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 this in the context of how exposure to different neighborhoods affects how casual workers in Nairobi make work location decisions. We offer short-term employment opportunities across the city and experimentally induce first-time exposure by training participants in either familiar or unfamiliar neighborhoods. We find that participants are willing to travel 3.5 km further or take a pay cut worth 22% of the median daily wage to avoid working in a location never visited before. This differential is fully offset after a single, short visit to an unfamiliar neighborhood. In a benchmark model, these results are inconsistent with sorting and uncertainty over neighborhood quality. Participants also partly anticipate they will be more willing to work in a neighborhood after they will train there, further weighing against the role of neighborhood quality. We find little evidence of first-time navigation costs or exploration risk. The results are most consistent with one-time psychological exposure costs or incorrect beliefs about the size of the fixed costs. Using a separate elicitation, we show that participants are also initially less likely to spontaneously consider working in an unfamiliar neighborhood and a single visit closes part of this gap. Our results suggest that exposure is an important component of urban mobility in cities like Nairobi.

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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 as a potential choice (Zajonc 1968; Bornstein 1989; Montoya et al. 2017; Goldstein and Gigerenzer 2002; Gigerenzer and Goldstein 2011; Schank 1982). Does exposure have an effect on economic decisions beyond revealing information? If past exposure directly influences preferences or consideration, then classical models may overstate effective access to opportunities, preference estimates may be biased, and policies that raise exposure may be beneficial.

We study the role of exposure in a spatial setting in Nairobi, Kenya, looking at how prior exposure with urban neighborhoods shapes work location choices. This is a useful domain for three reasons. First, 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] Second, exposure may contribute to explaining why people seem to not take full advantage of urban opportunities (Lall, Henderson, and Venables 2017; Pelnik 2024), the puzzle of high inferred migration costs (Kennan and Walker 2011; Bryan and Morten 2019; McKenzie 2024), and evidence of persistence in migration and commuting behavior after temporary shocks (Bryan, Chowdhury, and Mobarak 2014; Larcom, Rauch, and Willems 2017b; Okunogbe 2024). Third, exposure may be particularly relevant for spatial choices. 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).

We proceed in six 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 and how it changes with a single exposure. Third, we use a benchmark model of a Bayesian expected utility maximizer to explore uncertainty about neighborhood quality and fixed costs as possible mechanisms and look for evidence in the data. Fourth, we test whether individuals anticipate how exposure will change their preferences, which further helps distinguish between mechanisms while allowing for incorrect prior beliefs over neighborhood quality. Fifth, we measure the persistence of the exposure

[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).

effects after the experiment. Finally, we study how past and experimental exposure affect the formation of consideration sets when subjects choose where to work in an open-ended elicitation.

In the first step, we document that individuals in our setting have substantial gaps in their exposure to neighborhoods near their homes. In our sample of casual workers living in poor neighborhoods, participants who worked or searched for work outside their home neighborhood (around two thirds of the sample) travel up to 7.9 km or one hour on average. However, the median participant has never 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. 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 have no predictive power.

In the second step, we quantify the revealed costs of working in an unfamiliar neighborhood by offering short-term employment opportunities in a real-stakes setting. Participants are offered jobs to measure air quality in different neighborhoods across Nairobi – a task that requires being physically present in a specific location and plausibly justifies assignments to a variety of locations.

To experimentally induce exposure, we train participants on how to complete the employment task in three neighborhoods over three days. Participants in the treatment group are trained in neighborhoods they report never having been to at baseline (unfamiliar neighborhoods), while participants in the control group are trained in familiar neighborhoods.[2,3]

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. Each morning, these participants make seven binary choices between pairs of job offers for that day. The air pollution monitoring task is held fixed across all job offers, but we randomly vary job location, duration, and compensation. After participants make their choices, 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. Participants are willing to 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. This effect is driven by a preference for

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

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

neighborhoods that are the most familiar at baseline, defined as places where participants can name a landmark or report knowing how to get there. Ignoring familiarity would overstate the utility cost of distance by 20%, because more distant neighborhoods are more likely to be unfamiliar. Overall, this *cross-sectional 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 experimentally inducing a single exposure lasting one hour during training to a previously unfamiliar neighborhood is sufficient to completely eliminate this familiarity premium. We find that above and beyond the main effect of training, having visited 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.[4]

In our third step, we consider what might drive this effect of exposure and the baseline familiarity premium. We begin by interpreting our results through the lens of a benchmark model of a Bayesian agent with correct priors who maximizes expected utility. We allow the agent to be risk-averse over neighborhood attributes and model exposure as revealing information about the utility of working in a neighborhood, following previous work on "experience goods" (Milgrom and Roberts 1986; Ackerberg 2003). This allows us to consider whether our results are consistent with individuals avoiding unfamiliar neighborhoods because they either believe their quality is low or because they are reluctant to accept the risk that the unfamiliar neighborhood has an attribute they dislike. We also allow the agent to incur a fixed cost when they are exposed to a neighborhood for the first time.[5,6] These fixed costs may include "real" costs such as longer travel times or safety risks when visiting a location for the first time or "psychological" costs such as having to exert greater effort to process the novel neighborhood's stimuli.

From the lens of the benchmark model, the average cross-sectional familiarity premium equals the fixed cost plus a "sorting" term that captures that the agent may have a different level of average utility for familiar neighborhoods compared to unfamiliar ones. That is, the model says that participants may be reluctant to work in new neighborhoods both because these neighborhoods may have worse quality than the ones they are familiar with and because there may be some cost of visiting a neighborhood for the first time. This implies that the strong cross-sectional premium that we find in the data is the result of some combination of

[4]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.

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

[6]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.

these forces.

Next, we show show that exposure to an initially unfamiliar neighborhood on average changes the agent's utility of working there by 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. This relies on correct priors, a sufficiently large sample, and the fact that expected utility maximizers are risk-neutral *over utilities*, even if they may be risk-averse over choice attributes. (We discuss below whether incorrect priors and the possibility of tail risk in finite samples can explain our results.) Thus, in this benchmark model, uncertainty over neighborhood quality and risk-aversion do not imply an effect of exposure on the average revealed utility of working in a neighborhood.[7,8]

Combining these results allows us to decompose the cross-sectional premium into the contribution of fixed costs and neighborhood quality by comparing it to the experimental premium. The magnitude of the experimental premium identifies the size of the fixed costs, and any difference between the size of the two effects is the result of sorting based on neighborhood quality.

Thus, our result that the familiarity premium and experimental exposure effects are equal rules out sorting under the benchmark model, consistent with the descriptives above showing seemingly idiosyncratic patterns of past exposure. 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.[9] Through the lens of the benchmark model, the entire effect of exposure and the cross-sectional premium are the result of fixed costs, not reducing uncertainty over neighborhood quality.

In our fourth step, we consider whether relaxing the assumption that our participants have correct beliefs about the experience of working in a neighborhood can explain our results. For example, if individuals hold overly pessimistic priors, they will update positively after exposure. In the language of the benchmark model, this will inflate both the cross-sectional premium and the experimental exposure effect by the same amount. This mechanism is closely related to the original affective conditioning explanation for the mere exposure effect (Zajonc 1968). We find some evidence for this channel: participants' beliefs

[7]Even absent fixed costs, reducing uncertainty may affect later *behavior*, which may be a non-linear function of utility, and lead to persistent effects.

[8]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.

[9]This does not necessarily imply that familiarity patterns are sub-optimal. We do not observe real-world benefits so cannot rule out higher benefits from familiar neighborhoods. We hold benefits of working in a neighborhood constant through our experiment design.

about labor market opportunities and safety are more pessimistic about unfamiliar neighborhoods before exposure but converge with those for familiar neighborhoods afterwards.

To further test between the importance of neighborhood quality and fixed costs while allowing for incorrect beliefs, we assess whether individuals change their choices after learning they will visit a neighborhood, but before actually visiting. This timing of information revelation helps us distinguish between participants who hold inaccurate beliefs and those who anticipate a fixed cost of exposure. If results are driven by incorrect beliefs, the announcement of future information should not change choices because it does not affect information at the moment when the choice is made.[10] In contrast, if fixed costs are the mechanism shaping choice, agents should alter their choices. Announcing the training location 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 implement this test, we use binary job choice questions during training days to elicit preferences over jobs three days later. Participants make these choices after having been informed of where they will train on that day but before they have actually visited that location. Individuals partially anticipate the benefits of exposure. 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 $[30\%, 101\%]$. This suggests that uncertainty about neighborhood quality is unlikely to explain our entire effect, even when allowing for incorrect priors.[11]

Next, we consider whether our results can be explained due to neighborhood quality tail risk and the finite nature of our data. The benchmark model implies that uncertainty over unfamiliar neighborhood characteristics does not imply an effect of exposure. Importantly, this type of uncertainty is distinct from risk incurred during first visits, which the model interprets as a fixed cost and we discuss below. In the model, the risk that a neighborhood has an undesirable, persistent attribute cannot explain changes in average utility valuations after exposure.

One potential issue is if neighborhoods have a very low risk of being (persistently) ex-

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

[11] Another explanation is that workers expect that if they learn during training that the neighborhood is a worse place to work in than their outside option, they can later opt out of showing up for work. However, our data shows that individuals have a very strong preference for employment over unemployment. Respondents only choose to decline both job offers in 0.16% of the binary choices and refuse their assigned jobs 0.35% of the time. Thus we think it is unlikely respondents considered the possibility of opting out of showing up in the future while making choices.

tremely bad quality, then this type of realization may not occur in our finite sample of exposure events. We think this is highly unlikely to explain our results for several reasons.

First, we believe the type of neighborhood quality risk needed to justify our results is implausible. Study participants were trained in 1,200 initially unfamiliar neighborhoods without any observed incidents, which means that the risk must be very low. This then implies that, to justify the familiarity premium we observe, the tail risk neighborhood quality must be extremely bad, most consistent with bodily harm. However, to justify the fact that a short, one-time exposure entirely erases the premium, it must also be something that is easy to recognize and rule out based on spending an hour in the neighborhood, otherwise subjects would remain somewhat reluctant to return. Furthermore, when we estimate neighborhood level average utilities, we do not observe any outliers, which means that the tail quality event must not be consistently observed or experienced when visiting a neighborhood, and instead affects some individuals but not others. This means that to explain our results it would need to be a serious, rare, easily recognized and specific to the individual's experience of the neighborhood.

Second, even when we restrict the analysis to unfamiliar neighborhoods that respondents confidently believe are safe – around a third of all initially unfamiliar neighborhoods – we still find similarly sized familiarity premium and an exposure effect. Third, as noted above, individuals change their choices in response to learning that they will have visited a neighborhood. If the baseline familiarity premium was driven by potential neighborhood quality tail risk, respondents should still be concerned about this possibility in the anticipation elicitation, and their choices should be unaffected.

Given that both the benchmark model and the anticipation results imply the premium we observe cannot be explained fully by neighborhood quality, we next consider what fixed costs, "real" or psychological, 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 whether the fixed cost may be due to risks associated with first-time exploration. While we cannot completely rule out this kind of risk, four pieces of evidence suggest it is unlikely to account for our findings. First, the experiment entails minimal risk:

employment and compensation 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. We conduct a back of the envelope calculation showing that for a risk rare enough to not occur in our sample, and still explain the familiarity premium, its associated utility cost needs to be equivalent to at least three months of wage income. Third, we also do not find an effect of exposure on getting lost, a plausible precursor for more severe exploration risks. Finally, in order to be consistent with the premium disappearing after one visit, any kind of exploration risk would have to fully disappear after one visit. Thus, for exploration risk to explain the result it would have to be rare, severe, unrelated to getting lost, and fleeting.

Instead, we believe our results are most consistent with two types of "psychological" fixed costs. First, while we do not find evidence that actual exploration risk or navigation costs fully explain the familiarity premium, incorrect beliefs about these quantities could play a role. Second, 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, exposure to a neighborhood helps individuals learn how to process the neighborhood's stimuli, leading to increased processing fluency. This increase in fluency may then improve individuals' willingness to work in a neighborhood. These mechanisms could also be interrelated, for example if greater fluency in processing a neighborhood makes it feel safer.

In our fifth step, 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: cross-sectionally, with a different job offer, and by observing travel after the experiment. First, we estimate the impact of familiarity on choices separately based on when the respondent last visited the location. We find that the effect of familiarity is relatively stable for locations visited within the last three years. Second, we measure whether participants in our sample take up paid opportunities two to four months after the intervention. We invite each participant to a short survey, varying the amount they will get paid and the neighborhood where they need to show up. This is a take-it-or-leave-it offer, allowing us to measure extensive margin decisions. We repeat this exercise six times per individual, varying the neighborhood and wage. We find that baseline neighborhood familiarity strongly predicts show-up (all else equal), and the experimentally induced exposure is positive, roughly two thirds in magnitude, yet imprecisely estimated because of ceiling effects at high wages.[12] These results suggest that exposure to a baseline unfamiliar neighborhood

[12]These results hold for wage offers $\leq$ 500 KSH. For wages $\geq$ 600 KSH, show-up is flat around 80% and

may have a persistent effect in the medium run. Third, we use an endline survey and SMS surveys to measure where participants travel in general, outside of our study. Across both prompted and unprompted measures, we find that individuals return to the initially unfamiliar neighborhoods where they were trained for a variety of reasons, including searching for work and non-work reasons, covering fun/leisure and errands. These results are a revealed preference argument that participants found exposure to these neighborhoods valuable.

