Reaching the Novice or Nudging the Expert? Information Delivery and the Returns to Migration

Zachary Barnett-Howell Travis Baseler Thomas Ginn Stepan Gordeev

May 2026

Abstract

New information can increase the adoption of high-return technologies, but its welfare effects depend on whether it reaches those with high marginal returns. We study information dissemination strategies in the context of rural-urban migration, one of the best tools available for rural workers to increase their incomes. In a cluster-randomized experiment in rural Kenya, we test three delivery methods: providing information about the capital city privately to rural households, facilitating conversations between rural households and urban workers, and broadcasting information in rural villages. Each method increases migration to the capital city over the following year. Delivering information privately results in significant economic gains, while broadcasting it publicly does not. We show that public dissemination influences those with greater migration experience—and thus lower migration costs and marginal benefits—compared to private dissemination. We build and estimate a general-equilibrium Roy model that rationalizes these findings and predicts settings where they generalize. In many settings, the value of migration interventions may depend on whether they can reach higher-cost—or "novice"—migrants.

JEL Classifications: C93, J31, J61, J82, O15, O18, R23
Keywords: technology adoption, social networks, rural-urban migration

---

Barnett-Howell: Samsara, zackbh@gmail.com. Baseler: University of Rochester, BREAD, and J-PAL, tbaseler@ur.rochester.edu. Ginn: Center for Global Development, tginn@cgdev.org. Gordeev: Texas Christian University, s.gordeev@tcu.edu. We gratefully acknowledge funding for this project from Open Philanthropy. We appreciate comments from Michael Callen, Arun Chandrasekhar, Doug Gollin, Johannes Haushofer, Rachel Heath, Tobias Heidland, Erin Kelley, David McKenzie, Mushfiq Mobarak, Melanie Morten, Dev Patel, Sandra Rozo, Ashish Shenoy, Shekhar Tomar, Corey Vernot, and seminar participants. We thank Remit Kenya for overseeing implementation and data collection, especially Carol Nekesa, Blastus Bwire, Andrew Wabwire, Sam Balongo, Dominic Tanui, Michael Asiago, and Obadiah Ogega. Excellent research assistance was provided by Daliah Al-Shakhshir, Ali Amini, Daniel Chiang, Claire Manley, Margaret McKenna, and Shirley Yen. This study was approved by IRBs at Maseno University (01049/22), the National Commission for Science, Technology & Innovation (16234) and Rochester (6831) and was pre-registered in the AEA RCT Registry (10051) (Barnett-Howell et al., 2023).

1 Introduction

Many profitable technologies remain underused because people lack accurate information about their returns (Magruder, 2018). A large literature studies how to overcome these information frictions, showing that the strategy to disseminate information—through social networks, broadcasts, or extension services, for instance—affects the rates of learning and adoption (Beaman et al., 2021, Banerjee et al., 2024). Yet adoption is an incomplete measure of effectiveness when returns are heterogeneous (Suri, 2011, Suri and Udry, 2022). If different dissemination strategies induce different types of adopters, two modalities can have very different welfare effects even with similar take-up rates. The policymaker's objective is therefore not simply spreading information to boost adoption but targeting its dissemination toward those with high marginal returns.

Rural-to-urban migration is a canonical example of an underused, high-return technology (Bryan et al., 2014). One reason is that migration decisions are often made with poor information (McKenzie et al., 2013, Shrestha, 2020).[1] Improving access to information through household-level visits can therefore increase migration and income (Baseler, 2023). Are more effective dissemination strategies available? Connecting urban and rural residents may diffuse especially relevant information. Broadcasting information could reduce coordination and implementation costs. However, these methods might not reach those with low initial interest in migrating, who may have the most to learn. Given that the returns to migrating are likely to be highly heterogeneous (Borjas, 1987, Lagakos et al., 2020), the welfare value of delivering information about migration may depend critically on whom it influences.

We study how the modality of information delivery affects the decision to migrate—and the returns among new adopters—using a large-scale, cluster-randomized experiment in rural Kenya. We test three dissemination strategies. In villages assigned to an *Information* arm, we visit households individually to offer general information on incomes, prices, and living conditions in the capital city of Nairobi. In a *Mentor* arm, we supplement the information content with an offer to be matched to a Nairobi resident who provides additional information about migrating over the phone or in person in the city. In a *Group* arm, the information content is broadcast through a public village meeting and former migrants from that village answer questions from the group. We additionally collect data from a random subset of untreated households in the Information arm to study spillovers. In *Control* villages, households do not receive any intervention.

[1] Information frictions can be generated by high search and experimentation costs (Franklin, 2018, Abebe et al., 2020, Banerjee and Sequeira, 2023) or distortions in social learning such as hidden income, a phenomenon documented across a wide array of settings (Ambler, 2015, Jakiela and Ozier, 2016, Seshan and Zubrickas, 2017, Joseph et al., 2018, Baseler, 2023, Zhang, 2024).

We first confirm the existence of large information gaps in our population-representative sample spanning five Kenyan counties: across regions and demographic groups, villagers underestimate earnings in the capital city by 30–60% on average. All three of our study arms immediately increase beliefs about the returns to migrating and aspirations to migrate. Over the following year, the share of households sending migrants to Nairobi increased by about 2 percentage points (pp.) on a base of 17%—a 12% effect size—with similar impacts across treatment arms. We find large economic returns from our Information and Mentor arms: in these groups, the intent-to-treat impact on income is 7–9% one year after treatment, an effect robust to adjusting for spatial differences in prices and amenities. However, despite inducing a similar change in migration to the other arms, the Group treatment creates no measurable economic gains on average: impacts across a broad set of measures are close to zero and significantly lower than those in Information and Mentor.

We find that the way information is transmitted affects the type of workers who respond to it. The individualized Information and Mentor arms increase migration among "novice" households with little prior migration history. In contrast, group dissemination appears to act as a nudge: it increases migration among "expert" households with more migration experience and with pre-existing plans to migrate again. In our setting, it is the novice households who exhibit positive marginal returns to migrating.[2] We rationalize this finding through the lens of a heterogeneous-agent model of migration under imperfect information. The model helps formalize the intuition that rural households facing higher migration costs will exhibit greater gains from marginal migration in equilibrium. In our model, high-cost households face greater marginal gains if and only if migrants are positively selected from the sending population, as they are in our setting. This prediction has important implications for information targeting: in settings where migrants are positively selected—a feature typically evident from observational data alone—interventions should be designed to reach households facing greater migration costs.

To understand why the Group treatment did not lead high-cost households to migrate, we analyze program data on households' behavior during the interventions and their social interactions afterward. We find that public dissemination favored lower-cost, experienced households in two ways. First, during group meetings, less-experienced households were much less likely to be active participants compared to experienced households, while in the 1-on-1 treatments used in Information and Mentor—during which each household could ask questions of program staff privately—they were similarly or slightly more active. Moreover, we find that the migration decisions of experienced households appear highly responsive to

[2]Following the literature, we use the term *marginal migration* to refer to migration induced by a technology or policy intervention that reduces migration costs.

the presence of other active, experienced households in group meetings, while the decisions of inexperienced households do not. Together, these findings suggest that the information relevant to inexperienced households is distinct from what is relevant to experienced households, and that the latter crowded out the former during group treatments but not 1-on-1 treatments. Second, we find that the Information treatment led migrants from inexperienced households to co-migrate with more experienced households from their village. We find a similar increase in co-migration in Group villages, but the impact is driven by experienced households co-migrating with each other, again consistent with crowd-out effects. We find no significant impacts on co-migration in Mentor villages, suggesting that the mentor connection substituted for this support.

We find no significant diffusion of privately provided information: beliefs, aspirations, and migration are unaffected for the neighbors of treated households. One possible explanation is that households are strategically withholding information from each other: indeed, we find that households with more borrowing relationships (but not other social relationships) update their beliefs less when their neighbors are treated. Despite the lack of spillover effects on migration, we find suggestive evidence of positive economic spillovers for the neighbors of treated households, who report better economic well-being one year after the intervention. These gains are driven by increased rural labor earnings and business profits, consistent with general-equilibrium impacts of migration through demand multiplier effects (Egger et al., 2022, Khanna et al., 2022) as migrants remit or bring home their urban earnings.

Finally, we use our model to quantify the role of heterogeneous migration responses in explaining divergent economic returns across delivery modes and to estimate the aggregate role of information frictions in driving urban-rural productivity gaps. We first simulate the experiment in the model in partial equilibrium, inferring arm-specific treatment intensities from experimental impacts on migration and migrant selection. We find that the model successfully reproduces each treatment's income effect, which we interpret as evidence that heterogeneity in responses across high- and low-cost types can largely explain the lower economic returns from group dissemination. Next, we use the model to conduct a universal information treatment in general equilibrium. The model suggests that removing information frictions entirely would increase migration from 17% to 22%, reduce the agricultural productivity gap from 2.6 to 2.3, and reduce the rural-urban income gap from 5.1 to 4.1, even in the absence of any changes to the other costs of migrating.

Overall, our findings imply that a subset of rural households experiences substantial returns from lowering migration costs through better information or new social network connections. Policies or programs that improve access to information, such as online job portals (Kelley et al., 2024) or mentorship programs open to migrants (Baseler et al., 2025b), could help rural workers make more informed migration choices. However, merely broadcasting new information is not sufficient to improve economic outcomes. While group-level programs are attractive from a cost perspective, these discussions risk becoming dominated by participants who are enthusiastic but adversely selected on gains. Our more targeted or individualized policies, while more costly, were more effective at encouraging high-return migration.

Related Literature. This study makes four primary academic contributions. First, we show that the method of information dissemination matters not only for adoption, but also for the economic returns generated by that adoption. A large literature studies how information spreads through social networks and how diffusion can be shaped by the identity of initial informants, the structure of networks, incentives to communicate, and the breadth of initial information delivery (Conley and Udry, 2010, Banerjee et al., 2013, Cai et al., 2015, Miller and Mobarak, 2015, Banerjee et al., 2019, BenYishay and Mobarak, 2019, Beaman et al., 2021, Chandrasekhar et al., 2022, Banerjee et al., 2024). This literature is focused on adoption and largely abstracts from heterogeneity in the marginal returns to that adoption. An exception is Dar et al. (2024), which shows that changing the information channel from government agents to private input suppliers increases adoption of a new seed variety, especially among farmers with higher predicted returns. Our contribution is to examine this question in the context of dissemination through public channels, private channels, and new social connections, and to measure realized, rather than predicted, returns to adoption. Our finding that these modes generate similar increases in take-up but very different economic returns implies that focusing on knowledge or take-up may be insufficient to characterize optimal diffusion, especially when underlying returns are highly heterogeneous. We also introduce a new explanation for why public dissemination can fail: it can engage knowledgeable but low-return agents who crowd out higher-return types from learning.

Second, we show that the reason the method of diffusion matters in our setting is that it selects different types of marginal migrants: intensifying migration by "experts" yields lower marginal returns than creating new, "novice" migration. Using a variant of the foundational Roy (1951) model, we demonstrate theoretically that high-cost groups will exhibit greater experimental benefits from migrating precisely when migrant selection is positive, and we show that it is positive both in our setting as well as across much of the developing world. This finding offers a partial explanation for the higher experimental, compared to observational, returns to migrating identified across much of the economics literature.[3] Our

[3]In contrast to a handful of experimental papers finding high marginal returns to migration (Bryan et al., 2014, Akram et al., 2017, Baseler, 2023), several studies find low observational returns to migrating on average (Young, 2013, Alvarez, 2020, Hamory et al., 2021) Other studies have attempted to reduce migration

contribution is related to that of Lagakos et al. (2020), who demonstrate that average observational returns appear driven by large flows of low-cost, low-benefit migration in several settings, while experimental returns are driven by reductions in initially high costs facing high-benefit groups. We provide experimental evidence that reducing migration barriers for high-cost groups creates greater returns than doing so for low-cost groups.

Third, we add to the literature testing for information frictions facing rural-urban migrants. Several studies have experimentally tested the value of providing information about migration opportunities. Baseler (2023) finds a positive migration response from providing information about cities in Kenya to rural households. We confirm this finding at scale in a population-representative sample of rural households for five Kenyan counties: this scale helps our findings speak directly to the macro-development literature concerned with the persistence of rural-urban income gaps (Lagakos and Waugh, 2013, Gollin et al., 2014, Herrendorf and Schoellman, 2018, Bryan and Morten, 2019). Crucially, our rural sample is not self-selected on migration interest, which is endogenous to information frictions. This distinguishes our contribution from Shrestha (2020) and Bhatiya et al. (2026), which both find overestimation of destination wages in a sample of workers interested in migrating. It also distinguishes our paper from Frohnweiler et al. (2024), which studies an urban population. Bryan et al. (2014) find no impact of an arm informing rural Bangladeshi workers about typical wages in the capital, but it is unclear whether workers in this setting—where repeat, circular migration is common—lack this information.[4]

Fourth, we make a methodological contribution to the welfare analysis of interventions under incomplete markets (Benjamin, 1992, LaFave and Thomas, 2016, Agness et al., 2025). Evaluating welfare gains from a policy often requires estimating shadow prices that are imperfectly measured by markets, such as the value of non-pecuniary amenities, which can differ greatly between urban and rural areas (Gollin et al., 2021). Similarly, valuing the returns to migrating requires accounting for the price and quality differences of goods between the origin and destination, but such measures are not readily available. We develop simple, survey-based approaches to valuing amenity and price differences across space, validate these using our survey data, and discuss how migration researchers can implement these methods.

costs but found no impact on migration (Beam, 2016, Beam et al., 2016). Without a first-stage impact on migration, these studies cannot assess the gains to marginal migration. Gibson et al. (2017) and Mobarak et al. (2023) identify large returns to international migration using random visa lotteries.

[4] See Lagakos (2020) for a review of the broader literature on rural-urban gaps, which has found distortions due to poor information (Boudreau et al., 2024), financial constraints (Bryan et al., 2014, Akram et al., 2017, Cai, 2020, Miner, 2024), costs of migrating (Lagakos et al., 2023, Morten and Oliveira, 2024), land market regulations (De Janvry et al., 2015), and restrictions in accessing social welfare programs (Imbert and Papp, 2019, 2020, Tombe and Zhu, 2019, Baseler et al., 2025a).

2 Study Design

This section describes the setting and design of our study.

2.1 Setting and Geographic Scope

Kenya is a lower-middle income country in East Africa. Like many low and lower-middle income countries, its population is predominantly rural and engaged in smallholder agricultural production, but is urbanizing in part through rural-to-urban migration. Kenya is typical of sub-Saharan African countries in terms of income per capita, urbanization and agricultural share of employment, migration rates, and sectoral productivity gaps (Gollin et al., 2014).

We selected five out of 47 Kenyan counties for this study. These counties are typical of rural Kenya across several dimensions, as shown in Appendix Table A.1: for each variable, our selected counties lie within the middle 60% of the national county-level distribution. They are poorer than most counties (37th percentile), denser than most counties (77th percentile), and migration is more common among those born there (80th percentile). These five counties are also large, with their rural areas comprising four million people, approximately 15% of Kenya's rural population.

2.2 Sample Selection

Data on the universe of administrative areas were provided by the Kenya National Bureau of Statistics (KNBS). Within each study county, we selected villages and households randomly from the rural population using a three-stage sampling design:

1. *Sub-Location Selection.* We randomly selected 560 sub-locations—each roughly corresponding to a set of 10 villages—from the universe of sub-locations in our selected counties.[5]

2. *Village Selection.* We randomly selected one village (census enumeration area) within each chosen sub-location, after excluding villages with fewer than 50 households to ensure a sufficiently large sample of households per village. Selecting one village per sub-location increases the average distance between villages, reducing concerns about inter-village spillovers.[6]

[5]We excluded the 10% highest-density sub-locations by population per square kilometer (km) from each county to avoid urban areas. We excluded the bottom 5% to reduce the cost of data collection.

[6]Only 3% of Control villages are within 1 km of a treatment village, and the share of villages treated within 3 km or 10 km is not correlated with beliefs, migration, or economic outcomes in the Control group (see Appendix Table A.8).

3. *Household Selection.* In each sampled village, we censused the population of households with the assistance of village leaders. Comparing our census data to population records maintained by the KNBS allows us to assess the completeness of our data: overall, we successfully found 102 households per village compared to 99 in the KNBS records, increasing our confidence that our village sample is reasonably complete.[7] Of these 102, we were able to directly survey an average of 92, or about 90%. We then randomly selected approximately 30 households per village to form our experimental sample, replacing unavailable households as needed to reach the desired sample. We stratified household selection by intended migration, oversampling households who report that they might send a migrant to a city within the next year, and correct for non-uniform sampling and non-response in estimation using sampling weights.

2.3 Timeline and Data Collection

The household census was conducted from May–August 2022 with approximately 53,000 households across the 560 study villages. A longer baseline survey was conducted from September–November 2022 with around 17,000 households. Information treatments and group meetings were conducted in conjunction with the baseline survey. In Mentor villages, mentors were assigned between January and March of 2023. We collected an initial round of midline data through phone surveys from May–August 2023, roughly eight months on average after the information interventions. We conducted an in-person endline survey from February–May 2024, approximately 16 months from the information interventions. Endline surveys were successfully completed with 95% of the sample (Appendix B.1 contains additional details on attrition). For households we were unable to contact directly for the endline survey, we attempted to collect basic information on their migration status and economic activities indirectly from neighbors or local leaders. We use these data when available in our main tests, but they represent a small share of our endline sample (1.5%) and their inclusion does not change our main results, as shown in Appendix Table B.5. During each of the baseline, midline, and endline survey waves, we conducted additional phone surveys with other individuals within the household, focusing on current or former urban migrants.

2.4 Summary Statistics

Information Gaps. Our interventions are predicated on the existence of information frictions. To measure information gaps, we asked households in our census sample about typical incomes in Nairobi for members of a given demographic group (defined by age, education,

[7] The across-village correlation coefficient between our household count and KNBS's is 0.67.

and gender). We compare their mean responses to true incomes in Nairobi for the same group estimated on KIHBS survey data, normalizing both to a nearby reference area (towns in the respondent's home county).[8] Figure 1 presents results: we find that villagers substantially underestimate Nairobi income premiums for nearly every demographic group. Across the 65 groups for which we collected data, most perceived premiums range from 1.3 to 1.5 times local income, while most premiums in the KIHBS data range from 1.5 to 3 times local income.

Past Migration Experience. Less than half of households have any current or former member who worked in Nairobi, as shown in Appendix Table A.5. Households with Nairobi experience are on average better informed, better socially connected at both the origin at destination, and more likely to be planning on migrating than those without experience. Overall, many households lack destination connections: only about half know someone in Nairobi they could stay with or receive job search assistance from. These shares are about 30 percentage points higher for households with migration experience. Later in the paper, we use these measures of experience to proxy for idiosyncratic migration costs.

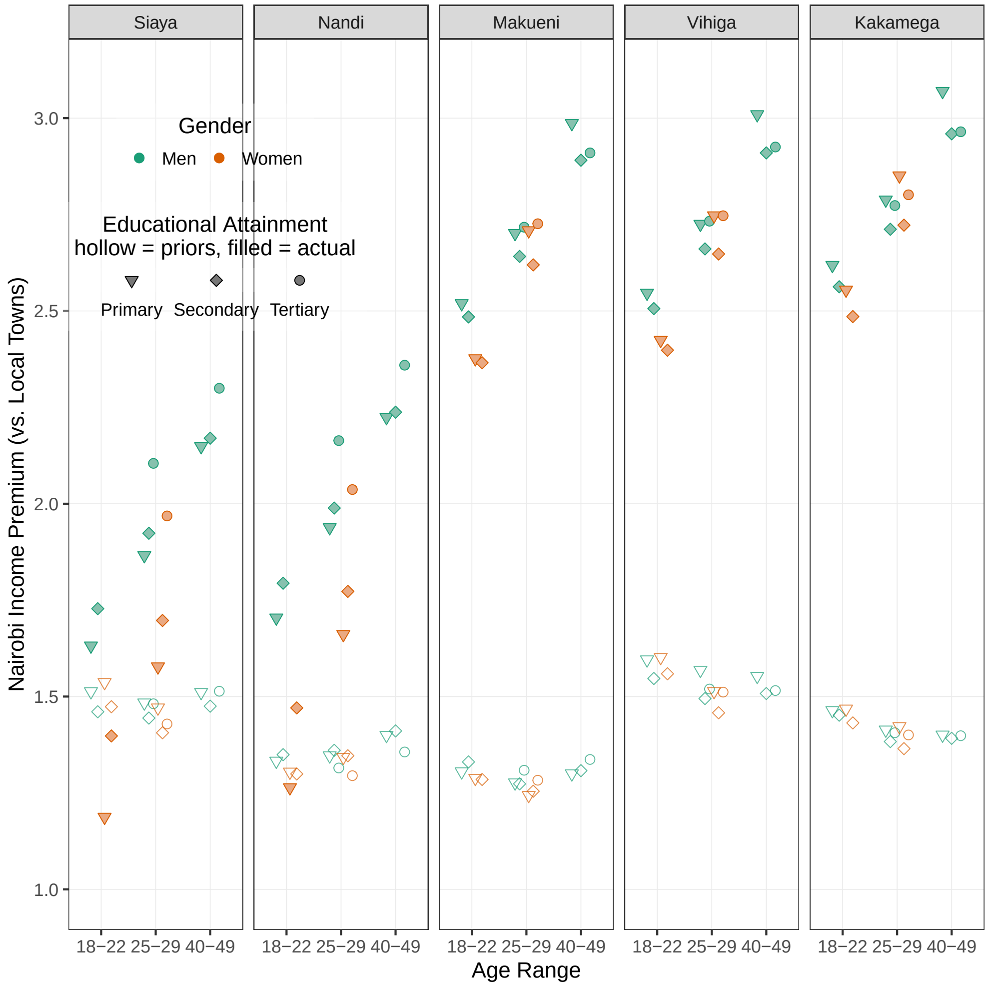
Migrant Characteristics. Appendix Figure A.2 shows summary statistics by migration status for individuals aged 16 or older who are current or former household members in the sample villages of this study, estimated using baseline data. Rural-to-urban migrants are slightly younger than non-migrants, or "stayers," and rural-rural migrants, by about one year. Rural-rural migrants are more likely to be female, while rural-urban migrants are more likely to be male, compared to stayers. Household heads are much less likely to migrate than other family members. Rural-urban migrants are positively selected on education, while rural-rural migrants look similar to stayers. Migrants are far more likely to be employed for a wage: employment shares are 16% for stayers, 33% for migrants in rural areas, and 60–62% for migrants in urban areas. Most employed migrants work in non-agricultural occupations: for rural-to-urban migrants, the most common occupations are casual non-farm worker, housemaid or cleaner, and construction worker.

Appendix Table A.2 shows trip-level descriptives for urban migrants leaving between the baseline and endline surveys. For one-third of trips, the migrant had returned to their home village by the time of the endline survey. The mean (censored) trip duration at the time of the endline was 6.6 months. While most migrants make the journey alone, the majority leave after receiving a job referral from someone in their village. Other assistance, including

[8] Normalizing accounts for any features driving true or perceived incomes that are common to local towns and Nairobi (helping to account, for example, for the time elapsed between the KIHBS survey and our census data collection) and thus isolates whether respondents misperceive incomes in Nairobi specifically.

housing assistance or borrowing money to migrate, is rarer. Most migrants find work as an employee, with a smaller share working as micro-entrepreneurs. It takes migrants four weeks on average to find a job after arriving.

Figure 1: Actual and Perceived Income Premium in the Capital, by Demographic Group



Each marker shows the average income in Nairobi divided by the average income in towns in the respondent's home county for a given demographic group defined by age, educational attainment, gender, and home county. Filled markers show actual incomes estimated using Kenya Integrated Household Budget Survey data from 2015–2016. Hollow dots show perceived incomes estimated from household census survey data. Example survey question: "For 18–22 year-old men in Nairobi, who finished secondary school (in other words, form 4) but did not go to college, how much money do you think they earn on average in a typical month?"

2.5 Treatments

We divided our study sample into five arms assigned at the village level:

1. In *Information* villages, each sampled household received an information sheet about Nairobi, focusing on earnings, employment, rent prices, and amenities. During treatment, program staff read a detailed script explaining where the information on the sheet came from and how to interpret it, and invited respondents to ask questions about the sheet or the script.

2. In *Mentor* villages, each sampled household received the same information sheet and script, plus an offer to be paired with a resident of Nairobi who agreed to serve as a mentor. We identified local residents who were established in Nairobi and fit the profile desired by the migrant (for example, having experience in the occupation the migrant wanted to work in). Mentors offered to speak with prospective migrants over the phone prior to migrating and meet them in Nairobi once they arrived for a total of four meetings over two months. Mentors were available starting in January 2023, shortly after the baseline surveys concluded, and the program was open for enrollment for three months.

3. In *Group* villages, each sampled household was invited to a group meeting where they received the same information sheet and heard the same script. Our project staff then facilitated group discussions about migrating to Nairobi by inviting prior migrants to describe their experiences and take questions, and breaking attendees into small groups to discuss migrating as well as potentially coordinating trips.

4. In *Spillover* villages, we randomly selected two-thirds of sampled households to receive the information sheet and script. The remaining one-third was surveyed, but not given any information.

5. In *Control* villages, sampled households were surveyed, but did not receive any treatment.

Randomization. Village-level treatment assignment was stratified by county, the share of households in each village intending to migrate to Nairobi, and average village income. In Spillover villages, assignment to the information intervention was conducted through simple permutation randomization. Balance tests are presented in Appendix B.1.

Content of the Information Intervention. Our Information arm delivered an updated version of the intervention studied in Baseler (2023). We gathered the information for this intervention from the Kenya Integrated Household Budget Survey (KIHBS) of 2015–2016, a household survey representative at the county level. Using these data, we estimated median incomes in Nairobi for several demographic groups, the ratio of median income in Nairobi to a reference area (cities in their home county), "low" and "high" incomes for some demographic groups (corresponding to the 25th and 75th percentiles, respectively), employment rates by group, typical and low/high rental prices for one-bedroom and two-bedroom units, and the most commonly used form of several home utilities (cooking fuel, water, toilet, and electricity).[9] We explained the data used to estimate these numbers, and we explained the meaning of the low/typical/high statistics by showing a graphic of people lined up from poorest to richest with the relevant quantile colored in. Information sheets and scripts for all treatment arms, translated to English, are available in Appendix B.4. During the midline survey, we provided a reminder of key points of the information as well as an individualized estimate of conditional income in Nairobi for the age, education, and gender group of the person the household listed as most likely to migrate to Nairobi at baseline. Our intervention is thus a bundle of information—spanning income levels, income risk, expected income growth, employment odds, amenities, and home rental prices—and should be interpreted as the impact of learning about conditions in Nairobi broadly.[10]

Mentor Recruitment and Matching. Mentors were recruited across Nairobi from neighborhoods where migrants commonly live or work. During recruitment, we collected information from mentors to facilitate matching. When villagers enrolled in the mentor program, we elicited their preferences over mentors by asking for their top two most important mentor attributes out of age, tribe, occupation, and meeting location (we always matched within gender). Matching between villagers and mentors was done by program staff on a rolling basis. We estimate that match quality was high across several measures.[11] The average matched mentor had lived in Nairobi for 17 years and worked in their occupation for 7 years. Average travel time between the preferred meeting locations was 6.6 minutes.

[9]Enumerators emphasized that migrants may earn different amounts than non-migrants, and that the information shared will not be correct for everyone given income variation. We allocated funding to cover return bus tickets for migrants who reported adverse outcomes in Nairobi and gave each household a program number to report these events, but none were reported. See Appendix B.4 for intervention details.

[10]An open-ended question asked at endline, categorized into pre-filled responses by surveyors, reveals that the most common points people remember from the information treatment are that: incomes are higher, living conditions are better, and rent is higher in Nairobi than they expected.

[11]Seventy-six percent of enrollees were matched to a mentor satisfying the attribute they deemed most important; among those whose mentors did not satisfy their most-important attribute, 88% satisfied their second-most-important attribute.

Content of the Group and Mentor Discussions. Households that attended group meetings most often report revising their beliefs about Nairobi incomes, prices, and lifestyle quality upward, as shown in Appendix Figure A.3. Consistent with the idea that the information mentors can share is more personalized than in the Information arm, we see that prospective migrants and mentors frequently discussed job leads (61% of matched households) and advice on where to live in the city (25%), as shown in Appendix Figure A.4. They also frequently discuss lifestyle (32%), consistent with the hypothesis that psychological factors such as fears and uncertainty about life in the destination loom large for potential migrants (McKenzie, 2023).

Take-Up. Take-up of the information sheet and script was 100%: no household in any treatment arm refused to hear the information or take the sheet. Attendance at the village-level meetings was 88% of the invited sample.[12] In Mentor villages, 471 households (or 13% of the sample) enrolled in the mentorship program, were matched to a Nairobi mentor, and were verified by our program staff to have spoken with their mentor at least once. Of these, 41 households physically met with their mentor in Nairobi, while the rest had conversations over the phone, WhatsApp, or both. The mentor take-up rate of 13% is small in absolute terms, but large relative to the share of migrating households (11% migrating to Nairobi after the intervention in the control group).

2.6 Estimating equations

Following our pre-analysis plan, we estimate intent-to-treat effects using either linear or Poisson regression depending on the outcome variable. For binary outcomes or outcomes potentially containing negative values, we use the following linear specification:

$$y_{ivt} = \sum_j \beta_j T_{jiv} + \gamma \bar{y}_{iv,pre} + \theta_{ivt} \times \text{date}_{ivt} + \alpha_v + \epsilon_{ivt}, \tag{1}$$

where y_{ivt} is an outcome for household i in village v measured in survey round t, $\bar{y}_{iv,pre}$ is household i's mean pre-treatment value of y, T_{jiv} are treatment assignment dummies, θ_{ivt} is a survey-month fixed effect which we interact with the survey date, α_v is a randomization-stratum fixed effect, and ϵ_{ivt} is an error term.[13] We winsorize continuous variables at the 1%

[12] Households that did not attend the village-level meeting were given the information sheet and script during the baseline survey, following the same protocol used in Information villages.

[13] Relative to our pre-analysis plan, this equation omits the variable $M_{iv,pre}$, an indicator for a missing value of $\bar{y}_{iv,pre}$. This is because there are no cases in our data of missing $\bar{y}_{iv,pre}$ when y_{ivt} is non-missing. It also omits a lasso-selected control vector X_i following recent evidence from Cilliers et al. (2024) that this procedure offers little expected gain in statistical power—especially when pre-treatment outcome values

level (within treatment arm and county for post-census rounds). When estimating treatment impacts or population descriptives, we apply sampling weights to correct for non-uniform selection into the experimental sample, as described in Section 2.2.

