The Role of Information and Networks in 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. This paper evaluates one potential explanation: information frictions about distant locations. Using novel data from over 50,000 Kenyan households and a randomized controlled trial, we characterize the state of information frictions and the value of interventions designed to reduce them. We find evidence of large, pervasive frictions: rural workers underestimate conditional earnings in the capital city by 30–60% on average. Providing unbiased information about urban earnings increases migration and improves economic outcomes. Expanding destination social networks amplifies these effects, while providing information through origin networks worsens economic impacts compared to individualized dissemination by screening out low-experience but high-return migrants.

JEL Classifications: C93, J31, J61, J82, O15, O18, R23
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1 Introduction

Deciding whether to migrate is among the most consequential decisions a person can make. At the same time, a growing literature finds that migration decisions are often made with little information (McKenzie et al., 2013, Shrestha, 2020, Bazzi et al., 2021, Baseler, 2023). When experimenting with migration is costly, prospective migrants may need to 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, Blumenstock et al., 2023). Given the substantial spatial income gaps present throughout nearly all low-income countries, the question of whether low-quality information keeps internal migration rates inefficiently low has major implications for aggregate economic efficiency and thus for economic policy (Gollin et al., 2014, Lagakos, 2020).

The question of whether spatial income gaps reflect market inefficiencies—and thus whether these gaps represent arbitrage opportunities for marginal migrants—remains unsettled. Several papers have studied this question by attempting to randomly alleviate migration barriers. Bryan et al. (2014), Akram et al. (2017), and Baseler (2023) find positive marginal returns to migrating: the first two identify these by relieving financial constraints, while the third does so by relieving information constraints.[1] In contrast, large-scale observational evidence shows strong evidence of spatial sorting and small observational returns to migrating (Young, 2013, Lagakos and Waugh, 2013, Hicks et al., 2018). There are at least two ways to reconcile these contrasting results. One possibility is that the experimental evidence does not generalize to large populations, possibly because experimental samples may be selected on gains (Gechter et al., 2024). Another is that migration costs and benefits are heterogeneous, implying that observational returns are not fully informative of the gains from reducing migration costs (Lagakos et al., 2020).

This paper makes progress toward reconciling these two strands of research by randomly relieving information constraints in a population-representative sample of rural households

[1] Following the literature, we use the term *marginal migration* to refer to migration induced by an intervention that reduces migration costs. Other 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.

across five Kenyan counties. We first collected data on beliefs about labor market conditions 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 for the majority of households.

Next, we conduct a cluster-randomized controlled trial with about 17,000 of these households. In villages assigned to the *Information* arm, we offer correct information about Nairobi during household visits. To study the role of networks in lowering migration barriers, we included two additional treatment arms facilitating social network connections either at the origin or the destination. In the *Group* treatment, households received the same information conveyed in the Information arm, but the information delivery took place during a village-level meeting in which former migrants were encouraged to answer questions from prospective migrants and groups of prospective migrants were encouraged to consider coordinating their trips. Given that information does not efficiently diffuse throughout villages (Baseler, 2023), origin network-based facilitation may improve information exchange. In *Mentor* villages, households again received the same information in the Information arm, plus an offer to be matched to a resident of Nairobi who had agreed to provide individualized information about migrating over the phone and later meet them in Nairobi in person. This design was motivated by research showing that 1–1 mentorship pairings can facilitate information exchange and improve economic outcomes (Brooks et al., 2018, Baseler et al., 2023b). We followed up with this sample around eight months and again around 16 months after the interventions took place to measure beliefs, migration decisions, and economic outcomes.

To study how new information about the returns to migrating disseminates through villages and to assess the spillover impacts of our programs, we randomly assigned a set of villages to a *Spillover* arm, in which two-thirds of households were randomly assigned to receive the same information as households in Information villages, while the other third of households was not given the information. Finally, we included a set of *Control* villages in which households did not receive any information or network intervention.

We find that information gaps are substantial and pervasive: on average, respondents under-estimate typical incomes in the capital by 30–60% for a broad range of demographic groups. Moreover, about half of rural households report having no social connections in their village they could go to for advice on migrating, and a similar share report having no connections in Nairobi they could rely on for migration support. Providing correct information about earnings in Nairobi immediately increases beliefs about the returns to migrating and aspirations to migrate, and facilitating network connections either at the destination or origin roughly doubles these impacts. Over the following 16 months, the share of households sending migrants to Nairobi increased by roughly 2 percentage points (pp.) on a base of

13%—a roughly 15% effect size—with similar impacts across treatment arms. While these changes are significant relative to the relatively low level of baseline migration, they are small compared to the number of non-migrating households, suggesting that information and network connections of the type we study are not the primary reason that most households do not migrate.

We find large economic returns from our Information and Mentor arms: in these groups, income is roughly 8% higher 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. To explain this surprising result, we show that disseminating information through groups creates adverse selection into treatment intensity. Program data show that group dissemination favored experienced migrants: households with members already in the capital were much more likely to be engaged in group-level discussions, but no more engaged in the 1–1 discussions used in the Information and Mentor arms. As a result, migrants from Information and Mentor villages are more likely to be inexperienced with migrating compared to those from Group villages. We argue that the lower economic impacts of the Group treatment are consistent with a Roy model in which more experienced non-migrants face lower migration costs, and thus have lower migration benefits in equilibrium. Together, these findings imply that individualized information, such as that provided in household-level meetings or through a mentor, can be more successful at inducing high-return migration when those with high migration costs select out of group-level dissemination.

To identify the spillover effects of providing correct information on untreated households, we compare untreated households in Spillover villages to households in Control villages. We find little to no diffusion of information: beliefs, aspirations, and migration are unaffected for the neighbors of treated households. However, we find substantial economic spillovers: neighbors of treated households report higher income one year after the intervention. These spillovers are consistent with general equilibrium impacts of migration through labor markets (Akram et al., 2017) or through demand multiplier effects (Egger et al., 2022).

Our findings imply that a subset of non-migrating households experiences substantial returns from lowering migration costs through better information or additional social networks 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., 2025), could help rural workers make more informed migration choices. However, the adverse selection problem documented under group dissemination implies that making information widely available may not improve outcomes when enthusiastic but low-return households dominate the discussion. 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 substantial literature has identified frictions distorting internal migration decisions, including poor information (Baseler, 2023), financial constraints (Bryan et al., 2014, Cai, 2020), 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., 2023a). To our knowledge, we provide the first experimental test of internal migration barriers in a large, population-representative sample. In doing so, we help reconcile the large frictions identified in experimental studies (Bryan et al., 2014, Baseler, 2023, Miner, 2024) with population-wide, non-experimental studies finding evidence in favor of efficient spatial sorting (Young, 2013, Hicks et al., 2018). We argue that our findings are intermediate between these two strands of research: while the share of the population identified as constrained is lower than in many of the smaller experimental studies, this sub-population exhibits substantial returns to migrating, allowing us to reject the efficiency hypothesis. Our findings are consistent with the macro-development literature finding that sorting explains a significant share—but not the entirety—of observed sectoral productivity gaps (Lagakos and Waugh, 2013, Gollin et al., 2014, Bryan and Morten, 2019).

A large literature studies 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 less familiar with labor markets and have fewer urban social connections compared to native-born urban residents on average. 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 social networks from migrating. We contribute to this literature by testing two new programs designed to expand social networks either at the origin or destination. We find that facilitating new social network connections does not increase average migration beyond the impacts of information provision alone, but does influence who migrates. Our findings

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

are consistent with high returns to light-touch, structured interactions between urban and rural residents.

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 adverse selection problem when baseline knowledge is negatively correlated with potential gains, and argue that these findings are consistent with a Roy model in which high-knowledge non-adopters have low returns to adopting in equilibrium.

Finally, we make two methodological contributions to the welfare analysis of interventions under incomplete markets (Benjamin, 1992, LaFave and Thomas, 2016). Evaluating welfare gains from a policy change often requires estimating shadow prices that are imperfectly captured by market prices, such as the value of non-pecuniary amenities (Gollin et al., 2017) or non-labor time (Agness et al., 2025). Similarly, valuing the returns to migrating requires accounting for price differences, 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 test simple, survey-based approaches to valuing amenity and price differences across space. 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 high-quality, item-level consumption data are available, whether available externally or collected by the researcher.

2 Study Design

This section describes the setting of the study, the process by which the sample was selected to represent county-level rural populations, the data collection timeline, and the information interventions.

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.

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 sets 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 density distribution (population per square kilometer).[3]

2. *Village Selection.* We randomly selected one enumeration area (or 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.

[3]Excluding the 10% highest-density sub-locations excludes urban centers. Excluding the bottom 5% reduces the cost of data collection, as some sub-locations are very sparse.