In our sixth and final step, we consider how prior exposure affects the *consideration* of locations as employment options. If individuals form mental representations of their urban surroundings based on their past exposures (Lynch 1960; O'Keefe and Dostrovsky 1971; Fyhn et al. 2004; Schank 1982; Simon and Gilmartin 1973; Mack and Palmeri 2024; M. K. Ho et al. 2022; Thalmann, Souza, and Oberauer 2019; Bordalo et al. 2025), they may be less likely to include unfamiliar locations in their consideration sets when deciding where to travel.

To assess this possibility we use an "open" elicitation for half of the sample where subjects must generate potential employment locations rather than being confronted with binary choices. Specifically, 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 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.[13]

Using this data, we first analyze the predictors of whether a neighborhood is mentioned in the list.[14] 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.

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.

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.

[13]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).

[14]In the logit consideration model from Goeree (2008), this is the same as estimating consideration. This happens because respondents will rank all neighborhoods in their consideration set, given that at the wages we offer, they prefer working in any neighborhood to not working at all.

Banerjee 1992; Eyster and Rabin 2010; Hanna, Mullainathan, and Schwartzstein 2014; Malmendier 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). This implies 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 et al. 2025). We extend this literature by focusing on a naturalistic, high-stakes task and demonstrating the separate contributions of experience 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; Vitali 2024; 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; Zárate 2022; 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.

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 2017b; Okunogbe 2024; McKenzie 2024). 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, consistent with a tradeoff between average returns and risk. One particularly relevant example is Brady and Mcnulty (2011) which studies the effect of consolidated polling places in Los Angeles during the 2003 gubernatorial recall election. The authors use a matching design to show that being assigned a new polling place increases the probability of not voting by approximately 2% regardless of the change in travel time to the new polling place. Our paper is particularly relevant for recent work that proposes aversion to rural to urban migration falls with sustained 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; Porcher 2022; Wiseman 2023; Porcher, Morales, and Fujiwara 2024).

2 Context and Participant Sample

Our study takes place in Nairobi, a city of 4.4 million people that doubled in size over the past two decades (KNBS 2019). Much of this growth has happened in an unplanned fashion. For example, informal settlements now house about 70% of the population (Gachanja et al. 2023). This growth has also resulted in a city that is poorly served by its transportation infrastructure. Avner and Lall (2016) estimate that a more efficient layout could double residents' labor market access. This is typical of the rapid and disconnected growth found in many African cities (Lall, Henderson, and Venables 2017).

2.1 Sample: Casual, Underemployed Workers

We recruited study participants in three low-income neighborhoods (Kibera, Kawangware, and Viwandani, see Figure 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. 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. 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 dropped 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 discuss these eligibility requirements in section 4.

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

Sample Descriptive Analysis 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 7.9 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 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.[15]

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.1 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:[16]

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

[15] Appendix A.3 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.12).

[16] 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.

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 objectively accessible, starting with our sample of neighborhood at most 75 minutes away from the respondent's home neighborhood.

Figure 2 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 2), and the last three columns further restrict to only neighborhoods within 7.8 kilometers of the participant's home neighborhood, which is the median commuting distance that participants report who travel outside of their home neighborhood in order to work or search for work.

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 2 uses this definition). We later also ask participants if they know how to get to the neighborhood and whether they can list a landmark for the neighborhood, regardless of whether they have ever been there. 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). The lower levels of familiarity for these more demanding definitions of familiarity are important because we later show in Section 5.1 that these measures are strong predictors of participants' willingness to work in a neighborhood.

Third, familiarity increases as we restrict to the sample of neighborhoods closer than the median commute distance among those who work or search for work outside of their home neighborhood, which is 7.8km. This restricts the sample of neighborhoods to 10-16 neighborhoods that are closest to the home neighborhood. The median participant has

visited 69% of these neighborhoods, although they can only name a landmark for 48% of the neighborhoods in this restricted sample. Note that half of these respondents travel further than this cutoff on a regular basis to work or search for work, so the results in the last three columns of Table 1 offer an upper bound on familiarity of objectively accessible neighborhoods.

To provide a more granular view of familiarity patterns, Figure 3a 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 blue 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 idiosyncratic variation in familiarity patterns. There are few neighborhoods that are very familiar or very unfamiliar, and vice-versa there are few participants who know most or none of the neighborhoods. This is suggestive 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 3b 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. We begin with the standard gravity predictors of neighborhood fixed effects and distance from home neighborhood. These predictors 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.[17] These results suggest that, as far as we can observe, individual-neighborhood match characteristics are not important drivers of exploration.

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

[17]We also analyze whether Nairobi residents partially sort to destination neighborhoods based on ethnicity, and whether this explains the familiarity patterns we document here. 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. We regress an indicator for whether an individual has ever been to a neighborhood on distance from their home and an indicator for whether they share the same ethnicity as the plurality of the neighborhood. While shared ethnicity alone is predictive, this effect vanishes after controlling for distance. In addition to being insignificant, the point estimates in columns 3 and 4 suggests that sharing the same ethnicity as the plurality of the neighborhood is approximately equivalent to a neighborhood being between a sixth and a third of a kilometer closer.

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 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. In these jobs, participants are asked to collect data on air quality in a specific neighborhood. These are jobs that obviously require being in a specific location, and they make it plausible to vary the location where the participant is working. We use these jobs for two purposes. First, we subtly induce familiarity with certain neighborhoods by randomizing whether training for these jobs takes place in familiar or unfamiliar locations. Second, we elicit participant choices over jobs in different locations. Given that we compensate participants for the work they do, these choices are real-stakes.

To complete the task individuals wear a backpack used in Berkouwer and Dean (2024) (shown in Figure A.2), 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.[18] After arriving at the location, participants use an app on the phone to begin data collection and confirm they are in the correct place. 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.3 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 respond to the first baseline survey providing demographic information and answering the familiarity questions discussed

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

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 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 a field guide accompanies the participant to the target neighborhood for that day and trains them on how to complete the air quality measurement task. The guide then leaves, and the participant continues work for one hour and 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 5.3. 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 5.3.

Target Neighborhoods. After the first baseline survey, we use an algorithm to 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. We then randomly designate 3 familiar and 3 unfamiliar neighborhoods as "main" familiar and "main" unfamiliar neighborhoods, respectively. Figure A.4 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 an already familiar neighborhood. The remaining *unfamiliar* neighborhood allows us to identify any potential spillover effect of increased willingness to visit unfamiliar neighborhoods.

We select the ten target neighborhoods using an algorithm that we run 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. (See appendix A.4 for details.)

Table A.8 shows that familiar and unfamiliar target neighborhoods chosen using this procedure are very similar. We find no differences on labor market and safety measures, unfamiliar neighborhoods are slightly (around 10%) more distant on average than familiar neighborhoods, and the two groups have almost identical average revealed utility of working in the neighborhood.[19]

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

[19]Formally, we use as outcome the estimated neighborhood fixed effect from Table 2 column 3.

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

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. On average, 98.7% of participants who showed up for their first day complied with their treatment assignments and completed the three days of training. However, there is a small imbalance between treatment and control group, with 0.6-1.6 p.p. more participants in the treatment group missing days 2 and 3 of training, with this difference growing over time (Table A.9). All analyses are based on intention to treat.

We re-measure familiarity when participants return from the training to the study venue. We ask each participant if they had already been to the assigned neighborhood before the day of the training. 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. This measures the role of whether a participant perceives a neighborhood as familiar when asked about it. We may understate the role of familiarity if our baseline familiarity data is noisy.[21] Depending on the source of discrepancy between the original data collection and the revised measure, and based on the desired notion of familiarity, it may be appropriate to use the treatment assignment to instrument for familiarity. This is roughly equivalent to inflating our later estimates by a factor of 1.85 (which is one divided by the first stage effect of 0.54). For example, this would be the case if the source of discrepancy is that participants make idiosyncratic mistakes when they respond to our baseline survey. However, to be conservative, we do not compute the IV in our analyses.

Eliciting Job Preferences and Consideration. We randomize the way we elicit job choices during the three employment days that follow the training period in order to study the relative importance of familiarity in shaping *preferences* of where to work and the formation of *consideration sets*.

In the first "preferences" elicitation method, we use binary choices where individuals choose between two potential jobs given information about their location, duration, wage, etc. These structured choices allow us to price the disutility of traveling to an unfamiliar

[21] However, in an earlier pilot study we found our measure of familiarity to have high test-retest reliability.

location, but 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, in the second, "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. If it is the case that unfamiliar neighborhoods are less likely to enter individuals' consideration sets, we should expect that respondents are more likely to omit or rank unfamiliar neighborhoods later than we would expect based on their preferences alone.

We also assess whether individuals anticipate how their utility of visiting a neighborhood will change after having visited it once. In section 5.2 we describe this procedure in detail and explain how we use this variation to assess the degree to which (possibly incorrect) beliefs about neighborhood quality versus fixed costs can explain our results.

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 variants of the 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 other job attributes, 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, before answering this type of question for the first time, the surveyor goes through a practice session with the participant.

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. By design, each question forces the respondent to consider working in the two neighborhoods. We interpret this as meaning that we estimate preferences, and we return to the issue of whether unfamiliar neighborhoods are less likely to enter

consideration sets in section 6, using a different "open" elicitation method for the other half of participants.

Job Choice Model and Estimation. We use the binary choices between potential jobs to estimate a random utility model.[22] 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 state that there are no cross-neighborhood spillovers in the utility of working in a neighborhood involving familiar neighborhoods. The first assumption says that when the neighborhood visited during training was already familiar at baseline, no other target neighborhoods are affected in terms of their utility of working there. Note that this is not a statement on the probability of deciding to work in one of these neighborhoods, because visiting a familiar neighborhood may make it *relatively* more or less attractive than other neighborhoods. The second assumption states that training visits to unfamiliar neighborhoods do not affect the utility of working in non-visited neighborhoods that the participant was already familiar with at baseline. Note that these assumptions do not preclude participants in the treatment group becoming more open to unfamiliar neighborhoods in general. Rather, we focus on assuming that they have "made up their minds" about familiar neighborhoods.[23]

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 $j \in$

[22] We also allow participants to turn down both jobs. Due to the underemployed sample and the competitive wages, this was extremely rare. We study extensive-margin decisions for a different type of work opportunities in section 5.3.

[23] 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

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

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

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.

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 correct prior p_i over u_{in} for unfamiliar neighborhoods.[24]

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

[24] 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.

being in a new place.

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 in the experiment.

Our first result is that the coefficient β^F on baseline familiarity from equation (1) identifies a mix of sorting and average fixed cost. Through the independence of treatment assignment and repeated application of the law of iterated expectation, we have

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

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

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

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

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

In this model, uncertainty about the utility of unfamiliar neighborhoods does not contribute to the familiarity premium. 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 neighborhood attributes. We discuss tail risk and finite sampling at the end of this section.

This benchmark model implies that on average across neighborhoods and across individuals, the expected revealed utility of working in an unfamiliar neighborhood equals the

average ex-post revealed utility after exposure, minus the average value of the fixed cost of exposure. We bring this model to the data using the specification (1) without neighborhood effects, so as to not condition in our analysis on the realized value of average utility in specific neighborhoods.[25]

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. 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 an 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 *cross-sectional familiarity premium* is a large amount given that the median daily wage outside our the experiment is 500 Ksh. We can also benchmark the cross-sectional familiarity premium with respect to distance and job duration. Working in a neighborhood that is familiar at baseline is valued equally to working in an unfamiliar neighborhood that is similar in all respects except 3.54 kilometers closer or having to work for more than an hour less.

These effects suggest that familiarity increases an individual's willingness to work in a neighborhood. Through the lens of the model this is due to a combination of sorting on unobserved preference heterogeneity and fixed costs. To decompose the role of these two forces, we now turn to the exposure experimentally induced by training.

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

[25]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 neighborhood, our result still applies and agents should be correct about the utility of working in an 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.

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.

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.

Discussion. We first interpret these results through the lens of our benchmark model and then consider potential deviations from the assumptions. The fact that the cross-sectional familiarity premium and the exposure effect are very similar in size means that at baseline, on average, participants do not have a systematically different utility cost of working in familiar vs unfamiliar neighborhoods. That is, we do not find evidence of sorting. It is important to mention that this is not a statement about whether familiarity patterns are optimal or not. The reason is that 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.) What it does imply is that, through the lens of the model, the effect of prior exposure that we observe is entirely driven by fixed costs.

