

Reaching the Novice or Nudging the Expert?

Networks, Information, and the Experimental

Returns to Migration

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Abstract

Large differences between rural and urban incomes prevail in almost every low-income country, but it is unclear why these gaps persist and what policies can help migrants take advantage of them. Using novel data from over 50,000 Kenyan households, we find that villagers underestimate earnings in the capital city by 30–60% on average. Providing unbiased information increases migration and improves economic outcomes, with greater impacts among households with no past migration history. Providing rural households with new social network connections in the capital amplifies economic benefits, while facilitating connections at the origin worsens economic outcomes as households with more migration experience crowd out higher-return, inexperienced ones. We rationalize these findings using a general-equilibrium Roy model, and find that the conditions generating high returns for inexperienced migrants are common in our setting. Estimating the model implies that removing information frictions in Kenya would increase the migration rate from 17 to 22% and reduce the rural-urban income gap by 25%. These findings imply that in many settings, the value of migration interventions may depend on whether they can reach less-experienced, novice migrants.

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1 Introduction

Deciding whether to migrate is among the most consequential decisions a person can make, with major implications for income (Bryan et al., 2014), access to modern amenities (Gollin et al., 2021), and the family unit (Yang, 2011, Bertoli et al., 2023). At the same time, a growing literature finds that migration decisions are often made with little or biased information (McKenzie et al., 2013, Shrestha, 2020, Bazzi et al., 2021, Baseler, 2023). When experimenting with migration is costly, prospective migrants may rely on secondhand information from their social networks to learn about potential destinations. Relying on existing networks, however, may shut out poorly connected workers from migration opportunities (Munshi, 2011, Kelley et al., 2023), and even well-connected networks can transmit biased information about migrants’ earnings due in part to income-sharing requirements (Ambler, 2015, Ashraf et al., 2015, Batista and Narciso, 2016, Seshan and Zubrickas, 2017, Joseph et al., 2018, Baseler, 2023). Given the substantial spatial income gaps present throughout nearly all low-income countries, the question of whether low-quality information hampers internal migration—and if so, what policies or programs can improve it—has major implications for aggregate economic outcomes (Gollin et al., 2014, Lagakos, 2020).

This paper studies the role of information and social networks in affecting migration outcomes using a cluster-randomized controlled trial in a population-representative sample of rural households across five Kenyan counties. We first collected data on beliefs about typical earnings in the most popular destination for internal migrants—the capital city, Nairobi—from over 50,000 rural households. We use these data to characterize the state of information gaps in this population, which we find to be large and pervasive. Next, we randomly assigned about 17,000 households to one of five treatment conditions or to a control. In villages assigned to an *Information* arm, we offer correct information about Nairobi during household visits. In *Group* villages, households received the same information conveyed in the Information arm, but delivered through a village-level meeting in which former migrants were encouraged to answer questions from prospective migrants. In *Mentor* villages, households again received the same information as in the Information arm, plus an offer to be matched to a resident of Nairobi who agreed to provide individualized information about migrating over the phone and later meet the migrant in person in Nairobi. To study how new information disseminates and to estimate spillover impacts, we randomly assigned a set of villages to a *Spillover* arm, in which some households were randomly assigned to receive the same information as households in Information villages, while other households were not given the information. Finally, we included a set of *Pure Control* villages in which households did not receive any information or network intervention.

Providing correct information about earnings in Nairobi immediately increases beliefs about the returns to migrating and aspirations to migrate, and facilitating network connections either through the Group or Mentor treatment roughly doubles these impacts. Over the following 16 months, 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 7–10% one year after treatment, an effect robust to adjusting for spatial differences in prices and non-pecuniary amenities. However, despite inducing a similar change in migration to the other treatment arms, the Group treatment creates no measurable economic gains on average: impacts across a broad set of measures are slightly negative and not statistically distinguishable from zero.

Why does disseminating information in groups worsen migration outcomes compared to individualized treatment? While all treatments increased migration, Information and, to a greater extent, Mentor, increased migration among households with little prior migration history: at the time of treatment, these households were significantly less likely to have current or former migrants in Nairobi or to be planning on sending future migrants to Nairobi compared to the control group. In contrast, migrant selection in Group villages looks broadly similar to the control group. In this setting, households with no migration history to Nairobi exhibit positive returns to marginal migration, while those with past migration experience exhibit close to zero marginal returns.¹ We rationalize these patterns with a Roy model in which rural households with no past migration history face higher costs of migrating, and thus exhibit greater benefits from interventions that reduce migration costs.

To understand why facilitating social connections at the origin did not lead inexperienced households to migrate, we analyze program data on households’ behavior during the interventions and their social interactions afterward, a measure of treatment intensity. These data show that group dissemination favored experienced migrants—generating adverse selection into treatment intensity—in two ways. First, during group meetings, inexperienced households were much less likely to be listed by our staff as 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 are highly responsive to the presence of other active, experienced households in group meetings, while the decisions of inexperienced households are not. Together, these findings suggest that the information relevant to inexperienced households is distinct from what is

¹Following the literature, we use the term *marginal migration* to refer to migration induced by a technology or policy intervention that reduces migration costs.

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. We find no significant impacts on co-migration in Mentor villages, suggesting that the mentor connection substituted for this support.

To identify the spillover effects of providing correct information onto untreated households, we compare untreated households in Spillover villages to those in Pure Control villages. We find no significant diffusion of information: beliefs, aspirations, and migration are unaffected for the neighbors of treated households. We find that households with more borrowing relationships (but not other relationships) update their beliefs less when their neighbors are treated, suggesting that households are strategically withholding information. These incentives can help explain the high value of new destination-based connections, who are outside of rural households' risk-sharing networks and can share information without creating an expectation of future transfers.

In spite of the lack of spillover effects on migration, we find positive economic spillovers: neighbors of treated households report better economic well-being one year after the intervention. These gains appear to be driven by increased profits of rural businesses and greater rural labor earnings, consistent with general equilibrium impacts of migration through demand multiplier effects (Egger et al., 2022) or through rural labor markets (Akram et al., 2017). Combining income impact estimates from Information and Spillover villages allows us to identify the income gain from migrating, which we estimate to be \$130/month. This estimate is in line with the typical income differences between Nairobi and rural Kenya and implies that the direct returns to migrating represent 37%, and spillover benefits 63%, of total income gains in the village.

To rationalize the potentially surprising contrast between our Group treatment and our other arms, and to predict the aggregate effects of reducing information frictions at scale, we build and estimate a general-equilibrium model of migration under imperfect information. Our model helps formalize the intuition that rural households with less migration experience—whom we assume face higher costs of migrating—exhibit greater returns to marginal migration in equilibrium. In our model, we show that greater returns to inexperienced migration arise if and only if migrants are positively selected from the sending population. Because migrant selection can generally be characterized with observational data, this result can be used to inform the design of migration interventions in new settings—in particular, whether they should target experienced or inexperienced households.

To validate the ability of the model to match our experimental findings, we simulate the experimental treatments in the model in partial equilibrium. We find that it is successful at reproducing the heterogeneous migration and income effects across treatment arms qualitatively and, to a lesser degree, quantitatively. Next, we use the model to assess the macroeconomic importance of information frictions by conducting a universal information treatment in general equilibrium. The model suggests that removing information frictions entirely could be transformative for rural Kenya: it predicts that the migration rate would increase from 17% to 22%, the agricultural productivity gap would fall from 2.7 to 2.4, and the rural-urban income gap would fall from 5.2 to 4.1, even in the absence of any changes to the monetary costs of migrating.

Overall, our findings imply that a subset of non-migrating households experiences substantial returns from lowering migration costs through better information or new social network connections. Policies or programs that improve access to information about urban markets, 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, migration responses and economic returns are highly heterogeneous in our setting, and the adverse selection problem induced by group dissemination implies that making information widely available may not improve outcomes when enthusiastic but low-return households dominate discussions. More targeted or individualized policies, while more costly, appear more effective at encouraging high-return migration.

Related Literature. This paper contributes to the study of rural-urban income gaps and the barriers that may prevent workers from migrating to locations with better economic opportunities.² A large literature has identified frictions distorting internal migration decisions, including poor information (Baseler, 2023, Boudreau et al., 2024, Frohnweiler 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). In contrast, several population-wide, non-experimental studies find evidence in favor of efficient spatial sorting (Young, 2013, Alvarez, 2020, Hamory et al., 2021). We make progress toward reconciling these two strands of literature by experimentally testing for internal migration barriers in a large-scale, population-representative sample. We argue that our findings are intermediate between these two strands: while the share of the population we identify as constrained is lower than in many smaller experimental studies, this sub-population exhibits substantial returns to mi-

²See Lagakos (2020) for a review of this literature.

grating, allowing us to reject the efficiency hypothesis.³ This interpretation is consistent with the macro-development literature finding that sorting explains a significant share—but not the entirety—of observed productivity gaps (Lagakos and Waugh, 2013, Gollin et al., 2014, Herrendorf and Schoellman, 2018, Bryan and Morten, 2019). Additionally, our finding that inducing more “intensive margin” migration among households with a past migration history yields lower returns than “extensive margin” migration among households with no migration history is consistent with higher experimental, compared to observational, returns to migrating (Lagakos et al., 2020). This finding has important implications for targeting: when the highest marginal returns come from less-experienced households, interventions should be designed to reach them.

We also contribute to the literature studying the role of information and social networks in influencing occupational outcomes in low-income countries. High unemployment, especially among youth, prevails throughout sub-Saharan Africa (Alfonsi et al., 2020, Bandiera et al., 2023), partly due to high search costs (Franklin, 2018, Abebe et al., 2020, Banerjee and Sequeira, 2023). Search costs are likely to be especially high for rural-urban migrants, who are typically less experienced with urban labor markets and have fewer urban social connections compared to urban-born residents. Larger social networks can facilitate migration, possibly by providing information and support to new migrants (Munshi, 2003, 2020, Blumenstock et al., 2023, Baseler, 2025), but relying on social networks may exclude individuals or groups with limited urban connections from migrating. We add to this literature by testing two programs designed to expand social networks either at the origin or destination. Our findings are consistent with high returns to light-touch, structured interactions between urban and rural residents, and suggest that the value of social connections operates in part through influencing *who* migrates.

Our finding that group-based information delivery worsened outcomes compared to individual delivery relates to the literature on social learning and the role of information seeding in affecting the diffusion and economic impacts of new information (Conley and Udry, 2010, Banerjee et al., 2013, Cai et al., 2015, Miller and Mobarak, 2015, Beaman et al., 2021). This literature, which has mainly focused on adoption decisions, has shown that social learning is a key determinant of technology adoption, that the characteristics of initial adopters matter for diffusion, and that widespread dissemination can slow diffusion (Banerjee et al., 2023). We add to this literature by showing that group-level dissemination can introduce an ad-

³Other studies have attempted to reduce migration 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. Shrestha (2020) finds that information about wages and mortality risk affect migration decisions, but does not measure economic returns to marginal migrants.

verse selection problem when high-knowledge non-adopters have low returns to intensifying adopting in equilibrium. Our findings also shed light on the social learning failures inherent in existing networks. We show that households have limited information about the migration decisions of their neighbors, that low-experience households have especially weak networks they could rely on for migration information and support, and that across-household learning is hampered by informal insurance networks. Importantly, both individually delivered information and cross-location links appear to help substitute for the role of existing networks for households inexperienced with migrating.

Finally, we make two methodological contributions 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 change often requires estimating shadow prices that are imperfectly measured by markets, such as the value of non-pecuniary amenities, which often differ greatly between urban and rural areas (Gollin et al., 2021). Similarly, valuing the returns to migrating requires accounting for price differences between the origin and destination, but consumer price indices are typically measured at the country level despite substantial within-country variation, and differences in the quality of goods and services available in rural and urban areas complicates measurement further. We develop simple, survey-based approaches to valuing amenity and price differences across space, and validate these using our survey data. Our amenity-adjustment method can be implemented by any researchers collecting primary data, as it does not require additional data beyond household surveys. Our price-adjustment method can be applied in any setting where item-level consumption data are available in each study location.

2 Study Design

This section describes the setting of our study, our sampling procedure, data collection timeline, experimental interventions, and estimating approach.

2.1 Setting and Geographic Scope

Kenya is a lower-middle income country in East Africa. Like most low and lower-middle income countries, its population is predominantly rural and engaged in smallholder agricultural production. Also like most predominantly rural countries, Kenya is urbanizing rapidly, changing from 16% urban in 1980 to 30% urban in 2023. Urbanization proceeds in part through rural-to-urban migration, as higher-paying job opportunities in cities increasingly attract rural residents. 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 along several dimensions, as shown in Appendix Table A.1, which presents population-weighted county-level medians for the five selected counties and for the entire rural population. For each variable spanning age composition, educational attainment, religious affiliation, income, population density, distance to Nairobi, and migration rates, our project counties lie within the middle 60% of the national county-level distribution. They are further from Nairobi than most counties (73rd percentile), poorer than most counties (26th percentile of income) and migration is more common among those born in these five counties (76th percentile). These five counties are also large, with their rural areas together representing four million people, or about 15% of the country’s rural population.

2.2 Sample Selection

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

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 each study county, after excluding sub-locations in the bottom 5% or top 10% of the county-specific population density distribution (population per square kilometer).⁴
2. *Village Selection.* We randomly selected one enumeration area (that is, village) 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.⁵
3. *Household Selection.* In each sampled village, we censused the full population of households residing there with the assistance of village leaders. Comparing our census data to population records maintained by the KNBS allows us to assess the completeness

⁴We excluded the 10% highest-density sub-locations to avoid urban areas. We excluded the bottom 5% to reduce the cost of data collection, as some sub-locations are very sparse.

⁵Only 3% of Pure Control villages are within 1km of a treatment village, and the share of villages within 3km or 10km that are treated is not correlated with beliefs, migration, or economic outcomes in the Pure Control group (see Appendix Table A.8).

of the 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.⁶ 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 this 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 were delivered in conjunction with the baseline survey. 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. 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 do not change our main results, as shown in Appendix Table B.5. Survey completion rates and tests of differential attrition are presented in Section 2.7.

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. 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.

⁶The across-village correlation coefficient between our household count and KNBS’s is 0.67.

2.4 Summary Statistics

Appendix Figure A.2 shows summary statistics by migration status for individuals ages 18–59 who are current or former household members in the sample villages of this study, estimated using baseline data.⁷ 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 descriptives on migration trips for migrants leaving for cities after the baseline survey, measured at endline. For one-third of trips, the migrant had returned to their home village by the time of the endline survey. The mean trip duration was 6.8 months. While most migrants leave alone, the majority leave after receiving a job referral from someone in their village. Other assistance, including housing assistance or borrowing money, is rarer. Most migrants find work as an employee, though a minority start businesses in the destination. It takes migrants 3 weeks on average to find a job after arriving. One-third are married, and among those, about two-thirds live with their spouse in the destination.

2.5 Treatments

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

1. In *Information* villages, each sampled household received a detailed 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 *Spillover* villages, we randomly selected two-thirds of sampled households to receive

⁷This study, which uses an origin-based sampling design, misses households that relocate entirely unless they did so after the baseline survey. As such, the sample should be viewed as a snapshot of all households—including current and former members—who were residing in the five study counties as of the baseline survey.

the information sheet and script, as described above. The remaining one-third was surveyed, but not given any information.

3. In *Group* villages, each sampled household was invited to a group presentation 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 *Mentor* villages, each sampled household received the same information sheet and script, plus an offer to be paired with an experienced resident in 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 wants 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.
5. In *Pure Control* villages, sampled households were surveyed, but do not receive any treatment.

Content of the Information Intervention. We gathered the information delivered through our interventions from the Kenya Integrated Household Budget Survey 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).⁸ 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.3. During the midline survey, we provided a reminder of key points of the information as well as an individualized

⁸Enumerators emphasized that migrants may earn different amounts than non-migrants, and that the information shared will not be correct for everyone given income variation. See Appendix B.3 for details on measurement.

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.

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 are frequently discussing 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).

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 Section 2.7.

Take-Up. Take-up of the information sheet and script among households assigned to any treatment condition was 100%: no household refused to hear the information or take the sheet. Attendance at the village-level meetings in villages assigned to Group was 88% of the invited sample.⁹ In Mentor villages, 471 households (or 13% of the sample) enrolled in the mentorship program, were matched to a Nairobi mentor, and were verified to have spoken with their mentor at least once by our program staff. Of these, 41 households physically met with their mentor in Nairobi while the rest had conversations over the phone, WhatsApp, or both.

Mentor Recruitment and Matching. Mentors were recruited from several Nairobi neighborhoods where migrants commonly live or work. During recruitment, we collected basic information on mentors to facilitate matching. When rural respondents enrolled in the mentor program, we elicited their preferences over possible mentor matches 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 rural households and mentors was done by program staff on a rolling basis. Match quality was high along several measures. Seventy-six percent of enrollees were matching to a mentor satisfying the

⁹Households 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.

attribute they deemed most important; among those whose mentors did not satisfy their most-important attribute, 88% satisfied their second-most-important attribute. The average matched guide had lived in Nairobi for 17 years and worked in their occupation for 7 years. Average travel time between enrollees’ and mentors’ preferred meeting locations was 6.6 minutes.

2.6 Estimating equations

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

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

where y_{ivt} is an outcome for family i in village v measured in survey round t , $\bar{y}_{iv,pre}$ is family 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.¹⁰ When estimating treatment impacts or population descriptives, we apply sampling weights to correct for non-random selection into the experimental sample, as described in Section 2.2.

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

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

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.2. We obtain these by permuting 2,000 treatment assignments following the true assignment method and retaining only balanced permutations.¹¹ We permute only those treatment assignments being compared in a given hypothesis, holding the others

¹⁰Relative 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 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.

¹¹Specifically, 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).

fixed. 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.¹² 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, supporting small saturation effects. 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).

2.7 Randomization Balance and Attrition

Appendix Table B.1 shows tests of randomization balance in the full sample. Across 31 pre-treatment characteristics, we reject the null hypothesis of equality across all treatment arms at the 10% level for three and at the 5% level for one, consistent with expectation under successful randomization.

