The Effect of Exposure: Evidence from Spatial Choices in Nairobi*

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Abstract

How much do people dislike opportunities they have never been exposed to, and why? We study how exposure affects work location decisions of casual workers. We offer short-term employment and randomize training locations to induce novel exposures. Participants sacrifice 22% of the median daily wage to avoid working in a location never visited before; one hour-long visit eliminates this premium. Workers anticipate two thirds of the effect exposure has on their later preferences. Results are most consistent with perceived fixed costs of exposure rather than sorting or quality uncertainty. Unfamiliar neighborhoods are also less likely to enter workers' consideration sets.

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1 Introduction

Economists typically assume that past exposure to an opportunity shapes choices because exposure reveals information, such as the unobserved quality of a product (Milgrom and Roberts 1986). However, evidence from psychology and marketing suggests that exposure to an option leads people to like it more and increases its salience (Zajonc 1968; Bornstein 1989; Montoya et al. 2017; Goldstein and Gigerenzer 2002; Gigerenzer and Goldstein 2011; Schank 1982). In this paper, we study how prior exposure affects economic decisions.

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 opportunities (jobs, schools, goods and services), and this is particularly relevant in low and middle-income countries where cities are key drivers of economic opportunity (Bryan, Glaeser, and Tsivanidis 2020).[1]

Standard economic models assume that if an opportunity exists and people know about it, they will choose it when it maximizes their utility. Yet substantial evidence suggests people do not take full advantage of urban opportunities (Lall, Henderson, and Venables 2017; Larcom, Rauch, and Willems 2017; Pelnik 2024) and that migration costs must be large to explain low migration even when it would be highly profitable (Kennan and Walker 2011; Bryan and Morten 2019; McKenzie 2024). Thus, understanding spatial choices has large welfare implications, especially in low-income settings.

Why might exposure matter for spatial choices? First, exposure may reveal information about the utility of being in a given neighborhood, such as its safety and amenities. Second, people may find it difficult to visit a neighborhood for the first time, due to navigation, risk of getting lost, or heightened alertness, which we model as a fixed (one-time) cost of exposure. Third, people may have miscalibrated beliefs about unfamiliar neighborhoods, which exposure may change. Finally, exposure may increase the likelihood a neighborhood is retrieved from memory as a potential choice.[2]

Understanding the effect of exposure requires measuring existing familiarity patterns, observing how choices change in response to novel exposures, and generating experimental variation to distinguish between mechanisms. To do so, we proceed in five steps. First, we document substantial gaps in individuals' exposure to nearby neighborhoods in Nairobi. Second, by making short-term job offers, we estimate the revealed cost of unfamiliarity,

[1] Our focus on the role of prior exposure complements work that focuses on how access to opportunities depends on quick, convenient, cheap, and safe transportation (Borker 2024; Davis 2021; Tsivanidis 2023).

[2] Being in a location is a vivid experience relative to other forms of exposure (e.g., seeing a product in a supermarket), and evidence from neuroscience and urban studies suggests that people build mental representations of space based on their past experiences (Lynch 1960; O'Keefe and Dostrovsky 1971; Fyhn et al. 2004).

how it changes with a single exposure, and whether participants anticipate how exposure will change their preferences. We also measure beliefs about neighborhoods and how these change with exposure. Third, we use a model of a Bayesian expected utility maximizer to distinguish between the first three channels described above, and look for evidence in the data. Fourth, we study how past and experimental exposure affect the formation of consideration sets when subjects choose where to work in an open-ended elicitation. Finally, we measure the persistence of the exposure effects after the experiment.

First, we document substantial gaps in participant's past exposure to neighborhoods near their homes. In our sample of casual workers living in poor neighborhoods, 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. The familiarity patterns also appear inconsistent with sorting based on neighborhood quality. Almost all neighborhoods have intermediate levels of average familiarity, and match-specific observables do not predict familiarity.

Second, we quantify the revealed costs of working in an unfamiliar neighborhood by offering short-term employment opportunities to measure air quality in different neighborhoods across Nairobi. This task requires being physically present in a specific location and plausibly justifies assignments to a variety of locations.

To induce 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. Both visit three neighborhoods over three days.[3,4]

To isolate the effects of exposure on preferences, separately from consideration, we present half of the participants with a series of job choices in the three days following training. The air pollution monitoring task is held fixed across all job offers, but we randomly vary job location, duration, and compensation. We randomly select one of the offer pairs and assign the option the participant chose as their job for the day.

Our first main result is that participants have a strong preference for working in familiar locations. We define the "familiarity premium" as the revealed preference for working in

[3]In order to avoid experimenter demand effects of exposure, neither participants nor the guides who led the training were informed about the treatment assignment.

[4]We choose potential treatment and control neighborhoods for each individual. This provides us with a clear counterfactual training assignment for each participant.

a neighborhood the participant has previously visited, holding other attributes constant, and find it is large. Participants will travel an additional 3.5 km of distance, work over an hour longer, or sacrifice 112 Ksh of salary (22% of the median daily 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 more likely to be unfamiliar. Overall, this baseline familiarity premium suggests that individuals may dislike visiting unfamiliar locations, but may also reflect unobserved, heterogeneous neighborhood preferences (sorting).

Our second main result is that a single exposure is sufficient to completely eliminate this familiarity premium. We find that above and beyond the main effect of training, being randomly assigned to visit an unfamiliar neighborhood once is equivalent to increasing the wage by 109 Ksh or bringing the neighborhood 3.4 km closer, fully offsetting the baseline familiarity premium.[5] We find some evidence that a similar pattern holds for beliefs, which we measure at baseline and after training. Participants' beliefs about labor market opportunities and safety are more pessimistic about unfamiliar neighborhoods before exposure, and a single exposure eliminates the difference relative to familiar neighborhoods.

Our third result is that participants partly anticipate that exposure will make them more likely to be willing to work in that neighborhood in the future. We implement this test using binary job choice questions during training days to elicit preferences over jobs three days later. Participants make these choices *after* learning where they will train on that day but *before* actually visiting the training neighborhood. On average, subjects make choices as if the exposure effect will be approximately 62.9% (CI $[29\%, 100\%]$) as large as what we estimate using choices they make after exposure.

In the third step of the paper, we evaluate the preferences channels for the effect of exposure on work location choices.[6] We interpret our results through the lens of a benchmark model of a Bayesian agent who maximizes expected utility. We allow the agent to be risk-averse over neighborhood attributes and model exposure as revealing information about the quality of a neighborhood, following previous work on "experience goods" (Milgrom and Roberts 1986; Ackerberg 2003). We also allow the agent to incur a (one-time) fixed cost when they are exposed to a neighborhood for the first time.[7,8] This fixed cost may include

[5]To estimate the main effect of training in a neighborhood, we use the fact that control participants are trained in baseline familiar neighborhoods, which we compare to a different set of familiar neighborhoods not visited during training.

[6]We return below to how familiarity and exposure may affect consideration, holding preferences fixed.

[7]The short duration of exposure during training rules out mechanisms such as new network links and habit formation.

[8]We make no additional restrictions of how familiarity emerges in the first place. For example, our model allows that agents choose which locations to become familiar with based on noisy signals.

longer travel times, safety risks when visiting a location for the first time, or greater effort needed to process a novel neighborhood's stimuli. We start by assuming the agent has well-calibrated priors and later relax this.

We prove two results under these benchmark assumptions. First, the average baseline familiarity premium equals the fixed cost plus a sorting term. The sorting term captures whether the utility costs of working in familiar and unfamiliar neighborhoods differ on average. Second, the exposure effect – the average effect of a single exposure on the utility of working in a neighborhood – equals the value of the fixed cost. This follows from the law of iterated expectations. Absent fixed costs, the expected utility elicited ex-ante is the same as the average of utilities elicited after exposure, when some or all of the uncertainty is resolved. In this model, neither learning about neighborhood quality nor risk aversion can generate an exposure effect.[9]

Given these two results, our empirical findings that the baseline familiarity premium and experimental exposure effects are roughly equal also rule out sorting. That is, participants on average do not appear to sort into becoming familiar with neighborhoods that they are more willing to work in during our experiment. This is consistent with the idiosyncratic patterns of past exposure documented in the first step above, and implies the strong aversion to unfamiliar locations is due to fixed costs of exposure.

Further, we consider whether neighborhood quality tail risk and the finite nature of our data can explain the exposure effect. One potential explanation is that unfamiliar neighborhoods have a very low risk of being (persistently) extremely bad quality, and this realization did not occur in our finite sample of exposure events.[10] This is highly unlikely to explain our results, because this type of tail quality would also need to be easily recognized based on a single visit, and be specific to an individual's experience of the neighborhood. This seems implausible. Moreover, when we restrict the analysis to unfamiliar neighborhoods that respondents confidently believe are safe – a third of all initially unfamiliar neighborhoods – we still find a large familiarity premium and a similarly sized exposure effect.

We next consider whether relaxing the assumption that participants have well-calibrated beliefs about neighborhood quality can explain our results. This is consistent with our finding that individuals hold overly pessimistic priors about labor market and safety in unfamiliar neighborhoods, and that they update positively after exposure.

However, miscalibrated beliefs about neighborhood quality alone cannot account for the

[9] We take this result to the data in a specification without neighborhood fixed effects, so as not to condition on the realized (true) value of the utility for a given neighborhood. Our results are very similar with or without neighborhood fixed effects, consistent with an exposure effect driven by a fixed cost.

[10] Our study includes 600 training sessions where participants were exposed to unfamiliar neighborhoods, so bad quality realizations must be very rare to not appear in our sample.

full magnitude of the exposure effect. As described above, we find that participants partly anticipate the benefits of exposure: when making job choices after learning their training location but before visiting it, they behave as if the exposure effect will be approximately two-thirds as large as what we estimate using post-exposure choices. If the exposure effect were driven solely by incorrect beliefs about neighborhood quality, the announcement of future exposure should not change choices because it does not affect the information available at the moment the choice is made.[11] In contrast, if fixed costs are the mechanism shaping choice, announcing the training location informs agents that by the time the employment day arrives, they will have already paid any fixed costs. Thus, our anticipation results suggest that uncertainty about neighborhood quality is unlikely to explain our entire effect, even when allowing for miscalibrated priors about neighborhood quality.[12,13]

Given that both the benchmark model and the anticipation results imply the premium we observe cannot be explained fully by neighborhood quality, sorting, or miscalibrated beliefs about neighborhood quality, we next consider what types of fixed costs of exposure could explain our results.

First, we find little evidence of first-time *navigation* costs. Our willingness to pay results discussed earlier show that all else equal, participants are willing to work in a familiar neighborhood over an unfamiliar one even if it means traveling 3.4 km farther or working more than an hour longer. Thus, in order for navigation costs to explain the premium, we would expect travel times to differ by similar magnitudes. Instead, using a Heckman selection correction model with the other randomized job attributes as instruments, we find small differences in travel times based on baseline familiarity and no effect of the exposure treatment on travel time.

Second, we consider the role of *exploration risk* when participants visit a neighborhood for the first time.[14] First, the experiment entails minimal risk: employment and compensation

[11]This argument applies to a wider class of models where agent update their beliefs about neighborhood quality only in response to new information, including other forms of nonstandard beliefs and preferences such as ambiguity aversion (Gilboa and Schmeidler 1989).

[12]One potential issue with this approach is if training in a location is interpreted as an endorsement of neighborhood quality. We think this is unlikely to explain our results because respondents are told that the training locations were chosen randomly by a computer. Additionally, during data collection we are offering to employ the individual in all of the locations asked about so it is unclear why training would provide a significant, additional level of endorsement.

[13]Another potential explanation is that workers expect they can later opt out of showing up for work if they learn during training that the neighborhood is worse than their outside option. This is unlikely because participants strongly prefer employment over unemployment. Participants refused the job assigned to them (based on their choices) only 0.35% of the time. Out of 89 participants who chose an unfamiliar neighborhood right before training in that location, and whose choice was later randomly selected to be implemented, only one later refused to go, similar to the overall rate of refusal for all jobs selected before training (five out of 584).

[14]Exploration risk is specific to the first visit and hence acts as a "fixed cost." It is distinct from the risk

are guaranteed, and participants spend only one to two hours in the assigned neighborhood during daylight hours. Second, we do not observe any adverse events in our sample, suggesting that if risks do exist, they are rare. This then implies the risk must be severe, yet we do not find an effect of exposure on getting lost, a plausible precursor for more severe exploration risks. Third, we do find marginally significantly smaller exposure effects when individuals are confident that *traveling to* the neighborhood is safe. This suggests some role for exploration risk. However, even in this sample, we find a strong familiarity premium and exposure effect, suggesting exploration risk is not the entire story. While we do not find evidence that actual exploration risk or navigation costs fully explain the exposure effect, miscalibrated beliefs about these quantities could play a role.

Third, our findings are also consistent with psychological fixed costs like processing fluency (Jacoby and Dallas 1981; Jacoby and Whitehouse 1989; Winkielman et al. 2003). In this account, when experiencing a novel environment individuals may have to exert more effort until they learn which parts of the environment they can ignore. These mechanisms could also be interrelated, for example if having to pay less attention to stimuli in a neighborhood makes it feel safer.

Taken together, our results imply that exposure impacts preferences over spatial choices primarily by removing a fixed cost of first-time visits. We find no evidence that this cost reflects realized navigation or travel costs, and weak evidence for first-time exploration risk. The fixed cost could reflect miscalibrated beliefs about the size of these costs, or psychological and effort costs, such as people being uncomfortable or more alert the first time they visit a place. Our experimental design rules out explanations based on well-calibrated uncertainty about neighborhood quality, but miscalibrated beliefs about neighborhood quality may contribute to the familiarity premium and exposure effect. We find no evidence that the familiarity premium reflects sorting on the true utility of working in the neighborhood.

In our fourth step, we consider 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; Iyengar and Lepper 2000; Conlon and Mortimer 2013; Phillips, Morris, and Cushman 2019; Bordalo, Gennaioli, and Shleifer 2020). Familiarity and new exposures may shape these consideration sets by increasing the likelihood a neighborhood is retrieved from memory as a potential option.

To study the impact on consideration sets we use an "open" elicitation for half of the sample where subjects are told that there are jobs available in different neighborhoods in Nairobi, and asked to tell the surveyor where they would most like to work, followed by

that the neighborhood is of persistently bad quality, which we discussed previously.

where they would next like to work, and so on. Subjects are told that we will assign them to the highest neighborhood on their list where a job is available.[15] Unlike our "preferences" elicitation, these participants must generate potential work locations, allowing us to study how exposure affects the formation of consideration sets.