For non-negative, unbounded outcomes, we use the analogous Poisson specification:

$$\mathrm{E}[y_{ivt}] = \exp\left\{\sum_j \beta_j T_{jiv} + \gamma \bar{y}_{iv,pre} + \theta_{ivt} \times \mathrm{date}_{ivt} + \alpha_v\right\}. \tag{2}$$

We estimate analytical standard errors and p-values accounting for the clustered treatment assignment. For main results, we also present finite-sample randomization-inference p-values in Appendix B.3.[14] Results estimated through randomization inference and regression-based methods are very similar, as shown in Appendix Table B.7.

Pooling Across Treatments. Following our pre-analysis plan, we pool households in Information villages with those assigned to receive information in Spillover villages.[15] At the household level, treatment is identical for these two groups. While the village-level saturation rate differs for these groups, the average difference is modest (31% average saturation in Information villages and 22% average saturation in Spillover villages) relative to natural variation in saturation rates arising from village size (SD = 29 households, average village size = 102 households). Differences in treatment impacts are small and statistically indistinguishable between these two groups, as shown in Appendix Table A.4. While pooling improves the precision of our estimates, point estimates are very similar in the fully disaggregated specification, as shown in Appendix Table B.6.

Multiple Hypothesis Testing. Following our pre-analysis plan, we compute an Anderson (2008) summary index comprising all of our pre-specified household welfare measures, and estimate sharpened q-values to control the false discovery rate for economic outcomes (Anderson, 2008).

---

are available and attrition rates are low, as in our case—at the risk of over-fitting. We show in Appendix Table B.8 that results estimated following the double-lasso procedure of Belloni et al. (2014) are very similar.

[14]We obtain these by permuting 2,000 treatment assignments following the true assignment method and retaining only balanced permutations. Specifically, we discard any permutation for which the null hypothesis of joint equality across treatment groups is rejected at the 10% level for more than three out of 31 tested pre-treatment variables, matching the level of balance achieved in the true treatment assignment (see Appendix Table B.1). We permute only those treatment assignments being compared in a given hypothesis, holding the others fixed.

[15]In these specifications, we exclude households in Spillover villages assigned to not receive information. Pooling them with Information villages is only appropriate under perfect transmission of information within villages. Pooling them with Control villages is only appropriate under the assumption of no spillover effects within villages. Both of these assumptions are clearly rejected by the results shown in Appendix Table A.8 and discussed in Section 3.5.

3 Experimental Results

In this section, we describe the impacts of our information experiments on beliefs, migration, and economic outcomes.

3.1 Impacts on Beliefs and Aspirations

The Information treatment significantly increases rural households' beliefs about their potential income from migrating and their migration plans measured immediately after the information was provided, as shown in Table 1. Planned migration to Nairobi within the next year increases by 4 pp. (on a base of 24%, effect size = 17%, $p < 0.01$). The impact on planned migration to any city is similar at 5 pp. (on a base of 30%, $p < 0.01$), implying that new information increases total planned migration to cities as opposed to only substitution across intended destinations. Expected income immediately after migrating increases by 4% ($p = 0.04$). Additional belief outcomes can be found in Appendix Table A.6.

The Mentor and Group treatments roughly double the impacts of the Information arm on beliefs and aspirations. Planned migration to Nairobi, and to any city, increases by 9–11 pp. in these arms. Expected income immediately after migrating increases by 10–12%.

3.2 Impacts on Migration

All treatments increase migration to the capital after 16 months, as shown in Table 1 columns 4–6. Households in Information villages were 2 pp. more likely to have migrated to Nairobi on a control-group base of 17% ($p = 0.09$) and 1 pp. more likely to have sent recent migrants to Nairobi (we refer to migrants who left after the baseline survey as "recent") on a control-group base of 11% ($p = 0.18$). Impacts on the number of migrants going to Nairobi are directionally similar but somewhat noisier, as shown in Appendix Table A.6. Treatment did not increase migration as of the 8-month midline survey, as shown in Appendix Table A.3, implying that compliers took close to one year to migrate on average. For this reason, we focus on treatment impacts measured in endline surveys for the following results.

Migration impacts in Mentor villages are similar or slightly larger than in Information. These households were 2 pp. more likely to migrate to Nairobi by endline ($p = 0.04$). This effect is driven by recent migration (2 pp. effect, $p = 0.05$) and constitutes net-new urban migration ($p = 0.08$), consistent with impacts on aspirations.

Migration impacts in Group villages are also similar. Households in these villages were 2 pp. more likely to migrate to Nairobi ($p = 0.10$) and 3 pp. more likely to send recent

Table 1: Impacts on Beliefs and Migration

	Belief Outcomes			Migration Outcomes		
	Plans to Migrate to Nairobi	Plans to Migrate to City	Expected Migration Income	Any Migrants to Nairobi	Recent Migrants to Nairobi	Recent Migrants to Any City
Disaggregated						
Information	0.04***	0.05***	0.04**	0.02*	0.01	0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.04]	[0.09]	[0.18]	[0.83]
Mentor	0.10***	0.11***	0.10***	0.02**	0.02**	0.02*
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.04]	[0.05]	[0.08]
Group	0.09***	0.10***	0.12***	0.02*	0.03**	0.03**
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
	[0.00]	[0.00]	[0.00]	[0.10]	[0.02]	[0.04]
Model	OLS	OLS	Poisson	OLS	OLS	OLS
p-Val: Info. = Mentor	0.00	0.00	0.00	0.54	0.34	0.07
p-Val: Info. = Group	0.01	0.01	0.00	0.72	0.13	0.04
p-Val: Mentor = Group	0.93	0.84	0.50	0.86	0.48	0.56
Control Mean	0.24	0.30	131.32	0.17	0.11	0.21
Observations	15,976	15,976	15,348	15,468	15,468	15,468
Pooled Treatment						
Any Info.	0.06***	0.07***	0.07***	0.02**	0.02**	0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.03]	[0.04]	[0.21]
Observations	15,976	15,976	15,348	15,468	15,468	15,468

Belief outcomes are measured in baseline surveys after treatment; migration outcomes are measured at endline. Dependent variables in columns 4–6 are binary indicators for whether any member of the household migrated. "Recent" migrants are those who left their home village after the baseline survey. *City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

migrants to Nairobi ($p = 0.02$). This is again driven by net-new urban migration (3 pp. effect, $p = 0.04$).

Given the similar migration impacts across treatment arms, we can improve power and reduce the risk of false discovery by pooling across treatment arms to estimate the impacts of receiving information averaged across the delivery modality (individual, mentor, or group). The lower panel of Table 1 shows that the average effect of information is 2 pp. on both any and recent migration to Nairobi (p-values = 0.03 and 0.04, respectively), and 1 pp. on recent migration to any city ($p = 0.21$).[16]

[16] Treatment impacts on migration to Nairobi over 16 months are only roughly 25–30% of treatment impacts on planned migration to Nairobi over the next year. One possible explanation of this difference is

3.3 Heterogeneity in Migration Responses

Although the three treatment arms produced similar average migration effects, they motivated different households to respond. To characterize this heterogeneity, we apply the generic machine-learning procedure of Chernozhukov et al. (2025): see Appendix C.1 for details and full results. We first construct a proxy for each household's conditional treatment effect. We find substantial heterogeneity in all three treatment arms on migration to Nairobi: a best-linear-predictor test rejects treatment effect homogeneity at $p < 0.01$ in every arm. In the most-responsive quintile, treatment effects on migration are large: 8 pp. in Information, 9 pp. in Mentor, and 15 pp. in Group.

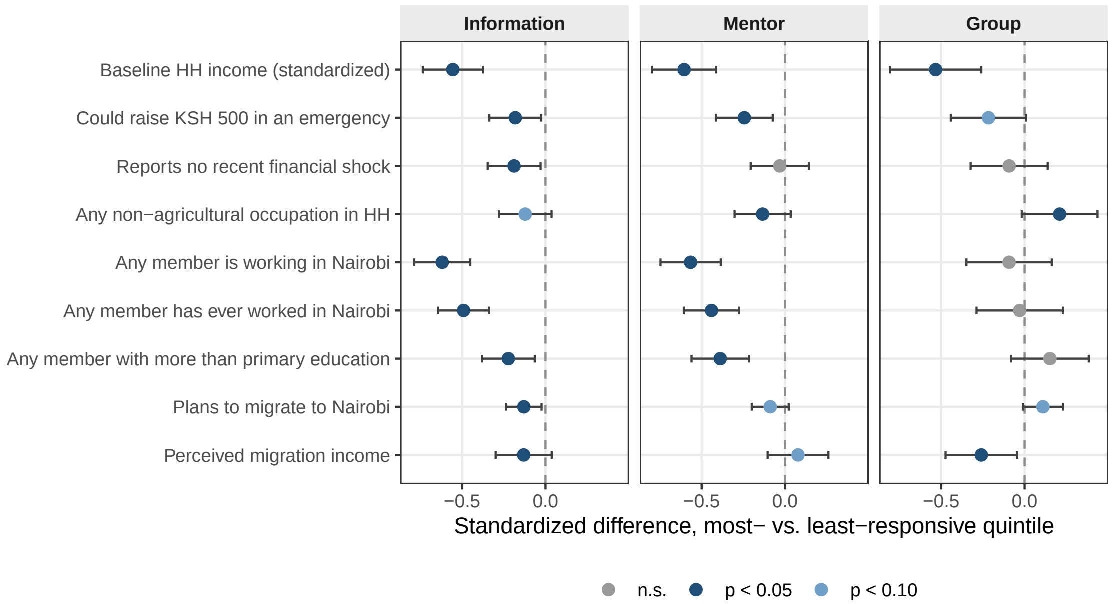
Figure 2 compares the baseline characteristics of households in the most- and least-responsive quintiles. Every arm was more impactful among the economically worse off: the most responsive households have lower baseline income ($\sim$ 0.5 sd, $p < 0.001$) and liquidity ($\sim$ 0.2 sd less able to cover a financial shock, $p < 0.10$). The arms differ sharply, however, in the migration experience of the households they influenced. In Information and Mentor, the most-responsive households are far less connected to Nairobi—fewer have a household member who currently works there (0.29 and 0.26 sd, both $p < 0.001$), has ever worked there (0.25 and 0.22 sd, both $p < 0.001$), or plans to migrate there in the future (0.12 sd and 0.11 sd, both $p < 0.05$). In Group, this gradient is absent or reversed: its most-responsive households are statistically indistinguishable from the least-responsive in Nairobi work experience and are more likely to be planning on migrating there (0.11 sd, $p = 0.05$). Overall, these contrasts indicate that the individualized arms reached poorer households with little prior foothold in Nairobi, while Group primarily moved poorer households that were not comparably detached from Nairobi.

3.4 Impacts on Economic Outcomes

Despite similar average impacts on migration, the impacts of our treatments on downstream economic outcomes differ markedly, as shown in Table 2. The Mentor treatment is the most impactful across the board, increasing our pre-specified welfare index by 0.11 standard deviations ($p < 0.01$, false discovery rate q-value = 0.01). Household income earned in the month before the survey—inclusive of wage income and business profits across all family

that households are over-optimistic about their own migration. Patterns in the control group support this explanation: 24% of control-group households expressed a plan to send an additional migrant to Nairobi within one year, while only 11% actually did so. While this "plan–do gap" may partly reflect the unusually high political uncertainty in Kenya between our intervention and endline survey, other migration studies, such as Baseler (2023), have also found that many households do not follow through on their migration plans in the medium-term.

Figure 2: Classification Analysis of the Most-Responsive Households



Each coefficient shows the difference in baseline characteristics between households in the top and bottom quintile of the predicted treatment effect on migration to Nairobi. For binary characteristics, coefficients are percentage-point differences; baseline income is standardized. Bars are 95% confidence intervals; color indicates statistical significance. Estimates follow the classification analysis of Chernozhukov et al. (2025).

members including migrants—is higher by \$9 on a base of \$97 ($p = 0.03$, $q = 0.05$). Note that our primary measures of income, consumption, savings, and remittances are measured in the month prior to the endline survey, and thus will exclude migration income earned by migrants who returned to the village prior to the recall window, but include any economic impacts downstream of that migration (for example, if migrant income finances a new business in the village). Income earned over the year before the survey—which may be measured with more noise but is more likely to capture migration income for return migrants—is higher by \$63 on a base of \$681 ($p = 0.07$; $q = 0.06$). Adding estimated crop profits to income does not change these results (coeff. = \$11 in the past month on a base of \$119, $p = 0.02$, $q = 0.04$).

When migrants face higher prices in cities, impacts on nominal income will overstate impacts on real standards of living. A typical solution is to deflate nominal income by a price index measuring the cost of a representative basket of goods and services. Unfortunately, such spatially disaggregated price indices are rarely available. We thus construct our own location-specific, quality-adjusted price indices, following our pre-analysis plan, by relying on nationally representative household consumption diaries collected during KIHBS surveys.[17] We apply urban price indices to income earned by urban migrants (net of remittances to

[17] Our adjustment method is described in detail in Appendix C. Estimates are similar in magnitude and remain statistically significant when using quality-unadjusted price measures.

the rural household). We find that treatment impacts on real income are similar to those on nominal income (\$10/month increase in Mentor villages, $p = 0.02$, $q = 0.04$).[18]

Spatial income differences may in part reflect compensating differentials—such as access to amenities like public utilities, education, and healthcare—though Gollin et al. (2021) find that many positive amenities are increasing in population density across much of sub-Saharan Africa. Following our pre-analysis plan, we construct an amenity-adjusted measure of monthly income by asking urban migrants what income would make them indifferent between staying in their destination city and their hometown. We validate this measure by showing that it is strongly positively correlated with several simple subjective and objective measures of quality of life in the city. We then assign migrants their "rural indifference income" when computing amenity-adjusted family income (see Appendix C for details on measurement and validation). We find that adjusting for subjective amenity differences does not substantially affect our estimates (coeff. = \$9/month in Mentor villages, $p = 0.07$, $q = 0.06$). This finding implies that urban migrants perceive the income-adjusted quality of life in the city to be not too different from that in the village, and is consistent with positive qualitative comparisons of life in the city relative to the origin (see Appendix Figure A.5).

In Information villages, impacts on income are directionally similar but slightly smaller than in Mentor villages. Income earned in the past month is higher, compared to the control group, by \$9 ($p = 0.02$, $q = 0.07$). The overall welfare index is higher by 0.04 standard deviations ($p = 0.20$; $q = 0.20$). However, economic impacts in Group villages are small and statistically zero—significantly lower than Information or Mentor impacts across most measures. In Section 5, we rationalize this potentially surprising result by demonstrating that Group increased migration among households likely to face lower migration costs, and who thus exhibit lower marginal returns.

Other Economic Outcomes. We do not detect significant changes in other pre-specified economic outcomes, including consumption, investment in businesses and the household, or access to improved utilities, as shown in Appendix Table A.7. Impacts on subjective well-being are positive in the Mentor arm but statistically insignificant (0.05 sd increase, $p = 0.19$, $q = 0.13$). Savings over the past month is much higher in Information and Mentor villages, by 27% and 34% respectively (p-vals < 0.01, q-vals < 0.05). Subjective financial health improves in Mentor villages by 0.08 sd. ($p = 0.01$, $q = 0.04$) and in Information villages by 0.05 sd. ($p = 0.06$, $q = 0.12$). The larger impacts on savings compared to consumption

[18] While prices are generally higher in Nairobi (see Appendix Table C.3), deflating incomes has only a small impact on our estimates. This is because part of urban income is remitted or brought back to origin villages and because spillover impacts within villages are a large share of total income gains.

Table 2: Economic Outcomes

	Welfare Index	Income	Yearly Income	Income + Crop Profit	Real Income	Amenity-Adusted Income
Information	0.04	8.67**	49.99*	8.09**	9.01**	6.88*
	(0.03)	(3.64)	(29.76)	(4.09)	(3.68)	(4.13)
	[0.20]	[0.02]	[0.09]	[0.05]	[0.01]	[0.10]
	{0.20}	{0.07}	{0.14}	{0.12}	{0.07}	{0.14}
Mentor	0.11***	9.02**	62.96*	11.13**	9.53**	8.55*
	(0.03)	(4.13)	(35.28)	(4.69)	(4.15)	(4.78)
	[0.00]	[0.03]	[0.07]	[0.02]	[0.02]	[0.07]
	{0.01}	{0.05}	{0.06}	{0.04}	{0.04}	{0.06}
Group	-0.03	-2.73	2.83	0.50	-2.70	0.10
	(0.04)	(5.02)	(48.25)	(5.43)	(5.06)	(5.60)
	[0.39]	[0.59]	[0.95]	[0.93]	[0.59]	[0.99]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
p-Val: Info. = Mentor	0.02	0.93	0.69	0.50	0.89	0.71
p-Val: Info. = Group	0.05	0.02	0.31	0.14	0.02	0.20
p-Val: Mentor = Group	0.00	0.03	0.24	0.06	0.02	0.15
Control Mean	-0.04	97.48	680.96	118.61	97.54	120.69
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Impacts are estimated on data from endline surveys. Welfare is a standardized index of all pre-specified economic outcomes. Linear regression is used for all outcomes. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group across outcomes in this table and Appendix Table A.7. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

may reflect a high marginal propensity to save out of migration income, anticipatory savings to finance additional migration, or complementarities between higher migration income and savings (for example to finance discrete, costly investments).

Interpretation. Overall, these findings point to large marginal returns for migrants moving from Information and Mentor treatments but lower and insignificant marginal returns for migrants induced by the Group arm. Together with evidence that the Group arm differentially impacted households with greater migration experience compared to Information and Mentor, this finding suggests heterogeneity in returns across each treatment's complier set. We develop this hypothesis further in Sections 4 and 5.

3.5 Spillovers

To measure the within-village spillovers of providing information, we exploit the random assignment of information across households within Spillover villages. Appendix Table A.8 shows estimates of (1) and (2) on the sample of Control households and untreated Spillover households. We find no significant impacts on beliefs about average incomes in Nairobi or on the household's own potential income from migrating to Nairobi.[19] There are no spillover impacts on migration to Nairobi or to any city (coeffs. = 0.5 and -0.6 pp., p-vals = 0.72 and 0.70 respectively).

Despite the lack of migration spillover impacts, we find positive spillover impacts on economic outcomes. Our pre-specified overall welfare index is higher by 0.13 sd ($p = 0.02$) among untreated households in Spillover villages. Appendix Table A.10 shows spillover impacts on each component of that index. Impacts are broadly positive but statistically insignificant for most components. In particular, income earned in the past month is higher by \$6/month on a base of \$97 ($p = 0.27$).

Why Didn't Information Diffuse Within Villages? The lack of spillovers on beliefs and migration outcomes is potentially surprising given the ease of sharing the information provided. However, there are competing incentives: while informing other households of the returns to migrating may make them more willing to co-migrate, it may also become harder to hide migration income from them, which could be costly for households in the same informal insurance networks. Moreover, a key benefit of sharing the information—convincing the other household to co-migrate—may be unattractive if desirable co-migrants are those who are already well-informed.

To test whether informal insurance networks can help explain the lack of information spillovers, we estimate heterogeneous spillover impacts based on households' relationships in the village using a difference-in-differences framework. Results are shown in Appendix Table A.11. We find that households in Spillover villages with more borrowing connections hold significantly lower beliefs about both typical Nairobi incomes and their own potential Nairobi income compared to the same difference in Control villages. We see no significant differences for migration advice connections or farm help connections. These result suggest the existence of across-household hidden income incentives as one factor maintaining low beliefs about urban incomes.

Economic Spillover Channels. Given the null impacts on migration for untreated households in Spillover villages, economic spillovers are most likely to reflect general-equilibrium impacts in village economies. Appendix Table A.9 presents spillover and direct impacts on village economies to investigate potential spillover channels. We find positive spillovers onto labor income earned in the village (17% increase, $p < 0.01$), and suggestive but smaller

[19] However, given the modest impact of Information on beliefs about potential migration income shown in Table 1, we cannot rule out partial diffusion of the new information.

spillovers onto business profits (coeff. = \$0.76 per month, 31% effect size, $p = 0.20$). While we find increases in labor supply at household businesses in the Information arm, there is no change in labor supply for untreated households in Spillover villages. We find no spillover impacts on labor supply at wage jobs, and a slight increase in commute time (10%, $p = 0.11$). The lack of spillovers on working hours—and the relatively small share of households migrating in response to treatment—are difficult to reconcile with the channel documented in Akram et al. (2017), in which migration reduces the pool of laborers, increasing wages and labor supply for those who remain. Instead, the increase in labor earnings is likely downstream of demand multiplier effects caused by the infusion of cash from migrants (Egger et al., 2022). We find that economic spillovers are generally larger in villages with high trust, high sociality, and a greater degree of financial inter-dependence between households, consistent with spillovers operating through economic relationships within villages (see Appendix Table E.9).

Estimating the Returns to Migrating. Estimating the return to migrating by instrumenting migration with treatment assignment would be biased by economic spillovers. Instead, we estimate the marginal return to migrating for compliers by combining estimates from Information and Spillover villages. Intuitively, because untreated households in Spillover villages were no more likely to migrate than Control households (and we do not observe any change in the selection of migrants in Spillover), the economic gains in this group identify the spillover effects of other households' migration. Subtracting spillover effects from the total economic gains observed in Information villages estimates the gains to migrating households. This procedure, which we formalize in Appendix C.2, gives an estimated marginal return to migrating of about \$50/month compared to an average spillover effect of \$8 per non-migrating household.[20]

Across-Village Spillovers. We attempted to mitigate the risk of across-village spillovers—which would threaten internal validity—at the sampling stage by choosing at most one village per sub-location (see Section 2.2). To test whether spillovers occurred across sub-locations,

[20]Because we analyze income in the month prior to the endline survey, these estimates should be viewed as a snapshot rather than an average over the entire post-intervention period. In particular, they exclude past urban earnings of return migrants (but include downstream impacts of migration on rural income). Consistent with direct effects falling in relative importance over time as migrants return to the village, using a yearlong recall window yields a direct gain of about \$100 per month and a spillover effect of \$3 per month, and thus that direct gains represent 40% of total gains over this broader horizon compared to about 10% in the month prior to the endline survey. While these estimates should be interpreted with caution given the low precision of the economic spillover magnitude, they are in line with that from Khanna et al. (2022), who find that 77% of the long-run income gains from a shock to international migrant income is driven by changes in domestic income.

we compute the share of nearby villages (within 3 km or 10 km) that were assigned to any treatment arm other than Control. While the number of nearby villages is not random, the share of those villages treated is. We do not see any evidence of across-village spillovers, as shown in the bottom panel of Appendix Table A.8: beliefs, migration, and economic outcomes are not significantly different depending on the treated share of nearby villages.

4 A Model of Heterogeneous Experimental Returns

Our main experimental finding—similar first-stage impacts on migration across treatments but widely varying economic returns—is consistent with heterogeneity in the marginal benefits of migrating. In this section, we develop a heterogeneous-agent migration model designed to offer predictions about which types of migrants will exhibit higher experimental returns. In Section 5, we test these predictions in our data and describe why our treatments acted on different types. In Section 6, we estimate the model to quantitatively assess the role of heterogeneity in returns and to characterize the aggregate role of information frictions in the economy.

4.1 Model Environment

The economy contains a continuum of households in two regions (rural r and urban u) and two sectors (agricultural a and non-agricultural n). Agricultural output is produced only in the rural region. Non-agricultural output can be produced in both regions.[21] Households choose their region and sector to maximize income net of migration costs, potentially subject to information frictions that distort perceived urban incomes.

Preferences. The economy is populated by a unit measure of households indexed by i. Each household chooses agricultural consumption c_i^a and non-agricultural consumption c_i^n to maximize Stone-Geary preferences with a subsistence requirement $\bar{a}$ in agricultural consumption:

$$\max_{c_i^a, c_i^n} \ \log(c_i^a - \bar{a}) + \nu \log(c_i^n)$$

such that

$$p^a c_i^a + p^n c_i^n \leq y_i + \pi + \widetilde{m},$$

where y_i is household i's labor income, π is the profit of the representative firm, and $\widetilde{m}$ is a uniformly rebated migration cost.

[21] In our baseline data, 20% of rural households earn most of their income from non-agricultural activities.

Endowments. Each household supplies one unit of labor inelastically to a sector of their choice. Households differ in their sector-specific productivity draws *à la* Roy (1951): urban non-agricultural productivity $z^n_{u,i}$, rural non-agricultural productivity $z^n_{r,i}$, and rural agricultural productivity $z^a_{r,i}$. These are distributed log-normally. A household's productivity draws in rural agriculture and non-agriculture are potentially dependent on the urban productivity draw, according to the parameter d:

$$
\begin{aligned}
z^n_{u,i} &= \exp \varepsilon^n_{u,i}, & \varepsilon^n_{u,i} &\sim \mathcal{N}(0, \sigma_u^2) \\
z^n_{r,i} &= \exp\left(d \log z^n_{u,i} + \varepsilon^n_{r,i}\right) & \varepsilon^n_{r,i} &\sim \mathcal{N}(0, \sigma_r^2) \\
z^a_{r,i} &= \exp\left(d \log z^n_{u,i} + \varepsilon^a_{r,i}\right) & \varepsilon^a_{r,i} &\sim \mathcal{N}(0, \sigma_r^2)
\end{aligned}
$$

Households also draw a migration cost m_i from a log-normal distribution:

$$
m_i = \exp\left(\mu_m + \varepsilon_{m,i}\right), \quad \varepsilon_{m,i} \sim N(0, \sigma_m^2).
$$

Households employed in urban non-agriculture, rural non-agriculture, or rural agriculture are paid wage w^n_u, w^n_r, or w^a_r, respectively, per efficiency unit of labor.

Sector Choice and Migration. Households are born in one of the two regions. Households born in the urban region u work in the urban non-agricultural sector, cannot migrate, and earn $y^n_{u,i} = z^n_{u,i} w^n_u$.[22] Households born in the rural region r have three options:

1. Stay in the rural region r, work in the rural non-agricultural sector, and earn $y^n_{r,i} = z^n_{r,i} w^n_r$;

2. Stay in the rural region r, work in the rural agricultural sector, and earn $y^a_{r,i} = z^a_{r,i} w^a_r$; or

3. Migrate to the urban region u, work in the urban non-agricultural sector, and earn $\frac{1}{1+m_i} y^n_{u,i} = \frac{1}{1+m_i} z^n_{u,i} w^n_u$ net of the proportional migration cost $\frac{1}{1+m_i}$. We interpret the migration cost to be inclusive of both monetary costs—like transportation to the city—and non-monetary costs, like job search costs or non-wage amenities.

Information Frictions. Rural households' migration choices are distorted by an information friction γ_i. While households perceive rural incomes $y^n_{r,i}$ and $y^a_{r,i}$ accurately, their

[22] Even though urban-to-rural migration flows are non-trivial (Young, 2013), we prohibit such flows as a simplification, given that the experiment focuses entirely on inducing rural-to-urban migration.

perceived urban non-agricultural income is $\frac{1}{1+\gamma_i}\frac{1}{1+m_i}y^n_{u,i}$. With $\gamma_i > 0$, households proportionally understate the income they would earn if they were to migrate. Thus, each household chooses a sector and region by solving

$$\max\left\{y^n_{r,i},\ y^a_{r,i},\ \frac{1}{1+\gamma_i}\frac{1}{1+m_i}y^n_{u,i}\right\}.$$

Therefore, rural household i will migrate if and only if

$$\frac{1}{1+\gamma_i}\frac{1}{1+m_i}y^n_{u,i} \geq \max\left\{y^n_{r,i},\ y^a_{r,i}\right\}.^{23} \tag{3}$$

4.2 Selection and the Marginal Gains From Migrating

Which rural households choose to move to the city? Which exhibit higher gains from lowering migration barriers? Our model offers the following proposition (see Appendix D.3 for the proof):

Proposition 1 (migrant selection)**.** Consider a continuum of rural households with common $(\gamma_i, m_i, \varepsilon^n_{r,i}, \varepsilon^a_{r,i})$ but varying $\varepsilon^n_{u,i}$. Assume $d \neq 1$.[24] Then:

1. There exists a unique threshold $\widehat{\varepsilon}^n_{u,i}$ and a corresponding $\widehat{z}^n_{u,i} = \exp\widehat{\varepsilon}^n_{u,i}$ at which a household with $z^n_{u,i} = \widehat{z}^n_{u,i}$ satisfies the migration condition (Equation 3) with equality, making them indifferent between migrating and staying:

$$\frac{1}{1+\gamma_i}\frac{1}{1+m_i}\widehat{z}^n_{u,i}w^n_u = \max\left\{(\widehat{z}^n_{u,i})^d\exp\varepsilon^n_{r,i}w^n_r,\ (\widehat{z}^n_{u,i})^d\exp\varepsilon^a_{r,i}w^a_r\right\}.$$

2. The decisions of households with $z^n_{u,i} \neq \widehat{z}^n_{u,i}$ depend on the parameter d:

   (a) If $d < 1$:

   - *Positive selection:* All households with $z^n_{u,i} > \widehat{z}^n_{u,i}$ migrate,
   - *Threshold increases with costs:* $\partial\widehat{z}^n_{u,i}/\partial\gamma_i > 0$ and $\partial\widehat{z}^n_{u,i}/\partial m_i > 0$.

   (b) If $d > 1$:

   - *Negative selection:* All households with $z^n_{u,i} < \widehat{z}^n_{u,i}$ migrate,
   - *Threshold decreases with costs:* $\partial\widehat{z}^n_{u,i}/\partial\gamma_i < 0$ and $\partial\widehat{z}^n_{u,i}/\partial m_i < 0$.

[23] To break ties, we assume that households that are indifferent between migrating and staying choose to migrate.

[24] If $d = 1$, the migration decision becomes trivial and no longer depends on $z^n_{u,i}$.

Proposition 1, illustrated in Panel A of Figure 3, shows that the nature of selection depends on the relationship between rural and urban productivities as governed by the parameter d. When d is positive but less than one, the benefit of migrating rises faster in $z^n_{u,i}$ than does the opportunity cost: high-$z^n_{u,i}$ households are still more likely to migrate. Thus, the $d < 1$ case is characterized by positive selection: migrants are more productive than stayers and the benefit of migrating for the worker at the margin, $\widehat{z}^n_{u,i}$, is increasing in the cost of migrating m_i and the information friction γ_i. However, when $d > 1$, the opportunity cost of migrating rises faster in $z^n_{u,i}$ than does the benefit: households that would be productive in the city are likely to be disproportionately more productive in the rural sectors and choose to stay. Thus, the $d > 1$ case is characterized by negative selection: migrants are less productive than stayers and the benefit of migrating at the margin is decreasing in the cost of migrating and the information friction.[25]

Selection in the Data. Our model predicts that households facing higher migration costs will exhibit higher marginal benefits from migrating if and only if migrant selection is positive in that environment. As shown in Panel B of Figure 3, migrants in our sample of Kenyan households appear strongly positively selected on education compared to non-migrants from the same origin village. Migrants have a higher average education than non-migrants in 91% of villages, and this difference is more extreme in villages where the migration rate is low (and thus where average costs are presumably high). Finally, the model predicts that migrants coming from high-cost regions should earn more in the city, which is also borne out in our data.