Appendix 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

¹²In 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 Pure 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 4.6.

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.

Appendix 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, indicating no significantly differential attrition along these dimensions. For the endline and endline + indirect sample, the analogous statistics are 5 out of 62 tests rejected at the 10% level, again consistent with no significant attrition bias.

Appendix 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 192 tests (24 household-level variables, four sample types, and two weighting options) we reject the null hypothesis of equality across all treatment arms at the 10% level for 19, consistent with expectation under successful randomization.

3 The State of Information Gaps

We begin by characterizing rural households’ beliefs about typical incomes in the capital city, Nairobi. To do so, we rely on surveys of a representative sample of over 50,000 households in 5 counties. Each of these households was asked a series of questions to measure their perception about typical incomes in Nairobi as well as a reference area (towns in their home county). Each question was phrased about members of a given demographic group—defined by age, educational attainment, and gender—both to make the questions more concrete and to study how information gaps vary over groups. These data yield estimates of the perceived Nairobi income premium for group j , \hat{Y}_j^N/\hat{Y}_j^L , expressed as a multiple of perceived income for the same group in the local reference area. To assess information gaps, we compare these perceived premiums to the true premiums Y_j^N/Y_j^L estimated on KIHBS survey data.

We find that rural households substantially underestimate Nairobi incomes premiums for nearly every demographic group, as shown in Figure 1. Moreover, there is remarkably little variation in perceived premiums, but substantial variation in true premiums, across demographic groups and home counties. Across the 65 groups for which we collected data, perceived premiums vary from 1.25 to 1.60, with half of groups lying between 1.33 and 1.48.¹³ In contrast, true premiums vary from 1.19 to 3.07. True premiums are especially underestimated for older workers: for example, the average perceived premium for workers

¹³The majority of this variation is across home counties rather than across demographic groups within home counties. The R^2 from the regression of perceived premium on a home-county fixed effect is 0.87.

aged 40–49 is 1.4 compared to an average true premium of 2.7. The limited variation in perceived premiums is consistent with a heuristic in which income differences between groups are the same in Nairobi as in local towns.¹⁴ These findings suggest that workers whose traits are rewarded to a greater degree in the capital compared to their local town are especially likely to underestimate the returns to migrating: in Section 4.2, we show that migration responses were generally larger in these groups.

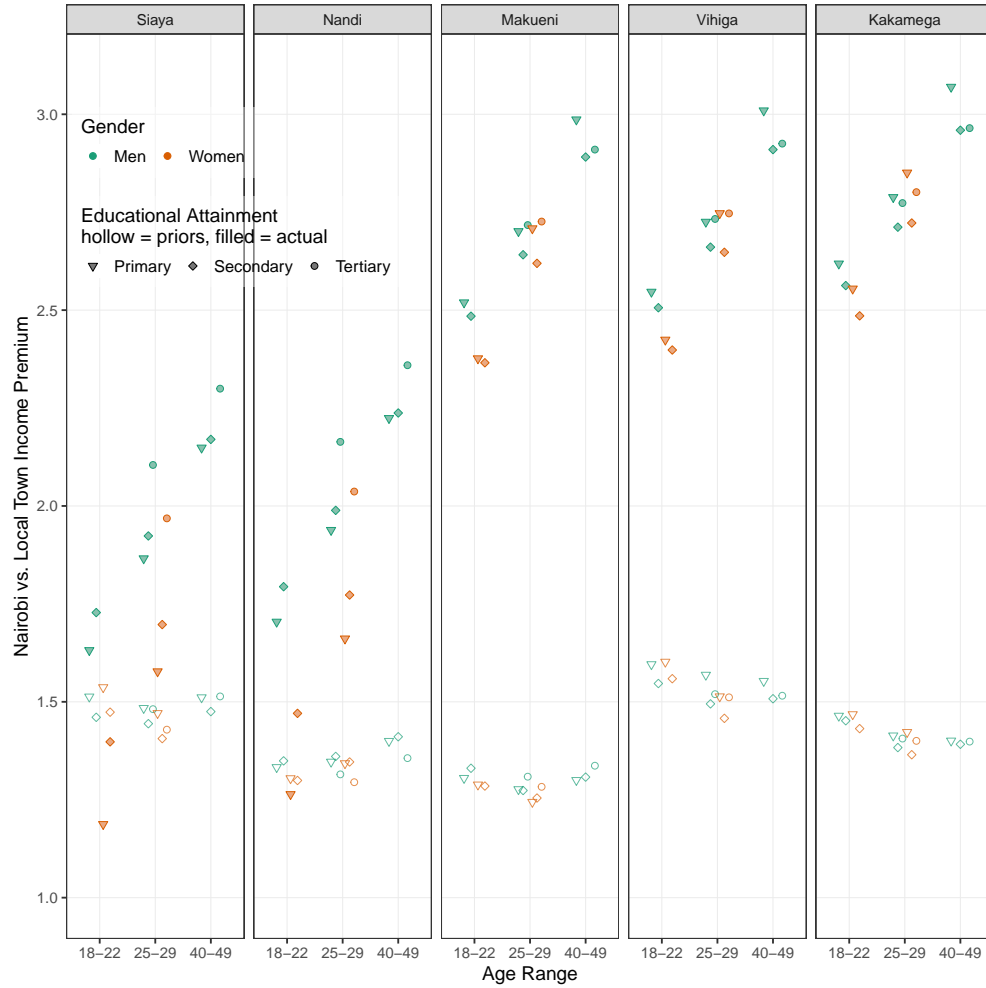
Table 1 shows summary statistics from census survey data on households’ migration experience, social networks, and migration aspirations. Over half of households have never had any current or former member working in Nairobi. Only about half of households know someone in Nairobi they could stay with or get job search assistance from. Perhaps surprisingly, households substantially under-estimate how many other households in their village are migrating: they believe 16% of households in their village have a member working in Nairobi, while the true share is 29% on average.¹⁵ This finding suggests that the within-family hidden-income incentives documented in Baseler (2023) may also operate across households in a village, possibly because across-household borrowing is common. In Section 4.6, we test this hypothesis more formally, and show that the diffusion of new information about the returns to migrating is reduced among households with a greater number of borrowing relationships within the village.

Households with past migration experience—that is, those with members who have worked in Nairobi at some point—are significantly better-connected and have higher migration aspirations, as shown in column 2 of Table 1. Their beliefs about the share of migrating households in the village are much more accurate, and they believe that both typical incomes and their own potential earnings in Nairobi are higher. They are much more likely to have social connections in Nairobi and to report that they could rely on those connections for support. They are also more socially central in their villages, reporting a significantly larger number of households who approach them for job advice. Later, we will use past migration experience as a proxy measure for idiosyncratic migration costs, and argue that effective migration interventions depend on encouraging migration for *inexperienced* households, who are more likely to be disadvantaged in existing migration networks.

¹⁴To see this, note that for any groups i and j , $Y_i^N/Y_j^N = Y_i^L/Y_j^L \iff Y_i^N/Y_i^L = Y_j^N/Y_j^L$.

¹⁵This difference is not due to varying conceptions of village boundaries: when eliciting these beliefs, we informed respondents about the number of households in their village, and we compute the perceived share by dividing the perceived number of migrating households by the total number of households.

Figure 1: True 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 true 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?”

Table 1: Households with migration experience are better-connected at the origin and destination and have higher migration aspirations.

| | Mean | Coeff. on Experience | <i>p</i> -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.

4 Experimental Impacts

In this section we describe the experimental impacts of relieving information constraints and facilitating social network connections for potential rural-urban migrants.

4.1 Impacts on Beliefs and Aspirations

The Information treatment significantly increases rural households' beliefs about their potential income from migrating and their migration plans, as shown in Table 2. Planned migration to Nairobi within the next year increases by 4 pp. (on a base of 27%, effect size = 15%, $p < 0.01$). The impact on planned migration to any city is similar at 5 pp. (on a base of 34%, $p < 0.01$), 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% ($p = 0.04$), and households increase their beliefs about the income growth they would experience over their first year in the city (by 32%, $p < 0.01$).

To assess impacts on other moments of the distribution of expected migration outcomes, we asked households how much they expect to earn after migrating under pessimistic and optimistic scenarios. While both beliefs are higher in the Information arm than the control group, impacts are relatively greater for pessimistic outcomes than optimistic ones (7% higher, $p < 0.01$ vs. 3% higher, $p = 0.18$, respectively), suggesting that new information affects perceived downside risk more than upside risk on average.

Table 2: Impacts on Beliefs

| | Plans to Migrate to Nairobi | Plans to Migrate to City | Expected Migration Income | Pessimistic Migration Income | Optimistic Migration Income | Expected Income Growth |
|-------------------------|-----------------------------------|--------------------------------|---------------------------------|------------------------------------|-----------------------------------|-------------------------------|
| <i>Disaggregated</i> | | | | | | |
| Info | 0.04*** (0.01) [0.00] | 0.05*** (0.01) [0.00] | 0.04** (0.02) [0.04] | 0.06*** (0.02) [0.00] | 0.03 (0.02) [0.14] | 29.43*** (9.88) [0.00] |
| Group | 0.09*** (0.02) [0.00] | 0.10*** (0.02) [0.00] | 0.11*** (0.03) [0.00] | 0.14*** (0.03) [0.00] | 0.07*** (0.03) [0.01] | 50.73*** (14.27) [0.00] |
| Mentor | 0.10*** (0.02) [0.00] | 0.11*** (0.02) [0.00] | 0.11*** (0.02) [0.00] | 0.14*** (0.03) [0.00] | 0.10*** (0.02) [0.00] | 6.52 (10.29) [0.53] |
| Model | OLS | OLS | Poisson | Poisson | Poisson | OLS |
| p -Val: Info.=Group | 0.01 | 0.01 | 0.00 | 0.00 | 0.07 | 0.13 |
| p -Val: Info.=Mentor | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.02 |
| p -Val: Group=Mentor | 0.93 | 0.84 | 0.78 | 0.86 | 0.34 | 0.00 |
| Control Mean | 0.24 | 0.30 | 131.21 | 93.37 | 184.89 | 90.40 |
| Observations | 15,976 | 15,976 | 15,348 | 15,342 | 15,344 | 15,268 |
| <i>Pooled Treatment</i> | | | | | | |
| Any Info | 0.06*** (0.01) [0.00] | 0.07*** (0.01) [0.00] | 0.07*** (0.02) [0.00] | 0.10*** (0.02) [0.00] | 0.06*** (0.02) [0.00] | 25.58*** (8.74) [0.00] |
| Observations | 15,976 | 15,976 | 15,348 | 15,342 | 15,344 | 15,268 |

Impacts are estimated on data from baseline surveys measured after treatment. Linear regression is used for outcomes with negative values or bounded between 0 and 1; poisson regression is used otherwise. *City* includes any urban area. *Expected Income Growth* is the difference in expected income after one year of living in the destination and immediately after arriving. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Facilitating network connections through the Group and Mentor treatments roughly doubles the impacts of information provision on beliefs and aspirations. Planned migration to Nairobi increases by 10 pp. in both treatment arms, again nearly identical to the impacts on planned migration to any city. Expected income immediately after migrating increases by

11–13%. Reflecting impacts from information alone, Group and Mentor treatments increase pessimistic expected migration income more than optimistic income (15–17% increase vs. 8–9% increase respectively, all p -values < 0.01).

4.2 Impacts on Migration

All treatments increase migration to the capital after 16 months, as shown in Table 3. Households in Information villages were 1 pp. more likely to send new migrants to Nairobi (we refer to migrants who left after the information intervention as “new”) on a control-group base of 13% ($p = 0.18$) and 2 pp. more likely to send any migrants to Nairobi (this broader definition includes household members living outside the village at the time of the intervention) on a control-group base of 19% ($p = 0.09$). Impacts on the number of migrants going to Nairobi are directionally similar but noisy. 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 Group villages are slightly larger compared to Information. In Group villages, households were 3 pp. more likely to send new migrants to Nairobi (23% effect size, $p = 0.02$) and 2 pp. more likely to send any migrants to Nairobi (11% effect size, $p = 0.10$). The impact on new migration to any city is 3 pp. ($p = 0.04$). This effect size is very similar to the impact on new migration to Nairobi, suggesting that the Group treatment induced net-new urban migration, consistent with impacts on aspirations shown in Table 2. The impact on the count of new migrants in Nairobi is 17% ($p = 0.05$).

Migration impacts in Mentor villages are similar to those in Group villages. These households were 2 pp. more likely to send new migrants to Nairobi ($p = 0.05$) and 2 pp. more likely to send any migrants to Nairobi ($p = 0.04$). Again, impacts on new migration to any city are similar (2 pp., $p = 0.08$), consistent with net new urban migration. In Mentor villages, the impact on the count of new migrants traveling to Nairobi is 14% ($p = 0.10$).

Given the generally 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, group, or mentor). The middle panel of Table 3 shows that the average effect of information is 2 pp. on both new and any migration to Nairobi (p -values = 0.04 and 0.03, respectively), 1 pp. on new migration to any city ($p = 0.21$), 9% on the count of new migrants to Nairobi ($p = 0.21$), and 5% on the count of any migrants to Nairobi ($p = 0.39$).

Table 3: Impacts on Migration

| | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-----------------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| <i>Disaggregated</i> | | | | | | |
| Info | 0.011 (0.008) [0.18] | 0.018* (0.010) [0.09] | 0.002 (0.011) [0.83] | 0.042 (0.075) [0.57] | 0.040 (0.065) [0.54] | -0.050 (0.061) [0.41] |
| Group | 0.026** (0.011) [0.02] | 0.022* (0.013) [0.10] | 0.034** (0.016) [0.04] | 0.170* (0.088) [0.05] | 0.114 (0.084) [0.17] | 0.121 (0.077) [0.12] |
| Mentor | 0.018** (0.009) [0.05] | 0.024** (0.012) [0.04] | 0.024* (0.014) [0.08] | 0.141 (0.087) [0.10] | 0.045 (0.073) [0.54] | 0.106 (0.069) [0.12] |
| Model | OLS | OLS | OLS | Poisson | Poisson | Poisson |
| p -Val: Info.=Group | 0.13 | 0.72 | 0.04 | 0.08 | 0.32 | 0.02 |
| p -Val: Info.=Mentor | 0.34 | 0.54 | 0.07 | 0.17 | 0.94 | 0.01 |
| p -Val: Group=Mentor | 0.48 | 0.86 | 0.56 | 0.73 | 0.40 | 0.85 |
| 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 |
| <i>Pooled Treatment</i> | | | | | | |
| Any Info | 0.015** (0.007) [0.04] | 0.020** (0.009) [0.03] | 0.013 (0.011) [0.21] | 0.089 (0.070) [0.21] | 0.052 (0.060) [0.39] | 0.021 (0.055) [0.71] |
| Observations | 15,468 | 15,468 | 15,468 | 15,468 | 15,468 | 15,468 |
| <i>Treatment Intensity</i> | | | | | | |
| Any Info \times Prior Gap | 0.035** (0.016) [0.03] | 0.024 (0.019) [0.20] | 0.017 (0.028) [0.54] | 0.089 (0.124) [0.47] | -0.057 (0.099) [0.57] | 0.002 (0.102) [0.99] |
| Any Info | 0.004 (0.009) [0.67] | 0.013 (0.011) [0.27] | 0.008 (0.014) [0.57] | 0.065 (0.077) [0.40] | 0.074 (0.070) [0.29] | 0.022 (0.064) [0.73] |
| Prior Gap | -0.051*** (0.014) [0.00] | -0.047*** (0.016) [0.00] | -0.053** (0.026) [0.04] | -0.285*** (0.104) [0.01] | -0.152* (0.084) [0.07] | -0.203** (0.089) [0.02] |
| 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. Standard errors clustered at the village-level; two-sided p -values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In the final panel of Table 3, we exploit the variation in treatment intensity induced by the differences between true and perceived Nairobi incomes shown in Figure 1. We include treatment intensity—the gap between the prior and the truth, re-centered at 0—as an interaction term in (1).¹⁶ We find that treatment does not affect migration when priors for the likely migrant are correct: the estimated coefficients on *Any Info* are close to zero and statistically insignificant. Migration responses are increasing in treatment intensity, although the difference is only statistically significant for the indicator of whether the household sent any new migrants to Nairobi (coeff. = 4 pp. per multiple of the prior belief, $p = 0.03$).

Comparing the results of Table 3 to those of Table 2 shows that 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 that households are over-optimistic about their own migration. Patterns in the control group support this explanation: 27% of control-group households expressed a plan to send a new migrant to Nairobi within one year, while only 13% actually did so.¹⁷ 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. While information and new social network connections can increase migration, a substantial gap between planned and actual migration remains.

4.3 Impacts on Economic Outcomes

While the Information, Group, and Mentor treatments have similar impacts on migration, their impacts on downstream economic outcomes differ markedly. The Mentor treatment is the most impactful across the board, increasing our pre-specified welfare index by 0.10 standard deviations ($p < 0.01$, false discovery rate q -value = 0.02). Household income earned in the month before the survey—inclusive of wage income and business profits across all family members including migrants—is higher by \$8 on a base of \$105 ($p = 0.06$, $q = 0.08$). 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 \$68 on a

¹⁶Specifically, we computed the predicted Nairobi income for the most likely person to migrate from the household, as reported at baseline, based on their demographic characteristics. This individualized statistic was shared with households during the midline survey. We divide the individualized predicted income by the average prior belief for that same demographic group to create a measure of treatment intensity *Prior Gap* and interact it with the pooled treatment variable *Any Info*. We de-mean *Prior Gap* so that the coefficient on *Any Info* is interpretable as the impact of treatment for those with correct priors.

¹⁷The coefficient from a bivariate regression of any new migration to Nairobi at endline on any planned migration to Nairobi at baseline is 4 pp. on a base of 12% ($p < 0.001$), indicating that while households are over-optimistic in their migration plans, planned and actual migration are nevertheless strongly correlated.

base of \$714 ($p = 0.06$; $q = 0.08$). Adding estimated crop profits to income does not change these results (coeff. = \$10 in the past month on a base of \$127, $p = 0.04$, $q = 0.08$).