Using this data, we first analyze the predictors of whether a neighborhood is mentioned in the list.[16] 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. We also estimate a model with both preferences and consideration costs, and find that baseline familiarity, the training, and exposure all significantly affect consideration costs. Our results show that people struggle to consider unfamiliar neighborhoods as potential places to work. Thus, familiarity and exposure can 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 offer new paid opportunities two to four months after the intervention and measure whether participants take them up. Baseline neighborhood familiarity strongly predicts show-up all else equal, and the experimentally induced exposure is positive, roughly two thirds in magnitude, but imprecisely estimated because of ceiling effects at high wages.[17] Third, based on endline phone and SMS surveys, training in a location leads participants to return there to search for work, fun/leisure, and errands. While this effect is not differential between baseline familiar and training neighborhoods, participants returning to previously unfamiliar neighborhoods because of the experiment suggests that participants value being exposed to these neighborhoods.

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 (Milgrom and Roberts 1986; Scharfstein and Stein 1990; A. V. Banerjee 1992; Eyster and Rabin 2010; Hanna, Mullainathan, and Schwartzstein 2014; Mal-

[15] We view this method as a bridge between methods that elicit preferences from explicit choice sets and methods that measure what is top of mind more directly (Haaland et al. 2024).

[16] 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.

[17] These results hold for wage offers $\leq$ 500 KSH. For wages $\geq$ 600 KSH, show-up is flat around 80% and none of the job and neighborhood attributes, including distance or wage, predicts show-up. When the ceiling effect on show-up was detected during field work, we lowered the entire wage distribution. For completeness, we present the full set of results, but focus on the wage levels where job offer attributes affect decisions and the rounds with the reduced wage offer distribution.

mendier 2021; Foster and Rosenzweig 1995; Weitzman 1979) by demonstrating additional possible effects of exposure in addition to resolving uncertainty. Our work is also related to a large literature in marketing that studies the mechanisms and effects of advertising (e.g. Shapiro 2018; Shapiro, Hitsch, and Tuchman 2021; Keller 1987; Milgrom and Roberts 1986; Kihlstrom and Riordan 1984; Nelson 1970; Nelson 1974; Sahni and Nair 2020; Ackerberg 2003; Ackerberg 2001). Our results imply that experience may be important for economic choices, even when there is no potential uncertainty about the quality of an opportunity. Future work should assess whether similar exposure effects are important in other economic domains such as occupation choice or technology adoption.

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; Bornstein 1989; Montoya et al. 2017). This literature predominantly focuses on repeated and controlled exposure for short durations (including subliminal exposures) to a variety of specific stimuli in the laboratory. We extend this literature by focusing on a longer duration, single exposures to holistic environments. We also relate to the literature demonstrating an aversion to unfamiliar individuals, and that individuals can fail to anticipate how much they will enjoy conversing with strangers (Allport 1954; Epley and Schroeder 2014; Schroeder, Lyons, and Epley 2022). We extend this literature by examining how prior exposure affects spatial preferences. 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 (Schank 1982; Simon and Gilmartin 1973; Mack and Palmeri 2024; M. K. Ho et al. 2022; Thalmann, Souza, and Oberauer 2019; Bordalo, Gennaioli, Lanzani, et al. 2025). 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 (Duranton 2015; Chauvin et al. 2017; Lall, Henderson, and Venables 2017; Bryan, Glaeser, and Tsivanidis 2020), complementing previous work that measures and unpacks transportation costs in these contexts for different groups (Borker 2021; Kreindler et al. 2023; Grosset-Touba 2024; Tang 2024; Jalota and L. Ho 2024; Cook, Currier, and Glaeser 2024), and research on policies that reduce transportation costs (Habyarimana and Jack 2015; Gonzalez-Navarro and Turner 2018; Tsivanidis 2023; Balboni et al. 2021; A. Banerjee and Sequeira 2023; Franklin 2018; Abebe et al. 2021; Field and Vyborny 2022). Policies to encourage exposure may be additional tools, beyond reducing access costs, to increasing effective access to opportunities. Indeed, in our setting, a fifth of the utility cost of distance in job choice is in fact due to the aversion to unfamiliar locations.

The exposure effect we demonstrate here offers a possible micro-foundation for several results showing people are reluctant to explore new locations or travel routes and persistence when people are exposed to such locations (Bryan, Chowdhury, and Mobarak 2014; Larcom, Rauch, and Willems 2017; Okunogbe 2024; McKenzie 2024; Brady and McNulty 2011). It also may help explain why economic benefits may not equalize across space. For example, in recent work in Kampala, Pelnik (2024) uses new data and an experiment to show that small entrepreneurs can earn substantially different profits in different neighborhoods. Our paper is particularly relevant for recent work that proposes that the aversion to rural to urban migration falls with experience in the city (Lagakos, Mobarak, and Waugh 2023). The exposure effect is also an additional type of spatial friction, complementing work that studies information frictions in commuting, migration and trade (Vitali 2024; Baseler 2023; Wiseman 2023; Porcher, Morales, and Fujiwara 2024).

2 Participant Sample: Casual, Underemployed Workers

We recruited study participants in three low-income neighborhoods of Nairobi (Kibera, 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 canvasing exercise, and participants who passed an initial filter were later invited to a nearby study venue for two days of additional surveys. The two surveys included demographic and employment questions, urban familiarity questions (discussed in section 3.1), and several measures of spatial ability. In order to limit selection into the study sample based on willingness to explore, each of the three home neighborhoods where we recruited had its own study venue located within the neighborhood.

A canvased participant was eligible for the study if they were older than 18 years old, lived in one of our three study neighborhoods, were casual workers actively searching for work and stated that they were available to work every day for the next 7 days.[18] Participants who did not show up for the two initial surveys at the study venue within a day of the invitation date were excluded from the sample. We also excluded from the experimental sample some participants with very high or very low familiarity of Nairobi, based on their responses in the first baseline survey, because we could not randomize them appropriately to the familiar or unfamiliar training locations.

We collected information from 1704 participants during the in-person recruitment. Of

[18] 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.

these participants, we invited 1600 who were eligible to participate in a baseline survey. Among these eligible participants, 1168 attended the first baseline survey, where 915 were eligible for the study intervention and thus invited to a second baseline survey. Of the 915 eligible at Baseline 1, 831 attended Baseline 2, and 799 of them began the first training day and hence constitute our experimental sample.

Table A.1 reports descriptive stats for the experimental study sample. 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 carpenter, mason, 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.

3 Neighborhood Familiarity Patterns

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.

3.1 Measuring Familiarity

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 which we sample. 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.[19]

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 shortest) 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. Figure A.2 plots the set of neighborhoods we ask about for each home neighborhood.

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 questions for each neighborhood:[20]

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.

3.2 Neighborhood Familiarity Patterns

We begin with a descriptive analysis that shows that participants in our sample have significant "spatial familiarity gaps." We focus on familiarity patterns for neighborhoods that are at most 75 minutes away from the respondent's home neighborhood. Figure 1a displays the CDF for our main measure of familiarity. It shows that the median participant has never visited around half of the 30-33 neighborhoods in the sample. A quarter of participants have visited less than 40% of the neighborhoods.

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 reports results for the sample of neighborhood within 75 minutes (like in Figure 1a), and the last three columns further restrict to only neighborhoods within 8 kilometers of the participant's home neighborhood, which is the mean maximum commuting distance that participants report who travel outside of their home neighborhood in order to work or search for work.

[19] Appendix A.5 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 have been 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).

[20] This design ensures that each participant answers the same number of questions regardless of their answer to the first question, which avoids incentives to misreport in order to change the duration of the survey.

The table highlights several results. First, participants have heard of almost all of the neighborhoods. The average rate is 92% and the median participant has heard of 94% of neighborhoods within 75 minutes. These numbers are slightly higher for the second (smaller) sample of neighborhoods. This shows that unfamiliarity is not driven by confusion about the names we use to refer to neighborhoods.

Second, familiarity is low, and even lower for more demanding definitions of familiarity. Focusing on the second column, the median participant reports ever having been to *or passed by* 63% of the neighborhoods, and ever been to 52% (Figure 1a uses this definition). The median respondent only has ever been and knows how to reach 43% of neighborhoods and can name a landmark for only 34% (around 13 and 10 neighborhoods, respectively). We later show in Section 5.1 that these more demanding measures are strong predictors of participants' willingness to work in a neighborhood.

To provide a more granular view of familiarity patterns, Figure 1b displays the levels of familiarity for each participant from Kibera. Each row is one of the 30 neighborhoods and each column is a respondent, with both axes sorted by average level of familiarity. A cell is grey if the respondent has been to the row neighborhood and black if they have not.

The key takeaway is that there is a significant amount of seemingly idiosyncratic variation in familiarity patterns. There are few neighborhoods that are very familiar or very unfamiliar, and few participants who know most or none of the neighborhoods. This suggests that familiarity patterns are not driven by consensus views of neighborhood attributes.

To evaluate whether this variation is driven by individual-neighborhood match characteristics, we fit a series of four random forest models that flexibly predict whether individual i is familiar with neighborhood j. Figure 1c presents ROC curves and the area under the ROC curves, which can be interpreted as the probability that the model correctly ranks which of two individual-neighborhood pairs is more likely to be familiar than another. The standard gravity predictors of neighborhood fixed effects and distance from home neighborhood do significantly better than a chance ranking of 0.5, giving an 86% chance of correctly ranking the pairs. We then add individual characteristics, neighborhood characteristics and both at the same time. These additions (and their interactions allowed by the random forest) do nothing to improve our accuracy.[21] As far as we can observe, individual-neighborhood match characteristics are not important drivers of familiarity beyond distance and destination neighborhood fixed effects.

To understand more about what types of people are more likely to have higher levels

[21] We also analyze whether Nairobi residents partially sort to destination neighborhoods based on ethnicity. Table A.4 studies whether the respondent sharing an ethnicity with the plurality of the population of a neighborhood is an important driver of familiarity. While shared ethnicity alone is predictive, this effect vanishes after controlling for distance.

of familiarity, in Table A.5 we regress the average level of familiarity at the individual-level on several demographic indicators and an index of spatial ability. We find that men, those that are older, those with more years of education or years living in Nairobi, and those with higher spatial skills have greater average levels of familiarity. In general this heterogeneity is meaningful, but not overwhelming. For example, men are 0.3 SD more familiar than women on average and an additional 10 years in Nairobi is associated with an increase of 0.1 SD.

4 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 (2024) (Figure A.3), that 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.[22] 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 one to 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.

Participant Timeline and Randomization. Figure A.4 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, participants provide demographic information and answer the familiarity questions discussed in section 3.1. On the second day, participants return to the study venue and complete the second baseline survey, which collects more demographic data, data on networks, more detailed employment data, data on self-reported spatial ability, and beliefs about the labor

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

market and safety of neighborhoods. Participants who complete this second survey and show up the next day form the analysis sample for the experiment.

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 get trained. The participants returns 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. 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 they begin training. We discuss this study component in section 7. Two to four months after the training, they receive invitations for a separate survey in various neighborhoods. We analyze their show-up decisions to these invitations in section 7.

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 the different groups.

We select 6 familiar neighborhoods and 4 unfamiliar neighborhoods for each person.[23] We then randomly designate 3 familiar and 3 unfamiliar neighborhoods as "main" familiar and "main" unfamiliar neighborhoods, respectively. Figure A.5 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. The remaining *unfamiliar* neighborhood allows us to identify

[23] Recall from Section 2 that we exclude participants from the sample who do not have 4 unfamiliar and 6 familiar neighborhoods.

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.6 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.[24] 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 (in case they need to return there). Once in the neighborhood, they instructed the participant how to collect data (stay outdoors, stay in vicinity of major roads, stand at the same location or move around as they prefer, and how to use 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 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 "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.[25] On average, 99% of

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

[25]We re-measure familiarity when participants return from the training to the study venue. Table A.10 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.

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.9).[26]

Eliciting Job Preferences and Consideration. We use two different methods to elicit jobs choices during the three employment days that follow the training period, and randomize the method across participants.

For half of the participants, we use a "preferences" elicitation method (Mas and Pallais 2017). We use binary choices where individuals choose between two potential jobs given the neighborhood where the job takes place, duration, wage, etc. These structured choices allow us to price the disutility of traveling to an unfamiliar neighborhood, because we directly confront the individuals with the possibility of working in unfamiliar neighborhoods.

However, 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.

5 Impact of Neighborhood Exposure: Results

5.1 The Revealed Preference for Familiar Neighborhoods

Job Choice Elicitation. We begin by discussing results for the half of our study sample where we elicit job preferences by offering choices between options. After the three days of training, participants are invited to the study venue for three additional days of employment. 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 job and asks the respondent which one they prefer for that day. 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 one of the questions will be randomly picked and implemented. To ensure and check comprehension, the surveyor first goes through a practice session with the participant.

[26] We show in Appendix A.2 that this small imbalance cannot explain our main results.

Participant responses to these real-stakes questions allow us to estimate preferences 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. We return to the issue of whether unfamiliar neighborhoods are less likely to enter consideration sets in section 6 using our "open" elicitation method.

Job Choice Model and Estimation. We use the binary choices between potential jobs to estimate a random utility model.[27] In order to estimate the impact of our experiment on the familiarity premium of working in an unfamiliar neighborhood, we make the following assumptions:

1. Visiting a familiar neighborhood during the training does not affect the utility of working in any other target neighborhood.
2. Visiting an unfamiliar neighborhood during the training does not affect the utility of working in any familiar target neighborhood.

These two assumptions essentially state that participants have made up their minds about *familiar* neighborhoods. That is, we assume no cross-neighborhood spillovers in the utility of working in a neighborhood involving familiar neighborhoods. Note that this is not a statement on the probability of deciding to work in one of these neighborhoods, e.g. visiting a familiar neighborhood may make it *relatively* more or less attractive than other neighborhoods. Note that these assumptions do not preclude participants in the treatment group becoming more open to unfamiliar neighborhoods in general. [28]

Given these assumptions, the utility of working in *non-visited familiar* neighborhoods is unaffected by either treatment assignment. This provides a stable utility benchmark and allows us to identify both any recency effect due to training (comparing visited and non-visited familiar neighborhoods) and the additional effect of exposure (comparing the effect of visiting a familiar neighborhood to the effect of visiting an unfamiliar neighborhood).

We further parameterize this relationship and assume that the utility of job offer $j \in$

[27]We also allow participants to turn down both jobs. Due to the underemployed sample and the competitive wages, this was extremely rare, only 0.35% of the time. We study extensive-margin decisions for a different type of work opportunities in section 7.

[28]One example of how these assumptions might fail is when a trip to a training neighborhood exposes participants to other target neighborhoods. This is why our algorithm for choosing target neighborhoods is designed to minimize the possibility of these spatial spillovers.

