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THE EFFECT OF EXPOSURE: EVIDENCE FROM SPATIAL CHOICES IN NAIROBI

Joshua T. Dean
Gabriel Kreindler
Oluchi Mbonu

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ABSTRACT

Do people dislike options they have never encountered? We study how exposure to new neighborhoods affects work location choices of casual workers in Nairobi, Kenya, where the median participant has never visited half the neighborhoods near home. We hire workers for day-long jobs and elicit location preferences. Initially, workers sacrifice 23% of a day's median wage, or travel 3.8 kilometers more, to avoid unfamiliar neighborhoods. Yet a single one-hour training visit to an unfamiliar neighborhood eliminates this aversion. Pessimistic beliefs about unfamiliar neighborhoods also converge after one visit, consistent with fear of the unknown. Merely announcing a visit raises valuations of an unfamiliar neighborhood by almost two-thirds as much as visiting. In our model this implies two-thirds of the visit effect reflects a first-visit cost, and one-third pessimistic beliefs. Workers also appear less likely to even consider unfamiliar neighborhoods. Unfamiliarity can deter workers from taking advantage of urban opportunities.

Joshua T. Dean
The University of Chicago
Booth School of Business
joshua.dean@chicagobooth.edu

Oluchi Mbonu
The World Bank
ombonu@worldbank.org

Gabriel Kreindler
Harvard University
Department of Economics
and NBER
gkreindler@fas.harvard.edu

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People are routinely confronted with choices involving options they have never been exposed to before, such as jobs, products, and places to visit. Economists typically assume that past exposure to an option predicts choice either because it is the result of past optimization or because exposure reveals information, such as the unobserved quality of a product (Milgrom and Roberts 1986). However, evidence from psychology and marketing suggests exposure to an option leads people to like it more and increases its salience, disadvantaging unfamiliar options (Zajonc 1968; Montoya et al. 2017; Goldstein and Gigerenzer 2002). In this paper, we study the impact of exposure and distinguish between mechanisms behind the effect.

We study the role of exposure in a spatial setting in Nairobi, Kenya, looking at how prior exposure to urban neighborhoods shapes short-term work location choices. Urban spatial choices govern how people access valuable opportunities (Chetty et al. 2016; Pelnik 2024). Yet, individuals appear resistant to changing location both within cities (Bergman et al. 2024; Grosset-Touba 2024) and across (Kennan and Walker 2011; Bryan and Morten 2019; Baseler 2023; McKenzie 2024), with a significant role for non-financial barriers. Choice across space is thus a useful context to explore the importance of prior exposure with potentially important welfare implications, especially in low-income settings.

Why might exposure matter for spatial choices? We consider five possibilities. First, people may be familiar with neighborhoods that are better for them (sorting). Second, exposure may reveal information about a neighborhood, such as its safety and amenities. Third, people may find it difficult to visit a neighborhood for the first time, due to navigation, risk of getting lost, or heightened alertness – a fixed (one-time) cost of exposure. Fourth, people may be pessimistic about unfamiliar neighborhoods, a form of "fear-of-the-unknown", which exposure may change. Finally, exposure may increase the likelihood a neighborhood is retrieved from memory as a potential choice. Estimating the effect of exposure requires measuring existing familiarity patterns, observing how choices change in response to exogenous novel exposures, and generating variation to distinguish between mechanisms.

In this paper, we first document substantial gaps in participants' exposure to nearby neighborhoods. We then offer short-term jobs across Nairobi and randomly assign participants to train for them in familiar or unfamiliar neighborhoods. Participants make job choices which reveal how much they value working in unfamiliar neighborhoods, and how these valuations change after one visit. We use a model of an expected-utility maximizer to interpret our results and distinguish between the first four channels described above. We also measure beliefs about neighborhoods, study consideration sets in an open-ended elicitation, and track the persistence of exposure effects.

First, we document substantial gaps in participants' past exposure to neighborhoods near their homes. Our sample consists of casual workers living in low-income neighborhoods — a group central to urban labor markets in developing countries. The median participant has never physically been to half of the neighborhoods within 75 minutes of where they live. They can recall a landmark for one in three such neighborhoods. This is not due to a lack of awareness; the median participant has heard of 94% of the neighborhoods in this radius. Nor is this due to a general lack of mobility; around two thirds of participants work or search for work outside their home neighborhood, and the average such worker travels up to 8 km or one hour. We find no evidence of sorting based on neighborhood quality. Almost all neighborhoods have intermediate levels of average familiarity, and after conditioning on neighborhood fixed effects and distance, individual-by-neighborhood observables have no incremental predictive power.

Second, we offer participants day-long jobs measuring air quality in varying neighborhoods across Nairobi. Each day participants make choices trading off job attributes and locations. We then use these choices to estimate the revealed costs of working in an unfamiliar neighborhood. This task requires being physically present in a specific location and plausibly justifies offering a variety of locations as options.

To induce exogenous variation in exposure, we randomize where participants are trained to complete the task. Participants in the treatment group are trained in neighborhoods they have never visited (unfamiliar neighborhoods), while participants in the control group are trained in familiar neighborhoods. Training control participants in familiar neighborhoods allows us to net out the effect of training from exposure to unfamiliar neighborhoods. Both groups visit three neighborhoods over three days. In order to avoid experimenter demand effects, participants and the guides who led the training are told that training locations were chosen randomly, and they are not informed of the treatment status.

To measure preferences, separately from consideration, we give half of the participants choices between jobs with varying attributes. After three training days, participants are employed for three additional days. On the morning of each employment day, we present them with seven binary choices where each option has a randomly chosen job location, duration and compensation. We then randomly pick one of the choice questions and assign the option the participant chose as their job for the day. This ensures participants face real stakes when choosing between options.

In the third step of the paper, we use a model of a Bayesian agent who maximizes expected utility to interpret how neighborhood valuations in our experiment vary with

familiarity and experimental exposure, and to disentangle mechanisms.[1] The agent may be risk-averse over neighborhood attributes, and we assume the worker updates about the quality of a neighborhood after exposure, following previous work on "experience goods" (Milgrom and Roberts 1986; Ackerberg 2003). Beliefs about unfamiliar neighborhoods may be miscalibrated, for example pessimistic on average.[2]

The agent may incur a first-visit cost when they are exposed to a neighborhood for the first time. This cost may include longer travel time, safety risks when visiting a location for the first time, or greater effort needed to process a novel neighborhood's stimuli. Training may also have a direct effect on choices, for example due to a recency effect.

We characterize two key estimands in the model. First, we define the *familiarity premium* as the average difference in valuations between familiar and unfamiliar neighborhoods, that is, the average willingness to pay for working in a neighborhood the participant has previously visited, holding other attributes constant. In our model, the familiarity premium equals the sum of three components: the average perceived first-visit cost, the average prior error for unfamiliar neighborhoods, and a sorting term that captures average differences in the true utilities of working in familiar and unfamiliar neighborhoods.[3]

Second, we define the *exposure effect* as the average experimental effect of a single exposure on the utility of working in an unfamiliar neighborhood, excluding any direct effects of training. In our model, the exposure effect is the sum of two components: first-visit costs, and the average belief revision after one visit. In particular, with calibrated beliefs, neither learning about neighborhood quality nor risk aversion can generate an exposure effect. This follows from the law of iterated expectations. Absent first-visit costs, the expected utility elicited ex-ante is the same as the average of expected utilities elicited after exposure, when some or all of the uncertainty is resolved.

Under the model, binary job choices follow a logit with random coefficients. We use a normal learning model that implies a normal mixed logit, and we estimate this using simulated maximum likelihood. A standard logit model gives very similar results.

[1] Our registration at https://www.socialscienceregistry.org/trials/11205 said, "We will estimate the familiarity premium and test whether certain behavioral biases may amplify this effect." Our exploration of mechanisms is guided by theory but was not preregistered.

[2] An alternative way of operationalizing "fear-of-the-unknown" would be to allow the agent to be ambiguity averse (Epstein and Schneider 2008). This could generate a positive familiarity premium and exposure effect. We opt instead for miscalibrated beliefs given the direct measures of pessimistic beliefs.

[3] In our static model, there is no continuation value for learning as in standard models of explore-exploit tradeoffs. This type of effect would push towards a smaller familiarity premium as individuals can take advantage of the effectively subsidized chance to explore.

Our first main empirical result is a large familiarity premium. Participants are willing to travel an additional 3.8 km, work around an hour and a half longer, or sacrifice 115 Kenyan Shillings (KSh) of salary (23% of one day's median wage) to avoid working in a neighborhood they have never visited. Ignoring familiarity would overstate the utility cost of distance by 20%, because more distant neighborhoods are also more likely to be unfamiliar.

Our second main empirical result is that a single exposure is sufficient to eliminate the familiarity premium. We find that above and beyond the direct effect of training, being randomly assigned to visit an unfamiliar neighborhood once is equivalent to increasing the wage by 119 KSh or bringing the neighborhood 3.9 km closer, fully offsetting the baseline familiarity premium. The exposure effect equals 1.04 times the familiarity premium (95% confidence interval [0.67, 1.33]). Across the mixed logit and a standard logit, each estimated with and without home-by-destination fixed effects, this ratio ranges from 0.98 to 1.26. The exposure effect also roughly matches the familiarity premium for participants with low and high baseline familiarity, for near and far neighborhoods, and for women and men.

In our model, sorting into higher quality neighborhoods raises the familiarity premium but does not affect the exposure effect. Sorting would thus make the premium larger than the exposure effect.[4] The fact that we find roughly equal effects means that under this model, there is no sorting based on the utility cost of working in a neighborhood. This is consistent with the idiosyncratic patterns of past exposure documented above.

We next turn to unpacking the exposure effect. As discussed above, in our model, uncertainty and learning under calibrated beliefs do not contribute to this effect.[5] Instead, the exposure effect reflects the combination of miscalibrated priors and a first-visit cost.

We measure beliefs at baseline and after training, and show they follow a pattern similar to the work choices. Participants' beliefs about labor market opportunities and safety are more pessimistic about unfamiliar neighborhoods initially, and a single exposure eliminates the difference relative to familiar neighborhoods. This suggests that at least part of the exposure effect is due to pessimism or a "fear-of-the-unknown."

We next use results from a separate job choice elicitation to decompose the exposure effect between first-visit costs and beliefs. On each training day, participants also make binary

[4]This holds unless participants become overly optimistic about previously unfamiliar neighborhoods after one visit, which would offset the gap.

[5]However, even with calibrated beliefs, learning may inflate the measured exposure effect in a finite sample. A small risk that an unfamiliar neighborhood is persistently and extremely bad would lower pre-visit valuations, but it might not have occurred among our almost 600 exposures. This scenario probably explains little of our results: we find similar familiarity premium and exposure effects in unfamiliar neighborhoods that participants confidently rate as safe (Table A.14).

choices for jobs that will take place on an employment day three days later. Participants make these choices *after* learning where they will train on that day but *before* actually visiting the training neighborhood. Announcing the training location informs agents that by the time the employment day arrives, they will have already paid any first-visit costs. However, the announcement does not affect the information about neighborhoods available at the moment the choice is made, so it should not change choices if the exposure effect were driven solely by incorrect beliefs.[6] Thus, in our model the anticipated exposure effect only captures perceived first-visit costs.[7]

We find that participants make choices as if the exposure effect will be on average about 64% as large as what we estimate using choices after exposure, with a 95% confidence interval of $[0.33, 0.97]$. That is, the announcement alone raises participants' valuation of an unfamiliar neighborhood, consistent with participants expecting to have paid any first-visit costs by the employment day. Through the lens of our model, this implies that about two-thirds of the exposure effect is due to perceived first-visit costs with the balance reflecting the revision in miscalibrated beliefs about unfamiliar neighborhoods. Together with the direct evidence on pessimistic beliefs that update after exposure, we have evidence of both channels for the exposure effect.

What drives perceived first-visit costs? Longer first trips and exploration risk during the first visit at most account for a limited share. First, initial trips to unfamiliar neighborhoods do not take much longer. We use a Heckman selection model with the randomized job attributes as instruments and reject at 95% confidence level that people take more than 15 minutes longer round trip to an unfamiliar neighborhood. We also find no effect of exposure on trip time, and can reject any decrease larger than 9 minutes. The familiarity premium, by contrast, is worth about an hour and a half of extra work. Second, exploring an unfamiliar neighborhood does not appear risky in our setting. During the experiment, we observe no adverse events. Getting lost, a plausible precursor of serious incidents, does not vary with familiarity or exposure. When participants are confident that traveling to an unfamiliar neighborhood is safe, the familiarity premium and the exposure effect are still 70–80% as

[6] If the agent was ambiguity averse and unsure of the informativeness of the visit, the announcement would weakly lower valuations because the agent evaluates the signal under a worst-case view of its informativeness (Epstein and Schneider 2008). Our anticipated exposure effect therefore remains a lower bound on perceived first-visit costs.

[7] The result requires two assumptions. First, the announcement reveals nothing about the neighborhood, e.g. no endorsement effect. Participants were told that a computer chose training locations at random, and respondents could receive job offers in every neighborhood asked about. Second, participants treat the choice as binding. In practice, respondents refused the job assigned from their choices only 0.51% of the time.

large. The perceived first-visit cost may instead reflect overestimated travel times and risks, or low processing fluency, namely the effort of attending to novel stimuli until people learn which parts of the environment they can ignore (Winkielman et al. 2003).

In our fourth step, we study how prior exposure affects the *consideration* of locations as employment options. So far, we elicited preferences by confronting participants with neighborhood options. In the real world, choices often require individuals to construct consideration sets (Goeree 2008; Phillips et al. 2019; Bordalo et al. 2020). Familiarity and new exposures may shape these consideration sets by increasing the likelihood a neighborhood is retrieved from memory as a potential option.

We use an "open" elicitation for half of the sample. We tell subjects that jobs are available in different neighborhoods in Nairobi and ask them to name where they would most like to work, then their next choice, and so on. We tell subjects they will be assigned to the highest neighborhood on their list with a job available, and analyze the predictors of whether a neighborhood is mentioned in the list.[8] Baseline familiarity and training strongly positively predict whether a neighborhood is mentioned. The effect of a single exposure to a location partially undoes the familiarity premium. Familiarity and exposure affect both the construction of consideration sets and choices within these sets.

Lastly, we find some evidence that exposure to an initially unfamiliar neighborhood has persistent effects on individuals' willingness to return. We measure persistence in three ways. First, we find that the baseline familiarity premium is relatively stable for locations visited within the last three years. Second, we invite participants to randomized locations to take short surveys about commuting two to four months after the intervention and measure whether participants show up. Baseline neighborhood familiarity strongly predicts show-up all else equal, and the experimentally induced exposure is positive, roughly two thirds of the baseline familiarity coefficient, but imprecisely estimated because of ceiling effects at high wages.[9] Third, based on endline phone and SMS surveys, training in a location leads participants to return there to search for work and for fun/leisure. This effect is similar for familiar and unfamiliar training neighborhoods, which suggests that training made locations more salient, and that participants continue to revisit neighborhoods where they trained,

[8] In the logit consideration model from Goeree (2008), this is the same as estimating consideration separately from preferences. At the wages we offered, participants prefer working in any neighborhood to not working at all, so they should rank all neighborhoods in their consideration set.

[9] These results hold for wage offers $\leq$ 500 KSh. For wages $\geq$ 600 KSh (20% above the median daily wage in our sample), show-up is flat around 80% and none of the job and neighborhood attributes, including distance or wage, predicts show-up.

including those they were unfamiliar with at baseline.

Taken together, how much might the frictions identified here limit access to opportunities in unfamiliar neighborhoods? More than a third of the exposure effect reflects pessimistic beliefs. A visit corrects these beliefs and raises the value of each later workday in the neighborhood by about 45 KSh (9 percent of the median daily wage). Pessimism can therefore deter exploration even by a forward-looking, patient worker. The first-visit cost can also deter workers who are impatient, present-biased, or liquidity constrained. Naive present-biased agents may postpone such investments indefinitely (O'Donoghue and Rabin 1999). In other settings, small upfront costs or frictions reduce high-return migration and job search (Bryan, Chowdhury, et al. 2014; Franklin 2018; Abebe, Caria, and Ortiz-Ospina 2021).[10] Together, the two frictions offer a micro-foundation for the reluctance to try new locations (McKenzie 2024; Brady and McNulty 2011), for the lasting effects of temporary exposure to new places and routes (Larcom et al. 2017; Okunogbe 2024), and for an aversion to rural–urban migration that falls with experience in the city (Lagakos et al. 2023).

Our results extend the existing literature on the economics of experience goods and a diverse literature in economics and finance studying the importance of experience and experimentation for learning by showing a role for experience beyond providing additional information (Milgrom and Roberts 1986; Hanna et al. 2014; Malmendier 2021). In spatial decisions, they complement work on information frictions in consumer search, migration, and trade (Vitali 2024; Baseler 2023; Wiseman 2023; Porcher et al. 2024). Our results also provide an additional, possible explanation for the levels of repeated choice observed in settings such as consumer choice. This extends a rich literature highlighting the importance of habit formation, limited consideration, and automaticity (Hussam et al. 2022; Landry et al. 2021). Our work is also related to a large literature in marketing that studies the mechanisms and effects of advertising (Milgrom and Roberts 1986; Nelson 1970; Ackerberg 2003; Ackerberg 2001). Overall, exposure effects may be important in other economic domains such as occupation choice or technology adoption. For example, Allcott et al. 2026 show induced experimentation affects choices and corrects beliefs about search engine quality.

Our paper builds on several areas of psychology. First, we build on an extensive literature studying the mere exposure effect and its underlying mechanisms (Zajonc 1968; Montoya et al. 2017). This literature predominantly focuses on repeated and controlled exposure for

[10] Small upfront costs also reduce take-up of health services (Thornton 2008), retirement plans (Madrian and Shea 2001), social benefits (Bhargava and Manoli 2015), and college financial aid (Bettinger et al. 2012).

short durations to a variety of specific stimuli in the laboratory. We extend this literature by focusing on single, longer duration exposures to holistic environments. We also relate to a broader psychology literature studying aversions to unfamiliar experiences such as conversing with strangers (Epley and Schroeder 2014), interacting with out-group members (Allport 1954), or trying new foods (Rozin and Vollmecke 1986). We extend this literature by studying spatial familiarity. Finally, we also draw inspiration from a diverse literature on how past experience can shape mental representations and how these representations can be used in planning and decision making. We extend this literature by focusing on a naturalistic, high-stakes task and demonstrating the separate contributions of exposure through preferences and formation of consideration sets.

Our paper has implications for realizing the benefits of urban agglomeration in developing countries (Bryan, Glaeser, et al. 2020), complementing previous work that measures and unpacks transportation costs in these contexts for different groups (Borker 2026; Grosset-Touba 2024; Cook et al. 2024), and research on policies that reduce transportation costs (Tsivanidis 2026; Banerjee and Sequeira 2023; Franklin 2018; Abebe, Caria, Fafchamps, et al. 2021; Field and Vyborny 2022).[11] These benefits may be particularly important to low-income individuals who are often spatially segregated from opportunities (Deffebach et al. 2025). Policies to encourage exposure may be additional tools, beyond reducing access costs, to increasing effective access to opportunities.

1 Participant Sample: Casual, Underemployed Workers

Our population of interest is casual, underemployed workers in Nairobi. We choose this population for three reasons. First, casual, underemployed workers who search for work spatially are a central feature of urban labor markets in many low- and middle-income countries.[12] Second, the short-term employment offers in the experiment are ecologically valid for this population, since these workers' primary source of employment is short-term offers. Third, casual workers frequently search for jobs, so they may be particularly open to new opportunities, including in unfamiliar neighborhoods, possibly making our estimates conservative relative to a population with more regular employment.

We recruited study participants in three low-income neighborhoods of Nairobi (Kibera,

[11] A subset of these papers estimate commuting elasticities and find low values, including for low-skilled workers, which suggests large positive roles for broad access to areas of the city.

[12] In Nairobi, the 2021 FinAccess Household Survey shows that casual labor is the primary source of livelihood for 31.1% of adults aged 16 and over, making it the most common source of livelihood, ahead of both regular employment and business ownership (Central Bank of Kenya et al. 2022).

Kawangware, and Viwandani, see Figure A.1) on a rolling basis between October 2023 and January 2024. Surveyors recruited participants in person based on a canvassing exercise, and participants who passed an initial filter were later invited to a nearby study venue for two days of additional surveys. In order to limit selection into the study sample based on willingness to explore, we set up a study venue within each of the three home neighborhoods.

Eligibility and samples. A canvassed participant was eligible for the study if they were older than 18 years old, lived in one of our three study neighborhoods, were a casual worker actively searching for work and stated that they were available to work every day for the next 7 days.[13] We collected information from 2,316 participants during recruitment. Of these participants, we invited 1,600 who were eligible to participate in a baseline survey. Among these eligible participants, 1,168 attended the first baseline survey, where we measured familiarity. This constitutes our "descriptive sample" that we use to characterize familiarity patterns. Of these, 915 were invited and 830 participated in a second baseline survey, where we collected additional demographic information. We describe the sample and sampling procedures for the second baseline and the experiment in section 3.

Survey Data. The two baseline surveys included demographic and employment questions, urban familiarity questions (see section 2.1), data on networks, self-reported spatial ability, and beliefs about the labor market and safety of neighborhoods.

Table A.1 reports descriptive statistics collected during the baseline surveys. The median participant is a rural migrant who has been living in Nairobi for 13 years. Three quarters are women. This sample is considerably under-employed. Participants have searched for work an average of 6.6 days in the past two weeks and only worked half that. Those who worked or searched for work outside their home neighborhood (around two thirds of the sample) also travel a significant distance, up to 8 km or one hour on average. The most common job occupations for women involve working for other households doing laundry, cleaning or other house-help (Table A.2). Occupations for men are more heterogeneous and typically include manual and semi-skilled work such as carpentry, masonry, factory work and electrician work. Table A.3 shows that referrals are the most common way that this group searches for and finds employment, yet participants also rely significantly on spatial search strategies such as going door to door, traveling to "hiring spots," and visiting potential employers to submit resumes.

[13]We define a casual worker as someone who does not work as a permanent employee – but rather does short-term daily contract jobs. A participant is classified as actively searching for work if they searched for work for at least three days in the past two weeks.

2 Neighborhood Familiarity Patterns

2.1 Measuring Familiarity

Throughout this paper, we say a participant is *familiar* with a neighborhood if they report having ever been to that neighborhood in the past. In this section we first discuss how we measured familiarity in our sample, and then present several stylized facts.

We partition Nairobi into the 61 neighborhoods with commonly used and recognized names. We coordinated a mapping team of Busara Center for Behavioral Economics employees with in-depth knowledge of Nairobi and asked them to generate the neighborhood names and boundaries and to seek input from field guides in various neighborhoods when necessary. Figure A.1 displays the resulting neighborhoods. We tested and piloted the neighborhood names extensively to make sure they are broadly and reliably recognized by the population that we sample from. The mapping team also generated 341 sub-neighborhoods within these 61 neighborhoods. Most of our analysis uses the main neighborhoods. We dropped 17 neighborhoods deemed irrelevant for the study.[14]

For each home neighborhood (Kibera, Kawangware, and Viwandani), we elicit participant familiarity with neighborhoods that are within 75 minutes walking or by transit (whichever is shorter) from the study venue in that neighborhood, based on data from Google Maps. This led to lists of 33, 30, and 31 neighborhoods for the three home neighborhoods, respectively.[15]

In the first baseline survey, we ask participants about all neighborhoods in this list, randomizing the order. We initially loop over all neighborhoods and ask two types of questions for each neighborhood:

1. Have you ever been to the neighborhood of X?
2. (if "yes") When was the last time you went to the neighborhood of X?
2. (if "no") Have you ever heard of the neighborhood of X?

We use responses to the first question as our main measure of familiarity due to high test-retest reliability in piloting. We then collect additional data on each neighborhood. We ask participants if they know how to get to X, and ask them to tell us a location, landmark or road in the neighborhood of X.

[14] Appendix A.4 discusses neighborhood selection in detail. Dropped neighborhoods include national parks, the airport, and neighborhoods that are informal settlements, which are unlikely to have work opportunities and may be unsafe. This may bias our sample towards better neighborhoods, which may affect our results. However, we show later that our results are robust to restricting to unfamiliar neighborhoods that participants are confident are safe or very safe (Table A.14).

[15] Figure A.2 plots the set of neighborhoods we ask about for each home neighborhood.

Table 1: Participants Have Significant Familiarity Gaps

Nbhd sample:	Share of Neighborhoods Visited					
	< 75min			< 8km		
	p25	p50	mean	p25	p50	mean
Measure of familiarity:						
Heard of	0.89	0.94	0.92	0.91	0.97	0.94
Ever been OR passed by	0.50	0.63	0.62	0.64	0.79	0.75
Ever been	0.39	0.52	0.52	0.55	0.65	0.66
Ever been + knows get there	0.33	0.43	0.45	0.42	0.58	0.57
Ever been + knows get there + gave landmark	0.24	0.34	0.34	0.33	0.50	0.46

Notes: This table reports statistics for the share of neighborhoods that a participant is familiar with for our "descriptive" sample, which includes all 1,157 participants who completed the first survey. For the last two rows we further restrict to 906 eligible participants who were asked more detailed familiarity questions. The sample of neighborhoods is restricted to within 75 minutes (shortest among walking or by transit) of the respondent's home neighborhood in the first three columns, and to within 8 km in the last three columns. We pick this cutoff because among participants who work or search for work outside their home neighborhood (two thirds of the sample), the mean participant travels up to 8 km to work or search for work.

2.2 Neighborhood Familiarity Patterns

Participants in our sample have significant "spatial familiarity gaps." The median participant has never visited around half of the 30-33 neighborhoods in the sample, while a quarter of participants have visited less than 40% of the neighborhoods (Figure A.3a).

Table 1 reports results from different measures of familiarity and varying the sample of neighborhoods around the respondent's home neighborhood. The first three columns report results for the sample of neighborhoods within 75 minutes, and the last three columns further restrict to only neighborhoods within 8 kilometers of the participant's home neighborhood.[16]

The table highlights several results. First, participants have heard of almost all of the neighborhoods. Three quarters of participants have heard of at least 89% of the neighborhoods within 75 minutes. Unfamiliarity is not driven by confusion about neighborhood names.