If migrants face higher prices in Nairobi or other cities, impacts on nominal income will overstate impacts on real standards of living. A typical solution is to deflate nominal income by price indices 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.¹⁸ We apply urban price indices to income earned by urban migrants (net of remittances to the rural household). We find that treatment impacts on real income are similar to those on nominal income (\$9/month increase in Mentor villages, $p = 0.04$, $q = 0.08$).

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 nearly all 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. = \$8/month in Mentor villages, $p = 0.09$, $q = 0.10$). 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 \$7 ($p = 0.06$, $q = 0.20$) while income over the past year is higher by \$51 ($p = 0.09$, $q = 0.23$). The overall welfare index is higher by 0.04 standard deviations ($p = 0.20$; $q = 0.25$). However, economic impacts in Group villages are substantially lower than in either Information or Mentor villages. In Group villages, impacts on each income measure are directionally negative but statistically zero. We discuss the reasons for this surprising result in depth in Section 5.

¹⁸Our adjustment method is described in detail in Appendix C, and Appendix Table C.1 shows our estimated price indices. Treatment impact estimates on real income are similar in magnitude and remain statistically significant when using quality-unadjusted price measures.

Table 4: Economic Outcomes

| Index and Income: | Welfare Index | Income | Yearly Income | Income + Crop Profit | Real Income | Amenity-Adjusted Income |
|------------------------|---------------------------------------|---------------------------------------|---------------------------------------|---------------------------------------|--------------------------------------|-------------------------------------|
| Info | 0.04 (0.03) [0.20] {0.25} | 7.08* (3.73) [0.06] {0.20} | 51.00* (29.94) [0.09] {0.23} | 6.22 (4.17) [0.14] {0.23} | 7.53** (3.75) [0.04] {0.20} | 6.56 (4.11) [0.11] {0.23} |
| Group | -0.04 (0.04) [0.33] {1.00} | -4.51 (4.97) [0.36] {1.00} | 1.38 (44.60) [0.98] {1.00} | -2.43 (5.40) [0.65] {1.00} | -4.42 (5.00) [0.38] {1.00} | -0.47 (5.43) [0.93] {1.00} |
| Mentor | 0.10*** (0.03) [0.00] {0.02} | 8.09* (4.26) [0.06] {0.08} | 68.01* (35.90) [0.06] {0.08} | 10.00** (4.83) [0.04] {0.08} | 9.03** (4.29) [0.04] {0.08} | 8.13* (4.77) [0.09] {0.10} |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |
| p -Val: Info.=Group | 0.04 | 0.01 | 0.25 | 0.09 | 0.01 | 0.17 |
| p -Val: Info.=Mentor | 0.05 | 0.80 | 0.61 | 0.40 | 0.71 | 0.72 |
| p -Val: Group=Mentor | 0.00 | 0.02 | 0.16 | 0.03 | 0.01 | 0.13 |
| Control Mean | -0.03 | 99.70 | 690.69 | 120.95 | 99.54 | 120.21 |
| Observations | 15,468 | 15,468 | 15,467 | 15,468 | 15,468 | 15,468 |
| Other Financial: | Consumption | Savings | Yearly Investment | Access to Improved Utilities | Subjective Financial Health | Subjective Well-Being |
| Info | 0.04 (0.03) [0.26] {0.27} | 0.27*** (0.08) [0.00] {0.02} | -0.00 (0.09) [0.98] {0.49} | -0.00 (0.01) [0.53] {0.36} | 0.05* (0.03) [0.06] {0.20} | -0.02 (0.03) [0.61] {0.38} |
| Group | -0.01 (0.04) [0.79] {1.00} | 0.16 (0.12) [0.18] {1.00} | -0.18 (0.11) [0.11] {1.00} | -0.01 (0.01) [0.47] {1.00} | 0.00 (0.05) [0.97] {1.00} | -0.04 (0.04) [0.33] {1.00} |
| Mentor | 0.02 (0.03) [0.65] {0.31} | 0.34*** (0.10) [0.00] {0.01} | 0.04 (0.11) [0.70] {0.31} | 0.00 (0.01) [0.68] {0.31} | 0.08** (0.03) [0.01] {0.04} | 0.05 (0.04) [0.20] {0.12} |
| Model | Poisson | Poisson | Poisson | OLS | OLS | OLS |
| p -Val: Info.=Group | 0.21 | 0.30 | 0.08 | 0.80 | 0.26 | 0.53 |
| p -Val: Info.=Mentor | 0.46 | 0.36 | 0.64 | 0.29 | 0.25 | 0.05 |
| p -Val: Group=Mentor | 0.50 | 0.11 | 0.06 | 0.28 | 0.08 | 0.03 |
| Control Mean | 175.59 | 9.36 | 5.80 | 0.15 | -0.06 | -0.04 |
| Observations | 15,232 | 15,232 | 15,232 | 15,320 | 15,055 | 15,215 |

Impacts are estimated on data from endline surveys. 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. Sharpened q -values in curly brackets are estimated using the method of Anderson (2008) within each treatment group. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Other Economic Outcomes. We do not detect significant changes in consumption, investment in businesses and the household, or access to improved utilities. Impacts on subjective well-being are positive in the Mentor arm but statistically insignificant (0.05 sd increase, $p = 0.20$, $q = 0.12$). 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. ($p = 0.06$, $q = 0.20$). The larger impacts on savings compared to consumption 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).

4.4 Distinguishing Mentors' Treatment From Selection Effects

Treatment impacts on economic outcomes are generally more positive in the Mentor compared to the Information arm, and this difference is statistically significant for the overall welfare index (p -value on comparison = 0.05). Do better outcomes arise in Mentor because of a selection effect—mentors convince high-return people to migrate—or a treatment effect conditional on selection—for example, if mentors help migrants find jobs in the city? Our experimental design does not allow us to cleanly distinguish between these two explanations. Instead, we analyze whether treatment effects on our welfare index and on income are greater in Mentor compared to Information within the set of migrating households at endline. Assuming an absence of selection impacts, a higher average income among migrants in Mentor compared to Information would be consistent with positive treatment effects. To account for the possibility of selection, we also estimate results controlling for baseline characteristics, choosing controls using lasso from the set of variables shown in Appendix Table A.11.

Our results are consistent with positive treatment effects of mentors, as shown in Appendix Table A.5. However, our statistical power is limited in this smaller sample, and while differences are economically meaningful they are not statistically significant. Within the set of households sending migrants to Nairobi, our welfare index is higher in Mentor villages compared to Information (0.09 vs. 0.02, $p = 0.41$), as is income (\$12/month vs \$3/month, $p = 0.40$). Controlling for characteristics slightly increases these differences, to 0.12 vs 0.03 in our welfare index ($p = 0.20$) and to \$16/month vs. \$5/month ($p = 0.29$) in income. If mentors had improved economic outcomes by persuading higher-value types to migrate—but had not had any direct impact on migrants' incomes—we would expect the differences shown in Appendix Table A.5 to be attenuated once baseline characteristics are included. Our findings are thus most consistent with mentor impacts driven through treatment effects

as opposed to selection effects, although our statistical power is too limited to make a definitive conclusion. Positive treatment effects are also consistent with the most common topic of discussion between mentors and prospective migrants (see Appendix Figure A.4): specific job leads.

4.5 Treatment Effect Heterogeneity

Appendix Table A.6 estimates heterogeneous treatment impacts on migration to Nairobi. For both measures of prior migration experience—whether the household had a member working in Nairobi as of the census survey, or at any point beforehand—the Information and Mentor treatments are significantly more impactful among inexperienced households (impacts on experienced household are close to zero). Mentor is also significantly more likely to increase migration among households that were *not* planning on migrating prior to treatment. Information and Mentor are both significantly more impactful among households with few social connections in the village at baseline, suggesting that these treatments are substitutes for existing origin social capital. Finally, all treatments are more impactful among poorer and less-educated households, though the differences are greatest for Mentor. Together, these patterns point to the Information, and to a greater degree the Mentor, treatments inducing more migration among households likely to face high migration costs: those that are inexperienced with migrating, poorly connected at the origin, poorer, and less well-educated.

Appendix Table A.7 estimates heterogeneous treatment impacts on income earned in Nairobi (heterogeneity in total income is less informative of differential returns to migrating in the presence of within-village spillovers, which we discuss in Section 4.6). These results show that impacts on Nairobi income are generally smaller for “low-cost” households: those with more migration experience, income, or education. As these differences are generally similar across treatment groups, we also present pooled impacts to improve statistical power. In these pooled specifications, income effects are significantly greater for households with no existing Nairobi worker ($p = 0.05$) or which have never had a worker in Nairobi ($p = 0.05$). These findings are consistent with higher marginal returns to inexperienced households—whose migration decisions were more impacted by Information and Mentor than by Group—a hypothesis we return to in Section 5.

4.6 Spillovers and Information Diffusion Through Networks

To measure spillover effects of providing information within villages, 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 Pure 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 the share of households sending new migrants, or any migrants, to Nairobi (coeffs. = 0 pp., p -vals = 0.72 and 0.79 respectively). We also see no evidence that migrant selection along observables changed in untreated households in Spillover villages, as shown in Appendix Table A.11: no selection coefficient is significant at the 5% level, and only one out of 14 is significant at the 10% level, consistent with expectation under no changes in selection.

Despite the lack of migration spillover impacts, we find large, positive spillover impacts on economic outcomes. Our pre-specified overall welfare index is higher by 0.13 sd ($p = 0.01$) among untreated households in Spillover villages. Income earned in the past month is also higher, though the difference is not statistically significant (coeff. = \$5/month higher on a base of \$117, effect size = 4%, $p = 0.40$).

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 significant positive spillovers onto business profits (37% increase, $p = 0.03$) and labor income earned in the village (14% increase, $p < 0.01$). While we find increases in labor supply at household businesses in the Information and Mentor arms, there is no change in labor supply for untreated households in Spillover villages. We find no spillover impacts on labor supply at wage jobs or the location of work, measured by commute time. The lack of spillovers on working hours, and relatively low direct impacts on migration, are somewhat 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).

Why Didn’t Information Diffuse Within Villages? The lack of changes in beliefs and migration among the neighbors of treated households in Spillover villages is potentially surprising given the ease of sharing the information contained in the intervention: a treated household could simply tell their neighbor what they learned or show them the information sheet. 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

¹⁹However, given the modest impact of Information on beliefs about potential migration income shown in Table 2, we cannot rule out partial diffusion of the new information.

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 the experienced households who already know the information: we return to a discussion of co-migration in Section 5.3. The perception gap documented in Table 1 suggests that a considerable share of households are hiding even the presence of their workers in Nairobi.

To test whether informal financial relationships can help explain the lack of information diffusion, we estimate heterogeneity in spillover impacts based on households’ financial relationships in the village. Specifically, in the sample of Pure Control villages and untreated households in Spillover villages, we interact a dummy for residing in a Spillover village with three social network measures: the number of households in the village the respondent lists as households they would approach to borrow money, the number they would approach for advice on migration, and the number they would approach for help on their farm. Results are shown in Appendix Table A.10. 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 (9% lower per connection, $p = 0.04$). We see no significant differences for migration advice connections or farm help connections, and results are similar when estimating all three interaction terms in a single regression. These findings point to across-household hidden income incentives as one factor maintaining low beliefs about urban incomes.

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, we compute the share of nearby villages (within 3km or 10km) that were assigned to any treatment arm other than Pure 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.

4.7 Estimating the Returns to Migrating

The positive economic spillover impacts documented above imply that estimating the return to migrating by instrumenting migration with treatment assignment would yield a biased result. However, we can estimate the marginal return to migrating for compliers under weaker assumptions by combining estimates from Information and Spillover villages. Because untreated households in Spillover villages were no more likely to migrate than Pure Control

households (and we do not observe any change in the selection of migrants), we can subtract the average economic impacts among untreated, Spillover households from those in Information villages to obtain a spillover-free intent-to-treat impact on income. Under the exclusion restriction that treatment assignment affects income only through migration or through other households' migration, dividing this modified intent-to-treat estimate by the first-stage impact on migration gives the direct income gain from migrating. This procedure, which we formalize in Appendix C.5, gives an estimated marginal return to migrating of \$130/month. This estimate is in line with the typical income differences between Nairobi and rural Kenya (see Appendix Figure B.1), and implies that—averaging over an entire village—direct returns to migrating represent 37%, and indirect returns 63%, of total income gains.

5 Why Did Group Treatment Worsen Migration Outcomes?

In this section, we discuss why Group treatment created no measurable economic benefits despite increasing migration at least as much as the Information and Mentor arms, which each led to positive economic gains. In short, we show that Group dissemination was more favorable to experienced migrants, who—under the right conditions—exhibit lower marginal returns to migrating because their migration costs are lower. In Section 6, we formalize this intuition and provide a framework for assessing when the marginal returns to migrating are higher for inexperienced households.

We first show that the types of migrants influenced by the Information and Mentor treatments were significantly less experienced than those in the control group, while those influenced by Group look more similar to control-group migrants. We then turn to program and survey data to investigate why low-experience households did not respond to the Group treatment, and find that they were crowded out by experienced households in two ways: they were less engaged during treatment, and less likely to co-migrate with others in their village after treatment.

5.1 The Information and Mentor Arms Had a Greater Impact on Inexperienced Households

Appendix Table A.11 shows estimates of (1) on the sample of households that sent migrants to Nairobi as of the endline survey and baseline variables on the left-hand side, allowing us to test whether treatment changed selection into migrating. Compared to control-group migrants, the Mentor treatment induces poor, inexperienced migrants who were not planning

to migrate at the time of the baseline survey. The impacts of the Information arm on migrant selection are similar, but muted. In contrast, Group induces few changes in selection relative to the control group. One exception is social connectedness at the origin: Information, but especially Mentor and Group, induce migration among households who report fewer connections at the origin they could use for migration advice, suggesting that the treatments substituted for this advice. These patterns help explain why the Information and Mentor arms brought about better economic outcomes than Group: as shown in Appendix Table A.7, it is exactly these inexperienced migrants who exhibit the greatest marginal returns.

5.2 Experienced Households Crowd Out Inexperienced Households During Group Meetings

Why did village-level meetings in the Group arm lead relatively more experienced migrants to move? A potential explanation is that people with less experience learned less from the Group treatment than the 1-on-1 treatments (Information and Mentor), possibly because more knowledgeable households dominated the group discussion. Such an explanation would be consistent with the image-concerns mechanism of Banerjee et al. (2023), which reduced learning when information was broadcast compared to when it was seeded.²⁰

To test this hypothesis, we leverage program data collected during group meetings and 1-on-1 information treatments to measure engagement during treatment depending on initial migration experience. To measure household engagement in the group meetings, facilitating staff recorded the households IDs of up to three of the most active participants in the discussion. We match these IDs to our household survey data to measure the characteristics of engaged households. To measure engagement during Information and Mentor treatments, we use 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 16%, $p < 0.01$) and who plan to migrate to Nairobi (by 9%, $p < 0.01$) after hearing the information.

Appendix Table A.12 presents regressions of engagement on several pre-treatment house-

²⁰It 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.15 is consistent with this: higher income strongly predicts longer intended duration in Nairobi (coeff. = 1.4 years per standard-deviation increase in income, $p < 0.01$).

hold characteristics. In Information and Mentor villages, households planning to migrate were slightly more engaged (facilitators spent 3% longer on treatment, $p = 0.05$). Households who already had migrants living in Nairobi were slightly less engaged (2% shorter treatment, $p = 0.05$), perhaps because they felt they already knew the information. We find very similar levels of engagement across education, income, and the number of social connections in Nairobi. While those planning to migrate were also more likely to be engaged in the Group treatment, engagement patterns are starkly different along migration experience. While those with current migrants in Nairobi were less engaged in Information and Mentor, they were *more* engaged in Group (3 pp. more likely to be among the most active participants on a base of 9%, effect size = 33%, $p = 0.05$). We find similar differences in Group for households with any past experience in Nairobi, more education, more income, and more social connections in Nairobi. These results suggest that less-experienced households—who 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.

Even if more experienced households were more engaged in group discussions, it may be that inexperienced households nevertheless benefited from hearing advice from knowledgeable former migrants. To test whether group discussions dominated by experienced households affected the migration decisions of experienced and inexperienced households differently, we examine heterogeneous effects of the Group treatment based on meeting characteristics, which we also interact with household characteristics. Because who is active during meetings is endogenous, these results should not be interpreted as causal. Appendix Table A.13 presents regression results with an indicator for sending new migrants to Nairobi as the outcome variable. Each column tests whether meetings in which the most engaged households, or “leaders,” were experienced with migration—measured respectively by an indicator for having a current migrant in Nairobi, having a worker in Nairobi pre-treatment, or having any past history of a member working in Nairobi—affect migration outcomes differently for experienced and inexperienced households. We find that experienced households are especially impacted by Group treatment, and even more so when the group meeting includes an experienced leader other than the household itself (for example, having a leader household with a current Nairobi migrant increases migration among households with a current Nairobi migrant by 13 pp. relative to the impact of experienced leaders on inexperienced households, $p < 0.01$). The impact of experienced leaders on inexperienced households is close to zero (in fact, it is slightly negative for one of our proxies) and statistically insignificant. Altogether, these patterns are consistent with meetings led by experienced households being less useful for inexperienced households.

5.3 Experienced Households Crowd Out Inexperienced Households in Co-Migration

In addition to being less engaged during group meetings, we also find that inexperienced households are less likely to co-migrate with others from their village following group meetings, compared to the Information arm. In Information villages, treatment increases co-migration by 0.6 pp. ($p = 0.01$), as shown in column 1 of Appendix Table A.14. 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 driven almost entirely by inexperienced households—measured based on whether they have a migrant working in Nairobi pre-treatment—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 experienced migrants. The Mentor arm did not increase co-migration, possibly because mentors substituted for the support provided by other co-migrants from the village.