$\{1, 2\}$ in target neighborhood n, for individual i is given by

$$u_{ijn} = \underbrace{\beta^F Familiar_{in}}_{\substack{\text{Baseline}\\ \text{familiarity}\\ \text{premium}}} + \underbrace{\beta^V Train_{in}}_{\substack{\text{Effect of}\\ \text{training}}} + \underbrace{\beta^U Train_{in} \times Unfam_{in}}_{\text{Effect of exposure}} + \underbrace{\beta^D d_{in}}_{\text{Distance}} + \underbrace{\gamma X_{ij}}_{\substack{\text{job}\\ \text{attributes}}} + \underbrace{\epsilon_{ijn}}_{EV(1)} \quad (1)$$

where $Familiar_{in}$ is an indicator for i being familiar with neighborhood n at baseline, d_{in} is distance from the study venue to n, and the set of randomly allocated job covariates X_{ij} includes the wage, duration, whether a portion of the compensation is a risky potential bonus, and any amount paid up front to reduce liquidity constraints. The terms ϵ_{ijn} are idiosyncratic preference shocks drawn from a Gumbel (extreme value type 1) distribution with scale parameter 1.

The term $Train_{in}$ measures whether i was trained in neighborhood n. This includes control participants trained in baseline familiar neighborhoods, and treatment individuals trained in baseline unfamiliar neighborhoods. The term $Unfam_{in}$ is defined as $1 - Familiar_{in}$, so the interaction term $Train_{in} \times Unfam_{in}$ is switched on only for treatment group participants and the (baseline unfamiliar) neighborhoods where they are trained in. Hence, β^V gives the effect of training while β^U provides the additional effect of having been trained in an unfamiliar location, which we call the exposure effect.

We also estimate versions of equation (1) with fixed effects for neighborhood (μ_n) or for home neighborhood by destination neighborhood ($\mu_{h(i)n}$).

Benchmark Model. In order to structure the interpretation of these coefficients we now present a benchmark model for how a Bayesian expected utility maximizer's revealed preferences vary with familiarity and how they would be affected by a one-visit exposure.

Consider an expected-utility maximizing agent i. When they enter the experiment, the set of all neighborhoods $\mathcal{N}$ is partitioned into baseline unfamiliar and baseline familiar such that $F_i \cup U_i = \mathcal{N}$. We assume that the agent i receives utility u_{in} from working in neighborhood n. We assume that the agent knows the utility they would receive if the neighborhood is familiar, and that they have a calibrated prior p_i over u_{in} for unfamiliar neighborhoods.[29] We discuss model predictions with potentially miscalibrated beliefs in section 5.2 and Appendix A.1.

We further assume that exposure (training) in an unfamiliar neighborhood reveals infor-

[29]We do not impose other restrictions on how the sets U_i and F_i emerged. For example, our model covers a situation where previously, the agent decided which neighborhoods to visit based on noisy signals about these neighborhoods. We can also extend the model to allow the prior to be conditional on known characteristics X_n of the neighborhood.

mation about u_{in} and that the agent forms a Bayesian posterior q_{in}. We also allow for a fixed utility cost c_i which the agent pays the first time they visit a new neighborhood. This might capture costly navigation, safety risks during exploration, or a psychological cost of being in a new place.[30]

Thus, in the experiment for a given individual i in location n, our binary choices elicit the following valuation

$$Y_{in} = \begin{cases} u_{in} & \text{if } n \in F_i, \\ \mathbb{E}_{p_i}[u_{in}] - c_i & \text{if } n \in U_i \text{ and } D_{in} = 0, \\ \mathbb{E}_{q_{in}}[u_{in}] & \text{if } n \in U_i \text{ and } D_{in} = 1, \end{cases}$$

where D_{in} is an indicator that person i visited neighborhood n during training. We show our derivations in Appendix A.1.

Our first result is that the coefficient β^F on baseline familiarity from equation (1) identifies a mix of sorting and average fixed cost.

$$\beta^F = \underbrace{\mathbb{E}_i[\mathbb{E}_n[u_{in}|n \in F_i] - \mathbb{E}_{p_i}[u_{in}|i, n \in U_i]]}_{\text{Sorting}} + \underbrace{\mathbb{E}_i[c_i]}_{\text{Fixed Costs}}$$

That is, the baseline familiarity premium identifies the difference in expected utilities between familiar and unfamiliar neighborhoods and the fixed costs of visiting the unfamiliar neighborhoods.

Our second result is the causal effect of exposure is the average fixed cost:

$$\beta^U = \mathbb{E}_i[c_i],$$

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. We discuss tail risk and finite sampling at the end of this section.

We bring this model to the data using the specification (1) without neighborhood effects, so as to not condition on the realized value of average utility in specific neighborhoods.[31]

[30] In the empirical analysis based on equation (1), we also allow training to have an effect β^V across all neighborhoods. For simplicity, in this model we abstract from this effect, although including it would not change our results.

[31] To see why this type of conditioning might be problematic, consider a case where agents have the same preferences $u_{in} = u_n$ and initial familiarity patterns are determined by initial noisy signals of quality. Across neighborhoods, our result still applies and agents should be correct about the utility of working in an

Revealed Preference Results. We estimate equation (1) using a binary logit model, and later show that results are qualitatively unchanged when we use a linear specification.

Table 2 shows the results. The sample includes nine target neighborhoods for each participant, the six main familiar and main unfamiliar neighborhoods, and the three other familiar neighborhoods.[32] The first column omits neighborhood fixed effects. The second column adds home by destination neighborhoods fixed effects, which makes distance drop out. The final column uses neighborhood fixed effects, which allows the distance coefficient to be identified because of variation in distance to a neighborhood based on the home neighborhood. The large overall scale of coefficients indicates that the logit model explains a large share of participant decisions. Results are generally similar across specifications, so here we only discuss the results from the first column.

Our first main result is that participants strongly prefer to work in neighborhoods that they are already familiar with, with a utility effect of 0.85. The magnitude is equivalent to 112 Kenyan shillings (Ksh) of additional compensation, which we obtain by dividing the baseline familiarity coefficient by the expected compensation coefficient (0.76). This *baseline familiarity premium* is a large amount given that the median daily wage outside the experiment is 500 Ksh. Equivalently, participants are willing to travel an additional 3.54 kilometers or work for more than an additional hour to avoid working in an unfamiliar neighborhood.

Our second main result is that the single one-hour long exposure during training is sufficient to erase this familiarity premium. Training in an unfamiliar location increases the utility of working there (0.83), equivalent to increasing the wage by 109 Ksh or bringing the neighborhood 3.46 km closer. This effect is in addition to the general effect of training in a neighborhood, which is also positive but smaller (0.29), and which applies both to familiar and unfamiliar neighborhoods. This exposure effect is indistinguishable from the familiarity premium, and their ratio has a 95% confidence interval of $[0.73, 1.36]$.

Results are very similar with a linear specification (Table A.11). Participants are 8-10 percentage points more likely to pick a familiar neighborhood option over an unfamiliar neighborhood, and experimental exposure has an effect of 9-10 pp. We show in Appendix A.2 that our results cannot be explained by the small amount of differential attrition (3.9%

unfamiliar neighborhood. However, for a given neighborhood – which corresponds to including neighborhood fixed effects – those who are unfamiliar with it had worse realizations of the signal, so will update up on average. The fact that our results are very similar with and without neighborhood fixed effects suggests that homogeneous valuations and sorting on past noisy signals is not the main driver of familiarity.

[32]To test for spillovers, we add the tenth target, unfamiliar neighborhood and estimate a separate coefficient for its interaction with treatment. This neighborhood is unfamiliar and was not included in the training. Table A.20 shows the results. We find a positive yet imprecisely estimated positive spillover to other unfamiliar neighborhoods.

vs 1.3%) during employment days.

Discussion. We first interpret these results through the lens of our benchmark model and then consider potential deviations from the assumptions. In the model, the baseline familiarity premium is a combination of sorting and fixed costs while the exposure effect is entirely fixed costs. The fact that the exposure effect is similar in size to the baseline premium implies that, on average, participants do not have a systematically different utility cost of working in familiar vs unfamiliar neighborhoods. That is, it does not appear that the aversion to unfamiliar neighborhoods we observe is due to sorting on quality.[33]

The large experimental exposure effect indicates that participants on average expect to experience a significant fixed cost when working for the first time in an unfamiliar neighborhood. This rationalizes why they avoid working these neighborhoods unless other job attributes compensate this exposure disutility. After a single visit to the neighborhood during training, this cost is sunk and participants no longer treat these neighborhoods differently compared to those they were already familiar with at baseline. In section 5.2, we look at different types of fixed costs such as navigation time, likelihood of getting lost, or a psychological cost of exposure.

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? (We consider the role of transient risks experienced during first-time exploration below.) If this rare realization does not occur in our finite sample of exposure events, this might explain why we observe participants reluctant to visit unfamiliar neighborhoods but this vanishes after training.

We believe this is very unlikely to explain 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 in initially unfamiliar neighborhoods. Thus the risk would need to be very rare in order for it to not happen in our sample. This in turn implies that, to explain the baseline familiarity premium we estimated, the tail neighborhood quality must be extremely bad. Next, to justify the fact that a short, one-time exposure entirely erases the premium, the bad quality must also be something that is easy to recognize completely based on spending

[33] 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.)

an hour in the neighborhood during training. Additionally, Figure A.6 shows there are no outliers in the distribution of neighborhood fixed effects estimated in column 3 of Table 2. 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.

To further test whether serious neighborhood risk may explain participants' reluctance to work in unfamiliar neighborhoods, we separate our analysis by individual baseline beliefs about safety in an unfamiliar neighborhood. If concerns about tail risk of neighborhood quality are driving our results, we would expect the premiums that we estimate to be concentrated among those who give the neighborhoods low safety ratings or express uncertainty in their responses. 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 results from Table 2 for comparison. We find very similar familiarity premium and exposure effects. These results are not consistent with participants avoiding unfamiliar neighborhoods because of safety concerns.

Overall, we find no evidence that our results can be explained by a calibrated belief that an unfamiliar neighborhood has a very low probability of being a very bad type of neighborhood.

Heterogeneity. In general, we find limited evidence of heterogeneous effects of familiarity and exposure. First, we find similar results when focusing on participants with low or high initial levels of familiarity (Table A.16). Next, we find similar effects of familiarity, both baseline and experimental, regardless of whether the neighborhood is close or far to the participant's home neighborhood (Table A.17). We find that familiarity matters more for men in our sample, but that a single exposure erases the premium for both men and women (Table A.18).

One exception is that we find our baseline familiarity premium effects are stronger for more familiar neighborhoods. In Table A.19, we separate the $Familiar_{in}$ measure in equation (1) based on whether the participant reported at baseline knowing how to get there, or not, and based on whether they were able to mention a landmark in the neighborhood. For both definitions, the familiarity premium is larger when the participant is more familiar with the neighborhood.

5.2 Mechanisms

In this section we consider what mechanisms may drive this effect of exposure. We begin by considering whether deviations from the benchmark beliefs model can explain our results, and then turn to assessing different sources of fixed costs and alternative psychological explanations.

Poorly-calibrated priors. Can deviations from well-calibrated priors explain our results? For example, if individuals hold overly pessimistic priors they will systematically update positively after exposure. This mechanism is closely related to the original affective conditioning explanation for the mere exposure effect (Zajonc 1968).

We collect data on beliefs for the main six 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 collected 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. For each neighborhood, we ask participants about the likelihood of finding a daily or casual work opportunity in that neighborhood, both for the average person in the same home neighborhood as them, and for the respondent themselves. We next ask whether the pay is good conditional on finding a job, whether the trip to the neighborhood is safe, and whether the neighborhood itself is safe. 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 ask how confident the respondent is in their answer. See Appendix A.7 for precise question wording.

For respondent i, a neighborhood n where i was trained, and data collection time $t = 0$ (baseline 2) or $t = 1$ (after training), we estimate:

$$\text{Belief}_{int} = \alpha AfterTrain_t + \beta BeforeTrain_t \times Treated_i + \gamma AfterTrain_t \times Treated_i + \epsilon_{int}. \quad (2)$$

$Belief_{int}$ is the belief or certainty rating from 1 to 5, for one of the five belief outcomes described above. We also report results with home by neighborhood fixed effects ($\mu_{h(i)n}$).

Respondents in the control group are asked about familiar neighborhoods, while those in the treatment group are asked about unfamiliar neighborhoods. We interact the treatment 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 0.25 points lower on their likelihood of finding a causal job in the neighborhood, and 0.25 points lower for the likelihood for an average resident of their neighborhood. We see no differences for wages. Travel safety to an unfamiliar neighborhood is rated 0.44 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 still shrinks substantially. This means that on average, a single in-person visit eliminates the initial imbalance in ratings about these neighborhoods.[34]

Do Subjects Anticipate How Their Preferences Change After Exposure? To further explore the role of beliefs about neighborhood quality as a mechanism while allowing for miscalibrated priors, we assess whether individuals change their choices for future employment after learning they will visit a neighborhood during training.

If the aversion to unfamiliar neighborhoods stems from miscalibrated concerns about neighborhood quality, the training location announcement should not change choices because it does not affect information available when the choice is made. This argument also applies to a wider class of models where agents only change their beliefs about neighborhood quality in response to new information including non-standard updating procedures and ambiguity aversion (Gilboa and Schmeidler 1989). Because the agent's information about the neighborhood's quality has not changed, within this class of models, any choices which are a result of their beliefs about neighborhood quality should also not change.

In contrast, if fixed costs are the mechanism shaping choice, agents should alter their choices. The announcement informs agents that, by the time the employment day arrives, they will have already paid any fixed costs. Thus, before learning the training location, agents should factor these fixed costs into their decisions, but after the announcement they should ignore these fixed costs as sunk.

To test this, we elicited 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 will 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 estimate a similar logit model as previously, but we now include the choices made

[34] For the interested reader, we present additional results on beliefs in Appendix A.8

on training days when individuals knew they were about to visit the neighborhood, but had not yet done so. The utility of working in neighborhood n is given by

$$\begin{aligned} u_{ijn} =& \beta^F Familiar_{in} + \\ & \underbrace{\beta^V Train_{in}}_{\text{training effect}} + \underbrace{\beta^U Train_{in} \times Unfamiliar_{in}}_{\text{exposure effect}} + \\ & \underbrace{\beta^{AV} AnticipTrain_{in}}_{\substack{\text{anticipation} \\ \text{training effect}}} + \underbrace{\beta^{AU} AnticipTrain_{in} \times Unfamiliar_{in}}_{\text{anticipation exposure effect}} + \\ & \underbrace{\gamma X_{ij}}_{\substack{\text{randomized} \\ \text{job attributes}}} + \beta^D d_{in} + \epsilon_{ijn} \end{aligned} \tag{3}$$

The coefficients β^{AV} and β^{AU} capture how participants value training that is about to happen in neighborhood n, in general and the additional effect for unfamiliar neighborhoods, respectively. The corresponding variables are switched on in the choices that participants make on training days, specifically for the neighborhood that will be visited that day. In the benchmark model from section 5.1, agents have well-calibrated priors and anticipate the fixed cost, which predicts $\beta^{AU} = \beta^U = \mathbb{E}_i c_i$. We show in Appendix A.1 that if we extend the model to allow for poorly-calibrated beliefs, β^U becomes the sum of the anticipated fixed costs and the average belief update due to the incorrect priors about neighborhood quality, while β^{AU} becomes only the anticipated fixed costs.