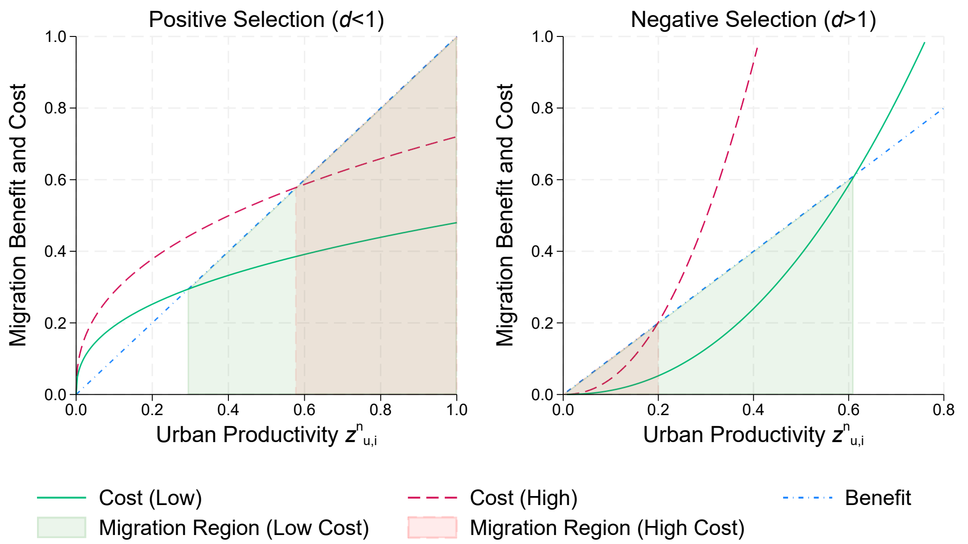
5 Heterogeneous Returns in the Experiment

This section analyzes differences in economic impacts across treatment arms through the lens of our model. We first show that our Information and Mentor treatments increased migration among groups likely to face higher migration costs—which our model predicts should exhibit higher marginal gains in our setting—compared to Group. We then show that households with higher estimated migration costs indeed exhibit greater experimental returns in our data. Finally, we discuss why our Group treatment failed to increase migration among high-cost households.

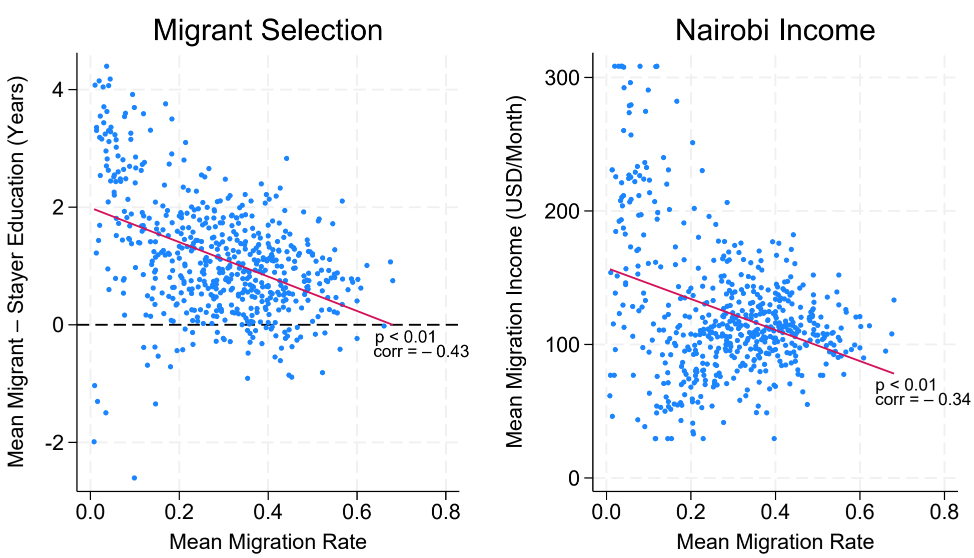
[25] Note that a negative d is allowed by the model, and gives the same predictions as the $0 \leq d < 1$ case. For $d < 0$, the benefit of migrating (the urban wage) rises in $z^n_{u,i}$ while the opportunity cost falls due to falling rural wages: households that draw a high urban productivity $z^n_{u,i}$ are likely to migrate.

Figure 3: Selection and Marginal Returns in the Model and Data

Panel A: Marginal benefits are higher for high-cost households if and only if migrant selection is positive.



Panel B: Migrant selection is positive in almost all villages, and decreasing in the village migration rate.



Panel A shows migration benefit and cost curves for low-cost andhigh-cost types in both positive ($d < 1$) and negative ($d > 1$) selection environments. Migration *benefit* is urban income $z^n_{u,i}w^n_u$; *cost* is inclusive of all frictions and opportunity costs: $(1+\gamma_i)(1+m_i)\max\left\{(z^n_{u,i})^d \exp \varepsilon^n_{r,i} w^n_r,\ (z^n_{u,i})^d \exp \varepsilon^a_{r,i} w^a_r\right\}$. **Panel B** shows how migrant selection (mean migrant education minus mean stayer education) and income earned in Nairobi vary by the migration rate (a proxy for lower cost). Each dot is a village, with means computed at the village level.

5.1 Information and Mentor Influence High-Cost Households

Our machine-learning procedure, described in Section 3.3, revealed that the Information and Mentor treatments were especially impactful among households with less migration experience along several dimensions. Reinterpreted through the lens of our model, this finding suggests that those migrating in the Information and Mentor arms may have faced higher costs than those in Group. However, these costs are not directly observable and must be proxied. Following the logic that migration rates should be higher among groups facing lower costs, we estimate a lasso regression predicting migration at baseline on a set of potential cost predictors measured in our census surveys.[26] We retain as proxies those with non-zero estimated coefficients and additionally compute fitted values from these models and subtract them from one. We then compute treatment impacts on migrant selection by estimating equation (1) on the sample of households that sent migrants to Nairobi as of the endline survey with pre-treatment cost proxies as dependent variables.

We find that Information, and especially Mentor, shifted the composition of migrants at endline toward those with higher estimated costs, as shown in Appendix Figure A.7. On average, estimated costs among households migrating in the Information arm were 0.13 standard deviations higher compared to those in Control ($p = 0.02$). This difference is driven by households with less migration experience (current or former members working in Nairobi), those that did not plan on migrating prior to treatment, and those with no pre-treatment social connections in Nairobi. Mentor draws in even higher-cost households, with an average estimated cost difference of 0.21 sd ($p < 0.01$). This difference is driven again by prior experience in Nairobi, pre-treatment plans to migrate, and existing social connections in Nairobi. Unlike the Information arm, the Mentor arm is somewhat more impactful among those with a primary education or less. In contrast, migrants from Group look very similar to those from Control (cost difference = -0.01 sd).

5.2 High-Cost Households Exhibit Higher Marginal Gains

Do migrants facing higher costs exhibit higher experimental returns to migrating? Answering this question is complicated by economic spillovers, which may be heterogeneous in distinct ways from private returns. We therefore estimate heterogeneous treatment impacts on income earned in Nairobi in Appendix Table A.12. We find that households with greater predicted costs experienced greater impacts on Nairobi income. In our pooled specifications, income gains are about 20% higher per standard deviation of estimated cost, with similar

[26] These proxies are likely negative correlates of migration benefits as well as positive correlates of migration costs. The finding that Group induced low-cost (and thus high-benefit) migrants, yet produced lower income gains, must therefore be driven by the cost side.

results for monthly and yearly recall windows and for costs estimated through a restricted (lasso) or a full (OLS) model. p-Values on the pooled interaction term range from 0.04 to 0.07. Disaggregated results are similar though slightly noisier: p-values on Information and Mentor interaction terms range from 0.03 to 0.12; Group interaction coefficients are positive but statistically insignificant.

5.3 Group Dissemination Favors Low-Cost Households

Why did village-level meetings in the Group arm lead migrants facing lower costs to move, while the individualized arms induced migrants facing higher costs? Our program data suggest that households with more migration experience dominated the group discussions and crowded out less experienced, higher-cost households both during and after the intervention. Such an explanation would be consistent with the image-concerns mechanism of Banerjee et al. (2024), which reduced learning when information was broadcast compared to when it was seeded.[27]

Engagement During Treatment. To test whether high-cost households were less engaged during group meetings, we leverage data collected by program staff on the three most active participants in each group meeting. We find that households facing higher migration costs were much less likely to be among the most active participants (coeff = 0.08 sd per one-sd higher cost; $p < 0.01$). This difference is driven by lower participation among households with no migration experience, no social connections in Nairobi, and less education. In contrast, high-cost households appear no less engaged during the one-on-one meetings in Information and Mentor: estimated engagement differences between low and high cost households are close to zero.[28] Overall, these results suggest that high-cost households—who are less experienced with migration and may have social image concerns around discussing a topic they are unfamiliar with—are less engaged in a social setting compared to the 1-on-1 discussions where they were able to ask questions of the facilitator in a private setting.

[27] It may also be that the information communicated during group meetings was biased, for example if migrants who earn little in Nairobi are more likely to return to the village and therefore be available during the group meeting. The evidence shown in Appendix Table A.16 is consistent with this: higher income strongly predicts longer intended duration in Nairobi (coeff. = 1.5 years per standard-deviation increase in income, $p < 0.01$).

[28] While we lack a direct measure of engagement outside group meetings, we proxy for engagement in one-on-one meetings using the time spent on the treatment as automatically recorded by the facilitator's data collection device. After partialling out a facilitator fixed effect, residual variation in time spent on treatment is likely to reflect questions from the household or back-and-forth discussion between the household and facilitator, and thus serve as a measure of household engagement. Supporting this assumption, we measure significantly higher 1-on-1 intervention engagement for those who report planning to use the mentor program (by 0.38 sd, $p < 0.01$) and who plan to migrate to Nairobi (by 0.12 sd, $p = 0.03$) after hearing the information.

Do High-Cost Migrants Learn from Experienced Types? Even if more experienced households were more active in group discussions, it may be that high-cost households nevertheless benefited from hearing advice from knowledgeable former migrants. Our data suggest the opposite: that when experienced types are active in group discussions, they primarily influence other experienced types. Appendix Table A.14 presents heterogeneous effects of the Group treatment based on household characteristics interacted with the characteristics of active group members, or "leaders."[29] Among households with a migrant in Nairobi prior to treatment, being in a group meeting with a leader who also had a current Nairobi migrant is associated with a 13 pp. greater migration impact ($p < 0.01$) compared to a meeting with no experienced leader. In contrast, the same difference for households with no migrant in Nairobi is close to zero. Altogether, these patterns are consistent with meetings led by experienced households being less useful for households facing higher migration costs. Instead, group meetings appear to act like a nudge for experienced households, whether by acting as a social commitment device or reducing coordination costs.[30]

Co-Migration. In addition to being less engaged during group meetings, high-cost households were also less likely to co-migrate with others from their village after group meetings, compared to the Information arm. In Information villages, treatment increases co-migration by 0.6 pp. (on a base of 0.5 pp., $p < 0.01$), as shown in column 1 of Appendix Table A.15. Because the 1-on-1 information treatment did not explicitly facilitate co-migration in any way, this implies that it led households to seek out others in their village to migrate with them after the treatment. The impact on co-migration is greater for high-cost households by 0.4 pp. per standard deviation ($p = 0.09$), as shown in column 2. While the Group treatment also increased co-migration (by 0.4 pp., $p = 0.16$), it did so predominantly for low-cost migrants (coeff. on cost interaction = -0.4 pp. per sd; $p = 0.21$). The Mentor arm did not increase co-migration, possibly because mentors substituted for the support provided by other co-migrants from the village. These findings suggest that group meetings reduce coordination costs, helping households who were already planning on migrating find co-migrants, but may crowd out inexperienced households who would otherwise have sought out experienced co-migrants after treatment.

[29] Note that who is active during meetings is endogenous, and so these results should not be interpreted as causal. Because only members residing in the village at the time of treatment could participate in village meetings, we analyze impacts on recent migration to Nairobi; impacts including baseline migrants are similar.

[30] We do not find evidence of a social commitment device mechanism: as shown in Appendix Table E.2, we do not find greater migration impacts of the Group treatment among households whose prospective migrants say they have trouble following through on plans.

6 Quantitative Estimation

Can the differences in migrant selection across treatment arms quantitatively explain the lower economic returns to group dissemination? In this section, we estimate the migration model from Section 4 to help answer this question. We proceed to use our model to quantify the aggregate role of information frictions in the Kenyan economy.

6.1 Equilibrium in the Model

The non-agricultural good n is produced by a representative firm in each of the two regions, and the agricultural good a is produced by a representative farm in the rural region using labor according to the production functions:

$$Y_u^n = A_u^n (L_u^n)^\theta, \quad Y_r^n = A_r^n (L_r^n)^\theta, \quad Y_r^a = A_r^a L_r^a.$$

The urban non-agricultural firm, the rural non-agricultural firm, and the rural farm pay wages w_u^n, w_r^n, and w_r^a, respectively, per efficiency unit of labor.

The first-order conditions of the consumption problem produce the following consumption rules (after normalizing the non-agricultural price p^n to 1):

$$\begin{aligned} c_i^n &= \frac{\nu}{1+\nu}\left(y_i + \pi + \widetilde{m} - p^a \bar{a}\right), \\ c_i^a &= \frac{1}{p^a}\frac{1}{1+\nu}\left(y_i + \pi + \widetilde{m}\right) + \frac{\nu}{1+\nu}\bar{a}. \end{aligned}$$

Goods markets clear, with goods being traded between regions costlessly:

$$\int_i c_i^n = Y^n = Y_u^n + Y_r^n, \qquad \int_i c_i^a = Y^a = Y_r^a.$$

Labor markets clear:

$$\begin{aligned} L_u^n &= \int_{i\in\Omega_u^n} z_{u,i}^n, \quad w_u^n = \theta A_u^n (L_u^n)^{\theta-1}, \\ L_r^n &= \int_{i\in\Omega_r^n} z_{r,i}^n, \quad w_r^n = \theta A_r^n (L_r^n)^{\theta-1}, \\ L_r^a &= \int_{i\in\Omega_r^a} z_{r,i}^a, \quad w_r^a = p^a A_r^a, \end{aligned}$$

where Ω_u^n, Ω_r^n, and Ω_r^a are the sets of households choosing urban non-agriculture (including urban locals), rural non-agriculture, and rural agriculture, respectively. Aggregate profit is distributed uniformly to households:

$$\pi = \Pi = Y^n - w_u^n L_u^n - w_r^n L_r^n + p^a Y^a - w_r^a L_r^a.$$

Finally, migration costs are rebated uniformly to all households:[31]

$$\widetilde{m} = M = \sum_{i \in \Omega_m} z_{u,i}^n w_u^n \left(\frac{m_i}{1 + m_i} \right).$$

6.2 Model Estimation

Parameters measured directly in the data or obtained from prior literature include the share of the population born in the urban region, the labor share θ, the non-agricultural consumption weight ν, and the dispersion of migration costs σ_m. The productivity of urban non-agriculture A_u^n and of rural agriculture A_r^a are normalized to 1. Parameters estimated using the simulated method of moments include the subsistence level of agricultural consumption $\bar{a}$, the rural productivity level A_r^n, the standard deviation of urban productivity σ_u, the standard deviation of rural productivity σ_r, the elasticity of rural productivities with respect to urban non-agricultural productivity d, and the average migration cost μ_m. Appendix D.1 discusses the estimation procedure and the parameter-moment mapping in detail. Table 3 summarizes the estimated parameters, their corresponding moments, moment values in the data, and moment values in the estimated model.

Relationship Between Rural and Urban Productivities. In the model, the direction of selection depends on the value of d, the elasticity of rural productivities with respect to the urban productivity (Proposition 1). We estimate d by targeting the slope of the relationship between log rural and log urban income for households that are similar on observables, which is slightly positive: Appendix D.1 discusses the details. This results in the estimated $d = 0.075$: well into the positive selection region of $d < 1$, reinforcing the empirical finding of Figure 3 that migrant selection is positive in our setting.

Information Frictions. All households are assigned the same initial information friction $\gamma_i = \gamma$. We measure information frictions using survey data (see Section 2.4): the average ratio of true income to perceived income across demographic groups—that is, the average of the gaps displayed in Figure 1—is 0.559. This implies a value of $\gamma = \frac{1}{0.559} - 1 = 0.789$.

[31] Because we model migration costs as monetary, any amount paid by migrating households must surface elsewhere in the economy, as otherwise markets will not clear.

Table 3: Model Estimation

Parameter	Value	Moment	Data	Model
$\bar{a}$	5.569	food cons. share	0.345	0.345
A_r^n	0.113	median urban-rural inc. gap	5.147	5.145
σ_u	1.547	SD of log urban inc.	1.491	1.492
σ_r	1.982	SD of log rural inc.	1.569	1.569
d	0.075	log urban-rural inc. slope	0.065	0.065
μ_m	2.869	migration rate	0.171	0.171

6.3 Experimental Treatments in the Model

Each treatment arm is applied to a separate model economy. One-third of all rural model households are randomly selected to receive the treatment (closely corresponding to the average share of households treated per village). Information treatments are represented with a reduction in the information friction γ_i faced by each household.

We discipline each treatment by requiring the model to match the observed migration impact and selection into migration.[32] We find the intensity of each treatment (the new value γ') required by the model to reproduce the experimental migration impacts shown in column 4 of Table 1. To reproduce treatment effect heterogeneity, we allow the treatment intensity to depend on the household's migration cost m_i: specifically, we model each treatment as impacting only households with m_i above (or below) some percentile of m_i. We solve the cutoff percentile required by the model to reproduce the migration cost differences across migrants from each treatment group discussed in Section 5.1.

Table 4: Model: Experimental Impacts, Matching Migration and Selection

	Matched Impacts					
	Migration	Migrant Selection	Pctile of m_i Treated	γ' (Among Treated)	Impact on Beliefs (Among Treated)	Income Impact
Information	0.018	0.13	Top 56%	0.38	68%	4.5%
Mentor	0.024	0.21	Top 50%	0.30	121%	6.2%
Group	0.022	-0.01	Bottom 62%	0.54	28%	2.4%

"Migration" is the difference in the migration rate between treated and control households. "Income Impact" is the mean change in observed income (gross of migration cost) divided by mean baseline observed income for treated households. For each treatment arm, "γ' (Among Impacted)" and "Pctile of m_i Treated" are calibrated jointly to match the measured "Migration Impact" and "Migrant Selection" (standardized migration cost difference between treatment and control migrants).

[32] An alternative approach is to match the measured effect on beliefs about migration incomes. Appendix D.4 explores this approach and shows that it leads the model to understate both migration and income impacts, suggesting that focusing on beliefs about migration impacts is an overly narrow interpretation of the experimental treatments.

Table 4 presents the results of each experiment. To match the experimental migration impacts, the model requires substantial changes in households' beliefs: among those impacted by a given treatment, beliefs must rise by 28–121%. To reproduce the finding that migrants in the Information and Mentor arms face higher migration costs than control migrants, the model requires roughly the top half of households by migration cost to engage with the treatments. To reproduce the finding that migrants in the Group arm have similar costs to control migrants, the model requires the top 40% or so of households by cost to not engage with the Group treatment, reflecting the findings in Section 5.3.

Crucially, matching the experimental impacts on migration and migration selection allows the model to match the experiment's income impacts fairly closely. It predicts a 4.5% increase in income in the Information arm (compared to 6–9% in the data), 6.2% in the Mentor arm (9–10% in the data), and only 2.4% in the Group arm ($\sim 0\%$ in the data). Despite having a migration impact similar to the two other arms, the Group arm in the model has by far the lowest income impact, just as the experiment found. We interpret this as evidence that differences in migrant selection are quantitatively important explanations of differences in economic impacts.

6.4 The Aggregate Role of Information Frictions

Simulated partial-equilibrium experiments in the model match the key experimental treatment effects. This suggests that the model can be used to assess the impacts of reducing information frictions at scale, in general equilibrium. Doing so allows us to assess the effects of information treatments were they to be rolled out at a national level, and to characterize the macroeconomic importance of information frictions.

Table 5 shows the results of scaling the targeted treatments to the national level. Scaling up the Information treatment boosts the national migration rate by 1.4 percentage points from a baseline of 17%. Real non-agricultural output goes up by 3.5%. The agricultural productivity gap and the urban-rural income gap fall modestly. Scaling up the Mentor treatment nationally is even more effective at boosting the migration rate, output, and lowering the spatial gaps. Finally, scaling the Group treatment nationally has the smallest effect on non-agricultural output but the largest effects on income gaps, since it selects for unproductive migrants.

While all treatments increased villagers' beliefs about urban earnings, the magnitude of these effects is dwarfed by the scale of the original gaps shown in Figure 1. What is the overall role of biased information in distorting migration? We answer this question by removing all information frictions in the model (setting $\gamma_i = 0$ for everyone). This

"Perfect Information" exercise suggests that removing information frictions entirely could be transformative. Our model predicts that the migration rate would increase from 17% to 22%, non-agricultural output would grow by 6% (while agricultural output would drop 0.5% as agricultural labor supply is reduced), the agricultural productivity gap would fall from 2.64 to 2.33, and the rural-urban income gap would fall from 5.15 to 4.09. These changes are generated entirely through reduced information frictions, without any changes to the migration cost m_i. It remains an open question, however, why even our more intensive information treatments—involving conversations with former migrants at home or in the city—had only modest impacts on beliefs. If the extent of updating is correlated with potential gains from migrating, the macroeconomic implications of "fully informing" villagers could obviously be quite different.

Table 5: Model: Aggregate Impacts of Universal Rollouts, Matched Impacts

	Migration Rate	Real Non-Agric. GDP	Real Agric. GDP	Agricultural Productivity Gap	Urban-Rural Income Gap
Baseline	0.171	1.000	1.000	2.638	5.145
Information	0.185	1.035	1.000	2.599	4.994
Mentor	0.189	1.048	1.001	2.593	4.955
Group	0.188	1.017	0.998	2.504	4.711
Perfect Information	0.219	1.060	0.995	2.326	4.092

Universal treatments matching migration impact and migration cost selection are applied to all rural households. All economies are solved in general equilibrium. Real non-agricultural and agricultural GDPs are expressed relative to the baseline.

7 Discussion

This study provides new experimental evidence on the role of information constraints affecting internal migration in Kenya. We show that under-estimation of urban earnings is widespread and that many rural households lack urban social connections. Simple programs that inform households about urban earnings or expand their social networks at the destination can substitute for existing networks, and the economic returns to these interventions can be large.

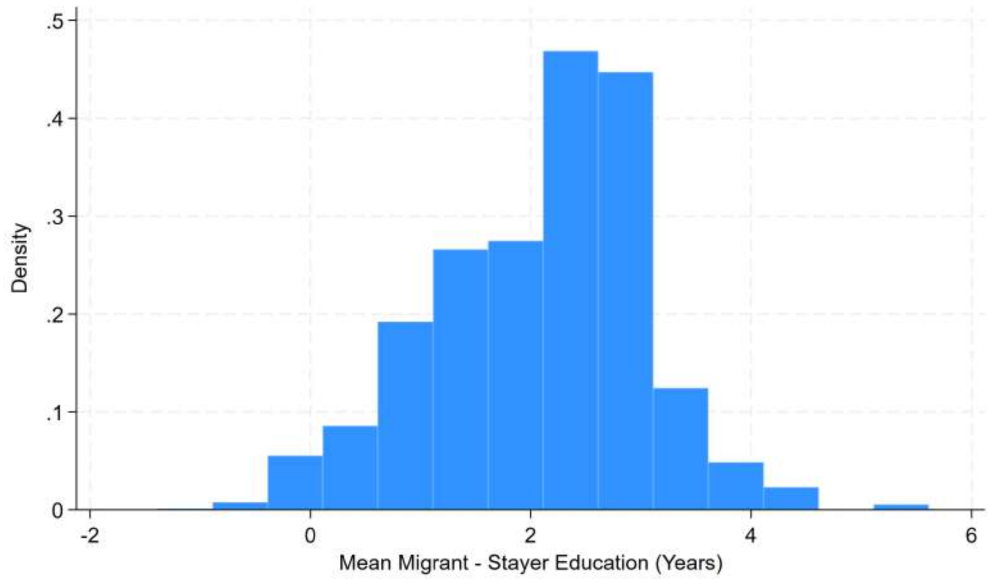
7.1 Implications for Migration Interventions

Our findings have important implications for the design and targeting of migration interventions. Given the substantial rural-urban wage gaps present in Kenya and elsewhere, many governments and humanitarian organizations are looking for programs that can make migration easier for rural households. Information treatments are attractive given their low marginal cost, and widespread dissemination may appear especially cost-effective. Our results caution against such an approach: while village-level meetings were indeed low-cost and effective at increasing migration, they brought no measurable economic benefits. Our explanation is that these interventions reached the wrong households. Our findings suggest that successful interventions will be those that can reach households without former migration experience, who tend to be less socially connected at the origin and destination and less likely to be planning on migrating in the future. Given that many interventions are likely to implicitly target experienced households—these households are more likely to show up to migration-related events and to be more engaged during migration interventions—deciding whom to target should be a key consideration in intervention design.

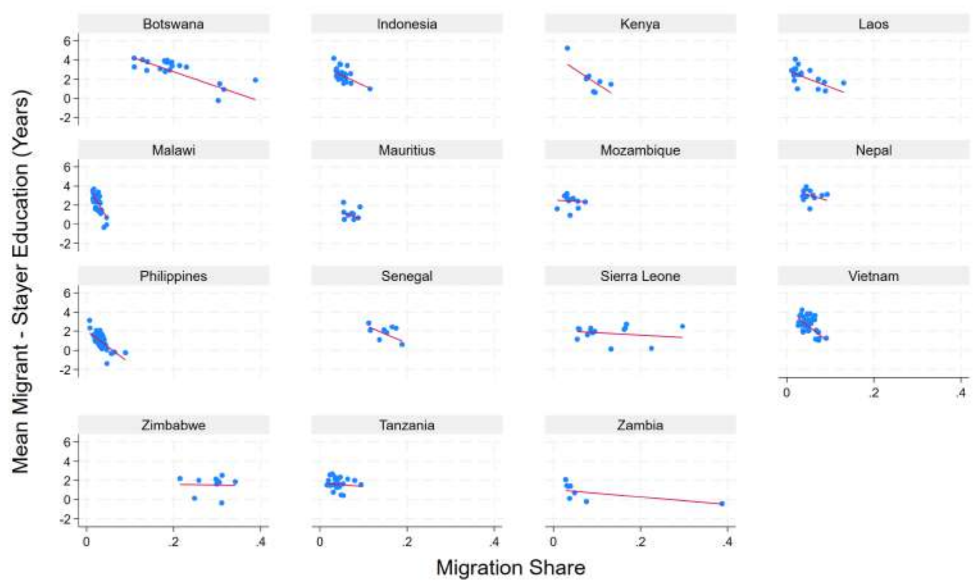
As illustrated by our model, we expect to observe higher marginal returns to migrating for less experienced households in environments where migrants are positively selected from origin populations. How common is positive migrant selection in low-income countries? We assess this using cross-country data available through IPUMS International (see Appendix C.3 for sampling and measurement details). As shown in Figure 4 Panel A, positive selection is the norm in most places within the sample of 15 sub-Saharan African or South or Southeast Asian countries where this exercise is possible. For 97% of 1st subnational geographic locations, migrants have a greater average education compared to non-migrants from the same locations; the difference in the median location is 2 years. Panel B reproduces the regressions shown in Figure 3 Panel B, by country, with each dot corresponding to a 1st subnational geographic location. The results are broadly consistent with our findings in Kenya: positive selection is most extreme in places where out-migration rates are low, and migration costs are presumably high. The relationship between migrant selection and the migration rate is negative—as predicted by our model—in all 15 analyzed countries. Note that positive selection is by no means universal: for example, Baseler et al. (2025a) find negative selection into internal migration in India along household consumption. In adapting a migration intervention to a new setting, researchers can characterize the nature of migrant selection—which can be done fairly easily with observational data—to help inform whether an intervention should target the experienced or the inexperienced.

Figure 4: Migrant Selection in Cross-Country Data

Panel A: Migrants are positively selected in most locations.



Panel B: In many countries, migrant selection is positive and decreasing in the location-level migration rate.



An observation is a 1st subnational geographic location. Data from IPUMS International (see Appendix C.3 for measurement details). Migrant selection is mean migrant education minus mean stayer education from a given location. Migration rate shows the location-level number of migrants as a share of population, a proxy for lower-cost locations. All estimates account for population weights.

7.2 Conclusion

We find large economic gains to individualized migration interventions, both for migrating households and their neighbors. Because the marginal cost of the mentorship program was small, it is the most cost effective for most economic outcomes among the programs we study. However, our study is unable to assess impacts on urban workers, who may be crowded out through increased competition in labor markets or pressure on infrastructure and public services. In view of this concern, our finding that significant economic gains are attainable from relatively low additional migration is arguably reassuring. Moreover, a sizable share of those income gains accrue to non-migrating households, implying that the gains from encouraging rural-urban migration are not as concentrated as they might appear from the migration impacts alone.

We note, however, that the migration response we observe is small relative to the share of non-migrating households. This suggests that information barriers alone are not the primary factor preventing migration for most households. We therefore view additional research into which programs can assist potential migrants as particularly high value.

References

Abebe, Girum, A Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn, "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City," *The Review of Economic Studies*, 12 2020, *88* (3), 1279–1310.

Adamopoulos, Tasso, Loren Brandt, Chaoran Chen, Diego Restuccia, and Xiaoyun Wei, "Land Security and Mobility Frictions," *The Quarterly Journal of Economics*, July 2024, *139* (3), 1941–1987.

Agness, Daniel, Travis Baseler, Sylvain Chassang, Pascaline Dupas, and Erik Snowberg, "Valuing the Time of the Self-Employed," *The Review of Economic Studies*, 01 2025, p. rdaf003.

Akram, Agha Ali, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Effects of Emigration on Rural Labor Markets," Working Paper 23929, National Bureau of Economic Research October 2017.

Alvarez, Jorge A., "The Agricultural Wage Gap: Evidence from Brazilian Micro-data," *American Economic Journal: Macroeconomics*, January 2020, *12* (1), 153–73.

Ambler, Kate, "Don't tell on me: Experimental evidence of asymmetric information in transnational households," *Journal of Development Economics*, 2015, *113* (C), 52–69.

Anderson, Michael L., "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 2008, *103* (484), 1481–1495.

Banerjee, Abhijit and Sandra Sequeira, "Learning by searching: Spatial mismatches and imperfect information in Southern labor markets," *Journal of Development Economics*, 2023, *164*, 103111.

_ , Arun G. Chandrasekhar, Esther Duflo, and Matthew O. Jackson, "The Diffusion of Microfinance," *Science*, 2013, *341* (6144), 1236498.

_ , Emily Breza, Arun G. Chandrasekhar, and Benjamin Golub, "When Less Is More: Experimental Evidence on Information Delivery During India's Demonetisation," *The Review of Economic Studies*, 2024, *91* (4), 1884–1922.