Columns 3 and 4 of Appendix Table A.14 examine the assortativity of matching in co-migration. We see that the treatment impact on co-migration from the Information arm is driven entirely by co-migration of inexperienced households with more experienced households. In Group villages, the pattern is reversed, and it is largely the experienced households reporting traveling with more experienced migrants. Overall, these findings are consistent with group meetings’ facilitating information exchange and connections between experienced households, leading to more migration and co-migration among those households, but crowding out inexperienced households who would otherwise have sought out experienced co-migrants after treatment.

6 Model

In this section, we present a model of migration designed to replicate the experimental results, provide an explanation for the differences in economic impacts across treatments, and predict the aggregate effect of addressing information frictions across the board. 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.

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

6.1 Model Environment

Preferences. The economy is populated by a unit measure of households indexed by i . Each household chooses agricultural consumption $c_{a,i}$ and non-agricultural consumption $c_{n,i}$ 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

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

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

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 $z_{u,i}^n$, rural non-agricultural productivity $z_{r,i}^n$, and rural agricultural productivity $z_{r,i}^a$. 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 :

$$\begin{aligned} z_{u,i}^n &= \exp \varepsilon_{u,i}^n, & \varepsilon_{u,i}^n &\sim \mathcal{N}(0, \sigma_u^2) \\ z_{r,i}^n &= \exp \left(d \log z_{u,i}^n + \varepsilon_{r,i}^n \right), & \varepsilon_{r,i}^n &\sim \mathcal{N}(0, \sigma_r^2) \\ z_{r,i}^a &= \exp \left(d \log z_{u,i}^n + \varepsilon_{r,i}^a \right), & \varepsilon_{r,i}^a &\sim \mathcal{N}(0, \sigma_r^2) \end{aligned}$$

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

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

Production. 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:

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

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 $y_{u,i}^n = z_{u,i}^n w_u^n$.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 $y_{u,i}^n = z_{r,i}^n w_r^n$;
2. Stay in the rural region r , work in the rural agricultural sector, and earn $y_{r,i}^a = z_{r,i}^a w_r^a$;
or
3. Migrate to the urban region u , work in the urban non-agricultural sector, and earn $y_{r,i}^{nm} = \frac{1}{1+m_i} z_{r,i}^n w_u^n$ net of the proportional migration cost. 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 $y_{r,i}^n$ and $y_{r,i}^a$ accurately, their perceived urban non-agricultural income is $\frac{1}{1+\gamma_i} y_{r,i}^{nm}$. 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

$$\max \left\{ y_{r,i}^n, y_{r,i}^a, \frac{1}{1+\gamma_i} y_{r,i}^{nm} \right\}.$$

Therefore, rural household i will migrate if and only if²³

$$\frac{1}{1+\gamma_i} y_{r,i}^{nm} \geq \max \{ y_{r,i}^n, y_{r,i}^a \}. \quad (3)$$

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

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

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

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

²²Even 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.

²³We assume that households that are indifferent between migrating and staying choose to migrate.

Labor markets clear:

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

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:

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

Migration costs are rebated uniformly to all households²⁴:

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

6.2 Estimation

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.
- The labor share θ is set to the 2019 share of labor compensation in Kenyan GDP from the Penn World Table.
- 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 1.²⁵

²⁴Because we model migration costs as monetary, any amount paid by migrating households must surface elsewhere in the economy, otherwise markets will not clear

²⁵The 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.

- 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.1. We assign σ_m to be the standard deviation of the log of estimated migration costs across groups.

Information Frictions. All households are assigned the same information friction $\gamma_i = \gamma$. We measure information frictions using survey data (see Section 3): 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 $\gamma = \frac{1}{0.559} - 1 = 0.789$.

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 .²⁶ 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 6.3 elaborates on this logic and Appendix D.1 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 5 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.

6.3 Selection in the Model

Which households choose to leave the rural region and move to the city: productive or unproductive ones? Our model offers the following proposition:

²⁶We use urban income gross of migration cost to calculate this gap in the model.

Table 5: Model Estimation

| Parameter | Value | Moment | Data | Model |
|------------|-------|-----------------------------|-------|-------|
| \bar{a} | 5.569 | food cons. share | 0.345 | 0.352 |
| A_r^n | 0.113 | median urban-rural inc. gap | 5.147 | 5.189 |
| σ_u | 1.550 | SD of log urban inc. | 1.491 | 1.492 |
| σ_r | 1.980 | SD of log rural inc. | 1.569 | 1.569 |
| d | 0.075 | log urban-rural inc. slope | 0.065 | 0.064 |
| μ_m | 2.860 | migration rate | 0.171 | 0.171 |

Proposition 1 (migrant selection).²⁷ Consider a continuum of rural households with common $(\gamma_i, m_i, \varepsilon_{r,i}^n, \varepsilon_{r,i}^a)$ but varying $\varepsilon_{u,i}^n$. Then:

1. There exists a unique threshold $\hat{z}_{u,i}^n$ at which a household with $z_{u,i}^n = \hat{z}_{u,i}^n$ is indifferent between migrating and staying:

$$\frac{1}{1 + \gamma_i} \frac{1}{1 + m_i} \hat{z}_{u,i}^n w_u^n = \max \left\{ (\hat{z}_{u,i}^n)^d \exp \varepsilon_{r,i}^n w_r^n, (\hat{z}_{u,i}^n)^d \exp \varepsilon_{r,i}^a w_r^a \right\}.$$

2. The decisions of households with $z_{u,i}^n \neq \hat{z}_{u,i}^n$ depend on the parameter d :

(a) If $d < 1$:

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

(b) If $d > 1$:

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

Proposition 1 shows that the nature of selection depends on the relationship between rural and urban productivities as governed by the parameter d . When d is negative, the benefit of migrating (urban wage) rises in $z_{u,i}^n$ while the opportunity cost falls due to the falling rural wages: households that draw a high urban productivity $z_{u,i}^n$ are likely to migrate. When d is positive but less than one, the benefit of migrating rises faster in $z_{u,i}^n$ than does the opportunity cost: high- $z_{u,i}^n$ households are still more likely to migrate. Thus, the $d < 1$ case is characterized by positive selection: migrants are more productive than stayers. However, when $d > 1$, the opportunity cost of migrating rises faster in $z_{u,i}^n$ 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 $d > 1$ is characterized by negative selection: migrants are less productive than stayers.

²⁷See Appendix D.2 for the proof.

Panel A of Figure 2 illustrates the selection patterns in the two cases. When $d < 1$, selection of migrants is positive: all else equal, households choosing to migrate to the city have higher urban productivity than households choosing to stay in the village.²⁸ Provision of improved information or reduction in migration costs shift the cost curve down, inviting new migrants that are less productive than prior migrants. This implies that households enjoying a lower cost of migration should exhibit a higher migration rate, lower average urban productivity, and correspondingly a lower average migration income. When $d > 1$, selection is negative: migrants have lower urban productivity than stayers, and reducing frictions invites new migrants that are more productive than before.

Testing the Model’s Predictions. In the estimated model, $d = 0.075$, implying positive selection and an increasing threshold in migration costs. Panel B of Figure 2 tests these predictions in the data. To characterize the sign of migrant selection, we compare average educational attainment for migrants and non-migrants from the same origin village. We find that migrants have higher average education in 91% of villages. While migration costs are not easily observable in the data, we expect the migration rate to be a reliable proxy: places with few migrants are likely those facing higher migration costs compared to places with many migrants. In line with the model’s prediction, we find a strong negative correlation between the migration rate and the degree of migrant selection (correlation coefficient = -0.43). Finally, the model predicts that migrants coming from high-cost areas should earn more, which is borne out in our data (correlation coefficient = -0.34).

Rationalizing Impacts in the Group Arm. Proposition 1 and Figure 2 provide an intuitive explanation for the finding that the Group treatment boosted migration but had an insignificant impact on incomes. Households induced to migrate in the Group arm were experienced compared to those in Information and Mentor. Under the assumption that experienced households face lower migration costs, then—under positive selection, as borne out in our data and model estimates—the observed benefit of marginal migration should be lower for experienced households.

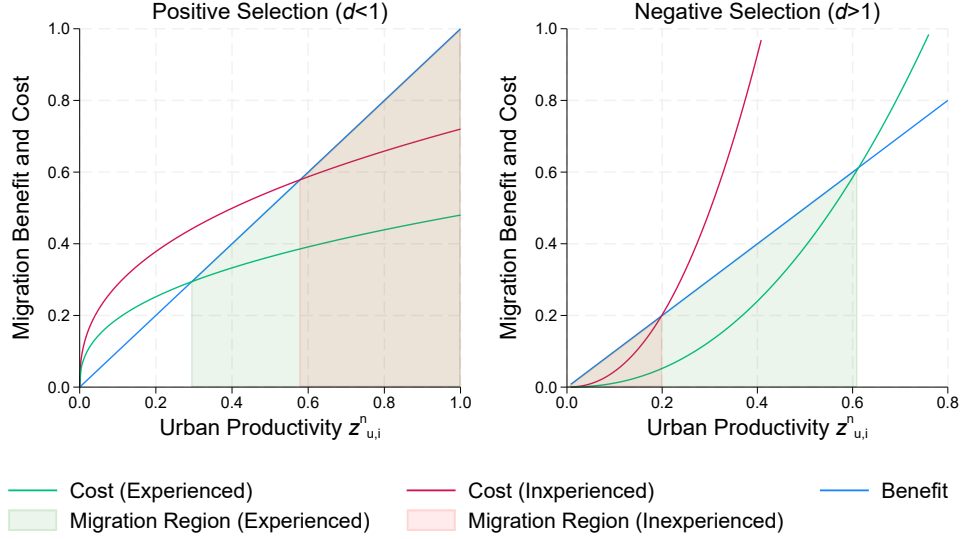
7 Quantitative Results

In this section, we conduct model equivalents of experimental treatments, assess the ability of the model to match experimental results, and use the model to conduct general equilibrium

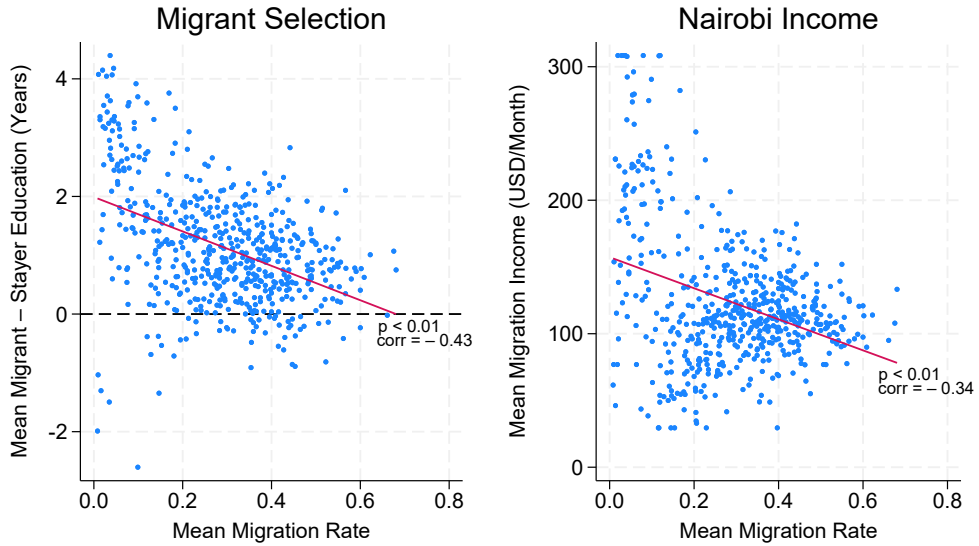
²⁸Note that $d < 0$ is allowed by the model: the cost curves are decreasing in $z_{u,i}^n$ in this case, but the selection pattern is the same as for $0 \leq d < 1$.

Figure 2: Selection and Marginal Returns in the Model and Data

Panel A: Marginal benefits are higher for inexperienced 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 experienced (low-cost) and inexperienced (high-cost) types in both positive ($d < 1$) and negative ($d > 1$) selection environments. Migration *benefit* is urban income $z_{u,i}^n w_u^n$; *cost* is inclusive of all frictions and opportunity costs: $(1 + \gamma_i)(1 + m_i) \max \{ (z_{u,i}^n)^d \exp \varepsilon_{r,i}^n w_r^n, (z_{u,i}^n)^d \exp \varepsilon_{r,i}^a w_r^a \}$. **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.

counterfactuals.

7.1 Replicating the Experimental Treatments

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 sampled per village).

We choose to discipline 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 2) 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.

The Information treatment is represented with a uniform reduction in the information friction γ , consistent with the roughly uniform engagement discussed in Section 5.2. As shown in Table 2, 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 $\gamma' = \frac{1+\gamma}{1+0.04} - 1 = \frac{1+0.789}{1+0.04} - 1 = 0.720$.²⁹

The Mentor treatment differs from the Information treatment only in the magnitude of the change in beliefs. As shown in Table 2, households in the mentor treatment arm saw an 11% increase in their expected migration income, corresponding to $\gamma' = \frac{1+0.789}{1+0.11} - 1 = 0.612$ in the model.

In the Group arm, we split treated households into two types to match the greater engagement of experienced households discussed in Section 5.2. Households whose migration cost m_i is above the median (“inexperienced”) get no improvement in their beliefs, reflecting their limited engagement during group meetings. Households whose migration cost m_i is below the median (“experienced”) get double the average improvement in beliefs $\gamma' = \frac{1+0.789}{1+2 \times 0.11} - 1 = 0.466$. Thus, the Group arm has the same average impact on beliefs as the Mentor arm—matching the experimental impacts shown in Table 2—but favors more experienced households, in line with the findings of Section 5.

²⁹Note that a separate Spillover treatment arm is not needed in the model: the two-thirds of rural households not receiving information treatment in the model effectively constitute the spillover arm.

7.2 Experimental Impacts in the Model

To assess the model’s performance, we begin by examining the effects of experimental treatments in partial equilibrium, not letting prices adjust from the pre-experiment baseline to clear markets. This is appropriate for comparing experimental impacts in the model to the data: despite its relatively large scale, the experiment treated only a small fraction of Kenya’s population, implying limited effects on aggregate prices.

Migration. Simulation results are presented in Table 6. 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 Group treatment increases the migration rate by 1.4 pp.: this result is smaller than the 2.2–3.4 pp. impacts in the data, but larger than the Information treatment by a similar ratio to the data. The Mentor treatment increases the migration rate by 1 pp.: like the Group treatment, this result is smaller than the 1.8–2.4 pp. impacts in the data but appropriately larger than the Information treatment.

Income. The model successfully recovers large economic impacts from small changes in migration. The Information treatment increases the incomes of treated households by 10.5%, on average: a similar magnitude to the data. The Mentor treatment boosts incomes by 27.7%: this result matches the greater economic impact of the Mentor arm compared to the Information arm in the data, but overstates the degree. The model also successfully recovers the adverse selection problem in the Group arm: despite having the largest impact on migration in the model, Group brings a much lower economic benefit compared to Mentor at 13.6% (counterfactually, however, this impact is greater than the null impact in the data). Intuitively, because the Group treatment targets low-cost (“experienced”) people, those induced to migrate in Group are those who do not benefit much from migration: if they did, they would have migrated even in the absence of treatment thanks to their low migration cost.

General Equilibrium. Next, we allow prices to respond to experimental treatments in order to clear markets. Migration and economic impacts are slightly dampened by the general equilibrium response. More importantly, the general equilibrium response affects households that were not treated: all three treatments boost the incomes of untreated villagers by 0.1–0.4%. This spillover effect is significantly smaller than that observed in the data (see Section 4.6) and comes largely from the rural labor market response: as some treated households

Table 6: Model: Experimental Impacts

| | Partial Equilibrium | | General Equilibrium | | |
|--------|-----------------------|-------------------|-----------------------|-------------------|------------------------------|
| | Migration rate change | Avg income change | Migration rate change | Avg income change | Avg income change, spillover |
| Info | 0.004 | 0.105 | 0.004 | 0.098 | 0.001 |
| Group | 0.014 | 0.136 | 0.013 | 0.134 | 0.005 |
| Mentor | 0.010 | 0.277 | 0.009 | 0.264 | 0.003 |

Universal treatments are applied to all rural households. All economies are solved in general equilibrium. “Migration rate change” is the difference in migration rate among treated households. “Avg income change” is the average relative change in observed income (gross of migration cost) for the treated households. “Avg income change, spillover” is the average relative change in observed income for the untreated rural households.

migrate, rural labor supply falls and rural wages rise, benefiting the stayers.

7.3 Scaling the Interventions

Simulated partial-equilibrium experiments in the model successfully match many of the experimental population-level treatment effects qualitatively and, to a lesser degree, quantitatively. 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.

Scaling up the Information treatment to the national level—so that all rural Kenyans increase their perceived urban income by 4%—boosts the national migration rate by 0.3 percentage points from a baseline of 17.2%, as shown in Table 7. Real non-agricultural output goes up by 0.4 pp. The agricultural productivity gap and the urban-rural income gap both decrease slightly. Scaling up the Mentor treatment nationally—so that perceived urban income rises by 11% for all rural Kenyans—is even more effective: the migration rate increases by 0.7 pp., non-agricultural output goes up 1%, the agricultural productivity gap falls from 2.728 to 2.681, and the urban-rural income gap falls from 5.189 to 5.026.

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 Info” exercise suggests that removing information frictions entirely could be transformative. Our model predicts that the migration rate would increase from 17.2% to 22%, non-agricultural output would grow 6.3% (while agricultural output would drop 0.4% as agricultural labor supply is reduced), the agricultural productivity gap would fall from 2.728 to 2.416,

and the rural-urban income gap would fall from 5.189 to 4.145. 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 7: Model: Aggregate Impacts of Universal Treatments

| | Migration Rate | Real Non-Agric. GDP | Real Agric. GDP | Agricultural Productivity Gap | Urban-Rural Income Gap |
|--------------|-------------------|------------------------|--------------------|----------------------------------|---------------------------|
| Baseline | 0.172 | 1.000 | 1.000 | 2.728 | 5.189 |
| Info | 0.175 | 1.004 | 1.000 | 2.709 | 5.135 |
| Mentor | 0.179 | 1.010 | 0.999 | 2.681 | 5.026 |
| Perfect Info | 0.220 | 1.063 | 0.996 | 2.416 | 4.145 |

Real non-agricultural and agricultural GDPs are expressed relative to the baseline.