We estimate equation (3) using a binary logit model. Table 4 shows the results. We use the pooled data on training days and employment days. We find $\beta^U = 0.77$ and $\beta^{AU} = 0.48$. Thus, on average, subjects make choices as if the treatment effect will be approximately 64% as large as what we observe, with a 95% confidence interval of $[29\%, 100\%]$. Column 2 shows similar results with neighborhood fixed effects. Finally, in column 3, we only use data from the training period. The advantage of this specification is that all choices made during this period are for a time in the future, whereas in column 1 we also include choices from the employment period which were made for the same day.[35] We find quite similar results with a ratio of $\beta^{AU}/\beta^U = 0.51$. The fact that subjects partially anticipate being more willing to return after visiting, suggests that even with poorly-calibrated priors about neighborhood quality, subjects act as if fixed costs are responsible for approximately half to two-thirds of the baseline premium.

An alternate explanation is that the positive β^{AU} reflects option value. In this scenario,

[35] To identify the effect of training in column 3 we leverage variation in realized familiarity induced by training on previous days.

subjects know that after they do the training, they can later opt out of the task they selected, if they learn that working in the neighborhood is worse than their outside option of not working at all. We think this is unlikely because ex-ante individuals have a very strong preference for employment over unemployment, including in unfamiliar neighborhoods. Respondents only choose to decline to leave for the job assigned to them 0.35% of the time. Among jobs assigned based on choices made during the training days, the refusal rate is five out of 584 overall, and one out of 89 for unfamiliar neighborhood chosen right before training was about to happen there.

Another possibility is that participants viewed their training assignments as endorsement of neighborhood quality. We view this as unlikely for two reasons. First, participants were (truthfully) told their training locations were randomly chosen by a computer. And second, it is not clear why participants would view this as a differential endorsement relative to being offered a job in the location as part of the measurement of our primary outcome.

These results are consistent with subjects who partially anticipate a fixed cost of exploration. The training announcement changes choices because they know that this cost will be incurred during the training. We now turn to assessing different types of fixed costs.

First-time Navigation 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 3.5 km and work for more than an additional hour 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 5 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 randomly allocated 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 greater 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. Table 5 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 not differential by the familiarity status of the neighborhood.

Exploration Risk. Another potential source of fixed-costs is 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 section 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. On the other hand, we do see a (marginally insignificant – p=0.12 to 0.13) smaller premium and effect of exposure in neighborhoods that respondents are confident it is safe to travel to, suggesting some potential role for exploration risk (Table A.15). However, even in this sample 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) suggesting this is unlikely the entire story.

"Psychological" Fixed Costs. We believe our results are consistent with two kinds of "psychological" fixed costs. First, it is possible that participants hold incorrect beliefs about the size of real fixed 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. This could then be consistent with the rapid updating either if individuals learn their beliefs were incorrect or only hold the distorted beliefs for first-time navigation costs.

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 (Jacoby and Dallas 1981; Jacoby and Whitehouse 1989; 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 of other fixed costs. For example, if individuals feel less safe when the stimuli they are processing are not fluent.

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 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, Gennaioli, and Shleifer 2020; Phillips, Morris, and Cushman 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 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, respondent 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 they want to rank, ease of consideration may also play a role.

We begin by interpreting this data using the classic logit consideration model from Goeree (2008). In this model applied to our elicitation, each participant first comes up 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

$$\eta^C_{in} = \alpha^{CF} Familiar_{in} + \alpha^{CV} Train_{in} + \alpha^{CU} Train_{in} \times Unfam_{in} + \alpha^{CD} d_{in} + \mu_n + (\alpha^C)' X_i, \quad (4)$$

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 our choice elicitation both jobs are refused only 0.35% of the time, so we bring this model to the data by assuming 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.

Table 8 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. The fact that we find similar effects on preferences and consideration is consistent with work showing that people are more likely to consider options that have been more useful in the past (Phillips, Morris, and Cushman 2019).[36]

The coefficient α^{CV} that measures the effect of training in a familiar neighborhood has a very large coefficient of around 1.4 log odds points on consideration. This means that training neighborhoods were very top of mind 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.[37] On days 2 and 3 the training coefficient is larger and experimental exposure no longer has an effect on consideration (Tables A.28 and A.29).

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, whereby participants mention neighborhoods one by one and later ranked choices were much less likely to be implemented, because an earlier choice is likely to have a job available. Thus the value of generating additional neighborhoods for consideration depends on the position in the list.[38]

[36]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.

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

[38]Indeed, we find that if we estimate a ranked logit with the lists, the preference rankings become atten-

We now model this sequential process explicitly.

6.1 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 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} has variance 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 neighbor-

uated as we include more elements from the list (Table A.30). This is inconsistent with the ranked logit model, which assumes preference coefficients would be stable, and it is consistent with memory costs being relatively more important for neighborhoods ranked later on the list.

hood, they act myopically and send the neighborhood[39]

$$n_k^* \in \arg\max_n \lambda_k u_{in} + c_{ink} \quad \text{or "stop" (with assumed net utility } \lambda_k \varepsilon_{i0} + \nu_{i0k}).$$

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 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, and training in an unfamiliar location, to the values we estimated using binary choices (Table 2, column 3). We estimate the other parameters, including how participants value the job wage and duration (which vary at the participant level), the subjective job success probability (λ), all the cost coefficients and the standard deviation of idiosyncratic cost shocks.

Table 9 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 2.53. 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. Having trained in a neighborhood has a large effect on memory costs, with a coefficient of 2.02, 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 1.08 and a bootstrapped 95% confidence interval of $[0.37, 2.13]$. Distance also matters (-0.4 for memory costs compared to -0.25 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).

Overall, the results from the open elicitation method show that above and beyond the fact that people dislike to work in unfamiliar neighborhoods, they also seem less likely to consider these places as options in the first place.

[39]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.

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.23 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.[40] 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 the distribution of wages, 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 500KSH, show-up plateaus around 80% (Figure A.7) and none of the neighborhood or job characteristics, including distance and wage, affect show-up. Second,

[40] 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.

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 specifications from Table 2, 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. 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 0.46 to 0.39.[41] Distance and wages also matter for show-up for wages below 500 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 (column 4-6), and when using a linear probability model (Table A.24).

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

Table 7 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.[42,43]

We find that respondents return to initially unfamiliar neighborhoods when they are trained there. This effect is captured by the sum of the "Training" and "Training × Unfamiliar" coefficients, which is positive and significant for most visit types.[44]

Participants return for a variety of reasons, including to search for work, as well as

[41]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).

[42]Table A.26 reports the results without neighborhood fixed effects.

[43]We also collect travel information in less prompted ways in the endline survey and present the results in Table A.27. In the first three columns, we ask participants to tell us where they have been for different purposes, but without asking about any particular neighborhoods. These results are qualitatively similar to those in Table 7, but are less precise. The last two columns report trips as measured by our GPS tracking app. Unfortunately, we were only able to obtain this data for 15% of the sample, but we include the results for completeness.

[44]Unlike our previous results, the coefficient on "Training × Unfamiliar" is smaller than that on "Baseline Familiar," which means that while participants return to these places, they do not treat them exactly the same as other familiar neighborhoods.

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.[45] Furthermore, 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 with city neighborhoods matters for how casual workers in Nairobi choose work opportunities, and a single experimentally induced exposure erases the initial aversion to work in unfamiliar neighborhoods. In our context, these results are most consistent with a fixed cost of exposure, which participants are partially aware of, and with incorrect beliefs about the fixed cost of exposure. We find some evidence of miscalibrated beliefs about unfamiliar neighborhoods, but this cannot explain the entire exposure effect, and we find no evidence of sorting. We also find that exposure may additionally affect choice by increasing the probability a neighborhood enters the consideration set.

We hypothesize that 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 get vaccinated (Jee, Karing, and Naguib 2025), 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 have a dividend in terms of effectively increasing market access for urban residents, both for employment and for other opportunities found in cities.

Additionally, policy makers may be able to decrease the cost of exploration by investing in making their cities more "readable." 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 fixed 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, and Mobarak 2014; Larcom, Rauch, and Willems

[45] We cannot rule out effects of up to +0.012 on the probability of working in a specific unfamiliar neighborhood, relative to a mean of 0.069.

2017; Okunogbe 2024; McKenzie 2024; Lagakos, Mobarak, and Waugh 2023).

Urban and spatial quantitative models can 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 decision 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 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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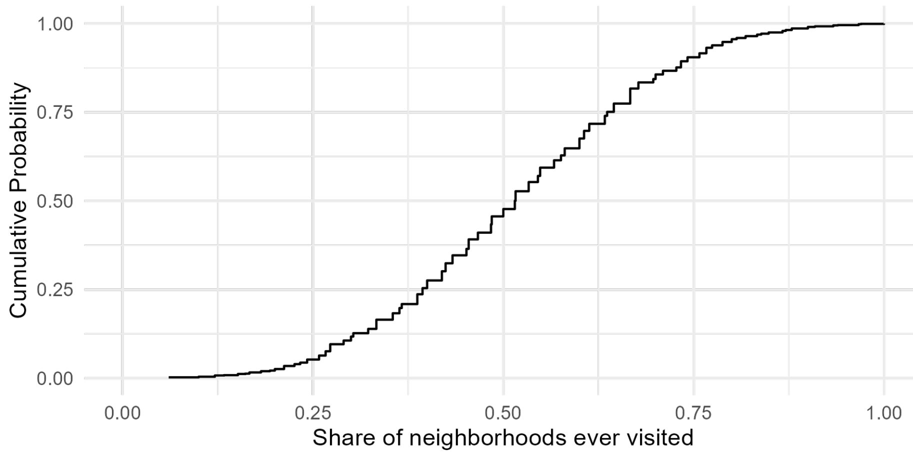
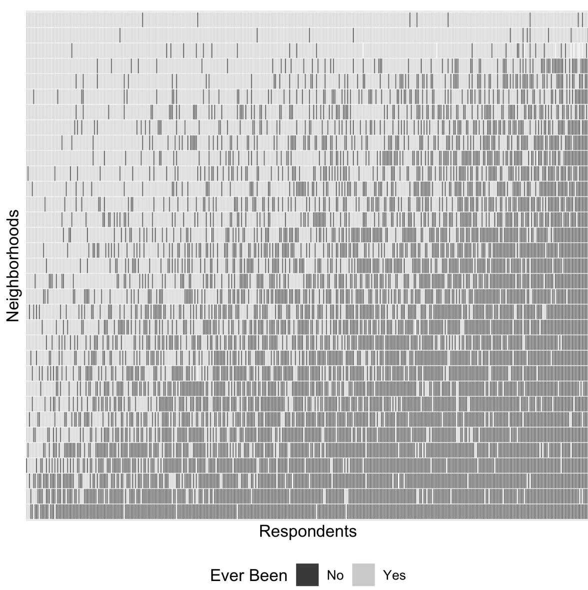
Figures and Tables

Figure 1: Urban Familiarity Patterns

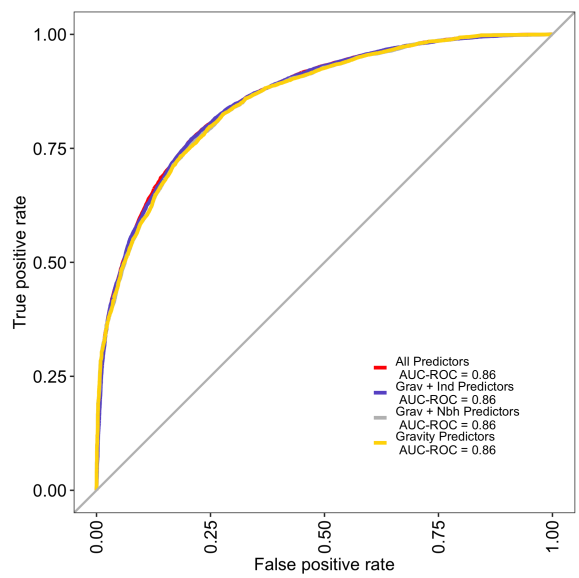
(a) CDF of Average Familiarity of Neighborhoods Within 75 Minutes from Home



(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 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; 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.

Table 1: Participants Have Significant Familiarity Gaps

	Share of Neighborhoods Visited					
Nbhd sample:	< 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 + gave landmark	0.24	0.34	0.35	0.33	0.50	0.46

Notes: This table reports statistics for the share of neighborhoods that a participant is familiar with. 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.