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 the next section, we look at whether this fixed cost is a "real" cost such as navigation time or 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 during exploration below). If this rare realization does not occur in our finite sample of exposure events, this might explain why we observe participants initially reluctant to visit unfamiliar neighborhoods but this vanishes after training. We now explain why we believe this is very unlikely to explain our results.

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. We conducted almost 1,200 training sessions in initially unfamiliar neighborhoods without any observed incidents. Thus the risk would need to be very rare in order for us to not observe it in our sample. This in turn implies that, to explain the cross-sectional familiarity premium we estimated, the tail risk neighborhood quality must be extremely bad, which is most consistent with bodily harm or death. 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 an hour in the neighborhood during training. Finally, we do not find any outlier neighborhood which participants very strongly dislike working in. Figure A.5 shows the distribution of neighborhood fixed effects estimated in column 3 of Table 2. There are no outliers, with the worst neighborhood having a (precisely estimated) disutility of around 125 KSH relative to the mean. This means that any tail risk must be individual-specific.

To further test whether serious neighborhood risk may explain participants' reluctance to work in unfamiliar neighborhoods, we separate our analysis by baseline beliefs about safety in unfamiliar neighborhoods. 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. To do so we define a variable for unfamiliar neighborhoods that the participant rates 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.12 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.

Additional Results. 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.13). Next, we find similar effects of familiarity, both cross-sectional and experimental, regardless of whether the neighborhood is close or far to the participant's home neighborhood (Table A.14). We find that familiarity matters more for the men in our sample, even after adjusting for a slightly higher effect of job duration and wages, but that the single exposure erases the premium for both men and women (Table A.15).

One exception is that we find our effects are stronger for more familiar neighborhoods. In Table A.16, 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 for more familiar neighborhoods.

Do participants trained in unfamiliar neighborhoods become more willing to work in other unfamiliar neighborhoods? To study this, we add the tenth target, unfamiliar neighborhood to the estimation sample and estimate a separate coefficient for its interaction with treatment. This neighborhood is unfamiliar and was not included in the training. Table A.17 shows the results. A positive coefficient indicates positive spillovers to other unfamiliar neighborhoods. In column 3, we find a positive yet imprecisely estimated coefficient of 0.27 with a standard error of 0.18.

Finally, we examine whether visiting neighborhoods makes any individuals less willing to return. We estimate heterogeneous treatment effects by neighborhood and individual using the machine learning methods proposed by Chernozhukov et al. (2023). This method allows us to systematically search for any group who may have been less willing to return to a neighborhood after visiting it. Specifically we focus on choices between a non-target familiar and an unfamiliar location and regress an indicator for choosing to work in the unfamiliar location on treatment status. We then estimate the Grouped Average Treatment Effects for both a median and a quartile split. Figures A.6 and A.7 present the results. We find that across all groups the treatment effect point estimates are positive and we can reject large negative effects.

5.2 Mechanisms

In this section we consider what mechanisms may drive this effect of exposure. Under the benchmark model, our results imply that fixed costs must explain the effect of exposure that we observe. We begin by considering whether deviations from the benchmark beliefs model can explain our results, and then turn to two sources of potential "real" fixed costs and alternative psychological explanations.

Non-standard Beliefs. Can deviations from correct beliefs explain our results? For example, if individuals hold overly pessimistic priors they will 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. Asking both questions allows us to distinguish if a participant believes that a neighborhood that is unfamiliar does not offer good job opportunities in general or specifically for them. 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.5 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:

$$\text{Belief}_{int} = \gamma_n + Post_t + Pre_t \times Treated_i + Post_t \times Treated_i + \epsilon_{int}.$$

$Belief_{int}$ is the belief or certainty rating from 1 to 5, for one of the five belief outcomes described above.

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 dummy with pre- and post- dummies to compare how beliefs vary across familiarity and across time.

Table 4 shows the results. At baseline, individuals are on average more pessimistic about unfamiliar neighborhoods, as indicated by the negative $Pre_t \times Treated_i$ coefficients. For example, respondents rate unfamiliar neighborhoods 0.24 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 $Post_t \times Treated_i$ 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.[26]

Do Subjects Anticipate How Their Preferences Change After Exposure? To further explore the role of beliefs about neighborhood quality as a mechanism while allowing

[26]For the interested reader, we present additional results on the beliefs in Appendix A.6

for incorrect beliefs, we assess whether individuals change their choices for future employment after learning they will visit a neighborhood. The announcement that the agent will acquire information in the future should not change choices because it does not affect information available when the choice is made. In contrast, if fixed costs are the mechanism shaping choice, agents should alter their choices. The training location 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, agent's 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. Study 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 analyze these choices and report how much a participant who has already visited a neighborhood changes their willingness to work there, compared to the effect of only learning that they are about to visit it.

We estimate a similar logit model as previously, but we now include the choices made 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

$$\begin{aligned} u_{ijn} = & \beta^F Familiar_{in} + \\ & \underbrace{\beta^V Train_{in}}_{\text{training effect}} + \underbrace{\beta^U Train_{in} \times Unfamiliar_{in}}_{\text{exposure effect}} + \\ & \underbrace{\beta^{AV} AnticipTrain_{in}}_{\substack{\text{anticipation} \\ \text{training effect}}} + \underbrace{\beta^{AU} AnticipTrain \times Unfamiliar_{in}}_{\text{anticipation exposure effect}} + \\ & \underbrace{\gamma X_{in}}_{\substack{\text{randomized} \\ \text{job attributes}}} + \beta^D d_{in} + \epsilon_{ijn} \end{aligned} \tag{2}$$

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 correct priors and anticipate the fixed cost, which predicts $\beta^{AU} = \beta^{U}$.

We estimate equation (2) using a binary logit model. Table 5 shows the results. We use the pooled data on training days and employment days. We find $\beta^{U} = 0.77$ and $\beta^{AU} = 0.48$. The ratio β^{AU}/β^{U} of the two coefficients provides a measure of to what degree individuals incompletely anticipate the utility change. The ratio is 0.63 and the p-value for equality is 0.026. 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 made for the same day. In column 3, we leverage variation in realized familiarity induced by training on previous days. 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 incorrect priors over neighborhood quality cannot fully explain our results.

An alternate explanation is that the positive β^{AU} reflects option value. In this scenario, 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 both job offers in 0.16% of choices.

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 what fixed costs, both "real" and "psychological" may explain our results.

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 3 we regress the travel time it takes participants to reach a neighborhood where they work, on baseline familiarity and experimentally induced visits using a Heckman (1976) two-step probit model to control for selection into working in an unfamiliar neighborhood where we use the other randomly allocated job attributes, like compensation, 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 8 minutes in both directions. Training has a small effect on travel times, but we do not find any additional effect for training in

unfamiliar neighborhoods. Table 3 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. We do find a small decrease in stating that the trip was "more difficult than expected" after training. But overall, we find little support that first-time navigation costs could explain the large familiarity premium documented earlier.

Exploration Risk. Another potential source of fixed-costs is if visiting a new neighborhood for the first time is risky. While we cannot completely rule out this explanation of our results, we believe several pieces of evidence weigh against it. 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.

One concern might be risks that are sufficiently rare that we would not observe them in our experiment. To assess this possibility, we can do a back of the envelope calculation of how bad the event would have to be in order to justify the premium given the low probability. Specifically, suppose every time the agent explores there's a probability p_u of a bad outcome such that

$$p_u[u(good) - u(bad)] = 109$$

We observe 243 trips to unfamiliar locations on employment days among the control group with zero adverse outcomes. That means that we can reject, with 95% confidence, that $p_u > 1.2\%$. In order to justify a premium of 109 Ksh, this would imply that the difference in utilities between the good and bad events must be at least 9,083 Ksh or three months of wage income for our respondents.

Psychological Fixed Costs. While we believe the evidence suggests there are not real exploration risks or navigation costs experienced by the participants, it is possible that participants hold incorrect beliefs about these quantities. For example, agents might be overly pessimistic about how long it will take them to reach a location for the first time or that they are particularly vulnerable 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 psychological fixed costs like processing fluency (Jacoby and Dallas 1981; Jacoby and Whitehouse 1989; Winkielman et al. 2003). In this account, when exposed to a neighborhood, individuals learn how to process the neighborhood's stimuli leading to increased processing fluency. 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.

5.3 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.20 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 commuting in Nairobi, scheduled to take place in a given neighborhood two days later. Because the survey focused on commuting, 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.[27] Surveyors recorded whether the respondent showed up on the designated survey day. After initially launching these invitations with wages randomized between 500

[27]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.

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 invitation sequence (i.e., whether it was the participant's 1st, 2nd, etc., successful invitation). 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.10) and none of the neighborhood or job characteristics, including distance and wage, affect show-up. Second, we present the results after restricting the sample to the second round of invitations where wages were randomized between 100 and 400 KSH.

Table 6 reports the results. In the first three columns, we replicate the 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.[28] Distance and wages also matter for show-up for wages below 500KSH, 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.21).

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 1-4 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 columns 5 and 6 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.[29]

[28] When we estimate a model with individual-level random effects to absorb some of the variation in show-up across participants, the exposure effect is more precisely estimated (Table A.22).

[29] We also collect travel information in less prompted ways in the endline survey and present the results in Table A.23. In the first three columns, we ask participants to tell us where they have been for different

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.[30]

Participants return for a variety of reasons, including to search for work, as well as 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.[31] Furthermore, participants did search for work in these neighborhoods (column 2). Direct labor market outcomes may take time to realize.

Individuals do continue to revisit the neighborhoods that they visited during training, including those that were unfamiliar at baseline.

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. In this section, we study whether unfamiliar neighborhoods are less likely to be considered as potential places to work.

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

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.

[30] 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.

[31] We can only reject a +0.025 effect on the probability of working in a specific Training × Unfamiliar neighborhood, relative to a mean of 0.069.

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

$$\eta^C_{in} = \alpha^{CF} Familiar_{in} + \alpha^{CV} Train_{in} + \alpha^{CU} Train_{in} \times Unfam_{in} + \alpha^{CD} d_{in} + \mu_n + (\alpha^C)' X_i, \quad (3)$$

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 0.16% 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 (3), 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). This will tend to reduce, but not completely eliminate, losses due to partial consideration, relative to a model with random consideration.

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.[32] On days 2 and 3 the training coefficient is larger and experimental exposure no longer has an effect on consideration (Tables A.24 and A.25).

These results suggest that, at least initially, participants are less likely to consider unfamiliar neighborhoods, and a one-time short exposure reverses part of this effect. The main advantage of the logit consideration model is that 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. Specifically, because there is a 0.25 probability a job is available in any neighborhood, the chance that the choice a respondent makes when asked for their k^{th} preference is implemented is $0.25 \times 0.75^{k-1}$. Thus the value of generating additional neighborhoods for consideration depends on the position in the list.[33] 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.

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

[33] Indeed, we find that if we estimate a ranked logit with the lists, the preference rankings become attenuated as we include more elements from the list (Table A.26). 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.

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

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

where ν_{ijk} are iid shocks with variance $= \sigma_\nu \geq 0$. (Again, $\mathbf{X}_{in}$ 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 each step when the memory self is asked to transmit a neighborhood, they act myopically and send[34]

$$j_k^* \in \arg\max_j \lambda_k u_{in} + c_{ijk} \quad \text{or "stop" (assumed net utility } \lambda_k \varepsilon_{i0} + \nu_{i0k})$$

Estimation and Results. 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 variance of idiosyncratic cost shocks.

Table 9 reports the results. 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 utility cost incurred for each kilometer of distance the agent must

[34]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.

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 dispersion $\sigma \approx 2$, 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.27 and A.28).

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.

7 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 psychological fixed cost of exposure, which participants are partially aware of, and with incorrect beliefs about the fixed cost of exposure. Thus, from a modeling perspective, urban and spatial 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). We now discuss more general implications of our results beyond the immediate context in which we study it.

