics of Small Retailers

Variable	N	Mean	SE	Min.	Median	Max.
Male	432	0.86	0.35	0	1	1
Business owner (%)	432	0.20	0.40	0	0	1
Year opening business	432	2014.97	8.81	1965	2017	2025
Brick shop structure (%)	432	0.62	0.49	0	1	1
South African customers (%)	432	76.43	15.48	40	80	95
More than 2 employees (%)	432	0.11	0.31	0	0	1
Perceived safety	432	3.44	1.20	1	4	5
Payment to safety patroller (R.)	390	35.02	45.68	0	20	300
Violent incidences last year (n)	432	1.09	0.29	1	1	2
Knows customer by name (%)	432	21.85	27.58	0	8	100

Note: This table presents summary statistics of the retail stores in our census sample.

Table 7: Differences in Safety and Customer Interaction between Migrant and Local Retailers

Variable	Local Mean/(SE)	Migrant Mean/(SE)	Pairwise t-test Mean diff.
South African customers (%)	78	76	1.9
	(1.5)	(.85)	
Knows customers by name (%)	37	15	22***
	(2.7)	(1.3)	
Perceived safety	3.6	3.4	.16
	(.12)	(.065)	
Payment to safety patroller (n)	23	41	-17***
	(2.7)	(3.1)	
Violent incidences last year (n)	1	1.1	-.095***
	(.014)	(.019)	
F-test of joint significance (p-value)			4.6e-51***
F-test, number of observations			390

Note: This table presents summary statistics and a balance check on observables between migrant and local retail stores. Column (1) reports the mean and standard error for local retail stores and column (2) for immigrant-run retail stores. Column (3) presents the mean difference between the two groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Do migrant-owned retail stores have lower markups, all else held equal?

	DV: Mark-ups (standardized)				
	(1) No controls	(2) + Competition	(3) + Product & Service Qual.	(4) + Local Share (%) of Customers	(5) + Location Controls
Migrant Store	-0.179***	-0.179***	-0.133**	-0.166***	-0.194***
	(0.062)	(0.062)	(0.059)	(0.057)	(0.059)
Number of competitors		0.002	0.002	0.002	0.000
		(0.002)	(0.002)	(0.002)	(0.002)
Product condition rating (std.)			-0.026	-0.011	-0.040
			(0.029)	(0.028)	(0.031)
Number of customer services			-0.076***	-0.060***	-0.061**
			(0.020)	(0.020)	(0.024)
% of local customers				-0.010***	-0.006***
				(0.002)	(0.002)
Observations	1217	1217	1217	1217	1194
R^2	0.007	0.007	0.020	0.045	0.119
Adjusted R^2	0.006	0.006	0.017	0.041	0.095

Robust standard errors in parentheses. Model (5) includes granular location controls that are suppressed from display: 18 market FE, street-position dummies (on a street with no crossings; on a square; 4-way cross; 3-way cross; informal road between houses), landmark indicators (shebeen/bar, migrant hostel, clinic/hospital, dancing club, church, police office), and distance to the mall (km). * p¡0.10, ** p¡0.05, *** p¡0.01.

D Empirical Specification: Conjoint Model

In the conjoint experiment, each respondent i faces a sequence of choice tasks t, each consisting of a finite set of alternatives $j \in \mathscr{J}_{it}$. Let $Y_{itj} \in \{0,1\}$ indicate whether alternative j is chosen in task t. Each alternative is characterized by experimentally randomized attributes: price P_{itj}, a migrant indicator M_{itj}, brand indicators, and, in the information condition, store services and store safety attributes.

We estimate the following random utility model:

$$\begin{aligned} U_{itj} = {} & \beta_p P_{itj} + \beta_m M_{itj} + \beta_{mI}\big(M_{itj} \times \text{InfoTreat}_i\big) \\ & + \gamma^\top \text{Brand}_{itj} \\ & + \mathbf{1}\{\text{InfoTreat}_i = 1\}\left(\delta^\top \text{Service}_{itj} + \kappa^\top \text{Safety}_{itj}\right) + \varepsilon_{itj}, \end{aligned} \tag{5}$$

where ε_{itj} is assumed i.i.d. Type I extreme value across alternatives within a task. This structure implies the conditional logit choice probability:

$$\Pr(Y_{itj} = 1 \mid \mathscr{J}_{it}) = \frac{\exp(U_{itj})}{\sum_{k \in \mathscr{J}_{it}} \exp(U_{itk})}. \tag{6}$$

We estimate this model as a conditional logit. Standard errors are clustered at the respondent level to account for within-respondent correlation across tasks. All categorical attributes enter as indicator variables with an omitted baseline category. The baseline categories are: local South African shopkeeper for nationality, "No Services" for store services, and "No Crime" for store safety.