Second, familiarity is low, and even lower for more demanding definitions of familiarity. The median participant reports ever having been to *or passed by* 63% of the neighborhoods,

[16]For the average participant who travels outside their home neighborhood to work or search for work, this is the distance to the farthest neighborhood they report visiting for these purposes.

has ever been to 52%, has ever been and knows how to reach only 43% of neighborhoods, and can furthermore name a landmark for only 34% (around 13 and 10 neighborhoods, respectively). Men, older participants, and participants with more education, more years in Nairobi, or higher spatial ability know slightly more neighborhoods (Table A.4).

Beyond distance and neighborhood fixed effects, familiarity is largely idiosyncratic. Few neighborhoods are known to almost all participants or to almost none, and few participants know most or none of the neighborhoods (Figure A.3b). Familiarity therefore does not reflect a consensus view of neighborhood attributes. To test this further, we fit random forest models to predict whether individual i is familiar with neighborhood n. Neighborhood fixed effects and distance give an area under the ROC curve of 0.86, yet adding individual and neighborhood characteristics does not improve the fit (Figure A.3c). Sharing an ethnicity with a neighborhood's plurality group predicts familiarity, but not after controlling for distance (Table A.5).

3 Experimental Overview

In order to experimentally identify the effect of novel exposure, we need to induce individuals to travel to unfamiliar locations without generating experimenter demand effects, and we need to be able to control for any recency effects where participants in general prefer more recently visited locations.

Air Pollution Measurement Jobs. To accomplish these objectives, we offer short-term job opportunities to participants in our sample in different locations throughout the city. Participants are asked to collect data on air quality in a specific neighborhood. These jobs obviously require being in a specific location, and they make it plausible to vary the location where the participant is working.

To complete the task individuals wear a backpack used in Berkouwer and Dean (2026) (Figure A.4), which contains a PM 2.5 or CO sensor and a smartphone, and carry it from the study venue in their home neighborhood to an assigned location.[17] After arriving at the location, participants use an app on the phone to begin data collection and confirm they are inside the correct neighborhood. They remain outdoors for the duration specified by the job (either one or two hours) and then return to the study venue. In order to ensure subjects were not simply unable to get to unfamiliar neighborhoods, all participants were offered paper directions to the locations.

[17] Neither type of sensor displays the pollution level, which rules out participants learning about neighborhood pollution levels from the task.

Participant Timeline and Experimental Sample. Figure A.5 presents an overview of the study timeline from the participant perspective. The first two days consist of baseline survey data collection at the study venue in the participant's home neighborhood.

On the first day, 1,168 eligible participants provided demographic information and answered the familiarity questions discussed in section 2.1. Based on these responses, 120 participants were classified as having very low familiarity (familiar with fewer than 6 neighborhoods), and 133 were classified as having very high familiarity (unfamiliar with fewer than 4 neighborhoods). These 253 participants were excluded from our sample because we could not randomize them appropriately to the familiar or unfamiliar training locations.[18] We invited the remaining 915 to a second baseline survey, and 830 of them returned and completed the second baseline survey. The surveys are described in section 1. The 799 participants who completed this second survey and showed up the next day for the intervention form the analysis sample for the experiment.

Randomization. We randomize participants who finish the second survey into completing the job training in familiar neighborhoods (control group) or unfamiliar neighborhoods (treatment group). We cross-randomize the method by which we will elicit their work location preferences, which we discuss below. We stratify the randomization by home neighborhood and familiarity level (above- and below median). Tables A.6 and A.7 report randomization balance comparisons.

The main intervention takes place over the next three days. Participants begin the day at the study venue where they answer a short survey, then travel with the guide to the target neighborhood where they are trained. The participants return on their own to the study venue to return the air pollution measurement equipment and answer a short survey. Each field guide trains a single participant at a time. Participants are trained in three different neighborhoods on three days.

Participants work unaccompanied for the following three days, which we call the employment days. They again start and end the day at the study venue in their home neighborhood, where they answer short surveys before and after the job.

Participants are later contacted by SMS and by phone for endline data collection at least a month after training. Two to four months after the training, they receive invitations for a separate survey in various neighborhoods. We analyze both components in section 7.

[18] Average familiarity was 46% and 48% for excluded and non-excluded participants, respectively.

Target Neighborhoods. After the first baseline survey, we select 10 neighborhoods for each participant, which we henceforth refer to as "target" neighborhoods. We make this selection for all participants, before we randomize them into experimental groups.

We select 6 familiar neighborhoods and 4 unfamiliar neighborhoods for each person.[19] We then randomly designate 3 familiar and 3 unfamiliar neighborhoods as "main" familiar and "main" unfamiliar neighborhoods, respectively. Figure A.6 plots the target neighborhoods of one participant to illustrate.

Participants will complete their training on the air pollution job in neighborhoods from one of these groups, depending on their treatment assignment. Control participants will visit the three target main *familiar* neighborhoods, while treatment participants will visit the three target main *unfamiliar* neighborhoods.

The remaining *familiar* neighborhoods allow us to identify any recency effects induced by visiting a neighborhood during training. The remaining *unfamiliar* neighborhood allows us to identify any potential spillover effect of increased willingness to visit unfamiliar neighborhoods.

We select the target neighborhoods to minimize spatial spillovers between categories (e.g. accidentally inducing a visit to an unfamiliar neighborhood while traveling to a familiar one), to keep neighborhoods in each category close to each other, to balance distance from home to each category, and to prioritize nearby neighborhoods. (See appendix A.5 for details.) Table A.8 shows that the unfamiliar neighborhoods we choose using this procedure are very similar to unfamiliar neighborhoods we do not choose.

Inducing Familiarity using Job Training. On each of the three training days, participants visited their assigned neighborhoods and received training on how to perform the air pollution task. Each participant was accompanied by a field guide, and each guide accompanied a single participant at a time.[20] Field guides used a pre-specified route to reach the assigned location, and they were instructed to point out landmarks along the route to the participant and generally ensure that the participant understands the route.

Once in the neighborhood, they instructed the participant how to collect data (stay outdoors, stay in the vicinity of major roads, stand at the same location or move around as they prefer, and how to use the smartphone app). Field guides then left the participant to continue their task in the neighborhood for one hour. Each participant was informed that they would return on their own from the training neighborhood. Participants were paid 700

[19] We exclude participants from the sample who do not have 4 unfamiliar and 6 familiar neighborhoods.

[20] Field guides were recruited by Busara from each home neighborhood and trained for this study.

KSh for each training day, and given 200 KSh for transportation.

In order to minimize experimenter demand effects, participants were told that the purpose of the study was "to better understand how casual workers travel in Nairobi and how this affects their search for work opportunities." We also did not make explicit that we were randomizing training locations based on familiarity, instead telling participants "based on random computer assignment, today we will be taking you to *[name of training neighborhood]*." Surveyors and field guides were also not informed of individual participants' treatment status and field guides were not informed of the design or purpose of the experiment.

Compliance with the training assignment was almost universal.[21] On average, 99% of participants who showed up for their first day complied with their treatment assignments and completed the three days of training, and average show-up for the three employment days was 97% overall. Show-up is slightly lower in the treatment group, but this difference is not statistically significant (Table A.10).

Eliciting Job Preferences and Consideration. After the three days of training, participants are invited to the study venue for three additional days of employment. We use two different methods to elicit job choices and randomize the method across participants.

"Preferences" Elicitation. For half of the participants, we use a "preferences" elicitation method (Mas and Pallais 2017). Each morning, they answer a series of seven binary choice questions about the air pollution job they will perform that day. Each question presents two job variants and asks the respondent which one they prefer for that day. We also allow participants to turn down both jobs. For each option, we randomly vary the neighborhood where the job will take place – selecting from among the ten target neighborhoods for that respondent – and the wage, job duration, upfront cash to finance transportation, and how much of the compensation is in the form of a risky bonus payment. Participants are informed that each question has an equal and positive chance to be randomly picked and implemented. To ensure and check comprehension, the elicitation begins with a practice session.

Participant responses to these real-stakes questions allow us to estimate preferences

[21] We re-measure familiarity when participants return from the training to the study venue. Table A.9 displays the results. In the control group, 88% of respondents report having visited that neighborhood before the training day, which means that 12% had never been there, despite reporting in the baseline survey that they had. In the treatment group, 34% of participants report having already traveled to the assigned neighborhood, despite reporting in the baseline survey that they had never been. Anecdotally, this is primarily due to either disagreement about the name of a location or visiting the location reminding the treatment group of a time that they had visited. In the rest of this paper, we report intent to treat (ITT) results using the baseline familiarity measure. This corresponds to analyzing how preferences and consideration depend on *subjective* baseline familiarity, rather than objective visit history.

for working in different types of neighborhoods, the effects of the training, and the impact of inducing familiarity via training. Because the questions list specific neighborhoods as options, we interpret the results as preference estimates under full consideration.

Anticipation Elicitation. As part of the "preferences" elicitation, we also elicited job choices during the three training days. On training day $d \in \{1, 2, 3\}$, surveyors asked participants about their employment preferences for employment day $d + 3$ – that is, for employment three days in the future. Crucially, each respondent received these questions *after* learning where they would train on day d, but *before* they have actually visited the location. Participants received the exact same seven questions that they later received on the corresponding employment days $d + 3$. These questions were incentivized: respondents were informed that each question has an equal chance of being selected to be implemented three days later. We pool the 7 questions from training day d and the 7 questions from the employment day $d + 3$, and we select one of the 14 at random to be implemented.

"Open" Elicitation. People may also be less likely to consider unfamiliar neighborhoods in the first place. To measure this effect, for the other half of participants we use an "open" elicitation method. We tell participants that jobs are available in different neighborhoods across Nairobi and ask them for their most preferred location, the next most preferred location, and so on. In this elicitation, participant responses are a combination of their preferences and how likely they are to consider a given neighborhood (for more information on this design, see Section 6).

4 A Model of Neighborhood Valuation

To structure the interpretation of our results, we now present a model of how an expected-utility maximizer's revealed preferences vary with familiarity and how they would be affected by a one-visit exposure. Section 4.1 presents the model and its main results without parametric assumptions. Section 4.2 adds parametric assumptions for estimation.

4.1 Model Setup and Main Results

Consider an expected-utility maximizing agent i. When the agent enters the experiment, they are familiar with some neighborhoods n ($\text{Fam}_{in} = 1$) and unfamiliar with others ($\text{Fam}_{in} = 0$). Denote by u_{in} agent i's utility from working in neighborhood n, which we also sometimes call neighborhood quality. This is a von Neumann–Morgenstern utility, so the model allows risk aversion over neighborhood attributes such as safety. Agent i knows u_{in}

for familiar neighborhoods and has prior p_{in} over u_{in} for unfamiliar neighborhoods, with mean $\widetilde{u}^0_{in} = \mathbb{E}_{p_{in}} u_{in}$.

Exposure to an unfamiliar neighborhood via training there reveals information about u_{in}, and the agent forms a Bayesian posterior q_{in} with mean $\widetilde{u}^1_{in} = \mathbb{E}_{q_{in}} u_{in}$.[22] Beliefs are calibrated if their mean errors before and after a visit are zero. A correctly specified prior with Bayesian updating has this property by the law of iterated expectations.[23]

The agent expects to incur an additive utility cost, c_i, the first time they visit an unfamiliar neighborhood, the same for all of i's unfamiliar neighborhoods. This perceived cost might capture costly navigation, safety risks during exploration, or a psychological cost of being in a new place. The cost is sunk after the visit, so future visits do not incur this cost.

We also allow training in any kind of neighborhood, either familiar or unfamiliar, to change the utility of working there directly, for example through a recency effect. We denote this additive direct effect of training by β^T_{in}.

Denote by $Y_{in}(0)$ and $Y_{in}(1)$ agent i's potential valuations of working in neighborhood n if the agent does not train in n and if they train in n, respectively. Valuations exclude the compensation and duration attributes of a given job offer. We define the following estimands:

$$\text{familiarity premium: } \Delta^F = \mathbb{E}[Y_{in}(0) \mid \text{Fam}_{in} = 1] - \mathbb{E}[Y_{in}(0) \mid \text{Fam}_{in} = 0],$$
$$\text{exposure effect: } \Delta^U = \mathbb{E}[Y_{in}(1) - Y_{in}(0) - \beta^T_{in} \mid \text{Fam}_{in} = 0].$$

Averages weight participants equally and, within participant, neighborhoods equally. The *familiarity premium* Δ^F compares familiar and unfamiliar neighborhoods without training. The *exposure effect* Δ^U is the effect of training in an unfamiliar neighborhood beyond the direct effect of training β^T_{in}.

Assumption 1 (No spillovers among target neighborhoods)**.** *Training in one target neighborhood does not affect agent i's utility, beliefs, or perceived first-visit cost for any other target neighborhood.*

Assumption 1 has two parts. The first part rules out spillovers from or onto a familiar neighborhood. This seems plausible because participants know familiar neighborhoods from past visits. An important threat to this part is spatial: training in a familiar neighborhood

[22] The short duration of exposure during training rules out new network links and habit formation.

[23] This also applies to a situation where i previously decided which neighborhoods to visit based on Bayesian updating over noisy signals about them. In that model, the prior p_{in} is calibrated across unfamiliar neighborhoods.

may expose a participant to an unfamiliar neighborhood that is on the way. Our algorithm for choosing target neighborhoods penalizes such overlaps between the main familiar, other familiar, and unfamiliar target neighborhoods (Appendix A.5).[24]

The second part rules out spillovers among unfamiliar neighborhoods. This part is more demanding. A visit can teach general lessons about safety or navigation, and our algorithm keeps each participant's unfamiliar target neighborhoods close to each other. We test this part using the tenth target neighborhood, which is unfamiliar, never trained, and often close to the trained unfamiliar neighborhoods. We find a small, positive but insignificant spillover onto it (Table A.20). If spillovers among unfamiliar neighborhoods do exist, our estimate of Δ^U measures the effect of exposure to all three nearby unfamiliar neighborhoods.

Assumption 1 concerns valuations, not the *probability* of choosing a neighborhood in our choice elicitation. For example, visiting an unfamiliar neighborhood may make a familiar neighborhood *relatively* more or less attractive.

Proposition 1. *Under Assumption 1, the familiarity premium and exposure effect are*

$$\Delta^F = \underbrace{\mathbb{E}[c_i]}_{\substack{\text{mean perceived} \\ \text{first-visit cost}}} + \underbrace{\mathbb{E}\left[u_{in} - \widetilde{u}^0_{in} \mid \text{Fam}_{in} = 0\right]}_{\text{mean prior error}} + \underbrace{\mathbb{E}[u_{in} \mid \text{Fam}_{in} = 1] - \mathbb{E}[u_{in} \mid \text{Fam}_{in} = 0]}_{\text{sorting}},$$

$$\Delta^U = \underbrace{\mathbb{E}[c_i]}_{\substack{\text{mean perceived} \\ \text{first-visit cost}}} + \underbrace{\mathbb{E}\left[\widetilde{u}^1_{in} - \widetilde{u}^0_{in} \mid \text{Fam}_{in} = 0\right]}_{\text{mean belief revision}}.$$

Appendix A.3 contains all proofs.

Under calibrated beliefs, the mean belief revision is 0. A visit, on average, raises or lowers beliefs about an unfamiliar neighborhood by the same amount. The exposure effect then equals the average perceived first-visit cost. In this model, uncertainty about the utility of unfamiliar neighborhoods does not contribute to the effect of exposure. This happens because we estimate the revealed utility of working in a given neighborhood and, by definition, expected utility maximizing agents are risk-neutral *over utilities*, even though they may be risk averse over the underlying neighborhood attributes (Strack and Taubinsky 2026). We discuss tail risk and finite sampling in Section 5.1. The familiarity premium also includes

[24] The assumption could also fail if agents are still uncertain about familiar neighborhoods and training leads to learning based on shared or spatially correlated attributes, or if agents have reference-dependent valuations and training changes the reference points. In these cases, we conjecture that modeling spillovers explicitly would allow us to identify the exposure effect.

a sorting term that captures that the agent may be familiar with a set of neighborhoods that is better (or worse) in expectation.

With miscalibrated beliefs, the familiarity premium also contains the mean prior error, and the exposure effect contains the mean belief revision.

The familiarity premium is weakly larger than the exposure effect ($\Delta^F \geq \Delta^U$) when the following two conditions hold. First, sorting is non-negative, that is, familiar neighborhoods are better in expectation. Second, priors for unfamiliar neighborhoods are pessimistic on average, and the posterior belief is on average in between the prior and the truth; that is, a visit corrects some or all of the pessimism without making participants overly optimistic.

Anticipated Exposure. On training days, participants also choose jobs for three days later, after learning that day's training location and before visiting it (see Section 3). We call this location the announced neighborhood. Before the visit, the agent anticipates a direct effect β_{in}^{AT} of training in n, which can differ from the realized direct effect β_{in}^{T}.

Denote by Y_{in}^{A} agent i's potential valuation of working in n, after the announcement and before the visit. We define

$$\text{anticipated exposure effect: } \Delta^{AU} = \mathbb{E}[Y_{in}^{A} - Y_{in}(0) - \beta_{in}^{AT} \mid \text{Fam}_{in} = 0].$$

It is the effect of an announced training visit to an unfamiliar neighborhood beyond the anticipated direct effect of training. We make the following assumption:

Assumption 2 (Anticipation)**.** *(a) The agent learns nothing about u_{in} from the announcement, and the announcement does not affect the utility, beliefs, or costs for any other target neighborhood; (b) the agent expects to incur the announced neighborhood's first-visit cost during training; and (c) the agent treats the choice as binding.*

Part (a) extends Assumption 1 to announcements and rules out learning from the announcement itself. This seems plausible because surveyors told participants that a computer chose the training neighborhoods at random. Part (b) says that the agent expects the first-visit cost to be sunk by the day of the job. Part (c) rules out option value from refusing the job after the visit, which in our context is plausible because participants strongly prefer employment to unemployment; they refused only 0.51% of assigned jobs.

Proposition 2. *Under Assumptions 1 and 2, the anticipated exposure effect is given by*

$$\Delta^{AU} = \underbrace{\mathbb{E}[c_i]}_{\substack{\text{mean perceived} \\ \text{first-visit cost}}}.$$

The anticipated exposure effect isolates the mean perceived first-visit cost. By Proposition 1, the difference $\Delta^U - \Delta^{AU}$ equals the mean belief revision. Comparing the two effects thus tells us about beliefs. Under calibrated beliefs, there is no mean belief revision, and the anticipated exposure effect equals the exposure effect. If instead beliefs are pessimistic and a visit raises them on average, the ratio Δ^{AU}/Δ^U is at most one, and it captures the share of the exposure effect due to the perceived first-visit cost.

4.2 Estimation Using "Preferences" Elicitation Choices

In the "preferences" elicitation, participants choose between two offers at a time.[25] Let Train_{in} indicate that i trains in n. By Assumption 1, i's valuation of n in the experiment is $Y_{in}(\text{Train}_{in})$. We assume that the prior mean is the same for all of i's unfamiliar neighborhoods, $\widetilde{u}^0_{in} = \widetilde{u}^0_i$. Only utility differences between the two offers matter, so we normalize the valuation of an untrained unfamiliar neighborhood, $\widetilde{u}^0_i - c_i$, to zero. On employment days, the utility of job offer j in neighborhood n is then

$$u_{ijn} = \beta^F_{in}\text{Fam}_{in} + \beta^T_{in}\text{Train}_{in} + \beta^U_{in}\text{Train}_{in}(1 - \text{Fam}_{in}) + \beta^D d_{in} + \gamma X_{ij} + \epsilon_{ijn}. \tag{1}$$

The first three terms express the valuation $Y_{in}(\text{Train}_{in})$. Here β^F_{in} is the valuation of an untrained familiar neighborhood relative to an untrained unfamiliar one, β^T_{in} is the direct effect of training defined in Section 4.1, and β^U_{in} is the additional effect of training in an unfamiliar neighborhood. Distance d_{in} is measured from i's study venue. The randomized job attributes X_{ij} include the wage, duration, the portion of compensation that is a risky potential bonus, and any amount paid up front to reduce liquidity constraints. The choice shocks ϵ_{ijn} are independent type-I extreme value with unit scale.

Choices in trained familiar neighborhoods reveal the direct effect of training β^T_{in}. In trained unfamiliar neighborhoods, choices reveal only the sum $\beta^T_{in} + \beta^U_{in}$. To separate the exposure effect from the direct effect, we assume:

[25] We also allow participants to turn down both jobs. Due to the underemployed sample and the competitive wages, this was extremely rare, only 0.18% of the time. We therefore focus on the choice between the two offers. We study extensive-margin decisions for a different type of work opportunities in section 7.

Assumption 3 (Equal mean training effects)**.** *The direct effect of training has the same mean in familiar and unfamiliar neighborhoods:* $\mathbb{E}[\beta_{in}^T \mid \text{Fam}_{in} = 1] = \mathbb{E}[\beta_{in}^T \mid \text{Fam}_{in} = 0]$.

Assumption 3 plays the role of parallel trends. It says that training affects valuations in the same way, on average, whether it happens in a familiar or an unfamiliar neighborhood (e.g., through a recency effect).

We randomize treatment status across participants within strata. For each participant and neighborhood type (familiar or unfamiliar), we draw main target neighborhoods at random from their target neighborhoods of that type (Section 3). Conditional on baseline familiarity, the coefficients of a participant's target neighborhoods are therefore jointly independent of treatment status and of the designation of main neighborhoods. Under Assumptions 1 and 3, the mean coefficients identify the estimands (Appendix A.3.2):

$$\mathbb{E}[\beta_{in}^F \mid \text{Fam}_{in} = 1] = \Delta^F,$$
$$\mathbb{E}[\beta_{in}^U \mid \text{Fam}_{in} = 0] = \Delta^U.$$

The mean coefficient $\mathbb{E}[\beta_{in}^T]$ measures the direct effect of training, called the training effect. These equalities hold for any coefficient distribution. Estimating the means from choices nevertheless requires distributional assumptions, because choice probabilities are nonlinear in the coefficients. We next derive a normal mixed logit from a normal learning model.

Normal Learning Model Parametrization. Consider a model of Bayesian learning with normal priors and signals. Conditional on all person-level terms, the utilities of i's neighborhoods are independent normal draws:

$$u_{in} \overset{\text{iid}}{\sim} \mathcal{N}\big(u_i^F,\, \tau_F^2\big) \text{ if } \text{Fam}_{in} = 1, \quad u_{in} \overset{\text{iid}}{\sim} \mathcal{N}\big(u_i^U,\, \tau_U^2\big) \text{ if } \text{Fam}_{in} = 0.$$

The person-level means u_i^F and u_i^U can differ because of sorting. Denote $s_i = u_i^F - u_i^U$. The level u_i^U can be normalized to zero without loss of generality. A training visit to unfamiliar n yields the signal $z_{in} = u_{in} + \eta_{in}$, with $\eta_{in} \overset{\text{iid}}{\sim} \mathcal{N}(0, \sigma_\eta^2)$ independent of all other terms.

Agent i knows u_{in} when n is familiar, and for an unfamiliar n has beliefs:

$$i\text{'s model:} \quad u_{in} \overset{\text{iid}}{\sim} p_i = \mathcal{N}\big(u_i^U - e_i,\, \widetilde{\tau}_U^2\big) \text{ if } \text{Fam}_{in} = 0, \qquad z_{in} \mid u_{in} \sim \mathcal{N}\big(u_{in},\, \widetilde{\sigma}_\eta^2\big).$$

The prior error e_i measures i's pessimism. The perceived variances $\widetilde{\tau}_U^2 > 0$ and $\widetilde{\sigma}_\eta^2 \geq 0$ are common across participants and may differ from the true ones. After a visit to n, agent i

updates by Bayes' rule and places weight $\lambda = \widetilde{\sigma}_\eta^2/(\widetilde{\tau}_U^2 + \widetilde{\sigma}_\eta^2) \in [0, 1)$ on the prior mean and $1 - \lambda$ on the signal. Because i treats the mean $u_i^U - e_i$ and both variances as known, a visit to n leaves i's beliefs about all other neighborhoods unchanged. These beliefs thus satisfy the belief part of Assumption 1.

We assume the training effect is $\beta_{in}^T = \bar{\beta}^T + \theta_i^T + a_{in}^T$, with a mean-zero person component θ_i^T and a pair component $a_{in}^T \overset{\text{iid}}{\sim} \mathcal{N}(0, \omega_T^2)$ independent of all other terms. Because the distribution of β_{in}^T does not depend on Fam_{in}, Assumption 3 holds.

Assumption 4 (Joint normality)**.** *Sorting s_i, the prior error e_i, the first-visit cost c_i, and the person-level training effect θ_i^T are jointly normal with an unrestricted covariance matrix.*

We refer to the assumptions in this section as the normal learning model.

Proposition 3. *Under the normal learning model, each coefficient in* (1) *is the sum of a mean, a person component, and a component specific to the participant–neighborhood pair:*

$$\beta_{in}^k = \bar{\beta}^k + \theta_i^k + a_{in}^k, \qquad k \in \{F, T, U\}.$$

The person components satisfy $(\theta_i^F, \theta_i^T, \theta_i^U)' \sim \mathcal{N}(0, \Omega)$ with Ω unrestricted. Each pair component is normal with mean zero and independent of all other terms. The means are $\bar{\beta}^F = \mathbb{E}[c_i] + \mathbb{E}[e_i] + \mathbb{E}[s_i]$ and $\bar{\beta}^U = \mathbb{E}[c_i] + (1 - \lambda)\,\mathbb{E}[e_i]$.

Proposition 3 gives a normal mixed logit with random coefficients at both the person and pair levels (Revelt and Train 1998). Appendix A.3.3 contains derivations.

Normal Mixed Logit Estimation. We estimate the normal mixed logit by simulated maximum likelihood. Appendix A.3.5 has details. We treat all variances and covariances as nuisance parameters. The employment-day model in equation (1) has seven identified variance and covariance parameters: four person-level moments and three pair-level variances. We obtain standard errors and 95% intervals from 500 Bayesian bootstrap replications that reweight participants.