8 Conclusion

This study provides new experimental evidence on the role of network and information constraints affecting internal migration decisions in Kenya. We show that the common underestimation of urban earnings documented in Baseler (2023) extends to the majority of the population of five Kenyan counties, and bring in new evidence that many rural households—especially those with no former migration experience—lack social connections who could help them navigate urban labor or housing markets. Simple programs that inform households of true urban earnings or expand their social networks at the destination can substitute for existing networks, and the economic returns to these interventions are large. We note, however, that the observed migration response is small relative to the scale of initial misperceptions, suggesting that information barriers alone are not the primary factor preventing migration for most households.

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. We cannot, therefore, reject the efficient sorting hypothesis for the experienced types affected by group-level treatments. Instead, we expect the highest returns to come from interventions that can reach households without former migration experience, who tend to be less socially connected both at the origin and destination. Migration researchers working in settings similar to ours can consider whether their intervention is likely to reach inexperienced households. In adapting a migration intervention to a new setting, our model demonstrates that characterizing the nature of migrant selection at baseline—which can be done fairly easily with observational data—is sufficient to indicate whether an intervention should target the experienced or the inexperienced.

One concern with scaling programs that attempt to facilitate rural-urban migration is that crowding out of current urban workers—for example, through increased competition in labor markets or pressure on infrastructure and public services—will dilute the aggregate gains from the policy. In view of this concern, our finding that significant economic gains are attainable from relatively low additional migration is arguably reassuring. That almost two-thirds of those income gains accrue to non-migrating households implies that the gains from encouraging rural-urban migration are not as concentrated as they might appear from the migration impacts alone.

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Appendix for “Reaching the Novice or Nudging the Expert? Networks, Information, and the Experimental Returns to Migration”

A Additional Tables and Figures

Figure A.1: Overview of Study Design

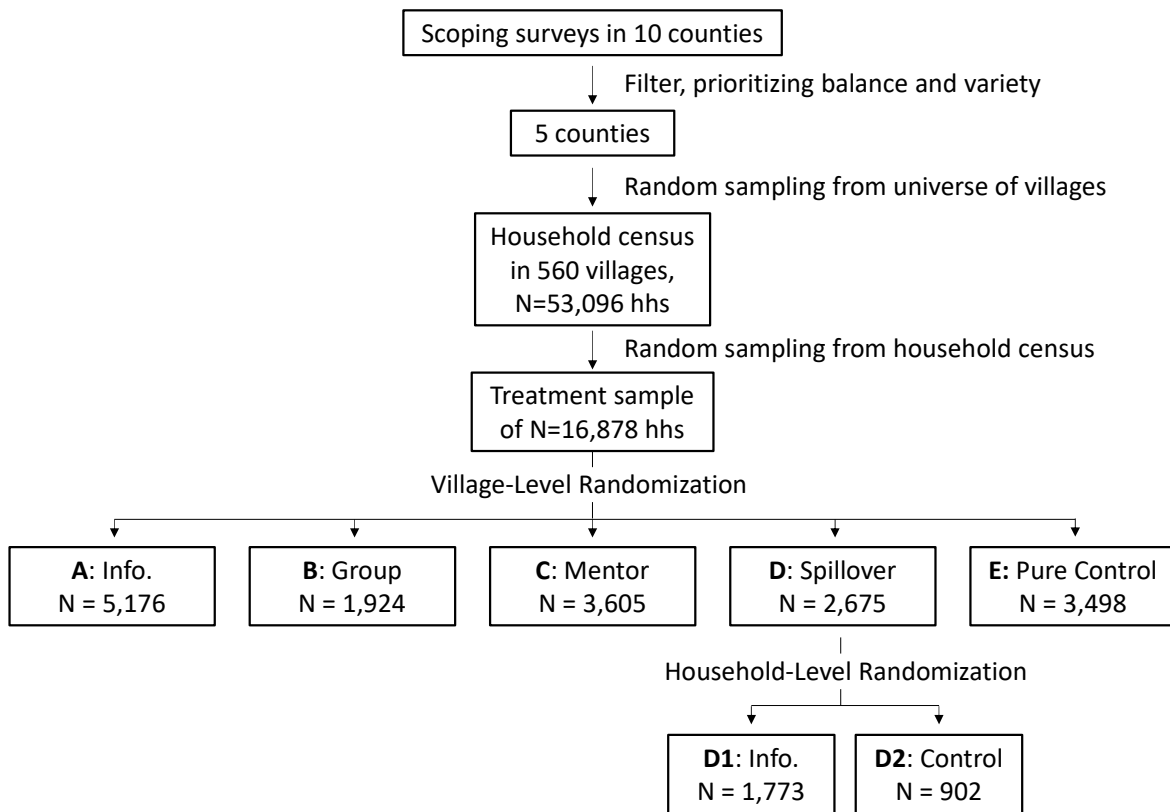
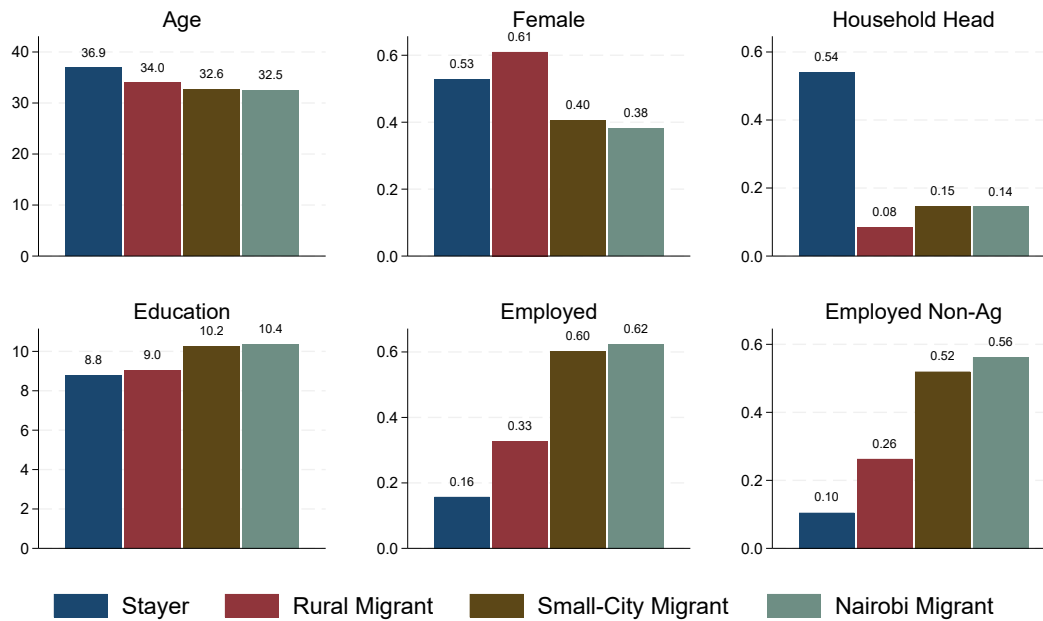
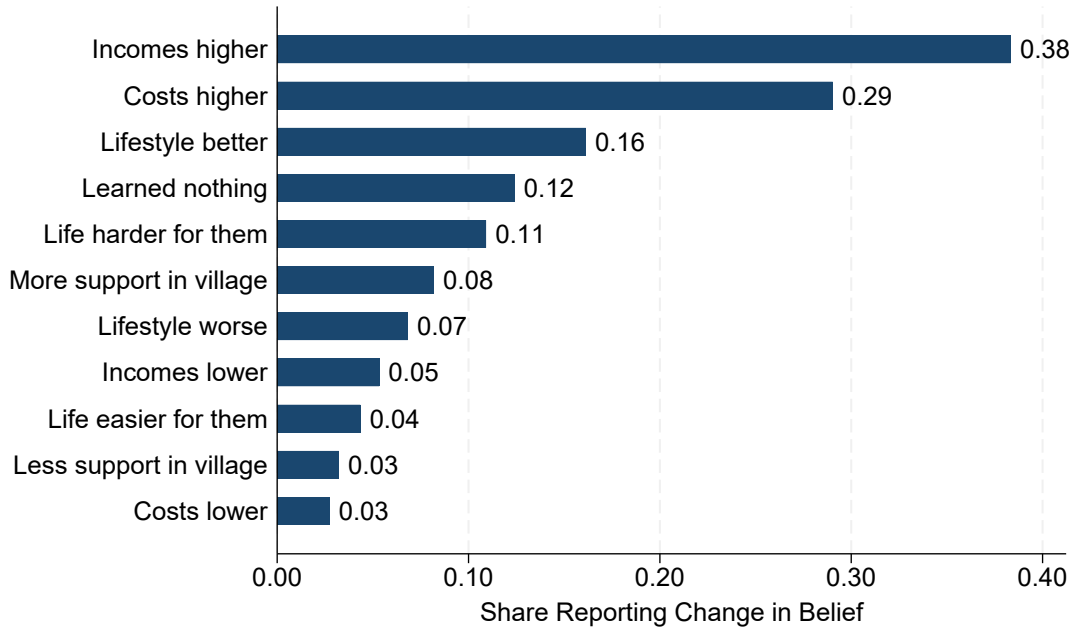


Figure A.2: Nairobi migrants tend to be young, male, educated, and not the head of their household.



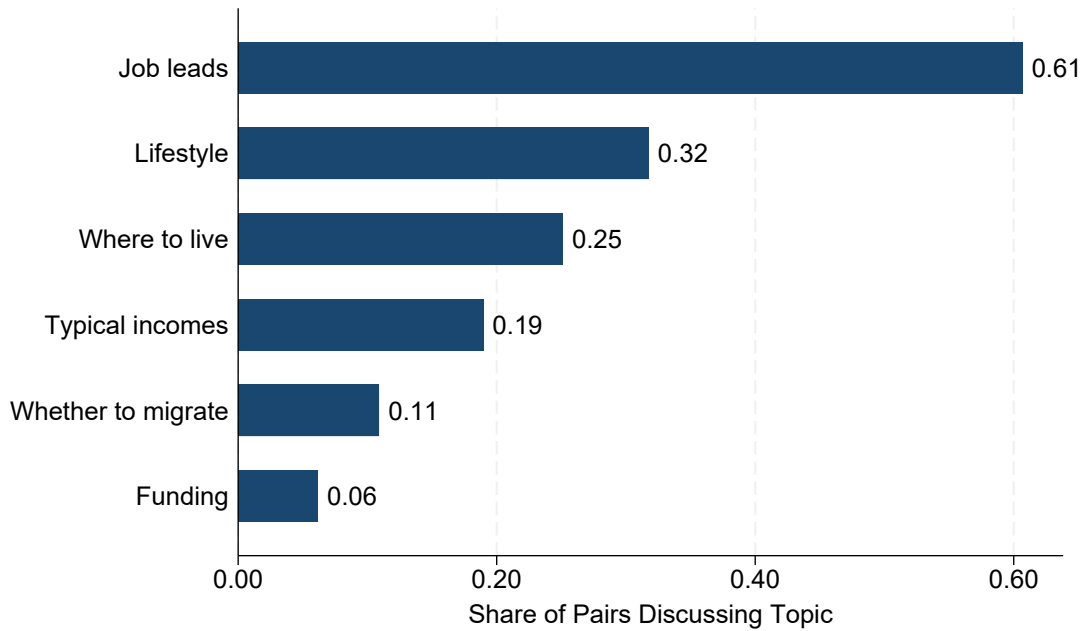
Data from $N = 60,833$ individuals aged 16 or older collected from baseline household surveys. *Stayers* are individuals residing in their origin village; *Migrants* are former household members residing outside the origin village. Rural/urban designation of the migrant's destination, and their destination 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.

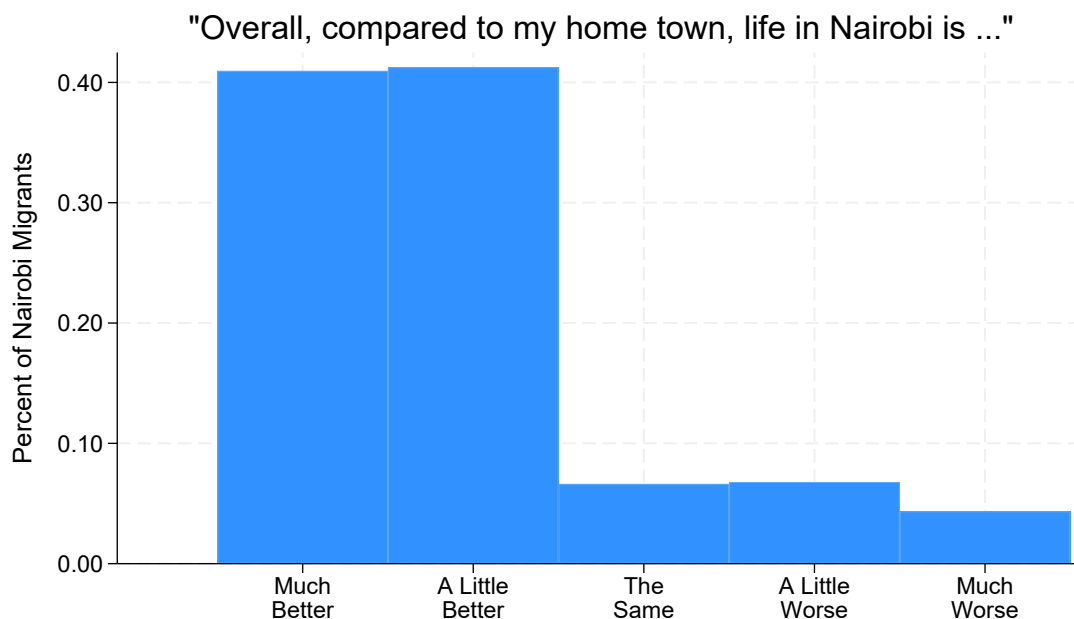
Figure A.4: Topics of Discussion in Mentor Arm



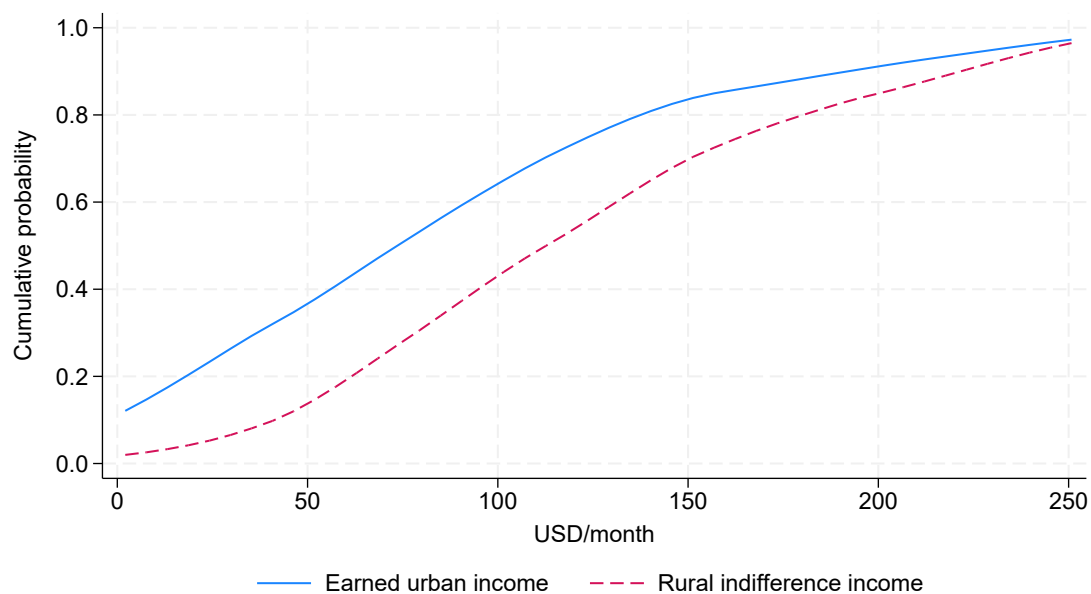
Data from midline surveys of household heads. The survey question was “What did [the mentor and the person from your household who talked to them] talk about?” and respondents could select multiple answers which were not read aloud.