Link to the conceptual model. Under the no-information condition, the coefficient on M_{itj} captures:

$$\beta_m = -\beta_3 + \beta_2(q^M - q^L),$$

that is, the total migrant penalty combining taste-based discrimination (β_3) and belief-based statistical discrimination ($\beta_2(q^M - q^L)$). Under the information condition, the coefficient isolates the

taste-based component:

$$\beta_m + \beta_{mI} = -\beta_3.$$

Willingness-to-Pay. To express migrant penalties in monetary terms, we convert utility differences into willingness-to-pay (WTP). Let β_p denote the price coefficient. The implied WTP for purchasing from a migrant-owned store is:

$$\text{WTP}(0) = -\frac{\beta_m}{\beta_p}, \tag{7}$$

$$\text{WTP}(1) = -\frac{\beta_m + \beta_{mI}}{\beta_p}, \tag{8}$$

for the no-information and information conditions, respectively. We report delta-method standard errors for all WTP objects and test equality across regimes using the delta method applied to $\text{WTP}(1) - \text{WTP}(0)$.

E Spatial Distribution of Foreign and Local Retailers

If migrant-owned retailers were spatially concentrated in specific neighborhoods, the conjoint nationality attribute could proxy for location-specific characteristics rather than shopkeeper identity. We evaluate this possibility using geo-referenced data from the retailer census in Table 10.

Nearest-neighbor distances. To test whether foreign and local retailers occupy systematically different micro-market environments, we compute the distance from each retailer to its nearest neighbor of any nationality using GPS coordinates (see Table 10, Panel A). Differential nearest-neighbor distances would indicate that one group clusters in denser retail settings while the other operates in more isolated settings. The average nearest-neighbor distance is 55 meters for foreign retailers and 52 meters for local retailers, the difference is not statistically significant ($p = 0.637$, Welch t-test). Both groups thus operate in retail environments of comparable density.

Micro-location characteristics. We examine whether foreign and local retailers differ in their proximity to six landmark types and in their street position, both recorded by enumerators

during the census (see Table 10, Panel B). Of six landmark categories, four show no significant difference between foreign and local shops. Landmark proximity is not independent of retailer nationality (based on a weighted $\chi^2 = 13.87$, $p = 0.014$), driven by these two categories. We do not find a significant difference in street position of shops between foreign and local retailers: on a crossing ($p = 0.986$), no crossing ($p = 0.306$), informal road ($p = 0.350$). A joint χ^2 test on the 2×3 contingency table does not reject independence ($\chi^2 = 1.48$, $p = 0.478$). Street position is independent of retailer nationality ($\chi^2 = 1.48$, $p = 0.478$).

Segregation indices. We compute standard segregation measures from the residential sorting literature (Massey, 1991). The Dissimilarity Index (D) quantifies the share of one group that would need to relocate across neighborhoods to equalize the spatial distributions of both groups; it ranges from 0 (identical distributions) to 1 (complete segregation), with values below 0.30 classified as low. In our setting, $D = 0.208$ is below this threshold. The Isolation Index (P^*) measures the probability that a foreign retailer shares its neighborhood with another foreign retailer; the observed value of 0.695 is close to the 0.674 expected under spatially random allocation. Both indices indicate that the magnitude of spatial sorting across neighborhoods is economically small (Table 10, Panel C).

Neighborhood composition. The study area comprises 18 enumeration polygons, each covering a contiguous set of streets in Alexandra municipality. Foreign-owned retailers constitute 67% of the census sample and are present in all 18 polygons, with the foreign share ranging from 48% to 100% across neighborhoods (Table 9). We test the null hypothesis that retailer nationality is independent of neighborhood assignment using a Pearson χ^2 test on the 18×2 contingency table of polygon by nationality. The test does not reject independence at the 5% level ($\chi^2 = 27.02$, $p = 0.058$). To benchmark whether the observed variation exceeds what sampling noise alone would generate, we compute polygon-specific 95% binomial confidence intervals under the null that each shop's nationality is drawn independently with probability equal to the overall foreign share (0.674). Of 18 polygons, 16 fall within the expected range, consistent with random allocation (Table 10, Panel D).