We also estimate a standard logit, which sets all random-coefficient variances to zero. A model with common first-visit costs, misperceptions, and sorting can generate homogeneous valuation differences, providing a motivation for this specification. In practice, we find the standard logit and the mixed logit yield almost identical conclusions. Thus, for most additional analyses we use the standard logit for simplicity.

Our baseline specifications omit neighborhood fixed effects, so as not to condition on the

realized average utility of particular neighborhoods.[26] We also report specifications with home-neighborhood by destination-neighborhood fixed effects, $\mu_{h(i)n}$, which absorb distance. Similar estimates with and without these fixed effects suggest that shared neighborhood valuations and sorting on past noisy signals are not the main drivers of the results.

Anticipated Exposure. On training days, participants make the same kind of choices for jobs three days later (Section 4.1). By Assumptions 1 and 2, the utility of job offer j on a training day or on an employment day is

$$\begin{aligned} u_{ijn} = \beta_{in}^{F}\text{Fam}_{in} + \beta_{in}^{T}\text{Train}_{in} + \beta_{in}^{U}\text{Train}_{in}(1-\text{Fam}_{in}) \\ + \beta_{in}^{AT}\text{Antic}_{in} + \beta_{i}^{AU}\text{Antic}_{in}(1-\text{Fam}_{in}) + \beta^{D} d_{in} + \gamma X_{ij} + \epsilon_{ijn}. \end{aligned} \tag{2}$$

Here Antic_{in} is an indicator for when n is i's announced neighborhood on a training day, β_{in}^{AT} is the anticipated direct effect of training defined in Section 4.1, and β_{i}^{AU} is the additional effect for an unfamiliar neighborhood. On training days, $\text{Train}_{in} = 1$ for neighborhoods visited on earlier training days. On employment days, $\text{Antic}_{in} = 0$ and equation (2) reduces to equation (1). The choice shocks have unit scale on training days as well.[27]

Assumption 5 (Equal mean anticipated training effects)**.** *The anticipated direct effect of training has the same mean in familiar and unfamiliar neighborhoods:* $\mathbb{E}[\beta_{in}^{AT} \mid \text{Fam}_{in} = 1] = \mathbb{E}[\beta_{in}^{AT} \mid \text{Fam}_{in} = 0]$.

Randomization again makes the coefficients independent of treatment status and of the designation of main neighborhoods, conditional on baseline familiarity. Under Assumptions 1, 2, and 5, the mean coefficient identifies the anticipated exposure effect: $\mathbb{E}[\beta_{i}^{AU}] = \Delta^{AU}$ (Appendix A.3.2). The mean coefficient $\mathbb{E}[\beta_{in}^{AT}]$ measures the anticipated training effect.

We extend the normal learning model to equation (2) with two additions. First, the anticipated training effect takes the same form as the training effect: $\beta_{in}^{AT} = \bar{\beta}^{AT} + \theta_{i}^{AT} + a_{in}^{AT}$, with a mean-zero person component θ_{i}^{AT} and a pair component $a_{in}^{AT} \overset{\text{iid}}{\sim} \mathcal{N}(0, \omega_{AT}^{2})$ independent of all other terms. Because the distribution of β_{in}^{AT} does not depend on Fam_{in}, Assumption 5 holds. Second, we add θ_{i}^{AT} to the jointly normal person-level

[26] For example, suppose agents share the same neighborhood utilities, $u_{in} = u_{n}$, and familiarity reflects past noisy signals of quality. Within a given neighborhood, unfamiliar agents have received worse signals and can revise their valuations upward after a visit even when beliefs are calibrated across neighborhoods.

[27] We later show this is a plausible assumption. We find similar results when we estimate equation (2) on a sample with training days only, see columns 4 and 5 in Table A.13.

terms of Assumption 4. Proposition 3 then extends to equation (2). The new coefficient $\beta_i^{AU} = \bar{\beta}^{AU} + \theta_i^{AU}$ has no pair component. Its mean is $\bar{\beta}^{AU} = \mathbb{E}[c_i]$. The five person components are jointly normal with an unrestricted covariance matrix (Appendix A.3.3). We estimate the resulting normal mixed logit as in Section 4.2. It has fifteen identified variance and covariance parameters: eleven person-level moments and four pair-level variances (Appendix A.3.4).

5 Impact of Neighborhood Exposure: Results

We now estimate the familiarity premium and the exposure effect from job choices in the "preferences" elicitation. We then use data on beliefs to test for pessimistic priors about unfamiliar neighborhoods, and use the anticipation elicitation to separate the exposure effect into belief revision and a perceived first-visit cost. Finally, we consider three sources of this cost: navigation, exploration risk, and psychological costs.

5.1 The Revealed Preference for Familiar Neighborhoods

Table 2 reports estimates of equation (1). The sample includes nine target neighborhoods for each participant: the three main familiar, the three main unfamiliar, and the three other familiar neighborhoods.[28] We mainly discuss column 1, which reports the normal mixed logit of Section 4.2. We convert coefficients to Kenyan shillings (KSh) by dividing them by the coefficient on expected compensation, γ^E.

First, participants strongly prefer to work in familiar neighborhoods. The familiarity premium Δ^F of 1.09 is equivalent to 115 KSh. It amounts to 23 percent of the median daily wage outside the experiment (500 KSh). Equivalently, participants are willing to travel an additional 3.8 kilometers or work about an additional hour and a half to avoid working in an unfamiliar neighborhood.

Second, a single one-hour exposure during training suffices to erase this familiarity premium. The exposure effect Δ^U of 1.13 is equivalent to raising the wage by 119 KSh or bringing the neighborhood 3.9 km closer. This effect is in addition to the general effect of training in a neighborhood ($\mathbb{E}[\beta_{in}^T]$), which is also positive but smaller (0.31). The exposure effect is statistically indistinguishable from the familiarity premium. The ratio Δ^U/Δ^F is 1.04 (95% interval $[0.67, 1.33]$).

[28] To test the second part of Assumption 1, we add the tenth target neighborhood, which is unfamiliar and was not included in the training, and estimate a separate coefficient for its interaction with treatment. Table A.20 shows that this neighborhood receives a positive but imprecisely estimated spillover.

Table 2: Revealed Preference Estimates from Job Choices

	(1)	(2)	(3)	(4)
β^F Baseline Familiar	1.09***	0.89***	0.85***	0.76***
	(0.14)	(0.14)	(0.11)	(0.12)
β^T Training	0.31*	0.35**	0.29**	0.33**
	(0.13)	(0.12)	(0.10)	(0.11)
β^U Training × Unfamiliar	1.13***	1.13***	0.83***	0.93***
	(0.26)	(0.23)	(0.21)	(0.20)
Distance (km)	-0.29***		-0.24***	
	(0.02)		(0.02)	
Job duration (hrs)	-0.73***	-0.74***	-0.58***	-0.62***
	(0.08)	(0.07)	(0.06)	(0.06)
γ^E $\mathbb{E}$ Compensation (100 KSH)	0.95***	0.97***	0.76***	0.83***
	(0.04)	(0.04)	(0.03)	(0.03)
Cash Upfront (100 KSH)	0.05	0.06	0.06	0.06
	(0.05)	(0.05)	(0.04)	(0.04)
Bonus (100 KSH)	-0.26***	-0.26***	-0.21***	-0.22***
	(0.01)	(0.01)	(0.01)	(0.01)
Familiarity Premium β^F/γ^E (100 KSH)	1.15	0.92	1.11	0.92
	[0.85, 1.43]	[0.63, 1.19]	[0.82, 1.37]	[0.62, 1.18]
Exposure Effect β^U/γ^E (100 KSH)	1.19	1.16	1.09	1.13
	[0.62, 1.71]	[0.65, 1.60]	[0.56, 1.58]	[0.63, 1.57]
Ratio β^U/β^F	1.04	1.26	0.98	1.23
	[0.67, 1.33]	[0.88, 1.63]	[0.59, 1.27]	[0.83, 1.58]
Model	Mixed logit	Mixed logit	Logit	Logit
Home × neighborhood FE		Yes		Yes
Choices	6,756	6,747	6,756	6,747
People	395	395	395	395

Notes: This table reports estimates of job choices on employment days in the "preferences" elicitation sample, based on equation (1). "Baseline Familiar" is an indicator equal to one if the respondent reported during the first baseline survey they have been to the neighborhood. "Training" is an indicator equal to one if a participant trained in the neighborhood, while "Training × Unfamiliar" is constructed similarly but only switched on for participants in the treatment group, for whom the neighborhood is always unfamiliar at baseline. The other job attributes are randomized: job duration, expected compensation, the amount offered in advance to ease liquidity constraints, and the amount that depended on a risky bonus. Columns 1-2: mixed logit in which β^F, β^T and β^U vary across participants and, within participant, across neighborhoods, following the normal learning model of Section 4.2. The table reports mean coefficients. (See Table A.11 for variance estimates.) Columns 3–4: logit. Standard errors (parentheses) and 95% intervals (brackets) are the standard deviation and the 2.5th and 97.5th percentiles across 500 Bayesian bootstrap replications that reweight participants. β^F/γ^E and β^U/γ^E are in 100 KSH. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Column 2 adds home-by-destination fixed effects $\mu_{h(i)n}$, which absorb distance. These fixed effects lower the familiarity premium by about a fifth, to 92 KSh, and leave the exposure effect nearly unchanged at 116. The 95% interval for Δ^U/Δ^F, [0.88, 1.63], includes one.

Columns 3 and 4 report results from a binary logit. In KSh, these columns give familiarity premia and exposure effects within about 10 percent of those from the mixed logit models.[29]

Results are very similar with a linear specification (Table A.12). Participants are 8-10 percentage points more likely to pick a familiar neighborhood option, and experimental exposure has an effect of 9-10 pp. Table A.10 shows that there is a small, statistically insignificant difference in attrition (3.9% vs 1.3%) during employment days.

Discussion. We interpret these results through the lens of the model of Section 4. By Proposition 1, the familiarity premium and the exposure effect share the mean perceived first-visit cost. The familiarity premium also includes sorting and the mean prior error, while the exposure effect includes the mean belief revision.

In column 1, the point estimates of the familiarity premium and the exposure effect are essentially equal. Their difference equals sorting plus the mean error remaining after one visit, $\mathbb{E}[u_{in} - \widetilde{u}^1_{in} \mid \text{Fam}_{in} = 0]$. Both sorting and this mean error are likely to be non-negative. The equal point estimates thus suggest that both terms are zero. A zero sorting term means that the aversion to unfamiliar neighborhoods we observe is not due to sorting on neighborhood quality.[30] A zero mean error after one visit means that a single visit is enough to bring participants' beliefs about an unfamiliar neighborhood in line with its true value, on average, consistent with the convergence in beliefs we document below in Section 5.2.

The exposure effect is the sum of the average perceived first-visit cost, and the mean belief revision after one visit. The results so far are silent on this decomposition, and we return to this issue below in section 5.2.

Tail Risk and Finite Samples. Could our results be explained by a calibrated belief of a small risk of unfamiliar neighborhoods having an extremely bad, persistent attribute? (Here we consider only the risk that the neighborhood is *persistently* bad, and we consider below the role of *transient* risks that are experienced only during first-time exploration.) If this rare realization does not occur in our finite sample of exposure events, it might explain the initial

[29] Relative to the mixed logit, the logit coefficients are smaller. The logit treats heterogeneity in valuations as part of the choice shocks. The scale cancels from money-metric values, which are ratios of coefficients.

[30] This is not a statement about whether baseline familiarity patterns are optimal or not. Our experiment is designed to measure the *utility cost* of traveling to and working in a neighborhood, but we do not measure the real-world *benefits* participants enjoy from different neighborhoods. (Our design controls these benefits of work and keeps them constant across neighborhoods.)

reluctance to visit unfamiliar neighborhoods, and why that reluctance vanishes after training.

We believe this is unlikely to explain an important part of our results for several reasons.

First, given our pattern of results this type of risk would need to be serious, rare, immediately recognized based on a short exposure, and specific to the individual's experience of the neighborhood. This seems implausible. In the "preferences" sample, we conducted almost 600 training sessions exposing individuals to initially unfamiliar neighborhoods. For a negative event that has a 1% probability independently across sessions, the chance it did not happen in a sample this large is 0.2%, so any such risk must be very rare. To explain the baseline familiarity premium, the tail neighborhood quality must then be extremely bad. Next, to explain why a single one-hour visit erases the premium, the bad outcome must also be easy to recognize during that visit. Additionally, Figure A.7 shows there are no outliers in the distribution of neighborhood fixed effects estimated in a logit with neighborhood fixed effects on choices with guaranteed compensation. The worst neighborhood has a (precisely estimated) disutility of around 125 KSh relative to the mean. This means that any quality tail risk must be individual-neighborhood specific rather than neighborhood specific.

Second, we separate our analysis by individual baseline beliefs about safety in an unfamiliar neighborhood. If concerns about tail risk are driving our results, the effects should be concentrated when participants rate an unfamiliar neighborhood as unsafe, or they are uncertain. We define an indicator for the participant rating an unfamiliar neighborhood as "Safe" or "Very Safe," and for which they report being "Completely Confident" about the rating. We find that 35% of all unfamiliar neighborhood ratings satisfy both conditions. Table A.14 runs the analysis interacting unfamiliar coefficients with this measure and reports the logit results from Table 2 for comparison. For neighborhoods that respondents confidently believe are safe, the familiarity premium and exposure effects retain the same sign as on the full sample. The coefficients are between 11% and 19% smaller than for other unfamiliar neighborhoods, and none of these differences is statistically significant at conventional levels. We can reject with 95% confidence a familiarity premium smaller than the equivalent of a 1.9 km longer commute, and an exposure effect smaller than the equivalent 0.7 km. These results suggest that, at most, only a small share of our results can be explained by participants avoiding unfamiliar neighborhoods because of safety concerns.[31]

[31]This exercise also helps address a separate potential problem, which is that our logit estimation using finite, bounded variation in wages and other attributes might not be able to capture the average exposure effect if some participants have extremely negative valuations. We also think this issue is not a major concern in our setting, because extremely few participants update negatively about neighborhood safety, and this happens at similar rates for visiting familiar and unfamiliar neighborhoods (Figure A.8, panels D and E).

Heterogeneity. We find limited heterogeneity in the effects of familiarity and exposure: results are similar for participants with low and high baseline familiarity, and for neighborhoods near and far from home (Tables A.16 and A.17). Familiarity matters more for men relative to wages, but a single exposure erases the premium for both men and women (Table A.18). The familiarity premium is larger for neighborhoods participants know better, such as those they know how to reach or where they can name a landmark (Table A.19).

5.2 Mechanisms

In this section we consider what mechanisms may drive the effect of exposure. In our experiment's registration we stated that "We will estimate the familiarity premium and test whether certain behavioral biases may amplify this effect." Our exploration of mechanisms is guided by theory but was not preregistered.[32] We begin by providing evidence on belief misperceptions and the split of the exposure effect between misperceptions and first-visit costs, and then turn to assessing different sources of costs and psychological explanations.

Poorly-Calibrated Priors. One reason people may avoid unfamiliar neighborhoods is that they expect these neighborhoods to be worse than they are, a form of fear of the unknown. This mechanism is closely related to the original affective conditioning explanation for the mere exposure effect (Zajonc 1968). In our model, such pessimism contributes to the exposure effect through the revision in beliefs after a visit (Proposition 1). Our revealed preference results in Section 5.1 suggest that a single visit fully corrects this pessimism: the familiarity premium and the exposure effect are nearly equal, which in our model is consistent with no sorting on average and beliefs that are correct on average after one visit. We now test both points using direct measures of beliefs.

We collect data on beliefs for the six main target neighborhoods (three familiar and three unfamiliar) during the second baseline survey for all participants. After each training day, when participants return to the study venue, we collect the same beliefs questions again referring to the neighborhood that was just visited.

We measure five dimensions of beliefs about labor market potential and safety. We record responses on a Likert-like scale and code responses on a scale from 1 to 5, where 5 is the best outcome. For each question, we also elicit respondent confidence in their answer. See Appendix A.6 for precise question wording.

For respondent i, a neighborhood n where i was trained, and data collection time $t = 0$

[32] The trial was registered at https://www.socialscienceregistry.org/trials/11205

(baseline 2) or $t = 1$ (after training), we estimate:

$$\text{Belief}_{int} = \alpha\, \text{AfterTrain}_t + \beta\, \text{BeforeTrain}_t \times \text{Treated}_i + \gamma\, \text{AfterTrain}_t \times \text{Treated}_i + \delta + \epsilon_{int}. \quad (3)$$

$Belief_{int}$ is the belief or certainty rating from 1 to 5.[33]

Control group respondents are asked about familiar neighborhoods, while those in the treatment group are asked about unfamiliar neighborhoods. We interact the Treated_i indicator with pre- and post- indicators to compare how beliefs vary across familiarity and across time.

Table 3 shows the results. At baseline, individuals are on average more pessimistic about unfamiliar neighborhoods, as indicated by the negative β coefficients. Respondents rate unfamiliar neighborhoods 0.24 points lower on their own likelihood of finding a casual job in the neighborhood, and 0.25 points lower on the likelihood that an average resident of their neighborhood finds a job in the neighborhood. We see no differences for wages conditional on finding a job. Travel safety to an unfamiliar neighborhood is rated 0.39 points lower, while the neighborhood's safety is 0.17 points lower.

After the training, these differences vanish. All γ coefficients are smaller in magnitude, close to zero, and never statistically significant at the 5% threshold. The probability of others finding a job remains significant at the 10% level, but the coefficient shrinks substantially. A single in-person visit eliminates the initial imbalance in ratings about these neighborhoods.[34]

The beliefs data thus show that participants hold pessimistic beliefs about unfamiliar neighborhoods, which may account for part of the exposure effect. They also confirm our interpretation of the revealed preference results in Section 5.1: one visit is enough to correct this pessimism. We next estimate how much of the exposure effect this pessimism explains.

Do Subjects Anticipate How Their Preferences Change After Exposure? The exposure effect is the sum of two components: first-visit cost, and the mean belief revision after one visit. The anticipation elicitation allows us to separate these two components.

On each training day, participants choose jobs for three days later after learning the training location but before visiting it (Section 3). Under Assumption 2(a), the announcement provides no information about the neighborhood. If the aversion to unfamiliar neighborhoods

[33] Results with home by neighborhood fixed effects ($\mu_{h(i)n}$) are similar.

[34] We avoid interpreting the direct effects of After Training on beliefs (worse impressions of job finding and better impressions of safety) because we are unable to disentangle true changes from time of day or mood effects. The randomized comparison does not suffer from either of these issues. Appendix A.7 has additional results on beliefs.

Table 3: Beliefs about Neighborhoods Before and After Exposure

	Dependent variable:				
	Find Job	Find Job (Others)	Pay is Good	Travel Safety	Overall Safety
	(1)	(2)	(3)	(4)	(5)
After Training	−0.134***	−0.189***	−0.055	0.140***	0.237***
	(0.039)	(0.039)	(0.037)	(0.033)	(0.037)
Before Training × Treated	−0.243***	−0.248***	−0.053	−0.394***	−0.172**
	(0.055)	(0.053)	(0.058)	(0.053)	(0.052)
After Training × Treated	−0.075	−0.090	−0.011	0.003	0.024
	(0.051)	(0.052)	(0.047)	(0.040)	(0.041)
N	4,573	4,564	4,457	4,579	4,507
Pre-visit Control Mean	3.418	3.427	3.580	4.030	3.844
SD	1.022	0.996	1.047	0.887	0.975
Before Train.×T=After Train.×T, p-val	0.004	0.005	0.439	0.000	0.000

Notes: This table reports results from regressions based on equation (3). The sample consists of our full experimental sample of 799 participants. All outcomes are rated on a Likert scale from 1 to 5 with 5 being the most positive outcome. Standard errors clustered at the participant level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

reflects beliefs about neighborhood quality, the announcement should therefore leave choices unchanged. The same argument applies to a broader set of models. For example, for a maxmin expected utility maximizer who thinks the visit fully reveals neighborhood quality, the announcement does not change valuations.[35]

In contrast, the announcement changes the timing of the first-visit cost. By the day of the job, participants expect to have paid this cost during training (Assumption 2(b)). A perceived first-visit cost therefore raises the valuation of the announced unfamiliar neighborhood. Propositions 1 and 2 formalize these arguments. The anticipated exposure effect Δ^{AU} equals the mean first-visit cost, and the difference $\Delta^{U} - \Delta^{AU}$ equals the mean belief revision.

We estimate Δ^{AU} from the pooled choices on training and employment days, using equation (2) and the normal mixed logit of Section 4.2.

Table 4 reports estimates of equation (2). Columns 1 and 2 report the normal mixed logit and columns 3 and 4 the standard logit. Columns 2 and 4 add home-by-destination fixed effects. In column 1, the familiarity premium of 121 KSh and the exposure effect of

[35]When the agent is instead unsure how informative the visit will be, she evaluates it under a worst-case view of its informativeness, and the announcement can only lower the valuation (Epstein and Schneider 2008). Our anticipated exposure effect therefore remains a lower bound on perceived first-visit costs.

Table 4: The Impact of Realized and Anticipated Exposure on Job Choices

	(1)	(2)	(3)	(4)
β^F Baseline Familiar	0.97***	0.84***	0.78***	0.72***
	(0.08)	(0.08)	(0.07)	(0.07)
β^T Training	0.18	0.21*	0.18*	0.21**
	(0.09)	(0.09)	(0.08)	(0.08)
β^U Training × Unfamiliar	1.00***	0.98***	0.77***	0.81***
	(0.16)	(0.16)	(0.13)	(0.13)
β^{AT} Anticipate Training	-0.08	-0.06	-0.06	-0.04
	(0.12)	(0.12)	(0.09)	(0.10)
β^{AU} Anticipate Training × Unfamiliar	0.64***	0.59**	0.48**	0.48**
	(0.18)	(0.18)	(0.15)	(0.15)
β^{AU}/β^U	0.64	0.60	0.63	0.59
	[0.33, 0.97]	[0.25, 0.95]	[0.26, 0.98]	[0.23, 0.94]
Familiarity Premium β^F/γ^E (100 KSH)	1.21	1.04	1.19	1.04
	[1.01, 1.39]	[0.85, 1.23]	[0.99, 1.35]	[0.85, 1.22]
Exposure Effect β^U/γ^E (100 KSH)	1.25	1.22	1.16	1.17
	[0.82, 1.63]	[0.81, 1.54]	[0.76, 1.55]	[0.76, 1.48]
Model	Mixed logit	Mixed logit	Logit	Logit
Home × neighborhood FE		Yes		Yes
Choices	13,658	13,643	13,658	13,643
People	400	400	400	400

Notes: This table reports estimates of equation (2) from job choices on training and employment days. The sample restrictions, variable definitions, columns, and inference follow Table 2. In the mixed logit, β^{AT} varies across participants and neighborhoods, and β^{AU} varies across participants only (Section 4.2). All columns also control for the job attributes, for distance (columns 1 and 3), and for an intercept for the first offer on training days. The table does not report these coefficients. Under the assumptions in Section 4 and if the mean belief revision and the mean perceived cost are nonnegative, β^{AU}/β^U is the share of the exposure effect due to the perceived first-visit cost. Table A.13 reports the variances of the random coefficients. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

125 KSh are close to the estimates in Table 2.

Participants value an unfamiliar neighborhood more as soon as they learn that they will train there. In column 1, the anticipated exposure effect Δ^{AU} of 0.64 is equivalent to 80 KSh. The ratio Δ^{AU}/Δ^U is 0.64 (95% interval $[0.33, 0.97]$). The other columns give ratios between 0.59 and 0.63. In all four columns, we reject equality of the two effects. The anticipated training effect is small and statistically insignificant in all columns.

Propositions 1 and 2 allow us to interpret these results. The perceived first-visit cost accounts for 59 to 64 percent of the exposure effect, with the remainder (36 to 41 percent)

due to mean belief revision. Together with the earlier results using belief data showing pessimistic beliefs on average that converge after one visit, we conclude that the exposure effect reflects both an anticipated first-visit cost, and an (unanticipated) miscalibrated belief about unfamiliar neighborhoods.

Two alternative explanations for a positive anticipated exposure effect would violate Assumption 2. First, option value would violate part (c). Participants might choose a job in the announced neighborhood and plan to opt out if training reveals that working there is worse than not working. We think this is unlikely because participants strongly prefer employment over unemployment. Participants refused the job that was drawn from their choices for implementation only 0.51% of the time. Among jobs drawn from training-day choices, they refused four of 583 overall and none of 88 in unfamiliar neighborhoods.

Second, an endorsement effect would violate part (a). Participants could interpret the training assignment as a signal of neighborhood quality. This is unlikely for two reasons. First, participants were told, truthfully, that a computer chose the training locations at random. Second, we offered jobs in every neighborhood that we asked about. A training assignment is unlikely to signal more about quality than these offers do.

In sum, the exposure effect reflects a combination of miscalibrated pessimism about unfamiliar neighborhoods, and perceived first-visit costs. We now turn to possible sources of the first-visit cost.

Types of First-Visit Costs. We find little evidence that **first-time travel costs** are large enough to explain the familiarity premium. Individuals are willing to travel an additional 3.8 km or work for an additional hour and a half in order to work in a familiar neighborhood. Thus, for first-time navigation costs to explain the premium, we would expect travel times to increase by a similar amount. This does not appear to be the case.

In Table A.21 we regress the travel time it takes participants to reach a neighborhood where they work, on baseline familiarity and experimentally induced visits. We use a Heckman (1976) two-step selection model to control for selection into working in an unfamiliar neighborhood, using the random compensation and other job attributes as instruments.

It takes participants slightly longer to reach neighborhoods they have never been to before, but we can reject an increase of more than 15 minutes for the entire trip there and back. Training has a small effect on travel times, but we do not find any additional effect for training in unfamiliar neighborhoods and can reject at 95% level a decrease in round-trip travel time of more than 9 minutes. Table A.21 also includes self-reported measures of navigation such as getting lost and asking for directions. Only 6% of respondents report

getting lost during the trips and this is neither differential by familiarity nor by exposure.