Figure A.5: Migrant Subjective Well-Being

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



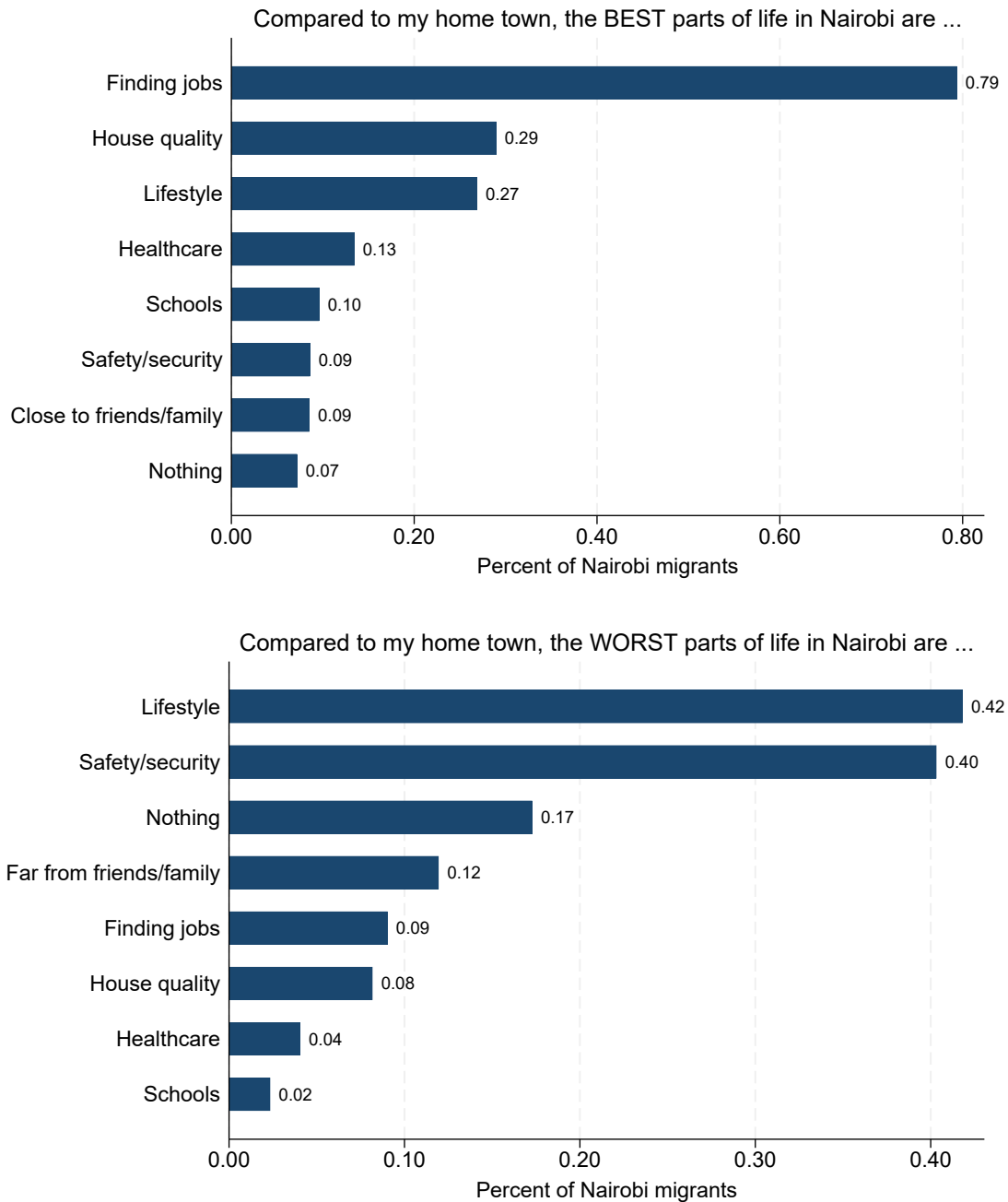
Panel B: Migrants report being willing to move home only for higher incomes.



Correlation coefficient = 0.42. Bandwidth = 20. Incomes are top-coded at 230 USD.

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.

Table A.1: Comparison of Study Counties to Country

| | Sample Counties | All Counties | Percentile |
|---|--------------------|-----------------|------------|
| % aged 18–50 | 0.36 | 0.37 | 0.41 |
| % with primary degree | 0.38 | 0.38 | 0.55 |
| % with secondary degree | 0.07 | 0.08 | 0.39 |
| % with post-secondary degree | 0.01 | 0.02 | 0.39 |
| % Muslim | 0.01 | 0.00 | 0.73 |
| Per-capita household income (USD/month) | 13.57 | 23.81 | 0.26 |
| Density (pop. per sq. km.) | 393.00 | 288.00 | 0.67 |
| Distance to Nairobi (km) | 393.26 | 305.93 | 0.73 |
| % of households migrated out of county | 0.23 | 0.20 | 0.76 |
| % of households migrated to Nairobi | 0.07 | 0.04 | 0.80 |
| 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 rural county-level distribution corresponding to the project sample median 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. All estimates use population weights.

Table A.2: Migrant Outcome Descriptives

| | Ctrl. Mean | Info Diff. | Group Diff. | Mentor Diff. | N |
|--|------------------|-----------------|-----------------|-----------------|-------|
| Returned to village | 0.35 (0.48) | 0.00 (0.03) | 0.01 (0.04) | -0.02 (0.03) | 4,809 |
| Duration (months) | 6.63 (5.15) | 0.13 (0.27) | 0.13 (0.40) | -0.00 (0.31) | 4,731 |
| Migrated with others from household | 0.04 (0.19) | 0.01 (0.01) | -0.02 (0.01) | -0.02 (0.01) | 4,809 |
| Migrated with others from village | 0.03 (0.17) | 0.03 (0.01) | 0.02 (0.01) | -0.00 (0.01) | 4,809 |
| Received job referral from village | 0.72 (0.45) | -0.01 (0.03) | 0.02 (0.04) | -0.00 (0.03) | 4,809 |
| Received housing assistance from village | 0.11 (0.32) | -0.02 (0.02) | -0.03 (0.02) | -0.03 (0.02) | 4,809 |
| Borrowed cash to migrate from village | 0.04 (0.19) | 0.00 (0.01) | -0.01 (0.01) | -0.00 (0.01) | 4,809 |
| Worked as employee | 0.59 (0.49) | -0.01 (0.03) | 0.01 (0.03) | -0.05 (0.03) | 4,809 |
| Worked as business owner | 0.07 (0.25) | -0.02 (0.01) | -0.02 (0.02) | -0.01 (0.01) | 4,809 |
| Income (monthly), among workers | 85.08 (73.07) | 3.97 (5.21) | -2.64 (7.11) | 3.84 (5.69) | 2,817 |
| Remittances (monthly), among workers | 13.94 (17.43) | 3.07 (1.47) | 0.94 (1.66) | -0.25 (1.41) | 2,817 |
| Weeks taken to find job after migrating | 3.82 (14.82) | -0.26 (1.06) | 0.82 (2.06) | 0.33 (1.25) | 2,914 |
| Married | 0.34 (0.47) | 0.00 (0.03) | 0.01 (0.04) | 0.04 (0.03) | 3,147 |
| Among married, lives with spouse | 0.64 (0.48) | -0.06 (0.06) | -0.08 (0.08) | 0.01 (0.07) | 947 |

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 baseline 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. 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

| | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-----------------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| <i>Disaggregated</i> | | | | | | |
| Info | 0.006 (0.010) [0.51] | 0.008 (0.011) [0.50] | 0.009 (0.012) [0.44] | 0.008 (0.087) [0.93] | -0.048 (0.064) [0.45] | 0.039 (0.069) [0.58] |
| Group | 0.005 (0.014) [0.74] | 0.018 (0.015) [0.26] | 0.007 (0.018) [0.71] | 0.005 (0.124) [0.97] | 0.079 (0.078) [0.31] | 0.056 (0.102) [0.58] |
| Mentor | -0.001 (0.012) [0.91] | 0.009 (0.013) [0.50] | -0.004 (0.014) [0.75] | -0.053 (0.104) [0.61] | 0.048 (0.074) [0.51] | -0.007 (0.079) [0.93] |
| Model | OLS | OLS | OLS | Poisson | Poisson | Poisson |
| <i>p</i> -Val: Info.=Group | 0.88 | 0.49 | 0.87 | 0.98 | 0.06 | 0.85 |
| <i>p</i> -Val: Info.=Mentor | 0.41 | 0.95 | 0.23 | 0.47 | 0.12 | 0.47 |
| <i>p</i> -Val: Group=Mentor | 0.66 | 0.55 | 0.52 | 0.63 | 0.69 | 0.51 |
| Control Mean | 0.12 | 0.19 | 0.19 | 0.13 | 0.24 | 0.22 |
| Observations | 12,977 | 12,977 | 12,977 | 12,977 | 12,977 | 12,977 |
| <i>Pooled Treatment</i> | | | | | | |
| Any Info | 0.004 (0.009) [0.68] | 0.009 (0.010) [0.37] | 0.005 (0.011) [0.67] | -0.010 (0.083) [0.90] | -0.002 (0.059) [0.97] | 0.028 (0.066) [0.67] |
| Observations | 12,977 | 12,977 | 12,977 | 12,977 | 12,977 | 12,977 |
| <i>Treatment Intensity</i> | | | | | | |
| Any Info × Prior Gap | 0.001 (0.020) [0.96] | 0.015 (0.025) [0.54] | 0.011 (0.024) [0.64] | -0.083 (0.155) [0.59] | -0.079 (0.121) [0.51] | 0.048 (0.121) [0.69] |
| Any Info | 0.004 (0.012) [0.77] | 0.005 (0.014) [0.75] | 0.001 (0.014) [0.93] | 0.016 (0.092) [0.86] | 0.026 (0.070) [0.71] | 0.016 (0.074) [0.83] |
| Prior Gap | -0.041** (0.018) [0.03] | -0.054** (0.023) [0.02] | -0.057*** (0.022) [0.01] | -0.278** (0.137) [0.04] | -0.176 (0.108) [0.10] | -0.288*** (0.107) [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

| | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|--------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| Spillover | 0.01 (0.01) [0.60] | -0.00 (0.01) [0.92] | 0.01 (0.01) [0.66] | 0.01 (0.09) [0.94] | -0.06 (0.08) [0.41] | 0.05 (0.08) [0.52] |
| Model | OLS | OLS | OLS | Poisson | Poisson | Poisson |
| Control Mean | 0.12 | 0.18 | 0.20 | 0.13 | 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: Distinguishing Selection From Treatment Effects of Mentors

| | Welfare Index | | Income | | Yearly Income | |
|------------------------|--------------------------|--------------------------|----------------------------|----------------------------|-----------------------------|-----------------------------|
| Info | 0.02 (0.07) [0.71] | 0.03 (0.06) [0.69] | 3.21 (10.80) [0.77] | 4.72 (10.56) [0.65] | 0.27 (90.81) [1.00] | -0.11 (89.85) [1.00] |
| Mentor | 0.09 (0.08) [0.29] | 0.12 (0.08) [0.14] | 12.43 (12.71) [0.33] | 15.75 (12.47) [0.21] | 51.07 (102.72) [0.62] | 65.54 (100.57) [0.51] |
| Controls | | X | | X | | X |
| p -Val: Info.=Mentor | 0.41 | 0.20 | 0.40 | 0.29 | 0.57 | 0.46 |
| Control Mean | 0.43 | 0.43 | 183.87 | 183.87 | 1283.75 | 1283.75 |
| Observations | 2,933 | 2,933 | 2,933 | 2,933 | 2,932 | 2,932 |

Sample includes households that sent migrants to Nairobi as of the endline survey. Impacts are estimated on data from endline surveys. Welfare index is a standardized Anderson (2008) index including all pre-specified welfare measures. Linear regression is used for all outcomes. Odd columns are estimated using (1); even columns include lasso-selected controls chosen from the set of variables shown in Appendix Table A.11. 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.6: Heterogeneity in Migration Impacts

| <i>Dimension of Heterogeneity X:</i> | Outcome: Sent Any Migrants to Nairobi | | | | | |
|--------------------------------------|---------------------------------------|-----------------------------|-----------------------------|-----------------------------|-------------------------------|-----------------------------|
| | Has Nairobi Worker | Has Ever Worked in Nairobi | Plans to Migrate to Nairobi | # Origin Social Connections | Standardized Household Income | More Than Primary Education |
| Info | 0.03*** (0.01) [0.00] | 0.04*** (0.01) [0.00] | 0.02* (0.01) [0.07] | 0.04*** (0.01) [0.01] | 0.02 (0.01) [0.14] | 0.02* (0.01) [0.08] |
| Group | 0.02** (0.01) [0.05] | 0.02* (0.01) [0.09] | 0.02 (0.01) [0.19] | 0.04** (0.02) [0.03] | 0.02 (0.01) [0.14] | 0.02 (0.02) [0.15] |
| Mentor | 0.04*** (0.01) [0.00] | 0.04*** (0.01) [0.00] | 0.03** (0.01) [0.02] | 0.04*** (0.02) [0.01] | 0.02** (0.01) [0.05] | 0.04*** (0.02) [0.01] |
| Info \times X | -0.06** (0.03) [0.03] | -0.05** (0.02) [0.03] | -0.02 (0.02) [0.34] | -0.02** (0.01) [0.02] | -0.02** (0.01) [0.04] | -0.01 (0.02) [0.65] |
| Group \times X | -0.02 (0.04) [0.53] | -0.00 (0.03) [0.92] | 0.02 (0.03) [0.40] | -0.01 (0.01) [0.17] | -0.04** (0.02) [0.02] | -0.01 (0.02) [0.83] |
| Mentor \times X | -0.05* (0.03) [0.06] | -0.04 (0.02) [0.10] | -0.04* (0.02) [0.08] | -0.02* (0.01) [0.06] | -0.04*** (0.01) [0.00] | -0.04 (0.02) [0.10] |
| X | 0.19*** (0.02) [0.00] | 0.12*** (0.02) [0.00] | 0.05** (0.02) [0.01] | 0.02*** (0.01) [0.01] | 0.06*** (0.01) [0.00] | 0.06*** (0.01) [0.00] |
| Control Mean | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 | 0.17 |
| Observations | 15,468 | 15,468 | 15,468 | 15,033 | 15,468 | 15,412 |

Impacts are estimated on data from endline surveys. Each column shows treatment impacts on an indicator for whether the household sent any migrants to Nairobi after treatment (top panel) or total household income in the past month (bottom panel) estimated within sample splits defined by baseline income (columns 1–2), whether the household had ever had a migrant working in Nairobi, the number of social connections in Nairobi, and the number of origin social connections who could assist with migration. Sample splits are made using the median value of each variable. 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.7: Heterogeneity in Nairobi Income Impacts

| <i>Dimension of Heterogeneity X:</i> | Outcome: Income Earned in Nairobi (USD/Month) | | | | | |
|--------------------------------------|---|-----------------------------|-----------------------------|-----------------------------|-------------------------------|-----------------------------|
| | Has Nairobi Worker | Has Ever Worked in Nairobi | Plans to Migrate to Nairobi | # Origin Social Connections | Standardized Household Income | More Than Primary Education |
| <i>Disaggregated</i> | | | | | | |
| Info | 1.26 (1.06) [0.23] | 2.13* (1.17) [0.07] | 0.07 (1.73) [0.97] | 2.26 (1.78) [0.20] | -0.37 (1.53) [0.81] | 0.73 (1.36) [0.59] |
| Group | 1.12 (1.28) [0.38] | 2.01 (1.33) [0.13] | -0.84 (2.21) [0.70] | 0.03 (2.51) [0.99] | -0.54 (2.02) [0.79] | 0.55 (2.04) [0.79] |
| Mentor | 2.34* (1.37) [0.09] | 2.07 (1.44) [0.15] | 0.35 (2.01) [0.86] | 2.59 (2.15) [0.23] | -0.18 (1.81) [0.92] | 2.59 (1.95) [0.19] |
| Info \times X | -5.83 (3.82) [0.13] | -5.67* (3.05) [0.06] | -1.57 (2.56) [0.54] | -1.79 (1.17) [0.13] | -3.30 (2.47) [0.18] | -1.74 (2.47) [0.48] |
| Group \times X | -6.97 (5.12) [0.17] | -6.43 (3.99) [0.11] | 3.04 (3.88) [0.43] | -0.20 (1.81) [0.91] | -3.13 (3.16) [0.32] | -1.94 (3.13) [0.54] |
| Mentor \times X | -8.77** (4.21) [0.04] | -4.96 (3.37) [0.14] | -2.32 (2.93) [0.43] | -1.98 (1.27) [0.12] | -4.68* (2.57) [0.07] | -4.92* (2.86) [0.09] |
| Control Mean | 13.67 | 13.67 | 13.67 | 13.63 | 13.67 | 13.58 |
| Observations | 15,468 | 15,468 | 15,468 | 15,033 | 15,468 | 15,412 |
| <i>Pooled Treatment</i> | | | | | | |
| Any Info | 1.57 (1.00) [0.12] | 2.09* (1.08) [0.05] | 0.01 (1.64) [0.99] | 2.02 (1.66) [0.22] | -0.32 (1.45) [0.83] | 1.25 (1.32) [0.35] |
| Any Info \times X | -6.83** (3.44) [0.05] | -5.56** (2.78) [0.05] | -1.22 (2.46) [0.62] | -1.63 (1.15) [0.16] | -3.61 (2.32) [0.12] | -2.70 (2.26) [0.23] |
| Observations | 15,468 | 15,468 | 15,468 | 15,033 | 15,468 | 15,412 |