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 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.[4] 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 more thorough 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 that 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.4.

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

[4] The across-village correlation coefficient between the household count in our data and in KNBS's is 0.67.

within one year.

Attrition and tests of randomization balance within the set of surveyed households are presented in Section 2.7.

2.4 Summary Statistics

Table 1 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.[5] 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 18% for stayers, 34% for migrants in rural areas, and 60–61% 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.

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 earnings in Nairobi. When they received the sheet, they also heard a detailed script read by the enumerator explaining where the information on the sheet came from and how to interpret it.

2. In *Spillover* villages, we randomly selected two-thirds of sampled households to receive 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+script) as the "Information" households.

[5] This 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.

Table 1: Summary Statistics

	Stayers	Migrants in Rural Areas	Migrants in Small Cities	Migrants in Nairobi	N
Age (years)	37.02	31.72	32.05	31.81	60,833
	(0.32)	(1.29)	(0.81)	(0.58)	
Female	0.53	0.63	0.51	0.37	60,833
	(0.01)	(0.03)	(0.10)	(0.03)	
Household head	0.57	0.04	0.11	0.12	60,833
	(0.01)	(0.02)	(0.03)	(0.02)	
Education: Primary or Less	0.42	0.36	0.32	0.35	60,527
	(0.01)	(0.05)	(0.07)	(0.03)	
Education: Secondary	0.22	0.19	0.41	0.42	60,527
	(0.01)	(0.05)	(0.09)	(0.03)	
Education: Greater Than Secondary	0.06	0.11	0.10	0.12	60,527
	(0.01)	(0.03)	(0.02)	(0.02)	
Employed	0.18	0.35	0.55	0.67	57,401
	(0.01)	(0.03)	(0.03)	(0.03)	
Employed in non-agriculture	0.12	0.28	0.46	0.61	57,401
	(0.01)	(0.03)	(0.03)	(0.03)	

Sample includes all family members ages 16 or older in the baseline data. *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 Nariobi. Column 5 shows observations summed across the mutually exclusive column samples. Standard errors in parentheses account for survey sampling methodology.

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 was given the same information as in the "Information" villages, and were given an offer to be paired with an experienced resident in Nairobi who agreed to serve as a local guide. We identified local residents who are established in Nairobi and fit the profile desired by the migrant (for example, having experience in the occupation the migrant wants to work in). The guides offered to speak with the prospective migrants over the phone prior to migrating, or meet them in Nairobi once they arrive. In Guide + Information villages, guides were available starting in January 2023, shortly after the baseline surveys concluded, and the program was open for enrollment for three months.

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

Content of the Information Intervention. We gathered the information delivered through our intervention from the Kenya Integrated Household Budget Survey of 2015–2016, a household survey representative at the county level. Using these data, we created information sheets containing the median incomes for several demographic groups, the ratio of median income to the same statistic for the largest city in their home county, "low" and "high" incomes for some demographic groups (corresponding to the 25th and 75th percentiles of the conditional income distribution, respectively), employment rates by group, typical and low/high rental prices for one-bedroom and two-bedroom units, and the most commonly used form for several utilities (cooking fuel, water, toilet, and electricity).[6] 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 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.

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 script. Attendance at the village-level meetings in villages assigned to Group was 88% of the invited sample.[7] 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.

[6] We measured individual income by adding individual wage income to enterprise profits (as this variable is measured at the household level in the KIHBS survey, we distributed it evenly across all members listed as entrepreneurs) and profits from livestock rearing, crop sales, net transfers, and other income (we distribute these household-level variables evenly across all adult members) and 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. We estimate conditional quantiles using recentered influence functions through the Stata command *rifhdreg*.

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

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:

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

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.[8] We estimate treatment impacts using 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:

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

We estimate analytical standard errors and p-values accounting for the clustered treatment assignment. For main results, we also present finite-sample randomization-inference p-values in Appendix B.2. We obtain these by permuting 2,000 treatment assignments following the true assignment method and retaining only balanced permutations.[9] We permute only those treatment assignments being compared in a given hypothesis, holding the others fixed. Results estimated through randomization inference and regression-based methods are very similar, as shown in Appendix Table B.6.

Pooling treatments to improve statistical power. When analyzing impacts of the Information treatment, we pool households in Information villages with those assigned to

[8]Relative to our pre-analysis plan, this equation omits the variable $M_{iv,pre}$, an indicator for a missing value of $\bar{y}_{iv,pre}$. This is because there are no cases in our data of missing $\bar{y}_{iv,pre}$ when y_{ivt} is non-missing. It also omits a lasso-selected control vector X_i following recent evidence from Cilliers et al. (2024) that this procedure offers little expected gain in statistical power—especially when pre-treatment outcome values are available and attrition rates are low, as in our case—at the risk of over-fitting. We show in Appendix Table B.7 that results estimated following the double-lasso procedure of Belloni et al. (2014) are very similar.

[9]Specifically, we discard any permutation for which the null hypothesis of joint equality across treatment groups is rejected at the 10% level for more than three out of 32 tested pre-treatment variables, matching the level of balance achieved in the true treatment assignment (see Appendix Table B.1).

receive information in Spillover villages.[10] 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 Table A.3, 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.5.

2.7 Randomization Balance and Attrition

Appendix Table B.1 shows tests of randomization balance in the full sample. Across 32 pre-treatment characteristics, we reject the null hypothesis of equality across all treatment arms at the 10% level for 3, consistent with expectation under successful randomization.

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%.

Appendix Table B.2 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 64 tests, indicating no significantly differential attrition along these dimensions. For the endline and endline + indirect sample, the analogous statistics are 5 out of 64 and 4 out of 64 tests rejected at the 10% level, again consistent with no significant attrition bias.

Appendix Table B.3 tests whether random assignment to treatment within Spillover villages achieved balance, both in the full sample and among those successfully surveyed by wave. Across 200 tests (25 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 22, consistent with expectation under successful randomization.

[10] In these specifications, we exclude households in Spillover villages assigned to not receive information. Pooling them with Information villages is only appropriate under perfect transmission of information within villages. Pooling them with Control villages is only appropriate under the assumption of no spillover effects within villages. Both of these assumptions are clearly rejected by the results shown in Appendix Table A.9 and discussed in Section 4.7.

3 The State of Information Gaps

We begin by characterizing 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 local reference town, chosen to be the largest urban center 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, $\widehat{Y}_j^N/\widehat{Y}_j^L$, expressed as a multiple of perceived income for the same group in the local reference town. To assess information gaps, we compare these perceived premiums to the true premiums Y_j^N/Y_j^L estimated on survey data from Nairobi residents.

There is remarkably little variation in perceived premiums, but substantial variation in true premiums, across demographic groups and home counties, as shown in Figure 1. 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.[11] 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 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 the local town.[12]

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 use this variation in intensity of treatment to test whether potential migrants facing larger preexisting information gaps are more responsive to new information.

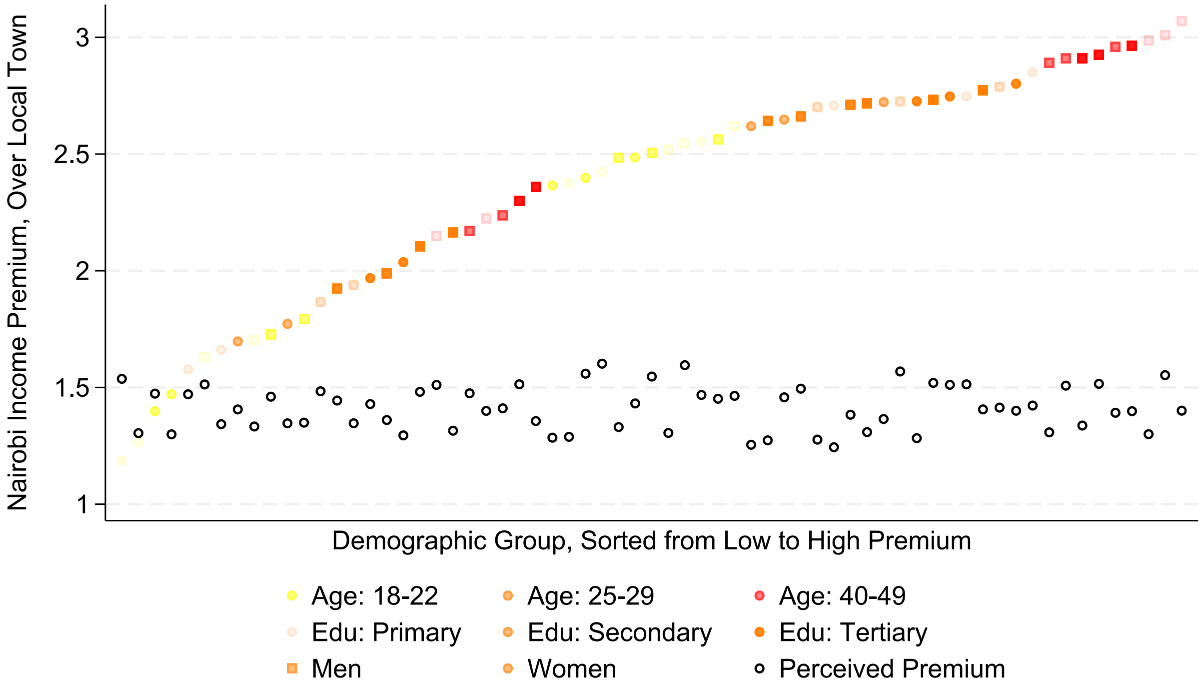
4 Experimental Impacts

In this section we describe the experimental impacts of relieving information constraints and facilitating network connections.