Table 2: Revealed Preference Estimates from Job Choices

	(1)	(2)	(3)
Baseline Familiar	0.85***	0.76***	0.86***
	(0.120)	(0.121)	(0.118)
Training	0.29**	0.33**	0.30**
	(0.109)	(0.103)	(0.105)
Training × Unfamiliar	0.83***	0.93***	0.91***
	(0.211)	(0.205)	(0.199)
Distance (km)	-0.24***		-0.25***
	(0.016)		(0.019)
Job duration (hrs)	-0.58***	-0.62***	-0.59***
	(0.062)	(0.061)	(0.059)
$\mathbb{E}$ Compensation (100 KSH)	0.76***	0.83***	0.80***
	(0.027)	(0.029)	(0.027)
Cash Upfront (100 KSH)	0.06	0.06	0.07
	(0.045)	(0.046)	(0.042)
Bonus (100 KSH)	-0.21***	-0.22***	-0.22***
	(0.010)	(0.011)	(0.010)
N	6,756	6,756	6,756
Home × neighborhood FE		Yes	
Neighborhood FE			Yes

Notes: This table reports binary logit estimation results based on equation (1) of choice data where individuals chose between two potential job offers. The sample is 400 participants in the "preferences" elicitation, and all choices during the three employment days, where both jobs are in one of the nine target neighborhoods, excluding the unfamiliar spillover neighborhood. (See Table A.20 for results using the full sample of seven choices each day, including all 10 target neighborhoods.) "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, total compensation, the amount offered in advance to ease liquidity constraints, and the amount that depended on a risky bonus. Standard errors in parentheses based on 500 individual-level bootstrap runs. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

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	-0.247^{***}	-0.249^{***}	-0.050	-0.393^{***}	-0.170^{**}
	(0.056)	(0.053)	(0.058)	(0.053)	(0.053)
After Training × Treated	-0.077	-0.092	-0.012	0.003	0.024
	(0.051)	(0.052)	(0.047)	(0.040)	(0.041)
N	4,584	4,575	4,468	4,587	4,518
Mean	3.418	3.427	3.58	4.03	3.844
SD	1.022	0.996	1.047	0.887	0.975
Before Train.×T=After Train.×T, p-val	0.003	0.004	0.492	0.000	0.001

Notes: This table reports results from regressions based on equation (2). The sample consists of participants in the treated group who rate their three main target unfamiliar neighborhoods, and participants in the control group who rate their three main familiar neighborhoods. These beliefs are elicited twice, first during the second baseline (Before Training) and again after participants return to the study venue after training in the respective neighborhood (After Training). The outcomes are beliefs about attributes of the visited neighborhoods. All outcomes are rated on a likert scale from 1 to 5 with 5 being the most positive outcome. The table shows that while beliefs are initially more negative for unfamiliar neighborhoods, the gap closes after visiting. Table A.21 repeats the analysis when we add home by neighborhood fixed effects, and Table A.22 repeats the analysis using belief confidence as the outcome. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

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

	(1)	(2)	(3)
Baseline Familiar	0.78***	0.81***	0.79***
	(0.071)	(0.069)	(0.076)
β^V Training	0.18*	0.20*	0.01
	(0.088)	(0.086)	(0.115)
β^U Training × Unfamiliar	0.77***	0.78***	0.80***
	(0.149)	(0.138)	(0.184)
β^{AV} Anticipate Training	-0.06	-0.04	-0.05
	(0.095)	(0.097)	(0.086)
β^{AU} Anticipate Training × Unfamiliar	0.48**	0.45**	0.41**
	(0.159)	(0.154)	(0.147)
N	13,658	13,658	6,902
P-value $\beta^U = \beta^{AU}$	0.026	0.012	0.020
Sample: Training	Yes	Yes	Yes
Sample: Employment	Yes	Yes	
Neighborhood FEs		Yes	Yes
Job Attribute Controls	Yes	Yes	Yes

Notes: This table reports binary logit estimation results of equation (3). Columns 1 and 2 pool all the choice data for the three training days, and for the three employment days. Column 3 only uses the training days data, leveraging variation in realized familiarity induced by training on previous training days (e.g. if respondent i visits neighborhood n on day 1 of training, then $Training_{in} = 1$ for choices made on training days $d \in \{2, 3\}$. Coefficients on expected compensation, liquidity, bonus and distance are included but not reported to save space. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table 5: Selection Model For Duration and Navigation to Unfamiliar Neighborhoods

	Duration (hours)		Get Lost		Ask Directions	
	To Job (1)	From Job (2)	To Job (3)	From Job (4)	To Job (5)	From Job (6)
Baseline Familiar	-0.06	-0.07	-0.01	0.00	-0.10*	-0.04*
	(0.04)	(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	0.01	-0.07	-0.03
	(0.06)	(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)
Data Source:	Task App		Survey Post-Training			
N	2,291	2,257	2,318	2,318	2,318	2,318
Control Mean	1.23	1.30	0.06	0.00	0.31	0.05

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, job duration, and whether there was cash upfront. 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. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

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
Baseline Familiar/Training × Unfamiliar		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 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.7). Columns 4 - 6 report results from the second round of invitations where all wages were below 500 KSH. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table 7: 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)	(0.014)	(0.009)	(0.004)	(0.001)	(0.003)	(0.005)	(0.001)	(0.004)	(0.001)	(0.003)	(0.006)
[p-value]	[0.000]	[0.000]	[0.341]	[0.160]	[0.534]	[0.002]	[0.008]	[0.006]	[0.004]	[0.001]	[0.000]
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 neighborhood, 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.26 presents the results without fixed effects. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table 8: 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. The three columns use the specifications from Table 2. The data is from the first day of job elicitation. Tables A.28 and A.29 report analogous results for days 2 and 3. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table 9: Estimates of Memory Costs from "Open" Elicitation

	Utility (α^U)	**Cost** (α^C)
Baseline Familiar	0.86	2.53
		[1.67, 4.02]
Training	0.30	2.02
		[1.34, 3.12]
Training $\times$ Unfamiliar	0.91	1.08
		[0.37, 2.13]
Distance	-0.25	-0.40
		[-0.62, -0.28]
Constant		2.49
		[1.19, 5.42]
Cost variance σ		1.98
		[1.40, 2.90]
Subjective λ		0.50
		[0.36, 0.66]
N		54,155
Participants		391
Top K choices		5

Notes: This table reports maximum likelihood estimates of the two-self memory model from section 6. We fix the first four preference parameters to those estimated in Table 2 column 3, and estimate the remaining parameters, including the memory cost parameters, 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. We parametrize $\lambda_k = \lambda(1-\lambda)^{k-1}$ and estimate the subjective job probability λ. Individual-level bootstrapped 95% confidence intervals in parentheses. Tables A.31 and A.32 report results varying changing the specification, the number of ranked choices used in estimation, and the day when choices are elicited.

A Appendix - For Online Publication

Contents

A.1 Model Derivations and Extension with Miscalibrated Priors

We first derive the baseline familiarity premium estimated from job choice data. This measures the average utility difference between familiar and unfamiliar neighborhoods.

Through repeated application of the law of iterated expectations, we obtain:

$$\begin{aligned}
\beta^F &= \mathbb{E}_{(i,n)}[Y_{in}|n \in F_i, D_{in}=0] - \mathbb{E}_{(i,n)}[Y_{in}|n \in U_i, D_{in}=0] \\
&= \mathbb{E}_i[\mathbb{E}_n[u_{in}|n \in F_i]] - \mathbb{E}_i[\mathbb{E}_{p_i}[u_{in}|i, n \in U_i] - c_i] \\
&= \underbrace{\mathbb{E}_i[\mathbb{E}_n[u_{in}|n \in F_i] - \mathbb{E}_{p_i}[u_{in}|i, n \in U_i]]}_{\text{Sorting}} + \underbrace{\mathbb{E}_i[c_i]}_{\text{Fixed Costs}}
\end{aligned}$$

That is, the baseline familiarity premium identifies the difference in expected utilities between familiar and unfamiliar neighborhoods plus the fixed costs of visiting the unfamiliar neighborhoods.

Our second result is the causal effect of exposure is the average fixed cost:

$$\begin{aligned}
\beta^U &= \mathbb{E}_{(i,n)}[Y_{in} \mid n \in U_i, D_{in}=1] - \mathbb{E}_{(i,n)}[Y_{in} \mid n \in U_i, D_{in}=0] - \\
&\quad - \left[\mathbb{E}_{(i,n)}[Y_{in} \mid n \in F_i, D_{in}=1] - \mathbb{E}_{(i,n)}[Y_{in} \mid n \in F_i, D_{in}=0]\right] \\
&= \mathbb{E}_{(i,n)}[\mathbb{E}_{q_{in}}[u_{in} \mid n \in U_i]] - \mathbb{E}_{(i,n)}[\mathbb{E}_{p_i}[u_{in} \mid n \in U_i] - c_i] \\
&= \mathbb{E}_i[\mathbb{E}_n[\mathbb{E}_{q_{in}}[u_{in} \mid n \in U_i]] - \mathbb{E}_{p_i}[u_{in} \mid n \in U_i]] + \mathbb{E}_i[c_i] \\
&= \mathbb{E}_i[c_i],
\end{aligned}$$

where the last line follows because the assumed accuracy of priors over neighborhoods implies that $\mathbb{E}_n[\mathbb{E}_{q_{in}}[u_{in} \mid n \in U_i]] = \mathbb{E}_{p_i}[u_{in} \mid n \in U_i]$.

Adding Miscalibrated Beliefs. We now allow for potentially incorrect beliefs about both quality and the fixed costs. Denote $\tilde{p}_{in}$ the perceived distribution of neighborhood quality and $\tilde{q}_{in}$ be the resulting posterior. (We allow a single exposure to provide some information about the true neighborhood quality u_{in} but not necessarily reveal it fully, so $\tilde{q}_{in}$ need not be a point mass at u_{in}.) We also allow participants to have miscalibrated beliefs about the fixed costs, and denote $\tilde{c}_i$ the perceived fixed costs.

To obtain the new baseline familiarity premium coefficient we first substitute in $\tilde{p}_i$ and $\tilde{c}_i$ into our previous result and then add and subtract $\mathbb{E}_{p_i}[u_{in} \mid n \in U_i]$ to obtain

$$\begin{aligned}\beta^F &= \mathbb{E}_i[\underbrace{\mathbb{E}_n[u_{in}|n \in F_i] - \mathbb{E}_{p_i}[u_{in}|i, n \in U_i]}_{\text{Correct Sorting}} + \\ &\quad \underbrace{\mathbb{E}_{p_i}[u_{in}|i, n \in U_i] - \mathbb{E}_{\tilde{p}_{in}}[u_{in}|i, n \in U_i]}_{\text{Incorrect beliefs}}] + \underbrace{\mathbb{E}_i[\tilde{c}_i]}_{\text{Fixed Costs}}\end{aligned}$$

Now, mis-perceived priors about the neighborhood quality also affect the familiarity premium. For example, even if familiar and unfamiliar neighborhoods do not differ on average (the correct sorting term is zero), individuals may believe this to be the case, which may explain why they are unwilling to work in baseline unfamiliar neighborhoods.

For our second result on the exposure effect, we can substitute in $\tilde{q}_{in}$ and $\tilde{c}_i$ to obtain

$$\beta^U = \mathbb{E}_i[\mathbb{E}_n[\underbrace{\mathbb{E}_{\tilde{q}_{in}}[u_{in} \mid n \in U_i]] - \mathbb{E}_{\tilde{p}_{in}}[u_{in} \mid n \in U_i]}_{\text{Belief updating}}] + \underbrace{\mathbb{E}_i[\tilde{c}_i]}_{\text{Fixed costs}},$$

where the first term is no longer zero because we no longer assume priors about neighborhood quality are well calibarted.

Finally, in the anticipation elicitation described in section 5.2, the agent does not update their beliefs since no information about neighborhood quality has been revealed, and only expects to pay the fixed costs. Thus

$$\beta^{AU} = \mathbb{E}_i[\tilde{c}_i]$$

Note that in this formulation of the model, the true fixed costs c_i never affect choices because before visiting the agent cares about the anticipated fixed costs and after visiting the fixed costs are sunk, regardless of whether the agent correctly perceived them or not.

A.2 Attrition Robustness: Revealed Preference Results

Participation in the study was very high but slightly imbalanced by treatment group. In the "preferences" elicitation group, in the control group one out of the 202 participants did not show up for any of the employment days, and the daily level no-show rate was 1.3% (598 out of 606 person-days). In the treatment group, four out of 198 did not show up for any of the employment days, and the daily level no-show rate was 3.9% (571 our of 594 person-days).

Differential attrition may in principle bias the results from Table 2 if the additional participants who do not show up in the treatment group have a strong aversion to the unfamiliar neighborhood they were trained in. This could make it look like treatment eliminates the aversion towards unfamiliar neighborhoods.

The very low overall level of attrition and the small degree of imbalance means that this type of bias cannot explain our results. We show this using three exercises. First, given the very small imbalance and large exposure effect we find, the differential attriters would have to have an extremely strong preference against their neighborhoods. Using baseline belief data we see that individuals in the treatment group who did not show up for employment days (and hence for whom we do not have job choice data) rate their unfamiliar training neighborhoods around 0.3-0.4 points lower than non-attriters on measures of safety, on a scale of 1 to 5. However, the vast majority of attriters (80-87%) rate the neighborhoods as "Neutral," "Safe" or "Very Safe" for neighborhood and travel safety, suggesting they do not have a strong aversion towards these neighborhoods.

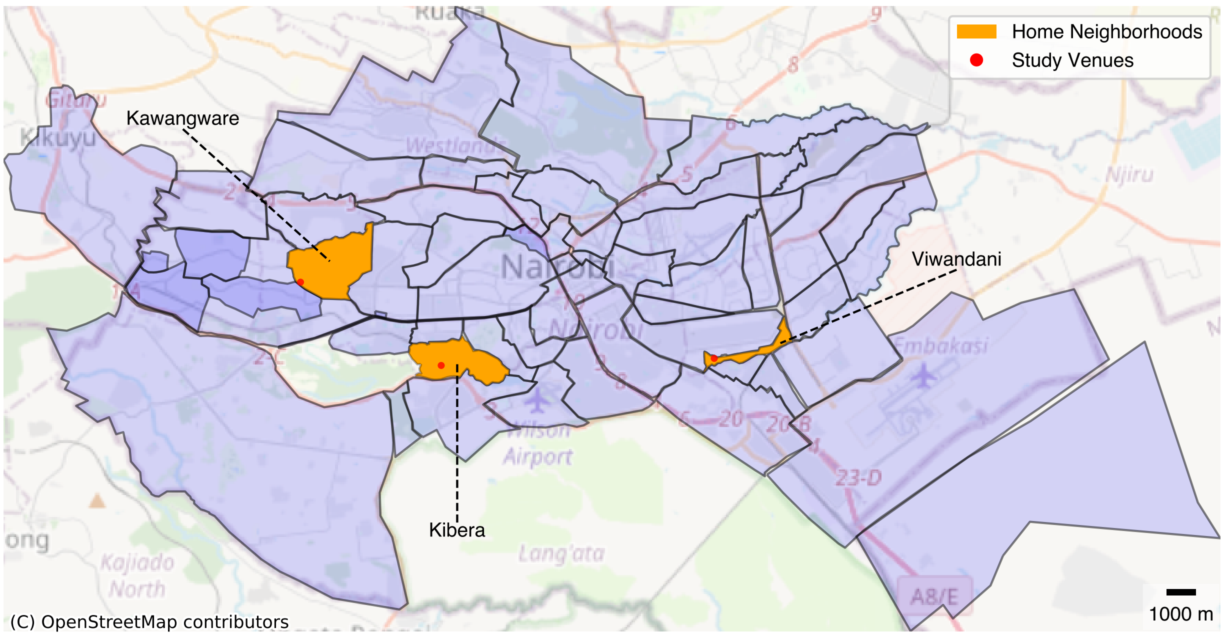
Second, in Table A.12 we estimate a linear version of the analysis in Table 2 using bounds based on Lee (2009). In order to apply this procedure, we restrict to job choices between a non-target familiar neighborhood and a target unfamiliar neighborhood. In this context, the treatment indicator captures the sum of the "Trained" and "Trained × Unfamiliar" coefficients, explaining why in column 1 we find a coefficient of 0.14 (compare with Table A.11). In column 2, we drop the (randomized) controls for job attributes, and in column 3 we aggregate the data from the choice to the individual level. Column 3 is thus a simple treatment versus control comparison with the outcome given by the shared of choices where the participant chose the unfamiliar neighborhood. In the last column, we implement the Lee (2009) bounds, and find they are very tight, precisely estimated, and far from zero.