Another potential source of first-visit costs is **exploration risk**, if visiting a new neighborhood for the first time is risky. We find mixed evidence for this channel. First, there is relatively little potential for risk in the experiment. Individuals are only in the locations for an hour or two, and their payment and employment status are guaranteed. We also do not observe any participants in our experiment encountering adverse experiences. Thus, for exploration risk to explain the premium we estimate, it must be a rare, non-employment risk that can occur in a short time period and that vanishes after one visit. The results in the previous paragraphs already speak to this issue by showing that the risk of getting lost along the way — a plausible proxy for more significant risks — is not statistically higher when traveling to unfamiliar neighborhoods.

We find some support for exploration risk when we analyze neighborhoods that respondents are confident it is safe to travel to. The familiarity premium and exposure effect are 20-30% smaller for this type of neighborhoods, though the difference is insignificant, p=0.16 to 0.32 (Table A.15). However, even for these neighborhoods there is a sizable premium and effect of exposure and we do not find any heterogeneity by beliefs about the safety of the neighborhood itself (Table A.14). Exploration risk is unlikely to be the entire story.

We believe our results are consistent with two kinds of **"psychological" first-visit costs**. First, it is possible that participants hold incorrect beliefs about the size of real first-visit costs. For example, agents might be overly pessimistic about how long it will take them to reach a location for the first time or overestimate their vulnerability in unfamiliar locations. In this case, participants' true valuation changes after visiting and *anticipated* valuation changes will be larger than the true costs we can measure.

Our results are also consistent with an effort required to travel to and spend time in a new location for the first time, for example due to processing fluency (Winkielman et al. 2003). In this account, when exposed to a neighborhood, individuals must exert more effort to attend to all of the novel stimuli. Over time participants then learn which stimuli they can ignore. This increase in fluency then may improve individuals' willingness to work in a neighborhood. This mechanism may also be interrelated with incorrect perceptions, for example, if individuals feel less safe when the stimuli they are processing are not fluent.

Implications of the Exposure Effect for Longer Horizon Decisions. Having established an exposure effect and analyzed mechanisms, we now discuss briefly how impactful this is likely to be for decisions such as exploring a new neighborhood to search for work or

other opportunities. In our experiment, participants make such decisions for one-day jobs, whereas in other settings, casual work opportunities may come with the possibility of repeat work in the future.

Belief misperceptions and first-visit costs have different effects on decisions over opportunities with a stream of future benefits. With misperceptions, the agent expects the neighborhood to be worse in all future visits. Under our decomposition based on Table 4, 36% of the exposure effect is due to misperceptions, which means that people believe that every day working in an unfamiliar neighborhood has a utility cost equivalent to 45 KSh ($= 125 \times 0.36$ from Table 4, column 1) or 9% of median daily wage. This bias applies even to a forward-looking and patient person who takes all potential benefits of a visit into account.

By contrast, a first-visit cost may be large for a single day but is paid only once, so one might expect it to be small relative to repeated benefits from knowing the neighborhood. Nevertheless, a large literature has shown that small upfront costs routinely shift high-stakes decisions across many domains, including spatial and labor-market decisions (Bryan, Chowdhury, et al. 2014; Franklin 2018; Abebe, Caria, and Ortiz-Ospina 2021), health (Thornton 2008), retirement savings (Madrian and Shea 2001), take-up of social benefits (Bhargava and Manoli 2015), and education and financial aid (Bettinger et al. 2012). One theoretical explanation of this type of behavior is naive quasi-hyperbolic discounting (O'Donoghue and Rabin 1999). In our context, these agents are not persuaded by persistent streams of benefits from exploration because they (incorrectly) believe they will not be present biased tomorrow and will explore then. We show in Appendix A.8 that such an agent will repeatedly procrastinate visiting an unfamiliar neighborhood they believe offers a benefit below 22 KSh for every day in the future, equivalent to commuting 0.7 km closer every day.

6 The Effect of Familiarity on Consideration Sets

So far, we discussed results from preferences elicited from study participants by confronting them with specific neighborhood options. This method plausibly measures respondent preferences for working in different types of neighborhoods, but it shuts down any role that familiarity and exposure may have on how likely people are to consider a neighborhood as an option to begin with. If individuals are less likely to retrieve unfamiliar locations from memory when constructing consideration sets, exposure may affect choices beyond the effects on preferences that we have studied so far (Bordalo et al. 2020; Phillips et al. 2019). In this section, we study whether unfamiliar neighborhoods are less likely to be considered as potential places to work and whether exposure changes consideration sets.

To measure how familiarity affects consideration sets, for half of our sample we elicit choices by asking respondents an open-ended question about locations where they would like to work. For each of the three employment days, when workers show up at the study venue in the morning, a surveyor tells them that air pollution monitoring jobs are available in some neighborhoods and not others that day, and that availability is random, with each neighborhood having an independent 1 in 4 chance of a job being available. The wage and duration are the same for all neighborhoods, and we randomize these at the participant by day level. The surveyor then asks the participant to begin to report an ordered list of neighborhoods where they would be willing to work, and the surveyor records the neighborhoods listed by the respondent, one by one. After the list is complete, the surveyor leaves to check which neighborhoods are available that day, and the respondent is assigned to work that day in the first available neighborhood on their list.

Under this elicitation method, choices reflect a mix of preferences and consideration. Respondents have an incentive to report neighborhoods in decreasing order of preference of performing the air pollution job in that location. However, because participants need to come up with the neighborhoods to rank, ease of consideration may also play a role.

We interpret this data using the classic logit consideration model from Goeree (2008). In this model applied to our elicitation, each participant first comes up with a consideration set over neighborhoods and an outside option, then ranks the options within the consideration set based on a ranked logit model. Neighborhood n enters the consideration set with probability ϕ_{in}, independent across neighborhoods and participants, given by $\phi_{in} = \frac{\exp(\eta^C_{in})}{1+\exp(\eta^C_{in})}$, where

$$\eta^C_{in} = \alpha^{CF}\text{Fam}_{in} + \alpha^{CT}\text{Train}_{in} + \alpha^{CU}\text{Train}_{in} \times \text{Unfam}_{in} + \alpha^{CD}d_{in} + \mu_n + (\alpha^C)'X_i, \quad (4)$$

where μ_n are a full set of neighborhood fixed effects, and X_i includes the randomly drawn job wage and job duration, which do not depend on the neighborhood in this elicitation. The outside option $n = 0$ of not working that day is always in the consideration set ($\phi_{i0} = 1$).

Under this model, respondents in the open elicitation will rank all neighborhoods in their consideration set that have utility above the outside option of not working that day. We documented earlier that in the "preferences" elicitation, both jobs are refused only 0.18% of the time, so we assume that participants mention all the neighborhoods in their consideration set. We estimate a binary logit model for whether a given neighborhood n was mentioned anywhere in the list, using equation (4), on the sample of all neighborhoods within 75

minutes from the participant's home neighborhood.[36]

Table 5 reports the determinants of consideration on the first day of the open elicitation, when participants had to first generate their consideration sets. Participants are more likely to consider familiar neighborhoods, and less likely to consider distant neighborhoods. We stress that through the lens of our model, these results only measure whether participants *think* about these options, not how much they like them, because participants should mention all jobs they are willing to work in and almost never turn down jobs. These consideration effects are similar to our earlier preferences estimates, consistent with work showing that people are more likely to consider options that were useful in the past (Phillips et al. 2019).[37]

The coefficient α^{CT} that measures the effect of training in a familiar neighborhood has a very large value of around 1.4 log odds points on consideration. This means that training neighborhoods were highly salient for study participants. On average, participants mentioned 80% of the familiar neighborhoods where they trained.

Above and beyond this effect, training in an unfamiliar neighborhood has a positive effect of 0.4-0.5 on consideration, significant at the 5% level. This means that experimental exposure partially eliminates the initial consideration familiarity premium.[38] On days 2 and 3 the training coefficient is larger and experimental exposure no longer has an effect on consideration (Tables A.22 and A.23).

These results suggest that participants are less likely to consider unfamiliar neighborhoods, and, at least initially, a one-time short exposure reverses part of this effect. The main advantage of the logit consideration model is that it is widely used to study the distinction between preferences and what people consider. One possible issue with this model is that it ignores the sequential nature of the elicitation procedure. In Appendix A.9, we estimate a two-self model where we model the sequential process explicitly. The results confirm the results from Table 5: participants are less likely to consider unfamiliar neighborhoods, and exposure partially reverses this effect, above and beyond a large effect of training.

[36] If some neighborhoods within the 75 minute radius are inferior to the outside option, this regression cannot separately identify consideration from not wanting to work in a specific neighborhood.

[37] Choices made under partial consideration lead to welfare losses relative to full consideration. When the determinants of consideration and preferences are positively correlated, these losses are lower than with random consideration, but not completely eliminated.

[38] Note, however, that due to the large α^{CT} coefficient, participants are more likely to consider an unfamiliar neighborhood where they trained than a familiar neighborhood where they did not train.

Table 5: Determinants of Consideration in "Open" Elicitation (Day 1)

	(1)	(2)	(3)
Baseline Familiar	1.06***	0.79***	0.92***
	(0.062)	(0.073)	(0.077)
Training	1.44***	1.38***	1.39***
	(0.126)	(0.149)	(0.139)
Training × Unfamiliar	0.39*	0.54*	0.45*
	(0.187)	(0.228)	(0.197)
Distance (km)	-0.19***		-0.21***
	(0.011)		(0.013)
Job Compensation (100 KSh)	0.08	0.09*	0.08*
	(0.041)	(0.039)	(0.039)
Job Duration (hours)	-0.11	-0.13	-0.12
	(0.084)	(0.085)	(0.082)
NBH x home FE		Yes	
NBH FE			Yes
N	10,443	10,443	10,443

Notes: This table reports estimates of the determinants of consideration in a model based on Goeree (2008), using a binary logit model based on equation (4). The outcome is an indicator of whether the respondent mentioned a specific neighborhood in their ranked list. The sample is all neighborhoods within 75 minutes of the respondent's home neighborhood, for all respondents in the "open" elicitation. The three columns use the logit specifications from columns 3 and 4 of Table 2 plus a logit with distance and neighborhood fixed effects. The data is from the first day of job elicitation. Tables A.22 and A.23 report analogous results for days 2 and 3. Standard errors clustered at participant level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

7 Persistence

We next consider whether the increased willingness to visit an unfamiliar neighborhood induced by exposure persists. We analyze persistence using a cross-sectional analysis, and two medium-term follow-up exercises, one experimental, and the other based on survey data.

Cross-sectional persistence. We first examine the cross-sectional evidence by estimating our logit equation based on equation (1), but splitting the cross-sectional familiarity term by how recently the individual reported having visited the neighborhood. Table A.24 shows that the familiarity premium is relatively stable up to neighborhoods that the respondent visited within the last 3 years. While cross-sectional, this pattern suggests that the effects of exposure are likely to persist.

Show-Up to Medium-run Task Invitation. We next examine whether participants remain more likely to return to neighborhoods they visited during the experiment when invited back. Approximately 2-4 months after the intervention, we organized new work opportunities in different neighborhoods and invited study participants to attend. Each invitation was to a short (5-minute) survey on how long it takes to travel in Nairobi, scheduled to take place in a given neighborhood two days later. Because the survey focused on urban travel, inviting participants to different neighborhoods did not appear artificial.

Invitations were issued in two rounds, with participants receiving three invitations per round, spread over about 10 days. The survey neighborhood and the offered wages were randomized.[39] Surveyors recorded whether the respondent showed up on the designated survey day. After initially launching these invitations with wages randomized between 500 and 1,000 KSh we observed show-up rates in excess of 80%. To mitigate ceiling effects, we then reduced wage offers, eventually settling on a distribution between 100 and 400 KSh.

We estimate equation (1) using these show-up data in a binary logit model, including fixed effects for the survey date, randomization strata, and the number of prior invitations made. To deal with ceiling effects we take two approaches. First, we split the results by whether the wage was above or below 500 KSh using the entire sample. We do this because for wages higher than 500 KSh, show-up plateaus around 80% (Figure A.9) and none of the neighborhood or job characteristics, including distance and wage, affect show-up. Second, we present the results after restricting the sample to the second round of invitations where wages were randomized between 100 and 400 KSh.

Table 6 reports the results. In the first three columns, we replicate the logit specifications from Table 2, plus a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11), and additionally interact with wage below 500 KSh, as described above. Baseline familiarity strongly predicts show-up for the survey. The experimental effect of training in an unfamiliar neighborhood is positive and its magnitude is around two thirds of the baseline effect. However, this exposure effect is imprecisely estimated, with p-values between 0.27 and 0.29.[40] Distance and wages also matter for show-up for wages below 500

[39] Neighborhoods were randomly selected from the participants' nine target neighborhoods (three main familiar, three main unfamiliar, and three other familiar), and the invitation order of neighborhood type was stratified across participants, separately for the two rounds. Neighborhoods used in the first round were excluded from the second round.

[40] The reduced effective sample size due to the ceiling effects significantly limits statistical power. With our estimated standard errors, the minimum detectable effect is larger than the baseline familiarity premium, which ranges from 0.31 to 0.47. When we estimate a linear model with individual-level fixed effects to absorb some of the variation in show-up across participants, the exposure effect is more precisely estimated (Table A.25).

Table 6: Impact of Familiarity and Experimental Exposure on Medium-run Job Acceptance

		All Invites			Second Round Invites		
		(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	× (Wage ≤ 500)	0.39***	0.32*	0.31*	0.47**	0.44**	0.46**
		(0.12)	(0.13)	(0.13)	(0.14)	(0.17)	(0.16)
Training	× (Wage ≤ 500)	0.13	0.17	0.14	0.19	0.26	0.21
		(0.12)	(0.12)	(0.12)	(0.14)	(0.15)	(0.15)
Training × Unfamiliar	× (Wage ≤ 500)	0.24	0.24	0.25	0.27	0.17	0.26
		(0.22)	(0.22)	(0.22)	(0.26)	(0.28)	(0.27)
Distance (km)	× (Wage ≤ 500)	-0.15***		-0.16***	-0.14***		-0.13***
		(0.02)		(0.02)	(0.02)		(0.03)
Wage	× (Wage ≤ 500)	0.53***	0.54***	0.54***	0.57***	0.60***	0.59***
		(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
(Wage > 500)		1.7*	3.0***	1.8**			
		(0.69)	(0.61)	(0.70)			
Baseline Familiar	× (Wage > 500)	0.09	-0.25	-0.06			
		(0.25)	(0.27)	(0.26)			
Training	× (Wage > 500)	0.09	0.07	0.11			
		(0.25)	(0.27)	(0.26)			
Training × Unfamiliar	× (Wage > 500)	-0.07	0.02	-0.08			
		(0.41)	(0.44)	(0.43)			
Distance (km)	× (Wage > 500)	-0.01		-0.03			
		(0.03)		(0.04)			
Wage	× (Wage > 500)	0.06	0.07	0.07			
		(0.07)	(0.07)	(0.07)			
N		3,960	3,939	3,960	1,814	1,795	1,803
Training × Unfamiliar/Baseline Familiar		0.63	0.73	0.81	0.58	0.37	0.57
Survey Day FE		Yes	Yes	Yes	Yes	Yes	Yes
Invite Sequence FE		Yes	Yes	Yes	Yes	Yes	Yes
Randomization Strata FE		Yes	Yes	Yes	Yes	Yes	Yes
Home × Neighborhood FE			Yes			Yes	
Neighborhood FE				Yes			Yes

Notes: This table reports binary logit estimates using a version of equation (1) for the show-up outcome for the travel survey invitations 2-4 months after the intervention. The sample consists of the 3,960 invitations (of 4,482 made to our full sample of 799 participants) in which the participant answered the invitation call; call pickup does not differ by treatment (88.6% vs 88.1%). The invitations were made for six travel surveys split between two rounds. The outcome is an indicator for whether the participant showed up. We include survey date, invite sequence, and randomization strata fixed effects. We interact all coefficients with an indicator for wage larger than 500 KSh due to persistently high show-up rates above that level (see Figure A.9). Columns 4 - 6 report results from the second round of invitations where all wages were below 500 KSh. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

KSh, with smaller magnitude than those estimated in the main experiment (Table 2). This is consistent with a higher dispersion of idiosyncratic shocks for this extensive margin decision. We find similar results when we restrict to rounds after wages were reduced (columns 4-6), and when using a linear probability model (Table A.26).

Do participants return to visited neighborhoods? We examine whether individuals return to the neighborhoods on their own as reported through SMS and phone surveys.

Table A.27 reports the results. The outcomes in columns 1-9 are from directly asking respondents about whether they have visited the target neighborhoods in the past two weeks, and if so for what purpose. The SMS survey results reported in the last two columns are "unprompted," asking participants if they worked or searched for work the day before, and if so, where (in their own words). We then code their response to our neighborhood list.[41]

We find that respondents return to their training neighborhoods for both initially familiar and unfamiliar neighborhoods. The main effect is likely the result of increased salience or consideration shown in section 6. The effect on visiting unfamiliar neighborhoods is captured by the sum of the "Training" and "Training × Unfamiliar" coefficients, which is positive and significant for many visit types.

Participants return for a variety of reasons, including to search for work, as well as for non-work reasons such as leisure and errands. We do not find any effect on working in unfamiliar neighborhoods, although our estimates are noisy so we cannot reject a meaningful effect relative to the baseline mean.[42] However, participants did search for work in these neighborhoods (column 2). Direct labor market outcomes may take time to realize.

Overall, these results show that individuals continue to revisit the neighborhoods that they visited during training, including those that were unfamiliar at baseline.

8 Discussion

We have shown that prior exposure to city neighborhoods matters for how casual workers in Nairobi choose work opportunities, and a single experimentally induced exposure erases the initial aversion to working in unfamiliar neighborhoods. Our results, through the lens of our model, suggest that this aversion is a combination of a first-visit cost of exposure (or incorrect beliefs about a cost of exposure) anticipated by participants, and miscalibrated beliefs about unfamiliar neighborhoods, or a "fear-of-the-unknown." We find no evidence

[41] Table A.28 reports the results without neighborhood fixed effects.

[42] We cannot rule out effects of up to +0.019 on the probability of working in a specific unfamiliar neighborhood, relative to a mean of 0.069.

for sorting on neighborhood quality. We also find that exposure may additionally affect choice by increasing the probability a neighborhood enters the consideration set.

What are the implications of our results for how people take advantage of urban market access? Several pieces of evidence suggest that unfamiliarity may meaningfully limit access to urban opportunities. We show that participants have a strong aversion toward working in unfamiliar neighborhoods, even when there is no uncertainty about compensation. They also have more pessimistic beliefs about these neighborhoods, including about safety, and they are less likely to consider working in these neighborhoods to begin with. However, the premium we identify is large for a single day but is paid only once, and it might be inconsequential relative to a stream of future benefits from exploring a neighborhood. We argue this is unlikely. First, the unanticipated component of the exposure effect, due to beliefs, means that people treat unfamiliar neighborhoods as worse in perpetuity, and hence even a forward-looking agent may not explore efficiently. Second, previous work shows upfront costs deter access to valuable opportunities in a wide range of settings, even when these costs are small and anticipated to be one-time. One theoretical mechanism is that people may procrastinate (indefinitely) to explore new neighborhoods, thinking they will do it another day.

Spatial exposure effects may be particularly important for take-up of services and opportunities in cities in low-income countries, where people are often required to visit new locations, such as to access health services (Thornton 2008), to respond to advertisements for factory jobs (Grosset-Touba 2024), or to apply for social protection (Alatas et al. 2016). The fact that one exposure has large effects suggests that low-cost policy interventions to increase urban and spatial exposure may effectively increase market access for urban residents, both for employment and for other urban opportunities.

Additionally, policy makers may be able to decrease the cost of exploration by investing in making their cities more "readable" (Lynch 1960). This is a concept from urban studies that notes that the logic of some cities is easy to infer (e.g. the grid structure of New York City) while for others it is difficult (e.g. Boston). This may be particularly useful if the first-visit costs are driven by processing fluency, and rapid growth in low- and middle-income contexts may lead to cities that are particularly difficult to read.

Exposure effects may help explain prior results showing that people are reluctant to migrate, and the persistence of temporary interventions or shocks that encourage migration or other spatial choices (Bryan, Chowdhury, et al. 2014; Larcom et al. 2017; Okunogbe 2024; McKenzie 2024; Lagakos et al. 2023).

Urban and spatial quantitative models can partly accommodate exposure effects in commuting, migration and other spatial choices by assuming agents incur a fixed cost when they first visit a location. This might take a similar form to fixed costs of entering a distant market in trade models (Melitz 2003; Allen 2014).

Finally, we have shown that spatial decisions are a compelling context to study the role of prior exposure; however, exposure may also be important in other economic domains. For example, one might expect past exposure to opportunities to shape choices in occupation, schools, or technologies. Two questions for future work are how exposure effects vary across economic domains and across different types of exposure.

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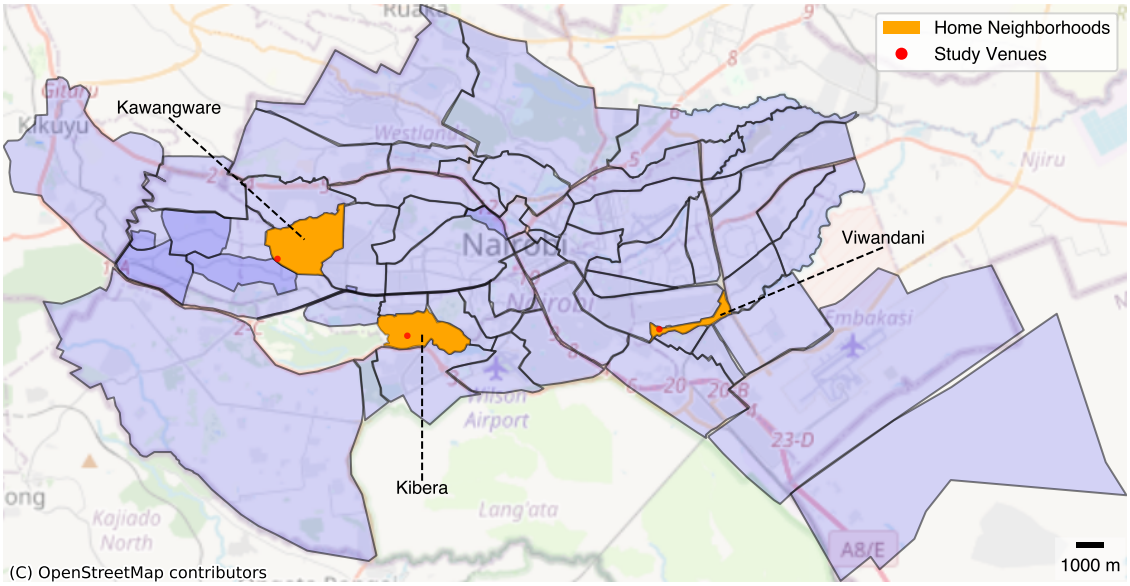
A Appendix - For Online Publication

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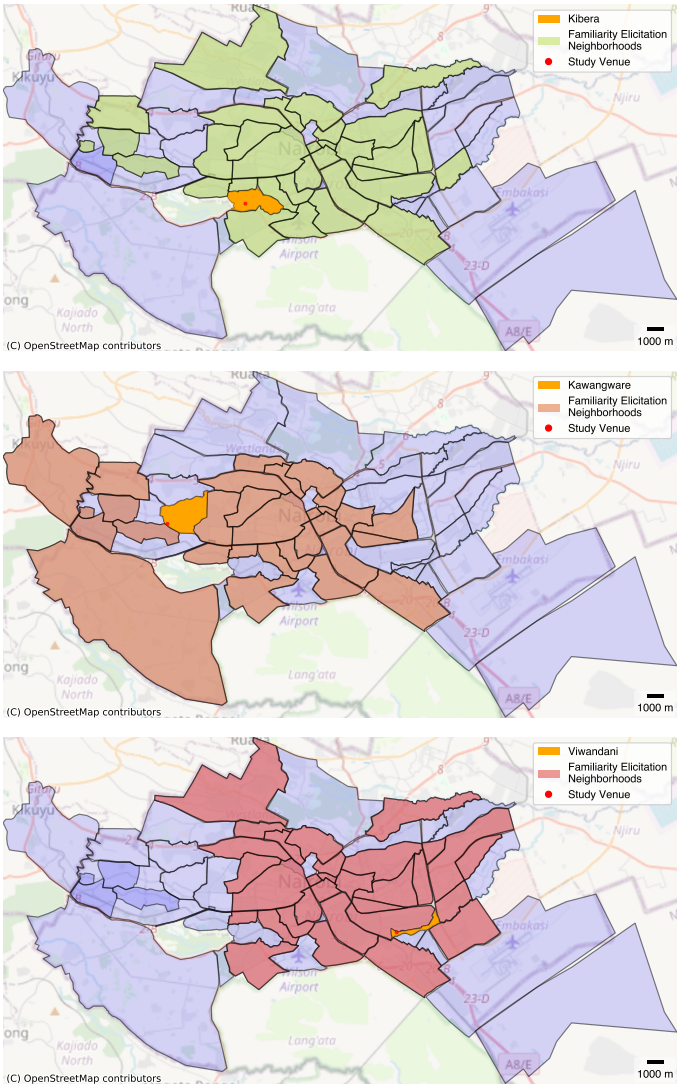
A.1 Appendix Figures

Figure A.1: Division of Nairobi Into Neighborhoods



Notes: This figure shows the partition of the main neighborhoods in Nairobi. The orange polygons represent the home neighborhoods—Kibera, Kawangware, and Viwandani—where participants were recruited. The locations of the study venues are highlighted in red. The remaining 58 neighborhoods are represented by light blue polygons.