[11] Moreover, the 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.

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

Figure 1: True and perceived income premium in the capital, by demographic group



Each dot shows the average income in the capital city, Nairobi, divided by the average income in the largest town in the respondent's home county for a given demographic group defined by age, educational attainment, gender, and home county. Colored dots show true incomes earned by members of that group estimated using recentered influence function (Rios-Avila, 2020) regression on Kenya Integrated Household Budget Survey data from 2015–2016. Black dots show perceived incomes earned by members of that group, 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?"

4.1 Impacts on Beliefs and Aspirations

The information treatment significantly increases rural households' expected migration income and migration plans, as shown in Table 2. Planned migration to Nairobi 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 74%, $p < 0.01$). 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 group than Control, 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 Migration Income Growth
Disaggregated						
Info	0.04***	0.05***	0.04**	0.07***	0.03	27.93***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(10.42)
	[0.00]	[0.00]	[0.04]	[0.00]	[0.18]	[0.01]
Group	0.10***	0.10***	0.13***	0.17***	0.08***	33.71**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(14.04)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]
Mentor	0.10***	0.11***	0.11***	0.15***	0.09***	-5.32
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(10.58)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.62]
Control Mean	0.27	0.34	130.50	92.20	185.30	38.01
Observations	15,468	15,468	14,862	14,856	14,858	14,781
Pooled Treatment						
Any Info	0.07***	0.07***	0.07***	0.10***	0.05***	18.94**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(8.94)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.03]
Observations	15,468	15,468	14,862	14,856	14,858	14,781

Impacts are estimated on data from basleline 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. Responses of "Don't Know"are coded as missing. 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

Providing better information and facilitating network connections increases migration to the capital after 16 months, as shown in Table 3. Households in Information villages were 1 pp.

more likely to have sent 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 have sent 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, estimated by Poisson regression, are directionally similar but noisy. Treatment did not increase migration as of the 8-month midline survey, as shown in Appendix Table A.4, 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 impacts across treatment arms, we can improve power and reduce the risk of multiple hypothesis testing 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 ($p = 0.39$).

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. Specifically, 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 demean *Prior Gap* so that its estimated coefficient is interpretable as the impact of treatment for those with correct priors. 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 are only roughly 25–30% of treatment impacts on planned migration to Nairobi. 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 households expressed a plan to send a new migrant to Nairobi within one year, while only 13% actually did so.[13] While this plan–do gap may partly reflect temporary shocks—the period between the intervention and endline survey was marked by unusually high political uncertainty in Kenya—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 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 ($p < 0.01$). Household income earned in the month before the survey—inclusive of wage income and business profits across all household members—is higher by \$8 on a base of \$105 ($p = 0.06$). 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 \$60 on a base of \$672 ($p = 0.07$). Adding estimated crop profits does not change these results (coeff. = \$10 in the past month on a base of \$127, $p = 0.04$).

If migrants face higher prices for the same goods in Nairobi or other cities, impacts on nominal income will incorrectly measure impacts on real standards of living. A typical solution is to deflate nominal income by price indices measuring the cost of a representative

[13] The 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 stongly correlated.

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 by KNBS (our method is described in detail in Appendix C). 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 (\$8/month increase in Mentor villages, $p = 0.05$).

Spatial income differences may in part reflect compensating differentials—such as access to amenities like public utilities, education, and healthcare—though Gollin et al. (2017) 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 living in their destination city and their former place of residence (as of the baseline survey). We then assign them their "indifference income" when computing amenity-adjusted family income (details in Appendix C). We find that adjusting for subjective amenity differences does not substantially affect our estimates (coeff. = \$8/month in Mentor villages, $p = 0.10$), implying that urban migrants perceive the quality of life in the city to be similar to that in the village, net of income differences.

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 Control, by \$7 ($p = 0.06$) while income over the past year is higher by \$54 ($p = 0.06$). The overall welfare index is higher by 0.04 standard deviations ($p = 0.18$). However, impacts in Group villages are substantially lower than either Information or Mentor villages. In Group villages, impacts on each income measure are directionally negative but statistically zero. In Sections 4.4 and 4.5, we show that, relative to the Information and Mentor treatments, the Group treatment induced migration among households more experienced with migrating. In Section 4.6, we argue that the Group treatment amplified existing misperceptions about Nairobi, inducing migration from households with low benefits from migrating compared to those induced by the Information and Mentor treatments.

Other economic outcomes. We do not detect significant changes in consumption over the past month, investment in businesses and the household over the past year, access to improved utilities, or subjective well-being. Savings over the past month is much higher in Information and Mentor villages, by 27% and 34% respectively (p-vals < 0.01). Subjective financial health improves in Information villages by 1 pp. ($p = 0.06$) and in Mentor villages by 2 pp. ($p = 0.01$). 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 How Does Treatment Affect Migrant Selection?

A potential explanation for the greater income effects from the Mentor compared to the Group treatment, given similar impacts on migration in these two groups—is that information conveyed by mentors induced high-value migration while information conveyed in groups induced low-value migration. To test this hypothesis, we estimate (1) on the sample of households that sent migrants to Nairobi as of the endline survey to test whether selection into migration changed as a result of treatment. Appendix Table A.5 presents results. 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. Information conveyed by mentors appears to be a substitute for existing origin networks: compared to control, migrating households in Mentor villages have fewer social connections in their village from whom they can get advice on migrating. While information conveyed in the Group treatment also appears to be a substitute for existing origin advice connections, migrants from Group villages are more likely to be experienced at migrating (as measured by an indicator for a household member having migrated to Nairobi in the past, or an indicator for having a migrant in Nairobi as of the census).

4.5 Treatment Heterogeneity: Can Better Information Be Targeted?

The Information treatment is more impactful among poor households, as shown in Appendix Table A.6, which estimates heterogeneous treatment impacts on new migration to Nairobi and household income. Among households with below-mean baseline income, the Information treatment increases new migration to Nairobi by 3 pp. (on a base of 11%, effect size = 27%, $p < 0.01$) and household income by about 10% ($p = 0.02$). The information treatment reduces migration among above-mean income households (by 4 pp., effect size = -22%, $p = 0.01$) without impacting income (3% change, $p = 0.57$). High-income households hold higher prior beliefs about Nairobi income on average, suggesting that better information may have dissuaded low-return, high-income migrants. These patterns are similar for Mentor and Group treatments.

The Information treatment is also more impactful among inexperienced households, defined as those without a migrant in Nairobi before treatment (impact on migration = 2 pp., $p = 0.03$; impact on income = 9%, $p = 0.06$). Impacts on households with existing Nairobi migrants are close to zero. While this pattern is similar for the Mentor treatment, it is reversed in Group, where high-experience households were more responsive (impact on migration = 7 pp., effect size = 33%, $p = 0.01$; impact on income = −6%, $p = 0.38$). Consistent with the selection patterns documented in Appendix Table A.5, this suggests that the Group treatment induced migration largely among low-return, experienced households.

Treatment impacts on migration are higher among households with few existing origin connections that they say could support their migration through information or financial assistance. Patterns are similar when comparing households with above- or below-median destination connections, though these differences are generally smaller. Impacts on income are greater among households with few destinations connections, especially in Information, where the impact on income is 12% ($p < 0.01$). Overall, these patterns imply that new information is a substitute for existing origin and destination networks.

4.6 What Explains Worse Outcomes of the Group Treatment?

Overall, the above results suggest that mentors persuaded those with little experience and who were not planning on migrating—but who were foregoing substantial benefits by not migrating—to migrate. In contrast, village-level meetings in Group villages led more experienced migrants with lower returns 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.

To test this hypothesis, we leverage program data collected during group meetings and 1–1 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.