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

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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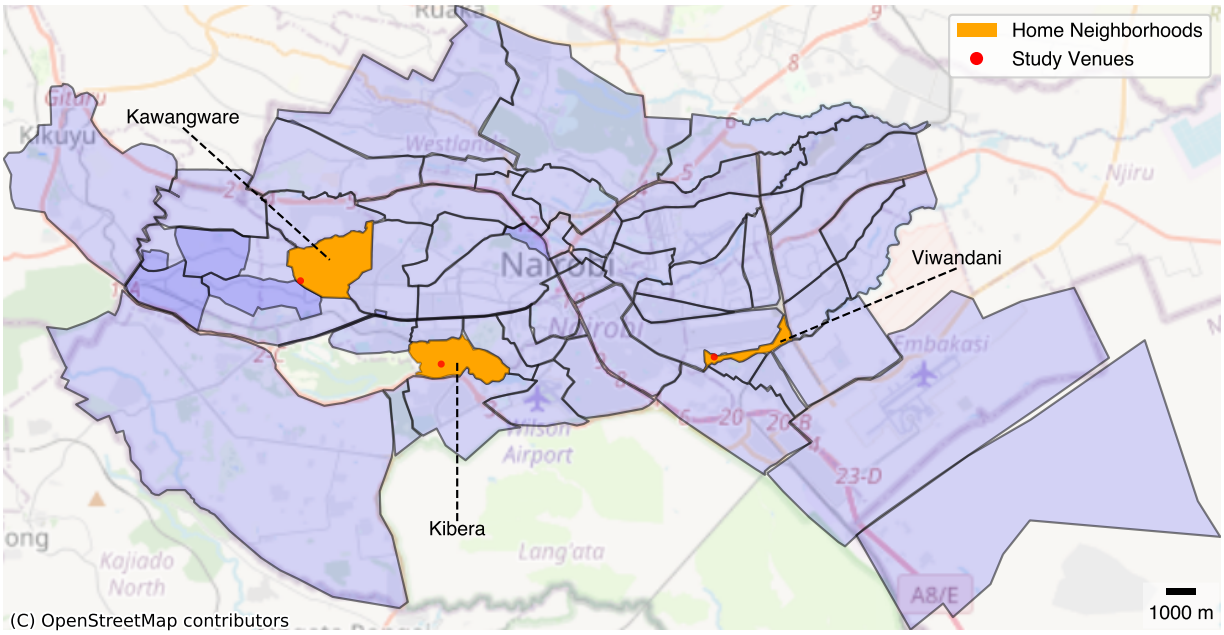
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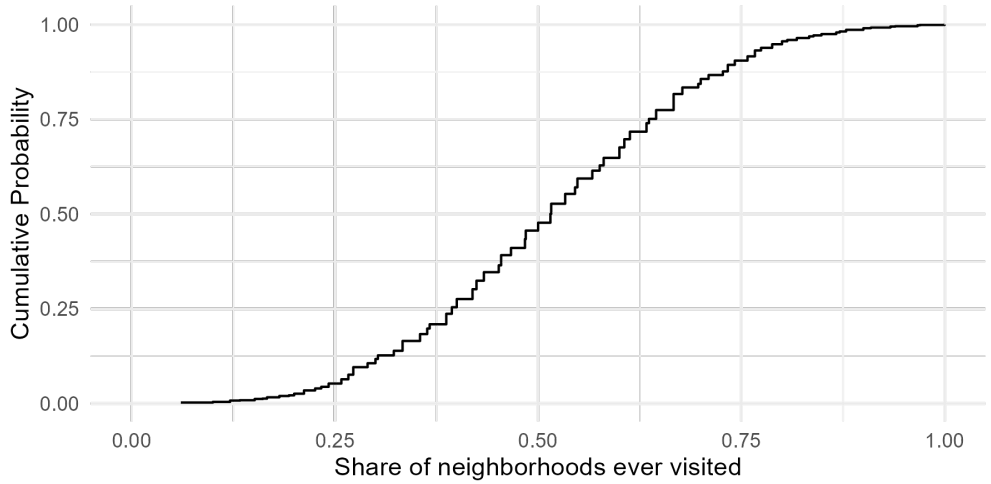
Figures and Tables

Figure 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.

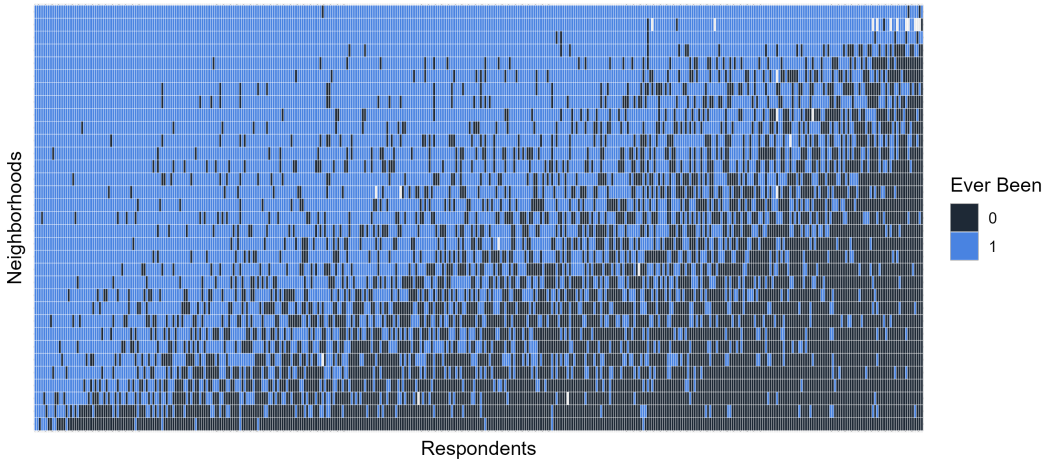
Figure 2: CDF of Familiarity Within 75 minutes from Home



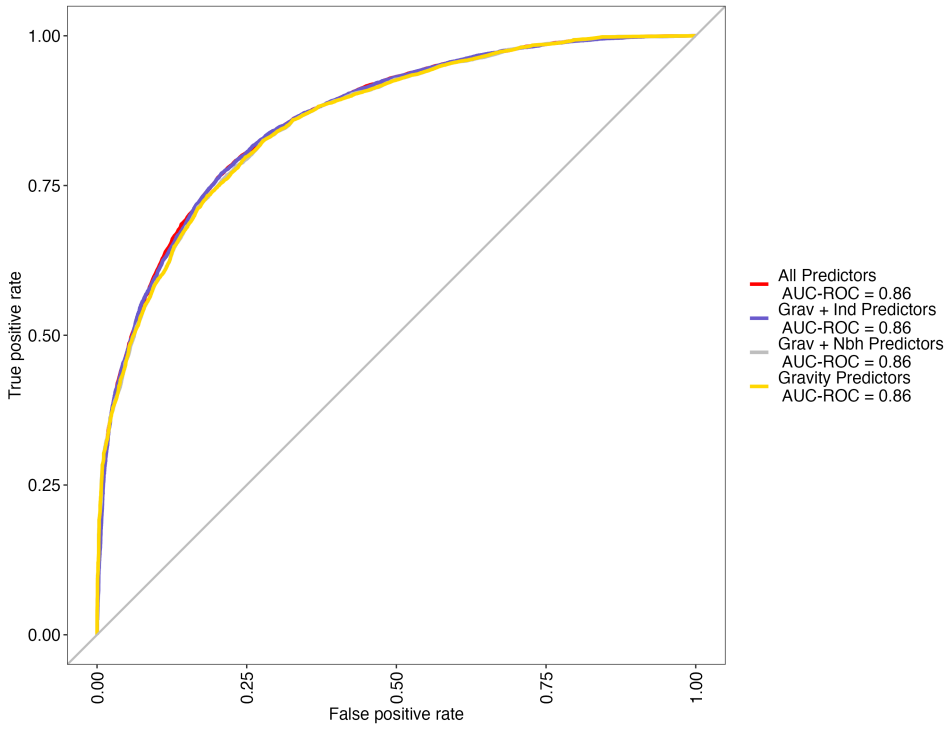
Notes: This figure plots the cumulative distribution function for our main measure of familiarity, across participants. The sample of respondents includes all 1,168 participants who completed the first baseline survey. The sample of neighborhoods is restricted to within 75 minutes (shortest among walking or by transit) of the respondent's home neighborhood. This amounts to approximately 30 neighborhoods for each of our three home neighborhoods. The X axis lists the share of neighborhoods N that a given respondent answers yes to the question "Have you ever been to the neighborhood of N."

Figure 3: Familiarity Patterns are Partly Idiosyncratic

(a) Idiosyncratic Variation in Familiarity (Kibera)



(b) No Evidence of Match-Specific Familiarity Patterns



Notes: Panel (a) plots the entire familiarity matrix for all study participants from Kibera who completed the first baseline, for all 30 neighborhoods that they were asked about. A cell is blue if the (column) respondent has ever been to the (row) neighborhood, and black otherwise. Neighborhood and respondents are ordered by average familiarity. Panel (b) shows the ROC curves for four different random forest models predicting whether a given individual is familiar with a given neighborhood. The standard gravity model predictors of distance and neighborhood fixed effects (yellow line) lead to a prediction better than chance. Including additional predictors such as individual and neighborhood characteristics (gray, blue, and red lines), does not meaningfully improve prediction. See Figure A.12 for further results using logit-lasso models.

Table 1: Participants Have Significant Familiarity Gaps

Nbhd sample:	Share of Neighborhoods Visited					
	< 75min			< 7.8km		
	p25	p50	mean	p25	p50	mean
Measure of familiarity:						
Heard of	0.89	0.94	0.92	0.90	0.97	0.93
Ever been OR passed by	0.50	0.63	0.62	0.62	0.79	0.74
Ever been	0.39	0.52	0.52	0.50	0.69	0.66
Ever been + knows get there	0.33	0.43	0.45	0.40	0.56	0.56
Ever been + gave landmark	0.24	0.34	0.35	0.33	0.48	0.46

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

Table 2: Revealed Preference Estimates of the Familiarity Premium

	(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 the "preferences" elicitation where individuals chose between two potential job offers which varied based on location, duration, total compensation, the amount offered in advance to ease liquidity constraints and the amount that depended on a risky bonus. "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. Standard errors at the individual level based on 500 bootstraps in parentheses. $^{*}p \leq 0.05$, $^{**}p \leq 0.01$, $^{***}p \leq 0.001$

Table 3: Selection Model For Duration and Navigation to Unfamiliar Neighborhoods

	Task App (hours)		Navigation (Post-Visit)				
	To Job	From Job	Get Lost There	Get Lost Back	Ask Directions There	Ask Directions Back	More Difficult Than Expected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline familiar	-0.06*	-0.07	-0.01	0.00*	-0.10***	-0.04**	-0.09***
	(0.04)	(0.04)	(0.02)	(0.00)	(0.04)	(0.02)	(0.03)
Training	-0.07**	-0.02	-0.05***	0.00	-0.18***	-0.01	-0.07***
	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Training × Unfamiliar	0.05	-0.02	0.01	0.01	-0.07	-0.03	-0.09***
	(0.06)	(0.06)	(0.02)	(0.01)	(0.04)	(0.02)	(0.03)
Distance (km)	0.09***	0.10***	0.00	0.00	0.01*	-0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Cons	0.51***	0.40***	0.07**	0.00	0.26***	0.07***	0.14***
	(0.05)	(0.07)	(0.03)	(0.00)	(0.06)	(0.02)	(0.04)
N	2,291	2,257	2,318	2,318	2,318	2,318	2,317
Control Mean	1.23	1.30	0.06	0.00	0.31	0.05	0.18

Notes: This table presents estimates from a Heckman (1976) two-step probit model applied to the work-choices bundles over the employment days. The sample includes participants in the closed elicitation group who completed the job during the employment period. In columns 1 to 6, odd-number columns refer to the trip to the job neighborhood, and even-number columns refer to the round trip to the home location. The dependent variables in columns 1 and 2 represent travel duration (in hours). Columns 3 and 4 use indicators showing whether participants got lost during their trips, while columns 5 and 6 use whether participants asked for directions. Column 7 takes the value of one if the participant considered the trip to the job destination difficult or a lot more difficult than initially expected, and zero otherwise. Bootstrap standard errors with 1000 replications in parenthesis. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table 4: Average Beliefs Converge After One Visit

	Dependent variable:				
	Find Job	Find Job (Others)	Pay is Good	Travel Safety	Overall Safety
	(1)	(2)	(3)	(4)	(5)
Constant	3.418***	3.427***	3.580***	4.030***	3.844***
	(0.037)	(0.035)	(0.040)	(0.035)	(0.036)
post	−0.134***	−0.189***	−0.055	0.140***	0.237***
	(0.039)	(0.039)	(0.037)	(0.033)	(0.037)
pre:treated	−0.247***	−0.249***	−0.050	−0.393***	−0.170***
	(0.056)	(0.053)	(0.058)	(0.053)	(0.053)
post: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
pre x treat = post x treat, p-val	0.003	0.004	0.492	0.000	0.001
Note:					*p<0.1; **p<0.05; ***p<0.01

Notes: This table regresses beliefs about attributes of the visited neighborhoods elicited at baseline two and after visiting on an indicator for the time point and an interaction with treatment status. In the analysis sample, those in the treated group are rating unfamiliar neighborhoods while those in the control are rating familiar 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.18 repeats the analysis including home by neighborhood fixed effects, and table A.19 repeats the analysis using belief confidence as the outcome. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table 5: 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	
Sample: only Q1			
Neighborhood FEs		Yes	Yes
Job Attribute Controls	Yes	Yes	Yes

Notes: This table reports binary logit estimation results of equation (2). 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 $d \in \{1, 2\}$. Coefficients on expected compensation, liquidity, bonus and distance are included but not reported to save space. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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.10). Columns 4 - 6 report results from the second round of invitations where all wages were below 500 KSH

Table 7: People Re-visit the Neighborhoods From the Study

	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	0.146***	0.062***	0.028***	0.001	0.007	0.018**	0.004	0.029***	0.016***	0.010**	0.062***
	(0.017)	(0.011)	(0.010)	(0.002)	(0.005)	(0.008)	(0.004)	(0.008)	(0.006)	(0.004)	(0.019)
Training	0.079***	0.037***	0.025**	0.004	−0.002	0.014	0.004	0.002	−0.003	0.017***	0.092***
	(0.020)	(0.014)	(0.012)	(0.003)	(0.006)	(0.009)	(0.004)	(0.008)	(0.006)	(0.005)	(0.028)
Training × Unfamiliar	−0.005	0.006	−0.021	−0.006	0.009	0.002	−0.006	0.018	0.010	−0.004	−0.028
	(0.035)	(0.023)	(0.021)	(0.004)	(0.010)	(0.015)	(0.007)	(0.015)	(0.012)	(0.009)	(0.044)
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	0.148
Training + Training × Unfamiliar	0.074	0.043	0.004	-0.002	0.006	0.016	-0.002	0.019	0.008	0.013	0.064
(se)	(0.02)	(0.013)	(0.011)	(0.001)	(0.006)	(0.008)	(0.004)	(0.009)	(0.007)	(0.005)	(0.021)
[p-value]	[0.000]	[0.001]	[0.683]	[0.162]	[0.269]	[0.043]	[0.660]	[0.026]	[0.264]	[0.005]	[0.002]

Notes: This table regresses whether individuals report returning to the target neighborhoods on an indicator for being familiar at baseline, whether any training occurred in the neighborhood, and whether the interaction of training and whether the neighborhood was unfamiliar at baseline. 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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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 (3). 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.24 and A.25 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 × 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.