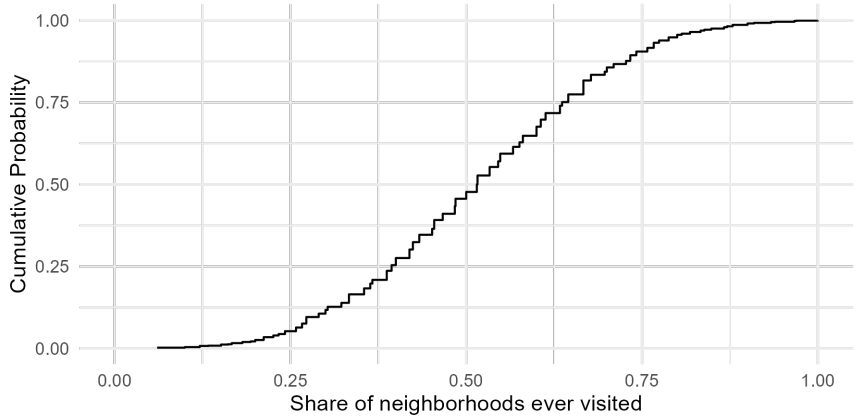
Figure A.2: Familiarity Elicitation Neighborhoods



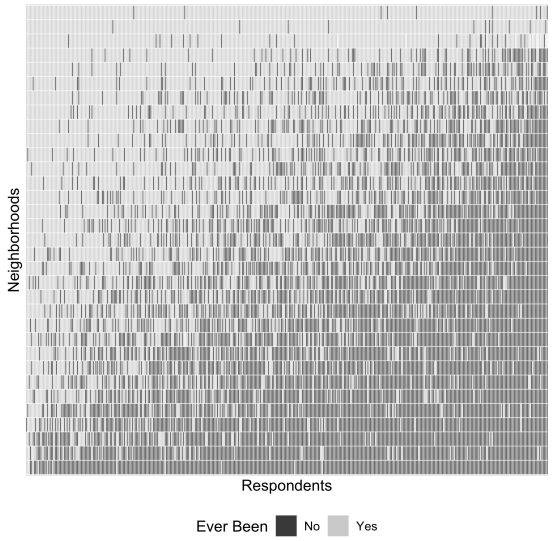
Notes: This figure replicates Figure A.1 for each home neighborhood and highlights the set of main neighborhoods for which we elicited familiarity. We elicited familiarity in 33 neighborhoods of Kibera, 30 in Kawangware, and 31 in Viwandani. For each home neighborhood, these neighborhoods were reachable in at most 75 minutes from the study venue. A small number of neighborhoods were dropped (see Appendix A.4 for a complete discussion of neighborhood selection).

Figure A.3: Urban Familiarity Patterns

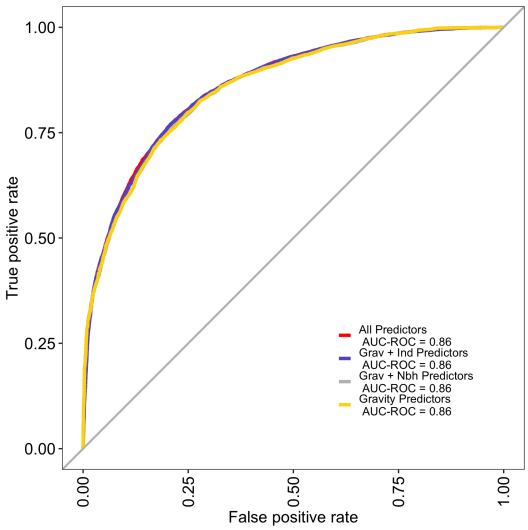
(a) CDF of Average Familiarity of Neighborhoods Within 75 Minutes from Home



(b) Idiosyncratic Variation in Familiarity (Kibera)



(c) No Evidence of Match-Specific Familiarity



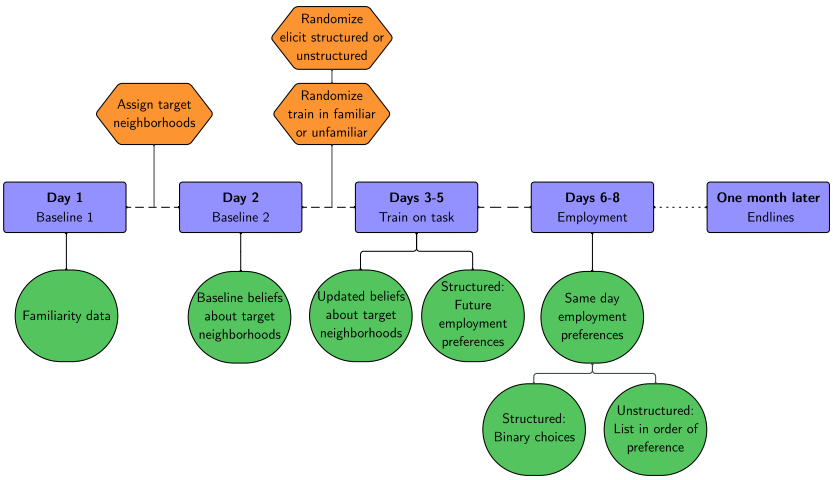
Notes: Panel (a) plots the CDF of our main familiarity measure across the "descriptive" sample, which includes all 1,168 participants who completed the first baseline survey. Neighborhoods are restricted to within 75 minutes (walking or transit) of respondents' homes, approximately 30 per home neighborhood. The X axis shows the share of neighborhoods for which a respondent answers yes to "Have you ever been to *N*?" Panel (b) displays the familiarity matrix for Kibera participants who completed the first baseline survey, across all 30 neighborhoods asked about in all participant batches; grey indicates the respondent has visited that neighborhood, black otherwise. Rows and columns are ordered by average familiarity. Panel (c) shows ROC curves for four random forest models predicting individual-neighborhood familiarity. Distance and neighborhood fixed effects alone (yellow) predict better than chance; adding individual and neighborhood characteristics (gray, blue, red) does not meaningfully improve prediction.

Figure A.4: Backpacks Used in Employment Task



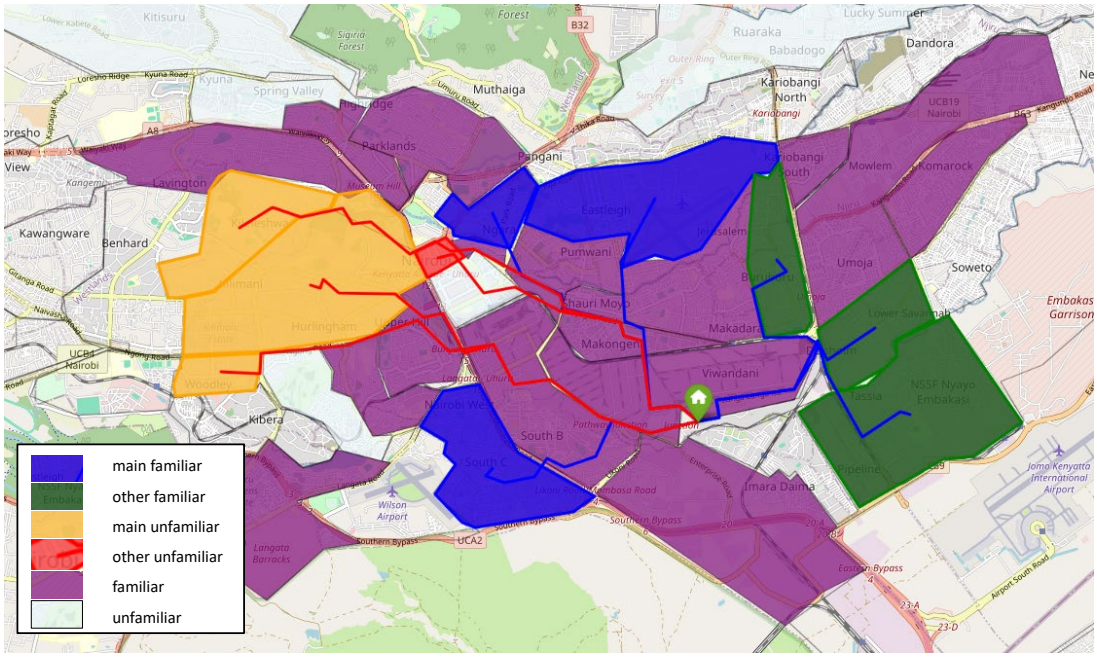
Notes: This figure shows a picture of an air pollution backpack used by study participants during training and employment days. The backpack contains an air pollution sensor (which has no display so does not reveal the air pollution level) and a smartphone with an app used to check-in and check-out from the job.

Figure A.5: Timeline of Study



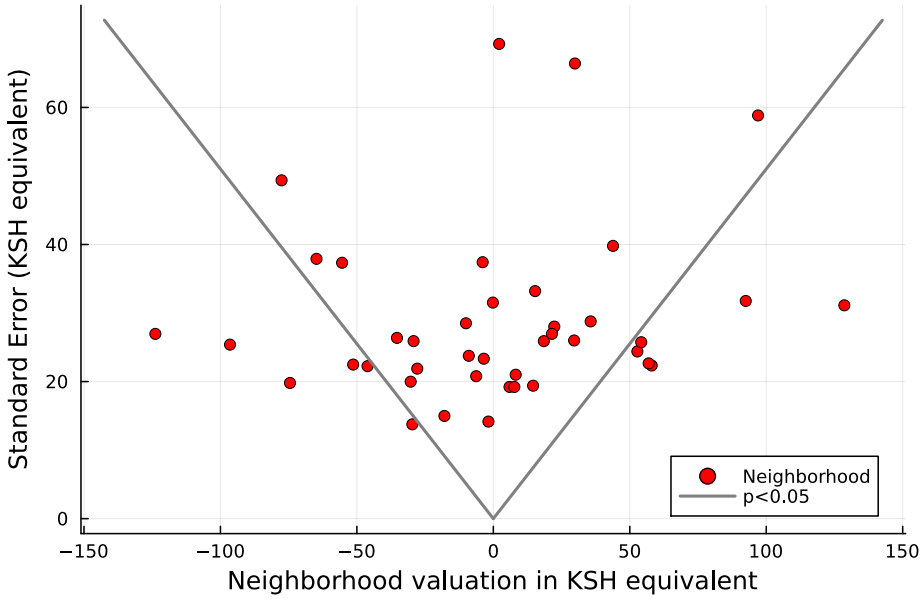
Notes: This figure shows the timing outline of the study. In the first two days, participants answered surveys about their neighborhood familiarity, demographics, beliefs about target neighborhoods, employment and job search status, and spatial ability. Over the next three days, participants were trained on the air pollution data collection task in either familiar or unfamiliar neighborhoods, and responded to a survey when they returned. On the last three days, participants worked in a neighborhood based on their choices and the elicitation method, either the "open" elicitation or the "preferences" binary choices. Each day after employment days they responded to a survey about trip duration, directions, and familiarity checks. After a month, participants were contacted by phone and SMS for an endline survey.

Figure A.6: Example of 10 Target Neighborhoods for One Participant



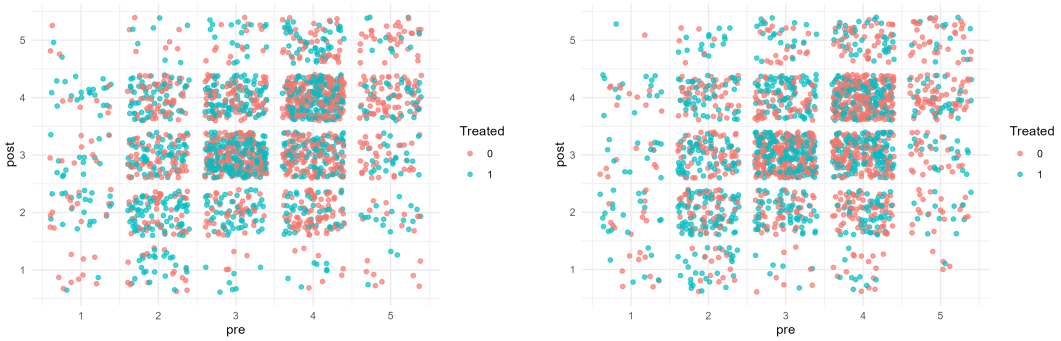
Notes: This map plots the ten target neighborhoods for one participant from the home neighborhood of Viwandani. Non-target neighborhoods are shaded in purple if the participant had ever visited them at baseline or white if they had not. The four target categories are shaded in different colors: blue for the three main familiar neighborhoods, green for the three other familiar neighborhoods, yellow for the three main unfamiliar neighborhoods, and red for the single other unfamiliar neighborhood. The walk or transit route to each neighborhood is also plotted.

Figure A.7: Distribution of Neighborhood Fixed Effects



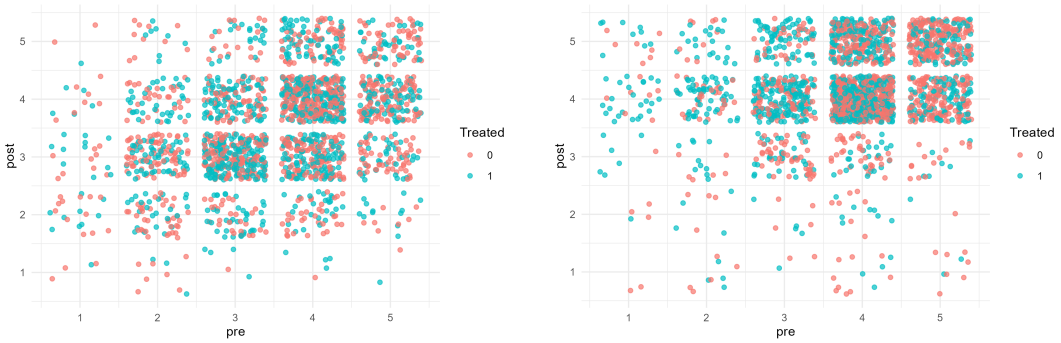
Notes: Neighborhood fixed effects from the specification of column 5 in Table A.11, re-estimated on the choices in which neither offer includes a risky bonus (4,871 of 6,756 employment-day choices). The vertical axis shows their bootstrapped standard errors, and the grey lines mark $p = 0.05$. The fixed effects capture the average attractiveness of each neighborhood in the job choices.

Figure A.8: Beliefs Transition Matrices



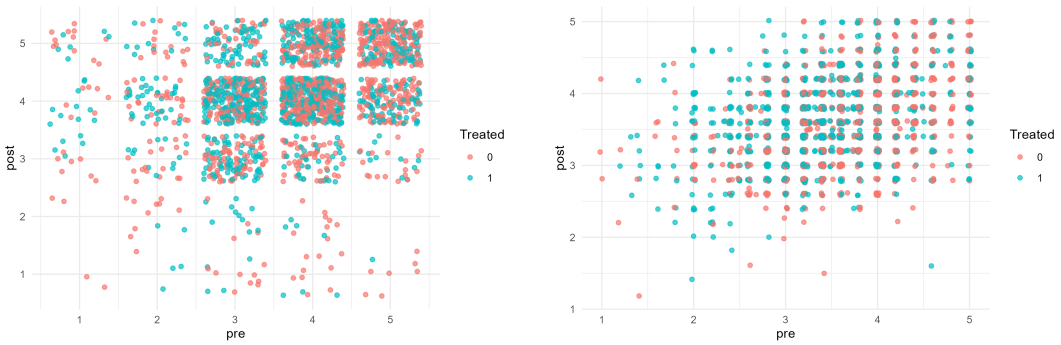
(A) Find Job

(B) Find Job - Others



(C) Pay

(D) Travel Safety

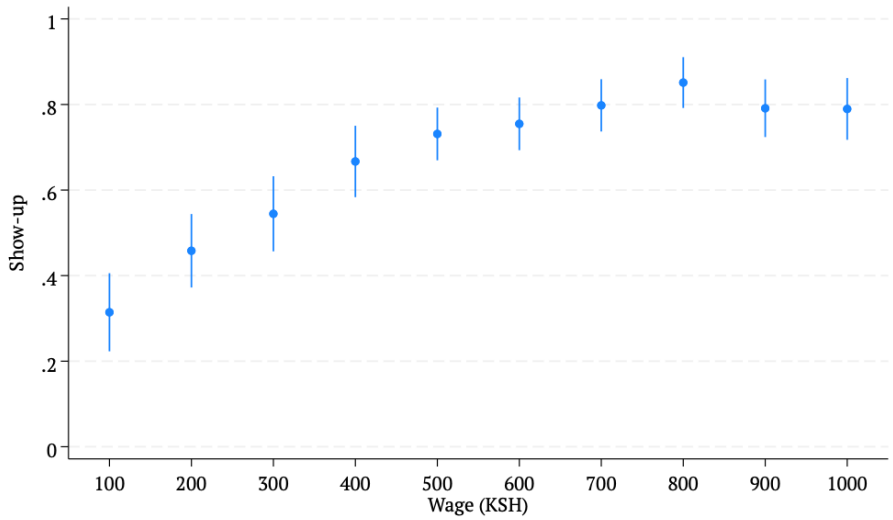


(E) Overall Safety

(F) Average (All Measures)

Notes: Each graph plots beliefs pre- and post-intervention on the X- and Y-axis, respectively. Higher values correspond to better outcomes. See Section A.6 for the precise questions underlying each measure.

Figure A.9: Show-up for Job Invitation 2-4 Months After Intervention: High Wage Plateau



Notes: This figure plots average show-up for the travel survey task by (random) wage offer. These job offers were made by phone 2-4 months after the intervention. Participants were invited for a short (5-minute) survey on how long it takes to travel in Nairobi.

A.2 Appendix Tables

Table A.1: Sample Characteristics

Variable	N	Mean	SD	Median
Female	830	0.74	0.44	1
Age	830	29.49	7.70	28
Education Years	829	10.65	2.82	12
Enrolled in School	830	0.04	0.20	0
Workdays Last 2 Weeks	830	3.09	2.87	3
Days Searched for Work Last 2 Weeks	830	6.55	3.39	6
Married	830	0.46	0.50	0
Years in Nairobi	830	15.22	9.29	13
Resided Outside Nairobi	830	0.67	0.47	1
Max Walk to Work/Search (km)	583	7.97	3.78	8
Max Transit to Work/Search (km)	578	11.54	4.92	12
Max Walk to Work/Search (min)	583	108.84	50.63	107
Max Transit to Work/Search (min)	578	56.65	14.22	57
Max Fastest Travel to Work/Search (min)	583	57.70	21.19	57

Notes: This table reports basic statistics on participant characteristics for the full sample of participants who completed the second baseline survey. The last five outcomes are restricted to participants who reported working or searching for work outside their home neighborhood in the past two weeks. We compute the maximum value of the outcome among all the neighborhoods where they report working or searching for work.

Table A.2: Top 10 Participant Jobs Over the Last Two Weeks

Women		Men	
Occupation	Share	Occupation	Share
Laundry	62%	Carpenter/Mason	33%
Cleaner	15%	Industrial/factory worker	9.8%
Househelp	11%	Electrician	5.7%
Washing dishes/utensils	8.6%	Cleaner	5.2%
Cook	7%	Mechanic	4.6%
Salon	6.6%	Small Business	4.6%
Waiter	4%	Plumber	4%
Small Business	3.5%	Carrying luggage	4%
Shopkeeper	2.6%	Boda boda operator	3.4%
Industrial/factory worker	2.6%	Cook	3.4%

Notes: This table lists the top 10 most frequent jobs performed by participants in the last two weeks, categorized by gender. The sample consists of 629 participants—455 women and 174 men—who worked at least one day during the two weeks prior to the intervention days. The percentage frequencies are derived from a multiple-choice format question, so each row represents the share of the total gender-specific sample who listed that type of occupation.

Table A.3: Casual Workers Use Spatial Search Strategies

	Job Search Strategies			
	In Last Two Weeks:		**Ever Found Work:**	
	Women	Men	Women	Men
Travel to other nbhd	0.64	0.60		
Door to door	0.36	0.20	0.51	0.29
Hiring spot	0.38	0.35	0.43	0.38
Drop CV	0.16	0.19	0.16	0.24
Ask people I know/employer	0.88	0.91		
In person referral			0.77	0.90
Receive call (referral)			0.72	0.73
Online	0.15	0.27	0.04	0.11
Observations	617	213	570	199

Notes: This table shows the share of participants who reported using specific spatial job search strategies, categorized by gender. The first two columns include participants who searched for work in the last two weeks prior to the first training day. The next two columns include participants who were contacted through phone calls at the endline. The shares should be interpreted relative to the sample size stated at the bottom of each column.

Table A.4: Correlates of Individual-level Average Familiarity

	Average "Ever Been"	
	(1)	(2)
Female	-0.052***	-0.057***
	(0.011)	(0.011)
Age	0.002**	0.001*
	(0.001)	(0.001)
Years of education	0.010***	0.009***
	(0.002)	(0.002)
Years in Nairobi	0.002***	0.003***
	(0.000)	(0.000)
Spatial ability index	0.015**	0.018***
	(0.005)	(0.005)
N	829	829
Outcome SD	0.171	0.171
Surveyor FEs	No	Yes
Adjusted R^2	0.124	0.203

Notes: This table reports the correlation between average familiarity at the participant level and participant characteristics. The sample includes participants who completed Baseline 2. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.5: Ethnicity is Not A Large Factor in Determining Familiarity

	Participant i "Ever Been" to neighborhood j				
	(1)	(2)	(3)	(4)	(5)
Distance (km)	-0.051***		-0.051***	-0.060***	
	(0.001)		(0.001)	(0.001)	
Same Ethnicity		0.057***	0.003	0.029*	0.019
		(0.016)	(0.012)	(0.012)	(0.012)
Individual FEs	Yes	Yes	Yes	Yes	Yes
Neighborhood FEs				Yes	
Home x Neighborhood FEs					Yes
Observations	16,799	16,799	16,799	16,799	16,799
Adjusted R^2	0.187	0.054	0.187	0.386	0.431

Notes: This table reports the correlation between participant familiarity and the main ethnicity of the neighborhood. The sample consists of participants who completed both Baseline 2 and the phone surveys at the endline. Participants were asked about their ethnic identity during the endline survey. A belief question about the largest ethnic group in the neighborhood was added in baseline 2 for later batches. About 50% of baseline 2 participants were asked this ethnic belief question. A neighborhood's dominant ethnicity is defined as the most common ethnic group reported by respondents familiar with that neighborhood. *Same Ethnicity* is an indicator for whether a respondent's ethnicity matches the neighborhood's dominant ethnicity. *Distance (km)* represents the walking distance in kilometers, from the study venues to the specific neighborhood centroid, as estimated from the Google Maps API. Standard errors in parentheses are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.6: Balance on Main Study Sample

	Obs	Treatment Mean	Control Mean	P-value
Female	799	0.72	0.77	0.23
Age	799	29.11	29.74	0.55
Education Years	798	10.63	10.71	0.67
Enrolled in School	799	0.05	0.03	0.40
Workdays Last 2 Weeks	799	3.02	3.14	0.50
Days Searched for Work Last 2 Weeks	799	6.51	6.61	0.64
Married	799	0.46	0.47	0.91
Years in Nairobi	799	15.24	15.14	0.52
Resided Outside Nairobi	799	0.65	0.69	0.28
Joint P-value				0.80

Notes: This table reports the mean test differences between participants assigned to treatment and control groups. The sample includes only those participants who attended the first training day. The last column reports the p-value for the test of treatment in a regression including randomization block fixed effects, and the joint p-value is a test of the joint equality of all listed treatment and control mean characteristics.

Table A.7: Balance on "Preferences" Elicitation Sample

	Obs	Treatment Mean	Control Mean	P-value
Female	400	0.72	0.75	0.31
Age	400	28.54	30.31	0.08
Education Years	399	10.69	10.79	0.54
Enrolled in School	400	0.05	0.00	0.04
Workdays Last 2 Weeks	400	2.96	3.05	0.69
Days Searched for Work Last 2 Weeks	400	6.32	6.87	0.08
Married	400	0.39	0.48	0.30
Years in Nairobi	400	14.88	16.08	0.27
Resided Outside Nairobi	400	0.67	0.66	0.71
Joint P-value				0.11

Notes: This table replicates Table A.6, restricted to participants in the "preferences" elicitation group.