Appendix Table A.7 presents regressions of engagement on several pre-treatment household characteristics. In Information villages, households planning to migrate were slightly more engaged (facilitators spent 3% longer on treatment, $p = 0.05$), but we find very similar levels of engagement across education, income, the number of social connections in Nairobi, and migration experience (whether the household has ever migrated to Nairobi or currently has a migrant in Nairobi). These patterns are very similar in Mentor villages. In contrast, we find significant heterogeneity in engagement in Group villages. Households with former or current Nairobi migrants were 4–5 pp. more likely to be among the most engaged households in their meeting (mean = 9%, p-vals < 0.01). More educated, richer, and better connected households were also significantly more engaged in group discussions. These results provide initial evidence that less experienced households were less engaged in group discussions, but no less engaged in 1–1 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. Appendix Table A.8 presents regression results with an indicator for sending any new migrants to Nairobi as the outcome variable. The top panel shows interactions between a Group treatment indicator and attendance at the meeting, meeting length, and an indicator for whether the meeting included time for a former migrant to discuss their experience and take questions, estimated in separate regressions. We find that group meetings increase migration most when they are small, long, and include former migrant discussions. These meetings are more likely to approximate the Mentor treatment: they include information delivered by someone knowledgeable about Nairobi to a smaller group more intensively.

The lower panel of Appendix Table A.8 tests whether meetings in which the most engaged households, or "leaders," were experienced with migration—measured either by an indicator for having a current migrant in Nairobi, having ever sent a migrant to Nairobi, or having social connections 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 (for example, having a leader household with a current Nairobi migrant increases migration among households with a current Nairobi migrant by 12 pp. relative to the impact of experienced leaders on inexperienced households, $p < 0.01$). The impact on inexperienced households is close to zero and statistically insignificant. These patterns hold when experienced is measured by past migration to Nairobi, and are directionally similar, though smaller, when measuring experience using current Nairobi connections.

4.7 Spillovers and Information Diffusion Through Village 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.9 shows estimates of (1) on the sample of Control households and untreated Spillover households, using endline data only so that potential spillovers have had at least one year to materialize. We find no significant impacts on beliefs about average incomes in Nairobi or the household's own potential income from migrating to Nairobi. However, 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. 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 find large, positive spillover impacts on economic outcomes. Income earned in the past month is \$12 higher (on a base of \$105, effect size = 11%, $p = 0.05$). Our pre-specified overall welfare index is higher by 0.14 sd ($p < 0.01$). Given the null impacts on migration for these households, these spillovers are most likely to reflect market-level general-equilibrium impacts such as higher wages resulting from a smaller pool of laborers (Akram et al., 2017) or through demand multiplier effects caused by the infusion of cash from migrants (Egger et al., 2022).

5 Conclusion

This study provides new experimental evidence on the role of information constraints in internal migration decisions. By relieving these constraints and facilitating network connections, we find that rural households substantially underestimate urban earnings, and that correcting these misperceptions increases both migration aspirations and actual migration rates. The observed migration response, however, is modest relative to the scale of initial misperceptions, suggesting that information barriers alone are not the primary factor preventing migration for most households. We also document that the effectiveness of information interventions depends on their delivery mechanism: while individualized information provision or mentorship programs increase migration and improve economic outcomes, group-based dissemination leads to lower economic returns, likely due to adverse selection in treatment intensity of group-based delivery.

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
Disaggregated						
Info	0.011	0.018*	0.002	0.042	0.040	-0.050
	(0.008)	(0.010)	(0.011)	(0.075)	(0.065)	(0.061)
	[0.18]	[0.09]	[0.83]	[0.57]	[0.54]	[0.41]
Group	0.026**	0.022*	0.034**	0.170*	0.114	0.121
	(0.011)	(0.013)	(0.016)	(0.088)	(0.084)	(0.077)
	[0.02]	[0.10]	[0.04]	[0.05]	[0.17]	[0.12]
Mentor	0.018**	0.024**	0.024*	0.141	0.045	0.106
	(0.009)	(0.012)	(0.014)	(0.087)	(0.073)	(0.069)
	[0.05]	[0.04]	[0.08]	[0.10]	[0.54]	[0.12]
Control Mean	0.129	0.191	0.234	0.153	0.257	0.323
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Pooled Treatment						
Any Info	0.015**	0.020**	0.013	0.089	0.052	0.021
	(0.007)	(0.009)	(0.011)	(0.070)	(0.060)	(0.055)
	[0.04]	[0.03]	[0.21]	[0.21]	[0.39]	[0.71]
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Treatment Intensity						
Any Info $\times$ Prior Gap	0.035**	0.024	0.017	0.089	-0.057	0.002
	(0.016)	(0.019)	(0.028)	(0.124)	(0.099)	(0.102)
	[0.03]	[0.20]	[0.54]	[0.47]	[0.57]	[0.99]
Any Info	0.004	0.013	0.008	0.065	0.074	0.022
	(0.009)	(0.011)	(0.014)	(0.077)	(0.070)	(0.064)
	[0.67]	[0.27]	[0.57]	[0.40]	[0.29]	[0.73]
Prior Gap	-0.051***	-0.047***	-0.053**	-0.285***	-0.152*	-0.203**
	(0.014)	(0.016)	(0.026)	(0.104)	(0.084)	(0.089)
	[0.00]	[0.00]	[0.04]	[0.01]	[0.07]	[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. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Economic Outcomes

Index and Income:	Welfare Index	Income	Yearly Income	Income + Crop Profit	Real Income	Amenity-Adusted Income
Info	0.04	7.08*	54.48*	6.21	6.95*	6.51
	(0.03)	(3.73)	(28.66)	(4.17)	(3.61)	(4.11)
	[0.18]	[0.06]	[0.06]	[0.14]	[0.05]	[0.11]
Group	-0.04	-4.50	-4.52	-2.42	-4.40	-0.34
	(0.04)	(4.97)	(40.30)	(5.40)	(4.79)	(5.41)
	[0.32]	[0.37]	[0.91]	[0.65]	[0.36]	[0.95]
Mentor	0.10***	8.08*	59.66*	9.99**	8.26**	7.94*
	(0.03)	(4.26)	(33.34)	(4.83)	(4.13)	(4.75)
	[0.00]	[0.06]	[0.07]	[0.04]	[0.05]	[0.10]
Control Mean	-0.00	105.33	672.47	126.69	102.68	130.01
Observations	15,468	15,468	15,468	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	0.27***	-0.00	-0.00	0.01*	-0.01
	(0.03)	(0.08)	(0.09)	(0.01)	(0.01)	(0.01)
	[0.26]	[0.00]	[0.98]	[0.53]	[0.06]	[0.61]
Group	-0.01	0.16	-0.18	-0.01	0.00	-0.01
	(0.04)	(0.12)	(0.11)	(0.01)	(0.01)	(0.01)
	[0.79]	[0.18]	[0.11]	[0.47]	[0.97]	[0.33]
Mentor	0.02	0.34***	0.04	0.00	0.02**	0.02
	(0.03)	(0.10)	(0.11)	(0.01)	(0.01)	(0.01)
	[0.65]	[0.00]	[0.70]	[0.68]	[0.01]	[0.20]
Control Mean	181.72	10.51	6.29	0.16	0.45	0.64
Observations	15,232	15,232	15,232	15,320	15,055	15,215

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. Standard errors clustered at the village-level; two-sided p-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Appendix for "The Role of Information and Networks in Migration"

A Additional Tables and Figures

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 Kenya Integrated Household Budget Survey. Density data from Kenya National Bureau of Statistics. Distance data from Google Maps. Density, distance, and migration data include urban population. All estimates account for population weights to reflect sampling methodology.