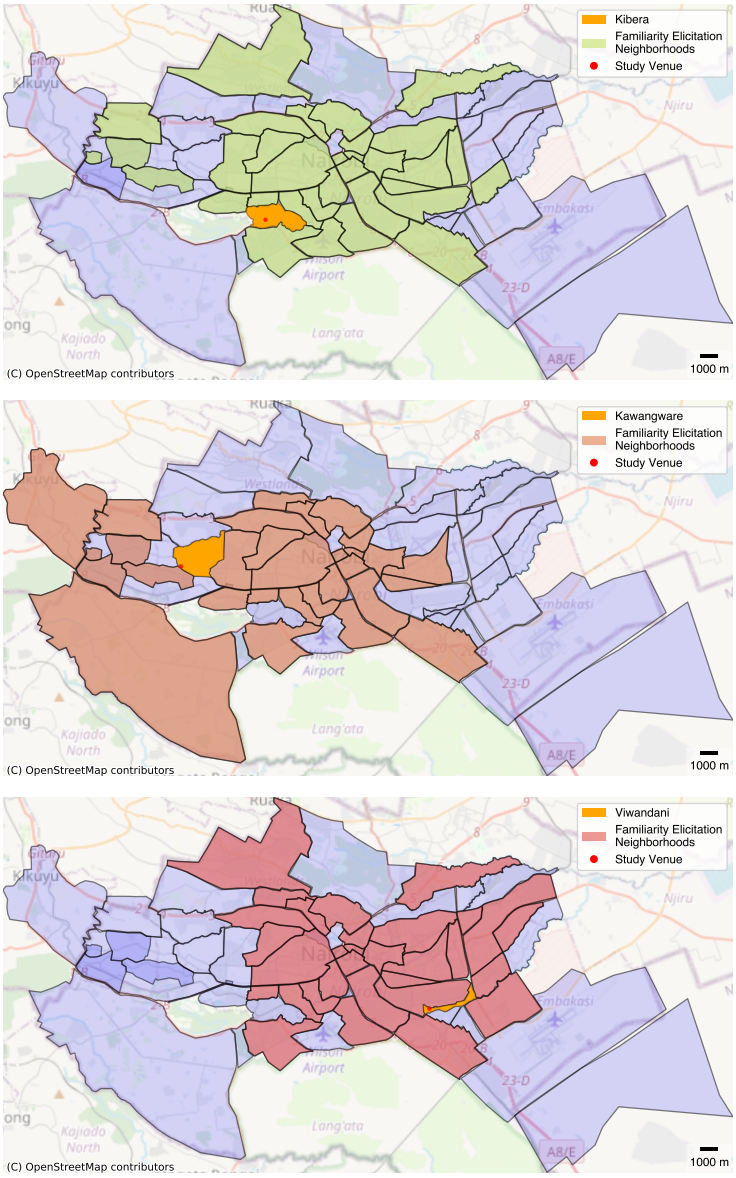
A Appendix - For Online Publication

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A.1 Appendix Figures

Figure A.1: Familiarity Elicitation Neighborhoods



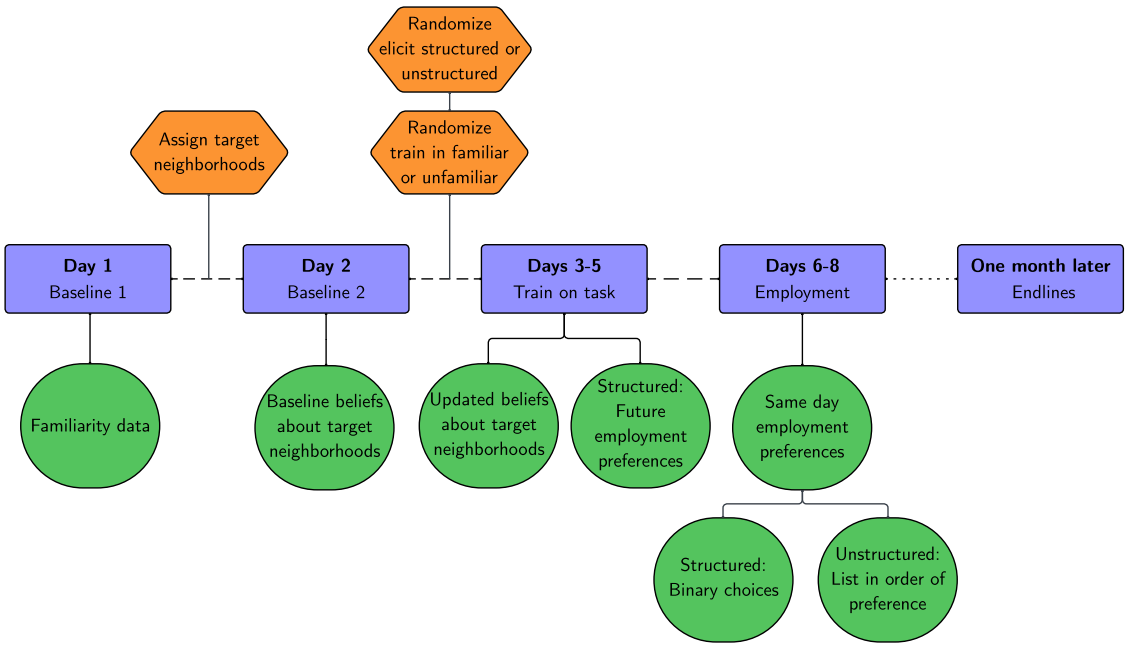
Notes: This figure replicates Figure 1 for each home neighborhood and highlights the set of main neighborhoods for which we elicited familiarity. We elicited familiarity in 33 neighborhoods of Kibera, 30 in Kawangware, and 31 in Viwandani. For each home neighborhood, these neighborhoods were reachable in at most 75 minutes from the study venue. A small number of neighborhoods were dropped due to safety or other concerns (See Appendix A.3 for a complete discussion of neighborhood selection).

Figure A.2: Backpacks Used in Employment Task



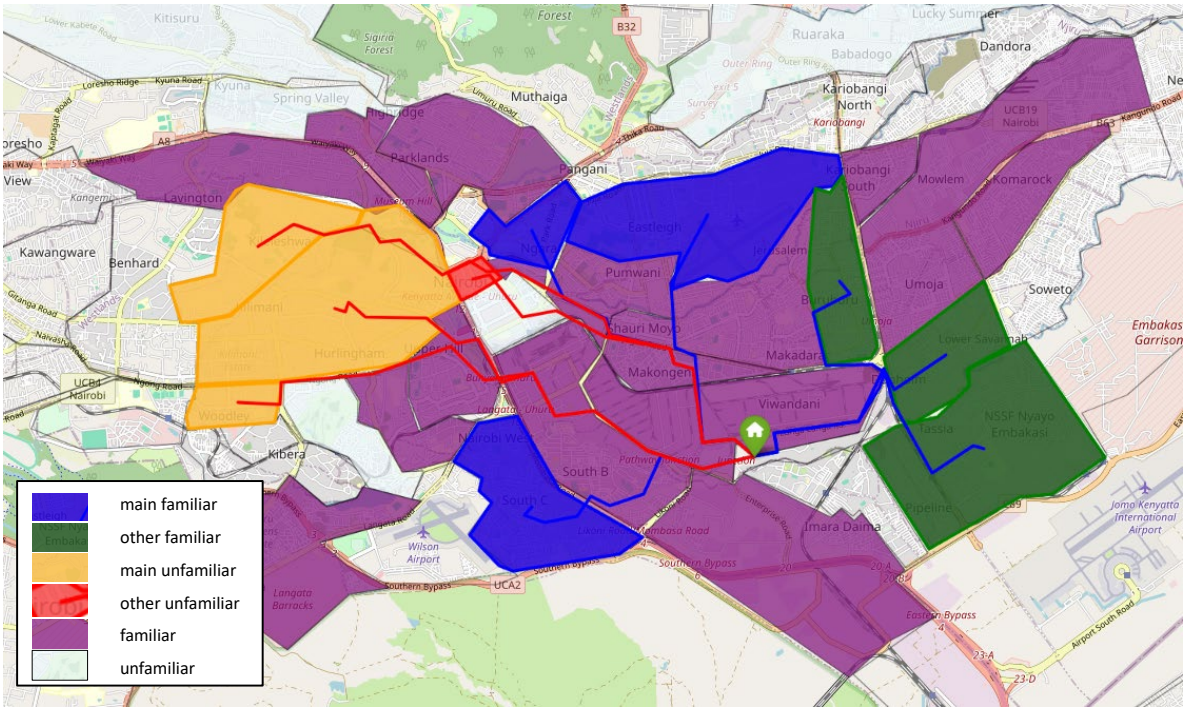
Notes: This figure shows a picture of an air pollution backpack used by study participants during training and employment days.

Figure A.3: Timeline of Study



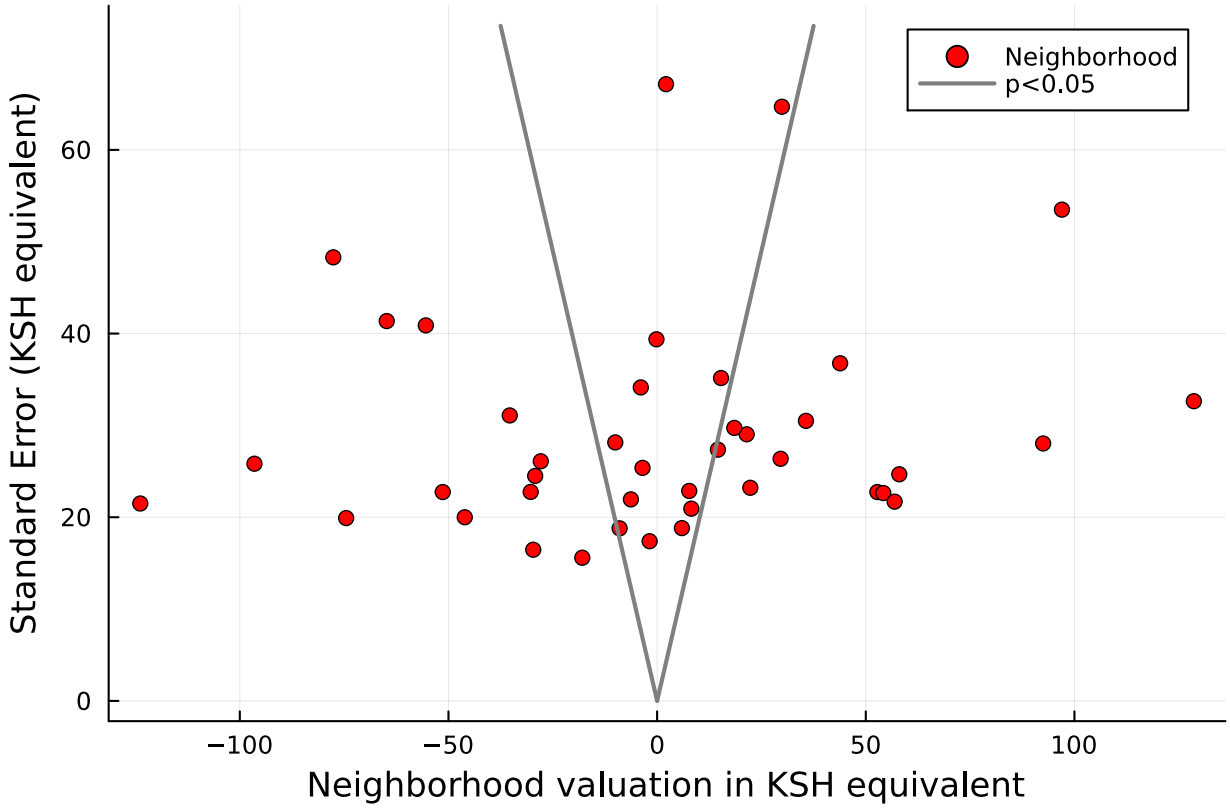
Notes: This figure shows the timing outline of the study. In the first two days, participants provided information about their neighborhood familiarity, demographics, beliefs about target neighborhoods, employment and job search status, and spatial ability. Over the next three days, participants were 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 "closed" 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" survey or "closed" binary choices—Section 4 discusses the "closed" 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 to inquire about their employment and job search status, spatial ability, and neighborhood trip patterns through SMS.