Table A.8: Comparing Target Unfamiliar Neighborhoods to All Other Unfamiliar Neighborhoods

	Main Unfamiliar (1)	All Other Unfamiliar (2)	Difference (3)
Belief: Find Job	3.306	3.254	0.052
			(0.081)
Belief: Find Job - Others	3.315	3.242	0.072
			(0.093)
Belief: Pay	3.600	3.560	0.041
			(0.072)
Belief: Travel Safety	3.968	3.922	0.046
			(0.042)
Belief: Overall Safety	3.824	3.748	0.076
			(0.058)
Walking Dist. (Meters)	9,280	10,142	-862*
			(332)
Walking Duration (Seconds)	7,685	8,343	-658*
			(266)
Transit Dist. (Meters)	12,158	13,188	-1030*
			(427)
Transit Duration (Seconds)	3,423	3,509	-85
			(77)
Great Circle Dist. (Meters)	7,354	8,174	-819**
			(297)
Nbhd Logit FE	-0.037	-0.039	0.002
			(0.052)

Notes: This table compares target unfamiliar neighborhoods to all other unfamiliar neighborhoods. Estimates are weighted by the number of respondents for whom the neighborhood is a target neighborhood or unfamiliar neighborhood. Beliefs variables are average beliefs at baseline for respondents that are familiar with the neighborhood. Distances and durations are measures using Google Maps. "Nbhd Logit FE" are the neighborhood fixed effects from the specification in column 5, Table A.11. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.9: Familiarity Check After Job Training

	(1) Ever Been
Treated	-0.54***
	(0.02)
Constant	0.88***
	(0.01)
Observations	2385

Notes: This table checks the familiarity reported by participants after job training and their treatment assignment. The sample is an unbalanced panel of 799 participants over the three training days. The dependent variable takes a value of one if, after returning to the study venue in the home neighborhood from a training day, the participant had previously visited the assigned neighborhood; it is assigned a value of zero otherwise. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.10: Show-up at Different Stages of the Study

	Training		Employment		Endline
Elicitation Sample:	Preference	Open	Preference	Open	
	(1)	(2)	(3)	(4)	(5)
Treated	-0.0093	-0.0083	-0.0203	-0.0199	-0.0013
	(0.0070)	(0.0095)	(0.0132)	(0.0154)	(0.0137)
Control Mean	0.9980	0.9950	0.9870	0.9800	0.9630
N	800	798	1,200	1,197	799

Notes: This table reports show-up at different stages of the study timeline for our two elicitation samples. Our study sample is the 799 participants who attended the first day of training. 400 participants were randomized to the preferences elicitation, and 399 were randomized to the open elicitation. Columns (1) and (2) present results on attrition for training days 2 and 3 for the preference elicitation group and open elicitation group respectively. Columns (3) and (4) present analogous results for the three employment days. Column (5) shows results for the endline survey administered via phone calls. All regressions include fixed effects for participant batch assignments in the study. Standard errors in parentheses are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.11: Revealed Preference Estimates from Job Choice – Full Mixed Logit Results

	(1)	(2)	(3)	(4)	(5)
β^F Baseline Familiar	1.09***	0.89***	0.85***	0.76***	0.86***
	(0.14)	(0.14)	(0.11)	(0.12)	(0.12)
β^T Training	0.31*	0.35**	0.29**	0.33**	0.30**
	(0.13)	(0.12)	(0.10)	(0.11)	(0.11)
β^U Training × Unfamiliar	1.13***	1.13***	0.83***	0.93***	0.91***
	(0.26)	(0.23)	(0.21)	(0.20)	(0.21)
Distance (km)	-0.29***		-0.24***		-0.25***
	(0.02)		(0.02)		(0.02)
Job duration (hrs)	-0.73***	-0.74***	-0.58***	-0.62***	-0.59***
	(0.08)	(0.07)	(0.06)	(0.06)	(0.06)
γ^E $\mathbb{E}$ Compensation (100 KSH)	0.95***	0.97***	0.76***	0.83***	0.80***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Cash Upfront (100 KSH)	0.05	0.06	0.06	0.06	0.07
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Bonus (100 KSH)	-0.26***	-0.26***	-0.21***	-0.22***	-0.22***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Familiarity Premium β^F/γ^E (100 KSH)	1.15	0.92	1.11	0.92	1.07
	[0.85, 1.43]	[0.63, 1.19]	[0.82, 1.37]	[0.62, 1.18]	[0.79, 1.34]
Exposure Effect β^U/γ^E (100 KSH)	1.19	1.16	1.09	1.13	1.13
	[0.62, 1.71]	[0.65, 1.60]	[0.56, 1.58]	[0.63, 1.57]	[0.60, 1.61]
Ratio β^U/β^F	1.04	1.26	0.98	1.23	1.05
	[0.67, 1.33]	[0.88, 1.63]	[0.59, 1.27]	[0.83, 1.58]	[0.69, 1.37]
Variance of coefficients					
Var(β^F)	1.90	1.28			
Var(β^T)	0.74	0.14			
Var($\beta^F - \beta^U - \beta^T$), treated	1.97	1.39			
Model	Mixed logit	Mixed logit	Logit	Logit	Logit
Home × neighborhood FE		Yes		Yes	
Neighborhood FE					Yes
Choices	6,756	6,747	6,756	6,747	6,756
People	395	395	395	395	395

Notes: This table repeats Table 2 and adds the estimated variances of the random coefficients in the mixed logit (columns 1–2), plus a logit with distance and neighborhood fixed effects (column 5). In columns 1-2, each variance is the sum of a person component and a pair (person–neighborhood) component. Control participants choose among untrained familiar, trained familiar, and untrained unfamiliar neighborhoods, which identifies the variances of β^F and β^T. On employment days, treatment participants choose only between untrained familiar and trained unfamiliar neighborhoods, which identifies the variance of $\beta^F - \beta^U - \beta^T$ but not that of β^U (Appendix A.3.4). The table omits covariances. We treat the variances as nuisance parameters and report no standard errors for them, because some variance components are estimated at or near zero, where standard errors are unreliable.

Table A.12: Revealed Preference Estimates of Familiarity Premium - OLS

	(1)	(2)	(3)	(4)
Baseline Familiar	0.099***	0.097***	0.081***	0.096***
	(0.014)	(0.014)	(0.014)	(0.014)
Training	0.039**	0.039**	0.042**	0.040**
	(0.014)	(0.014)	(0.013)	(0.013)
Training × Unfamiliar	0.093***	0.096***	0.098***	0.099***
	(0.026)	(0.027)	(0.025)	(0.025)
Distance (km)	-0.029***	-0.029***		-0.030***
	(0.002)	(0.002)		(0.002)
Job duration (hrs)	-0.069***	-0.071***	-0.072***	-0.071***
	(0.007)	(0.007)	(0.007)	(0.007)
$\mathbb{E}$ Compensation (100 KSH)	0.102***	0.101***	0.102***	0.101***
	(0.002)	(0.002)	(0.002)	(0.002)
Cash Upfront (100 KSH)	0.007	0.007	0.008	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Bonus (100 KSH)	-0.028***	-0.028***	-0.028***	-0.028***
	(0.001)	(0.001)	(0.001)	(0.001)
Person FE		Yes	Yes	Yes
Home × neighborhood FE			Yes	
Neighborhood FE				Yes
N	6,756	6,756	6,756	6,756
R^2	0.470	0.504	0.527	0.518
Within-R^2		0.470	0.495	0.486

Notes: This table replicates results from Table 2 and Table A.11 but uses a linear probability model. The outcome is an indicator for whether the respondent chose job $j = 1$. For each attribute, we include the difference between the value of the attribute for job $j = 1$ and $j = 2$ as independent variables. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.13: The Impact of Realized and Anticipated Familiarity on Job Choices – Full Mixed Logit Results

	(1)	(2)	(3)	(4)	(5)
β^F Baseline Familiar	0.97***	0.84***	0.78***	0.72***	0.72***
	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)
β^T Training	0.18	0.21*	0.18*	0.21**	0.03
	(0.09)	(0.09)	(0.08)	(0.08)	(0.12)
β^U Training × Unfamiliar	1.00***	0.98***	0.77***	0.81***	0.82***
	(0.16)	(0.16)	(0.13)	(0.13)	(0.19)
β^{AT} Anticipate Training	-0.08	-0.06	-0.06	-0.04	-0.07
	(0.12)	(0.12)	(0.09)	(0.10)	(0.09)
β^{AU} Anticipate Training × Unfamiliar	0.64***	0.59**	0.48**	0.48**	0.45**
	(0.18)	(0.18)	(0.15)	(0.15)	(0.14)
β^{AU}/β^U	0.64	0.60	0.63	0.59	0.54
	[0.33, 0.97]	[0.25, 0.95]	[0.26, 0.98]	[0.23, 0.94]	[0.20, 0.98]
Familiarity Premium β^F/γ^E (100 KSH)	1.21	1.04	1.19	1.04	1.16
	[1.01, 1.39]	[0.85, 1.23]	[0.99, 1.35]	[0.85, 1.22]	[0.92, 1.38]
Exposure Effect β^U/γ^E (100 KSH)	1.25	1.22	1.16	1.17	1.33
	[0.82, 1.63]	[0.81, 1.54]	[0.76, 1.55]	[0.76, 1.48]	[0.76, 1.88]
Variance of coefficients					
Var(β^F)	1.69	1.23			
Var(β^T)	0.53	0.36			
Var(β^{AT})	1.05	0.87			
Var($\beta^U + \beta^T$)	1.37	1.14			
Var($\beta^{AU} + \beta^{AT}$)	1.29	1.02			
Model	Mixed logit	Mixed logit	Logit	Logit	Logit
Home × neighborhood FE		Yes		Yes	Yes
Training days only					Yes
Choices	13,658	13,643	13,658	13,643	6,894
People	400	400	400	400	400

Notes: This table repeats Table 4 and adds the estimated variances of the random coefficients in the mixed logit (columns 1–2), and in column 5 reports a logit specification that re-estimates column 4 only on training days. In columns 1-2, variances are defined as in Table A.11. Control participants' choices on training days also identify the variance of β^{AT}. Treatment participants' choices identify the variances of $\beta^U + \beta^T$ and $\beta^{AU} + \beta^{AT}$, but not those of β^U and β^{AU} (Appendix A.3.4). As in Table A.11, the table omits covariances and reports no standard errors for the variances.

Table A.14: Preferences for Familiar Neighborhoods By Baseline Perceptions of **Neighborhood Safety**

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Unfamiliar	-0.85***		-0.76***		-0.86***	
	(0.120)		(0.113)		(0.119)	
Baseline Unfamiliar × High Safety Belief		-0.78***		-0.65***		-0.75***
		(0.158)		(0.175)		(0.170)
Baseline Unfamiliar × Not High Safety Belief		-0.88***		-0.80***		-0.91***
		(0.142)		(0.130)		(0.136)
Training	0.29*	0.29*	0.33**	0.33**	0.30**	0.30**
	(0.113)	(0.115)	(0.100)	(0.105)	(0.109)	(0.107)
Training × Unfamiliar	0.83***		0.93***		0.91***	
	(0.224)		(0.195)		(0.208)	
Training × Unfamiliar × High Safety Belief		0.74**		0.83***		0.81**
		(0.264)		(0.253)		(0.263)
Training × Unfamiliar × Not High Safety Belief		0.87***		0.97***		0.95***
		(0.244)		(0.217)		(0.225)
Distance (km)	-0.24***	-0.24***			-0.25***	-0.25***
	(0.016)	(0.015)			(0.018)	(0.019)
Job duration (hrs)	-0.58***	-0.58***	-0.62***	-0.62***	-0.59***	-0.59***
	(0.060)	(0.060)	(0.058)	(0.061)	(0.058)	(0.061)
$\mathbb{E}$ Compensation (100 KSh)	0.76***	0.76***	0.83***	0.83***	0.80***	0.80***
	(0.027)	(0.026)	(0.027)	(0.029)	(0.027)	(0.027)
Cash Upfront (100 KSh)	0.06	0.06	0.06	0.06	0.07	0.06
	(0.044)	(0.045)	(0.046)	(0.045)	(0.043)	(0.045)
Bonus (100 KSh)	-0.21***	-0.21***	-0.22***	-0.22***	-0.22***	-0.22***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)
Home × neighborhood FE			Yes	Yes		
Neighborhood FE					Yes	Yes
N	6,756	6,756	6,756	6,756	6,756	6,756
p-val (boot-t) High Safety = Not High Safety						
× Unfamiliar		0.590		0.420		0.338
× Training × Unfamiliar		0.600		0.534		0.592

Notes: Columns 1 and 3 replicate logit results from columns 3 and 4 in Table 2 using $Unfamiliar_{in} = 1 - Fam_{in}$ instead of Fam_{in}, and column 5 replicates a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11). Even columns interact the unfamiliar coefficients ($Unfamiliar_{in}$ and $Training \times Unfamiliar$) with an indicator equal to 1 when the unfamiliar neighborhood was rated as "Safe" or "Very Safe" at baseline, and the respondent indicated they are "Completely Confident" about this rating. (35% of unfamiliar neighborhoods were rated this way.) The neighborhood safety question has options "Very Unsafe," "Unsafe," "Neutral," "Safe," and "Very Safe," and the confidence question had options "Not confident at all," "Slightly confident," "Somewhat confident," "Fairly confident," and "Completely confident." $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.15: Preferences for Familiar Neighborhoods By Baseline Perceptions of **Travel Safety**

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Unfamiliar	-0.85***		-0.76***		-0.86***	
	(0.120)		(0.113)		(0.119)	
Baseline Unfamiliar × High Safety Belief		-0.72***		-0.64***		-0.72***
		(0.165)		(0.164)		(0.166)
Baseline Unfamiliar × Not High Safety Belief		-0.91***		-0.81***		-0.92***
		(0.138)		(0.134)		(0.141)
Training	0.29*	0.29*	0.33**	0.33**	0.30**	0.30**
	(0.113)	(0.116)	(0.100)	(0.105)	(0.109)	(0.107)
Training × Unfamiliar	0.83***		0.93***		0.91***	
	(0.224)		(0.195)		(0.208)	
Training × Unfamiliar × High Safety Belief		0.65*		0.75**		0.71**
		(0.269)		(0.249)		(0.253)
Training × Unfamiliar × Not High Safety Belief		0.91***		1.01***		0.99***
		(0.242)		(0.219)		(0.229)
Distance (km)	-0.24***	-0.24***			-0.25***	-0.25***
	(0.016)	(0.015)			(0.018)	(0.019)
Job duration (hrs)	-0.58***	-0.58***	-0.62***	-0.62***	-0.59***	-0.59***
	(0.060)	(0.060)	(0.058)	(0.061)	(0.058)	(0.061)
$\mathbb{E}$ Compensation (100 KSh)	0.76***	0.76***	0.83***	0.83***	0.80***	0.80***
	(0.027)	(0.026)	(0.027)	(0.029)	(0.027)	(0.027)
Cash Upfront (100 KSh)	0.06	0.06	0.06	0.06	0.07	0.07
	(0.044)	(0.045)	(0.046)	(0.045)	(0.043)	(0.045)
Bonus (100 KSh)	-0.21***	-0.21***	-0.22***	-0.22***	-0.22***	-0.22***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)
Home × neighborhood FE			Yes	Yes		
Neighborhood FE					Yes	Yes
N	6,756	6,756	6,756	6,756	6,756	6,756
p-val (boot-t) High Safety = Not High Safety						
× Unfamiliar		0.260		0.352		0.260
× Training × Unfamiliar		0.274		0.236		0.206

Notes: The same analysis as in Table A.14 splitting unfamiliar neighborhoods by baseline reported safety of *traveling* to the neighborhood. High Safety Belief is an indicator for neighborhoods the respondent rated safe or very safe travel, and indicated they were completely confident about the rating. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.16: Preferences for Familiar Neighborhoods by Baseline Familiarity

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	0.76***	0.74***	0.76***	1.00***	1.00***	1.12***
	(0.150)	(0.162)	(0.155)	(0.176)	(0.199)	(0.194)
Training	0.42**	0.42**	0.44**	0.08	0.14	0.13
	(0.142)	(0.140)	(0.141)	(0.178)	(0.173)	(0.180)
Training × Unfamiliar	0.78**	0.92***	0.78**	0.96**	1.34***	1.20***
	(0.274)	(0.267)	(0.280)	(0.323)	(0.330)	(0.342)
Distance (km)	-0.22***		-0.23***	-0.25***		-0.27***
	(0.019)		(0.026)	(0.027)		(0.031)
Job duration (hrs)	-0.52***	-0.56***	-0.53***	-0.69***	-0.79***	-0.73***
	(0.071)	(0.076)	(0.074)	(0.100)	(0.101)	(0.096)
$\mathbb{E}$ Compensation (100 KSh)	0.73***	0.80***	0.77***	0.82***	0.92***	0.88***
	(0.030)	(0.033)	(0.029)	(0.047)	(0.052)	(0.053)
Cash Upfront (100 KSh)	0.06	0.06	0.06	0.04	0.05	0.07
	(0.056)	(0.060)	(0.055)	(0.069)	(0.081)	(0.074)
Bonus (100 KSh)	-0.20***	-0.22***	-0.21***	-0.23***	-0.24***	-0.24***
	(0.012)	(0.013)	(0.013)	(0.017)	(0.018)	(0.018)
Participant familiarity sample:	Low	Low	Low	High	High	High
Home × neighborhood FE		Yes			Yes	
Neighborhood FE			Yes			Yes
N	3,918	3,918	3,918	2,838	2,838	2,838

Notes: This table replicates the logit results from Table 2, plus a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11), separately for participants with below-median average familiarity level at baseline (first three columns) and above-median average familiarity level (last three columns). $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.17: Preferences for Familiar Neighborhoods By Distance

	(1)	(2)	(3)
Baseline Familiar × Close	0.84***	0.89***	0.86***
	(0.147)	(0.173)	(0.151)
Baseline Familiar × Far	0.90***	0.65***	0.85***
	(0.142)	(0.158)	(0.144)
Training × Close	0.25	0.30*	0.29*
	(0.133)	(0.134)	(0.132)
Training × Far	0.28	0.35*	0.30
	(0.166)	(0.147)	(0.156)
Training × Unfamiliar × Close	0.96***	1.02***	0.93***
	(0.257)	(0.265)	(0.260)
Training × Unfamiliar × Far	0.83**	0.86***	0.91***
	(0.274)	(0.251)	(0.243)
N	6,756	6,756	6,756
Home × neighborhood FE		Yes	
Neighborhood FE			Yes

Notes: This table replicates the logit results from Table 2, plus a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11), interacting all coefficients with an indicator for proximity, defined as travel distance below or above the median (roughly 1 hour away). The other independent variables from Table 2 are included but not reported; all except distance and the fixed effects are interacted with the proximity indicator. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.18: Preferences for Familiar Neighborhoods By Gender

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	0.74***	0.63***	0.74***	1.24***	1.22***	1.42***
	(0.138)	(0.143)	(0.146)	(0.199)	(0.286)	(0.234)
Training	0.40***	0.43***	0.38**	-0.03	0.15	0.10
	(0.105)	(0.098)	(0.129)	(0.181)	(0.212)	(0.200)
Training × Unfamiliar	0.62**	0.72***	0.72**	1.51***	1.56***	1.54***
	(0.241)	(0.216)	(0.249)	(0.357)	(0.466)	(0.415)
Distance (km)	-0.24***		-0.26***	-0.24***		-0.25***
	(0.018)		(0.023)	(0.027)		(0.032)
Job duration (hrs)	-0.50***	-0.55***	-0.51***	-0.82***	-0.89***	-0.84***
	(0.066)	(0.070)	(0.066)	(0.140)	(0.151)	(0.143)
$\mathbb{E}$ Compensation (100 KSh)	0.74***	0.81***	0.78***	0.85***	0.98***	0.93***
	(0.025)	(0.032)	(0.031)	(0.053)	(0.072)	(0.057)
Cash Upfront (100 KSh)	0.07	0.06	0.07	0.01	0.04	0.02
	(0.048)	(0.052)	(0.050)	(0.083)	(0.094)	(0.097)
Bonus (100 KSh)	-0.21***	-0.22***	-0.22***	-0.22***	-0.25***	-0.24***
	(0.011)	(0.012)	(0.012)	(0.021)	(0.027)	(0.025)
Sample	Women	Women	Women	Men	Men	Men
Home × neighborhood FE		Yes			Yes	
Neighborhood FE			Yes			Yes
N	4,952	4,952	4,952	1,804	1,804	1,804

Notes: This table replicates the logit specifications in Table 2, plus a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11), for the women (columns 1-3) and men (columns 4-6) samples. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.19: Familiarity Premium Concentrated For Strong Familiarity

	(1)	(2)	(3)
Baseline Familiar		0.88***	
		(0.117)	
Baseline Familiar × Know Get There	0.90***		
	(0.116)		
Baseline Familiar × Not Know Get There	0.22		
	(0.177)		
Not Baseline Familiar × Know Get There		0.16	
		(0.153)	
Baseline Familiar × Landmark			1.00***
			(0.120)
Baseline Familiar × No Landmark			0.54***
			(0.124)
Training	0.30**	0.29**	0.31**
	(0.110)	(0.110)	(0.111)
Training × Unfamiliar	0.83***	0.85***	0.82***
	(0.211)	(0.207)	(0.212)
N	6,756	6,756	6,756
Job Attribute Controls	Yes	Yes	Yes

Notes: This table replicates the logit specification in Column 3 of Table 2 unpacking the baseline familiarity variable by the strength of the familiarity reported at baseline. The other independent variables from Table 2 are included but not reported. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.20: Spillovers To Other Unfamiliar Neighborhood

	(1)	(2)	(3)
Baseline Familiar	0.77***	0.70***	0.77***
	(0.103)	(0.098)	(0.106)
Training	0.29**	0.32***	0.29**
	(0.106)	(0.089)	(0.097)
Training × Unfamiliar	0.75***	0.87***	0.84***
	(0.202)	(0.164)	(0.193)
Unfamiliar Not Visited × Treated	0.25	0.24	0.27
	(0.173)	(0.163)	(0.184)
Distance (km)	-0.24***		-0.25***
	(0.014)		(0.017)
Job duration (hrs)	-0.58***	-0.61***	-0.59***
	(0.052)	(0.055)	(0.052)
$\mathbb{E}$ Compensation (100 KSH)	0.75***	0.80***	0.78***
	(0.024)	(0.026)	(0.025)
Cash Upfront (100 KSh)	0.08*	0.09*	0.09*
	(0.040)	(0.039)	(0.043)
Bonus (100 KSh)	-0.21***	-0.22***	-0.21***
	(0.010)	(0.010)	(0.010)
Home × neighborhood FE		Yes	
Neighborhood FE			Yes
N	8,183	8,183	8,183

Notes: This table replicates the logit results from Table 2, plus a logit specification with distance and neighborhood fixed effects (column 5 in Table A.11), to explore spillover effects on the unfamiliar neighborhoods not visited by the treated group. The sample is all job choices (seven per day for three days), including those that involve the tenth target neighborhood (unfamiliar spillover). The variable "Unfamiliar Not Visited × Treated" is switched on for the treatment group and the unfamiliar spillover target neighborhood. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.21: Selection Model For Duration and Navigation to Unfamiliar Neighborhoods

	Task App (hours)		Navigation (Post-Visit)			
	To Job	From Job	Get Lost There	Get Lost Back	Ask Directions There	Ask Directions Back
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline familiar	-0.07*	-0.06	-0.01	0.00	-0.10*	-0.04*
	(0.03)	(0.04)	(0.02)	(0.00)	(0.04)	(0.02)
Training	-0.07*	-0.02	-0.05***	0.00	-0.18***	-0.01
	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)
Training × Unfamiliar	0.05	-0.02	0.01	0.00	-0.07	-0.03
	(0.05)	(0.06)	(0.02)	(0.01)	(0.04)	(0.02)
Distance (km)	0.09***	0.10***	0.00	0.00	0.01	-0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Cons	0.50***	0.40***	0.06*	0.00	0.26***	0.07**
	(0.05)	(0.07)	(0.03)	(0.00)	(0.05)	(0.02)
Round trip, (1)+(2):						
Baseline familiar	-0.13 [-0.25, -0.01]					
Training × Unfamiliar	0.03 [-0.15, 0.22]					
Control Mean	1.23	1.30	0.06	0.00	0.31	0.05
N	2,291	2,257	2,318	2,318	2,318	2,318

Notes: This table presents estimates from a Heckman (1976) two-step selection model with a probit selection equation. The sample includes participants in the "preferences" elicitation group who completed the job during the employment period. Each day, we focus on the job choice question (among the seven questions asked) that was randomly selected to be implemented. The outcomes correspond to the job selected by the participant. We use the difference between the two job options of the (randomly chosen) job attributes as instruments for the selection problem: wages, wage bonus, and job duration. Travel duration is measured between the study venue (using survey data timestamps) and the job (using the smartphone task app check-in/check-out). The outcomes in the last four columns are based on survey responses when participants return to the study venue after the job. Odd columns refer to the trip toward the job, and even columns refer to the return trip to the study venue. Standard errors in parentheses based on 1,000 individual-level bootstrap runs. The *Round trip* panel reports the sums of the corresponding coefficients in columns (1) and (2), with 95% confidence intervals in brackets, from the same bootstrap, in which both models are re-estimated jointly on each replication. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.22: Determinants of Consideration in "Open" Elicitation (Day 2)

	(1)	(2)	(3)
Baseline Familiar	1.01***	0.71***	0.85***
	(0.060)	(0.063)	(0.070)
Training	1.66***	1.67***	1.64***
	(0.124)	(0.122)	(0.145)
Training × Unfamiliar	-0.04	0.04	-0.02
	(0.174)	(0.169)	(0.180)
Distance (km)	-0.21***		-0.24***
	(0.009)		(0.013)
Job Compensation (100 KSh)	0.09*	0.08*	0.09*
	(0.039)	(0.039)	(0.041)
Job Duration (hours)	0.09	0.11	0.10
	(0.078)	(0.080)	(0.079)
NBH x home FE		Yes	
NBH FE			Yes
N	10,413	10,413	10,413

Notes: Replicates Table 5 using data from day 2 of employment. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.23: Determinants of Consideration in "Open" Elicitation (Day 3)

	(1)	(2)	(3)
Baseline Familiar	0.94***	0.65***	0.81***
	(0.060)	(0.071)	(0.075)
Training	1.55***	1.55***	1.50***
	(0.139)	(0.136)	(0.126)
Training × Unfamiliar	0.17	0.28	0.22
	(0.192)	(0.192)	(0.200)
Distance (km)	-0.25***		-0.27***
	(0.010)		(0.012)
Job Compensation (100 KSh)	-0.01	0.01	-0.01
	(0.036)	(0.044)	(0.039)
Job Duration (hours)	-0.00	-0.01	-0.00
	(0.075)	(0.077)	(0.081)
NBH x home FE		Yes	
NBH FE			Yes
N	10,289	10,289	10,289

Notes: Replicates Table 5 using data from day 3 of employment. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.24: Baseline Premium Persists Up to Three Years Since Last Visit

	(1)	(2)
Baseline Familiar	0.86***	
	(0.125)	
Visited < 1 month ago		0.94***
		(0.130)
Visited < 1 year ago		0.97***
		(0.136)
Visited < 3 years ago		0.78***
		(0.134)
Visited ≥ 3 years ago		0.39*
		(0.169)
Training	0.30**	0.31**
	(0.109)	(0.106)
Training × Unfamiliar	0.91***	0.89***
	(0.213)	(0.215)
N	6,756	6,756
Job Attribute Controls	Yes	Yes
Neighborhood FE	Yes	Yes

Notes: This table presents results from a logit specification with distance and neighborhood fixed effects estimated on the "preferences" elicitation choices (column 5 in Table A.11), splitting the baseline familiar coefficient based on how long ago the individual last visited the neighborhood. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.25: Impact of Familiarity and Experimental Exposure on Medium-run Job Acceptance - OLS with Individual Fixed Effects