Table A.2: Migrant Outcome Descriptives

	Ctrl. Mean	Info Diff.	Group Diff.	Mentor Diff.	N
Returned to village	0.33	0.00	0.01	-0.01	4,806
	(0.47)	(0.03)	(0.04)	(0.03)	
		[0.89]	[0.76]	[0.64]	
Duration (months)	6.81	0.13	0.16	0.00	4,728
	(5.28)	(0.28)	(0.42)	(0.32)	
		[0.64]	[0.70]	[1.00]	
Migrated with others from household	0.05	0.01	-0.02	-0.01	4,407
	(0.22)	(0.01)	(0.01)	(0.01)	
		[0.43]	[0.17]	[0.22]	
Migrated with others from village	0.04	0.03	0.02	0.00	4,407
	(0.20)	(0.01)	(0.02)	(0.01)	
		[0.01]	[0.16]	[0.91]	
Received job referral from village	0.78	-0.01	0.01	0.02	4,400
	(0.42)	(0.03)	(0.04)	(0.03)	
		[0.72]	[0.83]	[0.55]	
Received housing assistance from village	0.12	-0.01	-0.03	-0.03	4,400
	(0.32)	(0.02)	(0.03)	(0.02)	
		[0.54]	[0.24]	[0.24]	
Borrowed cash to migrate from village	0.04	0.00	-0.01	-0.00	4,400
	(0.20)	(0.01)	(0.02)	(0.01)	
		[0.75]	[0.46]	[0.96]	
Worked as employee	0.58	-0.01	0.02	-0.06	4,659
	(0.49)	(0.03)	(0.03)	(0.03)	
		[0.65]	[0.51]	[0.07]	
Worked as business owner	0.07	-0.03	-0.03	-0.01	4,659
	(0.26)	(0.01)	(0.02)	(0.02)	
		[0.07]	[0.12]	[0.38]	
Income (monthly), among workers	61.37	1.13	-4.13	3.53	4,806
	(118.09)	(6.14)	(8.51)	(8.52)	
		[0.85]	[0.63]	[0.68]	
Remittances (monthly), among workers	11.60	0.65	-1.25	-1.03	4,806
	(23.91)	(1.42)	(1.54)	(1.35)	
		[0.65]	[0.42]	[0.45]	
Weeks taken to find job after migrating	3.23	-0.29	0.80	0.34	2,912
	(12.96)	(1.06)	(2.04)	(1.24)	
		[0.78]	[0.70]	[0.79]	
Married	0.34	0.00	0.01	0.04	3,147
	(0.48)	(0.03)	(0.04)	(0.03)	
		[0.99]	[0.84]	[0.29]	
Among married, lives with spouse	0.65	-0.06	-0.08	0.01	947
	(0.48)	(0.06)	(0.08)	(0.07)	
		[0.35]	[0.32]	[0.93]	

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. 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: 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	-0.00	0.01	0.01	-0.06	0.05
	(0.01)	(0.01)	(0.01)	(0.09)	(0.08)	(0.08)
	[0.60]	[0.92]	[0.66]	[0.94]	[0.41]	[0.52]
Control Mean	0.13	0.19	0.23	0.15	0.25	0.30
Observations	6,736	6,736	6,736	6,736	6,736	6,736

Sample includes villages assigned to Information and households assigned to receive information 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. 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: 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
Disaggregated						
Info	0.006	0.008	0.009	0.008	-0.048	0.039
	(0.010)	(0.011)	(0.012)	(0.087)	(0.064)	(0.069)
	[0.51]	[0.50]	[0.44]	[0.93]	[0.45]	[0.58]
Group	0.005	0.018	0.007	0.005	0.079	0.056
	(0.014)	(0.015)	(0.018)	(0.124)	(0.078)	(0.102)
	[0.74]	[0.26]	[0.71]	[0.97]	[0.31]	[0.58]
Mentor	-0.001	0.009	-0.004	-0.053	0.048	-0.007
	(0.012)	(0.013)	(0.014)	(0.104)	(0.074)	(0.079)
	[0.91]	[0.50]	[0.75]	[0.61]	[0.51]	[0.93]
Control Mean	0.127	0.199	0.206	0.145	0.255	0.248
Observations	12,977	12,977	12,977	12,977	12,977	12,977
Pooled Treatment						
Any Info	0.004	0.009	0.005	-0.010	-0.002	0.028
	(0.009)	(0.010)	(0.011)	(0.083)	(0.059)	(0.066)
	[0.68]	[0.37]	[0.67]	[0.90]	[0.97]	[0.67]
Observations	12,977	12,977	12,977	12,977	12,977	12,977
Treatment Intensity						
Any Info $\times$ Prior Gap	0.001	0.015	0.011	-0.083	-0.079	0.048
	(0.020)	(0.025)	(0.024)	(0.155)	(0.121)	(0.121)
	[0.96]	[0.54]	[0.64]	[0.59]	[0.51]	[0.69]
Any Info	0.004	0.005	0.001	0.016	0.026	0.016
	(0.012)	(0.014)	(0.014)	(0.092)	(0.070)	(0.074)
	[0.77]	[0.75]	[0.93]	[0.86]	[0.71]	[0.83]
Prior Gap	-0.041**	-0.054**	-0.057***	-0.278**	-0.176	-0.288***
	(0.018)	(0.023)	(0.022)	(0.137)	(0.108)	(0.107)
	[0.03]	[0.02]	[0.01]	[0.04]	[0.10]	[0.01]
Observations	12,977	12,977	12,977	12,977	12,977	12,977

Impacts are estimated on data from midline surveys. Linear regression is used for outcomes with negative values or bounded between 0 and 1; poisson regression is used otherwise. *Any City* includes any urban area. Responses of “Don’t Know”are coded as missing. 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: Treatment Impacts on Selection Into Migrating to Nairobi

	Ctrl. Mean	Info Diff.	Group Diff.	Mentor Diff.	N
More than primary education	0.69	0.00	0.00	-0.05	16,818
	(0.46)	(0.03)	(0.04)	(0.04)	
		[0.98]	[0.94]	[0.17]	
Belongs to minority tribe	0.06	-0.00	-0.01	-0.03	16,878
	(0.24)	(0.02)	(0.02)	(0.02)	
		[0.90]	[0.52]	[0.12]	
Baseline welfare index	0.16	-0.04	-0.15	-0.18	16,878
	(1.13)	(0.08)	(0.09)	(0.08)	
		[0.61]	[0.10]	[0.02]	
Monthly household income (USD)	106.82	-3.51	-15.44	-27.20	16,878
	(144.81)	(10.14)	(11.58)	(9.85)	
		[0.73]	[0.18]	[0.01]	
Has a migrant in Nairobi right now	0.69	-0.05	0.08	-0.05	16,878
	(0.46)	(0.03)	(0.04)	(0.03)	
		[0.06]	[0.04]	[0.12]	
Has ever migrated to Nairobi	0.79	-0.05	0.06	-0.05	16,878
	(0.40)	(0.02)	(0.03)	(0.03)	
		[0.04]	[0.11]	[0.10]	
Plans to migrate to Nairobi	0.48	-0.03	-0.01	-0.06	16,878
	(0.50)	(0.02)	(0.03)	(0.02)	
		[0.11]	[0.80]	[0.01]	
Plans to migrate to any city	0.55	-0.04	-0.01	-0.08	16,878
	(0.50)	(0.02)	(0.03)	(0.03)	
		[0.06]	[0.78]	[0.00]	
Perceived migration earnings	144.86	0.67	-7.19	-6.46	16,226
	(98.08)	(7.58)	(11.33)	(8.38)	
		[0.93]	[0.53]	[0.44]	
# Nairobi social connections	3.76	0.15	-0.07	0.14	16,868
	(3.81)	(0.29)	(0.32)	(0.35)	
		[0.61]	[0.84]	[0.70]	
Has Nairobi housing network	0.74	-0.03	-0.09	-0.04	16,868
	(0.44)	(0.03)	(0.04)	(0.03)	
		[0.26]	[0.02]	[0.20]	
Has Nairobi jobs network	0.61	-0.01	-0.08	0.02	16,868
	(0.49)	(0.03)	(0.04)	(0.04)	
		[0.75]	[0.08]	[0.65]	
# origin migration advice connections	0.67	-0.13	-0.23	-0.23	16,614
	(0.88)	(0.06)	(0.07)	(0.07)	
		[0.04]	[0.00]	[0.00]	
Village sociality index	0.81	0.00	-0.02	-0.02	16,878
	(0.10)	(0.01)	(0.02)	(0.01)	
		[0.89]	[0.15]	[0.16]	

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. 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 and Welfare Impacts