Impacts are estimated on data from endline surveys. Each column shows treatment impacts on an indicator for whether the household sent any migrants to Nairobi after treatment (top panel) or total household income in the past month (bottom panel) estimated within sample splits defined by baseline income (columns 1–2), whether the household had ever had a migrant working in Nairobi, the number of social connections in Nairobi, and the number of origin social connections who could assist with migration. Sample splits are made using the median value of each variable. 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.8: Within- and Across-Village Spillover Impacts

| | Perceived Nairobi Income | Perceived Own Nairobi Income | Sent New Migrants to Nairobi | Sent New Migrants to Any City | Income | Welfare Index |
|----------------------------------|--------------------------------|------------------------------------|------------------------------------|-------------------------------------|----------------------------|----------------------------|
| <i>Within-Village Spillovers</i> | | | | | | |
| Untreated HH in Spillover | -0.01 (0.04) [0.78] | 0.04 (0.05) [0.42] | 0.00 (0.02) [0.89] | -0.00 (0.02) [0.86] | 4.54 (5.39) [0.40] | 0.13** (0.05) [0.01] |
| Model | Poisson | Poisson | OLS | OLS | OLS | OLS |
| Control Mean | 126.90 | 188.07 | 0.17 | 0.29 | 99.70 | -0.03 |
| Observations | 2,910 | 4,124 | 4,247 | 4,247 | 4,247 | 4,247 |
| <i>Across-Village Spillovers</i> | | | | | | |
| Share Treated $\leq 3\text{km}$ | 0.02 (0.05) [0.70] | -0.01 (0.07) [0.87] | 0.03 (0.02) [0.15] | 0.02 (0.03) [0.43] | 3.09 (5.60) [0.58] | -0.02 (0.04) [0.66] |
| Model | Poisson | Poisson | OLS | OLS | OLS | OLS |
| Outcome Mean | 126.90 | 188.07 | 0.17 | 0.29 | 99.70 | -0.03 |
| Observations | 2,296 | 3,274 | 3,376 | 3,376 | 3,376 | 3,376 |
| Share Treated $\leq 10\text{km}$ | -0.10 (0.17) [0.54] | -0.11 (0.14) [0.43] | 0.03 (0.09) [0.77] | 0.04 (0.12) [0.71] | -6.59 (17.57) [0.71] | -0.14 (0.16) [0.36] |
| Model | Poisson | Poisson | OLS | OLS | OLS | OLS |
| Outcome Mean | 126.90 | 188.07 | 0.17 | 0.29 | 99.70 | -0.03 |
| 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) |
|----------------------------------|--------------------------------|-----------------------------|------------------------------------|---|--|-------------------------------|
| <i>Within-Village Spillovers</i> | | | | | | |
| Untreated HH in Spillover | 0.37** (0.17) [0.03] | 0.14*** (0.05) [0.00] | 0.01 (0.01) [0.30] | -0.00 (0.15) [0.99] | 0.02 (0.04) [0.70] | 0.06 (0.06) [0.31] |
| Model | Poisson | Poisson | OLS | Poisson | Poisson | Poisson |
| Control Mean | 1.66 | 63.47 | 0.09 | 3.95 | 57.95 | 34.52 |
| Observations | 4,247 | 4,247 | 4,247 | 4,247 | 4,247 | 4,247 |
| <i>Direct Treatment Impacts</i> | | | | | | |
| Info | 0.35*** (0.10) [0.00] | 0.08** (0.04) [0.04] | 0.01* (0.01) [0.07] | 0.32*** (0.10) [0.00] | 0.02 (0.03) [0.58] | 0.02 (0.03) [0.54] |
| Group | -0.33** (0.16) [0.04] | 0.01 (0.05) [0.78] | -0.01 (0.01) [0.26] | -0.02 (0.14) [0.91] | 0.05 (0.04) [0.24] | 0.09* (0.05) [0.07] |
| Mentor | 0.22* (0.12) [0.07] | 0.11*** (0.04) [0.01] | 0.01 (0.01) [0.55] | 0.19 (0.12) [0.12] | 0.05 (0.03) [0.13] | 0.04 (0.04) [0.30] |
| Model | Poisson | Poisson | OLS | Poisson | Poisson | Poisson |
| Control Mean | 1.66 | 63.47 | 0.09 | 3.95 | 57.95 | 34.52 |
| 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: Information spillovers are lower for more financially connected households.

| | Perceived Nairobi Income | | | | Perceived Own Nairobi Income | | | |
|---|-----------------------------|---------------------------|---------------------------|-----------------------------|------------------------------|---------------------------|---------------------------|---------------------------|
| Spillover Village \times Borrowing Connections | -0.09** (0.04) [0.04] | | | -0.11** (0.05) [0.02] | -0.09** (0.04) [0.04] | | | -0.08 (0.05) [0.12] |
| Spillover Village \times Migration Advice Connections | | 0.03 (0.04) [0.45] | | 0.07 (0.05) [0.16] | | -0.02 (0.05) [0.70] | | 0.01 (0.06) [0.81] |
| Spillover Village \times Farm Help Connections | | | -0.02 (0.03) [0.51] | -0.00 (0.04) [0.93] | | | -0.04 (0.04) [0.28] | -0.01 (0.05) [0.85] |
| Spillover Village | 0.04 (0.05) [0.42] | -0.03 (0.05) [0.52] | 0.00 (0.04) [0.98] | 0.00 (0.05) [0.95] | 0.09 (0.06) [0.11] | 0.06 (0.05) [0.28] | 0.07 (0.05) [0.22] | 0.08 (0.06) [0.13] |
| Model | Poisson | Poisson | Poisson | Poisson | Poisson | Poisson | Poisson | Poisson |
| Control Mean | 127 | 127 | 127 | 127 | 188 | 188 | 188 | 188 |
| 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.11: Treatment Impacts on Selection Into Migrating to Nairobi

| | Ctrl. Mean | Info Diff. | Group Diff. | Mentor Diff. | Spillover Diff. | N |
|---------------------------------------|--------------------|-----------------------------|------------------------------|------------------------------|----------------------------|--------|
| More than primary education | 0.64 (0.48) | 0.00 (0.03) [0.98] | 0.00 (0.04) [0.94] | -0.05 (0.04) [0.17] | -0.04 (0.05) [0.42] | 16,818 |
| Belongs to minority tribe | 0.06 (0.24) | -0.00 (0.02) [0.90] | -0.01 (0.02) [0.52] | -0.03 (0.02) [0.12] | -0.01 (0.03) [0.61] | 16,878 |
| Baseline welfare index | 0.16 (1.14) | -0.04 (0.08) [0.61] | -0.15 (0.09) [0.10] | -0.18** (0.08) [0.02] | -0.20* (0.11) [0.07] | 16,878 |
| Monthly household income (USD) | 110 (148) | -4 (10) [0.73] | -15 (12) [0.18] | -27*** (10) [0.01] | -3 (15) [0.83] | 16,878 |
| Has migrant working in Nairobi | 0.59 (0.49) | -0.05 (0.03) [0.11] | 0.01 (0.04) [0.73] | -0.08** (0.04) [0.03] | 0.03 (0.05) [0.54] | 16,878 |
| Has ever worked in Nairobi | 0.73 (0.45) | -0.05* (0.03) [0.07] | 0.01 (0.04) [0.73] | -0.08** (0.03) [0.02] | -0.03 (0.05) [0.56] | 16,878 |
| Plans to migrate to Nairobi | 0.27 (0.44) | -0.03 (0.02) [0.11] | -0.01 (0.03) [0.80] | -0.06*** (0.02) [0.01] | -0.03 (0.03) [0.34] | 16,878 |
| Plans to migrate to any city | 0.31 (0.46) | -0.04* (0.02) [0.06] | -0.01 (0.03) [0.78] | -0.08*** (0.03) [0.00] | -0.05 (0.04) [0.17] | 16,878 |
| Perceived migration earnings | 148.64 (102.30) | 0.67 (7.58) [0.93] | -7.19 (11.33) [0.53] | -6.46 (8.38) [0.44] | 2.74 (11.80) [0.82] | 16,226 |
| # Nairobi social connections | 3.88 (3.89) | 0.15 (0.29) [0.61] | -0.07 (0.32) [0.84] | 0.14 (0.35) [0.70] | 0.83 (0.49) [0.10] | 16,868 |
| # origin migration advice connections | 0.76 (0.98) | -0.13** (0.06) [0.04] | -0.23*** (0.07) [0.00] | -0.23*** (0.07) [0.00] | -0.05 (0.10) [0.64] | 16,614 |
| Village sociality index | 0.81 (0.09) | 0.00 (0.01) [0.89] | -0.02 (0.02) [0.15] | -0.02 (0.01) [0.16] | 0.00 (0.02) [0.95] | 16,878 |

Sample includes households that sent migrants to Nairobi as of the endline survey. First column shows the means (standard deviations) of baseline 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. Column 5 shows differences between control-village households and control households in Spillover villages. 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: Engagement During Information Meetings

| Outcome: Engagement During Intervention | 1-on-1 (Info.+Mentor) | Group |
|--|-----------------------------|-----------------------------|
| Plans to use mentor program (post-treatment) | 0.16*** (0.02) [0.00] | 0.00 (.) [.] |
| Plans to migrate to Nairobi (post-treatment) | 0.09*** (0.01) [0.00] | 0.03** (0.02) [0.03] |
| Plans to migrate to Nairobi (pre-treatment) | 0.03*** (0.01) [0.01] | 0.02 (0.01) [0.14] |
| Has migrant working in Nairobi | -0.02** (0.01) [0.05] | 0.03* (0.02) [0.05] |
| Has ever worked in Nairobi | -0.00 (0.01) [1.00] | 0.03** (0.01) [0.02] |
| More than primary education | 0.00 (0.01) [0.76] | 0.03** (0.01) [0.02] |
| Above median household income | 0.00 (0.01) [0.95] | 0.04*** (0.01) [0.01] |
| Above median Nairobi connections | -0.01 (0.01) [0.66] | 0.03** (0.02) [0.03] |
| Mean (Engagement) | 8.37 | 0.09 |
| Observations | 10,540 | 1,924 |

Each cell triplet shows estimates from a regression of intervention engagement on a single binary predictor variable. Column 1 restricts the sample to the Mentor and Information arms; Column 2 restricts to the Group arm. Engagement is measured as the minutes spent on the 1-on-1 information meeting in Column 1 (estimated using Poisson regression), and 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 treatment (estimated using OLS). *Plans to use mentor program* is measured after treatment and the estimation sample includes the Mentor arm only. 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.13: Heterogeneous Group Impacts by Meeting Dynamics

| | Outcome: Sent New Migrants to Nairobi | | |
|---|---------------------------------------|-----------------------------|------------------------------|
| | Has Migrant in Nairobi | Has Worker in Nairobi | Ever Worked in Nairobi |
| <i>Leader and Household Interactions:</i> | | | |
| Leader of Type $X \times X \times \text{Group}$ | 0.13*** (0.03) [0.00] | 0.05* (0.03) [0.07] | 0.06* (0.03) [0.05] |
| Leader of Type $X \times \text{Group}$ | -0.02 (0.02) [0.32] | 0.01 (0.02) [0.42] | -0.00 (0.02) [0.98] |
| $X \times \text{Group}$ | 0.07*** (0.01) [0.00] | 0.06*** (0.01) [0.00] | 0.03*** (0.01) [0.00] |
| Group | 0.00 (0.01) [0.89] | 0.01 (0.01) [0.64] | 0.01 (0.01) [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 new 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.14: Treatment Impacts on Joint Migration

| | Co-Migrated to Nairobi | | Co-Migrated to Nairobi With More Experienced Migrant | |
|---------------------------------|-------------------------------|-------------------------------|---|-------------------------------|
| Info | 0.006*** (0.002) [0.01] | 0.007*** (0.002) [0.00] | 0.006*** (0.002) [0.00] | 0.007*** (0.002) [0.00] |
| Group | 0.004 (0.003) [0.16] | 0.002 (0.002) [0.25] | 0.002 (0.002) [0.31] | 0.002 (0.001) [0.26] |
| Mentor | 0.001 (0.002) [0.51] | 0.003 (0.002) [0.15] | 0.001 (0.002) [0.48] | 0.002 (0.001) [0.18] |
| Info \times Mig. In Nairobi | | -0.005 (0.005) [0.31] | | -0.004 (0.004) [0.39] |
| Group \times Mig. In Nairobi | | 0.008 (0.009) [0.40] | | 0.003 (0.008) [0.70] |
| Mentor \times Mig. In Nairobi | | -0.006 (0.005) [0.19] | | -0.003 (0.004) [0.45] |
| 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 *Mig. In Nairobi*, an indicator for whether the household had a migrant working in Nairobi at the census survey (coefficient on *Mig. In Nairobi* 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.15: Predictors of Return Migration

| | Intended Duration (Years) | | Intended Duration > 1 | |
|--------------------------------------|-----------------------------|---------------------------|-----------------------------|---------------------------|
| | Nairobi | Other Towns | Nairobi | Other Towns |
| Income (standardized) | 1.40*** (0.38) [0.00] | 0.45 (1.02) [0.66] | 0.07*** (0.01) [0.00] | -0.02 (0.03) [0.45] |
| Utility quality index (standardized) | 0.78 (0.84) [0.35] | 2.95 (2.02) [0.14] | 0.00 (0.04) [0.94] | 0.02 (0.05) [0.71] |
| Education (years) | 0.29* (0.16) [0.07] | 0.59 (0.41) [0.15] | 0.01 (0.01) [0.13] | -0.01 (0.01) [0.34] |
| Female | -0.47 (0.81) [0.56] | 0.63 (2.21) [0.78] | 0.00 (0.03) [0.90] | -0.01 (0.06) [0.86] |
| Age (years) | -0.04 (0.04) [0.34] | -0.02 (0.12) [0.85] | -0.00 (0.00) [0.92] | 0.00 (0.00) [0.41] |
| 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: Randomization Balance

| | Mean: Control | Mean: Info | Mean: Spillover | Mean: Group | Mean: Mentor | Joint p -Value |
|---|------------------|---------------|--------------------|----------------|-----------------|---------------------|
| Household size | 4.91 | 4.86 | 4.85 | 4.83 | 4.84 | 0.86 |
| # children under 5 | 0.73 | 0.75 | 0.69 | 0.75 | 0.71 | 0.27 |
| # adults 18-35 | 1.53 | 1.54 | 1.48 | 1.44 | 1.58 | 0.29 |
| Highest years of education | 10.36 | 10.42 | 10.45 | 10.27 | 10.55 | 0.56 |
| Any member has primary degree | 0.83 | 0.83 | 0.84 | 0.81 | 0.83 | 0.89 |
| Any member has secondary degree | 0.53 | 0.53 | 0.55 | 0.51 | 0.54 | 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 | 1.32 | 0.91 | 1.27 | 0.73 |
| Number of non-agricultural income sources | 0.62 | 0.60 | 0.60 | 0.41 | 0.57 | 0.62 |
| Income | 58.28 | 53.70 | 53.43 | 50.86 | 50.78 | 0.06 |
| Expenditure | 17.70 | 18.19 | 18.72 | 17.02 | 18.48 | 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 | 0.25 | 0.22 | 0.27 | 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 | 0.29 | 0.27 | 0.28 | 0.27 |
| Has migrant working in a city | 0.43 | 0.45 | 0.44 | 0.42 | 0.41 | 0.35 |
| Plans to migrate to Nairobi | 0.19 | 0.19 | 0.18 | 0.14 | 0.19 | 0.17 |
| Plans to migrate to any city | 0.23 | 0.22 | 0.20 | 0.17 | 0.21 | 0.09 |
| Perceived migration earnings | 145.45 | 146.89 | 141.22 | 133.39 | 142.08 | 0.13 |
| # of social contacts in Nairobi | 3.69 | 3.51 | 3.03 | 2.70 | 3.28 | 0.03 |
| # of origin connections (farm assistance) | 1.12 | 1.19 | 1.12 | 0.77 | 1.15 | 0.94 |
| # of origin connections (job advice) | 1.39 | 1.52 | 1.44 | 1.43 | 1.62 | 0.15 |
| Participates in village association | 0.71 | 0.70 | 0.72 | 0.62 | 0.68 | 0.46 |
| Village sociality index | 0.82 | 0.82 | 0.84 | 0.80 | 0.83 | 0.77 |
| Village trust index | 0.71 | 0.71 | 0.71 | 0.69 | 0.72 | 0.69 |
| Village financial reliance index | 0.63 | 0.62 | 0.62 | 0.61 | 0.63 | 0.38 |
| Village distance to Nairobi (km.) | 249.67 | 243.95 | 242.45 | 289.84 | 247.30 | 0.44 |
| Village distance to county capital (km.) | 44.14 | 43.52 | 46.30 | 48.49 | 44.18 | 0.99 |
| Village households | 100.15 | 104.20 | 101.15 | 101.16 | 103.42 | 0.44 |
| Village population | 428.01 | 442.14 | 440.54 | 442.90 | 451.00 | 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 |
|--------------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|
| Info | -0.01 (0.01) [0.33] | -0.01 (0.01) [0.32] | 0.00 (0.01) [0.98] | -0.03 (0.03) [0.22] | -0.02 (0.02) [0.48] |
| Spillover | -0.01 (0.01) [0.53] | 0.00 (0.01) [0.47] | 0.01 (0.01) [0.23] | -0.04 (0.03) [0.22] | -0.01 (0.03) [0.84] |
| Group | 0.00 (0.02) [0.78] | -0.01 (0.01) [0.11] | -0.00 (0.01) [0.86] | -0.05 (0.04) [0.21] | -0.04 (0.03) [0.13] |
| Mentor | 0.01 (0.01) [0.55] | -0.00 (0.01) [0.60] | -0.00 (0.01) [0.61] | 0.01 (0.03) [0.69] | -0.02 (0.02) [0.43] |
| 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 and when using 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.2 Robustness to Sample Selection and Estimation Strategy

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

| Migration: | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| Info | 0.01 (0.01) [0.20] | 0.02* (0.01) [0.09] | 0.00 (0.01) [0.82] | 0.04 (0.08) [0.62] | 0.04 (0.07) [0.57] | -0.05 (0.06) [0.41] |
| Group | 0.03** (0.01) [0.01] | 0.02* (0.01) [0.07] | 0.04** (0.02) [0.03] | 0.17** (0.09) [0.05] | 0.12 (0.08) [0.14] | 0.13* (0.08) [0.10] |
| Mentor | 0.02* (0.01) [0.05] | 0.02** (0.01) [0.04] | 0.02* (0.01) [0.08] | 0.14 (0.09) [0.12] | 0.04 (0.07) [0.57] | 0.10 (0.07) [0.13] |
| 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 |
| Info | 0.04 (0.03) [0.20] | 7.05* (3.80) [0.06] | 51.87* (30.28) [0.09] | 6.22 (4.24) [0.14] | 7.51** (3.81) [0.05] | 6.43 (4.17) [0.12] |
| Group | -0.03 (0.04) [0.43] | -3.48 (4.96) [0.48] | 10.21 (44.51) [0.82] | -1.43 (5.37) [0.79] | -3.38 (4.99) [0.50] | 1.01 (5.46) [0.85] |
| Mentor | 0.09*** (0.03) [0.01] | 7.88* (4.32) [0.07] | 65.92* (36.28) [0.07] | 9.65** (4.88) [0.05] | 8.84** (4.35) [0.04] | 7.91 (4.83) [0.10] |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |
| Control Mean | -0.02 | 100.82 | 698.70 | 122.33 | 100.65 | 121.41 |
| Observations | 15,232 | 15,232 | 15,231 | 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 Info and Spillover villages.