Figure A.4: Example of 10 Target Neighborhoods for One Participant



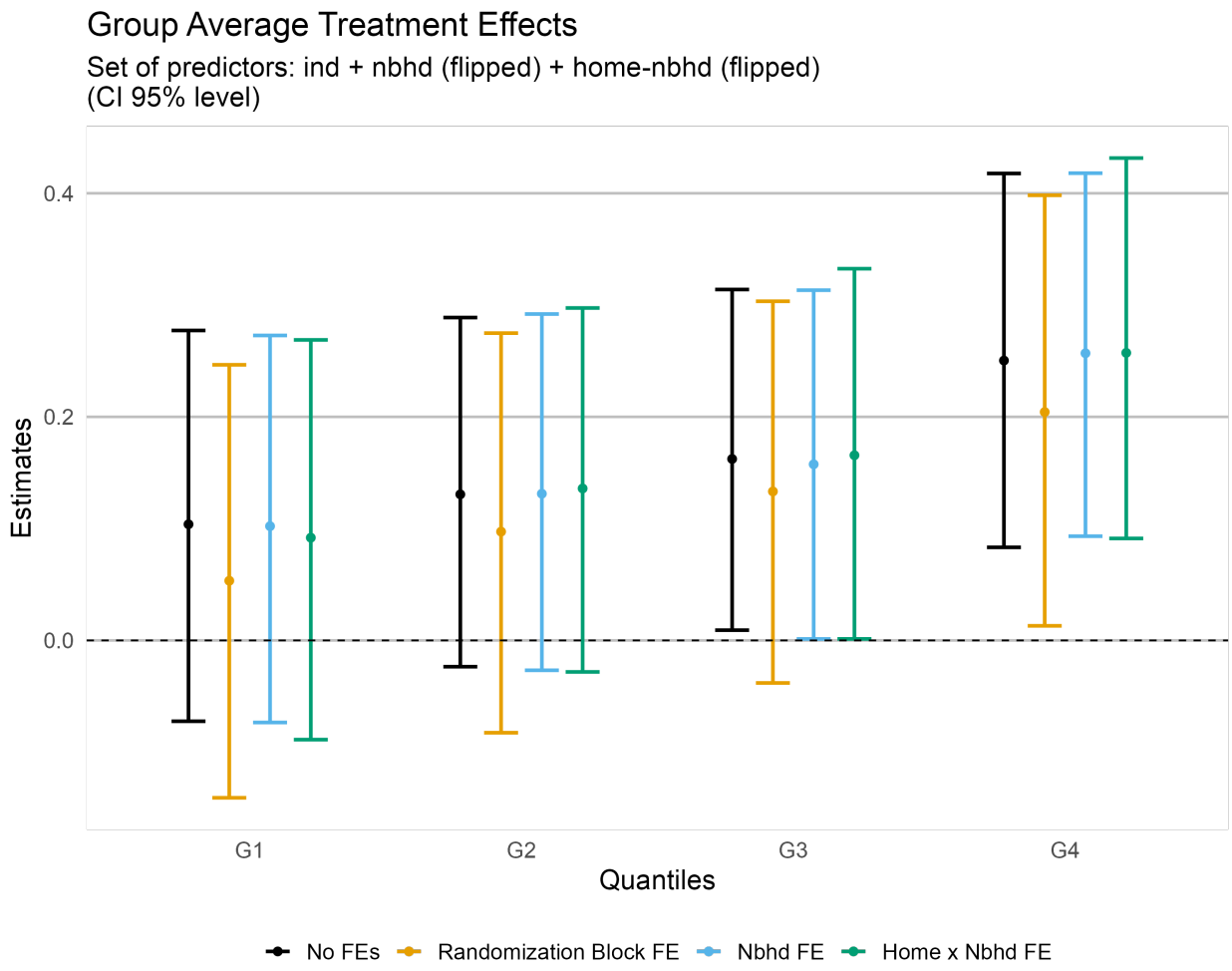
Notes: This map plots the ten target neighborhoods for one participant from the home neighborhood of Viwandani. Non-target neighborhoods are shaded in purple if they had ever visited at baseline or white if they had not. Ambiguous neighborhoods are not shaded. The four target categories are shared in 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.5: Distribution of Neighborhood Fixed Effects



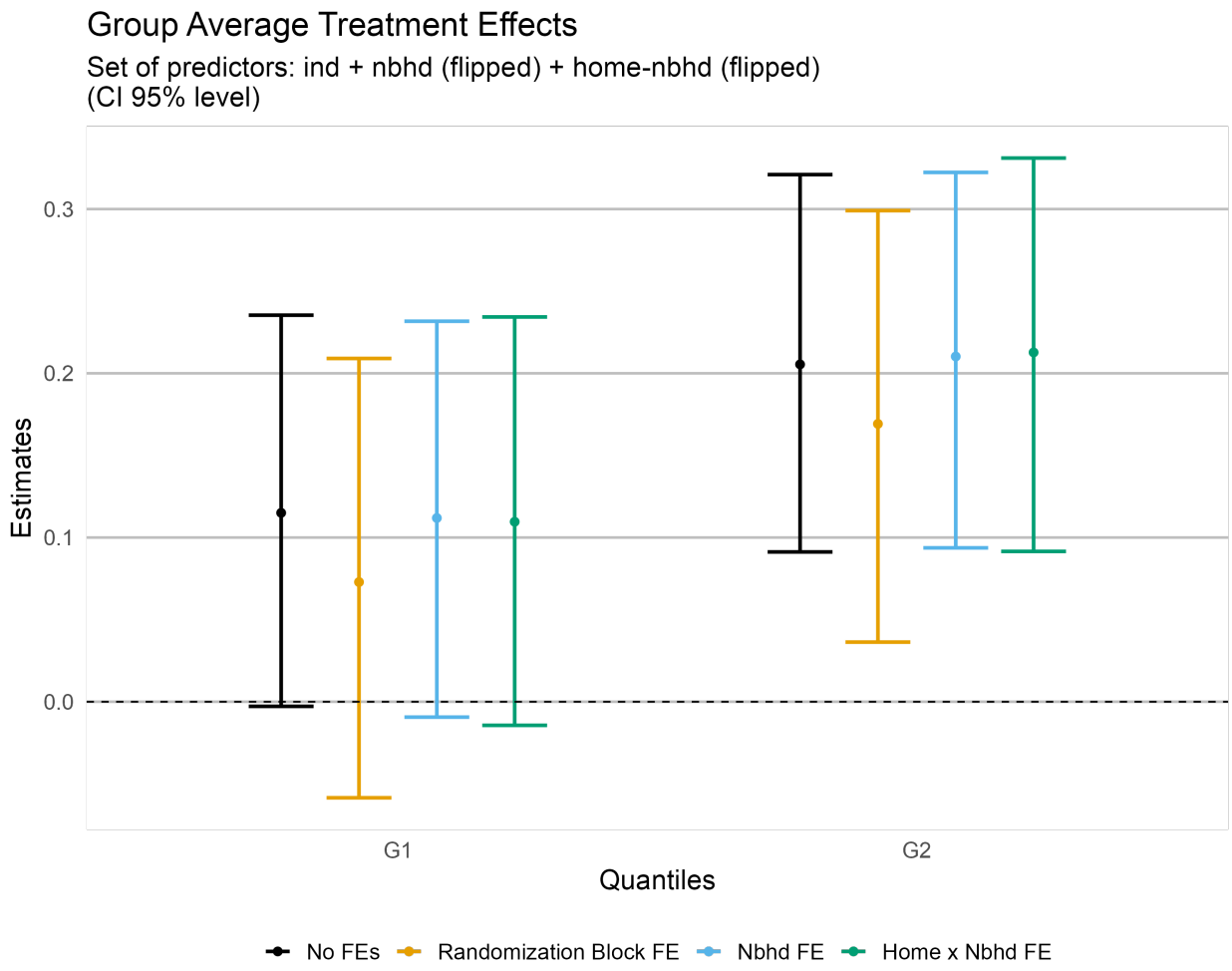
Notes: Estimated neighborhood fixed effects from column (3) of Table 2.

Figure A.6: Quartiles of Heterogeneous Treatment Effects



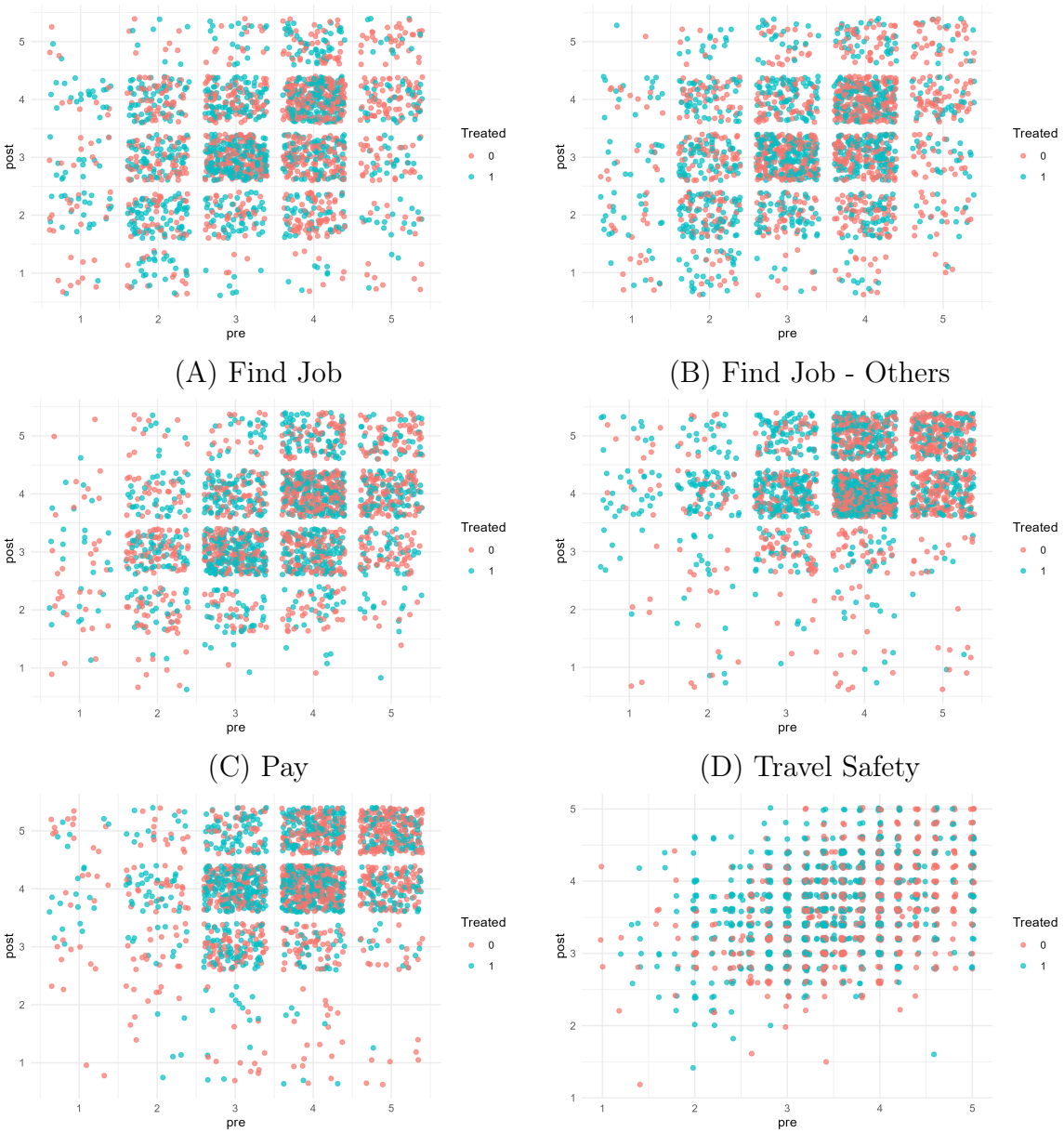
Notes: The figure shows four quartiles of the Group Average Treatment Effects, for the impact of training in an unfamiliar neighborhood, on willingness to later work in that neighborhood, estimated with the Chernozhukov et al. (2023) method. To employ the method we restrict to closed elicitation choices between an non-target familiar neighborhood and an unfamiliar neighborhood. We then use as the outcome an indicator for choosing to work in an unfamiliar neighborhood.