Outcome: Sent New Migrants to Nairobi	Low Income	High Income	No Mig. in Nairobi	Mig. in Nairobi	Low Nairobi Connections	High Nairobi Connections	Low Origin Connections	High Origin Connections
Info	0.03***	-0.04**	0.02**	-0.01	0.01	0.00	0.02	0.00
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
	[0.00]	[0.01]	[0.03]	[0.66]	[0.16]	[0.77]	[0.11]	[0.72]
Group	0.04***	-0.00	-0.00	0.07**	0.03**	0.02	0.04***	0.01
	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
	[0.00]	[0.94]	[0.99]	[0.01]	[0.01]	[0.37]	[0.00]	[0.67]
Mentor	0.03***	-0.02	0.02**	0.01	0.02*	0.02	0.04***	-0.00
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
	[0.00]	[0.42]	[0.04]	[0.56]	[0.05]	[0.42]	[0.00]	[0.77]
Control Mean	0.11	0.18	0.08	0.21	0.11	0.16	0.12	0.14
Observations	10,856	4,612	9,722	5,746	10,006	5,462	8,184	7,284
Outcome: Household Income								
Info	8.13**	4.64	8.06*	5.41	11.32***	-0.55	5.55	8.96*
	(3.54)	(8.27)	(4.24)	(6.58)	(3.92)	(7.00)	(4.91)	(5.35)
	[0.02]	[0.57]	[0.06]	[0.41]	[0.00]	[0.94]	[0.26]	[0.09]
Group	4.19	-23.24**	-4.27	-8.26	2.66	-17.55*	-7.78	-0.31
	(4.73)	(9.38)	(5.21)	(9.50)	(4.92)	(9.49)	(5.90)	(7.60)
	[0.38]	[0.01]	[0.41]	[0.38]	[0.59]	[0.06]	[0.19]	[0.97]
Mentor	7.18*	9.94	8.74*	6.39	8.17*	8.02	8.07	6.98
	(3.97)	(9.49)	(4.74)	(7.36)	(4.29)	(8.12)	(5.39)	(6.28)
	[0.07]	[0.30]	[0.07]	[0.39]	[0.06]	[0.32]	[0.14]	[0.27]
Control Mean	80.51	162.10	93.52	123.79	94.87	124.72	104.85	105.87
Observations	10,856	4,612	9,722	5,746	10,006	5,462	8,184	7,284

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 a migrant in Nairobi before treatment, 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. 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: Engagement During Information Meetings

Outcome: Engagement During Intervention	Info	Group	Mentor
More than primary education	-0.01	0.03**	0.01
	(0.01)	(0.01)	(0.01)
	[0.35]	[0.02]	[0.33]
Household income	-0.00	0.00***	0.00
	(0.00)	(0.00)	(0.00)
	[0.14]	[0.00]	[0.21]
Has a migrant in Nairobi right now	-0.01	0.04***	0.01
	(0.01)	(0.02)	(0.01)
	[0.44]	[0.01]	[0.62]
Has ever migrated to Nairobi	0.01	0.05***	0.01
	(0.01)	(0.01)	(0.01)
	[0.66]	[0.00]	[0.66]
Plans to migrate to Nairobi	0.03**	0.02	0.01
	(0.01)	(0.02)	(0.01)
	[0.05]	[0.15]	[0.52]
# Nairobi connections	0.00	0.01***	0.00
	(0.00)	(0.00)	(0.00)
	[0.66]	[0.00]	[0.61]
Mean (Engagement)	6.53	0.09	11.92
Observations	6,939	1,924	3,595

Each cell triplet shows estimates from a single regression of intervention engagement on a single predictor variable. Engagement is measured as the minutes spent on the 1–1 information meeting for Information and Mentor treatments (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). All regressions control for an enumerator fixed effect, a randomization-stratum 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.8: Heterogeneous Group Impacts by Meeting Dynamics

	Outcome: Sent New Migrants to Nairobi		
Group Meeting Characteristics (M):	Total Attendance	Length (Minutes)	Former Migrant Component
M × Group	-0.00*	0.00***	0.05**
	(0.00)	(0.00)	(0.02)
	[0.06]	[0.01]	[0.01]
Group	0.06**	-0.06*	-0.02
	(0.02)	(0.03)	(0.02)
	[0.01]	[0.07]	[0.36]
Group Mean (Outcome)	0.15	0.15	0.15
Group Mean (M)	24.16	46.23	0.89
Observations	15,468	15,468	15,468
Leader and Household Interactions:	Has Migrant in Nairobi	Ever Migrated to Nairobi	Has Connections in Nairobi
Leader of Type X × X × Group	0.12***	0.09***	0.02
	(0.03)	(0.03)	(0.02)
	[0.00]	[0.00]	[0.24]
Leader of Type X × Group	-0.02	-0.03	0.03**
	(0.02)	(0.02)	(0.02)
	[0.27]	[0.14]	[0.05]
X × Group	0.09***	0.08***	0.07***
	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.00]
Group	0.00	0.01	-0.02
	(0.02)	(0.02)	(0.01)
	[0.76]	[0.42]	[0.13]
Group Mean (Outcome)	0.15	0.15	0.15
Group Mean (X)	0.36	0.48	0.73
Group Mean (Leader of Type X)	0.72	0.78	0.95
Observations	15,468	15,468	15,458

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 in Panel A lists the group meeting characteristic M that is interacted with Group treatment in the regression. is a binary variable indicating whether a former migrant was available in that group meeting to discuss their migration experience and answer audience questions. Each column title in Panel B 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 has type X indicated in that column. All regressions estimated using (**??**) (coefficients for treatments other than Group not shown). 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: Spillover Impacts of Information Treatment on Untreated Households

	Perceived Nairobi Income	Perceived Own Nairobi Income	Sent New Migrants to Nairobi	Sent New Migrants to Any City	Income	Welfare Index
Untreated HH in Spillover Village	-0.01	0.04	0.00	-0.00	12.19**	0.14***
	(0.04)	(0.05)	(0.01)	(0.02)	(6.14)	(0.05)
	[0.78]	[0.42]	[0.72]	[0.79]	[0.05]	[0.01]
Control Mean	16654.53	24651.84	0.13	0.23	105.33	0.00
Observations	2,910	4,124	4,247	4,247	4,247	4,247

Impacts are estimated on data from endline surveys. Sample includes villages assigned to Control and households assigned to NOT 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. Standard errors clustered at the village-level; 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	4.85	4.85	4.83	4.84	0.89
# children under 5	0.73	0.75	0.69	0.75	0.70	0.25
# adults 18-35	1.54	1.54	1.48	1.44	1.58	0.30
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	58.41	53.70	53.90	51.19	51.04	0.09
Expenditure	17.82	18.19	18.82	17.02	18.39	0.92
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 ever migrated to Nairobi	0.50	0.50	0.47	0.44	0.48	0.21
Has ever migrated to a city	0.64	0.64	0.62	0.59	0.63	0.32
Has a migrant in Nairobi right now	0.36	0.36	0.35	0.33	0.35	0.62
Has a migrant in a city right now	0.49	0.49	0.48	0.47	0.46	0.61
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	145.74	147.31	141.25	133.89	142.43	0.12
# of social contacts in Nairobi	3.65	3.50	3.03	2.65	3.30	0.04
# of origin connections (farm assistance)	1.16	1.23	1.14	0.73	1.16	0.83
# of origin connections (job advice)	1.37	1.50	1.41	1.39	1.59	0.14
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.)	249.60	243.93	242.42	289.86	247.29	0.44
Village distance to county capital (km.)	44.11	43.54	46.24	48.49	44.18	0.99
Village households	100.15	105.13	101.22	101.16	103.41	0.34
Village population	430.18	445.53	440.41	446.24	450.93	0.69

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.

Table B.2: 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	0.51	0.95	0.97	0.96	0.92
# children under 5	0.18	0.24	0.44	0.20	0.47	0.25
# adults 18-35	0.57	0.20	0.84	0.37	0.83	0.34
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	0.16	0.08	0.38	0.09	0.26	0.08
Expenditure	0.99	1.00	0.91	0.90	0.87	0.85
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 ever migrated to Nairobi	0.35	0.30	0.54	0.50	0.45	0.35
Has ever migrated to a city	0.61	0.47	0.85	0.48	0.82	0.42
Has a migrant in Nairobi right now	0.45	0.41	0.64	0.66	0.58	0.62
Has a migrant in a city right now	0.48	0.47	0.49	0.57	0.50	0.54
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	0.57	0.20	0.37	0.10	0.47	0.13
# of social contacts in Nairobi	0.06	0.02	0.08	0.09	0.09	0.10
# of origin connections (farm assistance)	0.81	0.97	0.96	0.93	0.95	0.93
# of origin connections (job advice)	0.07	0.39	0.03	0.14	0.04	0.14
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	0.25	0.34	0.28	0.36	0.28	0.36
Village population	0.61	0.67	0.57	0.71	0.59	0.71
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 use sampling weights to recover population-representative estimates.

Table B.3: Randomization balance of treatment assignment within spillover villages.