Banerjee, Abhijit V., Arun G. Chandrasekhar, Esther Duflo, and Matthew O. Jackson, "Using Gossips to Spread Information: Theory and Evidence from Two Randomized Controlled Trials," *The Review of Economic Studies*, 2019, *86* (6), 2453–2490.

Barnett-Howell, Zachary, Travis Baseler, and Thomas Ginn, "The Role of Information and Networks in Migration," 2023. AEA RCT Registry. June 01. https://doi.org/10.1257/rct.10051-2.0.

Baseler, Travis, "Hidden Income and the Perceived Returns to Migration," *American Economic Journal: Applied Economics*, October 2023, *15* (4), 321–52.

_ , Ambar Narayan, Odyssia Ng, and Sutirtha Sinha Roy, "Social Welfare Portability and Migration: Evidence from India's Public Distribution System," *American Economic Journal: Economic Policy*, 2025. Forthcoming.

_ , Thomas Ginn, Ibrahim Kasirye, Belinda Muya, and Andrew Zeitlin, "Mentoring Small Businesses: Evidence from Uganda," 2025. Working Paper.

Beam, Emily A., "Do job fairs matter? Experimental evidence on the impact of job-fair attendance," *Journal of Development Economics*, 2016, *120* (C), 32–40.

_ , David McKenzie, and Dean Yang, "Unilateral Facilitation Does Not Raise International Labor Migration from the Philippines," *Economic Development and Cultural Change*, 2016, *64* (2), 323–368.

Beaman, Lori, Ariel BenYishay, Jeremy Magruder, and Ahmed Mushfiq Mobarak, "Can Network Theory-Based Targeting Increase Technology Adoption?," *American Economic Review*, June 2021, *111* (6), 1918–43.

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen, "High-Dimensional Methods and Inference on Structural and Treatment Effects," *Journal of Economic Perspectives*, 2014, *28* (2), 1–23.

Benjamin, Dwayne, "Household Composition, Labor Markets, and Labor Demand: Testing for Separation in Agricultural Household Models," *Econometrica*, 1992, *60* (2), 287–322.

BenYishay, Ariel and A. Mushfiq Mobarak, "Social Learning and Incentives for Experimentation and Communication," *The Review of Economic Studies*, 2019, *86* (3), 976–1009.

Bhatiya, Apurav Yash, Bhaskar Chakravorty, Clément Imbert, and Roland Rathelot, "How do beliefs and preferences over jobs affect enrollment in vocational training: Experimental evidence from India," *European Economic Review*, 2026, *184*, 105238.

Borjas, George J., "Self-Selection and the Earnings of Immigrants," *The American Economic Review*, 1987, *77* (4), 531–553.

Boudreau, Laura, Rachel Heath, and Tyler H. McCormick, "Migrants, Experience, and Working Conditions in Bangladeshi Garment Factories," *Journal of Economic Behavior & Organization*, 2024, *219*, 196–213.

Bryan, Gharad and Melanie Morten, "The Aggregate Productivity Effects of Internal Migration: Evidence from Indonesia," *Journal of Political Economy*, dec 2019, *127* (5), 2229–2268.

_ , Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangladesh," *Econometrica*, 2014, *82* (5), 1671–1748.

Cai, Jing, Alain De Janvry, and Elisabeth Sadoulet, "Social Networks and the Decision to Insure," *American Economic Journal: Applied Economics*, April 2015, *7* (2), 81–108.

Cai, Shu, "Migration under liquidity constraints: Evidence from randomized credit access in China," *Journal of Development Economics*, 2020, *142* (C).

Chandrasekhar, Arun G., Esther Duflo, Michael Kremer, João F. Pugliese, Jonathan Robinson, and Frank Schilbach, "Blue Spoons: Sparking Communication About Appropriate Technology Use," NBER Working Paper 30423, National Bureau of Economic Research September 2022.

Chernozhukov, Victor, Mert Demirer, Esther Duflo, and Iván Fernández-Val, "Fisher–Schultz Lecture: Generic Machine Learning Inference on Heterogeneous Treatment Effects in Randomized Experiments, With an Application to Immunization in India," *Econometrica*, 2025, *93* (4), 1121–1164.

Cilliers, Jacobus, Nour Elashmawy, and David McKenzie, "Using Post-Double Selection Lasso in Field Experiments," Policy Research Working Paper Series 10931, The World Bank September 2024.

Conley, Timothy G. and Christopher R. Udry, "Learning about a New Technology: Pineapple in Ghana," *American Economic Review*, March 2010, *100* (1), 35–69.

Dar, Manzoor H., Alain de Janvry, Kyle Emerick, Elisabeth Sadoulet, and Eleanor Wiseman, "Private Input Suppliers as Information Agents for Technology Adoption in Agriculture," *American Economic Journal: Applied Economics*, 2024, *16* (2), 219–248.

De Janvry, Alain, Kyle Emerick, Marco Gonzalez-Navarro, and Elisabeth Sadoulet, "Delinking land rights from land use: Certification and migration in Mexico," *American Economic Review*, 2015, *105* (10), 3125–3149.

Egger, Dennis, Johannes Haushofer, Edward Miguel, Paul Niehaus, and Michael Walker, "General Equilibrium Effects of Cash Transfers: Experimental Evidence From Kenya," *Econometrica*, 2022, *90* (6), 2603–2643.

Franklin, Simon, "Location, Search Costs and Youth Unemployment: Experimental Evidence from Transport Subsidies," *The Economic Journal*, 2018, *128* (614), 2353–2379.

Frohnweiler, Sarah, Bernd Beber, and Cara Ebert, "Information frictions, belief updating and internal migration: Evidence from Ghana and Uganda," *Journal of Development Economics*, 2024, *171*, 103311.

Gibson, John, David McKenzie, Halahingano Rohorua, and Steven Stillman, "The Long-term Impacts of International Migration: Evidence from a Lottery," *The World Bank Economic Review*, 04 2017, *32* (1), 127–147.

Gollin, Douglas, David Lagakos, and Michael E Waugh, "The Agricultural Productivity Gap," *Quarterly Journal of Economics*, 2014, *129* (2), 939–993.

_ , Martina Kirchberger, and David Lagakos, "Do urban wage premia reflect lower amenities? Evidence from Africa," *Journal of Urban Economics*, 2021, *121*, 103301.

Hamory, Joan, Marieke Kleemans, Nicholas Y Li, and Edward Miguel, "Reevaluating Agricultural Productivity Gaps with Longitudinal Microdata," *Journal of the European Economic Association*, 2021, *19* (3), 1522–1555.

Herrendorf, Berthold and Todd Schoellman, "Wages, Human Capital, and Barriers to Structural Transformation," *American Economic Journal: Macroeconomics*, April 2018, *10* (2), 1–23.

Imbert, Clément and John Papp, "Short-term Migration, Rural Public Works, and Urban Labor Markets: Evidence from India," *Journal of the European Economic Association*, 03 2019, *18* (2), 927–963.

_ and _ , "Costs and benefits of rural-urban migration: Evidence from India," *Journal of Development Economics*, 2020, *146*, 102473.

Jakiela, Pamela and Owen Ozier, "Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies," *Review of Economic Studies*, 2016, *83* (1), 231–268.

Joseph, Thomas, Yaw Nyarko, and Shing Yi Wang, "Asymmetric information and remittances: Evidence from matched administrative data," *American Economic Journal: Applied Economics*, 2018, *10* (2), 58–100.

Kelley, Erin M., Christopher Ksoll, and Jeremy Magruder, "How do digital platforms affect employment and job search? Evidence from India," *Journal of Development Economics*, 2024, *166*, 103176.

Khanna, Gaurav, Emir Murathanoglu, Caroline B Theoharides, and Dean Yang, "Abundance from Abroad: Migrant Income and Long-Run Economic Development," Working Paper 29862, National Bureau of Economic Research March 2022.

LaFave, Daniel and Duncan Thomas, "Farms, Families, and Markets: New Evidence on Completeness of Markets in Agricultural Settings," *Econometrica*, 2016, *84* (5), 1917–1960.

Lagakos, David, "Urban-Rural Gaps in the Developing World: Does Internal Migration Offer Opportunities?," *Journal of Economic Perspectives*, August 2020, *34* (3), 174–92.

_ , Ahmed Mushfiq Mobarak, and Michael E. Waugh, "The Welfare Effects of Encouraging Rural–Urban Migration," *Econometrica*, 2023, *91* (3), 803–837.

_ and Michael E Waugh, "Selection, Agriculture, and Cross-Country Productivity Differences," *American Economic Review*, 2013, *103* (2), 948–980.

_ , Samuel Marshall, Ahmed Mobarak, Corey Vernot, and Michael Waugh, "Migration costs and observational returns to migration in the developing world," *Journal of Monetary Economics*, 2020, *113* (C), 138–154.

Magruder, Jeremy R., "An Assessment of Experimental Evidence on Agricultural Technology Adoption in Developing Countries," *Annual Review of Resource Economics*, 2018, *10*, 299–316.

McKenzie, David, "Fears and Tears: Should More People Be Moving within and from Developing Countries, and What Stops this Movement?," *The World Bank Research Observer*, 01 2023, *39* (1), 75–96.

_ , John Gibson, and Steven Stillman, "A land of milk and honey with streets paved with gold: Do emigrants have over-optimistic expectations about incomes abroad?," *Journal of Development Economics*, 2013, *102* (C), 116–127.

Miller, Grant and A. Mushfiq Mobarak, "Learning About New Technologies Through

Social Networks: Experimental Evidence on Nontraditional Stoves in Bangladesh," *Marketing Science*, 2015, *34* (4), 480–499.

Miner, Gwyneth, "Overcoming Migration Barriers: The Impact of an Income Smoothing Program for Kenyan Migrants," *Working Paper*, 2024.

Mobarak, Ahmed Mushfiq, Iffath Sharif, and Maheshwor Shrestha, "Returns to International Migration: Evidence from a Bangladesh-Malaysia Visa Lottery," *American Economic Journal: Applied Economics*, October 2023, *15* (4), 353–88.

Morten, Melanie and Jaqueline Oliveira, "The Effects of Roads on Trade and Migration: Evidence from a Planned Capital City," *American Economic Journal: Applied Economics*, April 2024, *16* (2), 389–421.

Roy, A. D., "Some Thoughts on the Distribution of Earnings," *Oxford Economic Papers*, June 1951, *3* (2), 135–146.

Seshan, Ganesh and Robertas Zubrickas, "Asymmetric Information about Migrant Earnings and Remittance Flows," *The World Bank Economic Review*, 2017, *31* (1), 24–43.

Shrestha, Maheshwor, "Get rich or die tryin': Perceived earnings, perceived mortality rate and the value of a statistical life of potential work-migrants from Nepal," *The World Bank Economic Review*, 2020, *34* (1), 1–27.

Suri, Tavneet, "Selection and Comparative Advantage in Technology Adoption," *Econometrica*, 2011, *79* (1), 159–209.

_ **and Christopher Udry**, "Agricultural Technology in Africa," *Journal of Economic Perspectives*, 2022, *36* (1), 33–56.

Tombe, Trevor and Xiaodong Zhu, "Trade, Migration, and Productivity: A Quantitative Analysis of China," *American Economic Review*, May 2019, *109* (5), 1843–72.

Young, Alwyn, "Inequality, The Urban-Rural Gap, and Migration," *Quarterly Journal of Economics*, 2013, *128* (4), 1727–1785.

Zhang, Sally, "Hidden in Plain Sight: Asymmetric Information and Hidden Income within the Household," *SSRN Working Paper*, 2024. SSRN 4883384.

Appendix for "Reaching the Novice or Nudging the Expert? Information Delivery and the Returns to Migration"

Contents

A Additional Tables and Figures

Figure A.1: Overview of Study Design

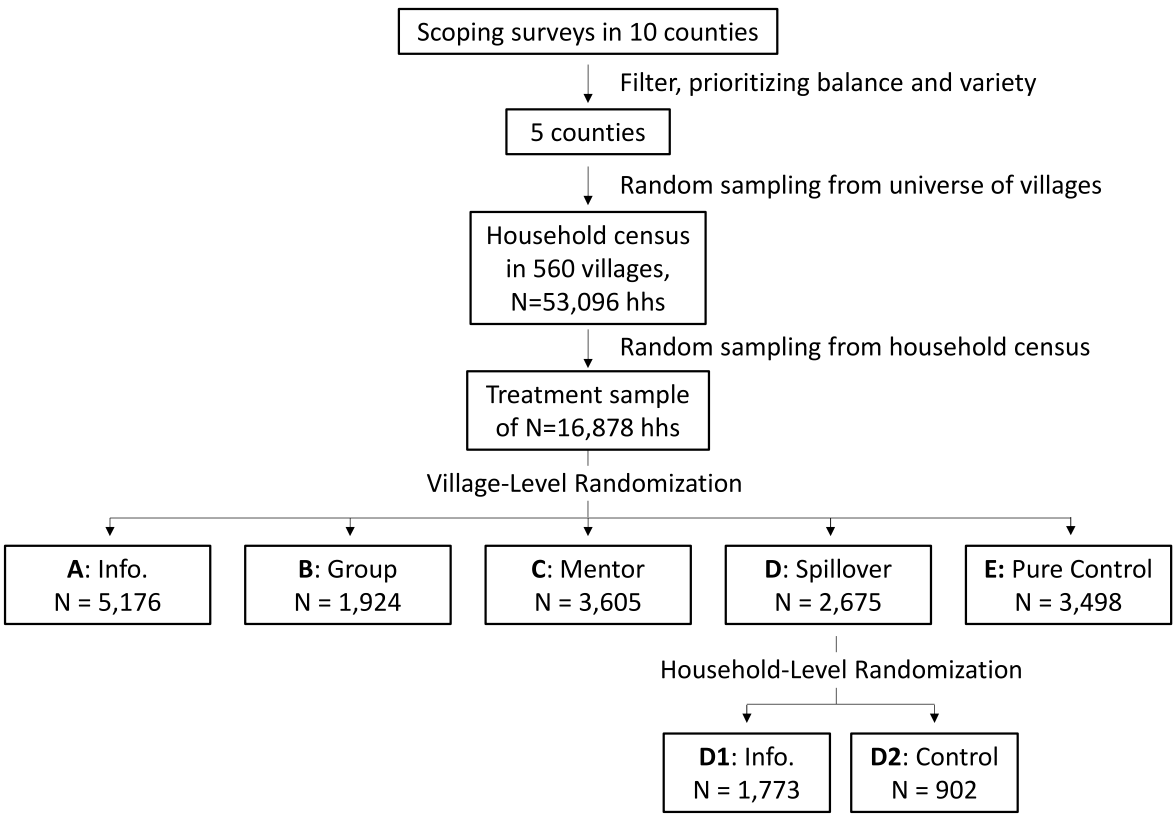
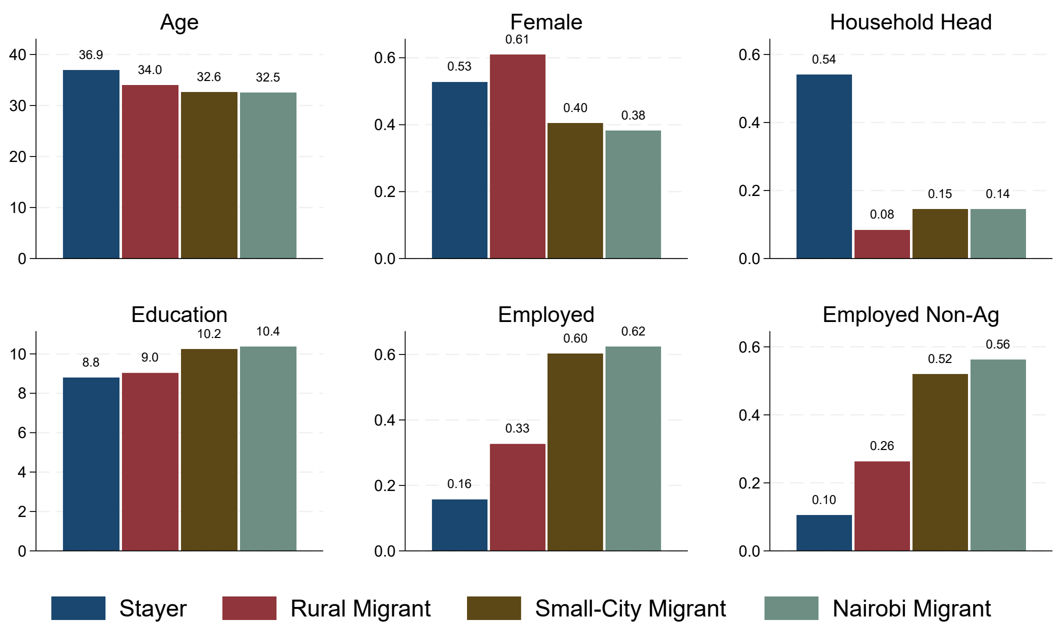
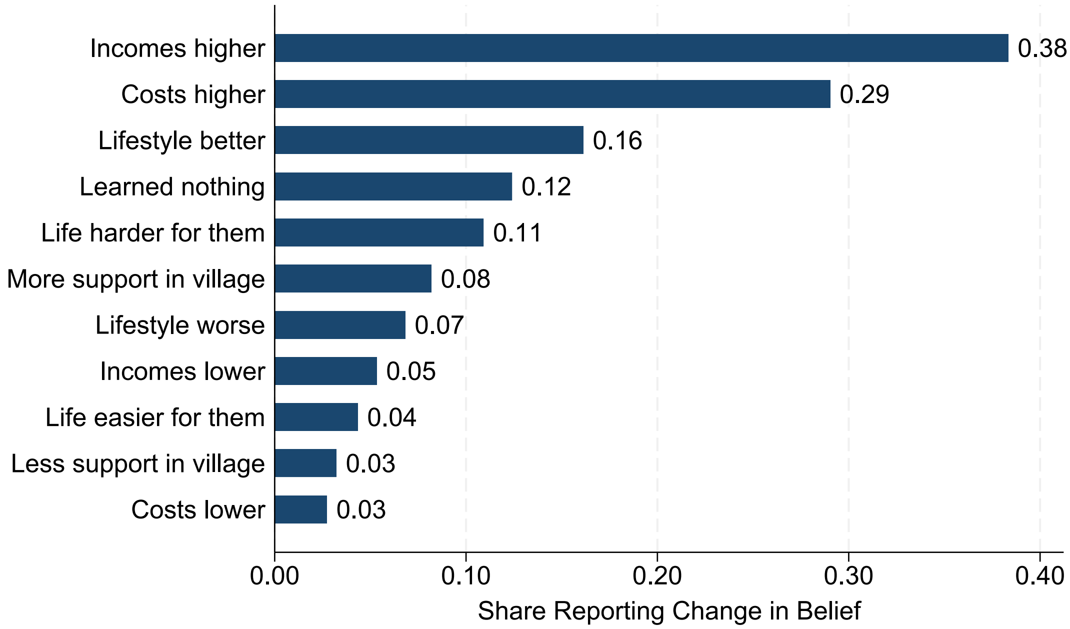


Figure A.2: Nairobi migrants tend to be young, male, educated, and not the head of their household.



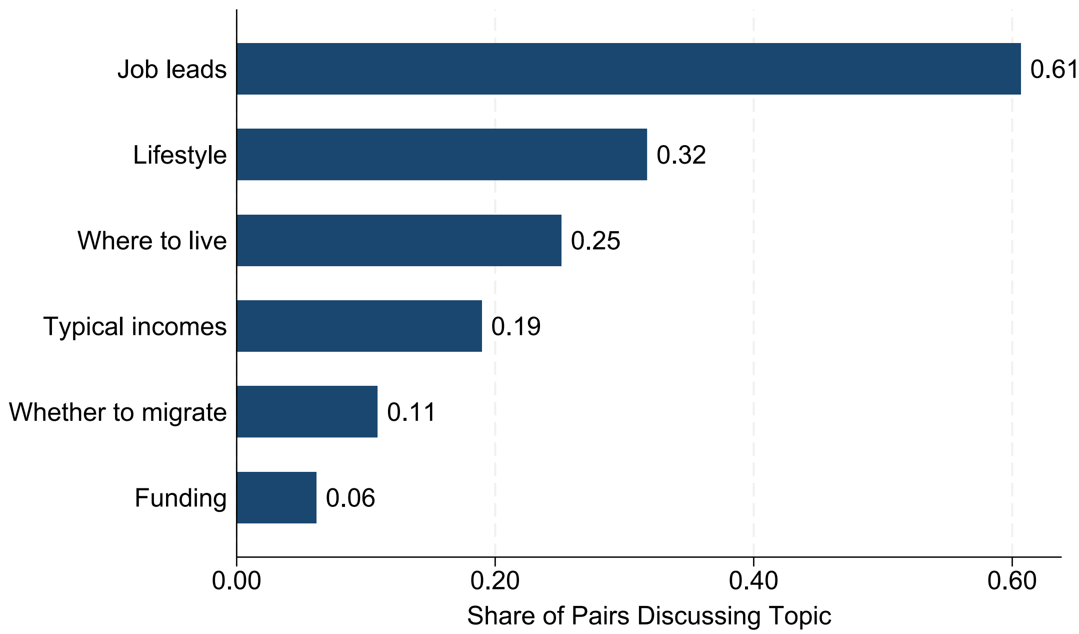
Data from $N = 60,833$ individuals aged 16 or older collected from baseline household surveys. *Stayers* are individuals residing in their origin village; *Migrants* are former household members residing outside the origin village. Rural/urban designation of the migrant's destination, and their destination city, are collected from survey data. *Small Cities* are all urban areas excluding Nairobi. Estimates are adjusted for sampling and response probabilities.

Figure A.3: Changes in Beliefs From Group Arm



Data from midline surveys of household heads. The survey question was "Did [the group] meeting change what [anyone from your household] think[s] about migrating to Nairobi? If so, what did it change?" and respondents could select multiple answers which were not read aloud.

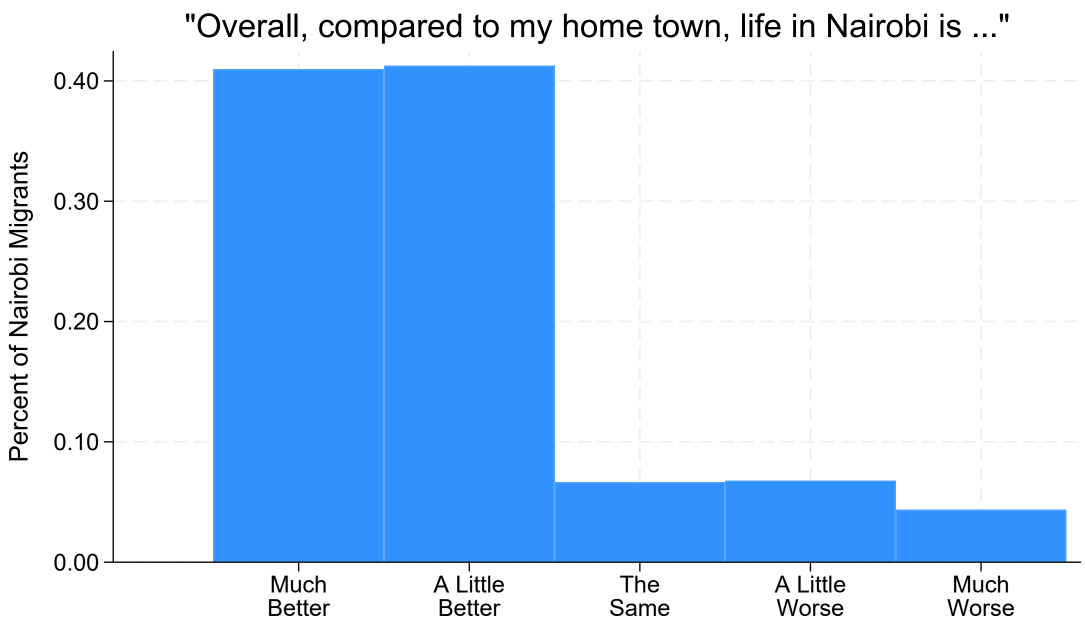
Figure A.4: Topics of Discussion in Mentor Arm



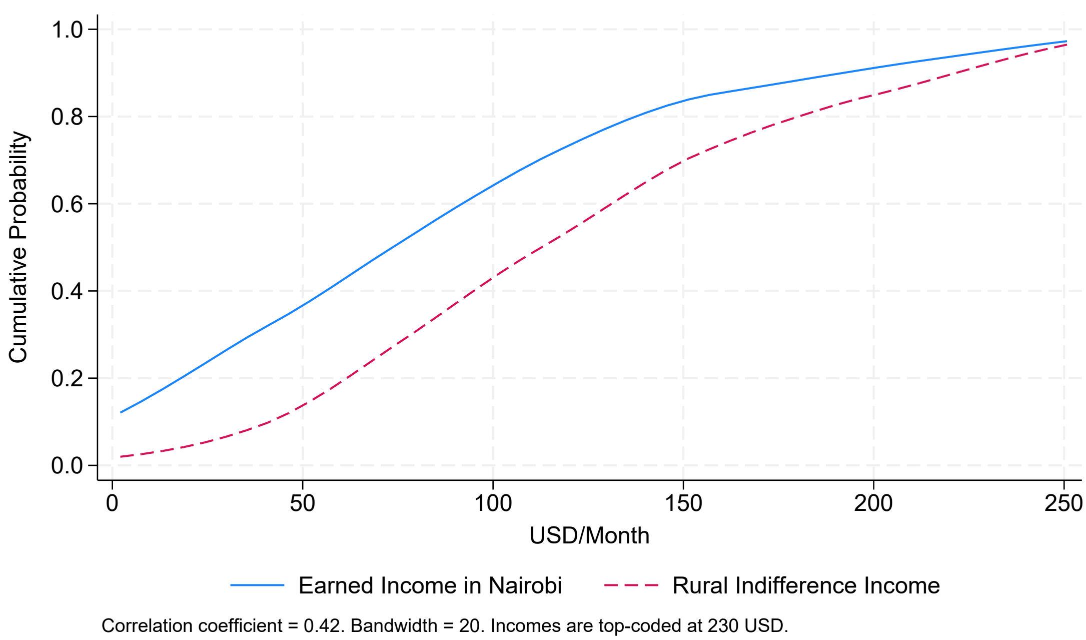
Data from midline surveys of household heads. The survey question was "What did [the mentor and the person from your household who talked to them] talk about?" and respondents could select multiple answers which were not read aloud.

Figure A.5: Migrant Subjective Well-Being

Panel A: Most migrants report preferring quality of life in the city.

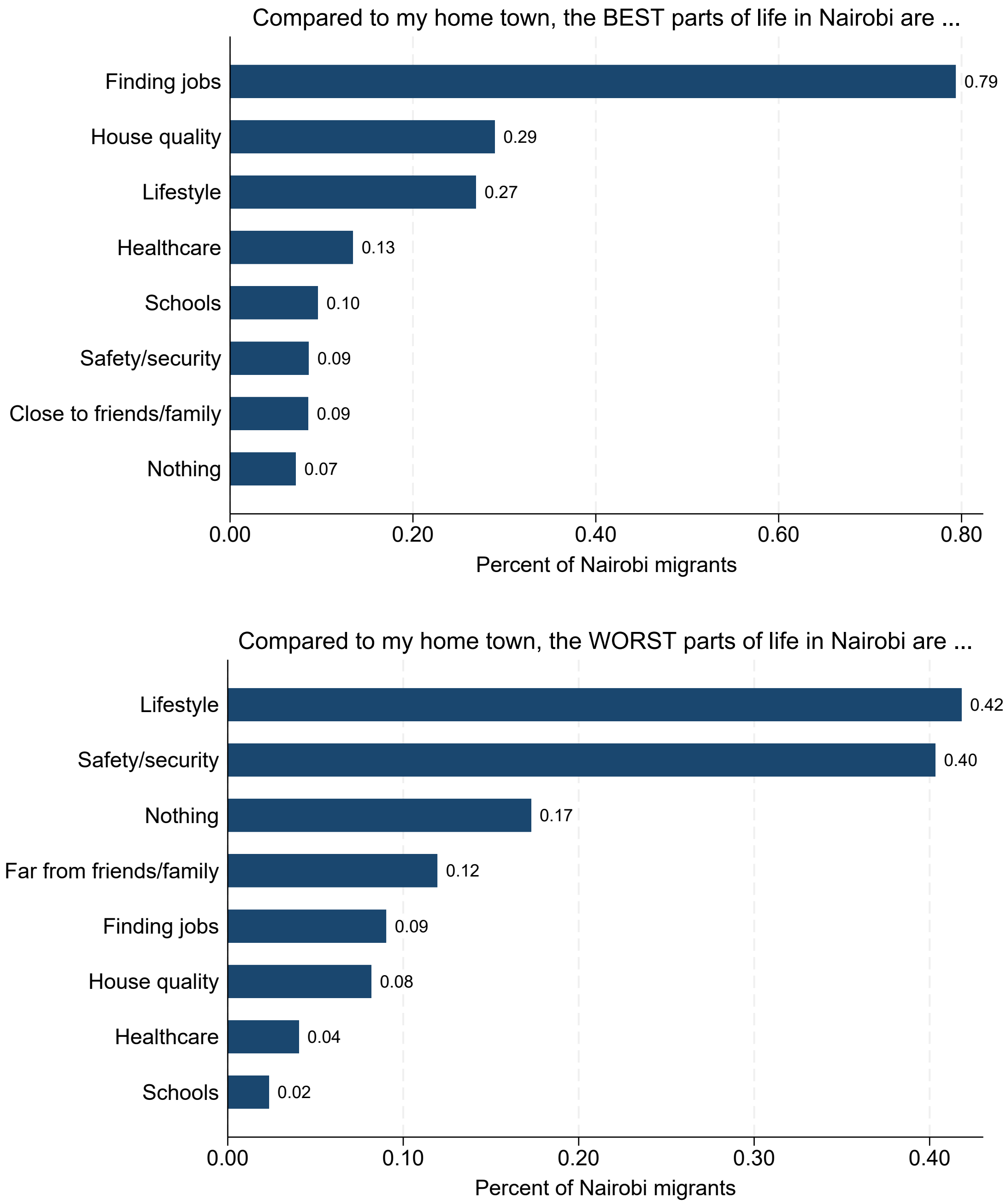


Panel B: Migrants report being willing to move home only for higher incomes.