| Migration: | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| Info | 0.01 (0.01) [0.30] | 0.02 (0.01) [0.13] | 0.00 (0.01) [0.95] | 0.04 (0.08) [0.65] | 0.05 (0.07) [0.46] | -0.07 (0.07) [0.31] |
| Spillover | 0.02 (0.01) [0.13] | 0.02 (0.01) [0.17] | 0.01 (0.01) [0.58] | 0.06 (0.10) [0.50] | 0.00 (0.08) [0.99] | -0.00 (0.08) [0.99] |
| Group | 0.03** (0.01) [0.02] | 0.02* (0.01) [0.10] | 0.03** (0.02) [0.04] | 0.17* (0.09) [0.06] | 0.11 (0.08) [0.18] | 0.12 (0.08) [0.11] |
| Mentor | 0.02** (0.01) [0.05] | 0.02** (0.01) [0.04] | 0.02* (0.01) [0.08] | 0.14 (0.09) [0.11] | 0.04 (0.07) [0.56] | 0.11 (0.07) [0.12] |
| 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- Adjusted Income |
| Info | 0.03 (0.03) [0.28] | 8.21** (4.03) [0.04] | 42.63 (31.56) [0.18] | 6.99 (4.52) [0.12] | 8.66** (4.06) [0.03] | 7.96* (4.49) [0.08] |
| Spillover | 0.05 (0.04) [0.25] | 3.57 (5.28) [0.50] | 76.22 (47.48) [0.11] | 3.76 (5.80) [0.52] | 3.99 (5.30) [0.45] | 2.04 (5.46) [0.71] |
| Group | -0.04 (0.04) [0.33] | -4.79 (4.95) [0.33] | -1.69 (44.79) [0.97] | -2.71 (5.37) [0.61] | -4.73 (4.98) [0.34] | -0.53 (5.44) [0.92] |
| Mentor | 0.10*** (0.03) [0.00] | 7.99* (4.24) [0.06] | 68.19* (35.85) [0.06] | 9.88** (4.81) [0.04] | 8.93** (4.27) [0.04] | 8.02* (4.75) [0.09] |
| Model | | | | | | |
| Control Mean | -0.03 | 99.70 | 690.69 | 120.95 | 99.54 | 120.21 |
| Observations | 16,339 | 16,339 | 16,338 | 16,339 | 16,339 | 16,339 |

Treatment impact coefficients estimated separately for households in Info villages and treated households in Spillover villages. 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: | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| Info | 0.01 [0.19] | 0.02* [0.10] | 0.00 [0.82] | 0.04 [0.56] | 0.04 [0.55] | -0.05 [0.13] |
| Group | 0.03** [0.02] | 0.02* [0.11] | 0.03** [0.06] | 0.17* [0.09] | 0.11 [0.22] | 0.12 [0.21] |
| Mentor | 0.02** [0.05] | 0.02** [0.04] | 0.02* [0.08] | 0.14 [0.11] | 0.04 [0.55] | 0.11 [0.05] |
| 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- Adjusted Income |
| Info | 0.04 [0.25] | 7.08* [0.08] | 51.00* [0.11] | 6.22 [0.17] | 7.53** [0.07] | 6.56 [0.14] |
| Group | -0.04 [1.00] | -4.51 [1.00] | 1.38 [0.98] | -2.43 [1.00] | -4.42 [1.00] | -0.47 [1.00] |
| Mentor | 0.10*** [0.01] | 8.09* [0.07] | 68.01* [0.06] | 10.00** [0.04] | 9.03** [0.04] | 8.13* [0.10] |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |
| Control Mean | -0.03 | 99.70 | 690.69 | 120.95 | 99.54 | 120.21 |
| Observations | 15,468 | 15,468 | 15,467 | 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: | Sent New Migrants to Nairobi | Sent Any Migrants to Nairobi | Sent New Migrants to Any City | # New Migrants in Nairobi | # Any Migrants in Nairobi | # New Migrants in Any City |
|-------------------|------------------------------------|------------------------------------|-------------------------------------|---------------------------------|---------------------------------|----------------------------------|
| Info | 0.01 (0.01) [0.25] | 0.02 (0.01) [0.11] | -0.00 (0.01) [0.93] | 0.01 (0.01) [0.35] | 0.01 (0.02) [0.33] | -0.01 (0.02) [0.61] |
| Group | 0.02** (0.01) [0.05] | 0.02 (0.01) [0.24] | 0.03 (0.02) [0.11] | 0.02* (0.01) [0.08] | 0.02 (0.02) [0.41] | 0.03 (0.02) [0.19] |
| Mentor | 0.01 (0.01) [0.11] | 0.02** (0.01) [0.05] | 0.02 (0.01) [0.16] | 0.02 (0.01) [0.18] | 0.00 (0.02) [0.76] | 0.02 (0.02) [0.31] |
| 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- Adjusted Income |
| Info | 0.03 (0.03) [0.23] | 7.86** (3.56) [0.03] | 54.07* (29.80) [0.07] | 5.88 (4.01) [0.14] | 7.72** (3.62) [0.03] | 6.64* (3.80) [0.08] |
| Group | -0.06* (0.04) [0.09] | -5.19 (4.80) [0.28] | -8.35 (44.13) [0.85] | -5.32 (5.32) [0.32] | -6.03 (4.89) [0.22] | -2.25 (5.03) [0.65] |
| Mentor | 0.09*** (0.03) [0.01] | 7.76* (4.01) [0.05] | 60.83* (35.94) [0.09] | 8.19* (4.55) [0.07] | 7.90* (4.11) [0.05] | 8.71** (4.27) [0.04] |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |
| Control Mean | -0.03 | 99.70 | 690.69 | 120.95 | 99.54 | 120.21 |
| Observations | 15,468 | 15,468 | 15,467 | 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.3 Intervention Details

B.3.1 Information Scripts (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 earning 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.3.2 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.3.3 Mentor Script (Used in Mentor Villages)

[The following script was delivered after the information script of Section B.3.1.]

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.3.4 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.3.5 Information Sheets

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

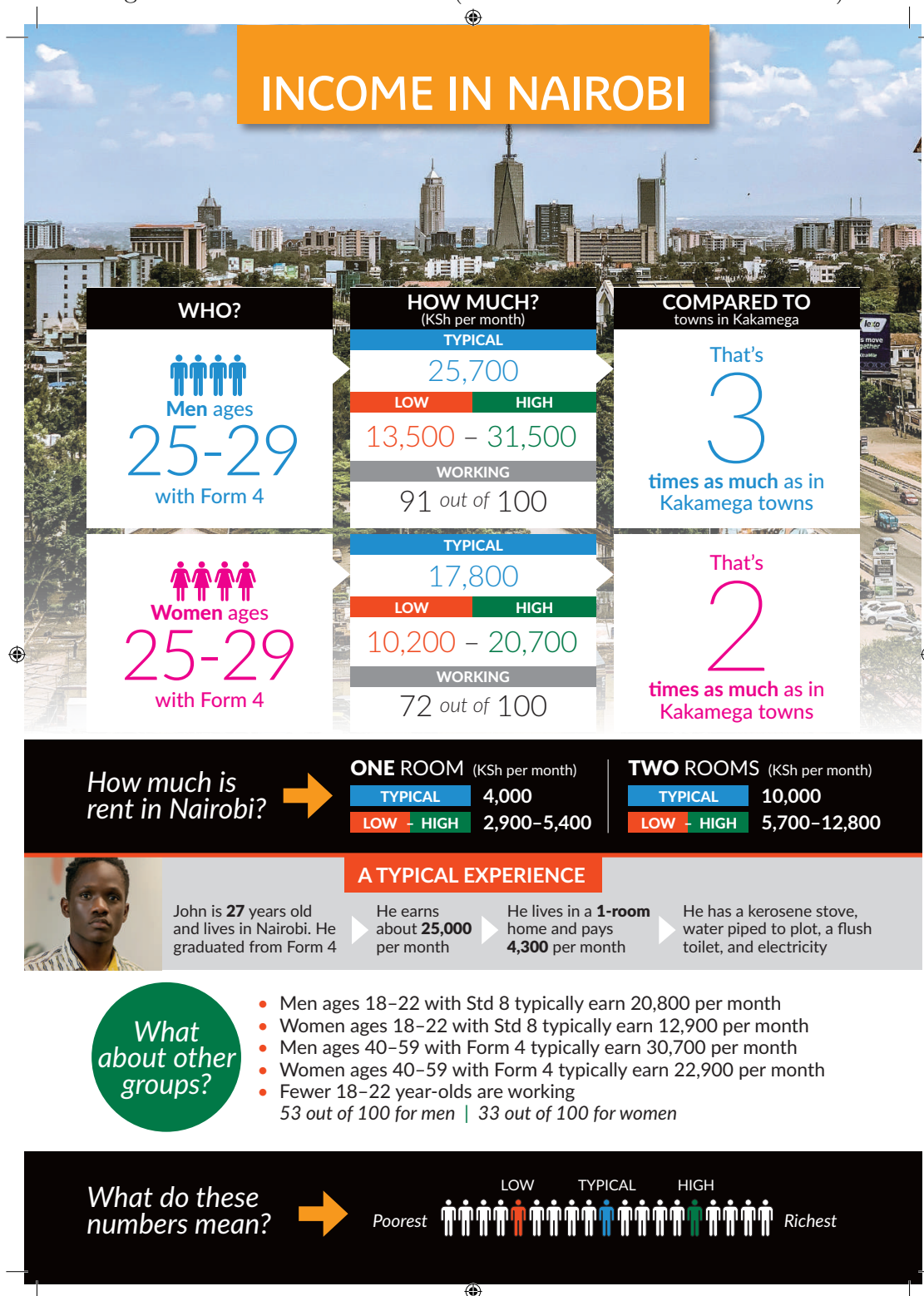
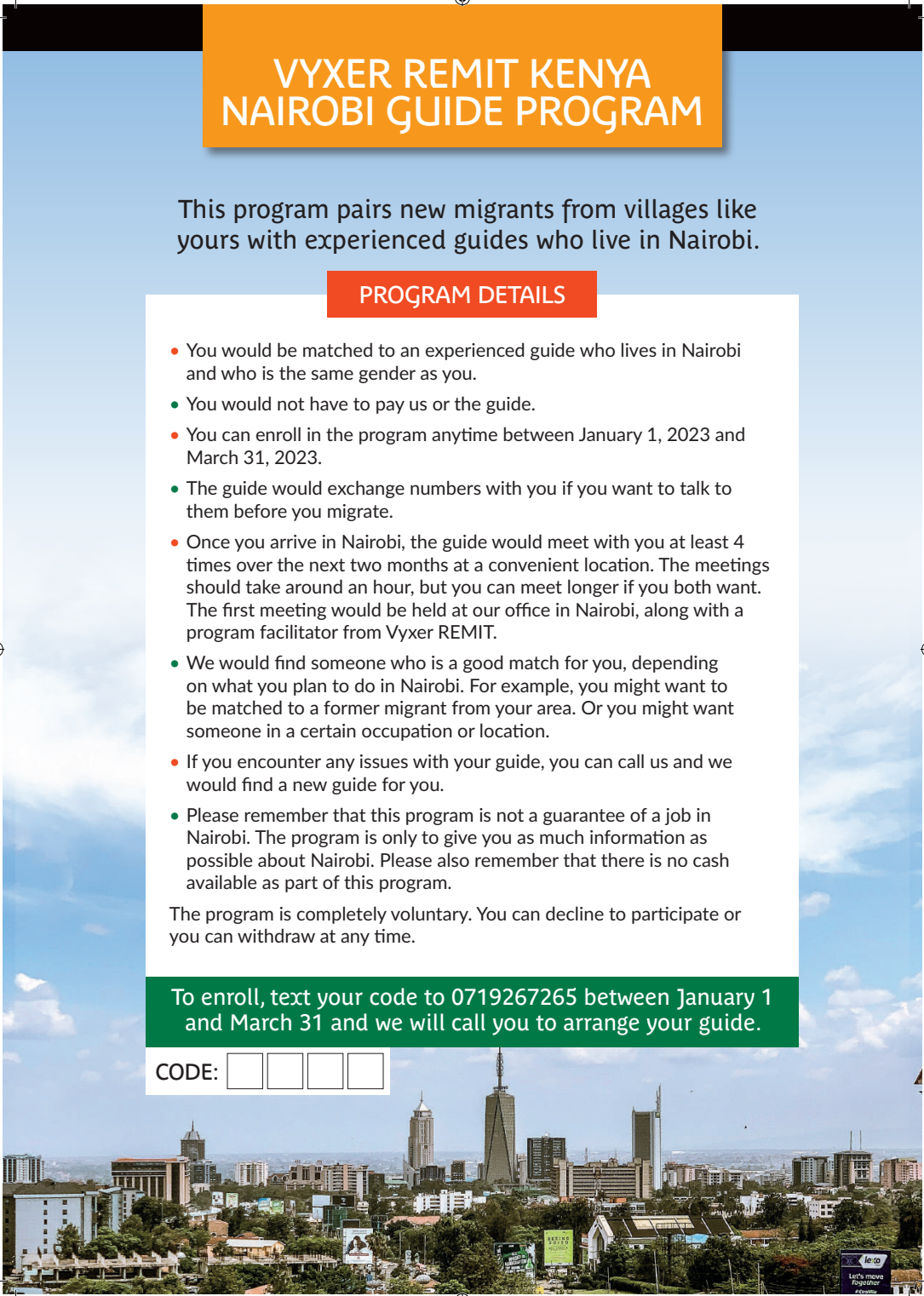


Figure B.2: Mentor Program Description (Given to All Households in Mentor Villages)



**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 Details: Income, Prices, and Amenity Adjustments

This appendix provides additional details on our measurement methodology, including how we compute family income, adjust income spent in Nairobi to account for price differences, and compute amenity-adjusted income that accounts for non-pecuniary amenity differences across space.

C.1 Family Income

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

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

where $\text{Village Profit}_{ib}$ 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 $\text{Migrant Profit}_{im}$ 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 $\{\text{business}\}$? 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 $\{\text{name}\}$ earned from all their businesses in $\{\text{city}\}$? 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 $\{\text{name}\}$ earn from any job, including casual jobs, in the past 30 days? Do not include profits from businesses that $\{\text{name}\}$ 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.

C.2 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 :

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

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.

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.3 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 in 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.1: 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.4 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 $\{\text{baseline.location}\}$, if you could earn it while living there? As a reminder, you told me that your current income is $\{\text{income}\}$ 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.2 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.2: Validating Amenity Adjustments

| | Outcome: Rural indifference income | | | | |
|---|------------------------------------|-----------------------------|-----------------------------|-----------------------------|-----------------------------|
| Earned urban income | 0.21*** (0.01) [0.00] | 0.21*** (0.01) [0.00] | 0.21*** (0.01) [0.00] | 0.19*** (0.02) [0.00] | 0.16*** (0.02) [0.00] |
| Prefer life in Nairobi to hometown | | 0.07** (0.03) [0.01] | 0.06** (0.03) [0.04] | 0.04 (0.03) [0.14] | 0.04 (0.03) [0.18] |
| Happy with life | | | 0.11*** (0.03) [0.00] | 0.08*** (0.03) [0.01] | 0.07*** (0.03) [0.02] |
| Utility quality index (standardized) | | | | 0.05*** (0.02) [0.00] | 0.06*** (0.02) [0.00] |
| Intended duration in Nairobi (standardized) | | | | 0.05*** (0.02) [0.00] | 0.05*** (0.02) [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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.5 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. 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 it doesn't migrate, 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.³⁰ We assume that f is linear in the share of migrants in i 's village, and 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).

Taking means by treatment group and letting β^{AT} and β^C denote the average returns to migrating for always-takers and compliers respectively (and similarly for γ^{AT} and γ^C) gives:

$$\begin{aligned} E[Y_i|Info] &= E[\alpha_i|Info] + (\beta^{AT} + \gamma^{AT}) \times E[Mig_i|PureControl] \\ &\quad + (\beta^C + \gamma^C) \times (E[Mig_i|Info] - E[Mig_i|PureControl]), \end{aligned} \quad (4)$$

$$\begin{aligned} E[Y_i|Control] &= E[\alpha_i|Control] + (\beta^{AT} + \gamma^{AT}) \times E[Mig_i|PureControl] \\ &\quad + \gamma^C \times (E[Mig_i|Spillover] - E[Mig_i|PureControl]), \end{aligned} \quad (5)$$

where *Info* refers to households in Information villages, *PureControl* to those in Pure Control villages, *Spillover* to treated households in spillover villages, and *Control* to untreated households in spillover villages. We exclude the direct benefits of migrating from (5) because we find zero indirect impact on migration. Subtracting (5) from (4), plugging in $E[Mig_i|Info] = E[Mig_i|Spillover]$ (Appendix Table A.4), and noting that $E[\alpha]$ is balanced across treatment groups by random assignment gives:

$$E[Y_i|Info] - E[Y_i|Control] = \beta^C \times (E[Mig_i|Info] - E[Mig_i|PureControl]).$$

Finally, plugging in the direct treatment impact on income from Table 4 (\$7.1/month), the indirect income impact from Appendix Table A.8 (\$4.5/month), and the migration from Table 3 (0.02) gives an estimated marginal return to migrating of \$130/month. This estimate is in line with the typical income differences between Nairobi and rural Kenya (see Appendix Figure B.1), and implies that—averaging over an entire village—direct returns to migrating represent 37%, and indirect returns 63%, of total income gains.

³⁰For 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.

D Model Details

D.1 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. 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:

$$\frac{1}{1 + \gamma_x} \frac{1}{1 + m_g} z_{u,g}^n w_u^n = \max\{z_{r,g}^n w_r^n, z_{r,g}^a w_r^a\}, \quad (6)$$

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 , $z_{u,g}^n$, $z_{r,g}^n$, and $z_{r,g}^a$ are group-level migration costs and productivities, which we assume to be constant within group.³¹ 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 5.2, 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).

D.2 Proofs

Proof of Proposition 1

Coming soon!

E Other Pre-Specified Results

³¹To see how (6) 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.