		All Invites			Second Round Invites		
		(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	× (Wage ≤ 500)	0.096***	0.083***	0.083***	0.119***	0.103**	0.114***
		(0.023)	(0.024)	(0.024)	(0.031)	(0.034)	(0.033)
Training	× (Wage ≤ 500)	0.008	0.016	0.011	0.014	0.029	0.020
		(0.022)	(0.022)	(0.022)	(0.031)	(0.033)	(0.032)
Training × Unfamiliar	× (Wage ≤ 500)	0.078	0.075	0.076	0.115	0.078	0.098
		(0.040)	(0.040)	(0.040)	(0.059)	(0.061)	(0.060)
Distance (km)	× (Wage ≤ 500)	-0.027***		-0.028***	-0.030***		-0.026***
		(0.003)		(0.004)	(0.004)		(0.006)
Wage	× (Wage ≤ 500)	0.107***	0.105***	0.106***	0.132***	0.129***	0.130***
		(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)
(Wage > 500)		0.201	0.459***	0.212*			
		(0.102)	(0.088)	(0.103)			
Baseline Familiar	× (Wage > 500)	0.020	-0.040	0.002			
		(0.035)	(0.035)	(0.035)			
Visited Any	× (Wage > 500)	0.040	0.037	0.045			
		(0.036)	(0.038)	(0.037)			
Visited Unfamiliar	× (Wage > 500)	-0.011	-0.009	-0.013			
		(0.061)	(0.062)	(0.062)			
Distance (km)	× (Wage > 500)	0.002		-0.001			
		(0.005)		(0.005)			
Wage	× (Wage > 500)	0.022*	0.022*	0.022*			
		(0.010)	(0.010)	(0.010)			
N		3,946	3,944	3,946	1,769	1,765	1,769
Training × Unfamiliar/Baseline Familiar		0.814	0.901	0.912	0.969	0.756	0.86
Survey Day FE		Yes	Yes	Yes	Yes	Yes	Yes
Invite Sequence FE		Yes	Yes	Yes	Yes	Yes	Yes
Randomization Strata FE		Yes	Yes	Yes	Yes	Yes	Yes
Individual FE		Yes	Yes	Yes	Yes	Yes	Yes
Home × Neighborhood FE			Yes			Yes	
Neighborhood FE				Yes			Yes

Notes: This table replicates the results from Table 6 using a linear probability model and individual fixed effects. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.26: Impact of Familiarity and Experimental Exposure on Medium-run Job Acceptance - OLS

		All Invites			Second Round Invites		
		(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	× (Wage ≤ 500)	0.086***	0.075**	0.069**	0.101***	0.089**	0.095**
		(0.025)	(0.026)	(0.026)	(0.030)	(0.033)	(0.032)
Training	× (Wage ≤ 500)	0.027	0.036	0.030	0.040	0.056	0.046
		(0.024)	(0.024)	(0.024)	(0.030)	(0.031)	(0.030)
Training × Unfamiliar	× (Wage ≤ 500)	0.054	0.053	0.054	0.058	0.036	0.053
		(0.046)	(0.046)	(0.046)	(0.055)	(0.055)	(0.055)
Distance (km)	× (Wage ≤ 500)	-0.031***		-0.033***	-0.030***		-0.028***
		(0.003)		(0.004)	(0.004)		(0.005)
Wage	× (Wage ≤ 500)	0.114***	0.111***	0.113***	0.126***	0.125***	0.126***
		(0.007)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)
(Wage > 500)		0.343**	0.593***	0.360***			
		(0.108)	(0.091)	(0.108)			
Baseline Familiar	× (Wage > 500)	0.019	-0.044	-0.006			
		(0.037)	(0.038)	(0.038)			
Training	× (Wage > 500)	0.016	0.010	0.018			
		(0.038)	(0.040)	(0.039)			
Training × Unfamiliar	× (Wage > 500)	-0.011	0.001	-0.012			
		(0.062)	(0.063)	(0.062)			
Distance (km)	× (Wage > 500)	-0.001		-0.006			
		(0.005)		(0.005)			
Wage	× (Wage > 500)	0.012	0.014	0.013			
		(0.011)	(0.011)	(0.011)			
N		3,960	3,958	3,960	1,814	1,811	1,814
Training × Unfamiliar/Baseline Familiar		0.629	0.705	0.794	0.576	0.402	0.562
Survey Day FE		Yes	Yes	Yes	Yes	Yes	Yes
Invite Sequence FE		Yes	Yes	Yes	Yes	Yes	Yes
Randomization Strata FE		Yes	Yes	Yes	Yes	Yes	Yes
Home × Neighborhood FE			Yes			Yes	
Neighborhood FE				Yes			Yes

Notes: This table replicates the results from Table 6 using a linear probability model. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.27: Travel Patterns One Month After the Experiment

	Endline Survey (prompted) In the last two weeks									SMS (unprompted) Yesterday	
	Any Trip	Search Work	Work	School	Shopping	Fun/Leisure	Healthcare	Other Errands	Other	Visited	Num Visits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Baseline Familiar	0.149***	0.062***	0.027***	0.002	0.004	0.023***	0.004	0.024***	0.015***	0.008*	0.048***
	(0.014)	(0.009)	(0.008)	(0.002)	(0.004)	(0.005)	(0.003)	(0.006)	(0.004)	(0.003)	(0.013)
Training	0.088***	0.039**	0.026*	0.003	0.001	0.014	0.005	0.008	0.001	0.020***	0.109***
	(0.019)	(0.015)	(0.012)	(0.003)	(0.005)	(0.008)	(0.004)	(0.007)	(0.005)	(0.005)	(0.026)
Training × Unfamiliar	−0.022	0.004	−0.022	−0.004	0.0004	0.002	−0.007	0.003	0.003	−0.010	−0.069*
	(0.028)	(0.021)	(0.015)	(0.003)	(0.006)	(0.011)	(0.004)	(0.010)	(0.006)	(0.006)	(0.031)
N	6,927	6,927	6,927	6,927	6,927	6,927	6,927	6,927	6,927	5,163	5,163
Mean	0.267	0.113	0.069	0.003	0.015	0.039	0.006	0.035	0.016	0.027	0.151
Training + Training × Unfamiliar	0.066	0.043	0.004	-0.001	0.002	0.016	-0.002	0.011	0.004	0.009	0.041
(se)	(0.016)	(0.011)	(0.007)	(0.001)	(0.004)	(0.007)	(0.002)	(0.006)	(0.003)	(0.003)	(0.014)
[p-value]	[0.000]	[0.000]	[0.580]	[0.408]	[0.695]	[0.020]	[0.295]	[0.071]	[0.256]	[0.007]	[0.004]
Home × Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports OLS results based on equation (1). All outcomes are measures of whether the individual reports returning to a neighborhood. The sample is all respondents and the nine target neighborhoods, excluding the unfamiliar spillover target neighborhood. Columns 1-9 are trips measured using the over-the-phone endline survey while columns 10 and 11 are visits measured by the high-frequency SMS. The results show that people re-visit the neighborhoods from the study, including initially unfamiliar neighborhoods, for a variety of reasons. Table A.28 presents the results without fixed effects. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.28: Travel Patterns One Month After the Experiment (No Fixed Effects)

	Endline Survey (prompted) In the last two weeks									SMS (unprompted) Yesterday	
	Any Trip	Search Work	Work	School	Shopping	Fun/Leisure	Healthcare	Other Errands	Other	Visited	Num Visits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Baseline Familiar	0.237***	0.113***	0.063***	0.003	0.010**	0.033***	0.005*	0.026***	0.018***	0.026***	0.144***
	(0.013)	(0.009)	(0.007)	(0.001)	(0.004)	(0.005)	(0.002)	(0.005)	(0.004)	(0.003)	(0.013)
Training	0.076***	0.032*	0.021	0.003	0.0005	0.012	0.004	0.008	-0.0004	0.019***	0.105***
	(0.020)	(0.015)	(0.012)	(0.003)	(0.005)	(0.008)	(0.004)	(0.007)	(0.005)	(0.005)	(0.027)
Training × Unfamiliar	−0.013	0.010	−0.019	−0.004	0.0005	0.003	−0.005	0.002	0.003	−0.009	−0.063*
	(0.028)	(0.021)	(0.015)	(0.003)	(0.006)	(0.011)	(0.004)	(0.010)	(0.006)	(0.006)	(0.031)
N	6,927	6,927	6,927	6,927	6,927	6,927	6,927	6,927	6,927	5,163	5,163
Mean	0.267	0.113	0.069	0.003	0.015	0.039	0.006	0.035	0.016	0.027	0.151
Training + Training × Unfamiliar	0.063	0.043	0.003	-0.001	0.001	0.016	-0.001	0.010	0.003	0.010	0.041
(se)	(0.016)	(0.011)	(0.007)	(0.001)	(0.004)	(0.007)	(0.002)	(0.006)	(0.003)	(0.003)	(0.013)
[p-value]	[0.000]	[0.000]	[0.703]	[0.317]	[0.818]	[0.020]	[0.661]	[0.100]	[0.392]	[0.003]	[0.002]

Notes: This table replicates the results from Table A.27 without including neighborhood fixed effects. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

A.3 The Model: Derivations and Estimation

This appendix proves Propositions 1 and 2, derives the estimating equations (1) and (2) and the means of their coefficients, proves Proposition 3, and describes the normalization and the likelihood of the normal mixed logit.

A.3.1 Potential Valuations and Proofs of Propositions 1 and 2

Under Assumption 1, agent i's potential valuations are

$$Y_{in}(d) = \begin{cases} u_{in} + \beta_{in}^{T} d, & \text{Fam}_{in} = 1, \\ \widetilde{u}_{in}^{0} - c_i, & \text{Fam}_{in} = 0,\ d = 0, \\ \widetilde{u}_{in}^{1} + \beta_{in}^{T}, & \text{Fam}_{in} = 0,\ d = 1. \end{cases} \tag{A.1}$$

The valuation of a familiar neighborhood is its known utility plus any direct effect of training. Before a visit to an unfamiliar neighborhood, i values it at the prior mean and expects to pay the first-visit cost. A visit replaces the prior mean with the posterior mean and sinks the cost.

Consider next the announced neighborhood n. By Assumption 2(a), the announcement leaves i's beliefs about u_{in} unchanged. By the law of iterated expectations, i's current expectation of $\widetilde{u}_{in}^{1}$ therefore equals $\widetilde{u}_{in}^{0}$. By part (b), i expects the first-visit cost to be sunk by the day of the job. Part (c) rules out option value from refusing the job after the visit. The valuation of the announced neighborhood is therefore

$$Y_{in}^{A} = \begin{cases} u_{in} + \beta_{in}^{AT}, & \text{Fam}_{in} = 1, \\ \widetilde{u}_{in}^{0} + \beta_{in}^{AT}, & \text{Fam}_{in} = 0. \end{cases} \tag{A.2}$$

Averages weight participants equally, and every participant has unfamiliar target neighborhoods. By (A.1), the familiarity premium is

$$\Delta^{F} = \mathbb{E}[u_{in} \mid \text{Fam}_{in} = 1] - \mathbb{E}\big[\widetilde{u}_{in}^{0} \mid \text{Fam}_{in} = 0\big] + \mathbb{E}[c_i]. \tag{A.3}$$

Adding and subtracting $\mathbb{E}[u_{in} \mid \text{Fam}_{in} = 0]$ gives the expression in Proposition 1. In an unfamiliar neighborhood, (A.1) gives $Y_{in}(1) - Y_{in}(0) - \beta_{in}^{T} = \widetilde{u}_{in}^{1} - \widetilde{u}_{in}^{0} + c_i$. The exposure effect is therefore

$$\Delta^{U} = \mathbb{E}[c_i] + \mathbb{E}\big[\widetilde{u}_{in}^{1} - \widetilde{u}_{in}^{0} \mid \text{Fam}_{in} = 0\big]. \tag{A.4}$$

In an unfamiliar neighborhood, (A.1) and (A.2) give $Y_{in}^{A} - Y_{in}(0) - \beta_{in}^{AT} = c_i$. The anticipated

exposure effect is therefore

$$\Delta^{AU} = \mathbb{E}[c_i]. \tag{A.5}$$

Subtracting the two expressions in Proposition 1 gives

$$\Delta^F - \Delta^U = \mathbb{E}[u_{in} \mid \text{Fam}_{in} = 1] - \mathbb{E}[u_{in} \mid \text{Fam}_{in} = 0] + \mathbb{E}\left[u_{in} - \widetilde{u}^1_{in} \mid \text{Fam}_{in} = 0\right].$$

This difference is sorting plus the mean error after one visit.

Calibration holds, for example, when i updates a correctly specified prior by Bayes' rule. The mean prior error $\mathbb{E}[u_{in} - \widetilde{u}^0_{in} \mid \text{Fam}_{in} = 0]$ is then zero. The law of iterated expectations gives $\mathbb{E}[\widetilde{u}^1_{in} \mid \text{Fam}_{in} = 0] = \mathbb{E}[u_{in} \mid \text{Fam}_{in} = 0]$, so the mean error after a visit is also zero.

A.3.2 Estimating Equations and Mean Coefficients

We label the job neighborhood alternatives by their baseline familiarity and training status. $F0$ and $F1$ denote untrained and trained familiar neighborhoods, and $U0$ and $U1$ denote untrained and trained unfamiliar neighborhoods. On a training day, FA and UA denote the familiar and unfamiliar neighborhoods where training was announced.

By Assumption 1, i's valuation of n on an employment day is $Y_{in}(\text{Train}_{in})$ as given by equation (A.1). On employment days, control participants face $F0$, $F1$, and $U0$ neighborhoods, and treatment participants face $F0$ and $U1$ neighborhoods. Without Assumption 1 applied to spillovers from training in unfamiliar neighborhoods on other unfamiliar neighborhoods, spillovers among a treatment participant's trained unfamiliar neighborhoods would add to β^U_{in}. Its mean would then measure the effect of training in all of these neighborhoods rather than the effect of a single visit. Because the estimation sample excludes the tenth target neighborhood, employment-day choices use this part of Assumption 1 only for trained neighborhoods.

On a training day, Assumption 1 rules out spillovers from earlier training days, so the announced neighborhood has the valuation Y^A_{in} in (A.2). By part (a) of Assumption 2, the announcement does not affect the other neighborhoods. By part (b), i expects to pay the first-visit cost on the job in any unannounced untrained unfamiliar neighborhood. The other neighborhoods therefore keep their valuations $Y_{in}(\text{Train}_{in})$, where $\text{Train}_{in} = 1$ for neighborhoods visited on earlier training days.

With the common prior mean of Section 4.2, $\widetilde{u}^0_{in} = \widetilde{u}^0_i$, subtracting the valuation $\widetilde{u}^0_i - c_i$ of an untrained unfamiliar neighborhood expresses each valuation through the coefficients

$\beta^F_{in} = u_{in} - \widetilde{u}^0_i + c_i$, $\beta^U_{in} = \widetilde{u}^1_{in} - \widetilde{u}^0_i + c_i$, and $\beta^{AU}_i = c_i$:

$$Y_{in} - (\widetilde{u}^0_i - c_i) = \begin{cases} \beta^F_{in}, & F0, \\ \beta^F_{in} + \beta^T_{in}, & F1, \\ 0, & U0, \\ \beta^T_{in} + \beta^U_{in}, & U1, \end{cases} \qquad Y^A_{in} - (\widetilde{u}^0_i - c_i) = \begin{cases} \beta^F_{in} + \beta^{AT}_{in}, & FA, \\ \beta^{AT}_{in} + \beta^{AU}_i, & UA. \end{cases} \tag{A.6}$$

The utility of a job offer adds distance, the job attributes, and the choice shock to the valuation. Substituting (A.6) gives (2). On employment days, equation (2) reduces to equation (1).

Training assignment and the designation of main neighborhoods are random. Conditional on baseline familiarity, the coefficients of a participant's target neighborhoods are therefore jointly independent of treatment status and of the designation of main neighborhoods. Each cell in (A.6) thus has the coefficient distribution of its familiarity class, and the three main unfamiliar neighborhoods represent all four unfamiliar target neighborhoods. By (A.1), (A.2), and the definitions of the estimands, the coefficients have the means

$$\mathbb{E}[\beta^F_{in} \mid \text{Fam}_{in} = 1] = \Delta^F, \qquad \mathbb{E}[\beta^U_{in} \mid \text{Fam}_{in} = 0] = \Delta^U, \qquad \mathbb{E}[\beta^{AU}_i] = \Delta^{AU}.$$

Choices identify the mean of β^F_{in} and, in familiar neighborhoods, the means of β^T_{in} and β^{AT}_{in}. In unfamiliar neighborhoods, choices identify only the means of the sums $\beta^T_{in} + \beta^U_{in}$ and $\beta^{AT}_{in} + \beta^{AU}_i$. Assumptions 3 and 5 equate the means of β^T_{in} and β^{AT}_{in} across familiar and unfamiliar neighborhoods. The remaining two means are then identified:

$$\begin{aligned} \mathbb{E}[\beta^U_{in} \mid \text{Fam}_{in} = 0] &= \mathbb{E}[\beta^T_{in} + \beta^U_{in} \mid \text{Fam}_{in} = 0] - \mathbb{E}[\beta^T_{in} \mid \text{Fam}_{in} = 1], \\ \mathbb{E}[\beta^{AU}_i] &= \mathbb{E}[\beta^{AT}_{in} + \beta^{AU}_i \mid \text{Fam}_{in} = 0] - \mathbb{E}[\beta^{AT}_{in} \mid \text{Fam}_{in} = 1]. \end{aligned} \tag{A.7}$$

The normal mixed logit gives β^T_{in} and β^{AT}_{in} the same distribution in familiar and unfamiliar neighborhoods. Its means $\bar{\beta}^U$ and $\bar{\beta}^{AU}$ therefore estimate the right-hand sides of (A.7). If Assumption 3 or 5 fails, these estimates measure Δ^U or Δ^{AU} plus the gap in the mean direct effect between unfamiliar and familiar neighborhoods.

A.3.3 Proof of Proposition 3 and Its Extension to Anticipation

In the normal learning model of Section 4.2, the prior mean is $\widetilde{u}_i^0 = u_i^U - e_i$, and Bayes' rule gives the posterior mean

$$\widetilde{u}_{in}^1 = \lambda\, \widetilde{u}_i^0 + (1-\lambda) z_{in}, \qquad \lambda = \frac{\widetilde{\sigma}_\eta^2}{\widetilde{\tau}_U^2 + \widetilde{\sigma}_\eta^2}. \tag{A.8}$$

Full revelation corresponds to $\widetilde{\sigma}_\eta^2 = 0$, so $\lambda = 0$. Calibrated variances have $\widetilde{\tau}_U^2 = \tau_U^2$ and $\widetilde{\sigma}_\eta^2 = \sigma_\eta^2$.

Substituting (A.8) into $\beta_{in}^U = \widetilde{u}_{in}^1 - \widetilde{u}_i^0 + c_i$ gives, for $\mathrm{Fam}_{in} = 0$,

$$\beta_{in}^U = (1-\lambda)\big(z_{in} - \widetilde{u}_i^0\big) + c_i = c_i + (1-\lambda) e_i + (1-\lambda)\big(u_{in} - u_i^U + \eta_{in}\big).$$

For $\mathrm{Fam}_{in} = 1$, $\beta_{in}^F = u_{in} - u_i^U + e_i + c_i = s_i + e_i + c_i + (u_{in} - u_i^F)$. Finally, $\beta_i^{AU} = c_i$. On average, a visit therefore corrects the share $1-\lambda$ of the prior error.

The coefficients then take the form in Proposition 3, with the means stated there, $\bar{\beta}^{AU} = \mathbb{E}[c_i]$, and

$$\begin{aligned}
\theta_i^F &= (s_i - \mathbb{E}[s_i]) + (e_i - \mathbb{E}[e_i]) + (c_i - \mathbb{E}[c_i]), & a_{in}^F &= u_{in} - u_i^F, \\
\theta_i^U &= (c_i - \mathbb{E}[c_i]) + (1-\lambda)(e_i - \mathbb{E}[e_i]), & a_{in}^U &= (1-\lambda)\big(u_{in} - u_i^U + \eta_{in}\big), \\
\theta_i^{AU} &= c_i - \mathbb{E}[c_i].
\end{aligned} \tag{A.9}$$

The person components are linear in the jointly normal terms of Assumption 4, so they are jointly normal with mean zero. The deviations $u_{in} - u_i^F$ and $u_{in} - u_i^U$ and the signal noise η_{in} are independent across pairs, of the person-level terms, and of (a_{in}^T, a_{in}^{AT}). The pair components are therefore independent normal, with variances $\omega_F^2 = \tau_F^2$ and $\omega_U^2 = (1-\lambda)^2(\tau_U^2 + \sigma_\eta^2)$.

On employment days, any covariance matrix Ω of $(\theta_i^F, \theta_i^T, \theta_i^U)$ is attainable. For example, a common prior error $e_i = \mathbb{E}[e_i]$ together with $c_i - \mathbb{E}[c_i] = \theta_i^U$ and $s_i - \mathbb{E}[s_i] = \theta_i^F - \theta_i^U$ reproduces it. In the extension of Section 4.2, θ_i^{AT} joins the jointly normal terms of Assumption 4, and

$$\begin{pmatrix} \theta_i^F \\ \theta_i^U \\ \theta_i^{AU} \end{pmatrix} = A \begin{pmatrix} s_i - \mathbb{E}[s_i] \\ e_i - \mathbb{E}[e_i] \\ c_i - \mathbb{E}[c_i] \end{pmatrix}, \qquad A = \begin{pmatrix} 1 & 1 & 1 \\ 0 & 1-\lambda & 1 \\ 0 & 0 & 1 \end{pmatrix}, \qquad \det A = 1 - \lambda.$$

For $\lambda < 1$, A is invertible. Any covariance matrix of $(\theta_i^F, \theta_i^T, \theta_i^U, \theta_i^{AT}, \theta_i^{AU})$ therefore arises

from the covariance matrix of $(s_i, e_i, c_i, \theta_i^T, \theta_i^{AT})$ obtained by inverting A. Exact normality requires a common λ. With a person-specific λ_i, the term $(1-\lambda_i)e_i$ in β_{in}^U would not be normal.

The means give a mean belief revision of $\bar{\beta}^U - \bar{\beta}^{AU} = (1-\lambda)\,\mathbb{E}[e_i]$ and a mean error after a visit of $\lambda\,\mathbb{E}[e_i]$. The normal learning model also predicts the gap in mean beliefs between familiar and unfamiliar neighborhoods. Before a visit, this gap is $\mathbb{E}[u_{in} \mid \text{Fam}_{in} = 1] - \mathbb{E}[\widetilde{u}_i^0] = \mathbb{E}[s_i] + \mathbb{E}[e_i]$. After a visit, it is $\mathbb{E}[s_i] + \lambda\,\mathbb{E}[e_i]$. Table 3 measures beliefs about neighborhood attributes rather than utility. If the normal learning model applies to these attributes, their gaps close after a visit only when $\mathbb{E}[s_i] + \lambda\,\mathbb{E}[e_i]$ is close to zero. The closing gaps in Table 3 are therefore consistent with a small λ and little sorting.

A.3.4 Identification and Normalization of the Variance Parameters

Within the treatment or the control group, only differences between the person-level parts of the alternatives in the group's choice sets enter the likelihood. The likelihood therefore depends on the person components only through the covariance matrix of these differences. Relative to $U0$, the person-level parts are

Alternative	Person-level part	Control participants	Treatment participants
$F0$: untrained familiar	θ_i^F	all days	all days
$F1$: trained familiar	$\theta_i^F + \theta_i^T$	after first visit	never
FA: announced familiar	$\theta_i^F + \theta_i^{AT}$	training days	never
$U0$: untrained unfamiliar	0	all days	training days
$U1$: trained unfamiliar	$\theta_i^T + \theta_i^U$	never	after first visit
UA: announced unfamiliar	$\theta_i^{AT} + \theta_i^{AU}$	never	training days

"After first visit" means from the second training day on, including all employment days. Repeated appearances of each pair, and several pairs per participant, separate the pair-level variances from the person-level moments. As in other logit mixtures, only utility differences within a choice set enter the likelihood (Walker et al. 2007). Each pair of alternatives that never appears in the same participant's choices therefore leaves one covariance unidentified.

On employment days, control participants face $U0$, $F0$, and $F1$. Their choices identify $\text{Var}(\theta_i^F)$, $\text{Var}(\theta_i^T)$, and $\text{Cov}(\theta_i^F, \theta_i^T)$. Treatment participants face only $F0$ and $U1$. Their choices identify only the variance of the contrast between these alternatives, $\theta_i^F - \theta_i^T - \theta_i^U$. We normalize this contrast to be independent of (θ_i^F, θ_i^T). Every value of its variance remains attainable, so the normalization does not restrict the likelihood. At the pair level, pairs in $F0$, $F1$, and $U1$ identify ω_F^2, $\omega_F^2 + \omega_T^2$, and $\omega_T^2 + \omega_U^2$. Pairs in $U0$ have no pair component,

because $\widetilde{u}_i^0 - c_i$ is the same for all of i's unfamiliar neighborhoods. The employment-day model therefore has $4 + 3 = 7$ variance and covariance parameters.

In the pooled choices, training days add FA for control participants and $U0$ and UA for treatment participants. Control participants' choices identify the covariance matrix of $(\theta_i^F, \theta_i^T, \theta_i^{AT})$. Treatment participants' choices identify the covariance matrix of $(\theta_i^F, \theta_i^T + \theta_i^U, \theta_i^{AT} + \theta_i^{AU})$. Randomization makes $\mathrm{Var}(\theta_i^F)$ common to both groups. The two matrices therefore contain $6 + 6 - 1 = 11$ identified moments. No participant faces $F1$ or FA together with $U1$ or UA. The covariances between the control-only terms $x_i \in \{\theta_i^T, \theta_i^{AT}\}$ and the treatment-only terms $y_i \in \{\theta_i^T + \theta_i^U,\ \theta_i^{AT} + \theta_i^{AU}\}$ are therefore not identified. We normalize these terms to be independent conditional on θ_i^F:

$$\mathrm{Cov}(x_i, y_i) = \frac{\mathrm{Cov}(x_i, \theta_i^F)\ \mathrm{Cov}(\theta_i^F, y_i)}{\mathrm{Var}(\theta_i^F)}. \tag{A.10}$$

With the terms ordered as $(\theta_i^F, \theta_i^T, \theta_i^{AT}, \theta_i^T + \theta_i^U, \theta_i^{AT} + \theta_i^{AU})$, normalization (A.10) is equivalent to the Cholesky factor

$$L = \begin{pmatrix} \ell_{11} & 0 & 0 & 0 & 0 \\ \ell_{21} & \ell_{22} & 0 & 0 & 0 \\ \ell_{31} & \ell_{32} & \ell_{33} & 0 & 0 \\ \ell_{41} & 0 & 0 & \ell_{44} & 0 \\ \ell_{51} & 0 & 0 & \ell_{54} & \ell_{55} \end{pmatrix}. \tag{A.11}$$

Rows 1–3 form the Cholesky factor of the control block, and rows 1, 4, and 5 form that of the treatment block. Any two positive semidefinite blocks with a common $\mathrm{Var}(\theta_i^F)$ are therefore attainable, so the normalization does not restrict the likelihood. The 11 free entries of L match the 11 identified moments. The pair level adds ω_{AT}^2. Because β_{in}^{AT} has the same distribution in familiar and unfamiliar neighborhoods, ω_{AT}^2 also governs pairs in UA. The pooled model therefore has $11 + 4 = 15$ variance and covariance parameters.