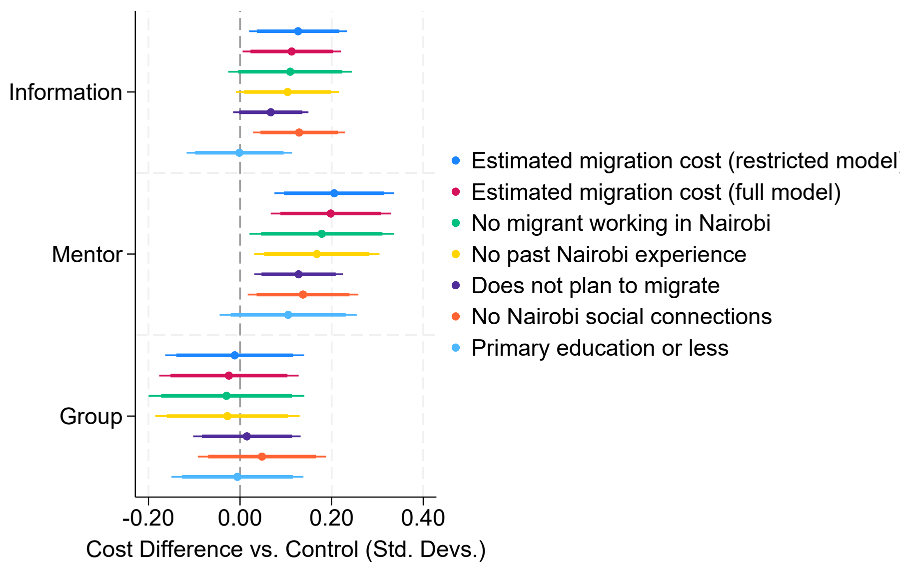
Data from endline phone surveys of migrants living in Nairobi. *Rural indifference income* is the lowest reported income the migrant would be willing to move back to their hometown for.

Figure A.6: Migrants' Reports of Best and Worst Aspects of City Life



Data from endline phone surveys of migrants living in Nairobi. The survey questions were "Compared to [your home town in 2022], what are the WORST parts about life in Nairobi for you?" and respondents could select multiple answers which were not read aloud.

Figure A.7: Information and Mentor draw in households with high estimated migration costs.



Estimated migration cost is 1 minus the household's predicted baseline migration rate from a lasso ("restricted") or OLS ("full") model using census variables as model inputs. For both cost estimates and the five lasso-selected cost predictors, this figure shows treatment impacts on migrant selection recovered from regressions of pre-treatment characteristics on treatment indicators and the control variables from equation (1) within the sample of households that sent migrants to Nairobi as of the endline survey. All characteristics are standardized to mean 0, sd 1. Lines show 90 and 95% confidence intervals.

Table A.1: Comparison of Study Counties to Country

	Sample Counties	All Counties	Percentile
% aged 18–50	36	37	46
% with primary degree	38	34	51
% with secondary degree	7	7	46
% with post-secondary degree	2	1	48
% Muslim	1	11	73
Per-capita household income (USD/month)	19	25	37
Density (pop. per sq. km.)	469	315	77
Distance to Nairobi (km)	331	329	53
% of households migrated out of county	26	19	80
% of households migrated to Nairobi	8	5	80
Population	3,972,090	26,384,420	

Column 1 shows the county-level median within the rural sample of five project counties (Kakamega, Makueni, Nandi, Siaya, Vihiga). Column 2 shows the county-level median in the full country (excluding urban areas). Column 3 shows the percentile in the county-level distribution corresponding to the project sample mean value shown in column 1. Demographic data from Kenyan 2009 household census. Income and religion data from the 2015–2016 Kenya Integrated Household Budget Survey. Density data from Kenya National Bureau of Statistics (2019). Distance data from Google Maps. Density, distance, and migration data include urban population but exclude Nairobi and Mombasa. All estimates use population weights.

Table A.2: Migrant Outcome Descriptives

	Ctrl. Mean	Info Diff.	Mentor Diff.	Group Diff.	N
Returned to village	0.35	0.00	-0.02	0.01	4,809
	(0.48)	(0.03)	(0.03)	(0.04)	
		[0.93]	[0.62]	[0.80]	
Duration (months)	6.63	0.13	-0.00	0.13	4,731
	(5.15)	(0.27)	(0.31)	(0.40)	
		[0.63]	[0.99]	[0.75]	
Migrated with others from household	0.04	0.01	-0.02	-0.02	4,809
	(0.19)	(0.01)	(0.01)	(0.01)	
		[0.43]	[0.16]	[0.17]	
Migrated with others from village	0.03	0.03***	-0.00	0.02	4,809
	(0.17)	(0.01)	(0.01)	(0.01)	
		[0.01]	[0.99]	[0.17]	
Received job referral from village	0.72	-0.01	-0.00	0.02	4,809
	(0.45)	(0.03)	(0.03)	(0.04)	
		[0.71]	[0.95]	[0.52]	
Received housing assistance from village	0.11	-0.02	-0.03	-0.03	4,809
	(0.32)	(0.02)	(0.02)	(0.02)	
		[0.44]	[0.16]	[0.21]	
Borrowed cash to migrate from village	0.04	0.00	-0.00	-0.01	4,809
	(0.19)	(0.01)	(0.01)	(0.01)	
		[0.80]	[0.83]	[0.49]	
Worked as employee	0.59	-0.01	-0.05	0.01	4,809
	(0.49)	(0.03)	(0.03)	(0.03)	
		[0.61]	[0.14]	[0.66]	
Worked as business owner	0.07	-0.02*	-0.01	-0.02	4,809
	(0.25)	(0.01)	(0.01)	(0.02)	
		[0.07]	[0.42]	[0.12]	
Monthly income, among workers	85.45	3.77	3.59	-3.68	2,817
	(74.65)	(5.32)	(5.75)	(7.15)	
		[0.48]	[0.53]	[0.61]	
Remittances, among workers	14.56	2.46	-1.06	0.19	2,817
	(19.44)	(1.64)	(1.54)	(1.79)	
		[0.13]	[0.49]	[0.91]	
Weeks taken to find job after migrating	3.54	-0.43	0.56	1.02	2,906
	(13.84)	(0.98)	(1.42)	(2.03)	
		[0.66]	[0.69]	[0.61]	
Married	0.34	0.00	0.04	0.01	3,147
	(0.47)	(0.03)	(0.03)	(0.04)	
		[0.99]	[0.29]	[0.84]	
Among married, lives with spouse	0.64	-0.06	0.01	-0.08	947
	(0.48)	(0.06)	(0.07)	(0.08)	
		[0.35]	[0.93]	[0.32]	

An observation is a migration event. Sample includes migrants ages 16 and older who left for urban destinations after the baseline survey. All data from endline surveys of rural households. First column shows the means (standard deviations) of variables within the control group. Columns 2–4 show differences in means (standard errors) between each treatment group and control, estimated from a two-sided t-test of equivalence of means. Estimates are adjusted for sampling and response probabilities. Monetary units are USD/month. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: No Migration Impacts as of 8-Month Midline

	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Disaggregated						
Information	0.006	0.008	0.009	0.018	-0.007	0.039
	(0.010)	(0.011)	(0.012)	(0.086)	(0.064)	(0.069)
	[0.51]	[0.50]	[0.44]	[0.83]	[0.92]	[0.58]
Mentor	-0.001	0.009	-0.004	-0.045	0.067	-0.007
	(0.012)	(0.013)	(0.014)	(0.104)	(0.075)	(0.079)
	[0.91]	[0.50]	[0.75]	[0.67]	[0.37]	[0.93]
Group	0.005	0.018	0.007	0.021	0.121	0.056
	(0.014)	(0.015)	(0.018)	(0.123)	(0.078)	(0.102)
	[0.74]	[0.26]	[0.71]	[0.86]	[0.12]	[0.58]
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson
p-Val: Info. = Mentor	0.41	0.95	0.23	0.45	0.25	0.47
p-Val: Info. = Group	0.88	0.49	0.87	0.98	0.06	0.85
p-Val: Mentor = Group	0.66	0.55	0.52	0.58	0.49	0.51
Control Mean	0.12	0.19	0.19	0.13	0.23	0.22
Observations	12,977	12,977	12,977	12,977	12,977	12,977
Pooled Treatment						
Any Info.	0.004	0.009	0.005	0.001	0.033	0.028
	(0.009)	(0.010)	(0.011)	(0.083)	(0.059)	(0.066)
	[0.68]	[0.37]	[0.67]	[0.99]	[0.58]	[0.67]
Observations	12,977	12,977	12,977	12,977	12,977	12,977
Treatment Intensity						
Any Info. × Prior Gap	0.001	0.015	0.011	-0.074	-0.081	0.048
	(0.020)	(0.025)	(0.024)	(0.156)	(0.117)	(0.121)
	[0.96]	[0.54]	[0.64]	[0.63]	[0.49]	[0.69]
Any Info.	0.004	0.005	0.001	0.025	0.062	0.016
	(0.012)	(0.014)	(0.014)	(0.093)	(0.070)	(0.074)
	[0.77]	[0.75]	[0.93]	[0.78]	[0.37]	[0.83]
Prior Gap	-0.041**	-0.054**	-0.057***	-0.285**	-0.169*	-0.288***
	(0.018)	(0.023)	(0.022)	(0.139)	(0.102)	(0.107)
	[0.03]	[0.02]	[0.01]	[0.04]	[0.10]	[0.01]
Observations	12,977	12,977	12,977	12,977	12,977	12,977

Impacts are estimated on data from midline surveys. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Any City* includes any urban area. Responses of “Don’t Know” are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Similar Migration Impacts Among Treated Households in Information and Spillover Villages

	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Spillover	0.01	-0.00	0.01	-0.00	-0.05	0.05
	(0.01)	(0.01)	(0.01)	(0.09)	(0.08)	(0.07)
	[0.60]	[0.97]	[0.66]	[0.99]	[0.55]	[0.52]
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson
Control Mean	0.12	0.18	0.20	0.14	0.24	0.26
Observations	6,736	6,736	6,736	6,736	6,736	6,736

Impacts are estimated on data from endline surveys. Sample includes villages assigned to Information and households assigned to receive information in Spillover villages. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Any City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Households with migration experience are better-connected at the origin and destination and have higher migration aspirations.

	Mean	Coeff. on Experience	p-Value	N
Any member has ever worked in Nairobi	0.44	1.00	.	53,096
Any member is working in Nairobi	0.29	0.66	0.00	53,096
Belief: share of village with Nairobi workers	0.16	0.09	0.00	52,000
Plans to migrate to Nairobi	0.21	0.14	0.00	53,096
Perceived typical income in Nairobi	67.16	4.64	0.00	51,396
Perceived migration earnings	137.11	9.55	0.00	50,788
# Nairobi social connections	2.55	1.93	0.00	52,975
Has Nairobi housing network	0.50	0.31	0.00	52,969
Has Nairobi jobs network	0.46	0.29	0.00	52,969
Would migrate with someone from village	0.48	0.04	0.00	53,096
Would meet someone from village in Nairobi	0.83	0.14	0.00	53,096
# households in village they give job advice to	1.39	0.51	0.00	52,870

Data from census surveys. Column 1 shows variable means. Column 2 shows coefficients estimated from a regression of each variable on a indicator for whether any household member has ever worked in Nairobi. Columns 3 and 4 show two-sided heteroskedasticity-robust p-values and sample sizes from those regressions, respectively. *Perceived typical income in Nairobi* is elicited for workers with a primary degree, with age and gender randomized across respondents and partialled out in estimation. *# Nairobi social connections* is the number of people in Nairobi the household has talked to in the past year (top-coded at 10). *Has Nairobi housing/jobs network* = 1 if the household reports that its migrant could stay with, or get help finding a job from, a Nairobi social connection, respectively. Monetary units are in USD/month.

Table A.6: Other Belief and Migration Outcomes

	Belief Outcomes			Migration Outcomes		
	Pessimistic Migration Income	Optimistic Migration Income	Expected Income Growth	# Any Migrants to Nairobi	# Recent Migrants to Nairobi	# Recent Migrants to Any City
Disaggregated						
Information	0.06**	0.03	30.03**	0.05	0.06	-0.07
	(0.02)	(0.02)	(12.09)	(0.07)	(0.07)	(0.06)
	[0.01]	[0.24]	[0.01]	[0.43]	[0.40]	[0.27]
Mentor	0.13***	0.09***	5.35	0.10	0.15*	0.08
	(0.03)	(0.02)	(12.43)	(0.07)	(0.09)	(0.07)
	[0.00]	[0.00]	[0.67]	[0.17]	[0.08]	[0.22]
Group	0.15***	0.07***	40.34***	0.13	0.19**	0.11
	(0.03)	(0.03)	(15.01)	(0.08)	(0.09)	(0.08)
	[0.00]	[0.01]	[0.01]	[0.13]	[0.03]	[0.14]
Model	Poisson	Poisson	OLS	Poisson	Poisson	Poisson
p-Val: Info. = Mentor	0.00	0.00	0.03	0.43	0.20	0.01
p-Val: Info. = Group	0.00	0.04	0.47	0.32	0.10	0.01
p-Val: Mentor = Group	0.69	0.51	0.02	0.77	0.69	0.68
Control Mean	93.90	185.54	94.37	0.23	0.13	0.28
Observations	15,342	15,344	15,268	15,468	15,468	15,468
Pooled						
Any Info.	0.092***	0.051**	24.215**	0.076	0.107	0.004
	(0.022)	(0.020)	(10.707)	(0.061)	(0.070)	(0.055)
	[0.00]	[0.01]	[0.02]	[0.21]	[0.13]	[0.94]
Observations	15,342	15,344	15,268	15,468	15,468	15,468

Belief outcomes are measured in baseline surveys after treatment; migration outcomes are measured at endline. *Expected Income Growth* is the difference in expected income after one year of living in the destination and immediately after arriving. Dependent variables in columns 4–6 are migrant counts. "Recent" migrants are those who left their home village after the baseline survey. *City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Other Financial Outcomes

	Consumption	Savings	Yearly Investment	Access to Improved Utilities	Subjective Financial Health	Subjective Well-Being
Information	0.03	0.27***	-0.02	-0.00	0.05*	-0.02
	(0.03)	(0.09)	(0.10)	(0.01)	(0.03)	(0.03)
	[0.30]	[0.00]	[0.82]	[0.53]	[0.06]	[0.62]
	{0.20}	{0.04}	{0.44}	{0.36}	{0.12}	{0.40}
Mentor	0.02	0.34***	0.04	0.00	0.08**	0.05
	(0.03)	(0.10)	(0.11)	(0.01)	(0.03)	(0.04)
	[0.64]	[0.00]	[0.76]	[0.70]	[0.01]	[0.19]
	{0.34}	{0.01}	{0.34}	{0.34}	{0.04}	{0.13}
Group	-0.00	0.12	-0.22*	-0.01	0.00	-0.04
	(0.04)	(0.12)	(0.12)	(0.01)	(0.05)	(0.04)
	[0.93]	[0.33]	[0.06]	[0.46]	[0.97]	[0.33]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Model	Poisson	Poisson	Poisson	OLS	OLS	OLS
p-Val: Info. = Mentor	0.54	0.37	0.56	0.31	0.25	0.06
p-Val: Info. = Group	0.35	0.18	0.05	0.80	0.26	0.53
p-Val: Mentor = Group	0.63	0.06	0.03	0.29	0.08	0.03
Control Mean	175.99	9.48	5.95	0.15	-0.06	-0.04
Observations	15,232	15,232	15,232	15,318	15,055	15,215

Impacts are estimated on data from endline surveys. Improved utilities, subjective financial health, and subjective well-being are standardized indices. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group across outcomes in this table and Table 2. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Within- and Across-Village Spillover Impacts

	Perceived Nairobi Income	Perceived Own Nairobi Income	Recent Migrants to Nairobi	Recent Migrants to Any City	Income	Welfare Index
Within-Village Spillovers						
Untreated HH in Spillover	0.00	0.03	0.00	-0.01	5.95	0.13**
	(0.04)	(0.06)	(0.01)	(0.02)	(5.35)	(0.06)
	[0.93]	[0.66]	[0.72]	[0.70]	[0.27]	[0.02]
Model	Poisson	Poisson	OLS	OLS	OLS	OLS
Control Mean	127.10	195.46	0.11	0.21	97.48	-0.04
Observations	2,910	4,124	4,247	4,247	4,247	4,247
Across-Village Spillovers						
Share Treated $\leq$ 3km	0.02	-0.03	0.01	0.00	1.61	-0.02
	(0.05)	(0.10)	(0.02)	(0.02)	(5.22)	(0.04)
	[0.67]	[0.74]	[0.58]	[0.94]	[0.76]	[0.69]
Model	Poisson	Poisson	OLS	OLS	OLS	OLS
Outcome Mean	127.10	195.46	0.11	0.21	97.48	-0.04
Observations	2,296	3,274	3,376	3,376	3,376	3,376
Share Treated $\leq$ 10km	-0.10	-0.08	-0.09	-0.09	-3.11	-0.12
	(0.17)	(0.21)	(0.05)	(0.08)	(16.92)	(0.15)
	[0.54]	[0.70]	[0.11]	[0.28]	[0.85]	[0.42]
Model	Poisson	Poisson	OLS	OLS	OLS	OLS
Outcome Mean	127.10	195.46	0.11	0.21	97.48	-0.04
Observations	2,296	3,274	3,376	3,376	3,376	3,376

Impacts are estimated on data from endline surveys. *Within-Village Spillovers* estimated on Pure Control villages and households assigned to NOT receive information in Spillover villages. *Across-Village Spillovers* estimated on Pure Control villages only. *Share Treated* is the share of other villages within 3 or 10km which were assigned to any arm other than Pure Control. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Perceived Nairobi Income* is the perceived typical earnings for workers of the same demographic group as the most likely migrant from their household: this outcome is missing for households with no most likely migrant at baseline. *Perceived Own Nairobi Income* is the household's expected income if it sends a migrant to Nairobi. *Any City* includes any urban area. Monetary units are USD/month. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Direct and Spillover Impacts on Village Economies

	Village Business Profits	Village Labor Income	Village Business Profits > 0	Village Labor Supply (Businesses)	Village Labor Supply (Wage Jobs)	Total Commute (Minutes)
Within-Village Spillovers						
Untreated HH in Spillover	0.76	0.17***	0.01	0.03	0.01	0.10
	(0.59)	(0.05)	(0.01)	(0.21)	(0.05)	(0.06)
	[0.20]	[0.00]	[0.30]	[0.88]	[0.74]	[0.11]
Model	OLS	Poisson	OLS	Poisson	Poisson	Poisson
Control Mean	2.46	66.67	0.09	9.06	59.16	35.52
Observations	4,247	4,247	4,247	4,247	4,247	4,247
Direct Treatment Impacts						
Information	1.14***	0.10**	0.01*	0.56***	0.02	0.04
	(0.37)	(0.04)	(0.01)	(0.21)	(0.03)	(0.04)
	[0.00]	[0.01]	[0.07]	[0.01]	[0.59]	[0.30]
Mentor	0.17	0.10**	0.01	0.22	0.06*	0.05
	(0.36)	(0.05)	(0.01)	(0.22)	(0.04)	(0.04)
	[0.63]	[0.02]	[0.55]	[0.32]	[0.09]	[0.24]
Group	-0.65	0.02	-0.01	0.01	0.05	0.13**
	(0.42)	(0.06)	(0.01)	(0.23)	(0.04)	(0.05)
	[0.12]	[0.71]	[0.26]	[0.95]	[0.27]	[0.01]
Model	OLS	Poisson	OLS	Poisson	Poisson	Poisson
Control Mean	2.46	66.67	0.09	9.06	59.16	35.52
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Impacts are estimated on data from endline surveys. *Within-Village Spillovers* estimated on Pure Control villages and households assigned to NOT receive information in Spillover villages. *Direct Treatment Impacts* estimated on the full experimental sample. Labor supply (in hours per week) and daily commute time (in minutes) variables are totaled across household members residing in the village at the time of the survey. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. Monetary units are USD/month. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Spillover Impacts on Components of Welfare Index

Index and Income:	Welfare Index	Income	Yearly Income	Income + Crop Profit	Real Income	Amenity-Adusted Income
Untreated HH in Spillover	0.13**	5.95	28.71	7.59	7.01	5.41
	(0.06)	(5.35)	(45.75)	(5.82)	(5.60)	(6.31)
	[0.02]	[0.27]	[0.53]	[0.19]	[0.21]	[0.39]
Model	OLS	OLS	OLS	OLS	OLS	OLS
Control Mean	-0.04	97.48	680.96	118.61	97.54	120.69
Observations	4,247	4,247	4,247	4,247	4,247	4,247
Other Financial:	**Consumption**	**Savings**	**Yearly Investment**	**Access to Improved Utilities**	**Subjective Financial Health**	**Subjective Well-Being**
Untreated HH in Spillover	0.07*	-0.03	0.41**	0.02*	0.05	0.00
	(0.04)	(0.11)	(0.18)	(0.01)	(0.05)	(0.05)
	[0.08]	[0.82]	[0.02]	[0.08]	[0.23]	[0.95]
Model	Poisson	Poisson	Poisson	OLS	OLS	OLS
Control Mean	175.99	9.48	5.95	0.15	-0.06	-0.04
Observations	4,186	4,186	4,186	4,206	4,129	4,183

Impacts are estimated on endline survey data from Pure Control villages and households assigned to NOT receive information in Spillover villages. Welfare, improved utilities, subjective financial health, and subjective well-being are standardized indices. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Information spillovers are lower for more financially connected households.

	Perceived Nairobi Income				Perceived Own Nairobi Income			
Spillover Village × Borrowing Connections	-0.09*			-0.11**	-0.11**			-0.14
	(0.05)			(0.05)	(0.05)			(0.08)
	[0.06]			[0.04]	[0.04]			[0.10]
Spillover Village × Migration Advice Connections		0.03		0.07		0.06		0.10
		(0.05)		(0.05)		(0.12)		(0.13)
		[0.48]		[0.18]		[0.63]		[0.46]
Spillover Village × Farm Help Connections			-0.02	-0.00			-0.02	0.01
			(0.04)	(0.04)			(0.05)	(0.06)
			[0.56]	[0.92]			[0.60]	[0.85]
Spillover Village	0.05	-0.02	0.01	0.02	0.09	0.00	0.05	0.04
	(0.05)	(0.05)	(0.05)	(0.06)	(0.08)	(0.07)	(0.07)	(0.07)
	[0.29]	[0.74]	[0.75]	[0.76]	[0.25]	[0.97]	[0.49]	[0.52]
Model	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
Control Mean	127	127	127	128	195	195	195	196
Observations	2,845	2,863	2,838	2,827	4,014	4,045	4,003	3,986

Belief impacts are estimated on data from endline surveys; network variables are measured at baseline. Sample includes villages assigned to Control and households assigned to NOT receive information in Spillover villages. Poisson regression is used for all outcomes. *Borrowing Connections* is the number of households in the village the respondent says they would ask to lend them 1,000 KSh. *Migration Advice Connections* is the number of households in the village they would ask for advice on finding a job after migrating. *Farm Help Connections* is the number of households in the village they would ask for help with plowing or harvesting their farm. All regressions control for the *Connection* variable(s) being interacted with *Spillover Village* (coefficients not shown). *Perceived Nairobi Income* is the perceived typical earnings for workers of the same demographic group as the most likely migrant from their household. *Perceived Own Nairobi Income* is the household's expected income if it sends a migrant to Nairobi. Outcome units are USD/month. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Impacts on Nairobi income are greater for high-cost migrants.

	Nairobi Income, Past Month		Nairobi Income, Past Year	
Disaggregated				
Information × Cost	2.33*	2.27*	22.76**	21.70**
	(1.32)	(1.32)	(10.68)	(10.74)
	[0.08]	[0.09]	[0.03]	[0.04]
Mentor × Cost	2.44*	2.37*	18.94*	17.67
	(1.39)	(1.38)	(11.30)	(11.37)
	[0.08]	[0.09]	[0.09]	[0.12]
Group × Cost	1.68	1.60	14.88	13.68
	(2.03)	(2.02)	(13.51)	(13.67)
	[0.41]	[0.43]	[0.27]	[0.32]
Information	0.02	0.03	-5.37	-5.19
	(1.29)	(1.29)	(10.26)	(10.23)
	[0.99]	[0.98]	[0.60]	[0.61]
Mentor	-0.08	-0.05	-6.17	-5.84
	(1.47)	(1.47)	(11.86)	(11.85)
	[0.96]	[0.98]	[0.60]	[0.62]
Group	0.10	0.15	-7.81	-7.34
	(2.07)	(2.07)	(13.23)	(13.25)
	[0.96]	[0.94]	[0.56]	[0.58]
Cost Model	Restricted	Full	Restricted	Full
Observations	15,468	15,468	15,468	15,468
Pooled Treatment				
Any Info. × Cost	2.27*	2.21*	20.51**	19.36*
	(1.20)	(1.20)	(9.98)	(10.03)
	[0.06]	[0.07]	[0.04]	[0.05]
Any Info	-0.00	0.02	-6.07	-5.82
	(1.20)	(1.19)	(9.68)	(9.66)
	[1.00]	[0.99]	[0.53]	[0.55]
Cost Model	Restricted	Full	Restricted	Full
Control Mean	11	11	83	83
Observations	15,468	15,468	15,468	15,468

Impacts are estimated on data from endline surveys using OLS. Each column shows treatment impacts on total household income earned in Nairobi in the past month or year in USD interacted with each household's estimated migration cost (see Section 5 for details on cost estimation). Restricted cost estimates are obtained through lasso regression. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Engagement During Information Treatments

Outcome: Standardized Engagement During Treatment	Group: Active in Discussion	1-on-1 (Info.+Mentor): Treatment Duration
Validating the Engagement Measures		
Plans to use mentor program (post-treatment)	-	0.38***
	-	(0.04)
	-	[0.00]
Plans to migrate to Nairobi (post-treatment)	0.12**	0.12**
	(0.06)	(0.06)
	[0.03]	[0.03]
Testing for Engagement Differences by Migration Cost Measures		
Estimated migration cost (restricted model)	-0.08***	-0.01
	(0.03)	(0.01)
	[0.00]	[0.61]
Estimated migration cost (full model)	-0.08***	-0.01
	(0.03)	(0.01)
	[0.00]	[0.59]
No migrant working in Nairobi	-0.11*	0.04**
	(0.06)	(0.02)
	[0.05]	[0.05]
No past Nairobi experience	-0.12**	-0.00
	(0.05)	(0.02)
	[0.02]	[1.00]
Does not plan to migrate	-0.08	-0.05**
	(0.05)	(0.02)
	[0.14]	[0.01]
No Nairobi social connections	-0.14***	-0.03
	(0.05)	(0.02)
	[0.00]	[0.21]
Primary education or less	-0.11**	-0.01
	(0.05)	(0.02)
	[0.02]	[0.78]
Mean (Engagement)	0.09	8.37
SD (Engagement)	0.28	5.06
Observations	1,924	10,540

Each cell triplet shows estimates from a regression of intervention engagement on a single binary predictor variable. Column 1 restricts the sample to the Group arm; Column 2 restricts to the Mentor and Information arms. Engagement is measured as an indicator for whether the household was listed as one of the three most engaged participants in the group meeting by implementing staff in Group, and as the minutes spent on the 1-on-1 information meeting in Information and Mentor. All models estimated through OLS with standardized outcome variables. *Plans to use mentor program* is measured after treatment and the estimation sample includes the Mentor arm only. Estimated migration costs are standardized to mean 0, sd 1. All regressions control for an enumerator fixed effect and the survey date interacted with survey month. Robust standard errors in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Heterogeneous Group Impacts by Meeting Dynamics

	Outcome: Recent Migrants to Nairobi		
Leader and Household Interactions:	Has Migrant in Nairobi	Has Worker in Nairobi	Ever Worked in Nairobi
Leader of Type X × X × Group	0.13***	0.05*	0.06*
	(0.03)	(0.03)	(0.03)
	[0.00]	[0.07]	[0.05]
Leader of Type X × Group	-0.02	0.01	-0.00
	(0.02)	(0.02)	(0.02)
	[0.32]	[0.42]	[0.98]
X × Group	0.07***	0.06***	0.03***
	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.00]
Group	0.00	0.01	0.01
	(0.01)	(0.01)	(0.01)
	[0.89]	[0.64]	[0.66]
Group Mean (X)	0.36	0.29	0.43
Group Mean (Leader of Type X)	0.71	0.62	0.72
Observations	15,468	15,468	15,468

Impacts are estimated on data from endline surveys. The dependent variable for each column is an indicator for whether the household sent migrants to Nairobi after treatment. Each column title lists a binary household characteristic X analyzed in that regression. "Leader of Type X" is a village-level variable indicating whether at least one of the three most active meeting participants (other than the respondent) has type X indicated in that column. All regressions estimated using (1) (coefficients for treatments other than Group not shown). Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Treatment Impacts on Joint Migration

	Co-Migrated to Nairobi		Co-Migrated to Nairobi With More Experienced Migrant	
Info.	0.006***	0.005**	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.01]	[0.01]	[0.00]	[0.00]
Mentor	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
	[0.51]	[0.60]	[0.48]	[0.48]
Group	0.004	0.005	0.002	0.003
	(0.003)	(0.003)	(0.002)	(0.003)
	[0.16]	[0.13]	[0.31]	[0.29]
Info. × Cost		0.004*		0.002
		(0.002)		(0.002)
		[0.09]		[0.19]
Mentor × Cost		0.002		-0.000
		(0.002)		(0.002)
		[0.29]		[0.88]
Group × Cost		-0.004		-0.002
		(0.004)		(0.003)
		[0.21]		[0.52]
Control Mean	0.005	0.005	0.003	0.003
Observations	15,468	15,468	15,468	15,468

Each column is a regression. *Co-Migrated to Nairobi* = 1 if the household sent a migrant to Nairobi after the intervention who traveled with another migrant from their village. *Co-Migrated to Nairobi With More Experienced Migrant* = 1 if the household reported that the co-migrant was more experienced with migrating than the household's migrant. Columns 2 and 4 interact treatment assignment with a standardized household-level migration cost (see Section 5.1; coefficient on *Cost* is controlled but not shown). Linear regression is used for all outcomes. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Predictors of Return Migration

	Intended Duration (Years)		Intended Duration ≥ 1	
	Nairobi	Other Towns	Nairobi	Other Towns
Income (standardized)	1.48***	0.18	0.07***	-0.03
	(0.37)	(0.97)	(0.01)	(0.03)
	[0.00]	[0.85]	[0.00]	[0.28]
Utility quality index (standardized)	0.77	3.15	0.00	0.03
	(0.84)	(2.01)	(0.04)	(0.05)
	[0.36]	[0.12]	[0.93]	[0.59]
Education (years)	0.29*	0.62	0.01	-0.01
	(0.16)	(0.41)	(0.01)	(0.01)
	[0.08]	[0.13]	[0.13]	[0.43]
Female	-0.45	0.43	0.00	-0.02
	(0.81)	(2.19)	(0.03)	(0.06)
	[0.57]	[0.85]	[0.93]	[0.80]
Age (years)	-0.04	-0.01	-0.00	0.00
	(0.04)	(0.12)	(0.00)	(0.00)
	[0.32]	[0.94]	[0.92]	[0.31]
Outcome Mean	12.41	12.48	0.87	0.92
Observations	1,401	201	1,401	201

Data from phone surveys with migrants at endline. The outcome variable is the number of years the migrant reports intending to stay in the destination (columns 1 and 2) or an indicator for whether they plan to stay at least one year (columns 3 and 4). Intended duration is top-coded at 25 years. Columns 1 and 3 restrict to migrants living in Nairobi at the time of the survey; columns 2 and 4 restrict to cities and towns other than Nairobi. *Utility quality index* is the standardized sum of five binary variables indicating safety from crime, an improved toilet, piped water, cooking fuel, and an electric connection at their home. Linear regression is used for all models. Robust standard errors in parentheses; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Experimental Design

B.1 Randomization Balance and Attrition

Table B.1 shows tests of randomization balance. Across 31 pre-treatment household and village characteristics, we reject the null hypothesis of equality across treatment arms at the 10% level for three, consistent with expectation under successful randomization.