Third, in the spirit of Broderick, Giordano, and Meager (2023), we study the sensitivity to dropping multiple "influential" participant by day combinations until the control and treatment groups are balanced. We define the influence of a participant day combination as the effect on a coefficient or statistic of interest after dropping the observations for that participant and day. Table A.13 shows that our results are robust to this exercise. The bounds for the baseline familiarity and exposure effect, both in absolute levels and relative to the total compensation coefficient, are far from zero, across all specification Table 2.

Overall, these results show that our main results cannot be explained by the small degree of differential attrition.

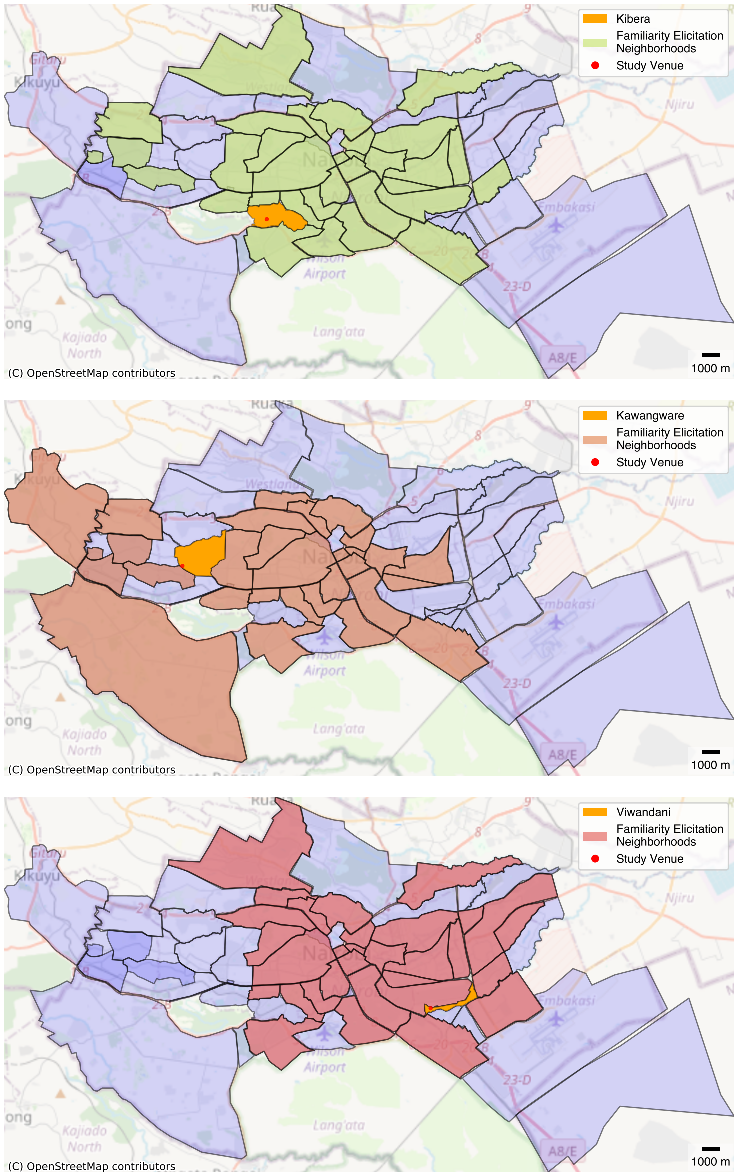
A.3 Appendix Figures

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 location of the study venues are highlighted in red. The remaining 58 neighborhoods are represented by light blue polygons.

Figure A.2: Familiarity Elicitation Neighborhoods

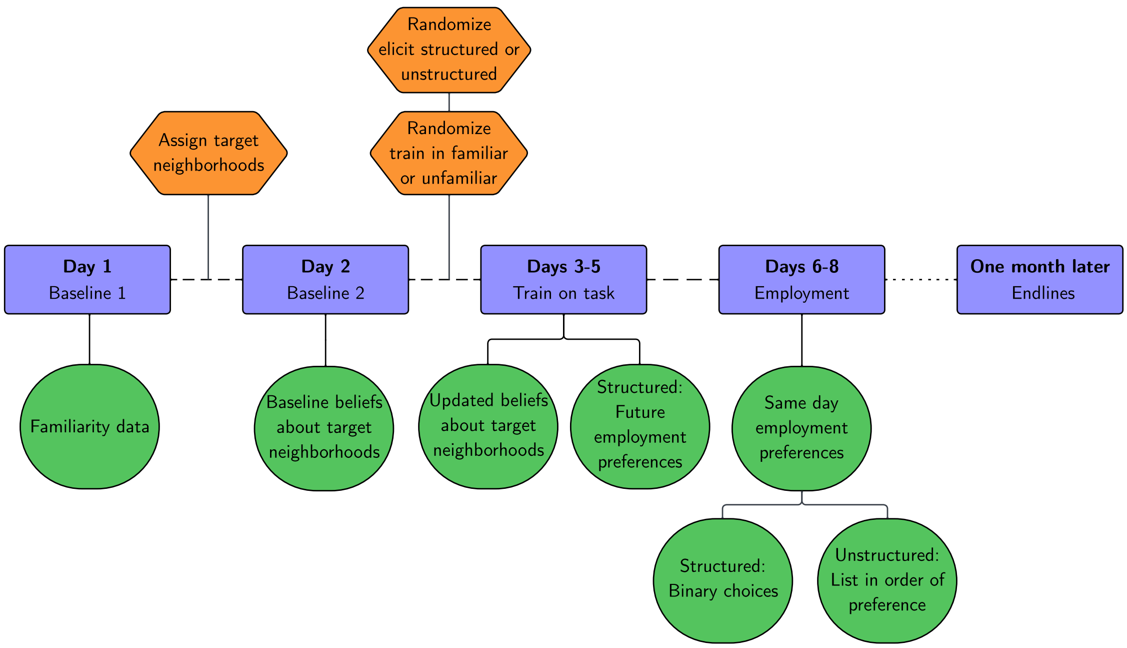


Notes: This figure replicates Figure A.1 for each home neighborhood and highlights the set of main neighborhoods for which we elicited familiarity. We elicited familiarity in 33 neighborhoods of Kibera, 30 in Kawangware, and 31 in Viwandani. For each home neighborhood, these neighborhoods were reachable in at most 75 minutes from the study venue. A small number of neighborhoods were dropped (see Appendix A.5 for a complete discussion of neighborhood selection).

Figure A.3: Backpacks Used in Employment Task

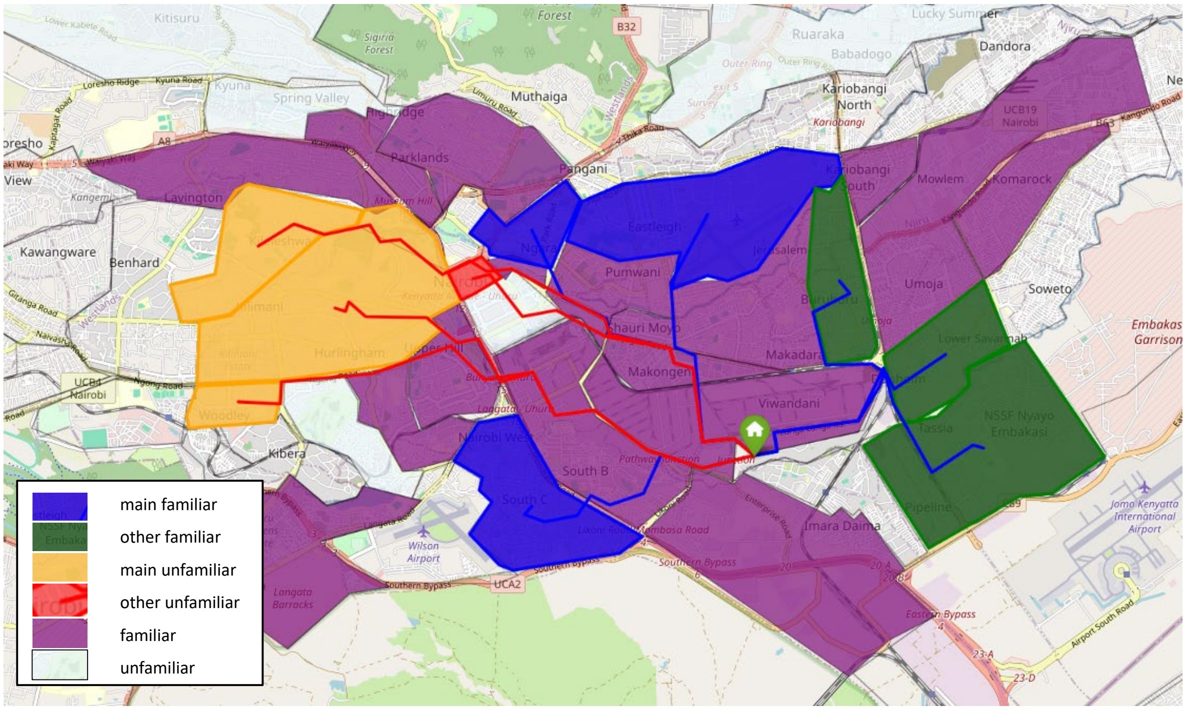
Notes: This figure shows a picture of an air pollution backpack used by study participants during training and employment days. The backpack contains an air pollution sensor (which has no display so does not reveal the air pollution level) and a smartphone with an app used to check-in and check-out from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

Figure A.4: Timeline of Study



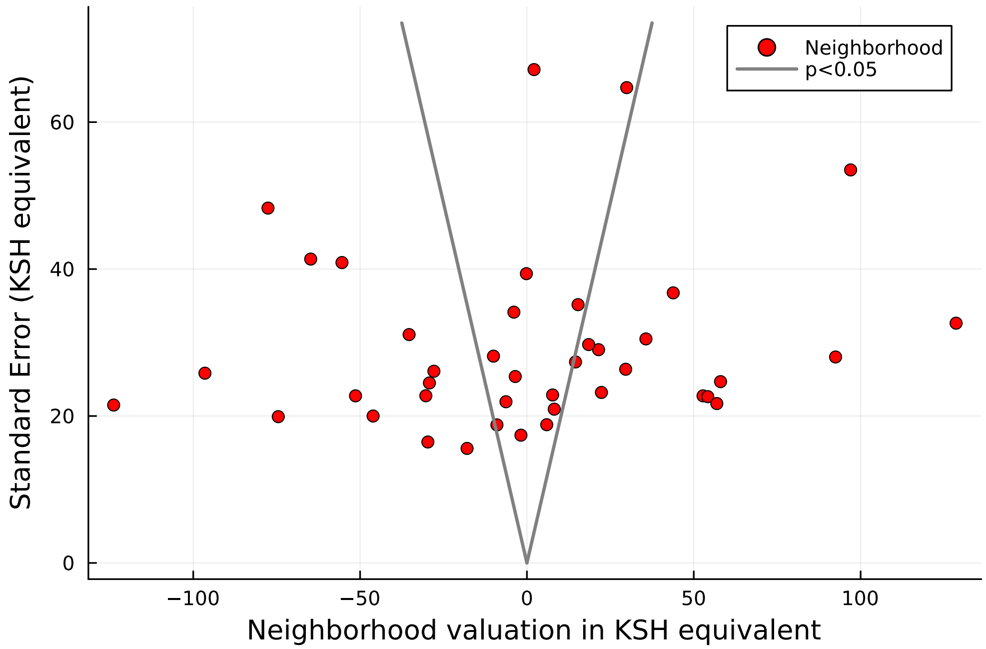
Notes: This figure shows the timing outline of the study. In the first two days, participants provided information about their neighborhood familiarity, demographics, beliefs about target neighborhoods, employment and job search status, and spatial ability. Over the next three days, participants were asked to collect air pollution data in either familiar or unfamiliar neighborhoods and received training on how to do so. Upon returning, they were asked about belief updating and familiarity with the neighborhood visited. Participants assigned to the "preferences" elicitation group were asked about their job preferences one day at a time for the upcoming employment days. On the last three days, participants worked in their top-preferred neighborhood, as elicited through either an "open" or "preferences" binary choices—Section 4 discusses the "preferences" surveys in more detail. They concluded the employment days with 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.5: Example of 10 Target Neighborhoods for One Participant



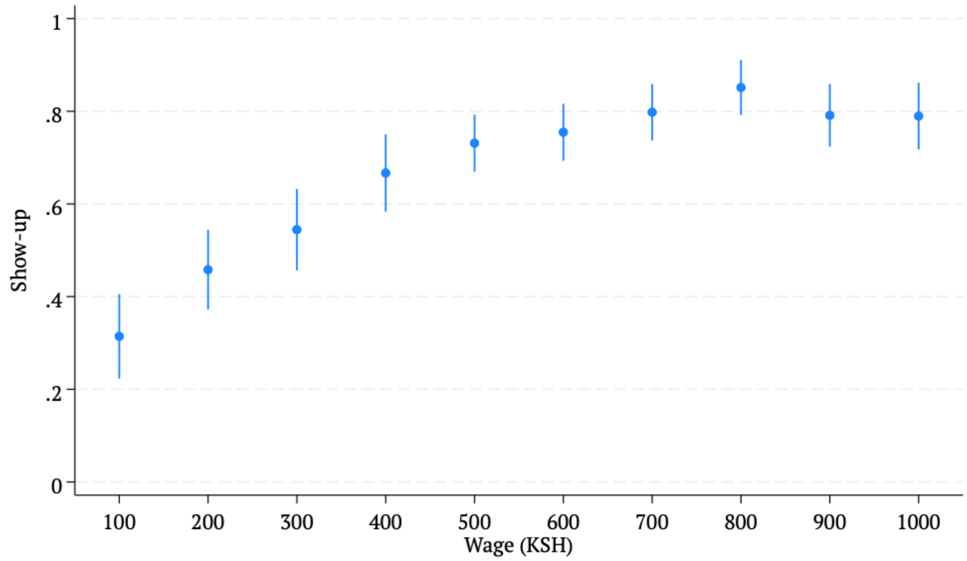
Notes: This map plots the ten target neighborhoods for one participant from the home neighborhood of Viwandani. Non-target neighborhoods are shaded in purple if they had ever visited at baseline or white if they had not. The four target categories are shared in different colors: blue for the three main 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.6: Distribution of Neighborhood Fixed Effects



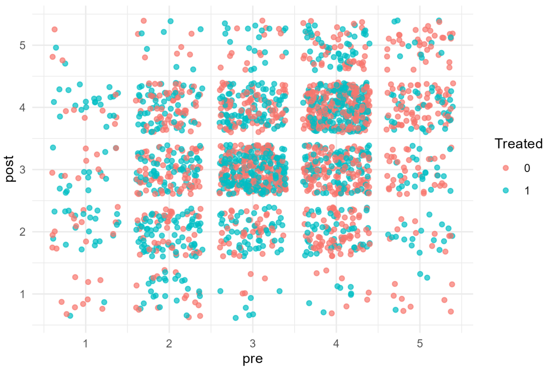
Notes: Estimated neighborhood fixed effects from column (3) of Table 2, alongside their bootstrapped standard error on the vertical axis. The fixed effects capture the average attractiveness of each neighborhood in the job choices.