Figure A.7: Median Split of Heterogeneous Treatment Effects



Notes: The figure shows two quartiles of the Group Average Treatment Effects estimated with the Chernozhukov et al. (2023) method. To employ the method we restrict to closed elicitation choices between an non-target familiar neighborhood and an unfamiliar neighborhood. We then use as the outcome an indicator for choosing to work in an unfamiliar neighborhood.

Figure A.8: Beliefs Transition Matrices



(A) Find Job

(B) Find Job - Others

(C) Pay

(D) Travel Safety

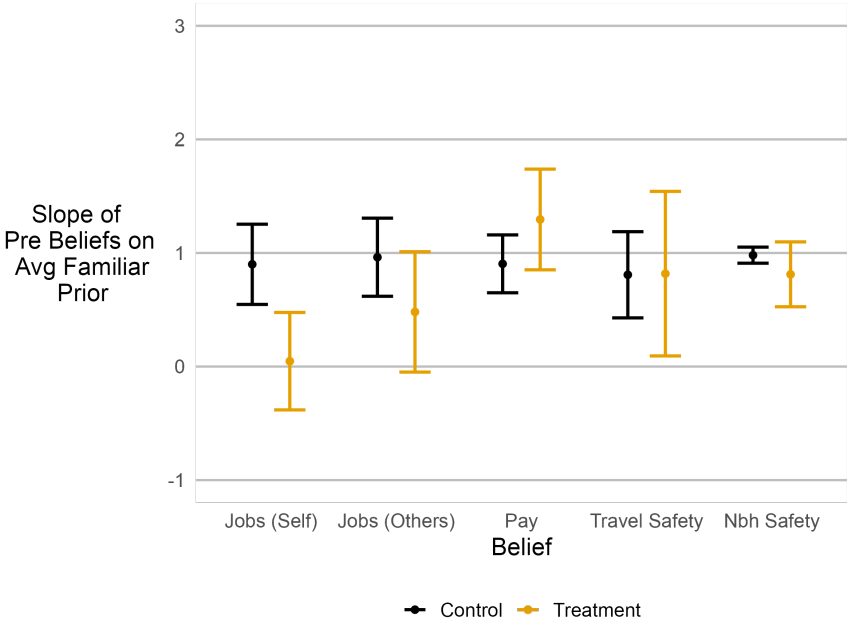
(E) Overall Safety

(F) Average (All Measures)

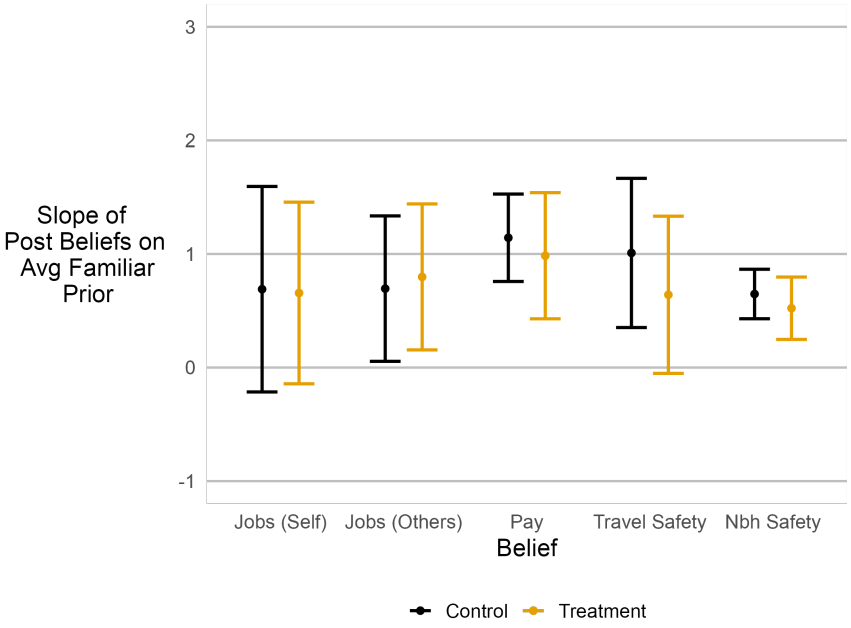
Notes: each graph plots beliefs pre- and post-intervention on the X- and Y-axis, respectively. Higher values correspond to better outcomes. See Section A.5 for the precise questions underlying each measure.

Figure A.9: Relationship Between Average Familiar Priors and Posteriors by Treatment

(a) Pre-Visit Beliefs

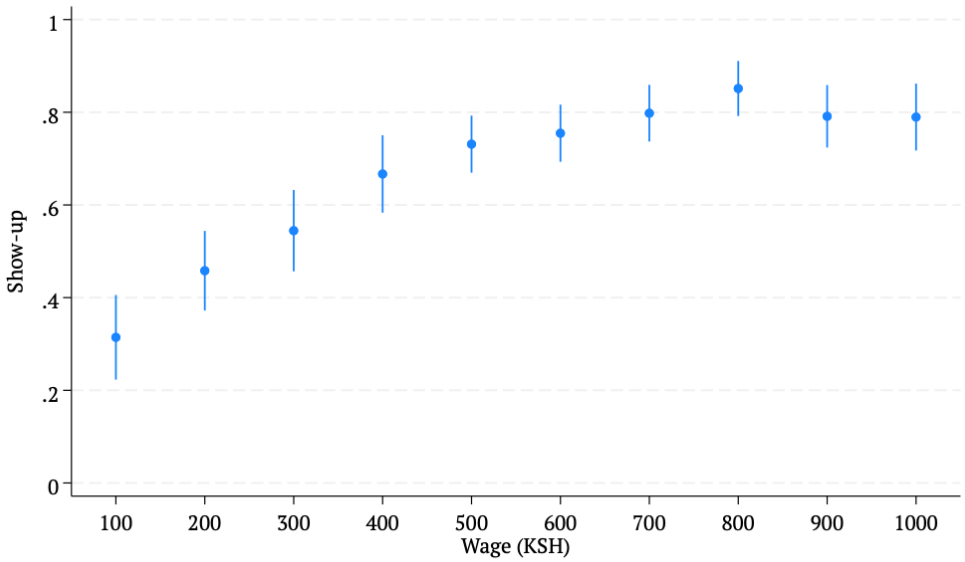


(b) Post-Visit Beliefs



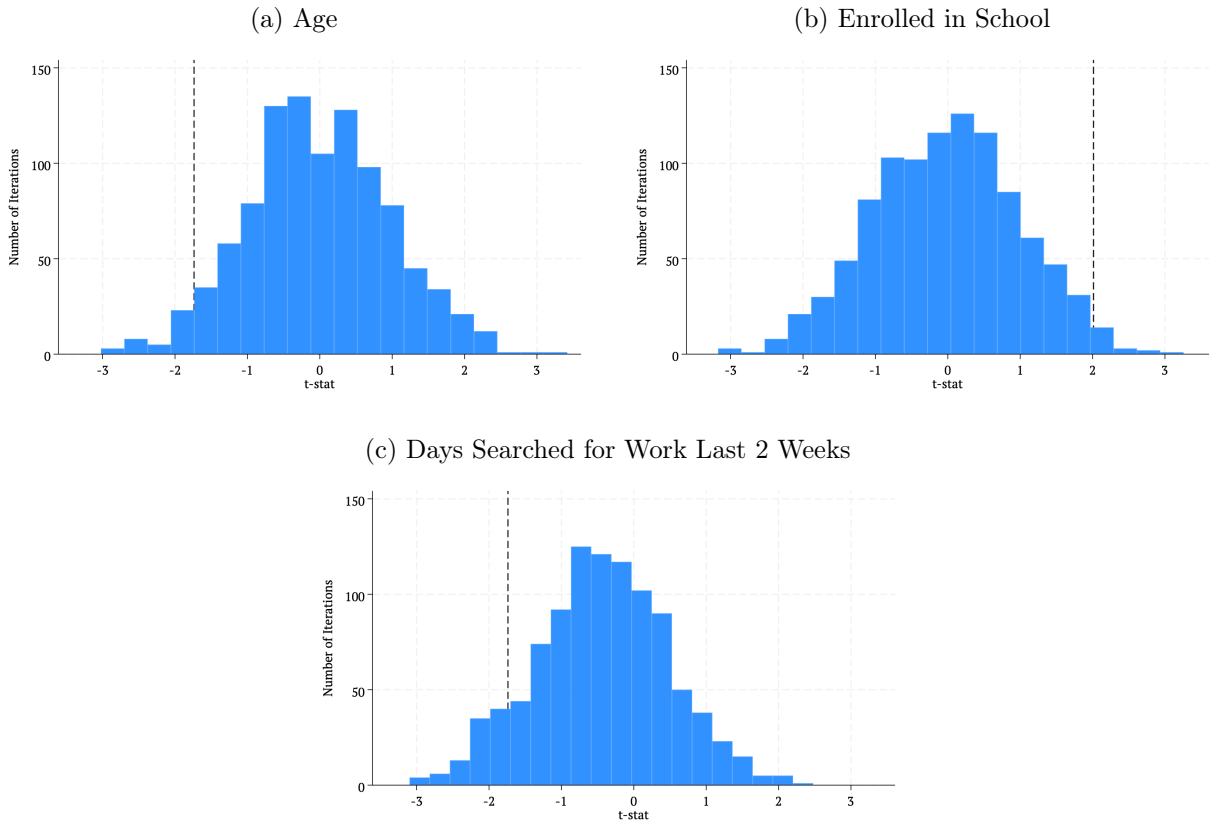
Notes: The figure presents the results comparing beliefs between different respondents, before and after the training intervention. We first construct for each neighborhood and each outcome a predicted rating based on priors from respondents familiar with the respective neighborhood, using a Bayesian 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.

Figure A.10: Show-up 2-4 Months After Intervention: High Wage Plateau



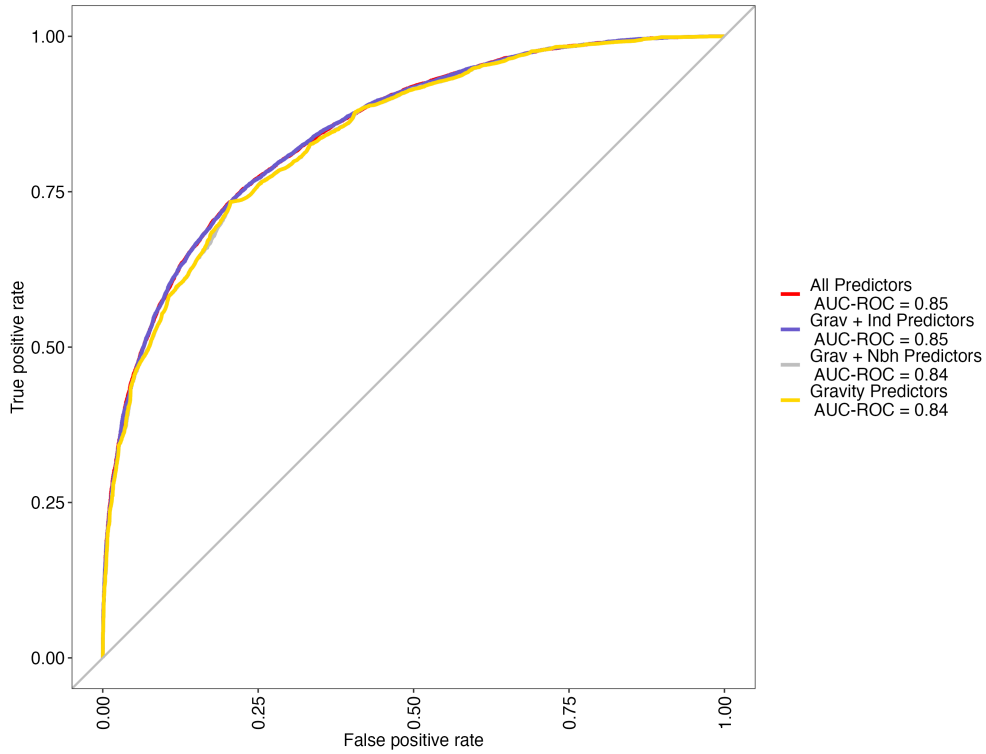
Notes: This figure plots average show-up for the travel survey task by (random) wage offer.