Table B.2 shows survey completion rates and tests of differential attrition by wave. Midline surveys, conducted over the phone, were successfully completed with 81% of the experimental sample. Direct endline surveys were successfully conducted with 95% of the sample. Including data collected indirectly from neighbors brings our endline completion rate to 97%. Phone surveys with individuals—largely migrants or return migrants—from surveyed households (see Section 2.3) were successfully conducted with 75% of sampled individuals at midline at 86% of sampled individuals at endline. Differences in attrition rates are small and statistically insignificant in every survey round and for every treatment group, and no test of joint orthogonality within survey round is rejected at the 10% level.

Table B.3 presents tests of whether randomization balance was retained within the set of households surveyed at midline, endline (direct surveys only), or endline (including data collected indirectly from neighbors), both with and without sampling weights. For the midline survey, we reject the null of joint equality at the 10% level for 4 out of 62 tests, consistent with no significantly differential attrition. For the endline and endline + indirect sample, the analogous statistics are 5 and 6 out of 62 tests rejected at the 10% level, respectively, again consistent with no significant attrition bias.

Table B.4 tests whether random assignment to treatment within Spillover villages achieved balance, both in the full sample and among those successfully surveyed by wave. Across 48 tests (24 household-level variables and two weighting options) we reject the null hypothesis of equality across households receiving and not receiving information within spillover villages at the 10% level for 5 in the full sample, 9 in the midline sample, 5 in the endline sample, and 4 in the endline + indirect sample. These patterns are consistent with expectation under successful randomization and a lack of significantly differential attrition in the endline and endline + indirect samples. While they potentially point to differential attrition in the midline sample, we do not use this sample to estimate treatment or spillover impacts.

B.2 Sampling for Individual Phone Surveys

Migrants' contact information was collected from the rural household during household surveys. During census surveys, we sampled up to one member per household currently working in Nairobi, asking the household to choose the highest earner in the event that multiple members were working in Nairobi at that time. We surveyed these individuals by phone shortly before interventions began in their home village. During the midline and endline surveys, we sampled all non-student migrants (both current and returned) aged 18–69 and a random sample of rural individuals aged 18–69 planning on migrating to Nairobi within one year.

Table B.1: Randomization Balance

	Mean: Control	Mean: Info	Mean: Mentor	Mean: Group	Mean: Spillover	Joint p-Value
Household size	4.91	4.86	4.84	4.83	4.85	0.86
# children under 5	0.73	0.75	0.71	0.75	0.69	0.27
# adults 18-35	1.53	1.54	1.58	1.44	1.48	0.29
Highest years of education	10.36	10.42	10.55	10.27	10.45	0.56
Any member has primary degree	0.83	0.83	0.83	0.81	0.84	0.89
Any member has secondary degree	0.53	0.53	0.54	0.51	0.55	0.93
Any member has post-secondary degree	0.16	0.17	0.17	0.15	0.17	0.52
Belongs to minority tribe	0.05	0.05	0.04	0.05	0.04	0.90
Number of income sources	1.29	1.30	1.27	0.91	1.32	0.73
Number of non-agricultural income sources	0.62	0.60	0.57	0.41	0.60	0.62
Income	58.28	53.70	50.78	50.86	53.43	0.06
Expenditure	17.70	18.19	18.48	17.02	18.72	0.79
Could cover emergency of 2,000 KSh	0.37	0.36	0.39	0.40	0.39	0.45
Would seek loan from village	0.24	0.26	0.27	0.22	0.25	0.76
Has member living in Nairobi	0.36	0.36	0.35	0.33	0.35	0.62
Has migrant working in Nairobi	0.29	0.31	0.28	0.27	0.29	0.27
Has migrant working in a city	0.43	0.45	0.41	0.42	0.44	0.35
Plans to migrate to Nairobi	0.19	0.19	0.19	0.14	0.18	0.17
Plans to migrate to any city	0.23	0.22	0.21	0.17	0.20	0.09
Perceived migration earnings	145.45	146.89	142.08	133.39	141.22	0.13
# of social contacts in Nairobi	3.69	3.51	3.28	2.70	3.03	0.03
# of origin connections (farm assistance)	1.12	1.19	1.15	0.77	1.12	0.94
# of origin connections (job advice)	1.39	1.52	1.62	1.43	1.44	0.15
Participates in village association	0.71	0.70	0.68	0.62	0.72	0.46
Village sociality index	0.82	0.82	0.83	0.80	0.84	0.77
Village trust index	0.71	0.71	0.72	0.69	0.71	0.69
Village financial reliance index	0.63	0.62	0.63	0.61	0.62	0.38
Village distance to Nairobi (km.)	249.67	243.95	247.30	289.84	242.45	0.44
Village distance to county capital (km.)	44.14	43.52	44.18	48.49	46.30	0.99
Village households	100.15	104.20	103.42	101.16	101.15	0.44
Village population	428.01	442.14	451.00	442.90	440.54	0.57

Survey data from household census. Distance data from Google Maps. Sub-location density data from KNBS. First five columns show pre-treatment variable means within treatment groups. Column 6 shows p-values from joint F-tests that means are equal in all treatment groups, recovered from a regression of each variable on treatment dummies and a randomization-stratum fixed effect, clustering standard errors at the village level and adjusting for sampling methodology using survey weights. Linear regression is used for outcomes bounded between 0 and 1; Poisson regression is used otherwise. Monetary units are 2022 USD/month. *Minority tribe* is defined at the county level. *Sociality*, *trust*, and *financial reliance* indices are the share of the village reporting that people in the village frequently socialize, trust each other, and frequently borrow money from each other, respectively. *Tribal diversity index* is $1 - HH_v$ where HH_v is a Herfindahl-Hirschman index of tribal concentration in village v. Estimates are adjusted for sampling and response probabilities.

Table B.2: Attrition

Surveyed in Round:	Midline	Endline	Endline w. Attrits	Midline Migrants	Endline Migrants
Information	-0.01	-0.01	0.00	-0.03	-0.02
	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)
	[0.33]	[0.32]	[0.98]	[0.22]	[0.48]
Mentor	0.01	-0.00	-0.00	0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)
	[0.55]	[0.60]	[0.61]	[0.69]	[0.43]
Group	0.00	-0.01	-0.00	-0.05	-0.04
	(0.02)	(0.01)	(0.01)	(0.04)	(0.03)
	[0.78]	[0.11]	[0.86]	[0.21]	[0.13]
Spillover	-0.01	0.00	0.01	-0.04	-0.01
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
	[0.53]	[0.47]	[0.23]	[0.22]	[0.84]
Joint Orthogonality p-Value	0.45	0.21	0.55	0.30	0.64
Mean	0.81	0.95	0.97	0.74	0.85
Observations	16,878	16,878	16,878	4,267	3,554

The outcome variables in columns 1–3 are indicators for whether the household was surveyed at midline, endline, or endline including survey data collected indirectly from another household in the village, respectively. In columns 4 and 5, the outcome is an indicator for whether a sampled individual from a surveyed household was surveyed over the phone in the midline or endline survey round, respectively. Each column shows coefficients from a regression of the outcome on treatment assignment and a randomization-stratum fixed effect. Joint orthogonality p-values computed from an F test that all treatment coefficients are equal to zero. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Randomization balance is maintained within surveyed households, with and without sampling weights.

	Joint p-Value in Sample Surveyed at:					
	Midline		Endline		Endline w. Attrits	
Household size	0.97	0.48	0.96	0.96	0.97	0.90
# children under 5	0.18	0.24	0.45	0.22	0.48	0.27
# adults 18-35	0.55	0.18	0.82	0.36	0.81	0.33
Highest years of education	0.79	0.95	0.46	0.59	0.45	0.53
Any member has primary degree	0.96	0.86	0.70	0.70	0.83	0.77
Any member has secondary degree	0.86	0.92	0.94	0.90	0.95	0.89
Any member has post-secondary degree	0.21	0.53	0.23	0.57	0.22	0.57
Belongs to minority tribe	0.87	0.85	0.92	0.90	0.94	0.90
Number of income sources	0.69	0.84	0.41	0.69	0.42	0.73
Number of non-agricultural income sources	0.51	0.61	0.15	0.56	0.16	0.55
Income	0.15	0.06	0.35	0.06	0.24	0.06
Expenditure	0.98	1.00	0.88	0.77	0.81	0.68
Could cover emergency of 2,000 KSh	0.39	0.36	0.29	0.22	0.35	0.29
Would seek loan from village	0.31	0.62	0.54	0.89	0.62	0.85
Has member living in Nairobi	0.45	0.41	0.64	0.66	0.58	0.62
Has migrant working in Nairobi	0.30	0.28	0.32	0.30	0.28	0.25
Has migrant working in a city	0.49	0.48	0.37	0.37	0.37	0.32
Plans to migrate to Nairobi	0.40	0.28	0.38	0.19	0.33	0.13
Plans to migrate to any city	0.17	0.18	0.12	0.09	0.10	0.08
Perceived migration earnings	0.61	0.20	0.40	0.11	0.50	0.14
# of social contacts in Nairobi	0.05	0.01	0.06	0.07	0.06	0.08
# of origin connections (farm assistance)	0.72	0.98	0.98	0.99	0.96	0.99
# of origin connections (job advice)	0.05	0.39	0.02	0.14	0.03	0.15
Participates in village association	0.20	0.21	0.26	0.48	0.21	0.44
Village sociality index	0.83	0.76	0.88	0.74	0.88	0.73
Village trust index	0.94	0.62	0.93	0.67	0.93	0.66
Village financial reliance index	0.36	0.36	0.33	0.35	0.34	0.37
Village distance to Nairobi (km.)	0.43	0.45	0.38	0.44	0.42	0.46
Village distance to county capital (km.)	1.00	1.00	1.00	1.00	1.00	1.00
Village households	0.32	0.44	0.34	0.46	0.35	0.46
Village population	0.59	0.63	0.50	0.57	0.53	0.58
Weighted		X		X		X
Observations	13,715	13,715	16,089	16,089	16,339	16,339

See Table B.1 for data and variable notes. Each column shows p-values from joint F-tests that means are equal in all treatment groups, recovered from a regression of each variable on treatment dummies and a randomization-stratum fixed effect, clustering standard errors at the village level. Linear regression is used for outcomes bounded between 0 and 1; Poisson regression is used otherwise. The first pair of columns restricts the same to households successfully surveyed at midline; the second pair restricts to those surveyed directly at endline; the third pair restricts to those surveyed directly or indirectly (through neighbor surveys) at endline. Even columns adjust for sampling and response probabilities.

Table B.4: Randomization balance of treatment assignment within spillover villages.

	Joint p-Value in Sample Surveyed at:							
	Baseline		Midline		Endline		Endline w. Attrits	
Household size	0.10	0.14	0.09	0.35	0.20	0.25	0.19	0.24
# children under 5	0.60	0.20	0.51	0.33	0.80	0.27	0.76	0.28
# adults 18-35	0.90	0.87	0.83	0.94	0.99	0.94	0.95	0.95
Highest years of education	0.51	0.66	0.13	0.02	0.21	0.29	0.34	0.42
Any member has primary degree	0.26	0.81	0.12	0.09	0.09	0.33	0.11	0.44
Any member has secondary degree	0.75	0.94	0.53	0.63	0.58	0.67	0.68	0.79
Any member has post-secondary degree	0.07	0.25	0.04	0.12	0.06	0.20	0.07	0.22
Belongs to minority tribe	0.93	0.71	0.92	0.76	0.55	0.64	0.72	0.58
Number of income sources	0.61	0.62	0.21	0.26	0.41	0.58	0.38	0.59
Number of non-agricultural income sources	0.21	0.62	0.65	0.98	0.27	0.55	0.29	0.53
Income	0.69	0.25	0.42	0.14	0.67	0.24	0.66	0.30
Expenditure	0.09	0.22	0.03	0.05	0.09	0.25	0.07	0.25
Could cover emergency of 2,000 KSh	0.85	0.97	0.57	0.89	0.76	0.94	0.90	0.96
Would seek loan from village	0.75	0.33	0.86	0.52	0.80	0.44	0.77	0.41
Has member living in Nairobi	0.53	0.15	0.58	0.43	0.86	0.26	0.74	0.24
Has migrant working in Nairobi	0.29	0.09	0.25	0.17	0.57	0.18	0.49	0.16
Has migrant working in a city	0.66	0.31	0.72	0.46	0.52	0.42	0.60	0.38
Plans to migrate to Nairobi	0.88	0.71	0.78	0.83	0.99	0.90	0.86	0.99
Plans to migrate to any city	0.57	0.49	0.90	0.89	0.70	0.68	0.83	0.75
Perceived migration earnings	0.13	0.03	0.04	0.01	0.11	0.03	0.12	0.03
# of social contacts in Nairobi	0.15	0.35	0.34	0.94	0.29	0.51	0.30	0.51
# of origin connections (farm assistance)	0.85	0.43	0.70	0.67	0.99	0.33	0.93	0.36
# of origin connections (job advice)	0.36	0.09	0.31	0.07	0.29	0.09	0.31	0.09
Participates in village association	0.63	0.25	0.65	0.12	0.38	0.13	0.39	0.17
Weighted		X		X		X		X
Observations	2,675	2,675	2,187	2,187	2,571	2,571	2,598	2,598

See Table B.1 for data and variable notes. Each column shows p-values from t-tests that means are equal across the set of households receiving and not receiving information within spillover villages, recovered from a regression of each variable on an information treatment dummy on the set of households in spillover villages. Linear regression is used for outcomes bounded between 0 and 1; Poisson regression is used otherwise. Even columns adjust for sampling and response probabilities.

B.3 Robustness to Sample Selection and Estimation Strategy

Table B.5: Estimates are robust to excluding endline data collected indirectly from neighbors.

Migration:	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Information	0.01	0.02*	0.00	0.06	0.05	-0.07
	(0.01)	(0.01)	(0.01)	(0.08)	(0.07)	(0.06)
	[0.20]	[0.09]	[0.82]	[0.43]	[0.47]	[0.27]
Mentor	0.02*	0.02**	0.02*	0.15*	0.10	0.08
	(0.01)	(0.01)	(0.01)	(0.09)	(0.08)	(0.07)
	[0.05]	[0.04]	[0.08]	[0.09]	[0.18]	[0.24]
Group	0.03**	0.02*	0.04**	0.19**	0.14	0.12
	(0.01)	(0.01)	(0.02)	(0.09)	(0.08)	(0.08)
	[0.01]	[0.07]	[0.03]	[0.03]	[0.11]	[0.12]
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson
Control Mean	0.11	0.17	0.21	0.13	0.23	0.28
Observations	15,232	15,232	15,232	15,232	15,232	15,232
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Information	0.04	8.67**	50.90*	8.13*	9.02**	6.76
	(0.03)	(3.70)	(30.07)	(4.16)	(3.74)	(4.19)
	[0.21]	[0.02]	[0.09]	[0.05]	[0.02]	[0.11]
Mentor	0.10***	8.84**	60.90*	10.79**	9.36**	8.35*
	(0.03)	(4.18)	(35.63)	(4.74)	(4.20)	(4.84)
	[0.00]	[0.03]	[0.09]	[0.02]	[0.03]	[0.09]
Group	-0.02	-1.71	11.53	1.50	-1.67	1.57
	(0.04)	(4.98)	(48.27)	(5.37)	(5.01)	(5.61)
	[0.53]	[0.73]	[0.81]	[0.78]	[0.74]	[0.78]
Model	OLS	OLS	OLS	OLS	OLS	OLS
Control Mean	-0.02	98.57	688.84	119.96	98.63	121.90
Observations	15,232	15,232	15,232	15,232	15,232	15,232

Impacts are estimated on data from endline surveys, excluding survey data collected indirectly from neighbors. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Any City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Estimates are similar when separately estimating effects in Information and Spillover villages.

Migration:	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Information	0.01	0.02	0.00	0.06	0.06	-0.08
	(0.01)	(0.01)	(0.01)	(0.08)	(0.07)	(0.06)
	[0.30]	[0.13]	[0.95]	[0.46]	[0.40]	[0.20]
Mentor	0.02**	0.02**	0.02*	0.15*	0.10	0.08
	(0.01)	(0.01)	(0.01)	(0.09)	(0.07)	(0.07)
	[0.05]	[0.04]	[0.08]	[0.08]	[0.17]	[0.23]
Group	0.03**	0.02*	0.03**	0.19**	0.13	0.12
	(0.01)	(0.01)	(0.02)	(0.09)	(0.08)	(0.08)
	[0.02]	[0.10]	[0.04]	[0.03]	[0.14]	[0.14]
Spillover	0.02	0.02	0.01	0.08	0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.10)	(0.08)	(0.08)
	[0.13]	[0.17]	[0.58]	[0.40]	[0.75]	[0.83]
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson
Control Mean	0.11	0.17	0.21	0.13	0.23	0.28
Observations	16,339	16,339	16,339	16,339	16,339	16,339
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Information	0.03	9.67**	41.94	9.03**	10.17**	8.39*
	(0.03)	(3.96)	(31.18)	(4.47)	(4.02)	(4.51)
	[0.29]	[0.01]	[0.18]	[0.04]	[0.01]	[0.06]
Mentor	0.11***	8.93**	63.20*	11.02**	9.44**	8.44*
	(0.03)	(4.11)	(35.24)	(4.67)	(4.13)	(4.77)
	[0.00]	[0.03]	[0.07]	[0.02]	[0.02]	[0.08]
Group	-0.03	-2.98	0.56	0.23	-2.98	0.05
	(0.04)	(5.00)	(48.46)	(5.40)	(5.04)	(5.62)
	[0.39]	[0.55]	[0.99]	[0.97]	[0.55]	[0.99]
Spillover	0.05	5.52	74.50	5.04	5.31	2.05
	(0.04)	(5.24)	(48.72)	(5.68)	(5.24)	(5.51)
	[0.25]	[0.29]	[0.13]	[0.37]	[0.31]	[0.71]
Model						
Control Mean	-0.04	97.48	680.96	118.61	97.54	120.69
Observations	16,339	16,339	16,339	16,339	16,339	16,339

Treatment impact coefficients estimated separately for households in Information villages and treated households in Spillover villages (untreated households in Spillover villages are dummied out in estimation). Impacts are estimated on data from endline surveys. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Any City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Results are similar when using randomization inference.

Migration:	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Information	0.01	0.02	0.00	0.06	0.05	-0.07
	[0.20]	[0.11]	[0.84]	[0.39]	[0.45]	[0.14]
Mentor	0.02**	0.02**	0.02*	0.15*	0.10	0.08
	[0.05]	[0.03]	[0.07]	[0.07]	[0.17]	[0.03]
Group	0.03**	0.02	0.03*	0.19*	0.13	0.11
	[0.02]	[0.12]	[0.07]	[0.06]	[0.19]	[0.22]
Model	OLS	OLS	OLS	Poisson	Poisson	Poisson
Control Mean	0.11	0.17	0.21	0.13	0.23	0.28
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Information	0.04	8.67**	49.99	8.09*	9.01**	6.88
	[0.25]	[0.03]	[0.12]	[0.06]	[0.02]	[0.13]
Mentor	0.11***	9.02**	62.96*	11.13**	9.53**	8.55*
	[0.00]	[0.03]	[0.08]	[0.02]	[0.03]	[0.07]
Group	-0.03	-2.73	2.83	0.50	-2.70	0.10
	[1.00]	[1.00]	[0.95]	[0.94]	[1.00]	[0.98]
Model	OLS	OLS	OLS	OLS	OLS	OLS
Control Mean	-0.04	97.48	680.96	118.61	97.54	120.69
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Impacts are estimated on data from endline surveys. Linear regression is used for outcomes with negative values or bounded between 0 and 1; Poisson regression is used otherwise. *Any City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Two-sided p-values estimated through randomization inference in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Estimates are robust to including lasso-selected controls.

Migration:	Recent Migrants to Nairobi	Any Migrants to Nairobi	Recent Migrants to Any City	# Recent Migrants to Nairobi	# Any Migrants to Nairobi	# Recent Migrants to Any City
Information	0.01	0.02	-0.00	0.01	0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[0.25]	[0.11]	[0.93]	[0.24]	[0.32]	[0.45]
Mentor	0.01	0.02**	0.02	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[0.11]	[0.05]	[0.16]	[0.18]	[0.43]	[0.50]
Group	0.02**	0.02	0.03	0.02*	0.02	0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
	[0.05]	[0.24]	[0.11]	[0.07]	[0.40]	[0.22]
Model	OLS	OLS	OLS	OLS	OLS	OLS
Control Mean	0.11	0.17	0.21	0.13	0.23	0.28
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Information	0.03	8.85**	57.40*	7.68*	9.02**	6.95*
	(0.03)	(3.49)	(29.51)	(3.96)	(3.55)	(3.80)
	[0.26]	[0.01]	[0.05]	[0.05]	[0.01]	[0.07]
Mentor	0.09***	7.83**	61.51*	8.92**	8.55**	9.35**
	(0.03)	(3.88)	(35.47)	(4.44)	(3.94)	(4.25)
	[0.00]	[0.04]	[0.08]	[0.04]	[0.03]	[0.03]
Group	-0.06*	-4.42	-3.41	-2.36	-4.29	-1.99
	(0.04)	(4.90)	(47.78)	(5.40)	(4.99)	(5.16)
	[0.09]	[0.37]	[0.94]	[0.66]	[0.39]	[0.70]
Model	OLS	OLS	OLS	OLS	OLS	OLS
Control Mean	-0.04	97.48	680.96	118.61	97.54	120.69
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Controls selected using double-lasso regression (Belloni et al., 2014) on a set of pre-specified pre-treatment variables. Impacts are estimated on data from endline surveys. Linear regression is used for all outcomes. *Any City* includes any urban area. Responses of "Don't Know" are coded as missing. Estimates are adjusted for sampling and response probabilities. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Intervention Details

B.4.1 Information Script (Used in Info, Spillover, Group, and Mentor Villages)

I would now like to tell you about the findings from our research on jobs in Nairobi. We started this program because our research has shown that many people in $county_name County think that incomes in Nairobi are lower than they actually are. When we conducted a smaller version of this project in 2016, we found that having better information encouraged some people to move to Nairobi, and they earned much more money as a result. So now we are back to scale up that program in hopes that more people across Kenya can benefit.

So far, we have looked specifically at workers (not students) in Nairobi, and we compared them to workers in towns in $county_name, such as $local_town. We analyzed data collected in 2015-–2016 by the Kenya National Bureau of Statistics. Let me tell you quickly about the data: KNBS hired a research team to survey tens of thousands of people across Kenya so that they could understand the lifestyles of many different types of people. In Nairobi alone, they talked to over 500 households about over 1,600 individuals.

Here is a summary of our findings. I'll leave this page with you after the survey, so you don't need to remember any of the numbers right now. I'm going to go through it and explain it to you. Stop me at any point if something doesn't make sense.

We started by looking at young adult men, ages 25–29, who graduated from Form 4 but did not go to college, and who were working at least 20 hours per week. We wanted to know how much they earn in Nairobi, and to compare that to how much they earn in $county_name towns. Of course, different people earn different amounts of money, so what we did is look at how much a typical person was earning.

If you look at the bottom of the sheet, you'll see what I mean by "typical". If you ordered every worker in a group from poorest to richest, the typical person would be in the middle, not poor and not rich. We found that these workers earn about 25,700 KSh per month in Nairobi. When we asked people across $county_name County, they said they thought these workers only earned $perception, but actually they earn about double that.

Does that make sense? Do you have any questions so far?

We also looked at people on the low and the high end. Again the picture at the bottom shows what this means. We found that people on the low end earn 13,500 and people on the high end earn 31,500 per month. Not everyone in this group was working 20 hours per week; the number working is 91 out of 100. Remember that we looked at their individual income: so, for example, a household of 2 working adults would be earning double compared to a single person.

Compared to the same group who live in $county_name towns, considering employed and unemployed together, that's about $ratio_men times as much. Specifically, for every 100 shillings people in $county_name towns are earning, people in Nairobi are earning $example_men.

We also looked at women ages 25–29 who graduated from Form 4. 72 out of 100 are working 20 hours or more per week. Typically they earn 17,800 per month. This goes from 10,200 on the low end to 20,700 on the high end. That's about $ratio_women times as much compared to all people in this group who live in $county_name towns. Specifically, for every 100 shillings people in $county_name towns are earning, people in Nairobi are earning

$example_women.

Please remember that this information is correct for most people, but not everyone. Some are earning more, and some are earnings less. Also, remember that people who migrate to Nairobi might earn a different amount compared to people who already live there.

Do you have any questions about this?

We also looked at how much rent costs in Nairobi. A typical one-room house costs 4,000 KSh per month. The ones on the cheaper end cost 2,900, and the more expensive ones about 5,400. For a two-room house, the typical cost is about 10,000 per month.

So, considering everything together, we can tell you about the typical experience for a young man in Nairobi, say 27 years old, whom we will call John. John earns about 25,000 each month. He lives in a 1-bedroom home and pays 4,300 per month in rent. That home has a kerosene stove, water is piped to the plot, there is a flush toilet, and an electric connection from main.

The last thing we looked at was people of other ages and educational attainment. Men ages 18–22 with Std 8 typically earn 20,800 per month. Women ages 18–22 with Std 8 typically earn 12,900 per month. Men ages 40–59 with Form 4 typically earn 30,700 per month. Women ages 40–59 with Form 4 typically earn 22,900 per month. Also, fewer 18–22 year-olds are working. For men, it is 53 out of 100 who work 20 or more hours per week. For women, it is 33 out of 100.

That is all, thank you very much for your time. I really appreciate it.

B.4.2 Group Script (Used in Group Villages)

We have organized the meeting today to share these findings with the village and also to provide a forum for individuals to discuss their future plans and past experiences. When we visited a few months ago, we found that many people did not have this information and believed incomes in Nairobi are lower than they actually are. So, it can be helpful to share and discuss these findings with your neighbors and learn any additional information about the economy in Nairobi and elsewhere.

[*Information sheets were shared with each attendee and the info script was read to the group at this point.*]

When we visited a few months ago, some of your neighbors said they are thinking about migrating soon. Even more people might be interested after hearing this information. Now I would like to ask about your plans and experiences.

It can be helpful to talk to others who have lived in Nairobi before about their experiences.

[*At this point, staff invited former migrants who have been to Nairobi to talk to the group about their experiences, if they were comfortable. Suggested topics for conversation were:*

- *How long ago were you in Nairobi?*
- *Why did you decide to go to Nairobi?*
- *Did you go alone, travel with someone, or meet someone there?*
- *How did you decide where to live?*
- *How did you decide where to look for work?*

- *What were the best and worst parts about living in Nairobi?*
- *What do many people in the villages not know about Nairobi?*]

How many people are thinking about moving to Nairobi for work in the next 6 months? Raise your hand if you might go – we know many people are not sure and still deciding.

It could be helpful to talk to others around you who are thinking about moving to Nairobi too. You could learn about job opportunities, save money by renting an apartment together, and / or feel more comfortable when you arrive with a friend. How many people would be open discussing their thoughts and plans with others? We encourage everyone who is interested to remain here after this meeting to exchange contact information, learn from each other, and discuss opportunities to coordinate.

Remember, only some of the people in the village are here today. Others who are not here today might also be thinking about moving to Nairobi. We encourage you to discuss the information we presented today in case they have additional information or you are able to coordinate plans together.

[*At this point, staff encouraged attendees to split into small groups of four. Suggested topics for small group conversation were:*

- *When will you go to Nairobi?*
- *Where do you plan to live?*
- *How do you plan to find work?*
- *How much does it cost to get to Nairobi?*
- *Do you have any friends or family in Nairobi you can ask questions to?*]

B.4.3 Mentor Script (Used in Mentor Villages)

[*The following script was delivered after the information script of Section B.4.1.*]

I now want to tell you about a program we are offering in your village to help new migrants get established in Nairobi. This program pairs new migrants from villages like yours with experienced guides who live in Nairobi.

We started this program because Nairobi is a big and complicated city, and it can be difficult for new migrants to learn their way around. I will tell you about the program, and also leave this sheet with you which describes how the program works.

- You would be matched to an experienced guide who lives in Nairobi and who is the same gender as you.
- You would not have to pay us or the guide.
- You can enroll in the program anytime between January 1, 2023 and March 31, 2023.
- The guide would exchange numbers with you if you want to talk to them before you migrate.

- Once you arrive in Nairobi, the guide would meet with you at least 4 times over the next two months at a convenient location. The meetings should take around an hour, but you can meet longer if you both want. The first meeting would be held at our office in Nairobi, along with a program facilitator from Vyxer REMIT.
- We would find someone who is a good match for you, depending on what you plan to do in Nairobi. For example, you might want to be matched to a former migrant from your area. Or you might want someone in a certain occupation or location.
- If you encounter any issues with your guide, you can call us and we would find a new guide for you.
- Please remember that this program is not a guarantee of a job in Nairobi. The program is only to give you as much information as possible about Nairobi. Please also remember that there is no cash available as part of this program.

The program is completely voluntary. You can decline to participate or you can withdraw at any time. To enroll, text your code to $phone between January 1 and March 31 and we will call you to arrange the guide.

Anyone in your household is eligible to use this program (maximum 1 person per household).

Do you have any questions about this?

B.4.4 Mentor Meeting Script (Used During Initial Meetings Between Mentors and Mentees in the City)

Purpose of the Program: REMIT is leading this project to improve economic well-being for people in Kenya. Many people in Kenya come to Nairobi to look for work, but do not know the city well. Our program matches new migrants in Nairobi with experienced residents who have offered to teach them about the city.

Design of the Program: Guides and migrants will meet 4 times, once per week, during the first month after the migrant arrives in Nairobi. You should agree with your partner on a convenient meeting location and schedule. Because many guides are very busy, we suggest meeting at the guide's place of business.

[At this point, mentors and mentees were asked to exchange contact information and agree on a regular meeting place and schedule. The migrant was asked to write down what they hoped to learn from the meetings and three goals for the next month in Nairobi. The mentor was asked to write down the most useful things they think they can teach the migrant and what they wished they had known when they first arrived in Nairobi. Staff then went over a code of conduct and frequently asked questions. Suggested topics for the first meeting were:]

For the Migrant:

- What kind of job do you want to find in Nairobi?

- Where do you plan to live, and who will you live with?
- How long do you want to stay in Nairobi? If you want to leave, how will you decide when to leave?
- Do you feel optimistic that you will find a good job in Nairobi?
- Do you plan to convince any of your friends or family to move to Nairobi?