Figure A.7: Show-up for Job Invitation 2-4 Months After Intervention: High Wage Plateau

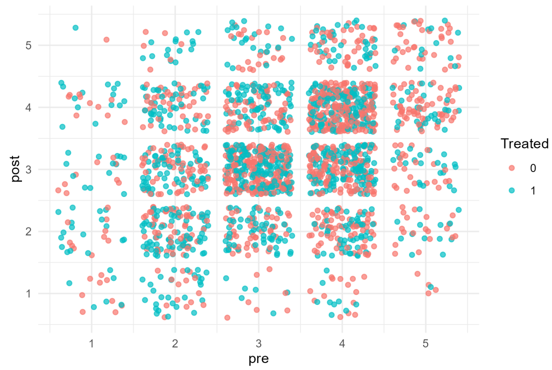


Notes: This figure plots average show-up for the travel survey task by (random) wage offer. These job offers were made by phone 2-4 months after the intervention. Participants were invited for a short (5-minute) survey on long it takes to travel in Nairobi.

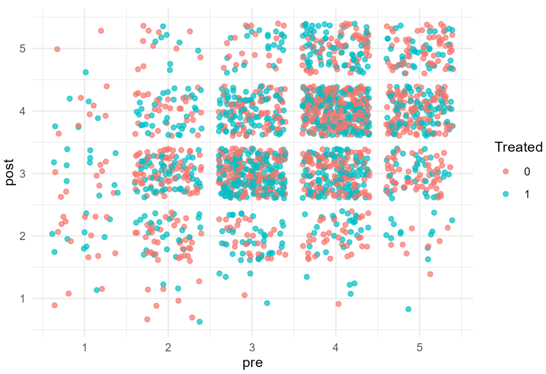
Figure A.8: Beliefs Transition Matrices



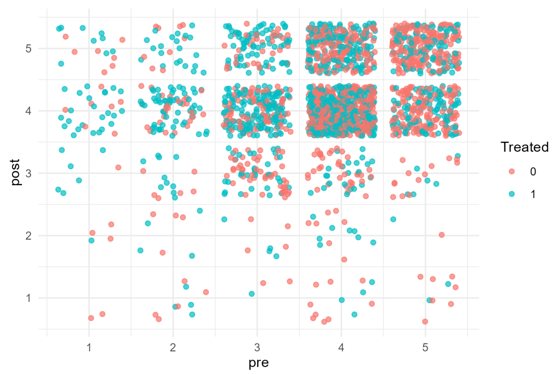
(A) Find Job



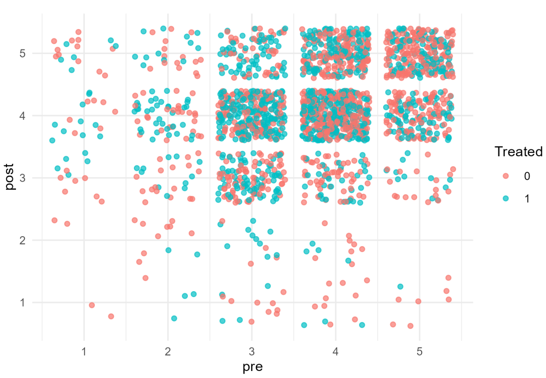
(B) Find Job - Others



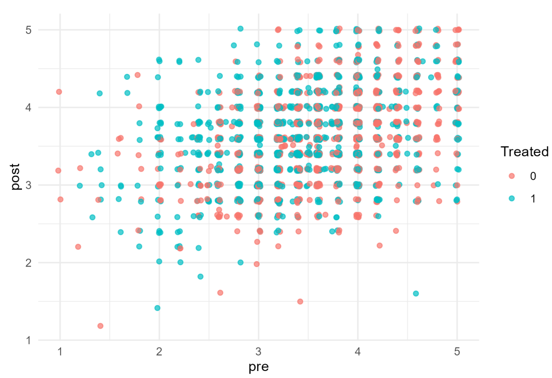
(C) Pay



(D) Travel Safety



(E) Overall Safety

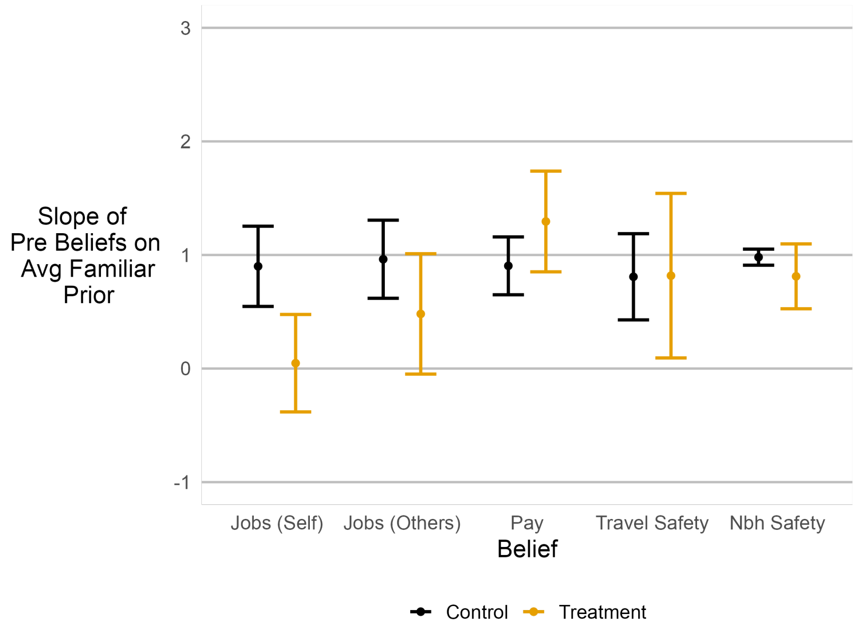


(F) Average (All Measures)

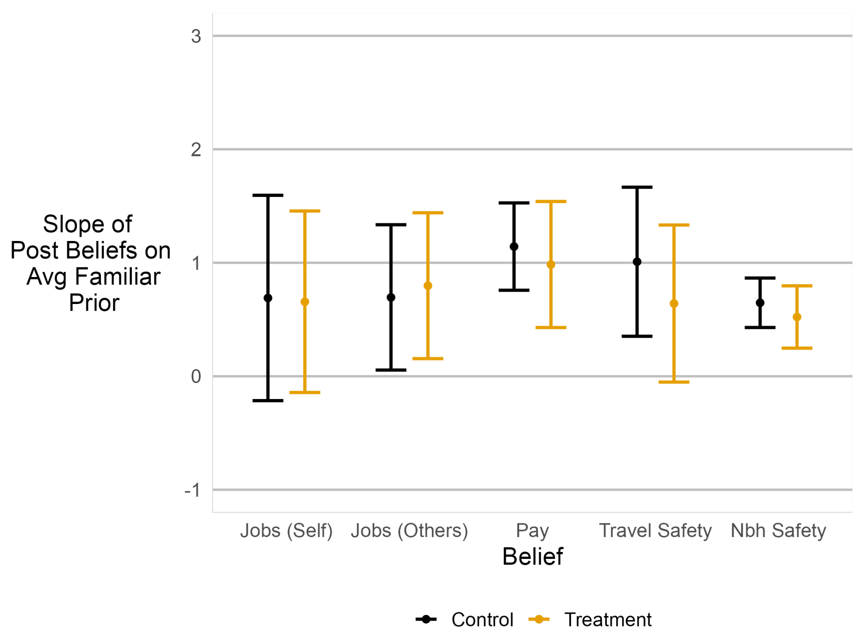
Notes: Each graph plots beliefs pre- and post-intervention on the X- and Y-axis, respectively. Higher values correspond to better outcomes. See Section A.7 for the precise questions underlying each measure.

Figure A.9: 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 folds. In panel (A.9a) each point represents the estimated slope of the prior beliefs (pre-visit) on these ratings. In (A.9b) 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.4 Appendix Tables

Table A.1: Sample characteristics

Variable	N	Mean	SD	Median
Female	799	0.74	0.44	1
Age	799	29.43	7.61	28
Education Years	798	10.67	2.78	12
Enrolled in School	799	0.04	0.20	0
Workdays Last 2 Weeks	799	3.08	2.86	3
Days Searched for Work Last 2 Weeks	799	6.56	3.38	6
Married	799	0.46	0.50	0
Years in Nairobi	799	15.19	9.29	13
Resided Outside Nairobi	799	0.67	0.47	1
Max Walk to Work/Search (km)	557	7.96	3.81	7.72
Max Transit to Work/Search (km)	552	11.49	4.97	11.41
Max Walk to Work/Search (min)	557	108.73	51.10	106.92
Max Transit to Work/Search (min)	552	56.54	14.41	57.12
Max Fastest Travel to Work/Search (min)	557	57.64	21.59	57.12

Notes: This table reports basic statistics on participant characteristics for the experiment sample, namely all participants who attended the first job training day. 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	14%	Industrial/factory worker	9.6%
Househelp	9.8%	Electrician	6%
Washing dishes/utensils	8.2%	Cleaner	5.4%
Cook	7.3%	Small Business	4.8%
Salon	6.6%	Mechanic	4.2%
Waiter	3.9%	Cook	3.6%
Small Business	3.7%	Carrying luggage	3.6%
Industrial/factory worker	2.7%	Plumber	3.6%
Sales person	2.7%	Boda boda operator	3%

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 605 participants—438 women and 167 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: 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.5: 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	0.138	0.138
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.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				0.84

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				0.25

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

Table A.8: Comparing Target Unfamiliar Nhbds to All Unfamiliar Nhbds

	Main Unfamiliar (1)	All Unfamiliar (2)	Difference (3)
Belief: Find Job	3.238	3.189	0.049
			(0.076)
Belief: Find Job - Others	3.192	3.122	0.070
			(0.083)
Belief: Pay	3.601	3.571	0.030
			(0.060)
Belief: Travel Safety	4.025	4.014	0.011
			(0.027)
Belief: Overall Safety	3.930	3.887	0.043
			(0.046)
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 unfamiliar neighborhoods. Estimates are weighed 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 3, Table 2. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.9: Attrition at Different Stages of the Study

	Training		Employment		Endline
	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 shows attrition at different stages of the study timeline for our two elicitation samples. Our study sample are 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) presents 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.10: 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 standard 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.11: Revealed Preferences 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 (KSH)	0.102***	0.101***	0.102***	0.101***
	(0.002)	(0.002)	(0.002)	(0.002)
Cash Upfront (KSH)	0.007	0.007	0.008	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Bonus (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 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.12: Treatment Effects on Choosing Unfamiliar Neighborhood: OLS and Lee Bounds

	(1) OLS Controls	(2) OLS No Controls	(3) OLS Individual	(4) Lee Bounds
Treated	0.143***	0.166***	0.160***	
	(0.023)	(0.027)	(0.030)	
Lower Bound				0.148
				(0.031)
Upper Bound				0.185
				(0.033)
Choice Attributes	Yes	No	No	No
Unit of Analysis	Choice	Choice	Individual	Individual
Control Mean	0.325	0.325	0.319	0.319
Observations	1,353	1,353	387	387

This table reports Lee (2009) bounds for the treatment effect on choosing the unfamiliar neighborhood, using the structured employment choices sample and the subset of choices between a non-target familiar and an unfamiliar neighborhood. The outcome is choosing the unfamiliar neighborhood. Among the 400 participants in the structured arm, 94.9% of the treated group and 98.5% of the control group have at least one such choice. Columns (1)–(3) report OLS estimates (linear probability models) at different levels of aggregation: Column (1) includes choice attributes as controls, Column (2) excludes controls, and Column (3) collapses the outcome to the individual level (proportion of choices selecting unfamiliar). Column (4) reports Lee bounds computed at the individual level. In columns (1)–(2), standard errors are clustered at the individual level. Column 3 reports robust standard errors. In column 4, standard errors are computed via 1,000 bootstrap replications resampling at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.13: Revealed Preference Estimation: Differential Attrition Trimming Bounds

	(1)	(2)	(3)
Baseline Familiar	0.85	0.76	0.86
	[0.76, 0.97]	[0.66, 0.89]	[0.77, 0.99]
Train × Unfamiliar	0.83	0.93	0.91
	[0.69, 1.03]	[0.78, 1.13]	[0.75, 1.11]
Baseline Familiar / $\mathbb{E}$ Total Pay	1.11	0.92	1.07
	[0.99, 1.25]	[0.79, 1.07]	[0.95, 1.21]
Train × Unfamiliar / $\mathbb{E}$ Total Pay	1.09	1.13	1.13
	[0.89, 1.34]	[0.93, 1.35]	[0.93, 1.36]

This table reports bounds for selected estimates from Table 2, using a procedure to trim observations based on their influence, to shed light on the implications of the small differential attrition documented in Table A.9. For each statistic, either a coefficient or a ratio of coefficients, and each of the three logit specifications from Table 2, we perform the following procedure. First, for each control group participant and employment day combination we compute its "influence" by dropping the associated choices and re-estimating the binary logit model. The influence is the difference between the newly estimated statistic and the statistic estimated using the entire sample. In order to balance the treatment and control groups we drop 15 participant × day combinations from the control group. (This equates the attrition rates at the participant day level, $(598 - 15)/606$ in the control group equals $571/594$ in the treatment group.) To construct the lower bound of the statistic, we re-estimate the logit model after dropping the 15 observations with largest influence, and vice-versa for the upper bound.