In sum, these results show that foreign and local retailers are broadly interspersed across the study area rather than sorted into distinct neighborhoods.

Table 9: Distribution of Retailers Across Neighborhoods by Nationality

Neighborhood	Local	Foreign	Total	Foreign Share	95% Binomial CI	Within CI?
Polygon 1	8	13	21	0.62	[0.48, 0.86]	Yes
Polygon 2	7	19	26	0.73	[0.50, 0.85]	Yes
Polygon 3	3	6	9	0.67	[0.33, 1.00]	Yes
Polygon 4	10	19	29	0.66	[0.48, 0.83]	Yes
Polygon 5	17	16	33	0.48	[0.52, 0.82]	No
Polygon 6	27	52	79	0.66	[0.57, 0.77]	Yes
Polygon 7	3	4	7	0.57	[0.29, 1.00]	Yes
Polygon 8	7	9	16	0.56	[0.44, 0.88]	Yes
Polygon 9	14	16	30	0.53	[0.50, 0.83]	Yes
Polygon 10	9	11	20	0.55	[0.45, 0.85]	Yes
Polygon 11	14	37	51	0.73	[0.55, 0.80]	Yes
Polygon 12[†]	0	1	1	1.00	[0.00, 1.00]	Yes
Polygon 13	4	9	13	0.69	[0.38, 0.92]	Yes
Polygon 14	3	3	6	0.50	[0.33, 1.00]	Yes
Polygon 16	2	18	20	0.90	[0.45, 0.85]	No
Polygon 17	11	40	51	0.78	[0.55, 0.80]	Yes
Polygon 18	1	13	14	0.93	[0.43, 0.93]	Yes
NA[†]	0	4	4	1.00	[0.25, 1.00]	Yes
Total	140	290	430	0.67	—	—

Notes: Each neighborhood corresponds to an enumeration polygon in the Alexandra municipality census. The 95% Binomial CI gives the expected range of foreign shares under the null hypothesis that each shop is independently assigned "foreign" status with probability equal to the overall sample share.

Table 10: Tests of Spatial Independence Between Foreign and Local Retailers

Test	Statistic	p-value	N
Panel A: Nearest-Neighbor Distances			
Foreign retailers: mean (median)	55 (41) meters		
Local retailers: mean (median)	52 (38) meters		
Welch t-test	0.47	0.637	426
Panel B: Micro-Location Characteristics			
Nearby landmarks (foreign − local)			
Shebeen/Bar	−0.021	0.748	430
Migrant Hostel	0.018	0.650	430
Clinic/Hospital	−0.089	0.010	430
Dancing Club	0.054	0.041	430
Church	0.000	1.000	430
Police Office	0.006	0.985	430
Landmarks (χ^2 test)	13.87	0.031	430
Street position (foreign − local)			
On a square	−0.075	0.042	430
4-way crossing	−0.009	0.874	430
3-way crossing	0.015	0.719	430
No crossing nearby	0.030	0.225	430
Informal road between houses	0.050	0.350	430
Street position (χ^2 test)	7.01	0.135	430
Panel C: Segregation Indices			
Dissimilarity Index (D)	0.208	< 0.30 = low segregation	
Isolation Index (P^*)	0.695	0.674 under random allocation	
Panel D: Neighborhood Distribution			
Pearson χ^2 test of independence	27.02	0.058	430
Binomial CI: polygons within expected range	16/18 (expected: ≈17/18)		

Notes: The sample comprises foreign-owned and locally-owned retailers from the census. Panel A compares haversine nearest-neighbor distances. Panel B reports differences in proportion between foreign and local retailers for six landmark categories and five street-position categories; p-values for individual rows are from χ^2 tests with Yates continuity correction. The landmark joint test is a likelihood-ratio χ^2 from a logistic regression of foreign status on all six landmark indicators. The street-position joint test is a Pearson χ^2 on the 2×5 contingency table of nationality by street position. Panel C reports standard segregation indices; values of D below 0.30 indicate low segregation. Panel D tests whether the distribution of retailers across neighborhoods is independent of nationality to ensure adequate expected cell counts (weighted χ^2). The binomial CI row reports how many polygon-level foreign shares fall within the 95% interval expected under random allocation.