	Joint p-Value in Sample Surveyed at:							
	Baseline		Midline		Endline		Endline w. Attrits	
Household size	0.09	0.14	0.08	0.34	0.19	0.23	0.18	0.23
# children under 5	0.62	0.21	0.51	0.31	0.83	0.27	0.78	0.28
# adults 18-35	0.89	0.87	0.85	0.97	0.95	0.95	0.98	0.97
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	0.67	0.26	0.42	0.16	0.65	0.25	0.64	0.32
Expenditure	0.08	0.14	0.03	0.02	0.07	0.15	0.06	0.15
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 ever migrated to Nairobi	0.23	0.10	0.37	0.34	0.28	0.11	0.31	0.13
Has ever migrated to a city	0.13	0.07	0.32	0.30	0.18	0.10	0.19	0.10
Has a migrant in Nairobi right now	0.53	0.15	0.58	0.43	0.86	0.26	0.74	0.24
Has a migrant in a city right now	0.51	0.34	0.47	0.67	0.37	0.46	0.45	0.41
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	0.17	0.04	0.05	0.01	0.13	0.04	0.14	0.03
# of social contacts in Nairobi	0.22	0.46	0.53	0.89	0.41	0.63	0.42	0.63
# of origin connections (farm assistance)	0.96	0.29	0.89	0.47	0.79	0.22	0.87	0.24
# of origin connections (job advice)	0.38	0.10	0.36	0.08	0.30	0.10	0.32	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 use sampling weights to recover population-representative estimates.

B.2 Robustness to Sample Selection and Estimation Strategy

Table B.4: 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	0.02*	0.00	0.04	0.04	-0.05
	(0.01)	(0.01)	(0.01)	(0.08)	(0.07)	(0.06)
	[0.20]	[0.09]	[0.82]	[0.62]	[0.57]	[0.41]
Group	0.03**	0.02*	0.04**	0.17**	0.12	0.13*
	(0.01)	(0.01)	(0.02)	(0.09)	(0.08)	(0.08)
	[0.01]	[0.07]	[0.03]	[0.05]	[0.14]	[0.10]
Mentor	0.02*	0.02**	0.02*	0.14	0.04	0.10
	(0.01)	(0.01)	(0.01)	(0.09)	(0.07)	(0.07)
	[0.05]	[0.04]	[0.08]	[0.12]	[0.57]	[0.13]
Control Mean	0.13	0.19	0.24	0.15	0.26	0.32
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	7.04*	55.54*	6.21	6.92*	6.38
	(0.03)	(3.80)	(28.92)	(4.24)	(3.67)	(4.16)
	[0.18]	[0.06]	[0.06]	[0.14]	[0.06]	[0.13]
Group	-0.03	-3.47	3.86	-1.42	-3.41	1.17
	(0.04)	(4.96)	(39.93)	(5.37)	(4.78)	(5.45)
	[0.42]	[0.48]	[0.92]	[0.79]	[0.48]	[0.83]
Mentor	0.09***	7.87*	57.44*	9.64**	8.05*	7.72
	(0.03)	(4.32)	(33.65)	(4.88)	(4.19)	(4.81)
	[0.01]	[0.07]	[0.09]	[0.05]	[0.06]	[0.11]
Control Mean	0.01	106.60	680.83	128.27	103.93	131.25
Observations	15,232	15,232	15,232	15,232	15,232	15,232

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

Table B.5: 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	0.02	0.00	0.04	0.05	-0.07
	(0.01)	(0.01)	(0.01)	(0.08)	(0.07)	(0.07)
	[0.30]	[0.13]	[0.95]	[0.65]	[0.46]	[0.31]
Spillover	0.02	0.02	0.01	0.06	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.10)	(0.08)	(0.08)
	[0.13]	[0.17]	[0.58]	[0.50]	[0.99]	[0.99]
Group	0.03**	0.02*	0.03**	0.17*	0.11	0.12
	(0.01)	(0.01)	(0.02)	(0.09)	(0.08)	(0.08)
	[0.02]	[0.10]	[0.04]	[0.06]	[0.18]	[0.11]
Mentor	0.02**	0.02**	0.02*	0.14	0.04	0.11
	(0.01)	(0.01)	(0.01)	(0.09)	(0.07)	(0.07)
	[0.05]	[0.04]	[0.08]	[0.11]	[0.56]	[0.12]
Control Mean	0.13	0.19	0.23	0.15	0.26	0.32
Observations	16,339	16,339	16,339	16,339	16,339	16,339

Index and Income:	Welfare Index	Income	Yearly Income	Income + Crop Profit	Real Income	Amenity-Adusted Income
Info	0.03	8.21**	43.95	6.98	7.94**	7.95*
	(0.03)	(4.03)	(30.19)	(4.52)	(3.89)	(4.49)
	[0.26]	[0.04]	[0.15]	[0.12]	[0.04]	[0.08]
Spillover	0.05	3.56	86.39*	3.75	3.86	1.90
	(0.04)	(5.28)	(46.70)	(5.80)	(5.17)	(5.46)
	[0.23]	[0.50]	[0.06]	[0.52]	[0.46]	[0.73]
Group	-0.04	-4.79	-6.85	-2.70	-4.69	-0.41
	(0.04)	(4.95)	(40.57)	(5.37)	(4.78)	(5.42)
	[0.31]	[0.33]	[0.87]	[0.61]	[0.33]	[0.94]
Mentor	0.10***	7.98*	60.13*	9.87**	8.17**	7.84*
	(0.03)	(4.24)	(33.37)	(4.81)	(4.11)	(4.74)
	[0.00]	[0.06]	[0.07]	[0.04]	[0.05]	[0.10]
Control Mean	-0.00	105.33	672.47	126.69	102.68	130.01
Observations	16,339	16,339	16,339	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. 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: 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	0.02*	0.00	0.04	0.04	-0.05
	[0.20]	[0.10]	[0.85]	[0.56]	[0.57]	[0.14]
Group	0.03**	0.02	0.03*	0.17*	0.11	0.12
	[0.03]	[0.12]	[0.07]	[0.09]	[0.23]	[0.21]
Mentor	0.02**	0.02**	0.02*	0.14	0.04	0.11
	[0.05]	[0.04]	[0.07]	[0.10]	[0.54]	[0.04]
Control Mean	0.13	0.19	0.23	0.15	0.26	0.32
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Info	0.04	7.08*	54.48*	6.21	6.95*	6.51
	[0.22]	[0.06]	[0.08]	[0.15]	[0.06]	[0.13]
Group	-0.04	-4.50	-4.52	-2.42	-4.40	-0.34
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Mentor	0.10***	8.08*	59.66*	9.99**	8.26*	7.94*
	[0.00]	[0.07]	[0.09]	[0.05]	[0.06]	[0.10]
Control Mean	-0.00	105.33	672.47	126.69	102.68	130.01
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. Two-sided p-values estimated through randomization inference in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: 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	0.02	-0.00	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[0.25]	[0.11]	[0.93]	[0.35]	[0.33]	[0.61]
Group	0.02**	0.02	0.03	0.02*	0.02	0.03
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
	[0.05]	[0.24]	[0.11]	[0.08]	[0.41]	[0.19]
Mentor	0.01	0.02**	0.02	0.02	0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	[0.11]	[0.05]	[0.16]	[0.18]	[0.76]	[0.31]
Control Mean	0.13	0.19	0.23	0.15	0.26	0.32
Observations	15,468	15,468	15,468	15,468	15,468	15,468
Index and Income:	**Welfare Index**	**Income**	**Yearly Income**	**Income + Crop Profit**	**Real Income**	**Amenity-Adusted Income**
Info	0.03	7.85**	59.05**	5.87	7.61**	6.48*
	(0.03)	(3.56)	(28.45)	(4.01)	(3.48)	(3.81)
	[0.20]	[0.03]	[0.04]	[0.14]	[0.03]	[0.09]
Group	-0.06*	-5.19	-8.92	-5.31	-5.26	-2.19
	(0.04)	(4.80)	(40.26)	(5.32)	(4.65)	(5.02)
	[0.09]	[0.28]	[0.82]	[0.32]	[0.26]	[0.66]
Mentor	0.09***	7.75*	54.41	8.18*	7.55*	8.49**
	(0.03)	(4.01)	(33.76)	(4.55)	(3.93)	(4.26)
	[0.00]	[0.05]	[0.11]	[0.07]	[0.05]	[0.05]
Control Mean	-0.00	105.33	672.47	126.69	102.68	130.01
Observations	15,468	15,468	15,468	15,468	15,468	15,468

Controls selected using double-lasso regression (Belloni et al., 2014) on a set of pre-specified pre-treatment variables. Impacts are estimated on data from endline surveys. Linear regression is used for all outcomes. *Any City* includes any urban area. Responses of "Don't Know"are coded as missing. 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 earnings less. Also, remember that people who migrate to Nairobi might earn a different amount compared to people who already live there.

Do you have any questions about this?