To implement the normalization, we draw the five terms from L and recover θ_i^U and θ_i^{AU} by subtracting θ_i^T and θ_i^{AT}. On employment days alone, $\mathrm{Cov}(\theta_i^F, \theta_i^T + \theta_i^U)$ is not identified. The employment-day normalization corresponds to rows 1, 2, and 4 of L with $\ell_{41} = \ell_{11}$. In the pooled model, treatment participants' training-day choices involving $U0$ identify ℓ_{41}.

The pair components are mutually independent. In particular, a_{in}^T and a_{in}^{AT} are independent. The sequences $F0 \to FA \to F1$ and $U0 \to UA \to U1$ of a single pair would identify their covariance.

A.3.5 Likelihood and Estimation

Conditional on the person components and on the pair components of i's target neighborhoods, each choice is a binary logit with the utilities in (2). Person i's likelihood contribution multiplies these probabilities over all of i's choices in the estimation sample and integrates over the components. The employment-day model uses i's employment-day choices, and the pooled model adds i's training-day choices. We approximate the integral by simulation with 16,000 draws and maximize the simulated log likelihood. Each simulation draw assigns one set of person components to the participant and one set of pair components to each of the participant's target neighborhoods. These components stay fixed across all of the participant's choices on training and employment days.

The comparison of $\bar{\beta}^{AU}$ with $\bar{\beta}^{U}$ relies on the assumption of Section 4.2 that the choice shocks have unit scale on training days as well.

A.4 Listing of Nairobi Neighborhoods and Selection into Sample

We divided Nairobi into 63 neighborhoods with commonly used and recognized names. To establish neighborhood boundaries, we worked with a mapping team from the Busara Center for Behavioral Economics in consultation with local field guides. Neighborhood names and landmarks were extensively piloted to make sure we were using labels which respondents would recognize.

We excluded 15 neighborhoods from our study that we deemed not relevant for work opportunities for our study population. Table A.29 lists these excluded neighborhoods and their descriptions. Excluded neighborhoods consist of national parks, the airport, university area, as well as the three home neighborhoods and six additional informal residential settlements, which we excluded because of limited work opportunities and potential lack of safety. We did not collect familiarity data for these neighborhoods and we did not offer jobs in these neighborhoods.

For each of our three home neighborhoods (Kibera, Kawangware, and Viwandani), we use the Google Maps API to calculate both walking and transit travel times from the study venue to the center of each of the 48 non-excluded neighborhoods. Neighborhoods within a 75 minute commute of each home neighborhood formed our final sample. This resulted in lists of 33, 30, and 31 sample neighborhoods for the three home neighborhoods, respectively. Participants were asked about their familiarity with these neighborhoods and were assigned target neighborhoods from this list.

We stopped collecting familiarity data in some neighborhoods around a third of the

Table A.29: Neighborhoods Excluded from Study

Name	Description
Dandora	Informal residential settlement
Golf course estate	Golf course
JKIA	Jomo Kenyatta International Airport
Kariobangi North	Informal residential settlement
Kawangware	Informal residential settlement and Recruitment Neighborhood
Kayole	Informal residential settlement
Kibera	Informal residential settlement and Recruitment Neighborhood
Langata - Bomas	Tourist village near national park
Mathare	Informal residential settlement
Mukuru Kwa Njenga	Informal residential settlement
Mukuru Kwa Ruben	Informal residential settlement
Nairobi National Park	National park
Ngong forest	Forest area
University of Nairobi	University area
Viwandani	Informal residential settlement and Recruitment Neighborhood

Notes: This table lists the neighborhoods in Nairobi that were excluded from familiarity elicitation and job offers for all participants.

way through data collection because their familiarity rates were either too high or too low. We excluded neighborhoods in batches 7 to 17 if, in the first 5 batches, more than 90% of participants or less than 10% of participants were familiar with them.[43] These familiarity rates, and the resulting exclusions, were calculated separately for each home neighborhood. After these additional exclusions, batches 7 to 17 had lists of 30, 26, and 23 sample neighborhoods for Kibera, Kawangware, and Viwandani, respectively. For our results on overall familiarity levels (e.g. Figure A.3a and Table 1), we impute familiarity for neighborhoods excluded from data collection in later batches. For the main measure of familiarity, we impute a value of 0 for neighborhoods excluded because they were too unfamiliar and a value of 1 for neighborhoods excluded because they were too familiar. These imputations were also calculated separately for each home neighborhood.

[43]Our study was implemented in 17 overlapping batches to organize field officer time. Every three business days, a new batch of respondents started the cycle of three recruitment and baseline days (phone recruitment/invitation, baseline 1, and baseline 2), three training days, and three employment days.

A.5 Algorithm to Select Target Neighborhoods

We select the ten target neighborhoods for each participant to minimize spatial spillovers between categories (e.g. accidentally inducing a visit to an unfamiliar neighborhood while traveling to a familiar one), to keep neighborhoods in each category close to each other, to balance distance from home to each category, and to prioritize nearby neighborhoods.[44]

For each participant, given their pattern of familiar and unfamiliar neighborhoods, we run an optimization algorithm to minimize the weighted sum of several cost components. First, we penalize spatial spillovers between the main familiar, other familiar, and all unfamiliar neighborhoods. We code such a spillover as happening when a 500-meter buffer around the Google Maps walk or transit route to a neighborhood n intersects a neighborhood m. Second, we penalize distance between neighborhoods within each group. Keeping neighborhoods within each group close to each other enables participants in the treatment group to develop deeper familiarity with a new area of a city. Third, we penalize differences between groups in the average distance from the home neighborhood to neighborhoods in that group. Finally, we penalize longer distance from the home neighborhood to each neighborhood.

Experimental Sample. We drop participants with fewer than four unfamiliar neighborhoods, and those with fewer than six familiar neighborhoods because we are unable to choose the correct number of target neighborhoods for them.

Starting with a sample of 1,168 participants who showed up at Baseline 1, we excluded 120 because they had fewer than 6 familiar neighborhoods, and 133 because they had fewer than 4 unfamiliar neighborhoods. Consequently, we invited 915 participants to continue in the study.

A.6 Measuring Beliefs

In our surveys, we ask the following beliefs questions to all participants:

1. Think about an average person who lives in your home neighborhood. If this person goes to X to find daily or casual work opportunities, they are likely to find one.
    - 1=Strongly disagree, 2=Disagree, 3=Neutral, 4=Agree, 5=Strongly agree.
2. If you go to X to find daily or casual work opportunities, you are likely to find one.
    - 1=Strongly disagree, 2=Disagree, 3=Neutral, 4=Agree, 5=Strongly agree.
3. If you find a daily or casual work opportunity in X, the pay is good.

[44] For large neighborhoods, we also elicit familiarity with smaller sub-areas within the neighborhood. If responses are internally inconsistent, e.g., a participant reports having visited a sub-area, but not the corresponding neighborhood, we exclude that neighborhood from the assignment algorithm.

- 1=Strongly disagree, 2=Disagree, 3=Neutral, 4=Agree, 5=Strongly agree.

4. If you travel alone, how safe is the trip from your home to X?
   - 1=Very Unsafe, 2=Unsafe, 3=Neutral, 4=Safe, 5=Very Safe.
5. How safe do you think X is?
   - 1=Very Unsafe, 2=Unsafe, 3=Neutral, 4=Safe, 5=Very Safe.

After each question, we also ask:

- How confident are you in the above answer?
  - 1=Not confident at all, 2=Slightly confident, 3=Somewhat confident, 4=Fairly confident, 5=Completely confident.

A.7 Additional Belief Results

We have also examined whether some participants update very negatively about the neighborhoods that they visit and do not find evidence for this. Figure A.8 displays the belief transition matrix for each respondent, their 3 visited neighborhoods, and for each of our five beliefs measures and their average. A key result is that there is little mass significantly below the diagonal. For the safety measures, almost all participants rate the trip to the neighborhood and the neighborhood itself as "Safe" or "Very Safe" and there are almost no participants who update downward significantly.

The results in Table 3 show that training visits also have an overall average effect on ratings across all neighborhoods. The α coefficient on $AfterTrain_t$ indicates that respondents update downward about job finding probabilities immediately after the visit, but update upward on safety. With only a pre-post comparison it is hard to interpret this coefficient (for example it could be due to mood differences between the end of a day of training and during the baseline, or it could be due to overly positive initial evaluations of familiar neighborhoods). Thus we focus on the treatment and control differences at both time points and include this coefficient for completeness.

We next assess whether this convergence in average beliefs is driven by increased agreement or increased randomness in responses. To do so, we use a split-sample approach to estimate the mean pre-belief for each neighborhood among those who were familiar prior to the experiment. Specifically, we randomly split the sample into 10 groups and for each group we predict familiar prior beliefs using the remaining 90% of the data. We then regress post beliefs for other participants on these average familiar priors.

Because we have relatively few observations per neighborhood, we estimate the priors for each neighborhood with a Bayesian partial-pooling random effects model. We assume rating

by an individual i for neighborhood n, $y_{in} \in \{1, 2, 3, 4, 5\}$, has an ordered logit likelihood where the latent rating is determined by a neighborhood level random effect μ_n. We estimate the model with the following priors for the random effects:

$$\begin{aligned} \mu_n &\sim N(0, \sigma^2) \\ \sigma^2 &\sim \text{InvGamma}(0.01, 0.01) \end{aligned}$$

And we use improper flat priors on the cut points for the ordinal logit. We estimate this model using the Stata Metropolis-Hastings algorithm with a burn-in period of 15,000 steps and a sample of 10,000 after thinning by retaining every 10th draw. We then compute the average prior belief for each neighborhood using the posterior predicted means of the model. We estimate the following regression for before training and after training beliefs

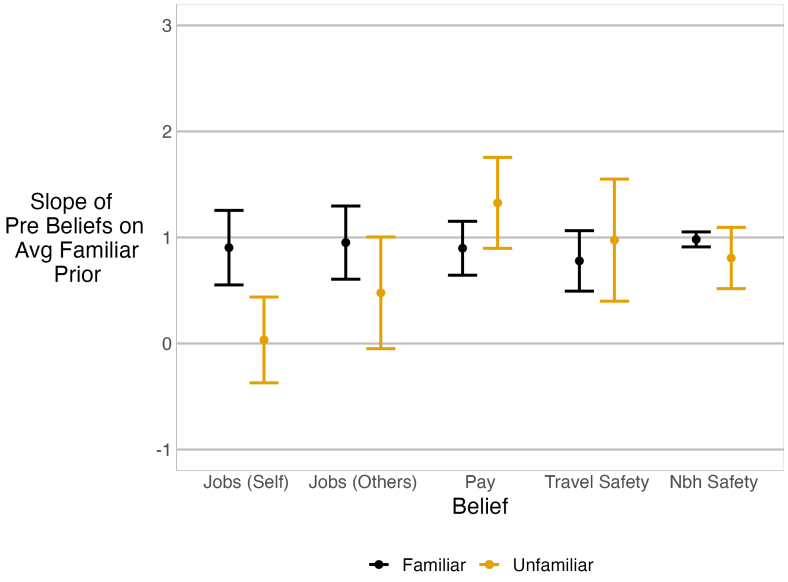
$$\text{Belief}_{in} = Treated_i + Prior_n + Treated_i \times Prior_n + \epsilon_{in}$$

with standard errors clustered at the neighborhood level.

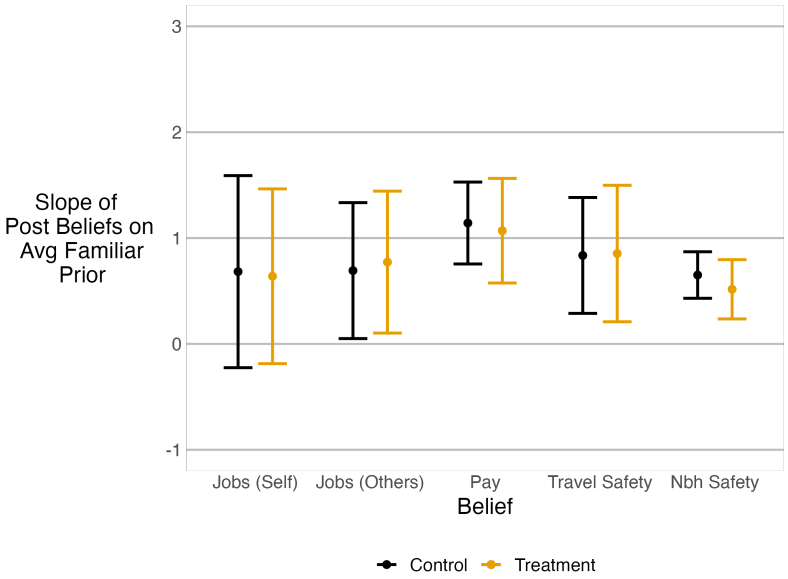
Figures A.10a and A.10b present the results for before training (pre) and after training (post) beliefs, respectively. The first result is that the beliefs we elicit have content: the Average Familiar Priors estimated on a subsample of participants strongly predict the beliefs of the held out participants. Second, in the post period, these results are always very similar for both those in the treatment group who had never been to the neighborhood before and control participants who had. This suggests that not only does one visit close the average beliefs gap between the two groups, it does so by leading the two groups to rate the individual neighborhoods in the same way. For the pre-beliefs, the Average Familiar Priors predict the beliefs of other participants familiar with the same neighborhood very well. For those unfamiliar with the neighborhood, the results are more variable depending on the outcome, and for four out of five outcomes we cannot reject a coefficient equal to 1. These results show that participants have agreement over how different neighborhoods compare on the dimensions in the beliefs data, and this also holds after a single visit to an unfamiliar neighborhood.

Figure A.10: Relationship Between Average Familiar Priors and Posteriors by Treatment

(a) Before Training Beliefs



(b) After Training Beliefs



Notes: The figure presents the results comparing beliefs between different respondents, before and after the training intervention. We first construct for each neighborhood and each outcome a predicted rating based on priors from respondents familiar with the respective neighborhood, using a Bayesian model and 10 cross-validation folds. In panel (A.10a) each point represents the estimated slope of the prior beliefs (pre-visit) on these ratings. In (A.10b) we repeat the exercise with posterior beliefs (post-visit). In both exercises, the outcome and the ratings are measured on different samples of participants. Standard errors are clustered at the neighborhood level in all regressions.

A.8 Procrastinating Exploration: A Back of the Envelope Calculation

In this section we illustrate how the perceived first-visit cost may lead some naive quasi-hyperbolic discounting agents to never visit neighborhoods that offer a persistent stream of benefits.

The agent incorrectly believes they will not be present biased tomorrow and will explore then, as long as exploration makes sense for an exponential discounter. This leads them to compare the cost of exploring today versus tomorrow. This can cause small costs to lead to forgone opportunities via procrastination (O'Donoghue and Rabin 1999).

To illustrate, we conduct a back of the envelope calculation. Suppose, conservatively, that the entire 119 KSh exposure effect estimated in Table 2 is a one-time cost. Suppose the agent believes that if they explore a new neighborhood their expected utility will persistently increase by y, but they must pay our estimated one-day cost of 119 KSh. They only choose to explore today rather than postponing to tomorrow if

$$\begin{aligned} y - 119 + \beta \sum_{t=1}^{\infty} \delta^t y &> \beta\delta(-119) + \beta \sum_{t=1}^{\infty} \delta^t y \\ \Leftrightarrow y &> (1 - \beta\delta)119 \end{aligned}$$

Given estimates of $\beta\delta$ from a recent meta-analysis, our agent will procrastinate unless y is larger than 22 KSh per day (Cheung et al. 2026).[45] For example, this is the value of commuting 0.7 km closer every day in the future, which is 9 percent of the mean distance to participants' farthest work or search location. (Table A.1).

This 22 KSh increase per day is for a guaranteed stream of benefits. However, agents additionally face uncertain benefits. For example, participants report only finding jobs half the days that they search (see Table A.1). This implies the same cost could sustain an even larger wage difference between neighborhoods. For example, with a 50% job finding probability the agent procrastinates unless they expect wages, conditional on finding a job, to be 44 KSh higher per day, or an almost 9% increase in the median daily wage.

A.9 A Two-Self Sequential Model with Memory Costs

We next set up a simple model of the process by which respondents list neighborhoods in our open elicitation task. The model includes preferences over jobs similar to those outlined

[45] We use the β estimate for tasks involving the timing of effort (0.815). The authors find less present bias for money-now-money later tasks (0.91) and more present bias for other non-money choices like food (0.651). For all tasks, daily $\delta \approx 1$.

previously in equation (1), but also leaves room for recall costs that may differ based on neighborhood characteristics.

Each respondent has two selves. The "memory" self has access to all neighborhoods and their utilities, but for each neighborhood faces a cost to transmit this neighborhood to the "action" self. The memory self optimally chooses which neighborhood to transmit. The action self simply tells the surveyor the neighborhood that they receive.

The memory self for agent i knows preferences

$$u_{in} = \alpha^U \mathbf{X}_{in} + \varepsilon_{in}$$

for each neighborhood n where $\mathbf{X}_{in}$ is a vector of neighborhood characteristics, including baseline familiarity and training indicators, and a constant term. We use exactly the same specification as in equation (1) except that we use more compact notation, and we also include a constant term. We assume that ε_{in} is drawn from a Gumbel distribution with scale equal to 1, and we normalize the utility of not working to $u_{i0} = \varepsilon_{i0}$.

For the k-th neighborhood to be ranked, the memory self incurs (negative) transmission cost

$$c_{ink} = \alpha^C \mathbf{X}_{in} + \nu_{ink}$$

where ν_{ink} are iid shocks with standard deviation = $\sigma_\nu \geq 0$. ($\mathbf{X}_{in}$ is the same as above and includes a constant term.) We normalize the memory cost associated with stopping, that is, not ranking any other neighborhoods, to $c_{i0k} = \nu_{i0k}$.

The neighborhood ranked k-th on the list carries weight $\lambda_k \leq 1$. Neighborhoods further down the list are less likely to be relevant as a previous neighborhood is likely to have a job available. Given that in reality we allow each neighborhood to have a job available with probability 0.25, the objective weights are $\lambda_k = \frac{1}{4} \cdot \left(\frac{3}{4}\right)^{k-1}$. However, we do not impose this.

We further assume that at step k when the memory self is asked to transmit a neighborhood not already included, they act myopically and send the neighborhood[46]

$$n_k^* \in \arg \max_{n \neq n_j^*, j<k} \lambda_k u_{in} + c_{ink} \quad \text{or "stop" (with assumed net utility } \lambda_k \varepsilon_{i0} + \nu_{i0k}).$$

Neighborhoods with higher c_{ink} are easier to recall and hence more likely to be transmitted.

We estimate this model using maximum likelihood based on the first K options ranked

[46]The model where the memory self is fully forward looking and optimizes over the entire list poses significant complications due to the combinatorial nature of that problem.

by the participant (or fewer if they stopped earlier), varying $K \in \{5, 10, 15, 20\}$. We fix the preference parameters over baseline familiarity, distance, training, training in an unfamiliar location, and other job attributes to the values we estimated using binary choices and a logit model (Table 2, column 3). We estimate the subjective job success probability (λ), all the cost coefficients and the standard deviation of idiosyncratic cost shocks.

Table A.30 reports the results for the top five neighborhoods listed on the first day. We estimate that baseline familiar neighborhoods are easier to remember, with a memory cost coefficient of 2.55. One way to interpret this magnitude is by comparing it to the disutility incurred for each kilometer of distance the agent must travel. We estimate that our subjects act as if remembering an unfamiliar neighborhood is as costly as having to travel more than 10 km for sure (that is, with $\lambda_k = 1$). Having trained in a neighborhood has a large effect on memory costs, with a coefficient of 2.03, while the additional effect of having trained in a neighborhood that was unfamiliar at baseline is also positive and significant but smaller, with a coefficient of 1.10 and a bootstrapped 95% confidence interval of $[0.39, 2.16]$. Distance also matters (-0.4 for memory costs compared to -0.24 for preferences). We estimate a relatively high memory cost standard deviation $\sigma \approx 2$, and a subjective probability λ of around half that a given neighborhood has a job on a given day (compared to the true $\lambda = 0.25$). The relative magnitudes of the first four coefficients are broadly stable to changing the specification, the number of ranked choices used in estimation, and the day when choices are elicited (Tables A.31 and A.32).

While these results depend on several assumptions, overall, they suggest that above and beyond the fact that people dislike working in unfamiliar neighborhoods, they also seem less likely to consider these places as options in the first place.

References for the Appendix

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Table A.30: Estimates of Memory Costs from "Open" Elicitation

	Utility (α^U)	**Cost** (α^C)
Baseline Familiar	0.85	2.55
		[1.68, 4.05]
Training	0.29	2.03
		[1.34, 3.15]
Training × Unfamiliar	0.83	1.10
		[0.39, 2.16]
Distance	-0.24	-0.40
		[-0.62, -0.29]
Constant		2.54
		[1.23, 5.49]
Cost variance σ		1.99
		[1.40, 2.92]
Subjective λ		0.50
		[0.36, 0.67]
N		54,155
Participants		391
Top K choices		5

Notes: This table reports maximum likelihood estimates of the two-self memory model from section A.9. We fix the preference parameters over baseline familiarity, distance, training, training in an unfamiliar location, and other job attributes to the values we estimated using binary choices (Table 2, column 3). We estimate the subjective job success probability (λ), all the cost coefficients and the standard deviation of idiosyncratic cost shocks using the ranked neighborhood data from the "open" preference elicitation on the first day of employment. We use the first (up to) 5 ranked neighborhoods, including the decision to stop ranking. At step k, the memory self chooses the neighborhood (among those not yet chosen) with the highest $\lambda_k u_{in} + c_{ink}$. Utility u_{in} includes a $Gumbel(0,1)$ shock, and memory costs c_{ink} include a $Gumbel(0,\sigma)$ shock. We parametrize $\lambda_k = \lambda(1-\lambda)^{k-1}$ and estimate the subjective job probability λ. Individual-level bootstrapped 95% confidence intervals in brackets. Tables A.31 and A.32 report results varying the specification, the number of ranked choices used in estimation, and the day when choices are elicited.

Table A.31: Estimates of Memory Costs from "Open" Elicitation: Different Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	1.88	2.27	2.55	0.88	0.62	0.58
	[1.38, 2.45]	[1.76, 3.00]	[1.68, 4.05]	[0.70, 1.08]	[0.49, 0.75]	[0.48, 0.69]
Training	1.50	1.72	2.03	0.89	0.67	0.64
	[1.11, 2.00]	[1.28, 2.30]	[1.34, 3.15]	[0.73, 1.09]	[0.52, 0.86]	[0.53, 0.77]
Training × Unfamiliar	0.80	1.02	1.10	0.37	0.28	0.26
	[0.20, 1.39]	[0.42, 1.73]	[0.39, 2.16]	[0.15, 0.57]	[0.11, 0.44]	[0.08, 0.42]
Distance	-0.29	-0.33	-0.40	-0.14	-0.10	-0.09
	[-0.36, -0.22]	[-0.40, -0.27]	[-0.62, -0.29]	[-0.17, -0.11]	[-0.12, -0.08]	[-0.10, -0.07]
Constant			2.54	-0.63	-0.81	-0.83
			[1.23, 5.49]	[-0.78, -0.41]	[-0.93, -0.67]	[-0.92, -0.72]
Cost variance σ	1.48	1.70	1.99	0.86	0.67	0.64
	[1.19, 1.91]	[1.41, 2.10]	[1.40, 2.92]	[0.73, 1.03]	[0.56, 0.76]	[0.56, 0.70]
Subjective λ	0.39		0.50	0.29	0.25	0.24
	[0.27, 0.48]		[0.36, 0.67]	[0.25, 0.33]	[0.21, 0.28]	[0.21, 0.26]
N	54,155	54,155	54,155	54,155	54,155	54,155
Top K choices	5	5	5	10	15	20

Notes: This table replicates the MLE estimates from Table A.30 varying the specification and the number of ranked positions used in estimation $K \in \{5, 10, 15, 20\}$.

Table A.32: Estimates of Memory Costs from "Open" Elicitation: Days 2 and 3

	Cost (α^C)		
	(1)	(2)	(3)
Baseline Familiar	2.55	3.09	2.94
	[1.68, 4.05]	[1.82, 6.19]	[2.02, 42.53]
Training	2.03	2.83	3.11
	[1.34, 3.15]	[1.61, 5.58]	[1.99, 38.93]
Training × Unfamiliar	1.10	1.46	1.00
	[0.39, 2.16]	[0.38, 3.73]	[0.17, 13.86]
Distance	-0.40	-0.66	-0.83
	[-0.62, -0.29]	[-1.33, -0.39]	[-9.90, -0.54]
Constant	2.54	6.37	8.35
	[1.23, 5.49]	[2.74, 15.25]	[5.06, 158.91]
Cost variance σ	1.99	2.60	2.89
	[1.40, 2.92]	[1.57, 4.95]	[2.02, 34.63]
Subjective λ	0.50	0.55	0.62
	[0.36, 0.67]	[0.31, 0.85]	[0.00, 0.73]
Sample	Day 1	Day 2	Day 3
N	54,155	54,000	53,355
Top K choices	5	5	5

Notes: This table replicates the MLE estimates from Table A.30 using data from days two and three of the open elicitation.