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)		(0.126)		(0.120)	
Baseline Unfamiliar × High Safety Belief		-0.78***		-0.65***		-0.75***
		(0.171)		(0.165)		(0.158)
Baseline Unfamiliar × Not High Safety Belief		-0.88***		-0.80***		-0.91***
		(0.130)		(0.133)		(0.140)
Training	0.29*	0.29**	0.33**	0.33**	0.30**	0.30**
	(0.112)	(0.110)	(0.100)	(0.101)	(0.114)	(0.110)
Training × Unfamiliar	0.83***		0.93***		0.91***	
	(0.219)		(0.204)		(0.217)	
Training × Unfamiliar × High Safety Belief		0.74**		0.83**		0.81**
		(0.266)		(0.258)		(0.254)
Training × Unfamiliar × Not High Safety Belief		0.87***		0.97***		0.95***
		(0.229)		(0.228)		(0.234)
Distance (km)	-0.24***	-0.24***			-0.25***	-0.25***
	(0.015)	(0.014)			(0.021)	(0.019)
Job duration (hrs)	-0.58***	-0.58***	-0.62***	-0.62***	-0.59***	-0.59***
	(0.058)	(0.057)	(0.063)	(0.062)	(0.063)	(0.058)
$\mathbb{E}$ Compensation (KSH)	0.76***	0.76***	0.83***	0.83***	0.80***	0.80***
	(0.026)	(0.024)	(0.029)	(0.028)	(0.027)	(0.027)
Cash Upfront (KHS)	0.06	0.06	0.06	0.06	0.07	0.06
	(0.043)	(0.042)	(0.047)	(0.047)	(0.045)	(0.044)
Bonus (KSH)	-0.21***	-0.21***	-0.22***	-0.22***	-0.22***	-0.22***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(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 High Safety = Not High Safety						
× Unfamiliar		0.33		0.2		0.16
× Training × Unfamiliar		0.29		0.3		0.28

Notes: Odd columns replicate results from Table 2 using $Unfamiliar_{in} = 1 - Familiar_{in}$ instead of $Familiar_{in}$. Even columns interact the unfamiliar coefficients ($Unfamiliar_{in}$ and $Train \times Unfamiliar$) 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)		(0.117)		(0.121)	
Baseline Unfamiliar × High Safety Belief		-0.72***		-0.64***		-0.72***
		(0.164)		(0.163)		(0.171)
Baseline Unfamiliar × Not High Safety Belief		-0.91***		-0.81***		-0.92***
		(0.133)		(0.139)		(0.135)
Training	0.29*	0.29**	0.33**	0.33**	0.30**	0.30**
	(0.115)	(0.112)	(0.106)	(0.101)	(0.110)	(0.114)
Training × Unfamiliar	0.83***		0.93***		0.91***	
	(0.217)		(0.205)		(0.204)	
Training × Unfamiliar × High Safety Belief		0.65*		0.75**		0.71**
		(0.258)		(0.243)		(0.261)
Training × Unfamiliar × Not High Safety Belief		0.91***		1.01***		0.99***
		(0.232)		(0.222)		(0.222)
Distance (km)	-0.24***	-0.24***			-0.25***	-0.25***
	(0.015)	(0.015)			(0.019)	(0.019)
Job duration (hrs)	-0.58***	-0.58***	-0.62***	-0.62***	-0.59***	-0.59***
	(0.060)	(0.058)	(0.060)	(0.061)	(0.060)	(0.062)
$\mathbb{E}$ Compensation (KSH)	0.76***	0.76***	0.83***	0.83***	0.80***	0.80***
	(0.024)	(0.024)	(0.027)	(0.029)	(0.028)	(0.026)
Cash Upfront (KHS)	0.06	0.06	0.06	0.06	0.07	0.07
	(0.042)	(0.042)	(0.048)	(0.044)	(0.043)	(0.044)
Bonus (KSH)	-0.21***	-0.21***	-0.22***	-0.22***	-0.22***	-0.22***
	(0.010)	(0.010)	(0.010)	(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 High Safety = Not High Safety						
× Unfamiliar		0.12		0.17		0.12
× Training × Unfamiliar		0.12		0.13		0.12

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 (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 (KHS)	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 (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 results from Table 2 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 Nbhds 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)
Visited Any × Close	0.25*	0.30**	0.29**
	(0.133)	(0.134)	(0.132)
Visited Any × Far	0.28*	0.35**	0.30*
	(0.166)	(0.147)	(0.156)
Visited Unfamiliar × Close	0.96***	1.02***	0.93***
	(0.257)	(0.265)	(0.260)
Visited 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 results from Table 2 interacting all coefficients with an indicator for proximity, defined as travel durations below or above the median (roughly 1 hour). The other dependent variables from Table 2 are included and interacted with the distance indicator but not reported. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.18: Preferences for Familiar Nbhds 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 KHS)	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 KHS)	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 KHS)	-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 specifications in Table 2 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 specification in Column 1 of Table 2 unpacking the baseline familiarity variable by the strength of the familiarity reported at baseline. The other dependent 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 (KSH)	0.75***	0.80***	0.78***
	(0.024)	(0.026)	(0.025)
Cash Upfront (KHS)	0.08*	0.09*	0.09*
	(0.040)	(0.039)	(0.043)
Bonus (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 results from Table 2 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: Average Beliefs Converge After One Visit (with neighborhood FEs)

	Dependent variable:				
	Find Job	Find Job (Others)	Pay is Good	Travel Safety	Overall Safety
	(1)	(2)	(3)	(4)	(5)
After Training	-0.131^{***}	-0.185^{***}	-0.058	0.142^{***}	0.239^{***}
	(0.039)	(0.039)	(0.037)	(0.033)	(0.038)
Before Training × Treated	-0.174^{**}	-0.116^{*}	-0.033	-0.375^{***}	-0.154^{**}
	(0.060)	(0.058)	(0.062)	(0.059)	(0.058)
After Training × Treated	0.000	0.047	-0.000	0.024	0.037
	(0.057)	(0.059)	(0.053)	(0.044)	(0.046)
N	4,584	4,575	4,468	4,587	4,518
Mean	3.418	3.427	3.580	4.030	3.844
SD	1.022	0.996	1.047	0.887	0.975
Pre×T=Post×T, p-val	0.003	0.003	0.556	0	0.002
Home × neighborhood FEs	Yes	Yes	Yes	Yes	Yes

Notes: This table replicates estimates from Table 3 including home by neighborhood FEs. All outcomes are rated on a likert scale from 1 to 5 with 5 being the most positive outcome. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.22: Confidence on Beliefs After One Visit

	Dependent variable:				
	Find Job	Find Job - Others	Pay	Travel Safety	Overall Safety
	(1)	(2)	(3)	(4)	(5)
After Training	0.143^{***}	0.092^{*}	-0.040	0.152^{***}	0.164^{***}
	(0.042)	(0.042)	(0.038)	(0.034)	(0.037)
Before Training × Treated	-0.107	-0.126^{*}	-0.147^{**}	-0.203^{***}	-0.145^{**}
	(0.058)	(0.060)	(0.055)	(0.053)	(0.053)
After Training × Treated	-0.087	-0.033	-0.046	-0.007	-0.016
	(0.046)	(0.048)	(0.046)	(0.037)	(0.038)
N	4,743	4,731	4,722	4,734	4,710
Mean	4.117	4.102	4.286	4.396	4.341
SD	1.028	1.066	0.935	0.916	0.908
Pre×T=Post×T, p-val	0.786	0.212	0.138	0.006	0.072

Notes: This table replicates estimates from Table 3 using participant confidence in their reported beliefs after one visit. All outcomes are rated on a Likert scale from 1 to 5, with 1 being not confident at all and 5 being the most confident. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.23: 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 model estimated on the "preferences" elicitation choices shown in Table 2 but 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.24: 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
Baseline Familiar/Training × Unfamiliar		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.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)
Visited Any	× (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)
Visited 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
Baseline Familiar/Training × Unfamiliar		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: People Re-visit the Neighborhoods From the Study (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)	(0.014)	(0.011)	(0.004)	(0.000)	(0.003)	(0.005)	(0.001)	(0.004)	(0.001)	(0.002)	(0.006)
[p-value]	[0.000]	[0.000]	[0.504]	[0.000]	[0.714]	[0.003]	[0.235]	[0.012]	[0.042]	[0.000]	[0.000]

Notes: This table replicates the results from Table 7 without including neighborhood fixed effects. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.27: Impact on Job Search and Work - Unprompted Measures

	Endline - (unprompted)				Smartphone	
	Any Trip	Search for Work	Work	Other	Visited	Num Visits
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	0.024***	0.016**	0.003	0.007*	0.016*	0.340
	(0.007)	(0.006)	(0.002)	(0.003)	(0.006)	(0.186)
Training	0.020*	0.023*	−0.0009	−0.003	−0.004	−0.192
	(0.010)	(0.009)	(0.003)	(0.004)	(0.008)	(0.211)
Training × Unfamiliar	−0.008	−0.018	0.005	0.008	0.007	0.312
	(0.015)	(0.014)	(0.005)	(0.007)	(0.014)	(0.411)
Observations	6,896	6,896	6,896	6,896	941	941
Mean	0.035	0.030	0.003	0.005	0.011	0.222
SD	0.185	0.169	0.059	0.071	0.058	1.315
Training + Training × Unfamiliar	0.012	0.005	0.004	0.005	0.003	0.120
[p-value]	[0.082]	[0.377]	[0.203]	[0.202]	[0.688]	[0.729]
Home × Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table regresses whether we observe individuals revisiting target neighborhoods through two unprompted measures on indicators for whether the neighborhood was familiar at baseline, whether the individual trained in the neighborhood during the experiment and whether the trained neighborhood was unfamiliar at baseline. Columns 1-4 show the results from asking participants open-ended questions about where they have recently visited, while columns 5 and 6 include trips measured by the GPS tracking application. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.28: 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 8 using data from day 2 of employment. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.29: 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 8 using data from day 3 of employment. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

Table A.30: Ranked Logit Estimates in "Open" Elicitation (Day 1)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Baseline Familiar	1.52***	1.38***	1.46***	0.96***	0.80***	0.90***	0.66***	0.59***	0.67***
	(0.287)	(0.335)	(0.313)	(0.154)	(0.188)	(0.176)	(0.100)	(0.110)	(0.111)
Training	0.58***	0.64***	0.65***	0.55***	0.58***	0.60***	0.58***	0.54***	0.55***
	(0.167)	(0.177)	(0.167)	(0.138)	(0.156)	(0.140)	(0.105)	(0.118)	(0.113)
Training × Unfamiliar	0.92*	0.98*	0.99*	0.43	0.51	0.48	0.02	0.15	0.11
	(0.407)	(0.429)	(0.401)	(0.270)	(0.314)	(0.291)	(0.221)	(0.232)	(0.220)
Distance (km)	-0.19***		-0.20***	-0.17***		-0.18***	-0.12***		-0.12***
	(0.023)		(0.033)	(0.020)		(0.028)	(0.013)		(0.017)
Top k choices	1	1	1	2	2	2	5	5	5
NBH x home FE		Yes			Yes			Yes	
NBH FE			Yes			Yes			Yes
N	4,134	4,134	4,134	7,880	7,880	7,880	16,798	16,798	16,798

Notes: This table reports ranked logit estimates of preferences based on the ranked lists provided by respondents on the first day. The model is estimated using the "exploded logit" method. For the i-th choice, the sample is all neighborhoods listed by the participant, excluding the neighborhoods ranked $1, \ldots, (i-1)$. We stack all these decisions are estimate a multinomial logit model. We restrict to the top choice (columns 1-3), top two choices (columns 4-6) and top five choices (columns 7-9). $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	1.89	2.28	2.53	0.88	0.62	0.58
	[1.38, 2.46]	[1.77, 3.01]	[1.67, 4.02]	[0.70, 1.08]	[0.49, 0.75]	[0.48, 0.69]
Training	1.51	1.73	2.02	0.89	0.68	0.64
	[1.12, 2.01]	[1.29, 2.31]	[1.34, 3.12]	[0.73, 1.09]	[0.53, 0.86]	[0.53, 0.78]
Training $\times$ Unfamiliar	0.79	1.01	1.08	0.37	0.28	0.26
	[0.19, 1.38]	[0.41, 1.72]	[0.37, 2.13]	[0.14, 0.57]	[0.10, 0.43]	[0.08, 0.42]
Distance	-0.29	-0.33	-0.40	-0.14	-0.09	-0.09
	[-0.36, -0.22]	[-0.40, -0.27]	[-0.62, -0.28]	[-0.17, -0.11]	[-0.12, -0.08]	[-0.10, -0.07]
Constant			2.49	-0.64	-0.83	-0.84
			[1.19, 5.42]	[-0.79, -0.42]	[-0.94, -0.68]	[-0.93, -0.73]
Cost variance σ	1.49	1.70	1.98	0.86	0.67	0.64
	[1.21, 1.92]	[1.41, 2.11]	[1.40, 2.90]	[0.73, 1.03]	[0.56, 0.76]	[0.56, 0.71]
Subjective λ	0.39		0.50	0.29	0.25	0.24
	[0.28, 0.48]		[0.36, 0.66]	[0.25, 0.33]	[0.21, 0.28]	[0.21, 0.27]
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 9 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	2.53	3.08	2.90
	[1.67, 4.02]	[1.81, 6.11]	[2.01, 21.29]
Training	2.02	2.82	3.07
	[1.34, 3.12]	[1.60, 5.44]	[1.97, 18.96]
Training $\times$ Unfamiliar	1.08	1.44	0.97
	[0.37, 2.13]	[0.36, 3.77]	[0.15, 9.25]
Distance	-0.40	-0.65	-0.82
	[-0.62, -0.28]	[-1.29, -0.39]	[-5.74, -0.53]
Constant	2.49	6.32	8.21
	[1.19, 5.42]	[2.69, 15.10]	[4.98, 76.26]
Cost variance σ	1.98	2.60	2.85
	[1.40, 2.90]	[1.57, 4.90]	[2.01, 18.47]
Subjective λ	0.50	0.55	0.62
	[0.36, 0.66]	[0.31, 0.84]	[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 9 using data from days two and three of the open elicitation.

Table A.33: 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.

A.5 Listing of Nairobi Neighborhoods and Selection into Sample

We divided Nairobi into 61 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 17 neighborhoods from our study that we deemed not relevant for work opportunities for our study population. Table A.33 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 44 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 interrupted collecting familiarity data in some neighborhoods around a third of the way through data collection because their familiarity rates were either too high or too low. For our results on overall familiarity levels (e.g. Figure 1a) we impute familiarity in these neighborhoods based on the initial average level. 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.[46] 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.

A.6 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. [47]

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 j intersects a neighborhood k. 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.

[46] 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.

[47] 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.

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

Starting with a sample of 1168 participants who showed up at Baseline 1, we excluded 119 because they had six or fewer familiar neighborhoods, 133 because they had four or fewer unfamiliar neighborhoods, and one participant who had fewer than four unfamiliar and six familiar neighborhoods. Consequently, we invited 915 participants to continue in the study.

A.7 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.
    - 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.8 Additional Belief Results

We also have 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 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 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.9a and A.9b presents 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, based on the outcome, while 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.