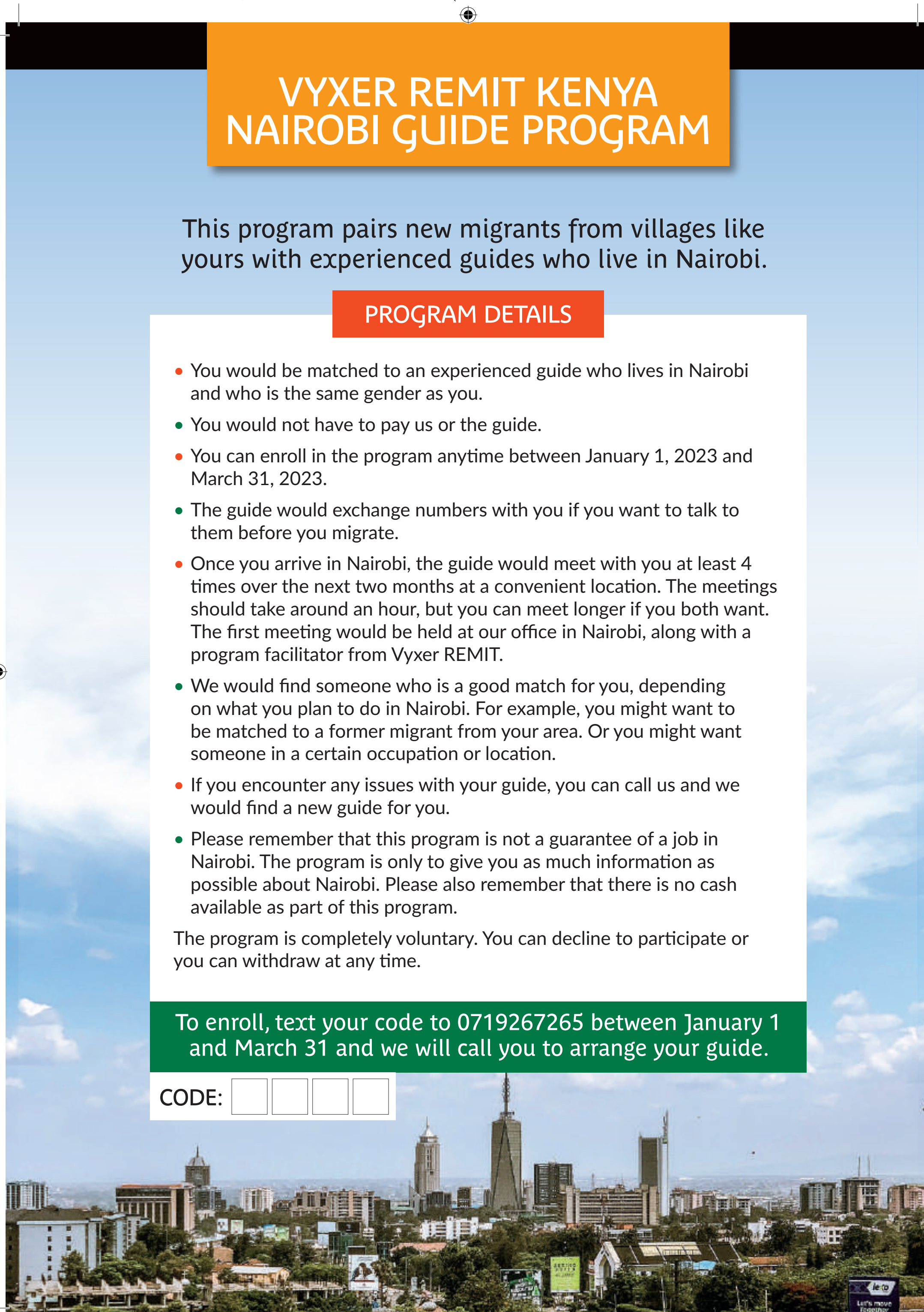
For the Guide:

- When did you come to Nairobi? Where did you come from, and who did you come with?
- Where did you live when you first started in Nairobi?
- How did you find your first job in Nairobi?
- What challenges have you faced in Nairobi (especially when you first arrived)? How did you overcome them?
- Have you ever changed jobs? How did you find the new job?
- Have you ever considered starting a business in Nairobi? What kind? If not, why not?
- Did you ever move to a new home within Nairobi? Why or why not?
- What do you think the best and worst places to live in Nairobi are? Why?
- What are the best places to look for work in Nairobi? What is the best way to find a job? What is the best way to convince possible employers that you are a good worker?
- What surprised you the most about living in Nairobi, compared to what you expected?

B.4.5 Information Sheets

Figure B.1: Information Sheet (Given to All Treated Households)

Figure B.2: Mentor Program Description (Given to All Households in Mentor Villages)



VYXER REMIT KENYA NAIROBI GUIDE PROGRAM

This program pairs new migrants from villages like yours with experienced guides who live in Nairobi.

PROGRAM DETAILS

- You would be matched to an experienced guide who lives in Nairobi and who is the same gender as you.
- You would not have to pay us or the guide.
- You can enroll in the program anytime between January 1, 2023 and March 31, 2023.
- The guide would exchange numbers with you if you want to talk to them before you migrate.
- Once you arrive in Nairobi, the guide would meet with you at least 4 times over the next two months at a convenient location. The meetings should take around an hour, but you can meet longer if you both want. The first meeting would be held at our office in Nairobi, along with a program facilitator from Vyxer REMIT.
- We would find someone who is a good match for you, depending on what you plan to do in Nairobi. For example, you might want to be matched to a former migrant from your area. Or you might want someone in a certain occupation or location.
- If you encounter any issues with your guide, you can call us and we would find a new guide for you.
- Please remember that this program is not a guarantee of a job in Nairobi. The program is only to give you as much information as possible about Nairobi. Please also remember that there is no cash available as part of this program.

The program is completely voluntary. You can decline to participate or you can withdraw at any time.

To enroll, text your code to 0719267265 between January 1 and March 31 and we will call you to arrange your guide.

CODE: | | | |

C Measurement and Estimation Details

C.1 Machine-Learning Analysis of Heterogeneous Migration Responses

We implement the generic machine-learning procedure of Chernozhukov et al. (2025), as specified in our pre-analysis plan. For each treatment arm we compare treated households to the pure control group. Using baseline listing- and pre-treatment-survey characteristics, we train an ensemble proxy for the household-level treatment effect with three machine-learning approaches (boosted trees, random forests, and generalized additive models), combined into a single proxy by convex non-negative weights estimated on a held-out auxiliary fold by minimizing the weighted Robinson residual loss.

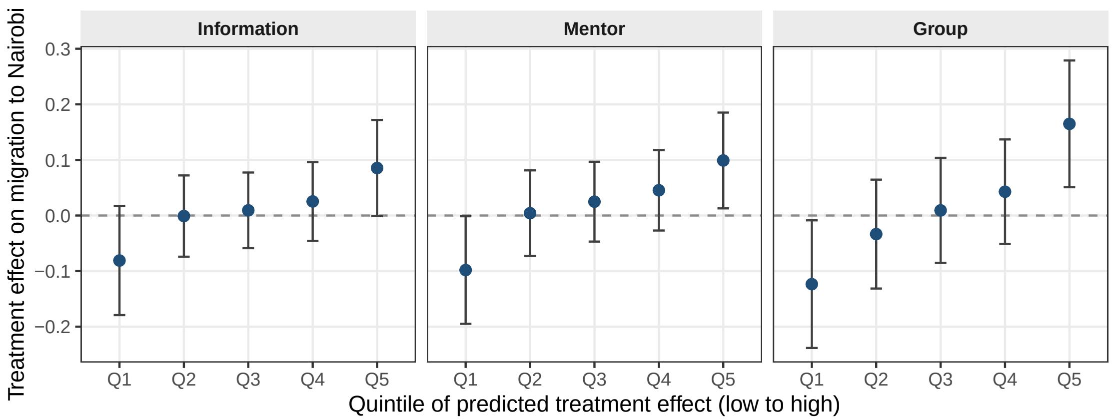
Concretely, each randomization split partitions the sample into an auxiliary split A (two-thirds) and a main split M (one-third); within A, the three meta-learners are trained on a sub-sample A_1 and the ensemble weights are estimated on a disjoint validation sub-sample A_2. The resulting proxy is evaluated on the main split M, which is untouched during meta-learner training, weight estimation, and ensemble selection. Per-split best linear predictor (BLP), sorted group average treatment effects (GATES), and classification analysis (CLAN) statistics are computed on M and aggregated across 250 repeated random sample splits by their median, with p-values pre-multiplied by two to control split-induced size distortion as in Chernozhukov et al. (2025). The comparison sample is trimmed to the region of common propensity-score support, which for the Group arm drops the strata in which Group was not assigned. The BLP regression of migration outcomes on the propensity-residualized treatment, the proxy, and their interaction tests whether the proxy captures genuine effect heterogeneity. The GATES divide households into five quintiles of predicted effect and estimate the average treatment effect within each. CLAN compares the baseline characteristics of households in the top and bottom quintile.

The best-linear-predictor test rejects effect homogeneity in every arm (Table C.1). Sorted effects rise sharply across predicted-effect quintiles in all three arms, and the difference between the tails is significant in each (Figure C.1). The heterogeneity is not specific to the binary indicator of whether a household has any migrant in Nairobi; it holds across alternative outcome definitions, including the number of migrants and recent migration to any city.

The most-responsive households in the *Information* and *Mentor* arms fit a high-cost profile (Appendix Table C.2; see Figure 2 for the visual cross-arm comparison). They have substantially lower baseline household income (Information −0.56 sd, Mentor −0.58 sd; both $p < 0.001$) and are more likely to report being unable to raise KSH 500 for an emergency (+8.5 and +11.5 pp; $p = 0.02$ and $p = 0.01$). They are also weakly attached to Nairobi: fewer have a household member currently working there (−0.29 and −0.26; both $p < 0.001$) or who has ever worked there (−0.25 and −0.22; both $p < 0.001$). They are also less educated (fewer with more than primary schooling, −0.11 and −0.19 sd; both $p < 0.01$) and less likely to have planned to migrate to Nairobi before treatment (−0.06 sd, $p = 0.01$, in Information; −0.04 sd, $p = 0.06$, in Mentor). *Group's* most-responsive households share the income gradient (a comparable −0.51 sd, $p < 0.001$) and the liquidity constraint (+10.3 pp unable to raise KSH 500, $p = 0.06$), but the weak Nairobi attachment that marks the individualized arms is absent: most- and least-responsive Group households are statistically indistinguishable on whether a member currently works in Nairobi (-0.04, $p = 0.19$) or has ever worked there (-0.02, $p = 0.22$). Group's most-responsive set is thus marked by financial fragility but not by detachment from Nairobi.

Several baseline dimensions do not separate the responsive households in any arm: non-agricultural livelihood is the one characteristic that flips sign across arms—the most-responsive are somewhat less likely to hold non-farm work in the individualized arms but more likely in Group (+10.5 pp, $p = 0.05$). Group's best-linear-predictor heterogeneity coefficient is nonetheless the largest of the three arms (0.103, $p < 0.001$).

Figure C.1: Treatment effect by quintile of predicted effect (GATES)



Average treatment effect on migration to Nairobi, by quintile of the machine-learning proxy for the household-level treatment effect, for each treatment arm versus the pure control group. Bars are 95% confidence intervals. Estimates follow the generic machine-learning procedure of Chernozhukov et al. (2025) with 250 sample splits.

Table C.1: Treatment-Effect Heterogeneity in Migration to Nairobi

	Information	Mentor	Group
Panel A. Any migrant to Nairobi			
BLP heterogeneity	0.061***	0.074***	0.103***
	[0.00]	[0.00]	[0.00]
GATES, bottom quintile	-0.081*	-0.098**	-0.123**
	[0.10]	[0.05]	[0.04]
GATES, top quintile	0.085**	0.099**	0.165***
	[0.04]	[0.03]	[0.00]
GATES, top − bottom	0.170**	0.204***	0.282***
	[0.01]	[0.00]	[0.00]
Panel B. BLP heterogeneity, alternative migration definitions			
Any recent migrant to Nairobi	0.058***	0.048**	0.069**
	[0.00]	[0.02]	[0.02]
Number of migrants to Nairobi	0.083**	0.100***	0.260***
	[0.02]	[0.01]	[0.00]
Number of recent migrants to Nairobi	0.068***	0.065**	0.077*
	[0.00]	[0.01]	[0.05]
Recent migration to any city	0.073***	0.061***	0.104***
	[0.00]	[0.01]	[0.00]

Notes. Estimates from the generic machine-learning procedure of Chernozhukov et al. (2025), applied to each treatment arm versus the pure control group, with repeated sample splitting (250 splits, median aggregation) following our pre-analysis plan. *BLP heterogeneity* is the best-linear-predictor coefficient on the demeaned machine-learning proxy, which tests effect homogeneity. For the migration outcome, *GATES* reports average treatment effects in the bottom and top quintile of predicted effect, and their difference. Panel B reports the heterogeneity test only, for alternative migration definitions; all rows are migration to Nairobi except the last, which is recent migration to any urban area. Common-support trimming drops the strata in which the Group arm is unsupported. Two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Baseline Characteristics of the Most-Responsive Households

Baseline characteristic	Information	Mentor	Group
Baseline HH income (standardized)	-0.564***	-0.576***	-0.511***
	(0.093)	(0.093)	(0.134)
	[0.00]	[0.00]	[0.00]
Could raise KSH 500 in an emergency	-0.085**	-0.115***	-0.103*
	(0.037)	(0.041)	(0.055)
	[0.02]	[0.01]	[0.06]
Reports no recent financial shock	-0.093**	-0.016	-0.046
	(0.040)	(0.044)	(0.059)
	[0.02]	[0.17]	[0.22]
Any non-agricultural occupation in HH	-0.060*	-0.066**	0.105**
	(0.040)	(0.042)	(0.058)
	[0.06]	[0.04]	[0.05]
Any member is working in Nairobi	-0.289***	-0.261***	-0.042
	(0.040)	(0.042)	(0.059)
	[0.00]	[0.00]	[0.19]
Any member has ever worked in Nairobi	-0.246***	-0.220***	-0.015
	(0.039)	(0.042)	(0.066)
	[0.00]	[0.00]	[0.22]
Any member with more than primary education	-0.110***	-0.192***	0.076
	(0.040)	(0.043)	(0.059)
	[0.01]	[0.00]	[0.16]
Plans to migrate to Nairobi	-0.063**	-0.043*	0.051*
	(0.026)	(0.027)	(0.028)
	[0.01]	[0.06]	[0.06]
Perceived migration income	-0.059**	0.035*	-0.123***
	(0.038)	(0.042)	(0.052)
	[0.04]	[0.06]	[0.01]
Nairobi migrant in education or training	0.051	0.024	0.232***
	(0.041)	(0.041)	(0.058)
	[0.20]	[0.39]	[0.00]
Nairobi migrant in casual labor	-0.060	-0.088	-0.240***
	(0.055)	(0.061)	(0.080)
	[0.20]	[0.13]	[0.00]

Notes. Classification analysis (CLAN) from the machine-learning procedure of Chernozhukov et al. (2025). Each cell is the difference in the baseline characteristic between households in the top and bottom quintile of predicted treatment effect on migration to Nairobi, with the standard error in parentheses and the two-sided p-value in brackets, aggregated by median over 250 sample splits. The characteristics are a fixed panel chosen for interpretability; CLAN is recomputed for each of them, and for every arm, from the saved per-split quintile assignments of the estimation run. Household income is standardized to mean 0, standard deviation 1; perceived migration income is a re-centred continuous index; the remaining characteristics are binary indicators. The two Nairobi-migrant-activity rows are defined only among the households that have an existing Nairobi migrant. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.2 Details on Estimating the Marginal Returns to Migrating

In this section, we describe our procedure for estimating the marginal return to migrating for compliers by combining estimates from Information and Spillover villages. Intuitively, we exploit the fact that the lack of information or migration spillovers onto untreated households implies that economic gains to these households identify the village spillover effects of migration, which we can then subtract from the total economic gains of the Information treatment—which includes both spillovers and direct effects.

Letting Y_i denote income of household i and $Mig_i \in \{0, 1\}$ denote whether the household sent a migrant to Nairobi, consider the following model of household income:

$$Y_i = \alpha_i + \beta_i Mig_i + \gamma_i f(Mig_{-i}),$$

where α_i is household i's potential income when no one in the village migrates, β_i is i's return to migrating, and γ_i is the economic spillover benefit i receives which depends on the vector Mig_{-i} of other households' migration decisions in their village and the function f. Our exclusion restriction is that i's treatment assignment affects i's income only through i's migration or through the migration of others in their village.[33] We make two additional functional-form assumptions. First, we assume that f is linear in the share of migrants in i's village: we expect the approximation error to be small given that treatment impacts on the share of the village migrating are low (about 1–3% on average). Second, we assume that there are no differences on average in the spillover coefficients γ_i for compliers, always-takers, and never-takers. While this is likely a stronger assumption, we expect its influence on our estimates to be small given that never-takers represent the great majority of the sample (about 80% on average).

Taking means by treatment group and letting β^C and γ^C denote the average returns to migrating for, and the average spillover benefit onto, compliers respectively gives:

$$\begin{aligned} E[Y_i|Info] - E[Y_i|Control] &= (\beta^C + \gamma^C) \times (E[Mig_i|Info] - E[Mig_i|Control]) \\ E[Y_i|\mathit{Spillover}, \mathit{Untreated}] - E[Y_i|Control] &= \gamma^C \times (E[Mig_i|\mathit{Spillover}] - E[Mig_i|Control]) \end{aligned}$$

where $Info$ refers to households in Information villages, *Control* to those in Control villages, *Spillover* to all households in spillover villages, and *Spillover, Untreated* to untreated households in spillover villages. We exclude the direct benefits of migrating from the second equation because we find zero indirect impact on migration.

Solving the above equations yields a direct return to migrating of about \$50 per month per migrating household and about \$8 per month per household in spillover effects.[34]

[33] For this exercise, we require only the weaker assumption that spillovers operate within treatment groups, but as our experiment is designed around village-level effects, we maintain the assumption of no inter-village spillovers here.

[34] We use a migration impact of 0.0176 in Information villages and 0.0132 in Spillover villages, and an income impact of \$8.67 per month in Information villages and \$5.95 per month for untreated households in Spillover villages.

C.3 Details on Cross-Country Data on Migrant Selection

To assess the prevalence of positive migrant selection from origin populations, we use census data available through IPUMS International. IPUMS provides several global migration variables that make it possible to measure out-migration rates—and characterize the sign and degree of selection—across countries. Specifically, we use the variables *GEOMIG1_1*, *GEOMIG1_5*, and *GEOMIG1_10*, which provide the 1st subnational geographic level of residence 1, 5, or 10 years prior to survey respectively.[35] We focus on low or low-middle income countries in sub-Saharan Africa and South and Southeast Asia, and use the most recent available census year. This produces a sample of 15 countries with non-missing migration data. We measure selection among individuals 18 and older using years of schooling (provided in the *yrschool* variable), or educational attainment (variable *edattaind*) if *yrschool* is missing. We assign individuals to origin locations using prior location for migrants (defined as those whose current and prior locations differ at the 1st subnational geographic level) and current location for non-migrants. Finally, we compute origin location-level means, weighting to account for sampling probabilities, to estimate migration rates and average education for migrants and non-migrants. Results are shown in Figure 4.

C.4 Family Income

Our income measures follow our pre-analaysis plan, hosted on the AEA RCT Registry (Barnett-Howell et al., 2023). Our primary measure of family income is computed as

$$\text{Income}_i = \sum_b \text{Village Profit}_{ib} + \sum_m \left(\text{Wage}_{im} + \text{Migrant Profit}_{im}\right),$$

where Village Profit_{ib} is the profit of business b located in the village over the past 30 days, Wage_{im} is the wage income from casual and formal jobs earned by family member m over the past 30 days, and Migrant Profit_{im} is the profit from all businesses located outside the village owned by family member m. Profits are asked in a single question for each business in the village: "In the past 30 days, how much profit did your household earn from ${business}? By profits I mean the earnings you kept after paying for costs like materials. FO: Do NOT subtract large, one-time costs such as stall upgrades, a generator, or durable tools." For businesses outside the village, profits are measured at the member level using the question, "In the past 30 days, how much profit do you think ${name} earned from all their businesses in ${city}? By profits I mean the earnings they kept after paying for costs like materials. FO: Do NOT subtract large, one-time costs such as stall upgrades, a generator, or durable tools." Wages were measured at the member level using the question, "How much money did ${name} earn from any job, including casual jobs, in the past 30 days? Do not include profits from businesses that ${name} owns or operates. FO: Include jobs in any location. Enter 0 if they earned nothing." When family members were directly surveyed over the phone (see Section 2.3), we use their direct report of their own income in place of the household head's report.

[35]In cases where multiple prior residence variables were available, we used the longest available time span (e.g. 10-year residence over 5-year residence).

C.5 Statistics Used for Information Treatment

Income. To estimate conditional moments of the distribution of individual income by location for the information treatment, we rely on microdata from the Kenya Integrated Household Budget Survey (KIHBS) 2015–2016 wave. Restricting to individuals aged 18–69, we compute, for members m living in households i:

$$\text{Income}_{im} = \text{Wage}_{im} + \frac{1}{N_a}\left(\text{Livestock}_i + \text{Crops}_i + \text{Transfers}_i + \text{Other}_i\right) + \frac{1}{N_b}\sum_b \text{Profit}_{ib},$$

where Wage_{im} is the wage income from casual and formal jobs earned by family member m over the past 30 days, Profit_{ib} is the profit of business b over the past six months (converted to a monthly value by dividing by six), Livestock_i is the households profit (computed as sales minus input costs) from selling livestock, Crops_i is the households profit (computed as sales minus input costs) from selling crops, Transfers_i is incoming transfers from outside the household minus outgoing transfers, Other_i is other income such as rental income, N_a is the number of adults aged 18 or older in the household, and N_b is the number of members listing "own-account worker" as their primary labor force status. Livestock_i, Crops_i, Transfers_i, and Other_i are measured over the past year and converted to monthly values by dividing by 12. We adjust for inflation between the KIHBS survey and our baseline survey using historical USD-KES exchange rate data and the US CPI over the same period. To estimate quantiles for specific demographic groups, we use recentered influence functions (Stata command: *rifhdreg*), controlling for demographic characteristics. We report income levels conditional on employment (as defined below) and ratios with respect to reference areas unconditionally.

Employment. We measured the share of a sub-population employed using the question, "How many hours does [NAME] usually work per week in all these [economic] activities?". We define "employed" as working at least 20 hours in a typical week, and explained this definition during the information treatment.

Rent. Spending on rent is measured directly, among renters only, using the question "How much per month does HH pay to rent this dwelling?". We condition statistics by dwelling size using the question "How many dwelling units does this household occupy?" (restricting to single-dwelling units) and "How many habitable rooms does this HH occupy? (DO NOT COUNT BATHROOMS, TOILETS, STOREROOMS, OR GARAGES)" (conditioning on one or two habitable rooms).

Utilities. We measure the presence of utility types by tabulating answers to the questions, "What is the [MAIN] type of appliance used for cooking?", "What is the main source of water for your household over the past 1 year for drinking?", "What kind of toilet facility does your household usually use?", and "What is the [MAIN] source of lighting?". We condition on single-dwelling, one-bedroom units.

C.6 Prices

Differences in nominal incomes across space may not correctly proxy for differences in standards of living if prices for the same goods or services vary across space. To test whether changes in nominal income due to migrating are likely to reflect real changes in standards of living, we adjust nominal incomes using location-specific price indices.

Following our pre-analysis plan, we construct a Nairobi consumer price index (NCPI) and an urban consumer price index (UCPI) which excludes Nairobi—both defined relative to rural parts of our study counties—by combining information from our surveys with information from KIHBS. KIHBS tracks household expenditure, including units purchased, at a granular level. Using these data, we compute, for each expenditure category c (food; rent and utilities including transportation costs; household items including furniture, appliances, and non-food consumables; education; and healthcare):

$$NCPI_c = \frac{\sum_{g \in c}(x_g^R \times p_g^N)}{\sum_{g \in c}(x_g^R \times p_g^R) \times QUAL_c^N}, \quad \text{and}$$

$$UCPI_c = \frac{\sum_T \lambda_T \sum_{g \in c}(x_g^R \times p_g^T)}{\sum_{g \in c}(x_g^R \times p_g^R) \times QUAL_c^U},$$

where x_g^R is the average rural quantity consumed of good g, p_g^R is the quantity-weighted average price of good g in rural areas, p_g^N is the quantity-weighted average price of good g in Nairobi, p_g^T is the quantity-weighted average price of good g in a town T other than Nairobi, λ_T is a town-specific weight (computed as the share of urban, non-Nairobi migrants in our baseline sample traveling to town T, for the top 10 destinations, with weights summing to 1), $QUAL_c^N$ is the quality premium for Nairobi goods in category c, and analogously for $QUAL_c^U$.

To aggregate up across categories, we compute the weighted average

$$NCPI = \sum_c NCPI_c \times Expenditure_c,$$

where $Expenditure_c$ is category c's share of total average expenditure in our sample.

To compute average quantities, we use household-by-item level data on quantities consumed over the relevant reference period (7 days for food, 3 months for clothing, 12 months for education and household goods, and one month for other categories) and compute averages within a location (Nairobi, other cities, or rural parts of the five study counties). Prices are computed for each location $L \in \{R, N, T\}$ as:

$$p_g^L = \frac{\sum_{i \in L} x_{ig} p_{ig}}{\sum_{i \in L} x_{ig}},$$

where i indexes over households in rural parts of our five study counties, Nairobi, or other Kenyan towns for p_g^R, p_g^N, and p_g^T respectively.

We estimate $QUAL_c^N$ and $QUAL_c^U$ using endline survey data. For $QUAL_c^N$, we ask all households who have sent a migrant to Nairobi in the past how much they would have been willing to spend, for each category c, for the same goods if they were the same quality as typical goods in Nairobi. These questions came immediately after questions measuring actual spending in each category, and respondents were reminded of their actual spending for each question. For $QUAL_c^U$, we do the same about the city other than Nairobi that a migrant from the household has spent the most time in over the past five years (skipping households with no past migrants in other cities). If a household is asked both questions, we randomize the order. The ratio of their answer to actual spending yields a household-level quality index, which we average across households to produce $QUAL_c^N$ and $QUAL_c^U$.

The NCPI and UCPI can be interpreted as the spatial analog of the Laspeyres index: that is, as the additional spending that would be required to consume the average basket of goods consumed in rural areas, net of quality differences, expressed as a multiple of average rural spending.

To compute real income, we deflate nominal income based on where it is spent. For income earned by migrants in the past 30 days, we deflate income net of remittances to the rural household, and do not deflate remittances. For income earned over the course of a year, we deflate income net of remittances and any savings brought back to the rural household.

The following table shows NCPI and UCPI, with and without quality adjustments, for each expenditure category and for an expenditure-weighted average consumer basket:

Table C.3: Nairobi and Urban Consumer Price Indices

	NCPI	NCPI	UCPI	UCPI
Food	1.16	0.58	1.10	0.55
Rent	4.22	2.11	1.71	1.02
Household items	1.70	0.85	1.35	0.90
Education	1.86	0.93	1.15	0.86
Healthcare	2.01	1.26	0.95	0.63
Average basket	**2.34**	**1.20**	**1.31**	**0.79**
Quality-Adjusted?		X		X

C.7 Non-Pecuniary Amenities

Spatial income differences may in part reflect compensating differentials, such as access to amenities like public utilities, education, and healthcare. Following our pre-analysis plan, we construct an amenity-adjusted measure of monthly income by asking urban migrants what income would make them indifferent between living in their destination city and their former place of residence (as of the baseline survey). For each migrant surveyed by phone, we ask "What's the lowest income per month that would convince you to move back to ${baseline_location}, if you could earn it while living there? As a reminder, you told me that your current income is ${income} per month." We then compute amenity-adjusted family income as:

$$\text{Income}_i = \sum_b \text{Village Profit}_i + \sum_{m \in i} \text{Rural Indifference Income}_{im},$$

where *Rural Indifference Income*$_{im}$ is the rural indifference income (for migrants) or actual income (for non-migrants).

For urban migrants we could not successfully survey, we predict their indifference income from a linear model estimated on surveyed migrants, selecting individual-level predictors using lasso.

Table C.4 presents validation tests of our measure of *Rural Indifference Income*$_{im}$ estimated on Nairobi migrants. If respondents understood the question, we expect them to report a higher rural indifference income the better they are doing in the city, subjectively and objectively. This is indeed the case: the reported rural indifference income is strongly positively correlated with earned urban income, as shown in column 1 ($p < 0.01$). It is also strongly positively correlated with an indicator for whether the migrant reports preferring life in the destination and being happy with their life, as shown in columns 2 and 3. In column 4, we add an index measure of the quality of utilities at the migrant's home in Nairobi and the migrant's intended duration of stay in Nairobi, both of which are strongly positively correlated with the rural indifference income. Finally, in column 5, we add demographic controls, which change the estimates little. These results increase our confidence that our survey question is capturing true variation in the valuation of non-pecuniary amenities in the destination.

Table C.4: Validating Amenity Adjustments

	Outcome: Rural indifference income				
Earned urban income	0.21***	0.21***	0.21***	0.19***	0.16***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Prefer life in Nairobi to hometown		0.07**	0.06**	0.04	0.04
		(0.03)	(0.03)	(0.03)	(0.03)
		[0.01]	[0.04]	[0.14]	[0.18]
Happy with life			0.11***	0.08***	0.07**
			(0.03)	(0.03)	(0.03)
			[0.00]	[0.01]	[0.02]
Utility quality index (standardized)				0.05***	0.06***
				(0.02)	(0.02)
				[0.00]	[0.00]
Intended duration in Nairobi (standardized)				0.05***	0.05***
				(0.02)	(0.02)
				[0.00]	[0.00]
Demographic Controls					X
Mean	120	120	120	120	120
Observations	1,056	1,052	1,052	1,033	1,029

Data from endline surveys of migrants living in Nairobi. The outcome variable is the lowest income the respondent reports would make them willing to move back to their pre-treatment location (or their hometown, if they were in Nairobi at baseline), in USD/month. All columns use Poisson regression. All incomes are top-coded at 230 USD/month. *Earned urban income* is the migrant's earned income over the past month. *Utility quality index* is the standardized sum of five binary variables indicating safety from crime, an improved toilet, piped water, cooking fuel, and an electric connection at their home. Demographic controls include age, gender, and years of education. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Model Details

D.1 Estimation Details

Assigned Parameters. We assign model parameters as follows:

- The share of the population born in the urban region is taken from the share of Kenya's population living in Nairobi: 0.088.

- The labor share θ is set to the 2019 share of labor compensation in Kenyan GDP from the Penn World Table: 0.68.

- When household income is high, subsistence agricultural consumption $\bar{a}$ becomes relatively negligible and the ratio of non-agricultural to agricultural expenditure $c_i^n/(p^a c_i^a)$ converges to ν. Following Adamopoulos et al. (2024), we set ν to 50 to match a conservative long-run value of this ratio.