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

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

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

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

B.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)

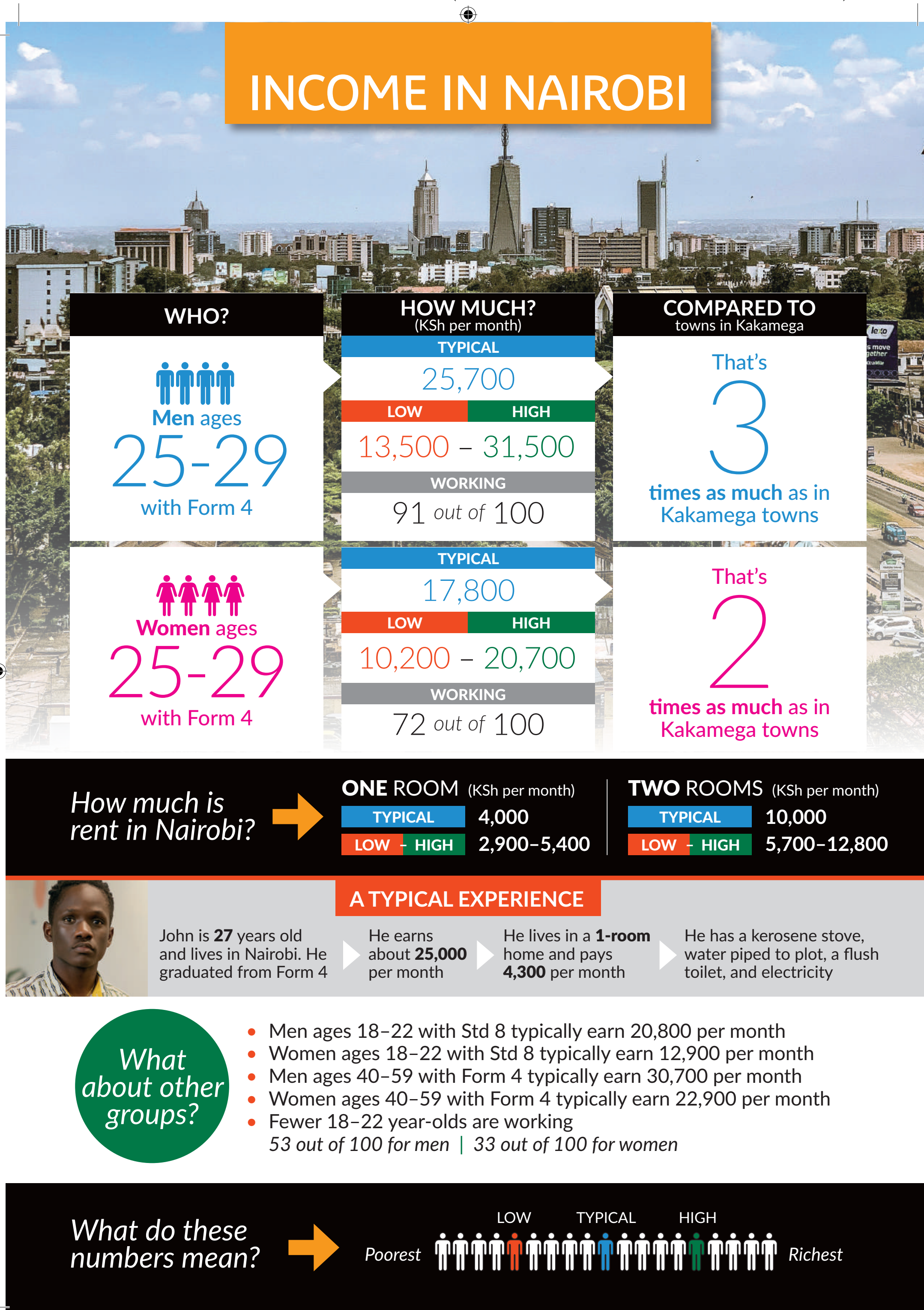
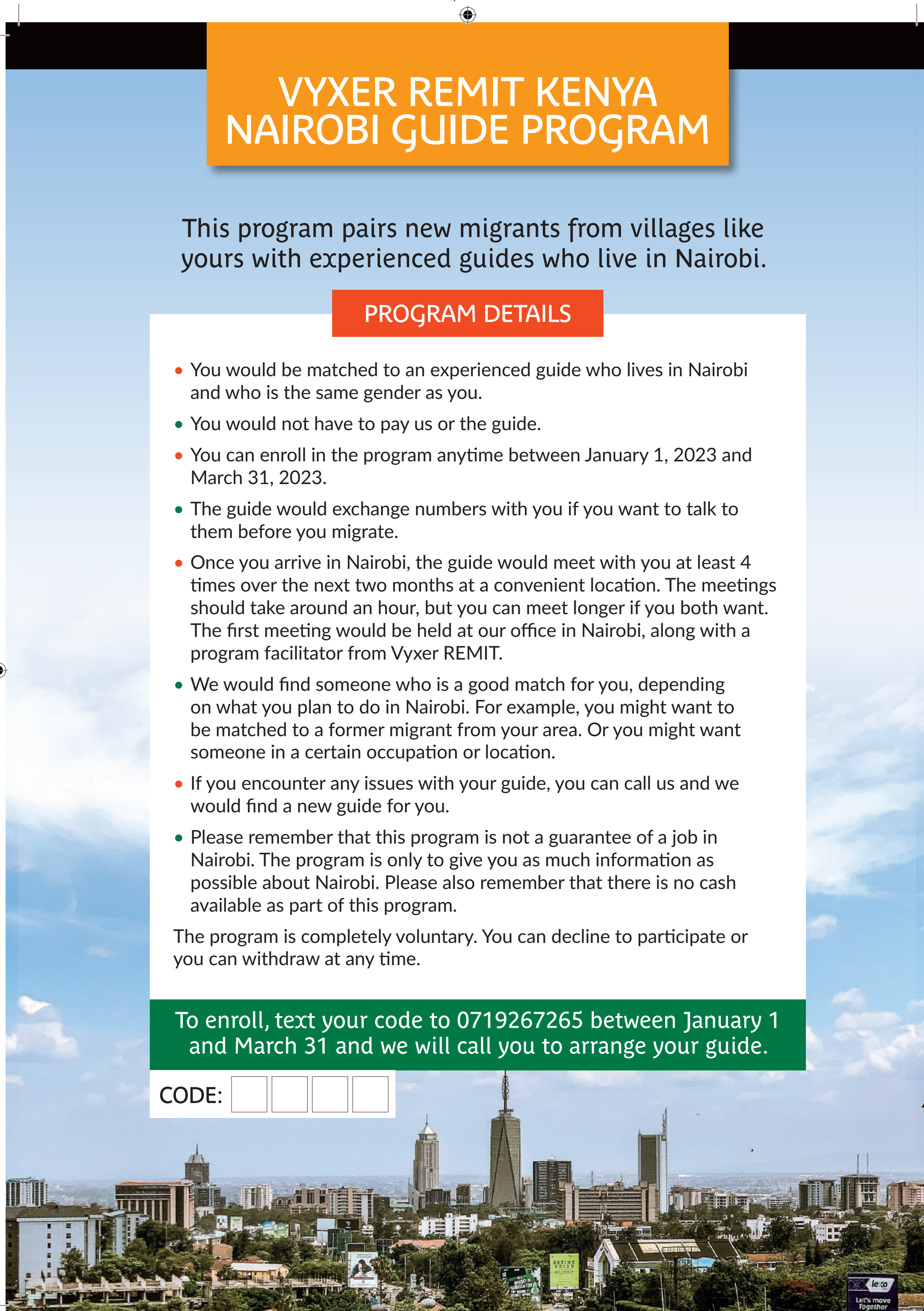


Figure B.2: Mentor Program Description (Given to All Households in Mentor Villages)



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

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

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

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.

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, non-food consumables, education, and healthcare):

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

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

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^U is the quantity-weighted average price of good g in cities other than Nairobi, $QUAL_c^N$ is the quality premium for Nairobi goods in category c, and analogously for $QUAL_c^U$.

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 as:

$$p_g^R = \frac{\sum_{i \in R} x_{ig} p_{ig}}{\sum_{i \in R} x_{ig}}, \quad \text{and}$$

$$p_g^N = \frac{\sum_{i \in N} x_{ig} p_{ig}}{\sum_{i \in N} x_{ig}}, \quad \text{and}$$

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

where i indexes over households in rural parts of our five study counties, Nairobi, or other Kenyan cities for p_g^R, p_g^N, and p_g^U respectively; and λ_i is a weight, measured at the county level (excluding Nairobi), equal to the share of rural-urban migrants who migrated to that county in our baseline data. Weighting by λ_i ensures that cities that are better-represented as migration destinations receive a higher weight in the UCPI index.

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.

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 ${baseline_location}, if you could earn it while living there? As a reminder, you told me that your current income is ${income} per month." We then compute amenity-adjusted family income as:

$$\text{Income}_i = \sum_b \text{Village Profit}_{ib} + \sum_m \text{Indifference Income}_{im},$$

where Indifference Wage_{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.