Figure A.11: Randomization Inference Results for Imbalances in the Closed Elicitation Sample



Notes: This figure plots the distribution of the t-statistics from mean difference tests between treatment and control groups across 1000 simulations of treatment assignment. The sample includes only participants in the closed elicitation group, focusing on variables with evidence of imbalance (See Table A.7 for further results). The dotted lines represent the observed t-statistics for the mean differences in the closed elicitation sample.

Figure A.12: No Evidence of Match-Specific Familiarity Patterns



Notes: This figure replicates Figure 3b results using logit-lasso models.

A.2 Appendix Tables

Table A.1: Sample characteristics

	Mean	Std. Dev.
Female	0.74	0.43
Age	29.4	7.6
Education Years	10.7	2.7
Enrolled in School	0.04	0.19
Workdays Last 2 Weeks	3.1	2.9
Days Searched for Work Last 2 Weeks	6.6	3.4
Married	0.46	0.50
Years in Nairobi	15.2	9.3
Resided Outside Nairobi	0.67	0.47
Observations	799	

Notes: This table reports basic statistics on participant characteristics. The sample includes only those participants who attended the first training day.

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.

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)	-0.045^{***}		-0.045^{***}	-0.056^{***}	
	(0.001)		(0.001)	(0.001)	
Same Ethnicity		0.063^{***}	-0.007	0.016	0.016
		(0.017)	(0.015)	(0.015)	(0.015)
Individual FEs	Yes	Yes	Yes	Yes	Yes
Neighborhood FEs				Yes	
Home x Neighborhood FEs					Yes
Observations	13,331	13,331	13,331	13,331	13,331
Adjusted R^2	0.165	0.062	0.165	0.347	0.384

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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.5: Correlates of Individual-level Average Familiarity

	Average "Ever Been"	
	(1)	(2)
Female	-0.053^{***}	-0.056^{***}
	(0.011)	(0.011)
Age	0.003^{***}	0.003^{***}
	(0.001)	(0.001)
Years of education	0.012^{***}	0.012^{***}
	(0.002)	(0.002)
Years in Nairobi	0.002^{***}	0.003^{***}
	(0.0005)	(0.0005)
Spatial Ability Idx	0.014^{***}	0.018^{***}
	(0.005)	(0.005)
N	827	827
Outcome SD	0.176	0.176
Surveyor FEs	No	Yes
Adjusted R^2	0.140	0.209

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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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 Closed 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 "closed" elicitation group. See Figure A.11 for randomization inference results on age, school enrollment, and days searched for work last 2 weeks.

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	3.93	3.887	0.043
			(0.046)
Walking Dist. (Meters)	9,280	10,142	-862**
			(332)
Walking Duration (Seconds)	7,685	8,342	-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 generated from the logit regression in Table 2. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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

	Training (1)	Employment (2)	All Days (3)	Endline
Treated	-0.0098*	-0.0221**		-0.0013
	(0.0058)	(0.0104)		(0.0137)
Day 3	-0.0075**		-0.0025	
	(0.0031)		(0.0025)	
Day 4			-0.0075*	
			(0.0043)	
Day 5		-0.0100***	-0.0150**	
		(0.0035)	(0.0061)	
Day 6		-0.0225***	-0.0199***	
		(0.0053)	(0.0070)	
Day 2 × Treated			-0.0055	
			(0.0051)	
Day 3 × Treated			-0.0156**	
			(0.0079)	
Day 4 × Treated			-0.0131	
			(0.0090)	
Day 5 × Treated			-0.0182	
			(0.0114)	
Day 6 × Treated			-0.0333**	
			(0.0137)	
Control Mean	0.996	0.983	0.989	0.963
Observations	1598	2397	3995	799

Notes: This table shows attrition at different stages of the study timeline. Our study sample are the 799 participants who attended the first day of taining. Column (1) presents results on attrition for days 2 and days 3 of training. Column (2) presents results for survey days 4, 5, and 6 which were employment days. Column (3) presents results across all training and employment days with differential attrition between control and treatment. Column (4) 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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

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. Standard errors in parenthesis are clustered at the individual level. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.12: Preferences for Familiar Neighborhoods By Baseline Perceptions of 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)
NBH x home FE			Yes	Yes		
NBH FE					Yes	Yes
N	6,756	6,756	6,756	6,756	6,756	6,756

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.13: 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).

Table A.14: Preferences for Familiar Nbhds By Distance

	(1)	(2)	(3)
Baseline Familiar × Close	0.89***	0.77***	0.88***
	(0.134)	(0.137)	(0.142)
Baseline Familiar × Far	0.83***	0.75***	0.85***
	(0.122)	(0.134)	(0.141)
Training × Close	0.39**	0.29*	0.29*
	(0.134)	(0.129)	(0.138)
Training × Far	0.18	0.36**	0.31*
	(0.138)	(0.125)	(0.137)
Training × Unfamiliar × Close	1.00***	0.98***	1.01***
	(0.243)	(0.233)	(0.258)
Training × Unfamiliar × Far	0.70**	0.88***	0.82**
	(0.240)	(0.223)	(0.255)
N	6,756	6,756	6,756
Home × neighborhood FE		Yes	
Neighborhood FE			Yes

Notes: This table replicates results from Table 2 accounting for proximity heterogeneity. The proximity interaction term is defined as travel durations below or above the median (roughly 1 hour).

Table A.15: Preferences for Familiar Nbhds By Gender

	(1)	(2)
Baseline Familiar	0.74***	1.42***
	(0.122)	(0.219)
Training	0.38**	0.10
	(0.130)	(0.212)
Training × Unfamiliar	0.72**	1.54***
	(0.235)	(0.436)
Distance (km)	-0.26***	-0.25***
	(0.022)	(0.032)
Job duration (hrs)	-0.51***	-0.84***
	(0.061)	(0.132)
$\mathbb{E}$ Compensation (KSH)	0.78***	0.93***
	(0.034)	(0.064)
Cash Upfront (KHS)	0.07	0.02
	(0.052)	(0.085)
Bonus (KSH)	-0.22***	-0.24***
	(0.011)	(0.022)
Controls	Yes	Yes
N	4,952	1,804
NBH FE	Yes	Yes
Sample	Women	Men

Notes: This table replicates results from Table 2 for the women (column 1) and men (column 2) samples.

Table A.16: 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 results from Table 2 considering strong familiarity at baseline.

Table A.17: 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)
N	8,183	8,183	8,183
Home × neighborhood FE		Yes	
Neighborhood FE			Yes

Notes: This table replicates results from Table 2 to explore spillover effects on the unfamiliar neighborhoods not visited by the treated group.

Table A.18: 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)
Post	-0.131^{***}	-0.185^{***}	-0.058	0.142^{***}	0.239^{***}
	(0.039)	(0.039)	(0.037)	(0.033)	(0.038)
Pre:treated	-0.174^{***}	-0.116^{**}	-0.033	-0.375^{***}	-0.154^{***}
	(0.060)	(0.058)	(0.062)	(0.059)	(0.058)
Post:treated	0.0002	0.047	-0.0002	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	4.117	4.102	4.286	4.396	4.341
SD	1.028	1.066	0.935	0.916	0.908
pre x treat = post x treat, p-val	0.003	0.003	0.556	0.000	0.002
Home × neighborhood FEs	Yes	Yes	Yes	Yes	Yes

Notes: This table replicates estimates from Table 4 including home by neighborhood FEs. 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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.19: Confidence on Beliefs After One Visit

	Dependent variable:				
	Find Job	Find Job - Others	Pay	Travel Safety	Overall Safety
	(1)	(2)	(3)	(4)	(5)
Constant	4.117***	4.102***	4.286***	4.396***	4.341***
	(0.039)	(0.041)	(0.036)	(0.035)	(0.035)
Post	0.143***	0.092**	−0.040	0.152***	0.164***
	(0.042)	(0.042)	(0.038)	(0.034)	(0.037)
Pre:treated	−0.107*	−0.126**	−0.147***	−0.203***	−0.145***
	(0.058)	(0.060)	(0.055)	(0.053)	(0.053)
Post: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 x treat = post x treat, p-val	0.786	0.212	0.138	0.006	0.072

Notes: This table replicates estimates from Table 4 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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.20: Cross-Sectional 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 $\geq$ 3 years ago		0.39*
		(0.169)
Training	0.30**	0.31**
	(0.109)	(0.106)
Training $\times$ Unfamiliar	0.91***	0.89***
	(0.213)	(0.215)
N	6,756	6,756
Job Attribute Controls	Yes	Yes
NBH FE	Yes	Yes

Notes: This table presents results from a logit model estimated on the "closed" elicitation choices shown in Table 2 but splitting the baseline familiar coefficient based on how long ago the individual last visited the neighborhood. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.21: 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)	(0.054)
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	3,960	3,960	1,814	1,814	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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.22: Impact of Familiarity and Experimental Exposure on Medium-run Job Acceptance - with Individual REs

		All Invites			Second Round Invites		
		(1)	(2)	(3)	(4)	(5)	(6)
Baseline Familiar	× (wage ≤ 500)	0.682***	0.763***	0.642***	0.907***	1.088***	0.900***
		(0.167)	(0.178)	(0.173)	(0.229)	(0.242)	(0.234)
Training	× (wage ≤ 500)	0.144	0.161	0.151	0.240	0.252	0.254
		(0.162)	(0.165)	(0.164)	(0.216)	(0.218)	(0.218)
Training × Unfamiliar	× (wage ≤ 500)	0.452	0.489*	0.457	0.670*	0.605	0.657*
		(0.291)	(0.294)	(0.294)	(0.385)	(0.385)	(0.388)
Distance (km)	× (wage ≤ 500)	-0.223***		-0.223***	-0.237***		-0.230***
		(0.023)		(0.025)	(0.033)		(0.035)
Wage	× (wage ≤ 500)	0.836***	0.827***	0.844***	1.012***	0.982***	1.018***
		(0.054)	(0.055)	(0.055)	(0.088)	(0.088)	(0.089)
(wage > 500)		2.171**	4.055***	2.227**			
		(0.919)	(0.789)	(0.928)			
Baseline Familiar	× (wage > 500)	0.127	-0.123	0.047			
		(0.343)	(0.349)	(0.351)			
Training	× (wage > 500)	0.192	0.200	0.209			
		(0.357)	(0.362)	(0.359)			
Training × Unfamiliar	× (wage > 500)	-0.035	0.033	-0.040			
		(0.557)	(0.568)	(0.562)			
Wage	× (wage > 500)	0.140	0.140	0.147			
		(0.094)	(0.095)	(0.095)			
Distance (km)	× (wage > 500)	-0.019		-0.024			
		(0.046)		(0.048)			
N.		3,960	3,960	3,960	1,814	1,814	1,814
Baseline Familiar/Training × Unfamiliar		0.663	0.641	0.711	0.739	0.556	0.730
Survey Day RE		Yes	Yes	Yes	Yes	Yes	Yes
Invite Sequence RE		Yes	Yes	Yes	Yes	Yes	Yes
Randomization Strata RE		Yes	Yes	Yes	Yes	Yes	Yes
Home × Neighborhood RE			Yes			Yes	
Neighborhood RE				Yes			Yes
Individual RE		Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table replicates the results from table 6 using random effects and including individual random effects. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.23: 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**	−0.001	−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)
Mean	0.035	0.03	0.003	0.005	0.011	0.222
SD	0.185	0.169	0.059	0.071	0.058	1.315
Visited Any + Visited Unfam	0.012	0.005	0.004	0.005	0.003	0.12
[p-value]	[0.082]	[0.377]	[0.203]	[0.202]	[0.688]	[0.729]
Observations	6,896	6,896	6,896	6,896	941	941

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. $^{*}p \leq 0.10$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$

Table A.24: 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.25: 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.26: 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 based on equation (**??**). 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.27: 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 × 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]
lambda	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.28: Estimates of Memory Costs from "Open" Elicitation: Days 2 and 3

	(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 × 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]
lambda	0.50	0.55	0.62
	[0.36, 0.66]	[0.31, 0.84]	[0.00, 0.73]
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.

A.3 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.29 lists these excluded neighborhoods and their descriptions. Excluded neighborhoods consist of national parks, the airport, university area, as well as the three home neighborhoods and six additional informal residential settlements, which we excluded because of limited work opportunities and potential lack of safety. We did not collect familiarity data for these neighborhoods and we did not offer jobs in these neighborhoods.

For each of our three home neighborhoods (Kibera, Kawangware, and Viwandani), we

use the Google Maps API to calculate both walking and transit travel times from the study venue to the center of each of the 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 2) 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.[35] 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.

[35]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.

Table A.29: Neighborhoods Excluded from Study

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

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

A.4 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.

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.

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.5 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.6 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 training visits also have overall average effect on ratings across all neighborhoods. The $Post_t$ coefficient 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:

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

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 then estimate the following regression

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

with standard errors clustered at the neighborhood level.

Figures A.9a and A.9b presents the results for pre-visit and post-visit 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.