- The productivity of urban non-agriculture A_u^n and rural agriculture A_r^a are normalized to 1.[36]

- To estimate the distribution of migration costs, we cluster households into similar groups and infer each group's cost from an optimality condition relating migration decisions to costs, information frictions, and productivities. This procedure is described in detail in Appendix D.2. We assign σ_m to be the standard deviation of the log of estimated migration costs across groups: 1.85.

- The information friction γ is assigned to match the average ratio of true income to perceived income across demographic groups, as discussed in subsection 6.2.

Estimated Parameters. We estimate the remaining parameters using the simulated method of moments. For each candidate set of parameter values, we simulate the model with 1,000,000 households and solve it in general equilibrium. The estimated parameters are the subsistence level of agricultural consumption $\bar{a}$, the rural productivity level A_r^n, the standard deviation of urban productivity σ_u, the standard deviation of rural productivity σ_r, the elasticity of rural productivities with respect to urban non-agricultural productivity d, and the average migration cost μ_m.

We find the values of these parameters that minimize the distance between data moments and model moments. Average food consumption expenditure as share of total consumption expenditure is a moment highly informative of $\bar{a}$. The median urban-rural income gap helps pin A_r^n.[37] The standard deviations of log urban and log rural incomes are informative of σ_u and σ_r, respectively. The slope of the relationship between urban and rural incomes across household clusters is informative of d: Section 4.2 elaborates on this logic and Appendix D.2 discusses the data estimation procedure. Finally, the migration rate (in the data, the share of control households that sent a migrant to Nairobi in the past year) helps pin μ_m. Table 3 lists the parameters, their corresponding moments, moment values in the data, and moment values in the estimated model. Note that the parameter-moment mapping is suggestive, since all moments are affected by all parameters simultaneously.

D.2 Estimating the Distribution of Migration Costs

Because migration costs in our model are abstract—they can include not only transportation costs but also psychic disutility from migrating and search costs—they are difficult to observe directly in data. Instead, we rely on the optimality condition in (3), which relates (observable) migration choices, rural and urban productivities, and the information friction γ to the (unobservable) migration cost m.

This leaves us with an identification problem, as urban and rural productivities are never observed for a single person. To get around this challenge, we group households sharing similar characteristics using cluster analysis. Specifically, we use k-means clustering to create 200 groups of households, clustering on pre-treatment educational attainment and income.

[36] The units of the two goods are meaningless in the model, allowing both A_u^n and A_r^a to be normalized. However, for a given A_u^n, the level A_r^n does change the real allocation between the two regions. Therefore, we leave A_r^n as a parameter to be estimated.

[37] We use urban income gross of migration cost to calculate this gap in the model.

Under the assumption that the counterfactual urban income for rural workers in a cluster is the same as the observed urban income for workers in that cluster (and similarly for rural income), this approach recovers urban and rural productivities for a single unit.

Then, we use the following relationship, derived from (3), to estimate group-level migration costs:

$$\frac{1}{1+\gamma_x}\frac{1}{1+m_g} z^n_{u,g} w^n_u = \max\{z^n_{r,g} w^n_r,\ z^a_{r,g} w^a_r\}, \tag{4}$$

where x is the share migrating in group g; γ_x is the x-th percentile of γ_i in group g; and m_g, $z^n_{u,g}$, $z^n_{r,g}$, and $z^a_{r,g}$ are group-level migration costs and productivities, which we assume to be constant within group.[38] Migration cost parameters can then be taken from the empirical distribution of group-level costs.

This exercise produces a distribution of costs that is approximately log-normal and negatively correlated with migration experience (correlation coefficient $= -0.25$, p-value from bivariate regression < 0.001). Consistent with the discussion in Section 5, the estimated migration cost is positively correlated with engagement in the *Information* and *Mentor* arms and negatively correlated with engagement in the *Group* arm (p-values $= 0.11$ and 0.10 respectively).

Furthermore, the productivities recovered using the cluster analysis can be used to estimate d. The elasticity of rural income with respect to urban income across clusters is a moment that is directly informative of the elasticity of rural productivities with respect to urban productivity, d, in the model. The empirical elasticity of 0.075 implies a positive relationship between potential rural and urban productivities in the model.

D.3 Proofs

Proof of Proposition 1.

Restating Equation 3 while expressing incomes as functions of fundamentals, a rural household i migrates if and only if

$$\frac{1}{1+\gamma_i}\frac{1}{1+m_i} z^n_{u,i} w^n_u \geq \max\left\{(z^n_{u,i})^d \exp\varepsilon^n_{r,i} w^n_r,\ (z^n_{u,i})^d \exp\varepsilon^a_{r,i} w^a_r\right\} \tag{5}$$

Let $\widehat{z}^n_{u,i}$ be the urban productivity that satisfies the condition with equality:

$$\frac{1}{1+\gamma_i}\frac{1}{1+m_i} \widehat{z}^n_{u,i} w^n_u = \max\left\{(\widehat{z}^n_{u,i})^d \exp\varepsilon^n_{r,i} w^n_r,\ (\widehat{z}^n_{u,i})^d \exp\varepsilon^a_{r,i} w^a_r\right\}.$$

As long as $d \neq 1$, this equation has a unique solution, taking the $(\gamma_i, m_i, \varepsilon^n_{r,i}, \varepsilon^a_{r,i})$ tuple as given:

[38] To see how (4) follows from (3), consider ranking members of a group by γ_i and note that the person whose γ_i is at the x-th percentile in the group (where x is the share of the group migrating) should be "close to" indifferent between migrating and not. Exact indifference is guaranteed if the distribution of γ_i in the group is continuous around γ_x, implying that the approximation error declines as group size grows. Since our groups are large (about 85 households per group on average), we expect this approximation error to be small.

$$\widehat{z}^n_{u,i} = \left((1+\gamma_i)(1+m_i) \frac{1}{w^n_u} \max \left\{ \exp \varepsilon^n_{r,i} w^n_r, \;\; \exp \varepsilon^a_{r,i} w^a_r \right\} \right)^{\frac{1}{1-d}}. \tag{6}$$

Selection. Divide both sides of Equation 5 by $\left(\widehat{z}^n_{u,i}\right)^{1-d}$ to get

$$\frac{\left(z^n_{u,i}\right)^{1-d}}{\left(\widehat{z}^n_{u,i}\right)^{1-d}} \geq 1,$$

or

$$\left(z^n_{u,i}\right)^{1-d} \geq \left(\widehat{z}^n_{u,i}\right)^{1-d}. \tag{7}$$

If $d < 1$, then $1 - d > 0$, the left-hand side of Equation 7 is increasing in $z^n_{u,i}$, so the condition is satisfied for all i with $z^n_{u,i} \geq \widehat{z}^n_{u,i}$. Thus, all such i migrate.

If $d > 1$, then $1 - d < 0$, the left-hand side of Equation 7 is decreasing in $z^n_{u,i}$, so the condition is satisfied for all i with $z^n_{u,i} \leq \widehat{z}^n_{u,i}$. Thus, all such i migrate.

Threshold and costs. Differentiate $\widehat{z}^n_{u,i}$ (Equation 6) with respect to γ_i and m_i:

$$\frac{\partial \widehat{z}^n_{u,i}}{\partial \gamma_i} = \frac{1}{1-d} \underbrace{\left((1+m_i) \frac{1}{w^n_u} \max \left\{ \exp \varepsilon^n_{r,i} w^n_r, \;\; \exp \varepsilon^a_{r,i} w^a_r \right\} \right)^{\frac{1}{1-d}} (1+\gamma_i)^{\frac{d}{1-d}}}_{\equiv A},$$

$$\frac{\partial \widehat{z}^n_{u,i}}{\partial m_i} = \frac{1}{1-d} \underbrace{\left((1+\gamma_i) \frac{1}{w^n_u} \max \left\{ \exp \varepsilon^n_{r,i} w^n_r, \;\; \exp \varepsilon^a_{r,i} w^a_r \right\} \right)^{\frac{1}{1-d}} (1+m_i)^{\frac{d}{1-d}}}_{\equiv B}.$$

Note that $A > 0$ and $B > 0$. Hence,

- $\frac{1}{1-d} > 0$, $\frac{\partial \widehat{z}^n_{u,i}}{\gamma_i} > 0$, and $\frac{\partial \widehat{z}^n_{u,i}}{m_i} > 0$ if and only if $d < 1$;
- $\frac{1}{1-d} < 0$, $\frac{\partial \widehat{z}^n_{u,i}}{\gamma_i} < 0$, and $\frac{\partial \widehat{z}^n_{u,i}}{m_i} < 0$ if and only if $d > 1$.

■

D.4 Additional Quantitative Results

Experimental Impacts Matching Belief Updating In this section, we conduct an alternative set of experimental treatments in the model, disciplining each treatment arm by the measured effect on beliefs about migration incomes. Given the observed percent change in expected migration income in each treatment arm (shown in Table 1) we can impute the necessary change in γ to achieve the same change in expected migration income in the model. This interpretation takes a narrow view of the potential benefits of each treatment, and thus constitutes a conservative test of the model's ability to match the experimentally measured impacts on migration and economic outcomes.

As shown in Table 1, the Information treatment increased the expected migration income by 4% in the data. In the model, all households start with the same γ. A 4% increase in perceived migration income implies that treated households' new information friction value is $\gamma' = \frac{1+\gamma}{1+0.04} - 1 = \frac{1+0.789}{1+0.04} - 1 = 0.721$. Mentor and Group treatments differ from the Information treatment only in the magnitude of the change in beliefs. As shown in Table 1, households in the Mentor and Group treatment arms saw 10% and 12% increases in their expected migration income, respectively, corresponding to $\gamma' = \frac{1+0.789}{1+0.10} - 1 = 0.627$ and $\gamma' = \frac{1+0.789}{1+0.12} - 1 = 0.598$ in the model.

Simulation results are presented in Table D.1. The Information treatment increases the migration rate among treated households by 0.4 percentage points: this result is within the range of the 0.2–1.8 percentage point impacts (depending on the measure) in the data. The Mentor treatment and the Group treatment increase the migration rate by 0.9 pp. and 1.1 pp., respectively: smaller than the 1.8–2.6 pp. impacts in the data but larger than the Information treatment.

Narrowly matching the belief updating in the model causes it to understate the economic impacts of interventions. The Information treatment increases the incomes of treated households by 0.57% of the average baseline income. The Mentor treatment boosts incomes by 1.39%: this result matches the greater economic impact of the Mentor arm compared to the Information arm in the data. Counterfactually, the Group treatment promises the largest increase in income of 1.66%, driven by its largest impact on beliefs.

Table D.1: Model: Experimental Impacts, Matching Belief Updating

	Matched Impact			
	Treated Expected Migration Income	Treated γ'	Migration Impact	Income Impact
Information	4.0%	0.721	0.004	0.57%
Mentor	10.0%	0.627	0.009	1.39%
Group	12.0%	0.598	0.011	1.66%

"Migration Impact" is the difference in migration rate among treated households. "Income Impact" is the mean change in observed income (gross of migration cost) divided by mean baseline observed income for treated households.

E Other Pre-Specified Results

Table E.1: Other Pre-Specified Hypotheses (Treatment Impacts)

	Migration Earnings Uncertainty	Aspirations for Children	Health Index
Disaggregated			
Information	-0.022	0.065*	0.009
	(0.013)	(0.038)	(0.031)
	[0.11]	[0.08]	[0.77]
Mentor	-0.046***	-0.007	-0.007
	(0.016)	(0.047)	(0.037)
	[0.00]	[0.89]	[0.86]
Group	-0.103***	-0.053	0.002
	(0.017)	(0.056)	(0.044)
	[0.00]	[0.35]	[0.97]
Model	OLS	OLS	OLS
p-Val: Info. = Mentor	0.12	0.06	0.64
p-Val: Info. = Group	0.00	0.02	0.86
p-Val: Mentor = Group	0.00	0.41	0.85
Control Mean	0.74	0.02	0.00
Observations	14,852	11,067	15,232
Pooled Treatment			
Any Info.	-0.040***	0.027	0.003
	(0.012)	(0.037)	(0.029)
	[0.00]	[0.46]	[0.91]
Observations	14,852	11,067	15,232

Impacts are estimated on data from endline surveys. *Migration earnings uncertainty* is difference between the household's optimistic and pessimistic belief about its potential income after migrating, dividing by its expected belief. *Aspirations for Children* is an Anderson (2008) index combining household heads' beliefs about the highest grade children in the household will obtain and whether those children will live outside the village when they grow up, averaged across children in the household. *Health Index* is an Anderson (2008) index combining an indicator for whether anyone in the household skipped meals in the past two weeks, the number of children with diarrhea episodes in the past two weeks, the number with fever episodes in the past two weeks, and the number of vaccine doses children have received. Linear regression is used for all outcomes . Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.2: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Household-Level, Part 1)

Dimension of Heterogeneity X:	# of Connections in Nairobi	Doesn't Make Decisions Independently	Says Migrating Alone Is Hard	Not Eager To Try New Things	Risk Averse	Has Trouble Following Through	Perceived Migration Earnings	Belief About Nairobi Incomes
Information × X	-0.00	-0.01	-0.01	-0.00	0.01	-0.00	0.00	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.79]	[0.41]	[0.47]	[0.62]	[0.18]	[0.73]	[0.83]	[0.12]
Mentor × X	-0.00	0.00	0.02	-0.01	0.03**	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[1.00]	[0.72]	[0.12]	[0.51]	[0.01]	[0.32]	[0.52]	[0.49]
Group × X	-0.00	-0.00	0.01	-0.01	0.04***	-0.00	-0.01	-0.01
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.88]	[0.75]	[0.55]	[0.57]	[0.01]	[0.77]	[0.59]	[0.42]
X	0.03***	-0.00	-0.00	-0.00	-0.02**	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.00]	[0.96]	[0.87]	[0.55]	[0.02]	[0.40]	[0.59]	[0.14]
p-Val: Info. × X = Mentor × X	0.77	0.19	0.02	0.81	0.16	0.44	0.37	0.52
p-Val: Info. × X = Group × X	0.96	0.64	0.23	0.83	0.05	0.97	0.47	0.79
p-Val: Mentor × X = Group × X	0.88	0.47	0.48	1.00	0.36	0.62	0.99	0.83
Observations	15,458	15,440	15,367	15,419	15,418	15,440	14,868	15,020

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by household-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1. Base treatment variables are controlled but not shown in output. Columns 2–6 use Likert measures asked specifically to the individual listed by the household as most likely to migrate. *Belief About Nairobi Incomes* asks for the respondent's perceived typical income for a specific demographic group living in Nairobi; the reference group was randomized at the household level and the regression in column 8 includes group fixed effects. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.3: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Household-Level, Part 2)

Dimension of Heterogeneity X:	# of Connections in Village	Age of Likely Migrant	Education of Likely Migrant	Household Consumption	Any Non-Agricultural Worker	Village Utilities Index	Happiness of Likely Migrant
Information $\times$ X	-0.02**	0.00	-0.01	-0.00	-0.00	-0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.03]	[0.91]	[0.46]	[0.76]	[0.94]	[0.93]	[0.59]
Mentor $\times$ X	-0.02*	0.00	-0.01	-0.00	-0.01	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.08]	[0.97]	[0.30]	[0.73]	[0.54]	[0.32]	[0.98]
Group $\times$ X	-0.01	0.02	-0.03**	0.02	0.00	0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.35]	[0.18]	[0.02]	[0.24]	[0.85]	[0.44]	[0.17]
X	0.02**	-0.00	0.02***	0.00	0.04***	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.02]	[0.60]	[0.00]	[0.66]	[0.00]	[0.13]	[0.64]
p-Val: Info. $\times$ X = Mentor $\times$ X	0.90	0.96	0.64	0.92	0.53	0.27	0.57
p-Val: Info. $\times$ X = Group $\times$ X	0.48	0.17	0.07	0.11	0.78	0.35	0.05
p-Val: Mentor $\times$ X = Group $\times$ X	0.59	0.25	0.26	0.14	0.46	0.11	0.14
Observations	14,969	15,468	15,442	15,468	15,468	15,468	15,468

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by household-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1. Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.4: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Village-Level, Geographic)

Dimension of Heterogeneity X:	Population Density	Distance to Nairobi	Distance to County Capital	County: Kakamega	County: Makueni	County: Nandi	County: Siaya	County: Vihiga
Information × X	-0.00	-0.01	-0.01	-0.01	0.03	-0.03	0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
	[0.65]	[0.41]	[0.41]	[0.74]	[0.34]	[0.15]	[0.46]	[0.35]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Mentor × X	-0.00	-0.01	0.01	-0.02	0.04	-0.04*	-0.01	0.03
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)
	[0.67]	[0.28]	[0.59]	[0.46]	[0.22]	[0.08]	[0.72]	[0.44]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Group × X	-0.00	0.03	-0.01	0.04	0.00	-0.04*	0.00	-0.02
	(0.02)	(0.03)	(0.01)	(0.03)	(.)	(0.02)	(0.04)	(0.03)
	[0.88]	[0.31]	[0.61]	[0.24]	[.]	[0.08]	[0.99]	[0.49]
	{1.00}	{1.00}	{1.00}	{1.00}	{.}	{1.00}	{1.00}	{1.00}
X	-0.00	-0.01	-0.03**	-0.03	-0.02	-0.07***	0.07**	0.04
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)
	[0.93]	[0.62]	[0.03]	[0.34]	[0.52]	[0.00]	[0.04]	[0.20]
Observations	15,468	15,468	15,468	15,468	15,468	15,468	15,468	15,468

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for county indicators). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.5: Other Heterogeneous Treatment Impacts on Household Income (Prespecified Village-Level, Geographic)

Dimension of Heterogeneity X:	Population Density	Distance to Nairobi	Distance to County Capital	County: Kakamega	County: Makueni	County: Nandi	County: Siaya	County: Vihiga
Information × X	-3.93	-7.06**	-0.45	-9.87	19.02**	-3.38	-5.40	-2.51
	(4.05)	(3.58)	(3.44)	(8.33)	(8.99)	(10.57)	(8.64)	(8.85)
	[0.33]	[0.05]	[0.90]	[0.24]	[0.03]	[0.75]	[0.53]	[0.78]
	{0.99}	{0.25}	{1.00}	{0.90}	{0.25}	{1.00}	{1.00}	{1.00}
Mentor × X	-2.06	-4.75	6.61	-7.80	11.27	-6.04	-16.70	20.18*
	(4.32)	(3.62)	(4.35)	(9.30)	(8.82)	(11.32)	(10.44)	(11.14)
	[0.63]	[0.19]	[0.13]	[0.40]	[0.20]	[0.59]	[0.11]	[0.07]
	{0.48}	{0.48}	{0.48}	{0.48}	{0.48}	{0.48}	{0.48}	{0.48}
Group × X	-7.33	-14.42	0.25	-28.15**	0.00	8.34	0.98	4.50
	(6.71)	(13.01)	(4.67)	(11.65)	(.)	(12.13)	(10.87)	(11.69)
	[0.28]	[0.27]	[0.96]	[0.02]	[.]	[0.49]	[0.93]	[0.70]
	{1.00}	{1.00}	{1.00}	{0.13}	{.}	{1.00}	{1.00}	{1.00}
X	9.82**	-8.29	-8.48	20.03*	-3.19	30.38**	-8.19	-13.96
	(3.82)	(7.42)	(5.81)	(10.92)	(10.32)	(11.78)	(10.41)	(10.64)
	[0.01]	[0.26]	[0.14]	[0.07]	[0.76]	[0.01]	[0.43]	[0.19]
Observations	15,468	15,468	15,468	15,468	15,468	15,468	15,468	15,468

Outcome is household income earned over the past month (control mean = 97 USD). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for county indicators). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.6: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Village-Level, Economic)

Dimension of Heterogeneity X:	Average Income	Average Consumption	Share With Non-Agriculture Workers	Average Highest Education	Average Utility Quality	Major Tribe: Luhya
Information × X	0.00	-0.00	0.01	0.01	0.02*	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
	[0.67]	[0.95]	[0.47]	[0.52]	[0.09]	[0.33]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Mentor × X	0.00	0.01	0.01	0.02*	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
	[0.98]	[0.57]	[0.35]	[0.07]	[0.30]	[0.94]
	{1.00}	{1.00}	{1.00}	{0.75}	{1.00}	{1.00}
Group × X	-0.00	0.03*	0.02	0.02	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)
	[0.98]	[0.07]	[0.43]	[0.14]	[0.65]	[0.69]
	{1.00}	{0.73}	{1.00}	{0.73}	{1.00}	{1.00}
X	0.01	-0.01	-0.01	-0.01	-0.02*	-0.07**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
	[0.20]	[0.37]	[0.45]	[0.21]	[0.05]	[0.02]
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for tribe indicator). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.7: Other Heterogeneous Treatment Impacts on Household Income (Prespecified Village-Level, Economic)

Dimension of Heterogeneity X:	Average Income	Average Consumption	Share With Non-Agriculture Workers	Average Highest Education	Average Utility Quality	Major Tribe: Luhya
Information × X	1.77	0.36	7.03*	3.48	2.40	-8.84
	(3.83)	(3.67)	(3.76)	(3.21)	(3.60)	(7.26)
	[0.64]	[0.92]	[0.06]	[0.28]	[0.51]	[0.22]
	{1.00}	{1.00}	{0.59}	{0.87}	{1.00}	{0.87}
Mentor × X	0.59	4.16	4.00	-1.84	3.07	6.32
	(4.37)	(4.09)	(3.79)	(4.58)	(4.75)	(8.33)
	[0.89]	[0.31]	[0.29]	[0.69]	[0.52]	[0.45]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Group × X	-10.31*	-3.30	-7.96	2.25	0.59	-18.01*
	(5.70)	(5.85)	(7.46)	(5.45)	(4.90)	(9.90)
	[0.07]	[0.57]	[0.29]	[0.68]	[0.90]	[0.07]
	{0.27}	{1.00}	{0.62}	{1.00}	{1.00}	{0.27}
X	2.02	1.03	-1.77	5.42**	-0.01	18.90**
	(2.93)	(2.88)	(3.54)	(2.45)	(2.77)	(9.58)
	[0.49]	[0.72]	[0.62]	[0.03]	[1.00]	[0.05]
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Outcome is household income earned over the past month (control mean = 97 USD). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for tribe indicator). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.8: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Village-Level, Networks)

Dimension of Heterogeneity X:	Average Advice Connections	Average Financial Connections	Average Nairobi Connections	Village Trust Index	Village Sociality Index	Village Financial Reliance Index	Average Hidden Income
Information × X	-0.00	-0.00	-0.00	0.01	0.02	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.70]	[0.69]	[0.75]	[0.23]	[0.17]	[0.45]	[0.10]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Mentor × X	-0.01	-0.01	0.02	0.00	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.54]	[0.41]	[0.16]	[0.94]	[0.32]	[0.52]	[0.55]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Group × X	0.03*	0.01	0.00	0.02	0.02	0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
	[0.06]	[0.65]	[0.83]	[0.19]	[0.20]	[0.47]	[0.70]
	{0.64}	{0.89}	{0.90}	{0.68}	{0.68}	{0.89}	{0.89}
X	-0.00	0.00	0.01	-0.01	-0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.89]	[0.84]	[0.43]	[0.26]	[0.95]	[0.47]	[0.63]
Observations	15,468	15,468	15,468	15,468	15,468	15,468	8,377

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1. Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Outcomes in columns 1–3 are the village average of the number of origin social connections who could give advice on migration, the number of households in the village the respondent says they would ask to lend them 1,000 KSh, and the number of social connections in Nairobi. Outcomes in columns 4–6 are the share of the village reporting that people in the village trust each other, frequently socialize, and frequently borrow money from each other respectively. *Average Hidden Income* is the average difference between migrant reports and household reports of the migrant's income, for migrants in Nairobi. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.9: Other Heterogeneous Treatment Impacts on Household Income (Prespecified Village-Level, Networks)

Dimension of Heterogeneity X:	Average Advice Connections	Average Financial Connections	Average Nairobi Connections	Village Trust Index	Village Sociality Index	Village Financial Reliance Index	Average Hidden Income
Information × X	7.23*	3.03	4.89	8.10**	5.85	7.75**	0.55
	(3.91)	(3.85)	(3.91)	(3.87)	(4.30)	(3.76)	(4.99)
	[0.06]	[0.43]	[0.21]	[0.04]	[0.17]	[0.04]	[0.91]
	{0.16}	{0.40}	{0.20}	{0.16}	{0.20}	{0.16}	{0.42}
Mentor × X	2.88	-0.83	5.29	6.01	4.60	2.53	-6.01
	(3.88)	(4.41)	(4.11)	(4.12)	(4.50)	(4.02)	(4.79)
	[0.46]	[0.85]	[0.20]	[0.15]	[0.31]	[0.53]	[0.21]
	{0.97}	{0.97}	{0.97}	{0.97}	{0.97}	{0.97}	{0.97}
Group × X	0.35	2.79	-4.71	12.27**	11.38**	8.19	4.34
	(5.68)	(5.48)	(5.23)	(4.85)	(5.12)	(5.03)	(9.21)
	[0.95]	[0.61]	[0.37]	[0.01]	[0.03]	[0.10]	[0.64]
	{1.00}	{0.74}	{0.58}	{0.09}	{0.09}	{0.21}	{0.74}
X	-1.99	-4.89	1.86	-5.75*	-3.77	-3.07	4.38
	(3.01)	(3.23)	(2.93)	(3.45)	(3.94)	(3.65)	(4.02)
	[0.51]	[0.13]	[0.53]	[0.10]	[0.34]	[0.40]	[0.28]
Observations	15,468	15,468	15,468	15,468	15,468	15,468	8,377

Outcome is household income earned over the past month (control mean = 97 USD). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for tribe indicator). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Outcomes in columns 1–3 are the village average of the number of origin social connections who could give advice on migration, the number of households in the village the respondent says they would ask to lend them 1,000 KSh, and the number of social connections in Nairobi. Outcomes in columns 4–6 are the share of the village reporting that people in the village trust each other, frequently socialize, and frequently borrow money from each other respectively. *Average Hidden Income* is the average difference between migrant reports and household reports of the migrant's income, for migrants in Nairobi. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.10: Other Heterogeneous Treatment Impacts on Migration to Nairobi (Prespecified Village-Level, Migration)

Dimension of Heterogeneity X:	Share With Migrants in Nairobi	Share With Migrants Ever in Nairobi	Share With Migrants in Any City	Share With Migrants Ever in Any City	Average Perceived Nairobi Income
Information × X	0.01	0.00	-0.00	0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
	[0.26]	[0.73]	[0.28]	[0.56]	[0.31]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Mentor × X	0.01	0.01	0.00	0.01	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
	[0.46]	[0.44]	[0.73]	[0.54]	[0.67]
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}
Group × X	-0.01	-0.01	-0.01	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.23]	[0.24]	[0.42]	[0.19]	[0.38]
	{0.67}	{0.67}	{0.67}	{0.67}	{0.67}
X	0.05***	0.04***	0.01*	0.03***	0.01
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.05]	[0.00]	[0.42]
Observations	15,468	15,468	15,468	15,468	15,468

Outcome is an indicator for whether the household sent any migrants to Nairobi as of the endline survey (control mean = 0.19). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1. Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.11: Other Heterogeneous Treatment Impacts on Household Income (Prespecified Village-Level, Migration)

Dimension of Heterogeneity X:	Share With Migrants in Nairobi	Share With Migrants Ever in Nairobi	Share With Migrants in Any City	Share With Migrants Ever in Any City	Average Perceived Nairobi Income
Information × X	1.84	0.35	-0.25	0.19	9.50**
	(3.69)	(3.74)	(1.87)	(3.86)	(3.94)
	[0.62]	[0.93]	[0.89]	[0.96]	[0.02]
	{1.00}	{1.00}	{1.00}	{1.00}	{0.09}
Mentor × X	5.15	2.82	4.19	0.69	-3.93
	(3.72)	(3.90)	(2.62)	(3.88)	(3.76)
	[0.17]	[0.47]	[0.11]	[0.86]	[0.30]
	{0.71}	{0.71}	{0.71}	{0.98}	{0.71}
Group × X	-5.21	-8.45*	-5.30**	-5.60	5.02
	(5.20)	(4.83)	(2.49)	(5.11)	(5.80)
	[0.32]	[0.08]	[0.03]	[0.27]	[0.39]
	{0.30}	{0.20}	{0.20}	{0.30}	{0.30}
X	4.38	2.93	1.43	2.64	1.76
	(3.29)	(3.54)	(1.93)	(3.47)	(2.78)
	[0.18]	[0.41]	[0.46]	[0.45]	[0.53]
Observations	15,468	15,468	15,468	15,468	15,468

Outcome is household income earned over the past month (control mean = 97 USD). Each column shows heterogeneous treatment impacts by village-level variables (measured prior to treatment). Variables used for heterogeneity are standardized to mean 0, standard deviation 1 (except for tribe indicator). Base treatment variables are controlled but not shown in output. Linear regression is used for all models. Standard errors clustered at the village-level; two-sided p-values in brackets. Sharpened q-values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E.12: Kolmogorov–Smirnov Tests for Primary Outcomes and Welfare Measures

	Control vs Info.		Control vs Mentor		Control vs Group		Info. vs Mentor		Info. vs Group		Mentor vs Group	
	KS Stat	*p*-Value	KS Stat	*p*-Value	KS Stat	*p*-Value	KS Stat	*p*-Value	KS Stat	*p*-Value	KS Stat	*p*-Value
Any Migrants in Nairobi	.076	0	.091	0	.126	0	.042	.001	.088	0	.088	0
# Migrants in Nairobi	.081	0	.083	0	.113	0	.034	.009	.087	0	.082	0
Income	.024	.152	.042	.005	.026	.37	.023	.161	.019	.657	.039	.048
Welfare Index	.011	.94	.038	.014	.018	.81	.039	.002	.023	.44	.041	.032
Yearly Income	.038	.003	.067	0	.038	.057	.034	.009	.024	.346	.039	.045
Income + Crop Profit	.025	.109	.041	.007	.03	.215	.027	.063	.017	.782	.028	.301
Real Income	.021	.271	.041	.006	.027	.351	.026	.08	.02	.626	.04	.039
Amenity-Adjusted Income	.022	.211	.05	0	.042	.027	.033	.015	.028	.21	.026	.402
Consumption	.016	.65	.05	0	.048	.008	.037	.005	.038	.032	.016	.909
Saving	.028	.065	.079	0	.063	0	.062	0	.037	.038	.064	0
Yearly Investment	.028	.067	.049	.001	.15	0	.046	0	.153	0	.112	0
Access to Improved Utilities	.032	.023	.042	.005	.027	.329	.024	.137	.022	.462	.031	.198
Subjective Financial Health	.02	.369	.043	.004	.038	.07	.03	.035	.04	.022	.038	.062
Subjective Well-Being	.038	.003	.053	0	.08	0	.062	0	.06	0	.105	0

This table reports Kolmogorov–Smirnov test statistics and p-values comparing the distributions of residualized outcomes across each pair of treatment arms.